Hilbert's Early Foundational Work, 1885–1921

Moritz Bodner

Department of Philosophy

McGill University, Montréal

A thesis submitted to McGill University in partial fulfillment of the requirements of the degree of Doctor of Philosophy.

May 2022

Abstract. This dissertation focuses Hilbert's early foundational work, meaning Hilbert's discussion of various methodological and philosophical issues before the 1920s. Stated in the most general terms, my aim is twofold: to show by means of several examples (A) how entangled Hilbert was in the (philosophical) discussions of his time and (B) that there is no «gap» between Hilbert's early and later foundational work. With respect to the first, I discuss in particular: (i) the controversy with Frege about the question of meaning in mathematics, with special attention to Alwin Korselt's contribution to the debate; (ii) Hilbert's uptake of Ernst Schröder's work on logic and arithmetic, showing that Hilbert studied Schröder's *early* work very closely; (iii) the integration of contemporary philosophical doctrines, especially a version of (neo-)Kantianism espoused by the Königsberg philosophy-professor Günther Thiele, into Hilbert's own philosophical position, in particular, into Hilbert's view of mathematics as a science of "pure thought". (As far as I am aware, Thiele's influence on Hilbert is here discussed for the first time, and Hilbert's reception of Schröder's work is here discussed for the first time *in detail*.) Of special interest as sources for Hilbert's philosophical position before 1920 are the three volumes of Hilbert's so called "scientific diary" discussed here at length for the first time, as well as other manuscripts and lectures notes from the years between 1904-1917, which show Hilbert continuously at work on foundational issues, especially the axiomatisation of the theory of natural numbers (from 1913-1921) and the proper solution of Richard's paradox (from 1910-1924).

Resumé. Cette thèse porte sur les travaux fondationnels de Hilbert avant 1920, c'est-à-dire, elle est consacré aux travaux de Hilbert sur des problèmes méthodologiques et philosophiques entre environ 1885 et 1920. À parler d'une manière genérale, j'y persuis le double but de montrer d'abord (A) les liens intimes entre les idées philosophiques de Hilbert et la philosophie de son époque et, en outre, (B) que il n'y a aucune hiatus entre une pensée fondatrice de Hilbert dite "antérieure" (avant 1917) et une autre pensée dite "mature" (après 1920). Quant au but premier je discute trois problemes particuliers dans ce thèse-ci: (i) la controverse entre Frege et Hilbert concernant la question de la signification dans les mathematiques avec une attention particulière aux contributions de Alwin Korselt; (ii) l'adaptation à part de Hilbert des oeuvres logiques et arithmétiques de Ernst Schröder dont je montre que Hilbert etudiait éminemment les écrits primitives de Schröder; (iii) l'intégration des idées de la philosophie des dernières décennies de la XIX$^{\text{ième}}$ siècle dans les avis de Hilbert lui-même sur la mathématique conçue comme une sciènce de "pensée pure" et, en particulier, de la pensée (néo-)kantienne soutenue par le professeur de la philosophie à Königsberg à l'époque, Günther Thiele. (Autant que je sache personne n'a discuté jusqu'à présent les idées de Günther Thiele et l'adaptation de Schröder à part de Hilbert n'a été discuté que superficiellement.) À titre de sources pour cerner la position philosophique de Hilbert avant 1920, les trois tomes du journal scientific de Hilbert sont d'un intérêt privilégié et je suis le premier à les discuter abondamment, ainsi que d'autres manuscripts et notes de cours de l'epoque entre 1905 et 1917 qui attestent que Hilbert travaillait de manière continue aux problèmes fondationnels, en particulier, à l'axiomatisation de la théorie des nombres entiers naturels (entre 1913-1921) et à la résolution véritable de la paradoxe de Richard (entre 1910-1924).

"As to fundamentals I am coming – in view especially of biological evolution and physics – relativity – not to speak of similar developments in most phases of life – to look at life itself as a tragically glorious adventure or experiment, and accordingly to have doubts as to achievable ultimates. The best individuals or groups of individuals can hope to do is to contribute something worth considering in the progressive experiment and to cherish the hope that gradually inconceivably higher and more profound types of life and of insight may be evolved."

(from a letter by E.H. Moore to Heinrich Behmann, dated 12.4.1931[1])

"if nothing else it's an idle curiosity"

(Pinegrove: "The Size of the Moon)"

[1]This (handwritten) letter is to be found in box 2 of the Behmann-archive (Nachlass 335) held by the Abteilung Nachlässe und Handschriften of the Staatsbibliothek Preußischer Kulturbesitz in Berlin (SBB), in a folder containing Behmann's correspondence with Moore, among others. It appears here with kind permission of the SBB. The letter is written originally in English and in response to a letter (dated 29/3/1930, a carbon-copy of which is included in the same folder) Behmann had sent to Moore asking for references to relevant publications of Moore's about symbolic logic. Contrary to what the beginning of the passage here quoted suggests, Behmann had not touched on "fundamentals" in his initial letter or posed any question to which this little confession can be seen as responding; rather, Moore is probably picking up on his own proclamation at the beginning of his letter that "[t]he primary purpose [of his notation] is to be of use in the practical (rather than critical-foundational) work of the mathematician".

Introduction

This dissertation focuses on David Hilbert's early foundational work: Hilbert's academic career began with his qualification to teach mathematics at the university level in the month of July 1886, and came to an end almost half a century later some time in the 1930s (Hilbert delivered his last regular lectures in 1934). I shall here be concerned mainly with the middle third of Hilbert's nearly 50-year-long academic career, i.e. the time between (the years leading up to) the publication of Hilbert's booklet on the *Grundlagen der Geometrie* in 1899 and his 1917 lectures on logical theory; and specifically with those among Hilbert's claims and ideas bearing on the nature, methodology and justification of mathematics. That is, broadly speaking, what I mean by "Hilbert's early foundational work".

While the later stages of Hilbert's career from 1917 onward, and to some extent also his early (meta-)geometrical work, have been the subject of a fair amount of study – and studies – at the hand of scholars, Hilbert's intellectual development during the 15-or-so years between, say, 1902, when Hilbert last lectured on geometry in the wake of the publication of his *Grundlagen der Geometrie*, and 1917, when Hilbert began his first lecture concerned mainly with logical theory, has received relatively little attention from scholars and historians. (Hilbert's 1905 lecture on entitled "Logical Principles of Mathematical Thinking" is an exception to this general neglect, but, as will become clear when seeing it in context, also rather an anomaly in Hilbert's teaching activities during those years.)

The ambition of this text is primarily to remedy this imbalance of attention some and show by example and on the basis of a series of still unpublished sources that these earlier years are not devoid of occasions for reflection (even for philosophers—since they certainly are not for mathematicians and physicists, and their specialist historians), and that Hilbert thought about foundational issues constantly throughout his life, though, owing to the fragmentation of sources, some reconstruction is necessary in order to appreciate the *offerts* of these years.

0.1 Organisation

The organising principle of my discussion is the prominence, availability and «obviousness» (from a the vantage point of philosophy and its historiography) of the sources I consider in each chapter. I begin by discussing the correspondence between Hilbert and Frege (chapter 1), which has been, relative to the amount of material available, discussed rather extensively in scholarship, and use it mainly as a means to set up two problems about the

meaningfulness of mathematics. The problems I raise are not the problems Frege himself raised and discussed – Frege was too focused on the problem of assigning a unique extension to mathematical expressions – in the form I give it, but rather one which, as I see it, emerges from reflecting on the reasons why Hilbert remained so completely untouched by the problems Frege urged him to consider. The reason, I take it, for Hilbert's indifference to Frege lies in the dependence of Frege's criticism on substantive assumptions about what constitutes meaning. The problem Frege's criticism of Hilbert thus raises to my mind is the following: how to conceive of meaning, or meaningfulness, such that one can regard the expressions constituting a mathematical theory as possessing unchanging meaning, or being meaningful, while at the same time allowing for them to be freely «applied» to different (classes of) objects.

In chapter 2 I consider a proposal put forward by Korselt, along with what strikes me as a persuasive reason for rejecting the assumption about meaningfulness subtending Frege's criticism of Hilbert, i.e. Frege's conviction that in order to be meaningful an expression must be associated with a uniquely determined extension.

In chapter 3 I finally turn my attention to Hilbert's own works, which, once again, I discuss in order of increasing obscurity: I look, first (in section 3.1) at methodological and philosophical remarks found in Hilbert's famous little booklet on geometry, then (in section 3.2) at the various unpublished (or only recently published) notes for lectures Hilbert delivered (before and) after the publication of the *Grundlagen der Geometrie*, and finally (in section 3.3) I explore miscellaneous manuscripts, which remain unpublished to this day, chief among them Hilbert's so called "Scientific Diary", which, in my opinion, ranges among the richest and most interesting almost entirely untapped sources both for finding out Hilbert's views and ideas, as well as for reading one outstanding mathematician's thoughts about his work, his discipline on the whole, and his times (and contemporaries).

As appendices to the dissertation a handful of hitherto unpublished manuscripts, all relatively short, appear with the kind permission of the libraries libraries housing them.

The appendixes contain transcriptions of three kinds of manuscripts:

- three letters found among the papers of Heinrich Behmann held by the Staatsbibliothek Preußischer Kulturbesitz in Berlin (SBB), which are, as far as I am ware, the best, and, indeed, only source for detailed information about the nature and content of Alwin Korselt's *later* work (which was never published, nor is its manuscript known to have survived): Behmann served, upon Bernays' request (letter A.1), as the referee for deciding whether to publish an apparently rather lengthy manuscript Korselt had submitted to the *Mathematische Annalen* in 1937, and a carbon-copy of the referee-report (letter A.2.1) Behmann sent to Blumenthal (then the managing editor) survives, along with a copy of a subsequent letter (letter A.2.3) also discussing some substantive issues of Korselt's manuscript (which was, ultimately, rejected from publication on the ground of being too long, diffuse and ignorant of the current state of research in logic);

- two manuscripts of talks delivered by Behmann – one in 1914 in Göttingen ("On Mathematical Logic"), the other in 1927 in Kiel ("On Russell's Theory of Logical Types") – on the subject of Russell's logical

theory, testifying to the nature of Behmann's interest in Russell. I take these texts to show that Behmann's was different from the interest Hilbert took in Russell's work, and Behmann therefore an unlikely mediator between Russell's work and the markedly Russellian logical theory Hilbert began developing in 1917.

- two unpublished shorter manuscripts of David Hilbert's, which shed light on his preoccupations and ideas during the years that are at the centre of this dissertation:
  - The convolute Cod. Ms. D. Hilbert 559, which contains (i) clean lecture notes produced by Bernhard Baule on the basis of Hilbert's lectures in 1913, (ii) Hilbert's own preparatory notes outlining the lecture-course in 1913, and (iii) Hilbert's preparatory notes for his 1914/15 lectures.
  - Cod. Ms. D. Hilbert 606 containing a predominantly philosophical discussion of Kant's claim that arithmetical propositions are analytic and of several subsequent criticisms of Kant's position.

    This manuscript likely dates from around 1885 and probably does not reflect Hilbert's own opinions or ideas (except possibly in its footnotes), but is of interest as a testament to Hilbert's exposure during his student days to philosophical discussions about mathematics.

Needless to say, this is a dissertation, and so by its very nature – as work undertaken for the purpose of academic qualification), a text written for a specialist audience, in whose eyes it is meant to qualify me as «one of them» on account of my training and talent, such as it is – concerned with very narrowly circumscribed problems; most of them, in my case, within the scholarship of Hilbert and persons surrounding him in his time or his field.

Indeed, I see the main scholarly accomplishments of this dissertation, at least insofar as it concerns Hilbert's intellectual biography, in (A) reconstructing the contours and contents of what I will called Hilbert's "philosophical position". In addition, I believe I have succeeded (B) in shedding some new light on the development of Hilbert's early logical theory (in 1905), and Hilbert's theory of (elementary) arithmetic (in 1913).

Although no chapter bears Schröder's name, I will come back to his work again (in section 1.2), and again (in section 3.2.1) and again (in section 3.4.1). Indeed, I am tempted to say that Ernst Schröder ultimately emerges as the secret hero of this dissertation, mainly because he appears – if my interpretation of the evidence is correct – to have had substantial influence on Hilbert's technical work in two key areas: logical theory and elementary number theory. To condense into a slogan one of the conclusions of this dissertation, in my opinion rather firmly, establishes, I'll say: As a matter of historical fact Ernst Schröder *was* what Frege has often (falsely) been taken for[2], the pivotal figure in the development of logic whose work synthesised the tradition preceding it and at the same time shaped mathematical logic in(to) the form it developed in the first decades of the 20$^{\text{th}}$ century.

[2]Recall, for example, Quine's quotable aphorism about the development of the discipline in Willard van Orman Quine. *Methods of Logic*. Revised Edition. 1959, p. vii: "Logic is an old subject, and since 1879 [the year of publication of Frege's *Begriffsschrift*] it has been a great one." If one wanted to indulge the notion of dating the beginning of contemporary mathematical logic to a precise year, that year should, I contend, be 1877, when Schröder's *Operationskreis des Logikkalkuls* was published.

This dissertation is thus mainly a work of historical scholarship. But I believe it lies in its nature that one cannot do work in intellectual historiography without thinking through the substantive issues. The historian's task, as I conceive of it, consists in pinpointing – by, so to speak, a kind of intellectual echolocation – the particular theoretical considerations which can be presumed to have been at work in the developments attested by the available evidence and the encompassing theories in whose light *these particular* considerations would appear so salient and forceful, both of which presupposes a grasp of the theory at issue also independently of its history.

0.2 Speculation

In fact, I take as the starting point for my discussion the controversy between Frege and Hilbert brief and largely one-sided, as it was, precisely because it raises interesting theoretical questions and problems to which the subsequent discussion will then suggest solutions and answers:

The first problem Frege's criticism of Hilbert raise concerns the meaning – and the precise sense of "meaning" at issue here – of assertions put forward within any theory, and especially within a mathematical theories.

A second problem concerns the justification of the theories themselves, criteria for their acceptability.

To the first problem I will eventually suggest an solution based in Davidson's work: The meaning of claims advanced within theories is determined by the decisions taken by researchers developing the theory in question about how to use the expressions which are primitive within the theory; according to this proposal, to *know* the meaning of theoretical statements is to know how to use the primitive terms, and nothing more—there are no entities which either are meanings of expressions, or are the correlates to expressions for such relations as denotation or satisfaction. To determine, and to understand, the meaning of theoretical terms and claims consists in, and is exhausted by, deciding on, and adopting, a particular way of conceiving of the world.

(As this sketch already indicates, I do not place much stock in the responses to these problems which are found in the correspondence between Hilbert and Frege, as well as those found in Frege's subsequent articles criticising Hilbert: Hilbert's views at the time, or, at any rate, Hilbert's expression of them in his only substantive letter to Frege, are underdeveloped, while Frege's own views seem to me deeply implausible and sterile, and their influence on subsequent philosophy has, in my opinion, been largely detrimental.)

To the second question I eventually suggest an answer based on Hilbert's unpublished reflections about the nature of mathematics. To justify the acceptability of theories one must ground them in cognitive abilities which have been recognised as fundamental to the constitution of human beings (at a certain point in time). It suffices, in other words, to show that human beings are capable of so-conceiving the world surrounding them as the theory in question projects; everything else is a matter of socio-political mediation about how best to conceive of things in light of the practical exigencies facing a society and its individuals at a given point in its history.

Of course, I claim no originality for either proposal because, if I am right with where whatever originality there

is in this dissertation lies concentrated, these were not least of all Hilbert's views.

I, in any case, develop both of these proposals out of a thoroughgoing discussion of (unpublished) reflections and lectures by Hilbert. These proposals thus constitute, at the same time, examples of how historical study can educate the theoretical mind by bringing into focus considerations which would not appear, or not as prominently, from the present day's point of view; or even by putting some things into an entirely new perspective.

This is how I have come to see the work of philosophy generally: as offering a vision, articulating a way of seeing things, and, at its best, of seeing things so that new paths lie suddenly close by and open for reflection and action. It is, in my opinion, one of the great disappointments of recent philosophical writing that much of it pursues no apparent visionary ambition, and, if I may add, one of its great deceptions to think that the order of exposition does not matter: Since it lies in the nature of writing that one cannot say everything at once, choices must be made about where to start and where to go on, and not all these choices are equally fortuitous[3].

I can only hope that I may have succeeded in articulating such a vision to make you see (some aspects of) Hilbert's work, or logic, differently and to suggest new projects and approaches to the matter and its issues.

Fundamental to Hilbert's position, as I have come to see it, is the conviction that the question of the applicability of a given theory, while certainly of great importance, is separate from the question about the meaning of the theorems, and should not be prejudged by it. This conviction fundamentally distinguishes Hilbert from Frege: According to Frege's view, theories which turn out to be false, and thus not to be applicable, have no value. That seems to me a reason to reject Frege's view: Even in cases where those theories eventually turn out to be wrong, one has learned something – chiefly about deductive dependencies, about how knowledge can be systematised: connected and classified – from developing them. That is, at any rate, what I think Hilbert thought, and how I, myself, am presently inclined to think about these questions.

[3] As Walter Benjamin. *Ursprung des deutschen Trauerspiels*. In: *Gesammelte Schriften I.1*. Suhrkamp, 1974, pp. 203–430, p. 209 observed: "[I]t is peculiar to writing that with every sentence it has to pause and begin anew".

0.3 Theses and Claims articulated and defended in this Dissertation

0.3.1 About Frege

- Frege's critique of Hilbert has force only if a core tenet of Frege's conception of definitions is accepted, namely, that definitions assign meaning to the expressions they define.

  Arguably, Frege's critique of Hilbert also depends on accepting an assumption Frege makes about meaning, namely, that «the meaning» of any given expression (however, exactly, it is conceived) must determine a unique extension to be associated with that expression.

  Frege's critique of Hilbert is thus not based on neutral ground.

- Frege's conception of "historical elucidation" as an indispensable foundation for the assignment of meaning to expressions was developed in response to a dilemma raised by Korselt, and thus constitutes a concession Frege was forced to make, not an integral part of Frege's conception of meaning as expounded in *Über Sinn und Bedeutung* or the *Grundgesetze der Arithmetik*.

0.3.2 About Hilbert

- The quotation from Kant Hilbert chose as the epigram for his *Grundlagen der Geometrie* can be taken seriously as an invocation of Kantian doctrine: The primitives of an axiomatic theory are like Kantian ideas in two crucial aspects: (i) No objects having just those properties the axioms describe are (ever) given and (ii) the primitives serve primarily the purpose of systematisation.

  Systematisation in this sense was the overarching goal of Hilbert's foundational investigations.

- Hilbert's notebooks document furthermore that Hilbert's engagement with Kant extends far beyond Hilbert's occasional explicit references – Kantian ideas underlie, not least of all, Hilbert's conviction that every mathematical problem is solvable and Hilbert's conception of mathematics in general as a science of "pure thinking" – and can be traced back to Hilbert's student days, especially to Hilbert's (probable) teacher Günther Thiele.

- The logical theory Hilbert expounded in his 1905 lecture takes inspiration from Ernst Schröder's early *Operationskreis des Logikkalkuls* (1877), not from Schröder's later *Vorlesungen über Algebra der Logik*.

  During the first half of the 1910s, Hilbert developed an axiomatisation of the natural numbers (by studying, Peano and probably also Schröder's *Lehrbuch der Arithmetik*.

  Schröder's influence on technical aspects of Hilbert's foundational work thus runs deep.

- (Hilbert's conception of) completeness as a meta-theoretic property did not develop out of Hilbert's axiom of completeness, but may be connected to his conviction that all mathematical problems are in principle solvable.

  Behmann's decision problem has no roots in Hilbert's earlier work (in particular, Hilbert's conviction that all mathematical problems are solvable does not prefigure it). It is Behmann's invention (although Behmann acknowledges Schröder's influence on his thinking about the decision problem).

0.3.3 About Philosophy of Logic

- As a matter of historical fact, Ernst Schröder *is* what Frege has falsely taken to be: the pivotal figure who shaped the development of mathematical logic in its present day form.

- The word "formal", as used in phrases like "formal logic" or "formal language", if it means anything, means being subject to a precisely defined concept of deducibility.

- The view that theories describe a particular reality (held by Frege, among others) faces a regress when it comes to determining that reality (for which it requires the terms of another theory already picking out that intended reality) and is therefore not viable. Ambiguity cannot be excluded ultimately; it is inevitable at some level. Recognising this (as Hilbert did) leads to a different view of theories which does not see their purpose or their value in describing a particular reality, but – relatively – in the cognitive complexity of their development and – absolutely – in articulating in detail *a* way of advantageously engaging with the world.

0.4 Debts and Gratitudes

My supervisors at McGill were Michael Hallett and Dirk Schlimm, both of whom read large sections of this text in preliminary versions: Michael read drafts of the entire thesis prior to its initial submission and left many long and engaging comments throughout. Dirk read drafts of everything up until what is now § 155 prior to the initial submission and made useful suggestions about how to clarify the structure of the exposition. Both Dirk & Michael pointed out to me heaps of typographical and orthographical errors, and stylistic oddities in the drafts, many of which I unfortunately subsequently replaced in a subsequent revision by new oddities and errors. Both also dealt for the years that it took to finish this work with my penchant for writing long e-mails, and with my arrogance, patiently; Michael especially found himself on the receiving end of many e-mails asking "Have you seen this?" or "Have you ever thought about that?", almost all overly long, and unfailingly handled them with kind indulgence.

For most of my time at McGill I worked as a Teaching Assistant to Michael, which afforded many opportunities for conversations after classes, often enough eventually about Hilbert or whatever else was oozing through my mind at the time. Looking back, I can say that I came to work on Hilbert – after arriving at McGill with the prospect of working on medieval philosophy or Aristotle – because of Michael, and I came to read Schröder for the first time in the wake of an enjoyable class on Boole taught by Dirk in the Fall Term of 2017; and to beat some half-thought thoughts into presentable shapes by preparing a handful of presentations for informal meetings he organised.

This dissertation would not have come to be without the kind and indulgent assistance of Bärbel Mund of the Abteilung für Handschriften und Seltene Drucke of the Staats- und Universitätsbibliothek (SUB) Göttingen, who answered many questions about Hilbert's papers, put forward, more often than not, in exceedingly lengthy e-mails, and helped me organise several visists to Göttingen in the summer and fall of 2021. It will be obvious to anyone who reads my discussion of Hilbert that it could not have been written without the access to Hilbert's papers Bärbel Mund afforded me; nor could the text of the dissertation be what it is without the permission kindly granted by the SUB to quote, at times extensively, from unpublished materials among Hilbert's papers.

I am also grateful to Philipp Kastendiek (librarian of the Mathematisches Institut) for granting me access to unpublished sets of notes for several of Hilbert's lecture courses[4].

I would also gratefully like to acknowledge the help of Emma C. Moore, Librarian for Mathematics-Natural Sciences, and Marcia Tucker, Librarian for Historical Studies-Social Sciences, at the Institute for Advanced Study (IAS), Princeton NJ for locating the set of notes held by the IAS recording Hilbert's 1914/15 lecture course entitled "Probleme und Prinzipienfragen der Mathematik" and providing me (and the world[5]) with a digitisation of it, and for permission to quote in full selections from the manuscript.

[4] All the unpublished lecture courses I discuss are tentatively scheduled for inclusion in the second volume of the edition of Hilbert's lectures on the foundations of mathematics and physics (edited by William Ewald, Michael Hallett, Ulrich Majer and Wilfried Sieg) published by Springer. For this reason no material from these notes is reproduced here, but page references are given to identify the relevant passages in the forthcoming edition.

[5] The digitisation is freely available through the IAS' website: https://hdl.handle.net/20.500.12111/7950 (checked 11/5/2022).

Finally, I am grateful to the members of the Handschriftenabteilung of the Staatsbibliothek zu Berlin – Preußischer Kulturbesitz for their helpful assistance in reading through most of Heinrich Behmann's papers (Nachlass 335), and to Ralf Breslau for kindly granting me permission to publish some of Behmann's correspondence and the manuscripts of two lectures. In connection with Behmann I would also like to thank Christian Thiel for patiently answering a handful of questions I had about Behmann's papers and their history.

0.4.1 Personal

Finally, but in another way first and foremost, members of my extended family created the circumstances – both during the time I was actively working on this dissertation, and before – which made the completion of this work possible, as much as its beginning, and the beginning of my pursuit to what would eventually become the research into the questions and issues here discussed in the way here trod.

When I was a child, my (great) aunt would ask me whether I wanted to grow up to become a safe-cracker or a Nobel prize winner, nudging me very obviously towards the latter alternative. Her husband, who was by nature and habit paradigmatically and deeply a university professor (for classical archaeology), was with his impressive presence another model. My great-grandmother kept the books and specialist publications of her children & children's children (and her children's spouses) on a shelf next to where I would almost always sit when the family came together for some larger gathering and though I did not, then, know what a dissertation was – and not until recently have I really understood what it means to produce one –, these spines of books decorated with names whose bearers sat, at different times, around the same table, made me phantasise about adding to that shelf such an item with my name on the spine in golden or more sober lettering. So, as I grew older and eventually old enough to understand what these bound piles of pages were, it was the idea of pursuing academic work at least up to the point where I had such a pile of pages to be bound and placed there that lodged itself in my mind.

Since neither my great-grandmother, nor my (great) uncle, have – as is the way of the world – lived to see this work completed, I will let this story stand as a dedication to their memory, and an acknowledgement that without their presence in my life as it took shape, I would not have come to write a dissertation.

Besides inspiration, role-models and ideals, members of my family gave me books and money (which I used, for the most part, to buy books) and places to live, rent-free, which afforded me the luxury of being largely free of external pressures to earn my keep and sustenance and let myself float uninterrupted down the stream of reflection, subject only to the pressures and pitfalls of ideas. This support, and the unquestioning patience with which it was continuously given, was invaluable in allowing me to make this work everything I thought it should be.

To the extent that I have succeeded to do good work, I have done so with at best haphazard support from the institutions – at all kinds of levels – in whose sphere this dissertation officially was produced, with the exception of the departmental administrators, above all Angela Fotopoulos and Judy Dear; and Andrew Stoten.

I am grateful for financial support in the form of a McCall MacBain Fellowship, a Dow Hickson Fellowship, a Kunz Fellowship. Michael Hallett supported my archival research generously with means provided by the SSHRC.

For getting me through these years, which have been filled with an unhappiness I can think to describe only as "spectacular", I am grateful to a number of persons and places, of which I want to expressly acknowledge a few here for moments of happiness: *Blue Skies Turn Black* for the exceptional light nights blurry with good music, the MUTEK Festival 2018 & 2019, the Cinéma Du Parc for its consistently good programming and Cheskie's Bakery.

For books: Encore Books, The Word, S.W. Welch's.

For company & conversation: Léa Derome, Nick A. Aubin, Hakan Genç, Jeanne Allard, David Collins, André Martin, Emily R. Douglas, Aline Medeiros Ramos, Nik & Robin & Willow the cat (especially for the annual thanksgiving feasts), Azadeh Radbooei, Sebastian Rodriguez Duque (for running a little fiction-writing-group for a while, and to David, Nik and Thom for participating), Matthew Scarfone, Renxiang Liu (for running a reading group on Heidegger's Kant-book) and towards the end Kristen Vanderwee and the Graduate Student Running Group from 2017-2019 (organised successively by Sumin, Rob and Oscar); for sincere sympathy: Marguerite Deslauriers; for refuge: Nick & Thom (in Montréal before I moved here in 2016), Paul (in London, UK in 2017), Parker & the whole Aubin family (in Boston & Connecticut in 2017), William (in Queens in 2017), Dave (and his housemates in Vancouver in 2019), Alice & Michel (on Bowen Island in 2019), Manish (in Toronto in 2016 and 2021); the students I taught along the way, among them Johnny, Theo, Pénélope, Ayo, Eli, Mingjuan, Alex.

For friendship, above all: Rob Seddon.

0.4.2 Academic

In the style of the curricula vitae traditionally attached to dissertation in Germany, I want to briefly acknowledge the academic teachers who shaped my perception and conception of academic work generally, philosophy specifically and the questions and issues here treated in particular. I will mention formative figures chronologically.

In my middle-school days, Karl Grabner encouraged what struck him as a talent I had for mathematics, and Sabine Wallner encouraged me to read books and watch films in English. There is no doubt in my mind now that without Matthias Kittel – whose example, encouragement, and support in facilitating my participation in a summer class shaped my interests and character –, Gerda Vogt – who encouraged me and a classmate to participate in a philosophical essay competition, through which I met most of the people whose friendships would shape my later adolescence into the form that then transformed itself into my present cast of mind – and Georg Gsenger – whose lessons in biology still remain a model of clarity and seriousness I remember well and fondly – I would not have become who have turned out to be. As Herbart says, it is the teacher's impossible task to help the child become better and more definitively who they would eventually turn out to be, and this is what, it seems to me, they did.

Though not a teacher, but (merely) my senior by three years in high-school, I have to mention Marlene Deibl.

Without the ties that bound us then, my life would surely have taken a different course; though, then again, I am surprised –perhaps we both were – at the extent to which my life has taken its own course, regardless.

At university, it was Hans-Jürgen Scheuer – professor for medieval and early modern German literature – whose cohesive and visionary lectures made the first deep impression on me, along with the self-proclaimed systematic exposition by Volker Gerhard of the philosophy of Kant (and Nietzsche), as he saw it, and the sweeping yet incredibly detailed and well-informed lectures on the history of the idea of dialectics in German philosophy after Kant delivered by Andreas Arndt, all of which I encountered in my first year at university.

Markus Asper – professor for ancient Greek language and literature – presented me, around the same time, with a model of a kind of academic and teacher I could admire: jocular, opinionated and suffused with the conviction that no academic matter should ever be taken «dead» seriously; but that one should still always take anything as seriously as any situation requires, and be prepared – both by one's effortless knowledge of the sources, and in one's willingness to engage in discussion – to do so. (I am also taking a proverbial page out of his book by quoting a line from a Pinegrove song as an epigram, like Markus Asper quoted Iggy Pop in his *Griechische Wissenschaftstexte*.)

Probably the most formative impact on my academic development was made by Stephen Menn, whose concentration and command of his own mind and of an incredibly wide and varied array of questions, answers and sources still strikes me now as being so without compare as it did then. Although I cannot claim to have his knowledge or expository talent, or, for that matter, his conviction, it seems to me obvious on the face of it how I have (unconsciously) assimilated my overall approach, my writing style, and even, in certain respects, my persona to Stephen's example, as it struck me first in the fall of 2012: To engage with history in full detail, and to figure out what problems the figures I discuss were trying to solve (and how) by seeing what circumstances and influences shaped their approach to the matter at hand, were principles I first learned from Stephen's shining example.

To be mindful – always – of details of a somewhat different kind was also one of several lessons – both academic and personal – I learned, over the years since I first encountered him, from Karl-Georg Niebergall, who taught me by various examples that without having dissolving a matter into its minutiae and then reconstituting it bit by bit one can, and should, never be sure about anything; and, as he once remarked towards then of a course on Gödel's Theorems with a frankness that greatly impressed me: perhaps all academic work involved exhibiting "more self-confidence than anyone should have in themselves". (Not to mention that all I know about the technical side of mathematical logic I have learned, or learned the skill to teach myself, from Karl-Georg Niebergall.)

I have taken to heart two remarks made by Tobias Rosefeldt: It was his statement of his consternation that when he asked certain scholars of, in the particular instance that struck me, Fichte whether they personally subscribed to whatever position they ascribed to Fichte, these scholar seemed not to understand what he was asking of them, which shook me out of my early excessively and hermetically historical preoccupation and marked – so it seems to me in retrospect –, the start of my, as it were, allowing myself to develop more or less distinctive views of my own

on the theoretical cogency of certain solutions I had previously been content to merely expound from a historical point of view; and it was, some time later, his comment on a class-paper of mine (of which he noted that the various point I made somehow failed to coalesce into an overarching whole) which made me aware of the real difficulty facing the adequate expression of ideas and – though I would expect that this was not his intention – planted the seeds of what has now grown into my belief that philosophy must, ultimately, be visionary.

When I met Michael Hallett, my mind was largely made up, yet he succeeded in remaking it by impressing on me his patient kindness and his tendency to emphasise the big ideas underlying (or overarching) the technical details I prefer(red) to get lost in: So I slowly learned to keep a bigger picture in mind even when dealing with minutiae, but have surely failed to do so with the same admirably balanced blend of caution and conviction that pervades his work. It was Michael's sympathetic and thoughtful engagement, patience and tolerance for my arrogance and outright insolence along the way that let the fire of my certainty burn finally away into space to leave this ember.

I have learned a lot about logic during the years I was (supposed to be) working on this dissertation from organising and preparing three reading groups – on Volker Halbach's *Axiomatic Theories of Truth* (Winter Term 2018), on Martin Davis' *Computability and Unsolvability* (Fall Term 2018) and on Dag Prawitz' *Natural Deduction* (Winter Term 2019) (followed, in the summer of 2020, by a reading group focused on Prawitz' philosophical articles, and the work of Per Martin-Löf). The original impulse for the first of these reading groups came from Dave Rattray (and David Milton), to whom my thanks also for this, and to the various reading-groups' participants, especially the regulars: Jonny Palucci, Pénélope de Turenne, Julien Ouellette-Michaud, Austen Friesacher, Mathieu Soucy!

Of those in whose writings I have found inspiration at the right moment, but whom I have never had personal interactions with, I will mention, first, some of those who have had the greatest impact to date on my overall outlook: Davidson, Quine, Dewey and Hermann Cohen, and, second, some of those whose work has shaped my perception of the subject matter here at issue: Carnap, Skolem, Paolo Mancosu, Jamie Tappenden, Volker Peckhaus.

Now I am exhausted. Montréal, QC/Berlin, GER: May 13, 2022[6]

0.5 Formalities

All translations from material originally written in languages other than English are my own.

I translate passages primarily according to what I think they mean, and, in doing so, take certain liberties – within what I see as the bounds of grammar and pragmatics, of course – to clarify the meaning of a given passage

[6]This dissertation was initially submitted for evaluation on March 2nd 2022 in Montréal. The oral defence took place on April 28th. Jamie Tappenden and Stephen Menn served as the external and internal examiner respectively. To both I offer my sincere thanks for investing their time into reading my work and my gratitude for their encouragements. Stephen Menn's detailed examiner's report led to a number of revisions – all, no doubt, improvements – in this final version of the dissertation. Dirk Schlimm also read through the entire dissertation in the version submitted for examination and shared his notes with me after the defence.

Errors remain, no doubt, both orthographical and (though hopefully fewer) errors of substance, and they are my fault. I can only hope, in the spirit of the quotation from E.H. Moore I placed at the outset of this dissertation, that more often than not I have conveyed my ideas well enough for somebody interested in the matter to understand and, where they are worth anything, develop them further.

where the original formulation is not as clear as my translation ends up being.

To hold myself accountable to you, every passage translated is included in its original language in full; so are supporting passages; the latter are included only in their original language.

In order not to overload the footnotes with source-material, all quotations which only provide further documentation – whether they provide the original text for a quotation which appears translated, excerpted, or merely paraphrased in the main text – have been relegated to the endnotes; where I want to provide additional corroboration of claims made in the text, or in footnotes, in addition to the main considerations I rely on, they are also given in endnotes. (The endnotes also contain occasional philological discussion, where such issues are raised by the passage quoted.) Endnotes are marked by superscripted lower-case Roman numerals in the text, and are therefore clearly distinguished from footnotes, which are marked by superscripted Arabic numerals.

My intention with this «division of labour» between footnotes and endnotes is to keep the main text surveyable. The endnotes can be consulted where and when you feel you need further evidence for what I am claiming, while they can be skipped where- and whenever you feel like taking my word for it. The main text has been written in such a way as to be intelligible without reading any of the endnotes and so as to nowhere depend on anything said *only* in endnotes (except for claims to the effect *that* certain endnotes contain relevant corroborating evidence).

Page-breaks in the edition from which the text is cited are marked with a horizontal stroke ("|"); occasionally, page breaks in the original pagination of the text – if the first edition is not the edition cited – are marked by putting the page-number of the *following* page in square brackets (e.g. "[42]").

Ellipses in quotations between square brackets ("[…]") indicate the omission of parts of the text being quoted; ellipses between round parentheses or dashes ("(…)" or "– …–") indicate the omission of text marked as parenthetical *in* the source-text by its author. Where such an omission-mark is followed by a period, comma or semicolon, this serves to indicate that I have only omitted the remainder of a sentence or clause; where such an omission-mark is followed by a mark signalling a page-break this is *not* generally to be construed as indicating that the quote continues *immediately* after the page-break, only that I continue quoting *somewhere* after the page-break.

Different quotation marks are used for different purposes. Double (upper) quotation marks are used for verbal quotations of text in English, or any other spoken or written language; single (upper) quotation marks are used to mention expressions of artificial languages. Guillemets are used as scare-quotes to indicate that I do not want to endorse the phrase enclosed by them in its literal sense, but rather accept it only as suggestive, or as fitting and at the same time misleading (what is intended will hopefully be clear in each particular case).

Contents

Introduction **i**

0.1 Organisation . . . i

0.2 Speculation . . . iv

0.3 Theses and Claims articulated and defended in this Dissertation . . . vi

0.3.1 About Frege . . . vi

0.3.2 About Hilbert . . . vi

0.3.3 About Philosophy of Logic . . . vi

0.4 Debts and Gratitudes . . . vii

0.4.1 Personal . . . viii

0.4.2 Academic . . . ix

0.5 Formalities . . . xi

1 Frege vis-à-vis Hilbert **1**

1.1 Frege's Criticism of Hilbert . . . 3

1.1.1 Frege's Account of Definitions . . . 3

1.1.2 Frege Criticism of Hilbert's Definitions . . . 7

1.2 Frege's Motives . . . 13

1.2.1 First Motivation: Discrediting Rivals . . . 14

1.2.2 Ultimate Motivation: «Uncontroversial» Grounding for Theories . . . 19

1.2.3 Last Words and Further Projects . . . 21

2 Korselt's Work **24**

2.1 Korselt himself . . . 24

2.2 Korselt's Negative Contributions . . . 30

2.2.1 Korselt's Critique of Frege's Principle of Completeness . . . 31

2.2.2 Korselt's Critique of Frege's Conception of Meaning . . . 32

2.3 Korselt's Positive Contribution . . . 40

Summary of chapters 1 and 2 **43**

3 Hilbert's Philosophical Position **45**

3.1 The Foundations of Geometry . . . 45

3.1.1 The Textual Context of the Epigraph . . . 46

3.1.2 The Historical Circumstances of Hilbert's Book . . . 52

3.1.3 The Contours of Hilbert's Philosophical Position . . . 63

3.1.4 Hilbert's Early Lectures . . . 65

3.2 Hilbert's lecture courses between 1904-1915 . . . 69

3.2.1 Hilbert's Early Logical Theory . . . 74

3.2.2 Hilbert's Reworking of Arithmetic . . . 89

3.2.3 Principles, Fundaments and Foundations . . . 96

3.3 Hilbert's Scientific Diary (and other Unpublished Sources) . . . 112

3.3.1 Dating the Notebooks and their Content . . . 113

3.3.2 Frege and Meaning . . . 116

3.3.3 Clebsch and the Essence of Mathematics . . . 120

3.3.4 Kronecker and Decidability . . . 126

3.3.5 Kant and the Promise of Knowledge . . . 135

Summary of the Discussion so far . . . 148

3.4 Developments leading into the 1920s . . . 152

3.4.1 Hilbert's reception of Schröder and Russell leading up to 1917 . . . 153

3.4.2 The evolution of Hilbert's logical theory . . . 158

3.4.3 A potential root of finitism . . . 165

Endnotes **168**

Bibliography **243**

Editorial Appendices **244**

A Material on Korselt's Later Work **244**

A.1 Bernay's initial letter to Behmann (dated Nov. 16 1937) . . . 247

A.2 Behmann's letters to Blumenthal . . . 248

A.2.1 Behmann's Report . . . 248

A.2.2 Blumenthal's Response to Behmann (dated March 4 1937) . . . 253
A.2.3 Behmann's Response to Blumenthal (dated March 8 1937) . . . 253
A.3 Behmann's Letters to Bernays . . . 256
A.3.1 Behmann's Response to Bernays . . . 256
A.3.2 Bernays' Response (dated April 13 1937) . . . 257
A.3.3 Bernays on Unifying Notation . . . 258

B The Convolute Cod. Ms. D. Hilbert 559 **261**
B.1 Baule-Ausarbeitung. . . . 262
B.1.1 Baule's Second Version of the Axioms of Whole Numbers . . . 278
B.1.2 Hilbert's presentation of Peano's axioms . . . 287
B.2 Hilbert's Preparatory Notes for the 1913 Lecture-Course . . . 288
B.3 Hilbert's Preparatory Notes for the 1914/15 Lecture-Course . . . 292

C Cod. Ms. Hilbert 606 **298**

D Behmann's Presentations on Russell **318**
D.1 Über mathematische Logik (1914) . . . 318
D.2 Die Russellsche Theorie der logischen Typen (1927) . . . 326

Analytical Table of Contents

Chapter 1: Frege 1

I offer reconstructions of two of Frege's objections to Hilbert's axiomatic approach to geometry, which Frege put forward both in correspondence with Hilbert and subsequently in several articles publicly criticising Hilbert. I go on to discuss Frege's motivation for accepting the presuppositions on which those arguments depend. That Frege's arguments depend on accepting assumptions Hilbert did not share, explains why Hilbert did not feel compelled by Frege's criticisms to make substantive changes to his *Grundlagen der Geometrie*, as Frege urged him to do.

§ 1 The Frege-Hilbert correspondence; Frege's articles as based on it; Frege's (lost) manuscripts.
§ 2 No public response from Hilbert; subsequent changes unlikely to be due to Frege's criticism.
§ 3 The need to clarify which methodological principles were at the heart of Frege's critique; outline of the chapter.

1.1 Frege's Critcism of Hilbert 3

I discuss two of Frege's arguments against Hilbert in detail: that being an axiom is incompatible with being part of a definition, and that Hilbert's axioms cannot be used to decide which particular objects are, as a matter of fact, objects of geometry. Analysing the arguments reveals that they are damaging to Hilbert's position only if one accepts, as Frege did (and Hilbert apparently did not), certain assumptions about definitions and about meaning.

§ 4 Frege objects mainly to Hilbert's conception of definitions.

1.1.1 Frege's Account of Definitions 3

Frege discusses definitions in different places. There are differences between these expositions and it is not obvious that they can be harmonised. Ultimately, three assumptions can be identified which are central to the version of Frege's account of definitions underlying his critique of Hilbert: (I) Definitions assign meaning to the expressions they define, (II) definitions must never lead to ambiguity by assigning more than one meaning to the same expression, and (III) the kind of meaning assigned by definitions effectively associates a unique extension with the expression defined.

§ 5 Frege's twofold interest in definitions: (a) general criteria for all definitions; (b) favouring specific definitions over (also acceptable) alternative definitions of particular concepts. I am interested in (a) only.
§ 6 Source-texts for Frege's views on definition; the question of their relation.
§ 7 Frege's views on definitions in 1903: Definitions confer a unique meaning (principle of meaningfulness).
§ 8 Two differences between Frege's conception of definition between the first and the second volume of the *Grundgesetze der Arithmetik*: whether the purpose of definitions is assigning meaning or exhaustive classification; whether definitions are given primarily of expressions or of concepts.
§ 9 Discrediting rival accounts as a motivation of Frege's own purportedly obvious adequacy criteria.

1.1.2 Frege Criticism of Hilbert's Definitions 7

Frege puts forward a tangle of arguments in his criticism of Hilbert. I extract two distinct arguments, which I call "the argument from incompatibility of axioms and definitions" and "the argument from inapplicability" respectively. When one assumes that definitions must effectively associate a unique extension with the expressions they define, these arguments together constitute a dilemma reducing to absurdity Hilbert's claim to give definitions of his primitives. However, because these assumptions are peculiar to Frege, there is no problem from Hilbert's point of view.

§ 10 Frege's conception of axioms as determinately true sentences.
§ 11 Frege accepted Hilbert's axioms in substance (but disputed Hilbert's methodological claims about them); Frege's lost manuscripts on geometry.
§ 12 Frege found some of Hilbert's definitions perfectly acceptable.
§ 13 The original fault Frege finds with Hilbert's *Grundlagen der Geometrie*.
§ 14 From Frege's point of view, being an axiom is incompatible with being a component part of a defining expression. Frege goes back and forth between two versions of this argument from incompatibility.
§ 15 Two presuppositions of Frege's argument from incompatibility.
§ 16 Frege's argument from incompatibility is neutral with respect to the specific conception of meaning one is inclined to accept. It depends rather on two assumptions about definitions: (1) that definition assign meaning; and (2) that definitions never assign multiple meanings (principle of univocity).
§ 17 Frege's "pocket watch" example reconstructed as a vicious regress argument.

§ 18 This argument Frege presupposes Frege's *particular* account of meaning.
§ 19 The two arguments taken together constitute a dilemma based on the alternative about axioms as truths: If axioms are determinately true, using them in defining expressions is incompatible with the nature of definitions (as Frege conceives of them); if axioms are not determinately true, the definitions relying on them fail to associate a unique extension with the expressions they define.
§ 20 The force of Frege's arguments against Hilbert rests on accepting a core principle of Frege's conception of definitions (that definitions assign meaning), as well as accepting one of Frege's particular assumptions about meaning (that it determines a unique extension).
§ 21 Rejecting any of these assumptions undermines Frege's criticism. Korselt's response to Frege (discussed in chapter 2) is ultimately based on rejecting Frege's assumption that meaning determine a unique extension.

1.2 Frege's Motives 13

I discuss Frege's motivations for accepting the assumptions about definitions and meaning Frege accepts. I suggest that Frege is motivated (at least to some extent) by a desire to discredit rival theoretical efforts by contemporaries.

§ 22 The question of Frege's reasons for accepting these assumptions; a rough paraphrase of their import: Theories must be developed along with a single intended interpretation; Frege's lost diaries.

1.2.1 Frege's Motives: Discrediting Rivals 14

§ 23 Discrediting rival accounts as a motivation for Frege's discussion in "Über Formale Arithmetik";
§ 24 and in the second volume of the *Grundgesetze der Arithmetik*;
§ 25 and in the first volume of the *Grundgesetze der Arithmetik*.
Schröder as the likely target of the criticism of creative definitions there.
§ 26 Frege's engagement with the first volume of Schröder's *Vorlesungen* between 1890 and 1895.

1.2.1.1: Schröder's Conception of Definitions 16

In his *Vorlesungen über Algebra der Logik* Ernst Schröder also presents definitions of the kind to which Frege objected in Hilbert's work on geometry. Frege thus has reason to object also to Schröder's definitions and does so in a review of Schröder's work. Looking in more detail at Schröder's underlying methodology, insofar as Schröder's occasional remarks shed light on it, reveals, however, that Frege is not accurately representing Schröder's position.

§ 27 Schröder's views on definitions must be reconstructed because he gives no consolidated account of definitions. For Schröder, definitions come with an indefeasible existence-presupposition.
§ 28 Schröder's methodology: Theories are formulated first, applied only afterwards.
§ 29 This was also Frege's approach in his *early* mathematical work on complex numbers.
§ 30 Interpreting Frege as motivated mainly by a desire to reject rival accounts just for the sake of it is unsatisfactory; this raises the question why else Frege would reject definitions without intended interpretations.
§ 31 Frege regarded Schröder's idea of "adjoinging" regions to the domain of an interpretation as a form of creation; this interpretation is not borne out by Schröder's own remarks.
§ 32 Frege also assimilates Schröder's position to formalism of a kind he would later sharply criticise, but, again, there is clear evidence that Schröder really held the view Frege ascribes to him.

1.2.2: Ultimate Motivation: «Uncontroversial» Grounding for Theories 19

Ultimately Frege appears to have been motivated by the conviction that theories must be «grounded» in uncontroversially existing objects, which the theories are taken to describe. This conviction explains why Frege criticises Hilbert and Schröder as he does, and why he accepts the conception of definitions and meaning he accepts.

§ 33 Frege's conviction that consistency could be established only by exhibiting a model as a constant throughout his career, and the root of his assumptions about definitions.
§ 34 Truth comes only from grounding in «uncontroversially existing» objects;
such are the objects given in intuition as the root principle of Frege's convictions, and as their anchor in the common opinion of the time (as exemplified also by Hankel).
§ 35 Frege eventually accepted "logical objects" as «uncontroversially existing», but remained committed to the principle that theories needed to be grounded in such objects. This principle has roots in Kant.

1.2.3: Last Words and Further Projects 21

Summary of the discussion up to this point.

§ 36 Summary of chapter 1: The task was analysing Frege's critique of Hilbert to bring into focus the basic assumptions it rested on, and what motivated Frege to accept these assumptions.
Preview for chapter 2: The task will be to explore a way of rejecting one key assumption.
§ 37 The puzzle of reconciling the rejection of Frege's requirement that meaning must determine a unique extension with the principle of univocity. Seeking an answer in a «thinner» conception of meaning.
§ 38 Hilbert's views on definition (sketched in his scientific diary) as syntactic abbreviations.

Chapter 2: Korselt 24

In 1906 and again in 1908 Alwin Korselt published two rejoinders to Frege's critique of Hilbert, who never responded publicly to Frege's critique. After a brief discussion of Korselt's life and work, I discuss his response to Frege. Korselt's response to Frege is based on a different understanding of what the value of axiom systems consists in and on a conception of meaning different from Frege's. In response to Frege's arguments Korselt exposes a genuine weakness of Frege's account, to which Frege appears incapable of giving a satisfactory response. Korselt also offers a reconstruction of Hilbert's axiomatic geometry based on Bolzano's theory of consequence.

§ 39 Hilbert did not articulate his views on definitions in any detail at the time, nor did he respond publicly to Frege's criticisms; Alwin Korselt defended Hilbert's position and engaged with Frege instead.

2.1 Korselt himself 24

I summarise the facts known about Korselt's life and work, and outline the basic assumptions on which Korselt's position, as I see it, rests: Korselt believed that theories have whatever value they have on account of the complexity of the deductive "webs" among the results they comprise; and that meaning was a matter of knowing when and how to use an expression.

§ 40 Korselt's engagement with Frege; Frege's (lost) second response.
§ 41 Korselt's life; his (lost) late manuscript on the algebra of logic.
§ 42 Overview of the chapter: Korselt thought theories should not be limited to describing a single «intended» interpretation; Korselt's criticisms exposed one genuine flaw of Frege's views.
§ 43 Korselt sees Frege's views as based on a particular conception of the value of theories, according to which the value theories have is due due to the portion of reality they describe.
§ 44 Frege does, indeed, hold such a view.
§ 45 Korselt, by contrast, sees the value of theories in the systematisation of knowledge by means of deductive relations, not in the description of any reality.
§ 46 The value of theories correlates with the complexity of deductions in them; even deductions within inconsistent theories can be of value on account of the complexity of the argument.
§ 47 Frege conceded that developing theories requires effort, but saw the source of their value elsewhere.
§ 48 Frege may have later changed his mind about the central role truth played in assigning value to theories; Frege's lost correspondence with Löwenheim.
§ 49 Korselt objected that Frege's position was not feasible in principle.

2.2: Korselt's Negative Contribution 30

Korselt rejected Frege's principle that definitions have to apply exhaustively to every object as idle, and Frege's conception of meaning as setting up a standard for meaningfulness which is impossible to fulfil. The latter objection to Frege's conception of meaningfulness can be recast in the form of a challenge for Frege to exhibit a process by which undefined expressions can be associated with a unique extension. Frege points to what he calls "elucidation" as a candidate for such a process but concedes that elucidation does not serve this purpose unfailingly.

§ 50 Korselt proposes to conceive of the meaning of a sentence as the class of sentences sufficient for deciding the truth of a sentence, and conceive of the meaning of sub-sentential expressions as the class of sentences sufficient for deciding the truth of certain sentences in which these expressions are used.
§ 51 Korselt's intentions are not always clear, but ultimately he seems to intend to articulate an alternative account of meaning, rather than reject the connection between definitions and the assignment of meaning.
§ 52 Korselt rejected Frege's principle of completeness for definitions as superfluous in light of the principle of explicit information (also accepted by Frege), i.e. that all assumptions used must be explicitly stated.
§ 53 This objection is not very damaging; but Korselt raises further, more serious problems, which, when taken together, constitute a dilemma threatening Frege's assumptions about meaning and definitions.

§ 54 Korselt's first argument: Axioms cannot fix extensions uniquely, because they only can only determine objects and classes in relation one to another.
§ 55 Axioms can only «relay» extension, not determine it absolutely.
§ 56 Definitions likewise can only «relay» extension, not determine it absolutely.
§ 57 Korselt's challenge to Frege: Exhibit a procedure which *can* determine a unique extension absolutely. Frege takes up the challenge in his response, but gives no satisfactory response.
§ 58 Frege's distinction between defined and undefined expressions does not address the problem.
§ 59 What Frege calls "elucidation" is meant to address the problem, but proves insufficient.
§ 60 Korselt's challenge may have contributed to Frege changing his mind (later in his life) about the possibility of associating a uniquely determined extension with any given expression.
§ 61 Others (i.a. Schönflies) have objected to Frege along the same lines as Korselt. This underscores the seriousness of the problem, and motivates a search for conceptions of meaning alternative to Frege's.
§ 62 Korselt's challenge could be met by assuming that elucidation eventually converges on a unique extension, but such an assumption runs contrary to how Frege conceives of elucidation: as essentially open-ended.
§ 63 Indeed, making such an assumption would arguably collapse Frege's position into Hilbert's.

2.3: Korselt's Positive Contribution 40

Korselt offers his own reconstruction of Hilbert's axiomatic approach to geometry.

§ 64 Korselt's 1908 article; his engagement with Bolzano.
§ 65 Korselt's reconstruction of Hilbert (and Frege) (on the basis of Bolzano). The difference between Korselt's and Frege's reconstruction; comparison with the later reconstruction in Hilbert and Bernays's *Grundlagen der Mathematik*.
§ 66 Korselt accepted Frege's conception of axioms, but inferred from it that there simply are no axioms (in that sense) anywhere in mathematics.
§ 67 Korselt says Hilbert's definitions by means of axioms define "relational concepts".
§ 68 Summary of chapters 1 and 2.

Chapter 3: Hilbert 45

Discussion of Hilbert's early foundational writings, in particular, his 1899 *Grundlagen der Geometrie* (section 3.1), his unpublished lectures (section 3.2) and his so called "scientific diary" (section 3.3). The aim of foundational work as Hilbert conceived of it lies in the systematisation of mathematical disciplines, that is, in discovering *exactly which and how* assumptions and results are connected. Isolating the core ideas of a discipline, formulating axiom systems to recapture those core ideas and studying the meta-theory of such axiom systems as well as comparing them with one another all serve the ultimate goal of systematisation. Hilbert's scientific diary furthermore suggests that Hilbert conceived of mathematics – along Kantian lines – as rooted in fundamental cognitive operations (and the systematicity the axiomatic method renders explicit is due to the systematicity of the human cognitive apparatus).

§ 69 Considering Hilbert's own work, beginning with his works on geometry.

3.1.1 The Epigraph of the *Grundlagen der Geometrie* 46

As the epigraph for his 1899 *Grundlagen der Geometrie*, Hilbert chose a quotation from Kant. I trace the quotation and discuss its meaning in its original context before discussing what whether Hilbert used it to signal acceptance of any Kantian doctrines, and, if so, which ones. I make two suggestions: (A) Kant's conception of ideas as means of systematising knowledge appealed to Hilbert, and (B) he may have regarded the primitives of his geometry as ideas in Kant's sense, i.e. as concepts to which no corresponding objects could be given. Regarding the primitives of geometry as ideas in Kant's sense, as Hilbert seems to do, was not unprecedented in Hilbert's time: Benno Erdmann had previously staked out such a view in his *Die Axiome der Geometrie*, which was known to Hilbert. To flesh out the Kantian perspective on geometry which may have been Hilbert's – while being different from Kant's own view – I discuss Erdmann's work in some detail.

§ 70 Whether by quoting Kant Hilbert means to signal affinity with (some) Kantian doctrine.
§ 71 The quotation Hilbert uses as his epigraph stems from a paragraph summarising Kant's discussion up until that point, i.e., in effect, summarising Kant's theory of knowledge.
§ 72 In Kant's theory, cognition and intuition are both indispensable for knowledge.
§ 73 Ideas can merely purport to grasp objects (because no intuition of the objects they purport to grasp is possible).

§ 74 The interpretation of Kant's conception of "ideas" Hilbert presents in "Über das Unendliche" (1925) is inaccurate.
§ 75 In Kant's theory, ideas serve the purpose of systematising knowledge.
§ 76 That is a point of contact: Hilbert's main aim is *systematising* geometry.
§ 77 The epigraph is meant to convey this ambition; Hilbert's priority is systematising geometry.
§ 78 Articulating a sense in which the primitives of an axiom system are ideas in Kant's sense: no object correspond to them exactly, they serve to systematise objects (according to which axioms the satisfy).
§ 79 Hilbert's use of models to prove the satisfiability of his axioms attests that he does not think the axiom system itself gives objects which are precisely such as it describes; – Hilbert's model Ω.
§ 80 Elements in the domain of models approximate but never coincide with objects as described purely by the axioms, the elements of models are, by comparison, overdetermined; parallel with Kant.
§ 81 The classification of appropriate structures as systematisation in Kant's sense.
§ 82 Three strata of objects related to an axiomatic system: the objects characterised by the axioms, the elements of the domains of various structures, and the «uncontroversially existing» basis out of which the elements in the domains of structures are constructed.
§ 83 Summary of the section;
There is historical precedent for construing the primitives of geometry as ideas in a Kantian sense.

3.1.2 The Historical Circumstances of Hilbert's Book 52

In his *Die Axiome der Geometre* Benno Erdmann aimed to present a philosophical theory of geometry which took duly into account recent advances both in the development of (non-Euclidean) geometry and in other branches of science relevant to the epistemology of mathematics. In Erdmann's view, geometry is based on concepts (rather than intuitions), and all concepts (including geometric concepts) are ideas because no intuition adequately corresponds to them.

§ 84 Benno Erdmann's book on the axioms of geometry; Hilbert's knowledge of it.
Hilbert's opinion of Erdmann's book remains unclear.
Helmholtz found it a competent philosophical discussion of the mathematical issues.

3.1.2.1: *Erdmann's Revisions of Kantian Doctrine*

One lesson to learn from considering Erdmann's book is that by Hilbert's time, the philosophical discussion had moved on from Kant's own views. Even quoting Kant, as Hilbert does for his epigraph, does not signal any allegiance with Kant's own doctrines so much as with some suitably «updated» Kantian position.

§ 85 In Hilbert's time, the philosophical discussion of mathematics had long moved on from Kant's own views; Erdmann's occasional open criticisms of Kant testify to this fact.
§ 86 Erdmann casts his disagreements with Kant as necessitated by a "changed situation" in the available evidence and the state of development of the sciences (especially the physiology and non-Euclidean geometries).
§ 87 Erdmann's own summary; his harmonising tendency; his re-interpretation of the a priori/a posteriori-distinction.
§ 88 By quoting Kant Hilbert subscribes to a «Kantian» position (different from Kant's own).
§ 89 Erdmann claims the primitives of geometry are ideas because nothing corresponds to them in experience;
the question of Erdmann's justification for his position.
§ 90 First argument: If geometry employs concepts at some stage of its development, then that conceptual content has to already be present also in its primitive basis.
§ 91 A slight generalisation of Erdmann's argument: Conceiving of proofs as logical deductions and of logic as somehow concerned with conceptual content entails that mathematics must be thoroughly conceptual.
§ 92 Second argument: Analytic geometry explicitly employs concepts. Granting that the analytic approach is viable implies that there is nothing to geometry which cannot be captured by (analytical) concepts.
§ 93 Erdmann's views are representative of Neo-Kantian philosophy more broadly; comparison with Cassirer.
§ 94 Erdmann's two arguments support the claim that concepts play a role in geometry;
that these concepts are ideas in Kant's sense still requires further argument.
§ 95 According to Erdmann's account, intuition is the result of the interaction of a psychological and a physiological component and represents neither the physical stimuli nor the cognitive structure accurately.
§ 96 In light of this conception of intuition, no geometric concept can have an intuition corresponding to it; nor can *any other concept*; – this view is widely held by Neo-Kantians and seems to go back to Herbart.
§ 97 Erdmann's view can be rephrased by saying: Experience is never as precise as (mathematical) concepts.
Klein expresses this view in an article; Hilbert cites this article with apparent approval.
§ 98 Hilbert subscribes to something like Erdmann's claim that geometry is entirely conceptual: Hilbert's speaks of "logical analysis of the faculty of intuition" in this sense in the introduction to the *Grundlagen der Geometrie*.

§ 99 Summary of the discussion: Hilbert quotes an epigraph mentioning ideas; I have argued that Hilbert may have seriously wanted to endorse certain features of Kant's conception of ideas – that no experience corresponds to them precisely; that they serve a systematising function – and apply them to his work.

3.1.3 The Contours of Hilbert's Philosophical Position 63

Ordering the propositions of geometry according to their deductive dependence was recognised as an open problem facing geometry in its current state by both Klein and Erdmann. Arguably, this is the problem Hilbert's foundational work in geometry was meant to address.

§ 100 Erdmann and Klein both explicitly outline a project of systematising geometry; Hilbert can be seen as taking on this project already articulated before him.
§ 101 Erdmann also offers a project-description of Hilbert's work: developing geometry systematically, i.e. in a way which exposes the (in-)dependence of certain results from certain assumptions.

3.1.4 Hilbert's Early Lectures 65

I survey Hilbert's lectures on geometry before (and shortly after) the publication of his *Grundlagen der Geometrie*. They offer some corroborating evidence, especially where Hilbert describes his overall aim.

§ 102 Hilbert's early lectures on geometry as sources of his philosophical views.
§ 103 Hilbert did not dedicate entire courses to geometry between 1902-1920, he kept developing his geometrical theory as part of other lecture-courses.
§ 104 Though the content and disposition of these lectures varies, the opening remarks constantly emphasise the task of clarifying the deductive structure of geometry; the metaphor of a "Fachwerk".
§ 105 Systematisation was not the only, but certainly a main component of Hilbert's foundational work.

3.2 Hilbert's Lecture-Courses between 1904-1915 69

Between 1902 and 1917 Hilbert continued to lecture, if not to publish much, on foundational issues. These lectures remain unpublished. I give an overview of the archival material and of the themes discussed in these lectures.

§ 106 Hilbert's lectures between 1902-1917; Hilbert's research and publications during that time.
§ 107 Overview of the manuscripts recording lectures (and a seminar) Hilbert taught during these years.
§ 108 The main themes in these lectures.
§ 109 The first impression that there is little to no substantive developments in these lectures is deceptive. Overview of the following discussion.

3.2.1: Hilbert's Early Logical Theory 74

The logical theory Hilbert presented in 1905 (and, substantively unchanged, in 1915) bears a striking resemblance to the logical theory presented by Schröder in his early *Operationskreis des Logikkalkuls* (1877), which Hilbert seems to have studied closely and modified when first presenting a logical theory. This comparison sheds much light on the relationship between Hilbert's work in logic and Schröder's, which has, until now, been the subject mostly of hasty conjectures. I also discuss potential reasons why Hilbert inverted "logical sum" and "logical product" (compared with Schröder's usage).

§ 110 Hilbert's logical theory does not change between 1905 and 1915.
§ 111 Hilbert's notation and terminology suggests that he at least initially reproduces a theory from the "algebra of logic" tradition; in principle, Schröder is the likeliest source for Hilbert. No evidence to think Frege's technical work had any decisive impact on Hilbert's.
§ 112 Comparison suggests that Hilbert had not read Schröder's *Vorlesungen über die Algebra der Logik* in 1905. Peirce's influence on Schröder's *Vorlesungen*.
§ 113 Reasons to doubt that Schröder's work was at all significant for the development of Hilbert's logical theory.
§ 114 These are outweighed by the similarity of Hilbert's logical theory to that presented in Schröder's *earlier* booklet "Der Operationskreis des Logikkalkuls" (1877). Detailed comparison.
§ 115 The mystery of Hilbert's failure to acknowledge Schröder's pioneering work.
§ 116 Hilbert's 1905 normal form theorem for sentential logic derives from Schröder (and Boole).
§ 117 The changes Hilbert makes to Schröder's notation.
§ 118 The change of notation for equality and negation is negligible.
§ 119 Hilbert's «inversion» of '0' and '1', '·' and '+'. First explanation: the analogy to the algebra of residue systems with two elements.

§ 120 Second explanation (due to Behmann): thinking about content, rather than extension.
§ 121 Hilbert's conception of proofs as "(logical) factors" of their results as a result of the inversion of '·' and '+'.
§ 122 In 1905 Hilbert does not conceive of proofs as having a significant internal structure.

3.2.2 Hilbert's Reworking of Arithmetic 89

In 1913 Hilbert dedicated himself to developing an axiom system for the theory of natural numbers. I outline Hilbert's efforts. Although Hilbert studied Peano at the time, Hilbert's axiom system bears no resemblance to Peano's; it does, however, have certain affinities with Schröder's exposition in the *Lehrbuch der Arithmetik und Algebra*. Becoming aware of these affinities renders them recognisable also in exposition of elementary number theory Hilbert gave as part of his lectures on proof theory in the 1920s. In particular, Hilbert's conception of number-signs and his fundamental assumptions about them seem to have been taken over from Schröder's *Lehrbuch*.

§ 123 Hilbert's development of an axiom system for the theory of the natural numbers in 1913.
§ 124 Hilbert first studies Peano's *Formulaire* (probably the second edition; perhaps in German translation) then.
§ 125 Hilbert's axiomatisation resembles Schröder's theory of the natural numbers in title, choice of primitives, and by taking one (not zero) to be the first natural number.
§ 126 Proposed axiomatisations of the natural numbers other than Schröder's – I briefly consider efforts by Huntington and Klein – differ from Hilbert's in at least one of these respects.
§ 128 Detailed comparison of Hilbert's axioms and propositions with Schröder's; all in all, the resemblance is not as striking as it is between Hilbert's logical theory and Schröder's, but still suggestive.
§ 127 Schöder offers an axiomatisation of the natural numbers in all but name.
§ 129 In studying Schröder's *Lehrbuch* Hilbert would have encountered certain key tenets of a formalist conception of the natural numbers; Hilbert rejects this position explicitly in 1913.
§ 130 Hilbert later adopts a kind of formalism, but never casts number-signs as the ultimate objects of arithmetic.
§ 131 Both Hilbert's conception of number signs in the 1920s and his condition that number-signs must always be recognisable bear striking resemblance to Schröder's discussion.
§ 132 HIlbert's "axiom of thinking" derives from Schröder (and Cantor).
§ 133 Summary of Hilbert's engagement with Schröder's (early) work.

3.2.3: Principles, Fundaments and Foundations 96

In 1910 Hilbert distinguishes two kinds of foundational projects: One can investigate the «propellant ideas» which are «central» to particular disciplines, or formulate precisely defined axiom-systems sufficient to reconstruct given disciplines. Hilbert speaks of a concern with "principles" to mean the former, while the latter is associated with the label "foundations". This distinction shows that Hilbert's conceived of foundational work as involving more than merely stating axiom-systems and investigating their meta-theoretic properties: To know which axioms to choose one first has to have a grasp of the principles of a discipline (and axiom systems guided in this way serve the systematisation of the discipline.

§ 134 The changing titles of Hilbert's lectures between 1905-1916 indicate that Hilbert is pursuing foundational projects of different kinds at different times, in different lectures (and a seminar).
§ 135 In his 1908 and 1910 lectures, Hilbert distinguishes the project of investigating "principles" (i.e. "core ideas") of mathematical disciplines form the project of investigating foundations.
§ 136 The 1908/9 seminar belongs to this project of investigating principles.
§ 137 Focusing on principles also serves a pedagogical purpose: to provide students with an overview of a given discipline in order to facilitate further foundational investigations.
§ 138 The relation of this project to Klein's project of expounding elementary mathematics from a higher standpoint: In later iterations, Hilbert refers to this lecture-course as an introduction to elementary mathematics.
§ 139 The project of a systematic exposition of the foundations of mathematics is repeatedly announced, but never carried out (and in the 20s renounced altogether for not being feasible).
§ 140 Three senses of "foundational work": (a) pinpointing core ideas ("principles"); (b) systematising disciplines by investigating deductive (in)dependencies; (c) rigorously developing a particular (axiomatic) theory.
Central to foundational work is a concern with deductive connections.
§ 141 The distinction between (a) and (c) suggests an explication of the contrast between "formal" and "informal" in terms of whether a precisely defined deductive system is employed.

3.2.3.1 Hilbert's early conception of completeness 101

When Hilbert speaks of "completeness" in his early work, he may have in mind as a standard the derivability of a few, known results regarded as «central» to the discipline in question. In this way Hilbert's concern with what he calls "principles" can be seen as contributing to the further development of the axiomatic method by sharpening Hilbert's standard of completeness.

§ 142 Hilbert speaks of completeness from very early one, but what he means remains unclear.
§ 143 Identifying central results provides a standard of completeness for a discipline.
§ 144 Hilbert does, in practice, seem to base his assertions of completeness on the derivation of central results.
§ 145 In 1913 (and later in 1917) Hilbert even suggests that every discipline has a *single* central result.
§ 146 "Axiomatisches Denken" is not a new beginning, but rather a compilation of Hilbert's efforts in the 1910s.
§ 147 Summary: Hilbert's foundational work begins with isolating principles, proceeds from there to proposing axiomatisations and finally to systematising the available axiom systems.

Interlude on Completeness and Maximality 105

I survey different uses of the word "complete" in contemporary logic and distinguish, in particular what I call "descriptive completeness" (i.e. having as theorems all the truths in a particular structure) from maximal-consistency. As construed in first-order model theory, descriptive completeness entails maximal-consistency. Implicit in this conception of completeness is a certain view of what precisely defined theories ideally (i.e. when complete) are for: They serve the purpose of exhausting all truths. Hilbert seems to have held a different view of the purpose of precisely defined theories as serving the purpose of exploring (not necessarily exhaustively) the deductive connections between certain results and certain assumptions. This view of the purpose of theories may seem inferior because it fails to give rise to a uniform criterion for completeness applicable to all theories in the same form, but, as an observation Davidson made about Tarski's definition of truth suggests, the uniformity of the model-theoretic definition of descriptive completeness is superficial only and covers up the substantive differences between what descriptive completeness requires in particular cases arising from which atomic sentences are true. Based on this observation and Davidson's comments on it, I observe that a theory of meaning along the lines of Davidson's theory can make sense of Hilbert's claim that axioms define (and give meaning to) the primitives of a deductive theory.

§ 148 Three senses of "complete": (i) deductive completeness (relative to an indefinite account of consequence), (ii) descriptive completeness (relative to an indefinite account of truth) and (iii) maximal-consistency.
§ 149 The model-theoretic conception of completeness presupposes that precisely defined theories should ideally (i.e. if complete) be exhaustive of all truths, not just known truths.
This assumption about the purpose of precisely defined theories differs from Hilbert's.
§ 150 Hilbert's later project of justifying mathematics by providing (finitist) consistency proofs arguably depends on a different conception of completeness than his earlier work.
§ 151 The assumption of an exhaustive standard is more in line with the decision problem as formulated in 1922 by Behmann; Behmann's problem has no roots in Hilbert's earlier work.
§ 152 Exhaustiveness is plausible as a standard for deductive completeness.
§ 153 Hilbert's conception of completeness seems to preclude a uniform definition of completeness for all theories; but, upon reflection, the model-theoretic definition of descriptive completeness is uniform only on the surface.
§ 154 An observation of Davidson's about the Tarski's theory of truth as a point of departure for developing a conception of meaning suited for understanding Hilbert's claim that axioms define expressions: Assigning meaning consists in axiomatising the expressions in question.
§ 155 Taking seriously Hilbert's claim that meaning is determined by the axioms governing an expression forces on to abandon that meaning is exhaustive in the sense that one must be able, relying on a sentence's meaning (along with other information) to settle the question of truth for any given sentence.

3.3 Hilbert's Notebooks (and other Unpublished Sources) 112

I discuss Hilbert's unpublished so called "Scientific Diary", i.e. 3 notebooks covering approximately the period from 1885 until 1912. These notebooks shed light on Hilbert's thoughts about his contemporaries and about his own philosophical views of certain aspects of mathematics and about mathematics on the whole.

§ 156 Description of the notebooks: Three notebooks (and a list of highlights compiled by Hilbert himself); entries are sometimes composed in the notebooks, sometimes copied into them by both David & Käthe Hilbert.
§ 157 No reason to think any notebooks were lost; Hilbert later recorded ideas in lecture notes and on loose sheets.
§ 158 The problem of dating the notebooks and their entries; the available evidence.
§ 159 Conjectures about the time-period covered by the first notebook. 1885-1892.

§ 160 The list of dissertation topics in the first, and of lecture-courses in the second notebook.
§ 161 Conjectures about the time-period covered by the second and third notebook: 1892-1903 and 1903-1912.
§ 162 A few sheets found in back of the third notebook date from the 1930s.
Hilbert's political observations there and in his address book.
§ 163 The notebooks thus cover approximately the first 25 years of Hilbert's career;
even though they do not cover much of the 1910s, or the 1920s, they are a valuable source.
§ 164 My discussion will be centered around Hilbert's discussion of different people.

3.3.2 Frege and Meaning 116

In the third notebook, Hilbert explicitly dismisses Frege's criticisms on the grounds that, contrary to how Frege construed his views, he did not regard the primitives of a precisely defined axiomatic theory as entirely meaningless.

§ 165 Frege is mentioned twice in the third notebook; Hilbert is severely critical of Frege here.
§ 166 Hilbert dismissed Frege's objections without much further thought.
§ 167 Frege's nonsense examples were no problem for Hilbert, because Hilbert *did* associate some (minimal) fixed meaning with the primitives of axiom-systems: that they were relations.

3.3.3 Clebsch and the Essence of Mathematics 120

Hilbert also criticises several of his contemporaries for relying too much on formulas and calculation; this critique brings into view Hilbert's own conception of mathematics as a science focused on thoughts and ideas. This conception of mathematics can make sense of Hilbert's view that consistency is sufficient for existence.

§ 168 Hilbert repeatedly criticises "the school of Clebsch" for excessive reliance on formulas and calculation.
§ 169 By contrast, Hilbert held that mathematics was concerned with "thoughts".
§ 170 Hilbert may first have reflected on this issue after encountering Thomae in 1888.
§ 171 Hilbert's conviction that mathematics is concerned with thoughts led him to reject applicability as a guide for mathematical developments; Hilbert's criticism of Klein in the notebooks.
§ 172 Hilbert later regarded his axiomatic method as allowing for *meaningful* calculation, because he saw the primitives as associated with *fundamental* operations of thought.
§ 173 This conviction can explain Hilbert's equation of consistency and existence: Because fundamental operations of thought were being invoked, just showing that the prescription of their application is coherent suffices to establish the existence of the product, because these operations always yield results.

3.3.4 Kronecker and Decidability 126

I survey the entries where Hilbert talks about Kronecker. In spite of some sharp criticisms, Hilbert eventually accepted Kronecker's principle that mathematical operations should be finitely decidable (and the requirement Hilbert ascribes to Gordan that there should only be finitely many fundamental operations). Unpublished manuscripts suggest that this change occurred in the late 1910s. The sense of decidability Kronecker was concerned with, which Hilbert adopted, is broader than the sense at issue in Behmann's decision problem (which has no discernible roots in Hilbert's earlier work).

§ 174 Hilbert's relationship with Kronecker; his philosophical opposition to Kronecker's position may have some roots in personal injury; still, Hilbert thought about Kronecker's views throughout his career.
§ 175 Hilbert eventually adopted a modified version of Kronecker's methodology.
§ 176 Kroencker's views as Hilbert saw them: Operations must be executable by means of an effective procedure, and in finitely many steps.
§ 177 Hilbert required furthermore that these operations must be more narrowly circumscribed to exclude results depending on deciding yet undecided questions; Hilbert also accepts a claim he attributes to Gordan, that there must only be a finite number of fundamental operations.
§ 178 Hilbert changed his mind about Kronecker's methodology some time in the 1910s.
§ 179 Hilbert's conception of syntactic operations as a kind of experiment.

Brief Interlude on the History of the Decision Problem 129

§ 180 The sense of "decidable" Hilbert comes to accept as a result of his reflections on Kronecker has three components: (a) finitude of fundamental operations, (b) finitude of operational prescriptions and (c) effectiveness of individual steps.

§ 181 This sense of "decidable" is different from (and more general than) the sense of "decidable" at issue in the decision problem formulated by Behmann in 1922.
Hilbert acknowledges this difference when first discussing the decision problem in 1922/23.
§ 182 Behmann's decision problem has no roots in Hilbert's earlier work.
§ 183 The question whether every mathematical problem is solvable is distinct from the question at the heart of Behmann's decision problem, whether there is a uniform, effective operation for settling every mathematical problem.

3.3.4.2 Hilbert's Conception of Number 131
Early on Hilbert accepted a conception of numbers as "bearers of laws". The notebook entries documenting how Hilbert arrived at this view suggest that it has its roots in Hilbert's reading of Kant's account of number.

§ 184 Kronecker also influence other aspects of Hilbert's thinking (if only negatively);
Hilbert's conception of (irrational) number is an example.
§ 185 Hilbert's conception of irrational number as "the bearer of a law".
§ 186 This conception of number is a testament to Hilbert's Kantianism.
§ 187 Kant's conception of number and Hilbert's adoption of it.
§ 188 Hilbert in fact engages with Kant throughout the notebooks.

3.3.5 Kant and the Promise of Knowledge 135
Hilbert's engagement with Kant in the first volume of the notebooks suggests that Hilbert's conviction that all mathematical problems were solvable derived from Hilbert's acceptance of Kant's philosophy of mathematics. Hilbert subsequently aimed to develop a systematic theory of mathematical problems.

§ 189 Hilbert's views on idealisation are linked (especially in his 1919/20 lecture-course) in his mind to Kant's conception of ideas; Hilbert's views on ideal elements are not.
§ 190 Hilbert engaged with Kant's views on the role of intuition and cognition in mathematics.

3.3.5.1 Hilbert's Theory of Problems 137

§ 191 This engagement is the root of Hilbert's conviction that every mathematical problem is in principle solvable.
§ 192 The notebooks contain further reflections on mathematical problems generally.
§ 193 In particular, Hilbert thought about: the choice of problems,
§ 194 the order in which problems should be tackled and can be solved,
§ 195 their solvability in finitely many steps.
§ 196 Hilbert seems to have worked out an entire abstract theory of problems;
At one point Hilbert meant to pose the development of such a theory as a 24th problem in his 1900 address.

3.3.5.2 Hilbert's Kantianism 143
I discuss both Hilbert's exposure to Kantian doctrine through Günther Thiele, who was a professor in Königsberg when Hilbert studied there, and Hilbert's acceptance of some of them.

§ 197 Two other unpublished manuscripts bear witness to Hilbert's (exposure to) Kantianism.
§ 198 In his student days Hilbert seems to have been aware (perhaps through a lecture by Günther Thiele) of much contemporary philosophy of mathematics, as Cod. Ms. D. Hilbert 606 shows.
§ 199 The department of philosophy in Königsberg during Hilbert's student years, his potential philosophical teachers: Julius Walter, Richard Quäbicker, Günther Thiele.
§ 200 The question of the nature and authorship of Cod. Ms. D. Hilbert 606.
§ 201 In a footnote to this manuscript Hilbert rejects Kant's philosophy of geometry explicitly, but seems to endorse Kant's overall conception for other parts of mathematics, especially arithmetic.
§ 202 A brief overview of Günther Thiele's views on mathematics.
§ 203 Two point of contact between Thiele and Hilbert: Mathematical theories serve the purpose of systematising antecedently given material; mathematics is a unified whole.

Summary of chapter 3 **148**

§ 204 Hilbert conceived of mathematics as a "science of pure thought".

§ 205 Hilbert can thus be considered a logicist; unlike other logicists he wisely avoided committing himself to a fixed list of logical operations from which all mathematics had to be constructed. Hilbert thought, rather, that the systematic analysis of mathematics should be used to find such a list.
§ 206 Many of the philosophical tenets Hilbert later prominently asserts are already implicit in earlier reflections, so there is no sharp break in Hilbert's intellectual development between an earlier and a later phase.
§ 207 Overall, Hilbert's views on mathematics are remarkably similar to Thiele's.

Epilogue **152**

Some questions about further developments, especially of Hilbert's logical theory, after 1917 are raised and (to some extent) addressed. This discussion furnishes further evidence that there was no «break» in Hilbert's development around 1917 or 1920: Both the exposition of Hilbert's mature logical theory in 1917 and of Hilbert's proof theory in the early 1920s can be recognised as continuous with earlier reflections found in unpublished manuscripts and the notebooks.

§ 208 Overview.
§ 209 In 1917 Hilbert has read read Schröder's *Vorlesungen*, but their impact on Hilbert's logical theory was minor.
§ 210 Russell's (and Whitehead's) work had a greater impact on Hilbert's later logical theory, the precise contours of this impact and the process of engagement require further study.
§ 211 The question of Heinrich Behmann's role in publicising Russell in Göttingen.
§ 212 An overview of some of Behmann's unpublished writings.
§ 213 Hilbert probably studied Russell before and independently of reading Behmann's work.
§ 214 The introduction of precisely defined languages is a major innovation in Hilbert's logical theory in 1917; Hilbert's struggle with Richard's paradox pushed Hilbert towards this innovation throughout the 1910s.
§ 215 Between 1905 and 1924 Hilbert goes through a series of different formulations and solutions of Richard's paradox.
§ 216 The emergence of Richard's paradox, and Hilbert's knowledge of it.
§ 217 In 1924 Hilbert declares the introduction of precisely defined languages to furnish a satisfactory solution to Richard's paradox. So Richard's paradox seems to lead Hilbert back to logical theory.
§ 218 The question of Hilbert's first encounter with Russell; the (scant) evidence from 1913; Poincaré and his controversy with Russell as the likely mediator introducing Hilbert to Russell's ideas.
§ 219 Finitism also seems to have earlier roots in Hilbert's thinking about Zermelo's axiom of choice.
§ 220 Hilbert's sketches for consistency proofs for his epsilon-axioms in the early 1920s echo these earlier reflections about Zermelo's axiom of choice and carry out ideas sketched there.

Chapter 1

Frege vis-à-vis Hilbert

§ 1 Frege's critique of Hilbert's work on the foundations of geometry, which eventually manifested in several articles published in the *Jahresberichte der Deutschen Mathematischen Vereinigung* in 1903 – followed by a second series of articles, directly mostly (but not wholly[7]) against Korselt, published in 1906 –, started in private with a handful of letters Frege and Hilbert exchanged in 1899 and 1900. Most of Frege's criticisms were first formulated in these letters and later transposed from there into the articles he published.

The Frege-Hilbert correspondence is not extensive. As published in the fullest edition of Frege's (surviving) correspondence it comprises 9 letters, and there is no reason to suspect that more letters existed which were destroyed along with most of Frege's posthumous papers in the bombing of Münster[8].

The letters in which Hilbert's *Die Grundlagen der Geometrie* is discussed are neither the first, nor the last of the 9. Frege first wrote to Hilbert in 1895 to continue a conversation they had started in person (to which Hilbert responded briefly), and Hilbert last wrote to Frege – at least so far as the surviving record of their correspondence goes – in 1903 to acknowledge the receipt of the second volume of Frege's *Grundgesetze*, and report briefly on his own developing views of logic (to which Frege either never responded, or the response to which has not survived).

[7] Between polemical tirades these articles contain positive proposals Frege made for reconstructing Hilbert's work in ways he found acceptable; in particular, the third of these articles contains Frege's sketch for a reconstruction of Hilbert's independence proofs. I will discuss these proposals only briefly (in section 2.3); they have been discussed at length in Michael Hallett. "More on Frege and Hilbert." In: *Analysis and Interpretation in the Exact Sciences. Essays in Honour of William Demopoulos.* Ed. by Melanie Frappier, Derek Brown, and Robert DiSalle. 2012, pp. 135–162.

[8] The most recent account of Frege's papers is Kai Wehmeier and H.-C. Schmidt. "The Quest for Frege's Nachlass." In: *Critical Assessments of Leading Philosophers: Gottlob Frege.* Ed. by Michael Beaney and Erich Reck. Vol. 1. Routledge, 2005, pp. 54–67.

The last person to have had access to the complete collection of papers Frege left to his (adoptive) son upon his death was Heinrich Scholz, who described its contents in an (unfortunately brief and superficial) fashion in a lecture held at the international congress for scientific philosophy in Paris in 1935 and published a year later: Heinrich Scholz and Friedrich Bachmann. "Der Wissenschaftliche Nachlass von Gottlob Frege." In: *Actes du Congrès international de philosophie scientifique 1935, Paris, Sorbonne.* Vol. 7. 1936, pp. 24–30. Scholz catalogued Frege's papers before their destruction, and Scholz' lists – one listing Frege's manuscripts, another of his correspondence – have survived and provide some (minimal) information about the content of items now lost. Scholz' list of Frege's manuscripts was published by Albert Veraart in 1976 (as an appendix to his article Albert Veraart. "Geschichte des wissenschaftlichen Nachlasses Gottlob Freges und seiner Edition. Mit einem Katalog des ursprünglichen Bestands der nachgelassenen Schriften Freges." In: *Studien zu Frege. Band I: Logik und Philosophie der Mathematik.* Ed. by Matthias Schirn. Problemata 42. Frommann-Holzboog, 1976, pp. 49–106, which also contains perhaps the fullest retelling of the story of Frege's Nachlass); the contents of Scholz' list of Frege's correspondence – in particular, their information about letters now lost – were incorporated into the edition of Frege's correspondence (Gottlob Frege. *Wissenschaftlicher Briefwechsel.* Ed. by Gottfried Gabriel et al. Felix Meiner Verlag, 1976).

§ 2 Though Frege states their purpose as starting "a public dialogue"[i], Hilbert never responded publicly to Frege's articles, nor does Hilbert, as far as I'm aware, ever acknowledge *in print* that he read read Frege's articles[9] and no "substantial reworking" ("erhebliche Umgestaltung"), as Frege envisioned it in the first letter he wrote to Hilbert about the *Die Grundlagen der Geometrie*, was ever carried out[ii]

It should be noted that Hilbert did revise some of the formulations Frege criticised later on: Starting with the second edition of the *Die Grundlagen der Geometrie* Hilbert systematically replaced the word "Definition" by "Erklärung" (perhaps best translated (in the sense Hilbert uses the word) as "declaration")[10]. Whether Hilbert made this change in response to Frege's criticism that he used these two different words seemingly interchangeably in the first edition[11], and decided to uniformise his terminology, cannot, as far as I can see, be conclusively determined, but it seems, although possible, unlikely, given Hilbert's dismissive response to Frege's letters. In any case, even if Frege's criticism prompted this change, the result still falls far short of the substantial reworking Frege aimed to effect, since it does not respond to any of Frege's major points of criticism which concerned *when* and *how* terms like "definition" and "declaration" should be used, not *which* terms were used in that way.

§ 3 What exactly Frege took issue with in Hilbert's work, and whether the problems he saw were problems Hilbert could – and should – recognise as such, is not on the face of it obvious. (Hilbert's responses to Frege seem rather to show that Hilbert as a matter of fact *didn't* recognise them as problems.) As a first approximation, one might observe that Hilbert's work violates some fundamental methodological principles Frege valued greatly. The first question I want to consider here is which principles Frege saw Hilbert as violating, and how.

I will consider two main criticisms Frege formulates in his letters and articles against Hilbert; I believe they are related, and together they provide a decisive reason to reject Hilbert's approach *if* one accepts certain presuppositions Frege's arguments can be seen to rely on. Given Frege's presentation of his critique, which is less than perfectly clear, I believe offering a careful reconstruction of them has value on its own, but it serves the ultimate purpose of bringing to light one of Frege's underlying commitments in particular.

After discussing the dependence of Frege's criticisms on these presuppositions in the abstract, I will point out in the next chapter that this dependence had already been noted, and made the basis for counter-criticism, in the contemporary reaction to Frege's criticism (by Alwin Korselt).

[9] In section 3.3.2 I will discuss the, as far as I can see, only piece of evidence available which suggests that Hilbert *did* at least *read* (some of) Frege's articles, even if his reaction as documented in the relevant note was dismissive. In any case, nothing suggests that Frege's objections had a significant impact on Hilbert's thinking either at the time or later on; the overall impression is, rather, as Michael Hallett describes it: "Hilbert [...] made no concessions to Frege's position" (Michael Hallett. "Frege and Hilbert." In: *The Cambridge Companion to Frege*. Ed. by Thomas Ricketts and Michael Potter. Cambridge University Press, 2010, pp. 413–464, p. 448))

[10] This change is also noted and discussed in the editor's introduction to David Hilbert. *Die Grundlagen der Geometrie. The* Festschrift *of 1899*. In: *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*. Ed. by Michael Hallett and Ulrich Majer. Vol. 1. David Hilbert's Lectures on the Foundations of Mathematics and Physics, 1891-1933. Springer, 2003, pp. 407–529, p. 421.

[11] See the opening of Frege's first letter to Hilbert about the foundations of geometry Frege, *Wissenschaftlicher Briefwechsel*, p. 61.

1.1 Frege's Criticism of Hilbert

§ 4 Frege initially takes issue with Hilbert's use of the words "axiom" and "definition". Frege held rather fortified views about their proper use, which he found Hilbert's practice of stating definitions in conflict with[12]. These views Frege held on the nature and purpose of definitions form the backdrop of Frege's criticisms of Hilbert.

For that reason, I want to bring them clearly into view first of all.

1.1.1 Frege's Account of Definitions

Before discussing Frege's views on definitions in detail, a clarification may be in order.

§ 5 Frege discusses definitions from at least two very different points of view: On the one hand, Frege is, especially in the two volumes of his *Grundgesetze der Arithmetik*, interested in setting out general criteria *any* «properly formulated» definition must fulfil; and in dismissing certain (kinds of) definitions because they violate these criteria.

On the other hand, Frege, especially in earlier work, such as the *Grundlagen der Arithmetik*, occasionally puts forward considerations to favour *particular* (or particular kinds of) definitions over others; these considerations do not, as Frege presents them, turn on the fact that any of the alternative definitions Frege compares is not properly formulated. In the context of comparing alternative definitions, Frege appeals rather to claims about the greater «fruitfulness» (in some sense) of certain definitions in comparison to others[13].

My interest here is in Frege's general criteria for definitions, because they form the basis for his critique of Hilbert, not in his considerations to favour some definitions over others. To separate these two different perspectives of Frege's concern with definitions clearly, I will reserve the label "Frege's account of definitions" exclusively for the former; the latter perspective would, in my opinion, be described most accurately by speaking of (particular) opinions Frege holds about the explication (in Carnap's sense) of certain mathematical concepts.

In a lecture from 1914, of which seemingly complete notes survive, Frege himself draws a distinction between two different kinds of usually so called "definitions" along these lines. Frege speaks, on the one hand, of "definitions building up" and, on the other hand, of "definitions breaking down" the expressions they define. Frege proposes to use the word "definition" only for the former, while describing the explication of an expression already in use, which a "definition breaking down" that expression would supply, as rather like accepting an axiom to the effect that the explication given adequately captures the sense of the expression as previously used[iv]. In the terms of this distinction, I will be interested only in what Frege there calls "definitions building up".

[12] Frege seems to have found Hilbert's work defective especially on account of his views on definitions, as a letter Frege wrote to Liebermann suggests: Frege tells Liebermann that above all "Hilbert, like many mathematicians, seems not to have a clear idea about what a definition should do, and that it cannot serve the same purpose as a proposition"[iii].

[13] These considerations have been discussed at length in Jamie Tappenden's very insightful 1995 article "Extending Knowledge and 'Fruitful Concepts'." Tappenden comes to the conclusion (pp. 448-50) that "fruitfulness" in the relevant sense is a matter of making apparent in the formulation of the definition quantificational and logical complexity, which remains hidden in alternative definitions. That is to say, a definition is more fruitful than another in case (but probably not only in case) it analyses the logical structure of the informal concept it explicates further, so that more consequences can be derived from the definiens by mere logic.

Sources. Frege expounded his own views on definitions in at least three places:

§ 6 The first volume of the *Grundgesetze der Arithmetik* contains what I consider Frege's most definitive statement of his views on definitions[14]; the second volume offers a slightly different, apparently generalised formulation of them[15]; another, less extensive formulation of them is included in Frege's letters to and articles against Hilbert.

To what extent the views Frege expresses in theses different places are mere extensions of the views expressed in the first volume of the *Grundgesetze der Arithmetik*, rather than revisions, requires some discussion.

Frege also expounded his views on definitions at length in a lecture in 1914, detailed notes of which survived and have been published under the title "Logik in der Mathematik" as part of Frege's *Nachgelassene Schriften*. However, as far as I can tell, the conception of definitions Frege presents there does not differ in substance from what he already presented in the three places I just mentioned[16]. So I will not discuss this text as a separate source.

I begin with Frege's statement of his views on definition in the letters (and articles) concerning Hilbert's *Die Grundlagen der Geometrie*, to work backwards in time from there (that is, 1899 and 1903) to the views Frege expressed almost a decade earlier in the first volume of his *Grundgesetze der Arithmetik* (in 1893).

§ 7 Definitions, according to Frege in 1903, should endow a previously meaningless term with meaning[vi]. To that end, a definition must, other than the expression being defined, contain only terms whose meaning has previously been determined (either by a definition, or informal explanation). This restriction, that only meaningful expressions may be used to define a new expression, is a core doctrine of Frege's account of definitions as I understand it. Frege re-iterates it – in one form or another[17] – in every exposition of his views, and the critical use Frege makes of his account of definitions – not least of all in his critique of Hilbert – often depends on it.

It strikes me as worth stressing at the outset that this restriction is peculiar because meaningfulness has, on the face of it, nothing to do with definitions, thought of as purely syntactical devices for convenient abbreviation[18],

[14] It may be worth noting in this connection that Heinrich Scholz and Friedrich Bachmann also thought that the full wealth of Frege's posthumous papers, including those since destroyed, "did not progress substantively beyond the standpoint of 1893 ('Basic Laws' I)" (see: Scholz and Bachmann, "Der Wissenschaftliche Nachlass von Gottlob Frege," p. 27).

[15] About the connection, and differences, between Frege's different statements of his views on definition in the first and the second volume Frege himself says the following: Since his aim in the second volume is the discussion of "definitions formulated in words" (rather than in a symbolic language) his general discussion of definitions in the second volume will "only let such differences emerge as are due to the different nature of these means of formulation"[v]. This statement unfortunately leaves open the question whether Frege sees what is said in the second volume as *going substantially beyond* what was said in the first volume of the *Grundgesetze der Arithmetik*, or as a mere informal re-statements, adapting *substantially the same* principles for further-reaching application.

[16] In the same lecture Frege also rehearsed his critique of Hilbert once again (see: Gottlob Frege. *Nachgelassene Schriften*. Ed. by Hans Hermes, Friedrich Kambartel, and Friedrich Kaulbach. Zweite, revidierte Auflage. Meiner Verlag, 1983, pp. 263–270), though, in this respect as well, this presentation offers, as far as I can see, nothing substantively new.

[17] In the first volume of the *Grundgesetze der Arithmetik*, this restriction appears in § 33 as the first and main guideline for defining expressions. It can be paraphrased as follows: There must always be a uniquely determined complex expression composed only of expressions taken to be primitively meaningful (where all previously defined expressions have been successively replaced by their defining expressions until only primitively meaningful expressions remain) which is equal in meaning to the defined expression.

Frege's own formulation of this guideline in the *Grundgesetze der Arithmetik* is more specific than my paraphrase, since it is tailored to serve as a guideline specifically for definitions framed in Frege's own deductive system. The peculiarities of Frege's system mainly relevant are that all expressions in Frege's system are (either simple or complex) singular terms (Frege speaks of "Eigennamen"), and that there are eight specific names which Frege takes to be primitively meaningful[vii].

I'll be discussing the status of this principle in the second volume of the *Grundgesetze der Arithmetik* below, in § 8.

[18] Such a purely syntactic view of definitions was later articulated in detail by Carnap in the *Logische Syntax der Sprache* (published in 1934), where Carnap claims that "for a rigorously formal way of proceeding the decision whether a given sentence is an admissible definition in a certain system or not must depend only on the form of the sentence and the formation rules of the system" and goes on to criticise requirements for correct definitions which require "the historical observation whether certain [other] sentences [viz., in particular, certain other definitions] have previously been established or not"[viii]; I take it that Carnap would find appeals to semantics

which is how Frege himself seems to speak of definitions initially, in the preface to the *Grundgesetze der Arithmetik*, where he says that definitions "merely introduce abbreviated names"[xi].

Relation between the sources. As I said, this is how Frege expresses his views on definitions in his discussion of Hilbert's work. The conception Frege is sketching here is in one important respect closer to the views expressed in the first volume of the *Grundgesetze der Arithmetik*, than it is to second volume, which had recently been published when Frege's articles appeared[19]: In his letters and articles against Hilbert, as in the first volume of the *Grundgesetze der Arithmetik*, the main principle governing the formulation of definitions is that they must assign a meaning to a symbol which previously had none; or, as Frege paraphrases the principle in the first volume of the *Grundgesetze der Arithmetik* (see the passage referred to in footnote 17): Every defined expression must be meaningful. I will use the label "principle of meaningfulness" to refer back to this principle. § 8

In the second volume of the *Grundgesetze der Arithmetik*, Frege seems to be basing his account on a different principle, which he calls the "principle of completeness". This principle says that a properly formulated definition must ensure that what it defines divides all things exhaustively according to whether the definiendum applies, or does not apply to them[xiii]. The cumbersome formulation of the principle I have just given – speaking of "what a definition defines" and "applying the definiendum" – glosses over another change of opinion: As presented in the second volume of the *Grundgesetze der Arithmetik*, definitions are given not merely of expressions, but also of concepts; indeed, Frege's formulation suggests that in the second volume of the *Grundgesetze der Arithmetik* he takes definitions to be given *primarily* of concepts and only derivatively of expressions: Expressions ("Namen") are no longer mentioned at all, and their apparent heirs, predicates ("Prädikate"[20]), are dealt with only parenthetically.

This change concerning the «native soil» of definitions can, I believe, explain why Frege chose the principle of completeness, rather than the principle of meaningfulness as the basis for his account of definitions in the second volume of the *Grundgesetze der Arithmetik*: If Frege has come to hold that definitions are given primarily of concepts, and of expressions only mediately through the concepts the expressions designate, then the principle that every expression must have a meaning no longer makes sense as a condition on admissible definitions. Concepts,

inadmissible as part of an account of definitions for the same reason that he finds appeals to "historical observation" inadmissible: Because semantics, like the order of exposition, is extraneous to the deductive system.

It is, in my opinion, plausible to think that eliminating the dependence on the (extraneous) notion of meaning from Frege's account of definition was an aim Carnap actively pursued in the *Logische Syntax der Sprache* because he remarks earlier that the Fregean distinction between names on the basis of whether they have absolutely fixed meaning or not is merely psychological, not logical[ix]; a concern with eliminating appeals to meaningfulness thus seems to run through Carnap's work. It strikes me as also worth noting that this concern with eliminating extraneous considerations from the evaluation of the legitimacy of definitions may have been suggested to Carnap by Frege's own work, who motivates his principle of "simplicity" in the second volume of the *Grundgesetze der Arithmetik* by claiming that the legitimacy of definitions must not depend on the result of further investigations[x].

[19] Given that Frege's articles draw a lot of arguments, examples, and even certain formulations, from his letters to Hilbert written around 1900 (3 years before the publication of the second volume of the *Grundgesetze der Arithmetik*), this need not appear surprising: Frege may not have written the relevant parts of the second volume of the *Grundgesetze der Arithmetik* at the time he was writing his letters to Hilbert, and may have come to adopt the position he holds in the second volume only subsequently; still, in his articles Frege refers readers to his exposition in the second volume of the *Grundgesetze der Arithmetik*, even though the views expressed are, as I explained, more akin to those expressed in the first volume (which he referred Hilbert to in his letters)[xii].

[20] That "Prädikat" means some kind of expression is suggested (though not explicitly said) by Frege's use of the phrase "whether a predicate can be truly said of some object"[xiii], which suggests that predicates are constituent parts of sentences, hence expressions.

by contrast, cannot sensibly be required to have a meaning assigned to them, since they *are* (potential) meanings (for expressions). The principle of completeness, on the other hand, makes sense as a requirement on definitions of concepts (as well). Concepts as well as expressions can sensibly be required to determine exhaustive divisions of objects. In fact, Frege seems to think that the principle of completeness does not simply replace the principle of meaningfulness, but rather entails it, and thus constitutes a generalisation of that earlier principle:

> We expressed this principle in the following fashion in the first volume: every name of a function must have a meaning. That is why all conditional definitions and all partial definitions must be rejected. Every symbol must be defined completely all at once in order that it will, as we say, come to have a meaning. All of this is intimately connected and can be regarded as a consequence of the principle of completeness of definitions.[xiv]

I find the change in Frege's account I have just been discussing – especially the shift of focus from defining expressions to defining concepts – remarkable; but just as remarkable is that Frege does not comment on it[21].

I will, in any event, take the proper objects of definitions to be expressions.

§ 9 Reflecting upon Frege's use of his account of definitions in criticising rival conceptions of irrational numbers in the second volume of the *Grundgesetze der Arithmetik* suggests, to my mind, that this shift of focus is perhaps even *motivated* largely by wanting to discredit other accounts: Since these accounts cannot be faulted on the basis of the principle of meaningfulness, Frege finds himself forced to adopt a stronger principle in whose light the rival accounts Frege considers will appear defective. Frege's rejection of what he calls "partial definitions" furnishes an example of such a case: A partial definition of, say, an arithmetical operation-symbol cannot, in general, be faulted for not assigning *any* meaning to the symbol; but it can be faulted for not assigning as the symbol's meaning a concept which is complete (in the sense of being defined for absolutely everything).

So, when Frege comes to discuss partial definitions of arithmetical operation-symbols in the second volume of the *Grundgesetze der Arithmetik*, he says that an expression which does not "mean a [for every object] sharply defined concept thus does not mean anything at all serviceable for logic"[xvi]. Frege does not claim there that, e.g., the phrase "something which, when added to itself, yields one" has *no* meaning assigned to it. From the point of view of the principle of meaningfulness, therefore, there is nothing wrong with this definition: The expression is, after all, assigned some meaning. So definitions like those Frege targets in his critical discussion in the second volume of the *Grundgesetze der Arithmetik* appear faulty only in light of an additional assumption about what definitions must accomplish: the principle of completeness, which requires that definition assign not just *any* meaning, but rather what Frege in this manuscript calls a "sharp" meaning[22].

An unpublished manuscript containing a critical discussion of Peano throws some additional light on the development of Frege's views of definition. In this manuscript, entitled "Begründung meiner strengeren Grundsätze des

[21] This silence is especially remarkable for a writer like Frege who shows – not least of all in the second volume of the *Grundgesetze der Arithmetik*[xv] – an almost neurotic awareness of the distinction between expressions and the objects they may pick out.

[22] It is worth noting that Frege invokes the law of excluded middle to motivate his principle of completeness. The law of excluded middle requires, according to Frege, that definitions must "sharply delineate" what they define because "it must be definitely determined for every object whether it falls under a given concept or not"[xvii].

Definirens", Frege criticises Peano's formulation of the principle that two classes have the same number of elements if and only if there exists a one-to-one mapping between them for explicitly incorporating restrictions imposed on the variables into the claim, rather than into the definitions of the concepts employed[xviii]. Frege then points out that there are other formulations, equivalent to Peano's, wherein, because the range-restrictions have been moved, the variables come to have different ranges[xix]. Frege takes this to be objectionable, thought it remains unclear why, exactly[23], and a reason to state definitions which, because they are applicable to absolutely all objects, obviate the need for explicit range-restrictions[xxi]. Note that, whatever the details and merits of Frege's example and reservation, this manuscript also corroborates my suggestion that Frege was motivated to accept his conception of definitions because it allowed him to dismiss rival – in this case, Peano's – theories as faulty on methodological grounds.

In any case, whatever Frege's motivation for focusing on definitions of concepts rather than expressions, and for adopting the principle of completeness instead of the principle of meaningfulness may have been, I take it, Frege would also still endorse the details of his account of definitions from the first volume of the *Grundgesetze der Arithmetik*; and if the principle of completeness does entail the principle of meaningfulness, as Frege claims, he is, in any case, still committed at least to this main principle, and to whatever it entails[24]. So, in particular, I take it he still accepts the principle that every defined expression must be assigned a meaning[25].

As I said earlier, this particular principle plays a decisive role in Frege's critique of Hilbert. The next section will be dedicated to bringing to light just where and how it enters into Frege's arguments.

1.1.2 Frege Criticism of Hilbert's Definitions

To Frege, and anyone else who accepts Frege's account definitions, and axioms[26], Hilbert's exposition in the first few sections of his *Die Grundlagen der Geometrie* is bound to appear faulty. § 10

However, care should be taken not to overstate the problems.

In fact, Frege never doubts that Hilbert's axioms could be used as axioms for developing (Euclidian) geometry[27]. § 11

[23] Frege merely says that such a variability of range-restrictions between equivalent formulations "does not exactly increase the logical perspicuity"[xx], but does not raise any substantive problem.

[24] Reading the first volume of the *Grundgesetze der Arithmetik* can, indeed, leave the impression that Frege conceives of his account of definitions as following entirely from this main principle (somehow). So Heck observes: "The last six [guidelines for definitions Frege presents in the first volume of the *Grundgesetze der Arithmetik*] more or less follow from the first" (Richard Kimberly Heck. *Frege's Theorem*. Oxford University Press, 2011. (Originally published under the name "Richard G. Heck, Jr".) P. 19).

[25] That Frege occasionally still appeals to the principle of meaningfulness to dismiss definitions in the second volume of the *Grundgesetze der Arithmetik* provides good evidence to confirm this conjecture: So, for example, in his critique of Thomae's views, Frege dismisses a definition because the symbol – not concept! – being defined "means nothing at all"[xxii].

[26] Frege's account of axioms can be summarised in a single sentence and, as far as I can tell, undergoes no changes between 1893 and 1906: that axioms must be (determinately) true sentences[xxiii]. In any case, his criticism of Hilbert depends only on taking this condition to be necessary for being an axiom.

Michael Resnik, in his 1974 article "The Frege-Hilbert Controversy" which seems to no small part to have shaped the scholarly perception of the correspondence between Frege and Hilbert, takes Frege's doctrine of axioms to be rooted in his conception of inference as proceeding from recognised truths to further truths to be recognised as such (see: Michael Resnik. "The Frege-Hilbert Controversy." In: *Philosophy and Phenomoenological Research* 34.3 [1974], pp. 386–403, p. 401).

[27] This has been noted (parenthetically) by Hallett, "Frege and Hilbert," p. 478n8: "Incidentally, Frege never challenges Hilbert's choice of axioms, but rather their formulation, what they can be considered as saying.". While I find myself basically in agreement with this observation, I want to phrase the aspect of Hilbert's work Frege *does* take issue with differently. It seems to me Frege could, in principle, even accept Hilbert's axioms *as formulated by Hilbert* as axioms for (Euclidian) geometry; what he finds unacceptable is a claim Hilbert makes *about* the axioms, namely, that they contribute to defining the basic concepts of geometry.

It is worth noting in this connection that among Frege's posthumous papers Heinrich Scholz did indeed find two manuscripts which Frege, who had put his unpublished manuscripts in some order before his death, had entitled "Frühere Versuche einer neuen Geometrie" and "Versuch einer neuen Geometrie"[28]. Though Scholz felt he could not date these manuscripts with any confidence, Frege's identification of them as "early [manuscripts]", in any event, makes plausible the conjecture that they are the "frühere unvollendete Versuche" Frege mentioned to Hilbert[xxiv] (or at least contained some of the work Frege was talking about at).

The fact that Frege told Hilbert nothing of their content, that he later titled the manuscripts "attempts" and apparently never resumed the investigations begun in them, in conjunction with Scholz' description of one of the manuscripts as "entirely fragmented" (see footnote 28), may suggest that these early geometrical investigations remained largely fruitless (without giving any indication of the problems Frege found himself facing).

Nor is it the case that Frege finds Hilbert's book *entirely devoid* of definitions in his sense[29], only that Hilbert § 12
occasionally also speaks of definitions with reference to passages which do not fit Frege's general account. Frege thus seems to want to ultimately take issue with some select aspects of the methodological framework in which Hilbert develops his theory; not with *what* Hilbert is presenting so much as with *how* he chose to present it.

For Frege, trouble starts in § 3 of *Die Grundlagen der Geometrie* where Hilbert claims: § 13

> The axioms [of order] define the concept "between" and make it possible, on the basis of this concept, to *order* points on a line, in a plane and in space.[xxvi]

This remark is at odds with both Frege's account of definition, as well as his account of axioms.

The former, roughly speaking, because the definiens – in this case (presumably) the conjunction of the order-axioms – contain the term to be defined, "between", and thus contains a term not yet assigned any meaning.

The latter because, containing an as of yet undefined term, these axioms cannot – at least not at this point in the development of geometry – be (recognised as) determinately true.

From the tangle of problems Frege raises in his articles two main arguments can be isolated, both made, I take it, to discredit Hilbert's definitions of the primitives "point", "line" and "plane" by means of axioms, but each in a slightly (but importantly) different way. I will discuss these two arguments at some length now.

On the other hand, Patricia Blanchette speculates that Frege may ultimately have endorsed different axioms (and primitives), and may have come to think that Hilbert's axioms were defective, since they did not reflect what Frege may, upon reflection, have come to regard as the correct analysis of the deductive structure and core concepts of (Euclidian) geometry: Patricia Blanchette. "Frege and Hilbert on Consistency." In: *The Journal of Philosophy* XCIII.7 (1996), pp. 317–336, pp. 326 sq.; this speculative suggestion is (entirely) based on a parenthetical remark in Frege's first letter to Hilbert where Frege says he believes – on the basis of earlier efforts of his own – he "could make do with fewer primitives"[xxiv].

[28] See: Veraart, "Geschichte des wissenschaftlichen Nachlasses Gottlob Freges und seiner Edition," p. 86.

According to Scholz' description of these lost manuscripts, as reported by Veraart, the manuscript entitled "Frühe Versuche einer neuen Geometrie" existed in two revisions, each running to a little more than 10 pages, while the manuscript entitled just "Versuch einer neuen Geometrie" was, according to Scholz, "völlig burchstückhaft", consisting only of disjoint notes and loose sketches (see: Veraart, "Geschichte des wissenschaftlichen Nachlasses Gottlob Freges und seiner Edition," p. 100).

[29] For instance, Hilbert's definition of a polygon in § 4 is a perfectly acceptable definition even by Frege's standards, and Frege tells Hilbert so in his first letter[xxv]: The expression being defined, "polygon", does not itself appear in the defining expression – unlike, e.g. "between" appears in the «definition» Hilbert gives since it appears in the axioms governing the word "between" –, and all the expressions which do appear in the defining expression ("set of lines determined by a sequence of points" ("Streckenzug"): "point", "two points determining a line",...) have been defined previously (though Frege would not accept some of *those* definitions as properly formulated, e.g. that of "point"), and did not, in turn, presuppose the term "polygon".

1.1.2.1 The Argument from Incompatibility

A first fundamental issue can be described in terms of incompatibility[30]. § 14

Hilbert's claim that a set of axioms for a concept can define the concept at issue is incoherent from Frege's point of view because it attempts to combine definitions and axioms in a way which leads invariably to a contradiction:

Suppose, for the sake of argument, (i) that Hilbert succeeded in giving a definition in Frege's sense. According to Frege's account of definition, (ii) the expression being defined is assigned meaning by the definition. (iii) It must, therefore, not have had any meaning prior to being assigned one by this definition. Since (iv) the expression being defined occurs essentially[31] in some of the axioms invoked in the definition, (v) these axioms cannot have a determinate truth-value prior to the definition; in particular, (vi) they cannot be determinately true prior to the definition. So (vii) the sentences invoked by the definition as axioms cannot really be axioms in Frege's sense.

Another version of the argument emerges upon running through the above steps, as it were, from back to front[32], starting from the denial of the claim numbered (vii) above: If one supposes (for the sake of the argument) that Hilbert is really invoking axioms (in Frege's sense, i.e. determinately true claims) in his definition, one arrives at the denial of claim number (i), i.e. the conclusion that Hilbert cannot also succeed in giving a definition in Frege's sense, since the expression being defined – e.g. in the case of the passage quoted above, the word "between" – must already have meaning prior to being assigned one by the definition at hand.

Presuppositions. Two background assumptions are crucial to the argument as I have stated it: § 15

The principle of meaningfulness – that definitions must assign meaning to expressions which previously had no meaning – licences the inference from the assumption (i) to step (ii); (or, in the reverse order, from the denial of step (ii) to the denial of step (i)).

In addition, some kind of compositional analysis of sentential meaning licences, by way of the observation (iv), the inference from step (iii) to the conclusion (v); (or in the reverse order, from the denial of the claim (v), again relying on the observation (iv), to the denial of step (iii))[33].

[30] Frege himself speaks in this way in his first article, where he rhetorically poses the question whether Hilbert's use of axioms to define concepts "is compatible" with (what Frege takes to be) the nature of axioms[xxvii]. (The answer is: "No!".)

[31] I am using "essential" in roughly the particular sense Quine gives to that term in his Willard van Orman Quine. *Mathematical Logic*. Revised Edition. 1981, p. 2. It seems to me once can explain this sense a little more precisely by saying that an expression of a certain category occurs essentially in another expression just in case there is at least one different expression of the same category such that when it is substituted for the expression in question the semantic value of the larger expression changes. Of course, this explanation remains vague, as long as no precise definition of "(linguistic) category" and "semantic value" is given.

Frege, incidentally, suggests drawing a seemingly similar distinction in the last of his articles on the foundations of geometry (published in 1906): He distinguishes between the concepts a given theory is "indifferent to" (which can thus be inter-substituted without impacting the truth of claims made by that science), and those that "belong to it" (which cannot be inter-substituted without changing truths to falsehoods at least sometimes). Frege explicitly says that words like "point" belong (in this sense) to geometry[xxviii].

[32] Frege himself seems to go back and forth between these two versions of the problem in his letters and articles: His charge that Hilbert's axioms do not, after all, express basic facts of intuition presupposes the above version; while his various complaints about Hilbert's definitions being improperly stated (not real definitions at all!) presuppose taking the argument «in reverse».

It is worth noting that Hilbert appears to have taken Frege's point, or (more probably) come to a similar conclusion by himself, quite a long time later: When presenting his axioms of geometry again in the context of lectures on the "Principles of Mathematics" delivered in 1918, Hilbert no longer speaks of axioms as defining concepts. Instead, he says that the axioms "axiomatically characterise" certain concepts[xxix]. Hilbert still also speaks of "defining" concepts, but only with respect to concepts which are explicitly defined in terms of previously defined (or axiomatically characterised) concepts (such as the concept of a "Nebenwinkel"[xxx]).

[33] To be exact, the principle required is that every essential component expression of a meaningful sentence is meaningful. Frege

I want to focus on the former, that is, the principle of meaningfulness.

Note, first of all, that the precise import of "meaning" seems not to make a difference to this argument. Whether § 16
"meaning" is analysed, as Frege proposed, in terms of sense and reference, or in terms of denotation (as Russell suggested), or some other way – or not at all! –, the argument remains the same.

The argument depends only on two assumptions about meaningfulness, which should hold regardless of the underlying conception of meaning: first, the assumption *that* definitions assign meaning (of whatever flavour) to the expressions they define. This assumption takes some concept of meaningfulness for granted, but does so to impose a requirement *on definitions*. The requirement does not change depending on which particular concept of meaningfulness is involved (or how the concept used must be intuitively recognisable as a concept of meaningfulness).

One may call the second assumption the above argument depends on (following Schröder) the principle of univocity[34], which states that no expression may be assigned *more than* one meaning (again, of whatever flavour). This principle is, I take it, what Frege's requirement that defined expressions "previously have no meaning" captures: A successful definition must assign meaning to the term it defines (as per the first assumption), which on pain of violating the principle of univocity (by becoming equivocal) must therefore not have had any meaning previously. The principle of univocity justifies the inference from step (ii) to step (iii); or, when the argument is taken in reverse, from the denial of step (iii) to the denial of step (ii).

1.1.2.2 The Argument from Inapplicability

Other considerations Frege leveraged against Hilbert, by contrast, are not indifferent to the underlying account of meaning. One of them in particular suggests that Frege also faulted Hilbert's definitions for not assigning meaning *in a particular sense of the word* "meaning" to the expressions they define.

I have in mind the following, odd example Frege uses to illustrate the defectiveness of Hilbert's definitions: § 17

> I don't know how to decide the question whether my pocket-watch is a point relying on [Hilbert's] definition. Already the first axiom deals with two points; if I wanted to know whether it holds of my watch I would first have to know of another object that it was a point. But even if I knew that, for example of my fountain-pen, I would still be unable to decide whether my watch and my fountain-pen together determine a line since I would not know what a line is.[xxxiv]

At the core of this passage, as I understand it, lies a regress-argument: Picking an arbitrary thing (his pocket-watch) and asking whether it is a point, Frege observes that applying Hilbert's definition to answer that question presupposes that one has already successfully applied Hilbert's definition of "point" – as an object which fulfills all those among Hilbert's axioms in which "point" occurs – to at least one other object. But that antecedent application of the definition would similarly presuppose a further application of the definition, and so would all presumed prior

formulates such a principle for his concept of reference ("Bedeutung") explicitly in "Über Sinn und Bedeutung"[xxxi].

[34] In the introduction to the first volume of his *Vorlesungen über die Algebra der Logik*, Schröder formulated "the fundamental requirement of univocity, through which alone a symbol can fully serve its purpose"[xxxii]. Frege himself emphasised the capital importance of this principle (motivating it as Schröder does) in his second series of articles against Hilbert[xxxiii].

attempts of applying Hilbert's definition. The resulting regress is vicious because, without a first application of the definition, no subsequent application of the definition is possible; *something* must first be known to be a point. Since there can be no application of Hilbert's definition without presupposing at least one further application, it appears that Hilbert's definition of a point cannot be applied at all.

In his articles published in 1903, Frege takes this regress to show that Hilbert's definitions do not successfully assign meaning to the terms they define. (Frege uses the example already in his letter, and arguably for the same purpose, but his intentions are less transparent there; so I rely on the published article at this point.)

§ 18 Frege seems to infer this conclusion from the result established by the regress-argument – which, Frege thinks, can be adapted to most (or all) of Hilbert's definitions – by relying on a peculiar assumption about meaning.

When setting up the example of his pocket-watch in the 1903 article Frege uses the phrase "finding out the meaning" (by looking at the definition) as apparently equivalent to the phrase "finding out when the defined expression applies"[xxxv]. I take this transition to reveal one particular aspect of Frege's conception of meaning, which this argument presupposes: The meaning of an expression must (at least) determine the class of things to which the expression applies – anachronistically speaking, the extension associated with the expression –, that is to say, of which it can be truly predicated; and that extension must be *uniquely* determined[35].

Frege's pocket watch-example, as I understand it, depends on this further assumption. It is meant to show, I take it, that whatever meaning Hilbert's definitions may assign to the expressions they purport to define does not determine the extension of those expressions uniquely and *therefore* does not properly qualify as meaning at all.

For that reason Hilbert's definitions now appear – as they did already at the end of the previous argument – to violate Frege's principle of meaningfulness, which Frege takes to be an uncontroversial requirement for being a proper definition. This inference relies on the assumption that meaning in the proper sense must determine extension, and thus shows Frege presupposing a particular (albeit minimal) account of "meaning"[36].

1.1.2.3 Combining the Arguments

§ 19 To appreciate how deeply this commitment to extension as uniquely determined by meaning (in the sense Frege seems to be presupposing) runs through Frege's critique of Hilbert, consider the two arguments side by side.

1. Observe, first, that they can be used to *support the same conclusion*, namely, that Hilbert's definitions fail to be definitions in the proper sense, as determined by Frege's account of definitions: Both arrive at this

[35] Note that Frege invokes his principle of completeness in the passage I quoted on page 6 to a similar effect: Frege says that a symbol comes to have a meaning in the sense he intends only if it is determined for absolutely everything whether the symbol «applies» or not; paraphrasing this requirement by saying that Frege is requiring that every meaningful expression has a uniquely determined extension associated with it strikes me as a small, reasonably uncontentious interpretative step.

[36] According to Dummett's reconstruction of Frege's views on sense and reference, it is important to appreciate that meaning *determines* the extension, but does not involve or consist in it. Extension, in Dummett's interpretation of Frege, is not a part of the meaning, but merely connected with it in a systematic way (cf. Michael Dummett. *Frege: Philosophy of Language*. Second Edition. Duckworth, 1981, p. 91); though extension is not a component of meaning for Dummett, the claim that meaning determines extension is none the less a substantive claim about the concept of meaning – by connecting "meaning" with another semantic concept, in this case, "extension" – and, as such, indicative of the particular account of meaning Frege is presupposing.

conclusion by noting that Hilbert's definitions fail to assign meaning to the expression they purport to define. Thus they violate Frege's account of definitions (specifically, the principle of meaningfulness).

The second argument arrives at the conclusion that Hilbert's definitions fail to assign meaning to the expressions they purport to define by giving a counterexample showing that whatever meaning they may assign is insufficient to determine an extension; but because meaning, as Frege conceives of it, must at least determine extension, whatever Hilbert's definitions may assign does not qualify as "meaning" in the required sense.

As I sketched in some detail above (in § 14), the reverse-version of the first argument arrives at the same conclusion (using considerations relying on the compositionality of sentence-meaning).

2. Observe, second, that both arguments reach the same conclusion based *on different grounds*: Both arguments depend on Frege's account of definitions, and on what I called "the principle of meaningfulness" in particular.

   The first argument depends, in addition, on Frege's conception of axioms as determinately true claims[37].

   The second argument depends additionally on Frege's particular conception of meaning (as (determining) extension); it does *not*, however, *depend* on the assumption that the axioms Hilbert incorporates into his definitions are determinately true claims. Indeed, Frege's presentation of the second argument suggests that it was designed specifically so as not to depend on assuming the truth of the axioms[38].

3. This leads me to a third observation: Together, the two arguments constitute a dilemma for Hilbert's account: Either axioms are determinately true or not. If they are, the first argument applies; whereas if axioms are not determinately true, the second argument applies. In either case, Hilbert's definitions turn out (albeit for different reasons) not to be proper definitions according to Frege's conception of definitions (as involving the assignment of meaing), and so Hilbert fails to define the expressions his theory employs.

For this dilemma to take effect, however, accepting Frege's account of definitions (including, in particular, § 20 his principle of meaningfulness) is not enough. The further assumption that the meaning to be assigned by a definition determines extensions must also be accepted: While the first argument depends only – note, however, the caveat indicated in footnote 37 – on accepting the claim that definitions assign meaning (of whatever kind) to the expressions they define and on accepting the claim that no expression may have more than one meaning assigned to it, the second argument – and the force of the whole dilemma is tied to it – depends on accepting in addition that meaning (at least) determines the extension an expression is associated with.

To understand Frege argument against Hilbert as decisive, thus requires three commitments:

(I) Definitions associate a meaning with the expressions they define.

[37] With Frege's conception of truth-values as *extensions* of sentences in mind, one might see in this assumption another manifestation of Frege's principle that meaning must determine extension. If one is inclined to understand this assumption about axioms in this way, the first argument also depends on Frege's peculiar conception of meaning and is not, as I suggested above (in § 16) neutral to it.

[38] Frege seems to be accepting for the sake of this argument that Hilbert's axioms do in fact "contain expressions whose meaning is still unknown"[xxxvi], which would, consequently, not qualify as axioms in the proper sense according Frege's conception of axioms.

(II) No expression can have more than one meaning associated with it (certainly not by means of definitions).

(III) If an expression has a meaning associated with it, then it also has a unique extension associated with it.

This is as far as I will take my discussion of the Frege-Hilbert correspondence.

§ 21 The precise role Frege's principle of meaningfulness plays in the arguments against Hilbert, which I have been discussing, has, I hope, become clear. In addition, the discussion has served to bring to light another key presupposition of Frege's critique of Hilbert's work on the foundations of geometry. Frege's critique, as I suggested reconstructing it, is (in part) based on a substantial assumption about what it takes for any (defined) expression to be meaningful: A meaningful expression must have a uniquely determined extension associated with it.

Both the principle of meaningfulness (number (I) in the above list) and the assumption that meaningful expressions must be associated with a uniquely determined extension (number (III) in the above list) can be disputed; in principle, so can the principle of univocity (number (II) in the above list), but as far as I am aware, nobody has ventured to do so. Rejecting any of the three presuppositions would undermine Frege's critique of Hilbert.

As the next chapter will detail, Alwin Korselt chose such a strategy in his defence of Hilbert's position: In his responses to Frege's articles, Korselt chose to reject Frege's assumption (III).

More important than deciding who was right and who was wrong, however, are the alternative conceptions of definitions and meaning built around various contraries to assumptions (I) and (III) above[39].

1.2 Frege's Motives

§ 22 Before leaving Frege behind I want to discuss possible reasons Frege may have had for accepting assumptions (I), (II) and (III); (which may also be reasons for accepting these assumptions independently of Frege).

To clarify what is intuitively at stake, I offer the following intuitive paraphrase of these principles: According to these three principles, every defined expression, in order to be properly defined, must be assigned a meaning (by (I)), which requires assigning an extension (by (III)), and no more than one (by (II)).

Loosely speaking, the total import of these assumptions can, I think, be paraphrased by saying that alongside every theory proposed, the theoretician must always construct an interpretation for the theory (in the sense, say, of a model-theoretic structure); and no more than one. Intuitively, then, accepting these three assumptions together amounts to accepting that every theory must be associated with (exactly one) «intended» interpretation[40].

It is of *this* intuitive principle I will now consider possible motivations Frege may have had for adopting it. Since

[39] For one such alternative conception of definitions see footnote 18 on Carnap's theory of definitions presented in *Logische Syntax der Sprache*, and the passages referred to there.

[40] Jamie Tappenden comes to a similar conclusion about how Frege differs with respect to methodology from his contemporaries in his 1995 article "Extending Knowledge and 'Fruitful Concepts'": "[Frege] does not hold that [Hankel & Grassman's] kind of algebraic generalisation should be despised in principle, but rather that the requisite standards of proof had not been met. In particular, Frege apparently requires the explicit construction of the objects the theory is a theory of." (Jamie Tappenden. "Extending Knowledge and 'Fruitful Concepts'. Fregean Themes in the Foundations of Mathematics." In: *Nous* 29.4 [1995], pp. 427–467, p. 338).

textual evidence to document Frege's intellectual development independently of his own narratives is scarce[41], I believe the best I can do is to sketch scenarios and supply reasons why Frege may have changed his views in the way described. I will offer two scenarios, which naturally remain plausible speculations at best.

1.2.1 First Motivation: Discrediting Rivals

The first is based on the observation that Frege often engages in critical discussion; to a point where it appears not implausible that he saw the rejection of rival accounts as a key part of his work (see § 9). Frege's conception of definition may in this way have been motivated (at least in part) by Frege's wish to debar certain kinds of definitions he saw his contemporaries using from qualifying as proper definitions. § 23

Frege's early lecture "On Formal Theories of Arithmetic" (from 1885) fits this pattern especially well: Frege announces at the outset that his aim is to refute a certain approach to arithmetic theory construction (which he does not attribute to anyone in particular in that lecture). Frege then goes on to fault what he calls "formal arithmetic" for being unable to supply definitions which assign meaning to the defined expression, even though definitions "after all serve the purpose of stating what sense one associates with a given symbol"[42].

The principle Frege appeals to here is not motivated independently of the aim Frege pursues here. As far as this lecture takes the discussion, the principle may well be motivated only by Frege's wish to fault the formal theories of arithmetic Frege is discussing on account of it. Although Frege's formulation suggests that he takes himself to be relying on an intuitive, platitudinous principle, his discussion supplies no reason to regard this suggestion as more than a blunt rhetorical manoeuvre masking the fact that Frege is begging the question against opponents who would never have granted that the purpose of definitions is to assign meaning to a given symbol.

Dismissing rival accounts by attacking the definitions they employ transparently motivates Frege's discussion of definitions in the second volume of the *Grundgesetze der Arithmetik*. Indeed, the first consequence Frege proceeds to draw (in § 57) from his principle of completeness (introduced in § 56) is that "this entails the inadmissibility of the practice of partial definitions, which are very popular in mathematics"[xxxviii]. § 24

The same motivation can be recognised as underlying Frege's account of definitions in the first volume of the *Grundgesetze der Arithmetik*, where Frege briefly comments on the status of definitions as follows: § 25

> In the course of this undertaking the need for definitions will become apparent again and again. The fundamental guidelines for giving them are listed in § 33. The definitions are not by themselves creative and, in my opinion, they must not be; they only introduce abbreviated labels (names), which could be dispensed with were the resulting lengthiness not to pose insurmountable external difficulties.[xi]

[41] For the purpose understanding Frege's intellectual development the loss of the "12 notebooks of 60 pages each, plus 1670 [!] [loose] sheets" of Frege's "Mathematische Einfälle" – a kind of intellectual diary, where Frege recorded ideas as they occurred to him (according to Scholz, who still had access to it, cf.: Veraart, "Geschichte des wissenschaftlichen Nachlasses Gottlob Freges und seiner Edition," pp. 100 sq.) – is perhaps the most lamentable. How enlightening such diaries can be, even when far less extensive than Frege's apparently was, will be amply illustrated by my discussion of Hilbert's intellectual diary in section 3.3

[42] It is also worth noting that the formulation Frege gives of it here[xxxvii] is nowhere near as terminologically refined as the later statements I have been discussing are: "associate" has an almost «psychologistic» ring to it, "sense" is given no precise meaning and in what form of "statement" one ought to use to that end is also left unsaid. This suggests, to my mind, that Frege, whether or not he adopted this principle freshly in 1885, he went on to revise and refine it substantially in the further course of his intellectual development.

Frege speaks of definitions being "by themselves creative" here, only to oppose the idea. This suggests that other writers Frege was engaging with at the time thought of definitions in this opposite way, as "by themselves creative"[43], and it was important enough to Frege to engage critically with their position to bring it into his discussion solely for that purpose. To explore the reasons Frege may have had already in the first volume of the *Grundgesetze der Arithmetik* for wanting to reject accounts which construed definitions as "by themselves creative", it will help to ask whose account(s) of definition Frege had in mind here. Frege mentions several mathematicians using creative definitions by name in the second volume, but since all of the creative definitions discussed there define irrational numbers (or other kinds of generalised numbers), which are not thematised in the first volume, those definitions are unlikely to be the target here. In fact, most of the authors criticised in the second volume are not even mentioned in the first at all[44]. I believe, rather, that these critical remarks are directed as somebody never discussed at any length in the body of either volume of Frege's *Grundgesetze der Arithmetik*: Ernst Schröder, and in particular the first volume of his *Vorlesungen über die Algebra der Logik*, is the likelier target for Frege's rejection of creative definitions in the first volume of the *Grundgesetze der Arithmetik*.

§ 26

That Schröder's work would have been looming large in Frege's mind as he was writing the (*Preface* to the) first volume of the *Grundgesetze der Arithmetik*, strikes me as particularly plausible for the following reasons:

First, Schröder's *Lectures* receive a brief (and derogatory) mention in the *Preface*, immediately before Frege launches into his second attack on creative definitions (which I referred to in footnote 43)[xl].

Second, Frege mentioned his wish to review Schröder's book as early as 1890 – the year Schröder's book was published –, as documented by a letter from the editor of the *Zeitschrift für Philosophie und philosophische Kritik*, Richard Falckenberg, dated June 16 1890. In this letter Falckenberg promises to send Frege a review copy of Schröder's book as soon as he receives one, and confirms, apparently in response to a proposal of Frege's (the letter from Frege to which Falckenberg is responding seems not to have survived), that he would be very interested in publishing Frege's review of Schröder's book[xli]. (Frege's review ultimately appeared in a different journal; since no other letters from or to Falckenberg survive, the reasons why will in all likelihood remain unknown.)

This letter suggests that Frege was eager to read Schröder's book when it was published in 1890 and thus would have (presumably) read it as soon as he could (in 1890 or 1891), and so plausibly before composing the preface to the *Grundgesetze der Arithmetik*, from which the above passage was taken[45].

Though the critical discussion note Frege eventually wrote appeared only in 1895, he seems to have read

[43] Similar remarks against attributing "creative power" to definitions are made a few pages later to criticise a "wide-spread inclination"[xxxix], which further suggests that criticism was (at least part of) Frege's motivation for his account of definitions.

[44] The exception is Dedekind, but even he is mentioned (in the *Introduction* of the first volume) in connection with his work on elementary arithmetic, *Was Sind und was Sollen die Zahlen*, rather than for his work on irrational numbers discussed in the second volume; cf.: Gottlob Frege. *Grundgesetze der Arithmetik*. Vol. 1. Verlag von Hermann Pohle, 1893, pp. 1 sqq.

[45] There was among Frege's papers a "convolute" entitled "Schrödersche Logik", which contained a draft (now lost) of Frege's review of the first volume of Schröder's *Vorlesungen*, and a short manuscript belabouring Frege's distinctions of concept and object, and sense and reference, now published under the title "Ausführungen über Sinn und Bedeutung" in Frege, *Nachgelassene Schriften*, pp. 128–136. *That* Frege saw these distinctions as related to his discussion of Schröder and worked them out for himself seemingly *after* first reading Schröder's *Vorlesungen* but *before* publishing his own review may even suggest that Frege's encounter with Schröder's work served as a catalyst for what have come to be regarded as Frege's greatest contributions to the philosophy of language.

Schröder's work, and made up his mind about it, as early as 1891 (and thus two years before the publication of the first volume of the *Grundgesetze der Arithmetik*), as a letter Frege wrote to Husserl in 1891 attests [46].

1.2.1.1 Schröder's Conception of Definitions

There can thus be little doubt that Frege was freshly aware of Schröder's views during the time when he would have been working on the first volume of the *Grundgesetze der Arithmetik*; and, as I pointed out in footnote 46, Frege did take issue with Schröder's conception of (creative) definitions in his 1895 review of Schröder's book. § 27

In any case, Schröder certainly speaks in ways Frege objects to, as the following passage documents:

> For as long as it remained unexamined whether there exist proper regions which possess the properties demanded by the symbols 0, 1, ab, $a+b$, we could say that our definitions | guaranteed the existence of what they define in the sense that they, as it were, produce it themselves or introduce it creatively.[xliii]

Figuring out what Schröder *meant by* this way of speaking, is another matter, however. Understanding Schröder's views on definitions is complicated somewhat by the fact that Schröder, unlike Frege, does not present a general account of definitions; indeed, Schröder disclaims the task of providing such an account, leaving his overall views (intentionally) to be inferred from the particular definitions he formulates[xliv]. Consider, for example, Schröder's definition of '+', which reads:

> If for any given regions a, b and c it holds that at once $a \not\in c$ and $b \not\in c$, then we shall say – more briefly – that $a+b \not\in c$. By this decree we have defined the identical sum as a subject [term].[xlv]

Schröder defines the sum of two regions here by stating a condition which such a sum must fulfill in order for a sentence containing it in a specific position to be true. So far as their role within Schröder's theory is concerned, these conditions are like Hilbert's axioms (thought Schröder intentionally does not call them "axioms" for reasons I will briefly discuss in footnote 159), and so Schröder's definitions are of a kind with the definitions in Hilbert's *Grundlagen der Geometrie* to which Frege objects. However, Schröder's conception of definitions differs from Hilbert's in one key aspect, which is what Frege targets when he attacks Schröder's belief in creative definitions: Schröder holds that definitions must be borne out without fail whenever the theory is applied to describe objects in the following sense: Whenever the conditions stated in a given definition are fulfilled – in the case of '+', whenever two different regions are both included in a single other region –, an object such as the definition describes – a logical sum of the two regions – must exist; or, if it does not within a given domain to which the theory is applied, such an object must be *taken to exist*, none the less. (Hilbert, by contrast, seems to presuppose that interpretations of a

[46] In Frege's first letter to Husserl (dated May 24 1891) Frege discusses certain details of Schröder's book, and Husserl's review of it, at some length; Frege even explicitly cites page 275 of Schröder's book, which comes after the page from which the passage about creative definitions quoted below is taken (p. 211f.). So, assuming that Frege was working through Schröder's book from beginning to end, he would have read that passage as early as 1891. In any case, Frege certainly noted the passage *when* he read through Schröder's book and, as his 1895 review attests, took issue with it: One of the main points of criticism Frege lists in his closing remarks there concerns definitions: "One cannot, by means of a definition, create an object with arbitrary properties [...]"[xlii], again using the same language of "creation" shared by Schröder and the preface to the *Grundgesetze der Arithmetik*.

theory are chosen so that their domains contain all objects required to make the axioms true; and interpretations whose domains lack objects are, for that reason, disqualified from being interpretations of the particular theory in question.) This is, I believe, the sense and the way in which Schröder takes definitions to be creative: Definitions carry with them an indefeasible existence-presupposition, as the following passage shows:

> As soon as we suppose that these [defined] symbols which we accepted as "regions" have a meaning, [that is, as soon as we] assert that there are real regions which have the properties in question, and which are accessible by means of intuition, we add to our definition postulates, [that is,] we demand that we can in general point to regions which are the meanings of our symbols, and can do so by making use of nothing but our intuition.[xlvi]

§ 28 According to Schröder's overall methodology, the construction of any theory proceeds through two separate stages. First, the formal theory is developed on the basis of symbolic laws and rules:

> We shall justify the laws of this [identitary[47]] calculus [of regions] first of all (with the aid of spoken language) in the general form of a mathematical sequence of deductions, for which the geometry of Euclid arose, in its time, as the paradigm, only to recognise later in retrospect that the inferences in this sequence of deductions also are only applications of the principles of this calculus themselves. Nobody who has any sense of the purity of the method and the stringency of the procedure will be untouched by the impression of the beauty and mathematical elegance of the resulting scientific system. One must, of course, overlook as much as possible all the flourish of the theory developed hereafter to gain that impression in untarnished form[xlvii]

In this process[48], new expressions can be introduced to aid the development by associating with them the symbolic laws governing their use[xlix]. Among the "flourish", Schröder includes any interpretation of the expressions of the calculus as claims about a particular domain of objects, as Schröder makes clear on the following page makes clear[l]. Interpretations, in Schröder's view, actually belong to a second, posterior stage in the development of a theory, where one moves to "apply" the theory, which requires assigning meaning to the expressions used:

> Concepts and propositions or formulas of the identitary calculus (also of the related logical [calculus]) will (in general) admit of the most diverse applications; applications which differ from one another only on account of the manner of making sense of, the interpretation of the symbols here used [as variables], and accordingly also of the operation- and relation-symbols linking them[49].[li]

The distinction Schröder is drawing between first introducing expressions and then, at a later point in time perhaps, assigning meaning to them (when "applying" the theory) is exactly what Frege's principle of meaningfulness forbids: It is inadmissible according to Frege's account to introduce expressions without at the same time associating them with a meaning (and no more than one), i.e. with a single *intended* interpretation. One way of accounting for this exact opposition between what Schröder proposes and what Frege's account forbids is that Frege adopted his account (partly) *in order to* disqualify Schröder's definitions from being definitions in a proper sense; and, in this way, to discredit one of the major rival efforts to Frege's own project of presenting a rigorous logical theory.

[47] I choose to translate Schröder's "identisch" using the neologism "identitary" because, though "identisch" is a perfectly fine German adjective, Schröder uses it in a distinctively abnormal sense to mean "concerned with idempotent operations". It seems preferable to me to render in English the abnormality of Schröder's use of the word, to literally translating its ordinary meaning.

[48] Later in the first lecture Schröder labels his way of proceeding a "strictly analytic method"[xlviii], by which he seems to mean that deductions must only rely on premisses which were explicitly stated, and not be aided in any way by intuition.

[49] Since Schröder officially accepts only one primitive symbol (as the passage I quoted in endnote xlix documents), '$\Subset$', to symbolise what he calls the "subsumption-relation", and regards his entire calculus as constituting "wholly and simply a study of subsumption", it will, in fact, suffice to assign meaning to the variables and the subsumption-relation-symbol.

§ 29 I find it worth noting in this connection that in his earlier mathematical work Frege seems to have accepted two important components of Schröder's (and Hilbert's) methodological framework, namely, that (some) concepts can be sufficiently defined by certain laws governing their use, and that the resulting theory can subsequently be "applied" in various ways[50]. Frege seems to have given up on this methodological approach later on – in fact, the 1885 lecture on formal theories of arithmetic, which already contains the not yet fully developed kernel of Frege's later account of definitions (cf. footnote 42), may be documenting the transitional period –, though, as far as I can see, Frege's extant writings unfortunately do not explicitly document his reasons for doing so.

§ 30 Be that as it may, the narrative I have been sketching – that Frege adopts his account of definitions *in order to disqualify* certain definitions – remains somewhat unsatisfactory. In its way, it seems to merely be pushing the problem of explaining Frege's motivation back one step, because it provides no answer to the further, obvious question why Frege would have wanted to disqualify definitions like Schröder's[51].

One reason why Frege could have wanted to reject definitions not immediately accompanied by the assignment of a meaning to the defined expression may have to do with the issues raised by the possible failure of interpretation: When discussing the interpretation of defined expressions, Schröder finds himself forced to consider the possibility that the objects which have been chosen as the domain of application turn out not to contain an object which is fit to serve as the interpretation of a particular defined symbol.

In such a case, Schröder says, an appropriate object must be "adjoined" to the domain[52].

§ 31 As Frege's review of Schröder shows, Frege construed Schröder's idea of "adjoining" objects to domains as an act of arbitrary creation. Given Frege's rejection of arbitrary creation in the second volume of the *Grundgesetze der Arithmetik* (though Frege had raised his main point of criticism already in his lecture on formal theories of arithmetic), Schröder's reliance on adjunction may have motivated Frege to want to reject Schröder's definitions because they may later turn out to require the adjunction – i.e. creation – of objects for their interpretation.

However, although Schröder does occasionally use the vocabulary of "creation" in his book, as the passage I quoted in endnote xliii shows, that phrase seems to carry a far less substantive sense for Schröder than it does for

[50] So, for example, in his Habilitationsschrift on "methods of calculus founded on an extension of the concept of magnitude", for example, Frege maintains that the concept of magnitude is ultimately characterised only by the fact that the laws of addition hold for it[lii]. Frege then goes on to use the very words he will later sharply criticise, saying that since "we ourselves create [!] the concept of magnitude", we can shape it "so as to allow for as wide an applicability as possible"[liii].

[51] To my mind one plausible, though entirely speculative account of what underlies Frege's predilection for ruthless criticism is a socio-psychological explanation, such as the one sketched by David Bloor. *Knowledge and Social Imagery.* 2nd ed. University of Chicago Press, 1991, pp. 93 sq., who observed: "[Frege's *Grundlagen der Arithmetik*] is steeped in the rhetoric of purity and danger, and full of the imagery of invasion, penetration, disparagement, contempt and the threat of | ruin [...]. It is a veritable picture of knowledge under threat.". By casting his opponents as inept to recognise problems and confusions Frege presents as obvious and fundamental; Frege engaged his opponents on a field where he made the rules and thus set up for himself a domain where he was the undisputed master. Frege's true talent, according to this line of thought, lay in his rhetorical gift to make people forget that those problems Frege raised were, at the time, far from recognisable as obvious or important to everybody else.

It has, in any case, been observed by scholars of Frege's work for some time now that what at first sight and to someone who has no first-hand acquaintance with the work of those Frege criticises may *seem like* decisive refutations are, in point of fact, mostly testaments to Frege's tendency to uncharitably misconstrue those he criticised in order to score points which more often than not pass the intentions and projects of the figures in question by completely.

[52] Schröder in fact thinks this is always the case with the defined symbol '0'[liv], and that, depending on which domain of objects is at issue, it can also happen that other defined symbols find no interpretation in the domain at hand[lv].

Frege. Admittedly, Schröder has no substantive positive account of adjunction to offer, and the details of his view remain shady: Schröder seems, rather, to think of adjunction as a kind of make-belief. After one has chosen a certain domain of objects for the interpretation of a given theory one must make-believe that objects appropriate for interpreting the defined expressions are part of the domain in order to do justice of what I called (on p. 17) the "indefeasible existence-presupposition" of definitions, as Schröder conceives of them.

Schröder describes the status of the resulting "adjunct regions" in a variety of ways as "fictional, imaginary or supposed region[s]"[lvi]. While it remains unclear what status such regions have, exactly, it is, to my mind, quite clear that for Schröder such regions have a distinctively different status than non-adjunct, «natural» regions; so even if they can be regarded as, in some sense, "created", what is thus created is – unlike in the accounts Frege criticises – not of a kind with the objects it supplements; indeed, Schröder does not regard adjoined objects as objects in the same "proper" sense as the «natural» objects in a given domain, but rather as "improper".

§ 32 Frege furthermore assimilated Schröder's position to the formalist position Frege would later criticise in the second volume of the *Grundgesetze der Arithmetik*, claiming that Schröder regards the symbol '0' *itself* as the (newly created) object serving as its own interpretation[lvii]. Central to Frege's criticism of formalist accounts, as I understand it, is the claim that the signs themselves cannot serve as interpretations (and if they seem to, that is because they are, after all, taken to have an interpretation of their own)[53]. However, it seems to me that in this respect, too, what Schröder actually says about the interpretation of, e.g., the symbol '0' (in the passage quoted in endnote lvi) does not support Frege's attribution. Schröder explicitly speaks of a(n adjoint, improper) "null-region", which is associated with the symbol '0', and does not seem to hold that this region *just is* the symbol '0'.

At the bottom line, it seems to me that when Schröder's account is considered on its own terms, independently of Frege's distorting presentation, it falls through the cracks of the classification of positions underpinning Frege's critical discussion in the second volume of the *Grundgesetze der Arithmetik*: Schröder neither regards the expressions "created" by definitions as meaningless, nor believes, as Frege's Formalists do, that the symbols *themselves* are the objects; nor does Schröder subscribe to a Creationist view in the sense Frege targets in the second volume of the *Grundgesetze der Arithmetik*, which allows for the creation of new (proper) objects[54].

1.2.2 Ultimate Motivation: «Uncontroversial» Grounding for Theories

§ 33 I want to turn, for one last look, back to Frege to consider a further possible answer to the question why Frege may have wanted to debar definitions which have no interpretation directly attached to them. I think another «deeper» motivation for this attitude derives from a principle Frege appeals to both in his discussion with (and of) Hilbert('s

[53] This point of criticism appears in the lecture on formal theories of arithmetic as Frege's opening salva[lviii]. The same criticism is later re-iterated in the second volume of the *Grundgesetze der Arithmetik*[lix].

[54] Schröder's position strikes me as of a kind with the unobjectionable creationist position Michael Hallett isolates in his "Frege on Creation," where the creation involved "is, as it were, linguistic, logical and conceptual" and "not adding to the material of the mathematical world *ex nihilo*" (Michael Hallett. "Frege on Creation." In: *Essays on Frege's Basic Laws of Arithmetic*. Ed. by Marcus Rossberg and Philip A. Ebert. Oxford University Press, 2019, pp. 285–324, p. 310).

work), as well as in the second volume of the *Grundgesetze der Arithmetik*: that the consistency of a given theory can be established only by exhibiting «uncontroversially existing» objects (ultimately, perhaps, objects «given in intuition»), which the theory at issue can be taken to describe.

One of Frege's reasons for rejecting creative definitions derives from this principle: Frege takes it to be commonly conceded that creative definitions cannot define objects with contradictory properties. Since Frege believes that the consistency of the ascriptions a definition uses to characterise the object defined can only be established by exhibiting such an object, creation turns out to be redundant. Legitimising any given creative definition presupposes that one can find an object of the kind defined prior to creating it through the definition[55].

Given that Frege (mis)construed Schröder's talk of "adjunction" as invoking a kind of creation, everything I said previously about Frege's motivation for rejecting Schröder's definitions on account of their creativity can be seen as further motivated by this principle that establishing consistency requires exhibiting objects.

One may further speculate that this principle was the root of Frege's later convictions, as I summarised them at the beginning of this subsection (in § 22), insofar as the principle that the viability of any theory must be justified by exhibiting «uncontroversially existing» objects the theory describes is *weaker than* the principle that any theory must, from its inception, be associated with a particular *intended* interpretation; so the latter might plausibly constitute a further development from the former.

This principle finally seems to be a true constant in Frege's intellectual life. § 34

He clearly held it as early as 1885, when he appeals to it in his lecture on formal theories of arithmetic[lxiii].

Yet Frege also still appeals to it prominently and with firm conviction in his exchange with Hilbert.

A version of it can, I believe, even be recognised as underlying Frege's earliest (mathematical) work: In his dissertation on "the geometrical representation of planar imaginary figures", Frege develops an interpretation of imaginary geometrical figures (i.e. geometrical objects described in analytic geometry by equations having solutions (only) in the complex numbers[56]) in terms of geometrical objects directly accessible to intuition. While Frege's interest lies mainly with the development of the representation itself, he frames this development as responding to "a need to see with our eyes" even such imaginary figures[lxiv]. Frege seems to regard having representations which make all kinds of figures accessible to intuition as valuable in itself. One may speculate that the value of such a grounding of theory in intuition arises from the belief that only by finding intuitive representations of imaginary figures could establish the veracity of the theory[57], because only objects (capable of being) given in intuition «exist

[55] This criticism is presented in §§ 143f. of the second volume of the *Grundgesetze der Arithmetik*[lx].

Some writers, working with frameworks less sophisticated than Schröder's, did indeed claim that one could define anything as long as no contradictions became apparent. Thomae, for example, claimed that "the existence of new [kinds of] numbers is grounded solely in the fact that the rules of calculation abstracted from the integers can be applied without giving rise to contradictions."[lxi].

Schröder himself was more careful, however (and in a way remarkably like Frege's). Schröder held that one could really be certain of the freedom from contradiction of a theory once an interpretation of it using objects given in intuition had been provided[lxii].

[56] For some background on this work, see Mark Wilson. "Frege's Mathematical Setting." In: *The Cambridge Companion to Frege*. Ed. by Thomas Ricketts and Michael Potter. Cambridge University Press, 2010, pp. 379–412.

[57] Another short mathematical lecture held in 1883 also testifies to the importance Frege attributed early on to basing theory on intuition. In this lecture on "the geometry of pairs of points in a plane", Frege calls it one of the most consequential recent advances of analytic geometry that it allows an intuitive representation of higher-dimensional figures[lxv].

uncontroversially» and to be true a theory must describe «uncontroversially existing» objects.

The sentiment that finding objects given in intuition for a theory to describe served to cement the theory's status as true (or at least possibly true) was, in fact, a common sentiment in the late 19th century. One finds it prominently articulated, for example, by Hermann Hankel in his *Vorlesungen über die Complexen Zahlen und Ihre Funktionen*, where Hankel maintains that the question whether (generalised) numbers exist comes down to

> whether in the realm of the real – i.e. what is present to intuition, the actual – there are substrates of them, that is, whether there are objects which exhibit the intellectual relations of the relevant kind in appearance[lxvi]

§ 35 Although Frege eventually came to believe that there were also «uncontroversially existing» objects *not* accessible to intuition (at least when "intuition" is taken to mean "sense perception") – such as the "logical objects", among which Frege includes courses-of-values and functions[58] –, even then he still held that the veracity of a theory depends on exhibiting objects which the theory describes; Frege came to accept a wider array of «uncontroversially existing» objects, not a different principle about what they were needed for[59].

1.2.3 Last Words and Further Projects

§ 36 Without wanting to try to settle the question what really motivated Frege to accept the account of definition he accepted, I hope the discussion in this chapter has clarified what the main fundamental commitments of Frege's account of definitions were, how Frege employed them in his critical discussion of Hilbert's work (among others), and what may have motivated Frege to adopt them.

Given the importance Frege himself assigns to his account of definitions in both volume of the *Grundgesetze der Arithmetik*, and the central role it seems to play in his controversy with Hilbert, providing a fuller story, insofar as this is possible on the basis of the scant evidence available, of how Frege came to adopt his view of definitions, and how it developed, strikes me as a worthwhile endeavour. The project would consist in trying to narrate Frege's intellectual development around his developing views on what is required to state proper definitions in mathematics; in particular, linking the development of these views to other doctrines Frege eventually developed, like the distinction between sense and reference, could potentially be quite enlightening.

In fact, Frege himself was conscious of having come a long way in developing his views on definitions and of his oppositional position, as he tells Hilbert in his second letter: "In thinking about definitions my requirements have become ever more rigid and in that way I have probably distanced myself so far from most mathematicians that

[58] Cf.: Gottlob Frege. *Grundgesetze der Arithmetik*. Vol. 2. Verlag von Hermann Pohle, 1903, p. 149. In an unpublished manuscript, Frege speaks of "logical capacities" by which we "take charge" of courses-of-values[lxvii].

[59] One notable precursor of Frege's view that definition in the proper sense of the term must always be accompanied by the construction of an object fitting the definition in Kant. Kant discusses definitions at some length in the first section of the second main part of the *Kritik der reinen Vernunft*, the so called "Transzendentale Methodenlehre", and comes to the conclusion that only in mathematics can there be definitions in the proper sense of the word[lxviii]. In the course of the discussion leading to this conclusion, Kant also discusses the possibility of "defining concepts thought-up arbitrarily", which Kant rejects "because I cannot say that I have thereby defined a true thing"[lxix]. Frege seems thus to have accepted one key tenet of Kant's philosophy of mathematics for himself, namely, that objects must be given and cannot be «thought-up».

communication has become very difficult."[lxx]; and seems to have thought about the topic of definitions continuously throughout his career, as certain (now lost) portions of his intellectual diary suggest[60].

I will now move on to focus on reasons for rejecting the principles underlying Frege's account of definitions.

My motivation for discussing Frege was, after all, to figure out the reasons why Hilbert could so easily ignore Frege's criticism, and to understand what it said about Hilbert's philosophical standpoint that he did so.

To that end, I will discuss at some length the work of Alwin Korselt, who, when Frege published his article criticising Hilbert, took it upon himself to publicly defend Hilbert's position. As I already mentioned in § 21, the key to Korselt's defensive strategy lies in rejecting specifically the assumption I labelled (III) above (in § 20), that assigning meaning to an expression requires assigning a particular and uniquely determined extension to it.

§ 37 To raise one leading question, I want to note here that the question how one can reconcile a firm commitment to the principle of univocity with the Hilbert's method of ever-possible re-interpretation (or, more generally, with the rejection of Frege's commitment to assigning unique extensions) strikes me as a real, and difficult, puzzle. Frege himself, in fact, already noted this conflict between the principle of univocity and the admission of variable interpretation in his second series of articles[lxxii]. It seems also to underlie Frege's charge that Hilbert hadn't falsified the parallel-postulate in his non-Euclidian counter-model, but rather an altogether different claim which he merely *equivocally called* "the parallel postulate"[lxxiii]. My discussion of Korselt's work in the upcoming chapter will explore one way of dealing with this puzzle: by «thinning out» the account of "meaning" relevant to the principle of univocity: If determining an extension was not part of what the meaning associated with an expression did, then associating different extensions with the same expression on different occasions need no pose a problem.

§ 38 Hilbert own response to the puzzle I just raised, insofar as it can be reconstructed (on the basis of two entries in the first volume of his scientific diary (which I will discuss at length in section 3.3), appears also to revolve around «thinned out» conception of meaning. On the one hand, associating an expression with a unique extension appears to have been what characterised definitions in Hilbert's view, as the following diary-entry shows:

> This definition[61] suffices, in any case; whenever we are presented with any thing one can decide on the basis of this definition whether the thing is a series-law or not. But that is in my opinion really the only mark of a definition. Therefore a definition using negative characteristics is not deficient.[lxxiv]

Another entry found slightly earlier in the first volume of Hilbert's scientific diary suggests that Hilbert (at least at the time) adhered to a conception of definitions as what I called "syntactic abbreviations" (on page 4):

[60] According to Scholz' (unfortunately minimal) report of the contents of Frege's intellectual diary, Frege thought about definitions from very early on in his career: The first volume (of 12) of Frege's intellectual diary contains, according to Scholz, the idea (apparently new to Frege at the time, since he recorded it in his diary for "mathematische Einfälle") that the defined expression must contain exactly the same free variables as the defining expression[lxxi]. Then in volume 6, which Scholz dated (without recording his reasoning) to the year 1879, Frege was thinking about the "definition of objects" (Veraart, "Geschichte des wissenschaftlichen Nachlasses Gottlob Freges und seiner Edition," p. 100); and in volume 7, presumably contemporaneous with the second volume of the *Grundgesetze der Arithmetik*, about partial definitions (see: Veraart, "Geschichte des wissenschaftlichen Nachlasses Gottlob Freges und seiner Edition," p. 101).

[61] The definition in question is a definition of irrational numbers as "bearers of series-laws", which Hilbert develops seemingly as a response to Kronecker's criticisms; I will discuss it at some length in section 3.3.4.2.

> A definition can never be "false"[,] nor can it ever be illogical. Because it is something purely linguistic. Accordingly it can be unintelligible or admit of multiple interpretations. Only in that case [i.e. when a definition is unintelligible or ambiguous] can it be discarded as useless without further reflection. The word being defined can also be in conflict with common usage. That, however, is not intrinsically problematic.[lxxv]

Note that this passage, in any case, shows Hilbert's firm commitment to what I called the "principle of univocity". Hilbert thus seems bound by the puzzle I just raised to deny that extensions have any connection with meaning and thus committed to a «thinner» conception of meaning than that held by Frege, according to which, whatever «meaning» – if any – is assigned by definitions, the relevant sense of "meaning" does not determine extension: If definitions assign meaning, and meaning determines extension, then re-interpretation would be impossible (if re-interpretation is construed as involving a change of extension associated with a given expression).

Chapter 2

Korselt's Work

Since Hilbert's own work gives no clear indication about his view on definitions – the two entries from his scientific diary are, as far as I am aware, all Hilbert says on the topic of definitions at the time of his exchange with Frege –, I turn to other people who thought of themselves as defending Hilbert's position. Hilbert does comment on *other* methodological and philosophical issues in his writings, both published and unpublished, dating from that time, but that he does not comment at any length or in any significant detail on definitions specifically. § 39

I will discuss Hilbert's writings from that time in sections 3.1.4 and 3.2.

2.1 Korselt himself

After the first series of Frege's essays criticising Hilbert's *Die Grundlagen der Geometrie* appeared in the *Jahresberichte der Deutschen Mathematiker Vereinigung* in 1903, Alwin Korselt took it upon himself to respond to Frege's critique; Korselt's first response, in the form of a short note, appeared in the next monthly fascicle of the *Jahresberichte*. Korselt published two articles dealing directly with Frege's criticism of Hilbert: first the short note I just mentioned, published immediately after Frege's 1903 articles, then another – substantially longer – article published later in 1908, responding to the second series of critical articles by Frege which appeared (once again in the *Jahresberichte der Deutschen Mathematiker Vereinigung*) in 1906.

The fact that Korselt took it upon himself to defend Hilbert's position is in itself not unremarkable because Korselt never studied with Hilbert himself[62]. So Korselt apparently convinced himself of the merit of Hilbert's § 40

[62] Lothar Kreiser. "Die Hörer Freges und sein Briefpartner Alwin Korselt." In: *Wittgenstein-Studien* 2.1 (1995), pp. 1–106, section 3 contains a biography and reproduces some pertinent documents (e.g. the reports on Korselt's dissertation, a list of lectures Korselt attended at university in Lepzig (by Hankel, Klein, Lie, Schur and Wundt, among others)) and recollections, and a list of the few remaining "Nachlasssplitter" Kreiser succeeded in locating. One of the recollections Kreiser quotes from (written by a former student of Korselt's called "Heinrich Koeberlein") mentions that Korselt corresponded with Hilbert. Of this correspondence, if, indeed, it took place – with what authority Koeberlein can speak on details of Korselt's life remains unclear –, no trace remains, as far as I can see, among Hilbert's papers; most of Korselt's papers have been lost after his death, including the letters from Hilbert or Frege, if any existed; some letters Korselt wrote *to* Frege are edited along with other surviving correspondence in Frege, *Wissenschaftlicher Briefwechsel*.

position independently of any personal connection on the basis of Hilbert's writings.

Korselt also engaged with Frege's own, constructive work in a critical discussion note of the second volume of the *Grundgesetze der Arithmetik* (published in 1905 as "Über die Grundlagen der Mathematik") and in a brief remark in an article written in response to an article by Schoenflies (published as "Über mathematische Erkenntnis" in 1911, where Korselt *defended Frege* against Schoenflies' criticism of Frege's account of definitions)[63].

According to a list made of Frege's posthumous papers before their destruction, Frege drafted a further response to Korselt's second article, which was, however, never published and whose manuscript has not survived[64]. So it cannot be ascertained whether Korselt succeeded in making Frege change his mind about Hilbert's work on the whole, or about any particular aspect of it, and, if so, to what extent and in what way.

Scholz' brief description of the content of this lost manuscript mentions that Frege was discussing "[the role] of meaning in science; concepts [...]; components of sentences; [...] sense and reference" in the manuscript, which – together with the fact that Frege felt he needed to write a further response to Korselt in the first place – suggests to my mind that he did not accept Korselt's position[lxxvii]; but for some reason Frege never completed work on this manuscript, nor seems to have attempted to publish it. To suggest one possible explanation of why Frege may have given up on this manuscript, I want to point to another another manuscript, also now lost, described by Scholz: Among Frege's posthumous papers Scholz found an unpublished manuscript criticising Thomae (item N[achlass] 79 on Scholz' list) which Frege *had* completed and submitted for publication (in 1907), but which was apparently rejected by the editor of the *Jahresbericht der Deutschen Mathematiker Vereinigung*[65]. It strikes me as possible that the rejection of this critical discussion of Thomae discouraged Frege from investing the time and energy required for finishing (and submitting for publication) the manuscript of his second critical article against Korselt; another example of an article dating from this period which Frege either never submitted to or which was rejected by the *Jahresberichte* is, according to the editors, the (surviving) article-manuscript criticising Schoenflies[66].

About the reasons for the rejection of Frege's article criticising Thomae (and the article criticising Schoenflies, if, indeed, Frege submitted it for publication) one can, as far as I see, only speculate: It seems to me, one possibility is that after Frege's relentless criticism of Hilbert, the editors of the *JDMV* were wary of Frege and disinclined to publish any more critical pieces written by him. Another possibility is that Frege eventually came to the conclusion

[63] Frege himself briefly addresses Schoenflies' criticisms of his conception of definition in, but without engaging at all with the substance of Schoenflies' criticisms: Frege simply rejects Schoenflies' criticism as depending on a word whose meaning remains unclear[lxxvi].

Beyond his exchange with Frege, Korselt published some work on the algebra of logic and set theory. The editors' introduction to the Frege-Korselt-correspondence presents a brief overview of Korselt's work, see: Frege, *Wissenschaftlicher Briefwechsel*, pp. 140 sq.

[64] Scholz's list of Frege's manuscripts (see footnote 8) mentions such a manuscript as item N[achlass] 87 (which is marked as lost) a "reply to Korselt's ['Über die Logik der Geometrie'] of 13 pages and further drafts" (see: Veraart, "Geschichte des wissenschaftlichen Nachlasses Gottlob Freges und seiner Edition," p. 100).

Nor have any letters between Frege and Korselt on the topic of Hilbert's work survive: The editors of Frege's correspondence mention at least one (lost) "postcard" about "axioms as definitions", written by Korselt and dated January 5 1920 (! — this is item XXV/5 in the editors' numbering of Frege's correspondence; if the date ascribed to this postcard by the editors (on the basis of lists written by Scholz) is correct, then Korselt wrote this postcard more than 10 years after his public exchange with Frege); as far as the editors of Frege's correspondence seem to be aware, Frege and Korselt exchanged no other letters between 1903 and 1920.

[65] See: Veraart, "Geschichte des wissenschaftlichen Nachlasses Gottlob Freges und seiner Edition," p. 99.

[66] See the editorial introduction to the text in note 1 on: Frege, *Nachgelassene Schriften*, p. 191.

that he would just be wasting his time by engaging with Korselt (or Schoenflies) any further and for this reason decided to not submit the critical pieces for publication; however, since Frege seems never to have lost his taste for criticism[67], I find it hard to imagine that Frege would have, of his own volition, decided to let the matter be.

I hope, in any case, to show that Korselt's worries and suggestions deserve some attention[68].

Korselt's life. Before discussing Korselt's work in any detail, however, I want to briefly summarise what is known § 41
about Korselt's life and personality: In his compilation of biographical material Lothar Kreiser claims that "the years from 1903 until 1915 were Korselt's scientifically most productive time". The overall picture of Korselt which emerges from the documents Kreiser compiles, however, is that of an unremarkable, ultimately failed life: Korselt appears to have been unpopular, somewhat querulous, and unfit to be a school-teacher but at the same time not academically brilliant enough to find recognition, much less work as a practising mathematician.

In 1937, Korselt submitted a manuscript entitled "Eine Fortbildung des Schröderschen Logikkalküls" to the *Mathematische Annalen* for publication. The manuscript was rejected (at least in the form submitted) on the recommendation of Heinrich Behmann, who reviewed Korselt's manuscript (at Bernays' request), and has subsequently been lost. From what can be inferred about its contents and character, this manuscript appears to have constituted a sum-total of Korselt's life's work in three respects: (i) both Bernays' initial description and Behmann's referee-report suggests that the manuscript was rather long[69]; (ii) it seems to have presented not only results Korselt obtained in the years leading up to 1937, but, in fact, also results from much earlier, which were, as Behmann writes, "already known" (in one instance, because Löwenheim had published a result he explicitly credited to Korselt); (iii) although mainly of technical interest, the manuscript appears to have contained lengthy sections discussing philosophical issues or technical issues in a philosophical perspective[70]. The loss of the manuscript is thus all the more regrettable, because without it nothing much remains of whatever work Korselt did in the three decades following his public controversy with Frege. The only evidence now remaining which gives any indication about the contents of Korselt's lost manuscript are, as far as I am aware, the letters Bernays and Behmann exchanged, as well as the referee-report Behmann wrote (and sent to Blumenthal, then editor-in-chief of the *Mathematische Annalen*), which are reproduced in appendix A (with kind permission of the Handschriftenabteilung of the Staatsbibliothek Berlin).

I will present Korselt's work in two sections: First, I will focus on Korselt's (counter-)criticism of Frege's § 42
critique of Hilbert. As I said above, I take Korselt's strategy of defence to focus on rejecting Frege's requirement

[67] So, for example, Frege spent a fair amount of time in his 1914 lecture entitled "Logik in der Mathematik" bashing Weierstrass in much the same manner Frege bashed Korselt in 1906 (see: Frege, *Nachgelassene Schriften*, pp. 235–245).

[68] The only study dedicated to Korselt's work I am aware of is a survey by Volker Peckhaus. "Alwin Reinhold Korselt." In: *Arbeitsberichte aus dem DFG-Projekt Fallstudien zur Begründung einer Sozialgeschichte der Logik* 6 (1986). Ed. by Volker Peckhaus, pp. 10–24, which contains a brief biography (sections II & III; based, in part, on the sources reproduced in Kreiser's compilation of biographical materials), an annotated bibliography of Korselt's publications (section IV), and an evaluation of Korselt's work (sections V and VI). In his evaluation, Peckhaus claims that Korselt was an unoriginal thinker; however, that judgement appears to be based primarily on the fact that most of Korselt's publications took the form of discussion notes of other people's work[lxxviii].

[69] Bernays wrote in the letter he sent to Behmann that, if at all, it could be published only in (two) instalments; Behmann's report includes occasional page-references which refer to pages up to a page numbered "80".

[70] Passages of philosophical content are mentioned by Behmann (see A.2.1) and also by Bernays (see A.1), both of whom were of the opinion that they should (for the most part) be cut because they contributed nothing to the technical portion of the article.

that definitions in the proper sense must assign a uniquely determined extension to the expression they define.

Korselt raised one objection to this aspect of Frege's conception of definitions, in particular, to which Frege seems to have had no satisfactory response. I believe Korselt thus exposed a genuine weakness of Frege's views.

Second, I will turn to the positive proposal for a reconstruction of Hilbert's work which Korselt developed in his longer 1908 article as an alternative to the reconstruction Frege had proposed (in his 1906 articles).

Fundamental to Korselt's work, as I understand it, is the conviction that precisely defined theories must not be limited to describing a single «intended» domain of objects. They must, rather, admit of as wide a variety of interpretations as possible. In this respect, Korselt appears to embrace a meta-theoretical standpoint Frege sought to discredit and avoid. Korselt's work, especially his later article published in 1908, can be construed as an effort to articulate a cogent methodological framework which allows for the wide applicability of formal theories.

As interesting as the fact that Korselt championed such a position are his reasons for doing so.

The Value of Theories. I believe Korselt's approach to theories is shaped fundamentally by a particular view he has of the purpose mathematical theories serve in the project of human inquiry overall. Not only does Korselt see no point in singling out particular interpretations of a theory as the only («intended») interpretation, Korselt goes as far as to suggest that the value theories posses attaches to them independently of any interpretation. § 43

Korselt seems to think that emphasising the need for interpretations of theories – or insisting, furthermore, that one interpretation must be singled out as *the* («intended») interpretation of a theory – is symptomatic of a certain conception of the value of theories, and the role the project of human inquiry: Underlying this emphasis, according to Korselt, is the assumption that theories have value as a result of describing (important) portions of reality.

Frege did indeed hold such a view, and expressed it quite clearly in the unpublished manuscript I mentioned in footnote 45 that if the terms used in scientific claims did not name particular, definite objects "our reflection comes to naught"[lxxix], and "belongs to poetry, not to science"[lxxx]. § 44

With respect to geometry, in particular, Frege expounds this view in an unpublished manuscript entitled "Über Euklidische Geometrie", where he says that "we care most with respect to any given thought about the question of its truth", which in turn depends on the question of its meaning (which would, according to Frege's conception of meaning, have to determine a unique extension)[lxxxi]. In his 1914 lecture, Frege even asserts that two individuals who associate different extensions with the same scientific terms are engaged in altogether different science[lxxxii].

Korselt suggests, to the contrary, that theories have distinctive value of their own on account of exhibiting complex and rigorously developed deductive relations. In the project of human inquiry overall, theories serve the purpose of organising and thereby refining knowledge by systematising it: In his first article against Frege, Korselt claims that "every science strives to take the form of the simplest possible, objective [in the sense of 'at least satisfiable by some structure'] formal theory"[lxxxiii]. The value Korselt seems to see in theories is that they contribute to the systematisation of knowledge by providing a common reference-framework – the deductive § 45

structure of the theory – by way of which all its different interpretations can be understood as unified, and enable scientists to judge whether they can account for all the relevant phenomena[71]. On this view, theories do not have value on account of describing reality, but rather on account of unifying and systematising knowledge[72].

This perspective on theories implicit in Korselt's claim (made in his 1903 article) that deductive webs[73], such as one finds in a rigorously developed mathematical theories, require no further boost to their "dignity" due to describing particular (dignified) objects; such a theory has enough value on its own:

> Whoever wants a logical and as simple a connection of all our knowledge as possible must therefore not demand that every formal theory be objective [in the sense of "having a model"] from the start. One may call it "empty game of expressions, meaning nothing" and the like; as a strictly lawful connection of sentences it does not need any more "dignity". A formal theory is noteworthy as long as the sentences occurring in it do not lead to a contradiction; indeed, even a contradictory theory remains interesting if the contradiction only emerges after a significantly long sequence of inferences about the objects of the theory.[lxxxvi]

§ 46 Especially striking in this passage is Korselt's claim that even contradictory theories can be valuable. Korselt's explanation suggests that the extent to which even a contradictory theory can be considered valuable depends on how complicated a chain of deductions is needed to bring the contradiction inherent in the theory to light.

The value of theories, as Korselt seems to conceive of it in this passage, apparently depends (entirely?) on how complex the deductions carried out within that theory are; how much they contribute towards the aim Korselt refers to at the outset of the passage, establishing "a logical and simple connection of cognitions". On the other hand, the value of a theory has, as far as this passage goes, nothing to do with the objects it describes; but rather with how the theory describes these objects and how it reasons about them.

§ 47 Frege in fact seems to have conceded already in the second volume of the *Grundgesetze der Arithmetik* that considerable intellectual effort was required to develop any precisely defined theories rigorous, perhaps even the same kind and amount of intellectual effort as the development of Frege's own («interpreted») deductive system had required of him. In § 90 Frege grants that it would have been possible to develop his deductive system in a purely formal fashion without thinking about the meaning of the symbols, granting that "obviously this would require intellectual effort (just like when solving chess-problems)", but such developments "have their interest conferred on them only by [expressing] a train of thought"[lxxxvii]. Still, Frege held that it made a decisive difference whether one understood the formal theory as the expression of particular thoughts (about particular objects).

Thinking about the source of whatever value is ascribed to a theory provides a way of clarifying what is at issue between Korselt and Frege in this connection: Unlike Korselt, Frege seems to have held that the value of a theory

[71] Korselt formulates these goals as (respectively) as "forming connections between as many different experiences as possible" and "seeing whether completeness, purity and independence of the basic principles and basic concepts has been attained"[lxxxiv].

[72] That there was value in systematising knowledge seems to have been not entirely alien to Frege: In his (unpublished) discussion of Boole's logic, Frege asserts that "the value of explanation consists precisely in the compression and simplification accomplished"[lxxxv], though Frege seems not have been inclined to see such systematisation as the main source of the value of theories (but rather only as a criterion for preferring some theories (with a narrower base of primitive expressions and principles) over others (with a wider base).

[73] Korselt uses the metaphor of a web in his 1903 article to illustrate how the primitives of a theory relate to one another: They form "a web in which one may move from one to any other, but which can still not be resolved into a single thread" (Alwin Korselt. "Über die Grundlagen der Geometrie." In: *Jahresberichte der Deutschen Mathematiker-Vereinigung* 12 [1903], pp. 402–407, p. 406).

derives from the importance of the objects it describes, rather than, as Korselt thought, merely from the intellectual labour epitomised by the theory, the efforts required to develop the deductions.

§ 48 Section 90 of the second volume of Frege's *Grundgesetze der Arithmetik* supposedly served as the basis for Leopold Löwenheim to subsequently convince Frege (in a "lengthy correspondence", which is tragically lost) that the formal approach to arithmetic was unobjectionable and perfectly viable[74].

One finds a remark of a seemingly similar spirit in an unpublished manuscript (dated by the editors to around the same time as the second volume of the *Grundgesetze der Arithmetik*) entitled "Logische Mängel in der Mathematik", where Frege explicitly identifies "the how [...] and the insight into logical connections" as the source of the value of mathematical knowledge[lxxxviii]; how such remarks cohere with Frege's commitment to seeing the truth of a theory as the source of its value (see footnote 44), remains, to my mind, a question to be resolved; one possibility, substantiated by the Löwenheim-story, is that Frege changed his mind about the source of value of deductive theories not too long after the publication of the second volume of the *Grundgesetze der Arithmetik*.

The best evidence to suggest that Frege did, indeed, change his mind about the purpose theories serve is, so far as I can see, a late manuscript entitled "Meine grundlegenden logischen Einsichten"[75]. In this text, Frege modifies his earlier view that logic was built around the concept of truth in the same way ethics was build around the concept of goodness and aesthetics around the concept of beauty[lxxxix] as follows:

> So the word "true" seems to make the impossible possible, namely to make whatever corresponds to assertive force appear as a component of the thought. And this attempt shows, even though it fails or rather because of its failure, a peculiarity of logic, which thus seems to be essentially different from aesthetics and ethics. Because the word "beautiful" really does indicate the essence of aesthetics, and so does "good" for ethics, while "true" really makes only a failed attempt to show [the essence] of logic, since what really matters [in logic] isn't the word "true" at all, but rather the assertive force with which a sentence is enunciated.[xc]

While this passage is far from a complete renunciation of *all* of Frege's earlier views, even the slight change in focus – from truth to assertive force – strikes me as significant. By placing assertive force, rather than truth, at the heart of logic, Frege appears to shift his attention away from the question whether the sentences people assert are true to the question, perhaps, what reason these people have for asserting these sentences; away from whether sentences accurately describe the world to whether they are embedded in a suitable deductive web.

§ 49 Korselt had reasons to disagree with Frege's earlier conception of numbers and of the value of theories as due to describing a certain reality. Korselt's strongest reason, as I see it, was that, in Korselt's view, Frege's requirement that theories be construed as describing particular, uniquely determined objects was not feasible in principle. No theory *could*, even in principle, be associated with a *uniquely determined* class of objects; and since one could thus not speak of "*the* objects described by a theory", no value judgements about the theory could be made on that basis. I want to focus on two of Korselt's (in my opinion) best reasons for thinking that Frege's standards were

[74] This information is reported (third-hand) by the editors of Frege's correspondence, who cite Bernays' recollection of an exchange he had with Löwenheim: Frege, *Wissenschaftlicher Briefwechsel*, pp. 157 sqq.

[75] See Frege, *Nachgelassene Schriften*, pp. 271 sq. Scholz originally dated this short text to 1915.

unreasonable (on account of being ultimately impossible to fulfil).

2.2 Korselt's Negative Contributions

§ 50 Both reasons I will focus on target the principle I designated (III) above, i.e. that a meaning expression must be associated with a uniquely determined extension. I believe Korselt's intention to dispute this principle first manifests in the opening remarks of his 1903 article, where Korselt claims that one must pose the question "what claiming that an expression has meaning *means* anyway", and goes on to propose a different conception of meaning:

> Why should axioms and theorems not contain expressions whose "meaning" was not previously determined? In the wake of that one must continue asking: What does the claim that the meaning of an expression is determined "mean" anyway? Many things. Either: that one recognises the expression where it occurs as the same symbol for the same object, or: that one recognises some or all simple sentences using the expression always as true, false or definitions. [...] If one were unable to agree on the "meaning" of an expression, then that is a sign that one of the disagreeing parties must acquire more sentences about this expression, or containing it. "The expression has no meaning" will thus amount to: "We do not know any sentence which regulate the use of this expression generally or in a given domain".[xci]

According to the proposal Korselt puts forward in the second half of the above passage, an expression is meaningful relative to a set of sentences if these sentences can be used as a criterion for deciding whether to accept the expression as true or reject it as false. (If the expression in question is not a truth-value bearer, then it is meaningful relative to a set of sentences if every atomic sentence including the given expression is meaningful relative to these sentences.)

One way of using sentences as a criterion for deciding whether to accept another sentence consists in asking whether the sentence in question follows logically from the set, in which case it ought to be accepted, or if its negation follows from the set, in which case the sentence ought to be rejected; if, however, the sentence is independent of the set, it is meaningless relative to it. It is unclear to me how this account of "meaning" would address Frege's "pocketwatch"-argument, since any atomic sentence involving, e.g., the expression "is a line" is, in fact, independent of Hilbert's axioms and hence, as Frege claims, not given any meaning by the axioms, even when "meaning" is understood as Korselt suggests here. Still, the idea underlying Korselt's account, that "meaningful" need not be explained in terms of objects, is a decisive break away from Frege's conception of meaning.

§ 51 The first rhetorical question Korselt poses may also be understood as expressing Korselt's rejection of the principle that definitions associate meaning with expressions, my (I) above, or the compositional account of sentential meaning according to which a sentence (and hence, in particular, an axiom) can have a determinate meaning only if all of the expressions occurring in it essentially do. However, since Korselt goes on to consider different senses of "meaning", interpreting Korselt's efforts as aimed at rejecting Frege's particular conception of meaning seems more appropriate than it would be to interpret Korselt as rejecting the idea that definitions assign any meaning at all. But Korselt's position in his 1903 article does not fall clearly on one side or the other[76].

[76] While in the above passage Korselt considers different senses of "meaning" which may be assigned by definitions invoking axioms,

Later, in his 1908 article Korselt does seem to want to associate some kind of meaning, but not a uniquely determined extension, with expressions like "point" or "between". I will consider Korselt's positive proposal for how to assign meaning to the expressions Hilbert defines using axioms later. That Korselt made such a proposal at all is, to my mind, a sufficient reason for interpreting Korselt's overarching project in the way I am suggesting, as rejecting Frege's principle that every meaningful expression must be associated with a uniquely determined extension (my (III) above), while retaining the idea that definitions assign some kind of meaning (my (I) above).

2.2.1 Korselt's Critique of Frege's Principle of Completeness

Before discussing the arguments Korselt puts forward against the principle I numbered (III) earlier, I want to briefly discuss an argument against Frege's principle of completeness (see § 8) which can be found in Korselt's critical discussion note on the second volume of Frege's *Grundgesetze der Arithmetik* (published in 1905). This argument is of some interest for my current purpose because, even though it does not target Frege's conception of meaning directly, it brings into focus a particular assumption which plays a key role in Korselt's arguments against Frege's conception of meaning as I understand them. Korselt wants to reject Frege's principle of completeness – that is the requirement that a definition must give rise to an exhaustive division of all objects whatever – because it is, in some sense, beside the point whether definitions are complete: § 52

> If it is in many cases impossible to decide whether an object belongs to a certain concept, then what good is a sharp definition of the concept? One does not even know if the definition is sharp. One is bound to ask in that case: How does one recognise that one has defined the concept in question sharply? Certainly only by the known complexes in which the concept occurs, or by the fact that something can be "done" with it, that it shows up in a certain way in a lawful and *clear* connection of conscious experiences. But what "a clear, lawful connection of conscious experiences" is cannot be defined by means of genus and species; but it is easily pointed out and made intelligible through examples. A representation which shows up in no lawful connection with other representations is of no importance, be it sharp or not. For the purpose of proofs the only thing that matters is drawing correct conclusions and recognising all axioms and inference rules used to do so. Thus the additional requirement of dealing with "sharp concepts" has become superfluous.[xciii]

The assumption, which lies at the heart of this passage, as I understand it, can be formulated in the abstract as follows: Any information conveyed by a theory must be encoded by components internal to the theory, such as its axioms and inference rules, rather than being attached to the theory from the outside, e.g. as a clause in the definition which fixes the extension of the defined expression by, as it were, semantic fiat; I will label this claim, which I take Korselt to assert quite generally, "the principle of explicit information"[77].

I take Frege to be committing himself to (some version) of this principle when stressing that his concept-script serves the purpose of rendering every assumption which enters into a deduction explicit[xciv].

Korselt also says later in the article, for instance, that "expressions used in formal theories have no meaning". Frege took remarks like the latter more seriously and construed Korselt as "taking Hilbert's theory to be a formal theory"[xcii].

[77] As I suggested in footnote 48, Schröder seems to be presupposing a similar principle when he claims to be developing his calculus "by the analytic method". A similar point was urged, decades later, by Hao Wang in response to Quine & Goodman's nominalism. Wang held that demanding an interpretation using only «concrete» objects was without substance so long as one could not provide axioms which served to characterise concrete objects from within the allegedly nominalistic theory.

A First Argument. Based on this principle, an argument against Frege's principle of completeness can be constructed in the following fashion: If an expression used in the construction of a given theory is completely defined in Frege's sense, then every object whatever can be classified according to whether the defined expression applies to it or not (in the case of terms, "applies to" should be construed as "denotes", in the case of formulas "applies to" should be construed as "is satisfied by"). According to the principle of explicit information, the theory must then *convey* that fact through its axioms and rules of inference, somehow.

In discussing Korselt's alternative account of meaning (in § 50) I suggested one possible way for a theory to «convey information» about the meaning of an expression was to provide deductive resources to decide every claim involving that expression. Reformulated in this way, the requirement concerns the bases of the deductive system, the axioms and inference rules of the theory – what it requires is that the deductive system decide every sentence in which the expression in question occurs –, not the definitions of expressions used in the theory.

I want to suggest that Korselt has a consideration of roughly this sort in mind when he claims, at the end of the passage I quoted, that Frege's principle of completeness is superfluous: Either the theory at hand constitutes a sufficiently potent deductive framework to decide any sentence in which the expression in question occurs; in this case the further requirement that the expression be defined completely – insofar as it calls for a special kind of definition – is redundant, since a complete classification of objects with respect to that expression can be derived from the deductive system without any special contribution from the definition.

In the absence of a sufficiently potent deductive system, even if an expression has been given a complete definition, the theory will not be able to convey that information (in the stricter sense of "convey" prescribed by the principle of explicit information), and so in this case Frege's requirement of completeness turns out to be idle.

In either case, the requirement that definitions be complete adds nothing to the theory, or the information conveyed by it, and for that reason it is, in any case, superfluous as a requirement.

If Frege's principle of completeness is related as Frege claimed it was – see the discussion above on page 6 – to Frege's principle that every definition must assign a meaning (my (I) above), then this criticism also casts some doubt on Frege's views of the role of meaningfulness in theories generally. But I want to stress that it does so not by raising explicit problems for Frege's views – the principle of meaningfulness could still be justified in another way –, but only by undermining Frege's apparent motivations for accepting these views. § 53

2.2.2 Korselt's Critique of Frege's Conception of Meaning

In any case, Korselt's critique goes further. He ultimately claimed that Frege's requirement calling for a uniquely determined extension to be associated with every meaningful expression is, in fact, impossible to fulfil.

Korselt raises such an objection against Frege's entire framework both in his critical note on the second volume of the *Grundgesetze der Arithmetik*, and in his 1903 response to Frege's critique of Hilbert.

Two Arguments. Each of these articles offers a different argument to the effect that Frege's requirement of associating a uniquely determined extension with a given expression is not tenable in practice. I think that, just as in Frege's case, the arguments can be connected in a way that strengthens the case Korselt can be seen as making against Frege's view (and just as in Frege's case, this is a connection Korselt himself seems not to have drawn).

§ 54 I begin by discussing the argument Korselt offers in his 1905 discussion note of the second volume of Frege's *Grundgesetze der Arithmetik*. The argument Korselt offers in support of his conclusion here builds on the principle of explicit information. Korselt assumes that whatever meaning an expression used in a given theory is associated with must (in some sense) emerge from the axioms and inference-rules governing that expression in the theory. In light of this principle, Korselt goes on to claim that there are no axioms or inference-principles which can serve the purpose of determining a unique extension to be associated with the expression:

> The question: "what is a number"? already involves a certain limitation, it requires that the object "number" be determined on the basis of its *inner* nature. However, we cannot determine many objects, such as "point", by their properties but only by their circumstances, that is to say, by the relations they bear to other objects. And for this reason the question here must rather be: which properties of a class, or which relations between classes are designated by the name "class of numbers"? The answer to this question is given by those principles of inference in which the word "number" occurs.[xcv]

Korselt presents the central premiss of this argument – that most axioms do not bear on the precise extension to be assigned to the expression they govern, but only on the relations such objects stand in to others – as an observation, which can be arrived at by considering what axioms used by mathematicians are like in practice.

An Observation about Axioms. Most axioms, Korselt claims to observe, bear only on the relations between certain objects or properties designated by the expressions they govern, while giving no information about the "inner nature" of the objects in question, i.e. about which *particular* class the expressions designate[78].

§ 55 Hilbert's axioms for geometry, which Korselt is probably alluding to by mentioning points as examples of objects "determined only by their circumstances", serve well to make Korselt's claim plausible: To say, for example, that for every pair of points there is at most one straight line on which both of them lie is not saying anything about what points and lines are. This axiom, which is the second of Hilbert's axioms of incidence (governing the expression "lies on"), bears only on how points and lines – whatever they are – relate.

Even in the idealised case of an axiomatic atomic and monadic sentence, that is to say, of an axiom merely

[78] While Korselt gives no reason to think that there are *no* axioms which bear on what particular object(s) the expression they govern designates, he seems to assume that the axioms used for most theories of genuine mathematical interest, and of number theory in particular, are not of this kind. Since Frege is concerned primarily with certain branches of number theory, Korselt's criticism of Frege does not depend on the general claim that *all* axioms bear only on relations between expressions.

This position was not peculiar to Korselt; in 1911 Schoenflies claimed in a similar fashion (but apparently independently of Korselt, on the basis of Hilbert's work) that "with every group of axioms it is *only* the *relations* governed by it, which constitute the mathematical content of the axioms [...]. The names of the objects, on the other hand, are [...] completely irrelevant."[xcvi].

This same sentiment was later famously seized by Skolem who claimed (in his "Einige Bemerkungen zur Axiomatischen Begründung der Mengenlehre") that it was, generally, impossible to assign a unique extension to any expression characterised by means of axioms; rather, any extension assigned would be relative to the extension assigned (from outside the axiomatic system, somehow) to the primitive expressions used in the theory (Skolem was especially concerned with the extension of the primitive predicate-expression "is a set", which Skolem, following Zermelo, designated 'B' (for "Bereich"))[xcvii].

applying a predicate to a constant – such as the claim that zero is a number –, a similar observation can be arrived at. Even in such a case, the axiom bears only on the relation (of inclusion) between a particular class, the numbers, and a particular object, zero; not on which particular object and which particular class are so related.

Generally speaking, any well-formed sentence will involve at least two expressions; and if taken as an axiom the sentence will succeed in assigning a uniquely determined extension to one of these expression only if the other expression(s) it involves are somehow already associated with a uniquely determined extension. Axioms function, so to speak, only as «relays» of uniquely determined extensions; that is to say, they determine the extension of the expression they govern uniquely only if all other expressions which occur in the axiom are already associated with a uniquely determined extension; but axioms cannot determine the extension associated with the expression they govern any further than the extensions of the component expressions occurring in the axioms are determined. This entails, among other things, that in a situation where all expressions which occur in an axiom are in turn governed by further axioms – as, for example, in Hilbert's axiom system for geometry –, none of these axioms will determine the extension to be associated with the expression they govern uniquely.

This consideration weighs neither against Frege's views, nor Hilbert's, since Hilbert did not claim that his axioms associated a *unique extension* with the expressions they govern, while Frege did not claim that unique extensions were associated with expressions *by means of axioms*. But the argument does constitute a difficulty for Frege's requirement that meaning must determine unique extensions when taken in conjunction with another argument Korselt puts forward, which I will present next, to suggest that definitions are like axioms in this respect, that is to say: definition are likewise incapable of associating a unique extension with an expression.

Another Argument, about Definitions. The only other way of associating an expression with a unique extension, at least as far as Korselt is concerned, are definitions. At this point another argument, which appears in Korselt's 1903 article, becomes relevant. In his 1903 article, Korselt claims that – on pain of a vicious infinite regress – definitions cannot associate expressions with a uniquely determined extension either. § 56

> [...] one must not – as Frege does[79] – require the solvability, much less the unique solvability, of every system of basic laws with respect to the unknowns (basic concepts) which occur in them; that would require supplying definitions endlessly.[xcviii]

I take Korselt to be raising a similar objection as in this case as in the case of axioms: The requirement that an expression be assigned a unique extension by means of a definition presupposes that all expressions which are used (in the definition of the expression, or in the axioms governing it) to specify the unique extension of the defined expression must, in turn, have a uniquely determined extension associated with them previously, because definitions, like axioms, can ultimately only «relay» the combined extension of their component parts by stipulating that the defined expression is to be associated with the same extension as the defining expression.

[79] Frege makes this claim in two passages I discussed above, which can be found in endnotes x and xxxv.

§ 57 Korselt apparently takes for granted that no alternative procedure exists by means of which expressions can be associated with (uniquely determined) extensions, other than by definitions and possibly axioms. I suggested above, on the basis of Korselt's other argument, that axioms cannot serve this purpose any more than definitions can: Both axioms and definitions assign a uniquely determined extension to the expressions they govern or define only if at least some expressions are *already* associated with uniquely determined extensions, which are then «relayed» by the axioms or definitions to the expressions they govern or define.

Korselt's Challenge. Since Korselt offers no reason to think axiomatisation and definition exhaust all possible means of associating a uniquely determined extension with an expression, he can really only claim to issue a challenge for Frege to exhibit such an alternative procedure, if his requirement that a unique extension be associated with any meaningful expression is to be tenable in practice, to accomplish this association.

Frege takes up Korselt's objection as such a challenge in his 1906 articles, and spends a considerable amount of time and effort responding to it. But in spite of Frege's efforts, his discussion reveals in the end that he does not have the means to meet the challenge Korselt issued; that is to say, Frege does not succeed in describing an alternative procedure of associating a uniquely determined extension with an expression.

Frege's Response. § 58 Frege begins his discussion by rejecting the objection as Korselt phrased it: Because all definitions, as Frege conceives of them, are ultimately composed of expressions which are not themselves defined but rather "ultimate logical elements"[xcix], the chain *of definitions* will invariably come to an end.

However, this response does not really address the challenge Korselt's raised: that, in order to determine the extension to be associated with a defined expression uniquely, the expressions used to do so – whether defined or undefined – must themselves be associated with uniquely determined extensions somehow.

This worry remains even, and especially, if one accepts the existence of undefined expressions.

§ 59 Indeed, explaining the assignment of unique extensions to undefined expressions remains an open problem in Frege's framework. Frege does propose a process – he calls it "elucidation" – which is meant to do for undefined expressions what definitions do for the expressions they define. However, Frege finds himself forced to admit that this process of determining the meaning of undefined expressions through elucidation, as he describes it, does not in general lead to the assignment of a uniquely determined extension

There is, as Frege admits, no reason why elucidations *must*, so to speak, «converge» on associating a unique extension with any given undefined expression, and no guarantee that agreement on the extension associated with the expression in question *must* be reached at some point. Frege finds himself forced to admit that one must "have faith that agreement can be reached through elucidations, though theoretically the contrary cannot be excluded"[c].

§ 60 It is worth noting that Frege does not newly come up with his idea of elucidations in response to Korselt. It appears already, much earlier, in Frege's article "On Concept and Object"[ci]; new in the discussion of elucidation

Frege produces in response to Korselt, however, is the admission that elucidation need not succeed in assigning a unique reference to the expression it elucidates[80]. A later, unpublished manuscript entitled "Logische Allgemeinheit"[81] finally shows Frege at his most disillusioned. After rehearsing again his old adage of how in understanding even ordinary language one depends on "meeting the other half-way", Frege says:

> [...] that expectation [of meeting another half-way] can be disappointed. Also the use of language requires care. We must not overlook the deep chasm which separates the realm of language from the realm of thought and imposes certain limits on the mutual correspondence of these realms[cii]

The pessimism about language Frege expresses in this passage is, in any case, also found in other unpublished writings dated by the editors of Frege's *Nachgelassene Schriften* to the last years of Frege's life, of which I want to briefly discuss two: First, a short manuscript entitled simply "Zahl", where Frege diagnoses the failure of his attempt to account for numbers as objects as the result of "being mislead by language"[ciii].

Second, a longer manuscript entitled "Erkentnisquellen der Mathematik und Naturwissenschaften", which contains Frege's most pessimistic assessment of language:

> For the reliability of thought the most damning property of language is its tendency to create proper names to which no object corresponds. When that happens in poetry, which everyone perceives as such, that is no drawback. When it happens in the presentation of something claiming to be rigorously scientific, it is different. An especially noteworthy example of this is the formation of proper names according to the pattern "the extension of a concept *a*" [...] From this the paradoxes of set-theory arose, which annihilated that set-theory. I myself, in an effort to provide a logical foundation for numbers was deceived by this, since I wanted to give an account of numbers as sets. [...] It is hard, perhaps impossible, to investigate the propriety of every expression language offers us. So a major portion of the work of the philosopher consists – or, anyway, should consist – in a fight with language.[civ]

The speculative suggestion I want to make in light of the evidence from Frege's subsequent, unpublished writings cited here and in § 48 is that Korselt's challenge played a part in Frege's eventual disillusionment with the use of language. It seems to me, Frege's programme, as expounded in the *Grundgesetze der Arithmetik*, subsequently went through *two separate crises*, one technical, the other philosophical in nature, and only together these two crises produced the pessimism expressed in the passage above: The first crisis was precipitated by Russell's paradox, which forced Frege to give up the particular axiom system he had been working with (and, as Dummett noted, Frege was apparently unable to remedy the defect in his axiom system to his own, or anybody else's satisfaction[82]).

But the diagnosis of the reasons which brought about this technical failure and the estimation of its philosophical significance is another matter. In this passage Frege suggests that the technical problem arose from a deception due to the propensity of language to allow the formation of what turn out to be empty singular terms, though nothing

[80] James Conant, in his article "Elucidation and Nonsense in Frege and Early Wittgenstein" amalgamates these discussions into a single, stable account; he connects a passage from "On Concept and Object" (from 1892) with a passage from Frege's response to Korselt written almost 15 years (!) later (published in 1906) with the misleading phrase "as Frege goes on to say" (p. 183), thus obscuring entirely what I take to be a *development* in Frege's own views of elucidation Korselt's criticisms forced on Frege.

[81] The editors of Frege's *Nachgelassene Schriften* date this very short text on solid grounds – it mentions the third of Frege's "Logische Untersuchungen" as already published – to 1923 (or later), see the editorial note 1) on Frege, *Nachgelassene Schriften*, p. 278.

[82] See the discussion in Dummett, *Frege: Philosophy of Language*, pp. xl sq.

about them indicates this fact. The admission of such expressions effectively contradicts Frege's earlier presumption that every acceptable expression must be associated with a uniquely determined extension.

In this way, Frege's remark that it is impossible to ensure the propriety of every expression seems to me a final concession to Korselt's objection that it is impossible – whether by definition or otherwise – to associate with a given expression a uniquely determined extension. I will stress, however, that Frege even in his last manuscripts held on to this principle. Rather than give it up, Frege gave up the idea that *language* could be of any use to this end and that one must rather presume some sort of prior, language-independent grasp of thought itself[cv].

Summary. It seems to me Korselt revealed a genuine weakness of Frege's position: Since Frege has no way of *ensuring the uniqueness* of the extension associated with expressions, Frege's demand that meaningful expressions be associated with a unique uniquely determined extension, and the conception of meaning it is part of, becomes very implausible, because it would effectively make meaningfulness impossible. § 61

Korselt is not alone in objecting to Frege's project along these lines. Similar objections have been raised by others both in Frege's lifetime[83], and since then[84]. The combined force of Korselt's and similar objections should, I believe, motivate explorations of alternative semantic frameworks, which embrace the possibility of a theory having various different interpretations, rather than calling for a single privileged one. This is, in any case, what Korselt did. But before I discuss Korselt's considered positive proposal – as expounded in his 1908 article "Über die Logik der Geometrie" –, I want to briefly test the strength of his objection.

[83] Schoenflies raised a similar objection in his article "Über die Stellung der Definition in der Axiomatik." Schoenflies claims that "when [definitions] are used for the fundamental relations (as, for example, by Frege), that amounts to reducing an axiomatic mathematical relation to unmathematical words of uncertain meaning"[cvi]. Schoenflies terminology suggests that he did not appreciate Frege's distinction between definitions and elucidations: Schoenflies characterises the kind of definition whose use he is reproaching Frege for (Schoenflies speaks of "Wortdefinitionen") broadly enough to include what Frege calls elucidations[cvii]; and in saying that Frege is defining his primitives, Schönflies probably has Frege's idea of elucidation in mind.

To make his framework mathematically acceptable (by Schoenflies' standards) Frege would have to axiomatise his undefined expressions, rather than using elucidations to explain their meaning (and determine their extension). Schoenflies thus takes a position in a sense diametrically opposed to Frege's: He finds *only* axiomatically characterised expressions mathematically acceptable[cviii]

As I mentioned in § 40, Korselt defends Frege against Schoenflies on this point. However, Korselt's defence is hardly worth noting, since it consists merely of asserting – without argument – that the expressions Frege uses are, in fact, mathematically acceptable (Alwin Korselt. "Über Mathematische Erkenntnis." In: *Jahresberichte der Deutschen Mathematiker-Vereinigung* 20 [1911], pp. 464–379, p. 371). In general, Korselt's response to Schoenflies does not shine the most favourable light on Korselt's powers of judgement, since it also shows him accepting Dedkind's proof of the existence of countably infinite sets, which Schoenflies had criticised in the same manner as Frege's definitions as not mathematically acceptable; (for Schoenflies critique, see: Arthur Schoenflies. "Über die Stellung der Definition in der Axiomatik." In: *Jahresberichte der Deutschen Mathematiker-Vereinigung* 20 (1911), pp. 222–254, p. 230; for Korselt's acceptance, see: Korselt, "Über Mathematische Erkenntnis," p. 369).

I want to note here that if Schönflies' and Korselt's reactions to Frege's position are representative of how mathematicians perceived Frege at the time, that may explain why Frege's work found so little recognition among working mathematicians of the time: It was seen as impracticable. That Frege's exchange with Korselt informed the perception of Frege among German mathematicians is, in my opinion, also not unlikely in light of the fact that the first writings of Frege's to appear in a journals having wide circulation among mathematicians were his articles about the foundations of geometry, and Korselt's responses.

[84] Michael Hallett reaches the same conclusion in Hallett, "Frege and Hilbert," p. 435: "[O]ne of the very purposes of the extended *Begriffsschrift* project is to circumvent the ambiguities, unclarities and misunderstandings that the 'language of everyday life' is heir to. It seems as if the reliance on elucidation, at least for fixing reference, is tantamount to an admission that this circumvention is ultimately impossible.". He comes to this conclusion mainly on the basis of passages from the articles Frege wrote in response to Korselt. I have tried to suggest that this is not only a conclusion to be inferred from Frege's remarks in historical retrospect, but rather that it was this conclusion, raised as an objection to Frege by Korselt, *which prompted these remarks in the first place*.

2.2.2.1 Remedies and a General Point

There are, it seems to me, in principle two possible responses: Either you find yourself persuaded by Korselt's challenge, and Frege's failure to provide a satisfactory response, to give up on Frege's conception of meaning, characterised, in particular, by his requirement that any meaningful expression must be associated with a uniquely determined extension. Or Korselt's considerations do not (fully) convince you that Frege's position is not viable.

Still, even if you find yourself in this position, it is in my opinion hard to deny that Korselt's challenge poses a serious problem and that Frege's own response fails to fully address it.

Thus the question arises, whether there are other, satisfactory responses to Korselt's challenge which Frege for whatever reason failed to see, or neglected to consider.

I will discuss this question only briefly because I find Korselt's considerations persuasive enough to at least explore alternatives to Frege's conception of meaning before attempting to fix up Frege's own.

Meeting Korselt's Challenge. In fact, I will only consider one possible response to Korselt's challenge. However, it seems to me that this response is representative of a broader class of responses, all of which, while theoretically satisfactory, would probably have been unacceptable to Frege himself.

§ 62 One obvious way of ensuring that the expressions defined in terms of undefined expressions will unfailingly be associated with a unique extension, would be to simply assume that agreement is eventually reached through elucidation, sooner or later, but certainly at some point. This assumption seems plausible enough, but there is a good reason to think that it would have been unpalatable to Frege. To see why, consider the following portion of Frege's explanation of elucidation in his 1906 article, which I omitted from quotations until now:

> One can assign [elucidation] to a preparatory study. In the system of science it has no place; in that system no inference is based on it. Anyone doing research just for themselves has no need of it. The purpose of elucidations is practical, and when it has been achieved, one must be content with that. This depends on the good will and cooperative understanding, and on guesswork; since one often cannot do without figurative ways of expression. But the originator of an elucidation can always be required to know with certainty what is meant, to remain internally consistent and, when the possibility of a misunderstanding arises even in the face of goodwill, to supply further or better elucidations.[cix]

This passage suggests, to my mind, that Frege conceived of the process of giving elucidations, on the one hand, as in principle open-ended and, on the other hand, as in principle different from the process by which somebody offering an elucidation "knows exactly what is meant". To assume that elucidation would eventually resolve the problem of assigning a uniquely determined extension to an expression to the satisfaction of everyone involved in the discussion, and associate beyond all doubt a certain unique extension with a given expression seems, in light of this passage, to go against a fundamental aspect of Frege's conception of elucidations.

This claim requires further discussion and justification.

§ 63 As a point of departure, I will rely on an observation Susan Sterrett once made in an insightful, unpublished

talk. She observed that the central point of difference between Frege and Hilbert concerned the question "how expressions get their meaning"[85]. As Sterrett portrays it, Frege's view is that meaning is gotten by as a result of using words which have meaning in ordinary discourse and then made more precise through explication:

> For Frege, "explication" starts with taking words that are already in use, in some ordinary language shared by a group of people who are able to carry on communication with it; [...] this is somewhat different than "agreement" by members of a group, which is all to easily assimilated to stipulation. Explication makes words already in use more precise, sometimes relying on non-linguistic procedures (a "meeting of minds") to make clear what is meant.[86]

This way of looking at Frege's position suggests to my mind that the uncertainty inherent in the process of elucidation is not an unfortunate, unwanted side-effect, but rather a testament to a central philosophical commitment of Frege's: It is what distinguishes explication from stipulation; and, as Sterrett suggests, this uncertainty inherent in the process of how words get their meaning is what ultimately distinguishes Frege from Hilbert.

It does, indeed, seem to me difficult to distinguish between a case of successful elucidation as Frege conceives of it, and axiomatic characterisation of the kind Hilbert relies on: Presumably in elucidating a given expression, one would put forward a handful of sentences, which are true from the point of view of the speaker, in the hope that another, upon hearing these sentences, will come to associate with the (to this other person) so far meaningless expression a meaning like, or even the very same as that associated with it by the speaker. But given the indefinite nature of elucidation, all Frege can really be assured of, even and especially in case such an elucidation succeeds, is that whoever hears it will associate a *suitable* meaning with the expression elucidated, i.e. one in light of which all the sentences put forward are true; but that is precisely what Hilbert relies on axiomatic characterisations for.

In light of these considerations it seems to me that any assumptions which would, so to speak, «stabilise» the process of elucidation, would even out the difference between Frege's point of view and Hilbert's[87]. It would thus be unacceptable to Frege, because it fails to take the use of words in ordinary language fully seriously, or to take properly into account, as Frege's conception of elucidation does, the changeable nature of ordinary language.

Sterrett's observations serve, in my opinion, to reinforce the idea that the tension between, on the one hand, Frege's commitment to associating a uniquely determined extension with any meaningful expression, and, on the other hand, his conception of the procedure by means of which this association is to be accomplished as inherently uncertain, runs very deeply through his position; it strikes me as a hard challenge to stake out a position in between these two commitments, which emerged from my discussion as equally fundamental to Frege's standpoint overall.

[85] This point of difference is raised (in the formulation I quoted) in Susan Sterrett. *Frege and Hilbert on the Foundations of Geometry*. Unpublished Talk. 1994. URL: http://philsci-archive.pitt.edu/723/1/SterrettFregeHilbert1994.pdf, p. 15.
Sterrett's remarkably insightful talk was initially brought to my attention by Dirk Schlimm.

[86] Sterrett, *Frege and Hilbert on the Foundations of Geometry*, p. 19.

[87] Sterrett also comes to the conclusion that if Frege's procedure of elucidation is assumed to be successful, it is on a level with Hilbert's procedure of interpretation: "If we think, as [Frege] did not, that the notion of 'interpretation' is no more problematic than 'explication', then the difference [between Frege's approach and Hilbert's] does not seem so great, and this shows only, as a point of interest, that one can often do with 'sense' and fully sensed propositions, what is sometimes held could only be effected by 'interpretation' of statements. But, if one considers interpretation problematic, or at least more problematic than 'explication', the difference is a philosophical one." (Sterrett, *Frege and Hilbert on the Foundations of Geometry*, p. 19).

I hope, in any case, that this difficulty I have been labouring to bring into clear focus sufficiently motivates the quest for an alternative to Frege's framework as the basis of a philosophical account of mathematical theories, and to (the emphasis on uniquely determined extensions central to) his conception of meaning in particular.

2.3 Korselt's Positive Contribution

This quest is also what Korselt proceeded to focus his energy on in his next big, and remarkable article on the topic, entitled "Über die Logik der Geometrie," published in 1908. Korselt begins this article by expounding Bolzano's views, and continues to do so for more than half the article, occasionally interspersing his exposition with comparative remarks to identify certain technical terms of Frege's with Bolzano's[88]. § 64

Then, in the second (of three) sections of the article, Korselt develops his own positive meta-theoretical account of Hilbert's work – including suggestions for understanding the logical workings and status of Hilbert's theorems, as well as his independence results, and his definitions which involve axioms – on the foundations of geometry.

Korselt's 1908 article thus presents an alternative to the reconstruction Frege proposed in his second series of articles. Frege's reconstruction of Hilbert's work was centrally informed by the conviction that only sentences can feature in definitions. Korselt's reconstruction does not depend on this assumption. § 65

The conviction that only sentences can figure in definitions required Frege, first, to turn all claims Hilbert was working with into sentences – that is to say, Frege needed to find a way to attribute to them a determinate truth-value –, and second, to do so in such a way that the resulting definitions would define what Frege took to be the objects Hilbert's work described. Frege's way of accomplishing this goal was to reconstruct every claim Hilbert made as a universally quantified, second-order conditional sentences: Frege replaced the expressions Hilbert treated as re-interpretable – like "point" or "line" – by universally-quantified second-order variables; to ensure the truth of these sentences, Frege ruled that every theorem must «carry with it» all axioms it depends on as conditions[cx].

According to Frege's reconstruction, Hilbert axiomatic geometry was, contrary to what Hilbert's presentation suggests, not «about» geometrical objects – points, lines, planes –, but rather about the concepts under which these objects, as well as the objects of certain non-Euclidean geometries (and, in theory, the objects of various models of geometry) fall, and about the connection among these concepts.

Korselt offers instead – in line with what I identified as his basic conviction above – a way of conceiving of Hilbert's work which takes the re-interpretability of certain expressions seriously. According to Korselt's reconstruction, rather than working with true sentences at every point in a deduction, one is working with what would nowadays be called open formulae using what would nowadays be identified as model-theoretic methods.

[88] Wolfgang Künne credits Korselt with being the first to compare Frege's views and Bolzano's, but finds Korselt's appropriation of Bolzano's work overall "unsatisfactory" and "naive"; more interesting than Korselt's work, Künne would have found Frege's response, whose loss he laments (see: Wolfgang Künne. "'Die Ernte wird erscheinen...' Die Geschichte der Bolzano-Rezeption (1848-1939)." In: *Versuche über Bolzano*. Beiträge zur Bolzano-Forschung 21. Academia Verlag, 2008, pp. 305–404, pp. 340 sq.).

It is worth noting that Hilbert & Bernays present Hilbert's early work on geometry in this way much later, in the first volume of the *Grundlagen der Mathematik*, where Hilbert & Bernays stress that "for formal axiomatics the primitive relations play the role of predicate variables"[cxi]. In the *Grundlagen der Geometrie* – like in Korselt's, but unlike Frege's reconstruction – the claims of Hilbert's axiomatic geometry are reconstructed as second-order *open* formulas, not (universally quantified) sentences.

Underlying Korselt's entire framework is an account of logical consequence derived from Bolzano. The definition of "(logical) consequence" is (except for its formulation) the same as the current one; a sentence is a consequence of one or many other sentence(s) if every structure satisfying the latter also satisfies the former[cxii].

On the basis of this account of logical consequence, Korselt offers an account of Hilbert's foundational work[89]. Korselt's account of Hilbert's independence results is – setting aside terminological differences again – like the current account. Independence, Korselt explains, is non-consequence (in Bolzano's technical sense):

> Hilbert's claims about the independence etc. of his axioms are thus meaningful and correct, though they require some clarification (which the preceding discussion hopefully delivers). Instead of "invalid axioms" one will, for example, have to speak of a class of sentences from which a certain other class of sentences cannot be deduced with respect to a certain predicate-variable.[cxvi]

Given the above definition of consequence, "not being deducible" from a set of sentences requires finding a "class of concepts" (i.e. as structure; see footnote 91) which makes the set of sentences Φ from which a given sentence ϕ is independent all true, while making ϕ false when the relevant predicate-variables are interpreted as the properties or relations of the structure furnishing the counter-model.

§ 66 As the above passage already indicates, Korselt agrees with Frege in finding Hilbert's use of the word "axiom" faulty. In fact, Korselt came to accept Frege's conception of axioms as determinately true sentences; consequently, axioms (properly so called) could never be invalidated[cxvii]. However, Korselt proceeds to draw a consequence from Frege's conception of axioms, which Frege would presumably not have wanted to accept: Korselt holds – with Frege – that what Hilbert calls "axioms" are not really axioms; nor are there, Korselt goes on to note, real axioms *anywhere in mathematics*[cxviii]. Indeed the propositions which are used as part of definitions are, according to Korselt, as close as one can get to stating real axioms, which presumably justifies calling these sentences "axioms", in spite of the fact that they are not really axioms in the proper sense of the word at all.

§ 67 This leads me finally to discuss Korselt's account of those among Hilbert's definitions which Frege found problematic. According to Korselt, these definitions should be understood in the following sense:

> The declarations I through V above do not entail that only a single concept Λ, only a single concept Π_Λ etc. may satisfy the conditions there exhibited, rather a structure is being defined, a relation of (four) concepts[90], i.e.

[89] Korselt also seems to think that his consequence-based framework can be used to *reconstruct Frege's deductive framework* as based merely on a special case of Bolzano's consequence-relation: Korselt identifies the deductive relation Frege is working with with a relation Bolzano calls "Abfolge"[cxiii], which is the relation that results from applying Bolzano's consequence-relation only to true sentences (both as premisses and as consequences)[cxiv] In his closing remarks Korselt further conjectures that Bolzano's concept of "consequence" captures the intuitive idea of a logical inference[cxv].

[90] For orientation, Korselt provides a reconstruction of Hilbert's axioms based on four primitives: a binary incidence-relation (which

> a class of ordered classes of objects. This definition is fulfilled by *any* four concepts, provided only they exhibit, when taken in a certain order, the properties and relations listed for Λ, Z, $K\Sigma$ and KW.[cxx]

In all but formulation this account is the same as the current one, according to which axiom-systems define classes of structures[91]. In one respect, Korselt agrees, once more, with Frege about Hilbert's own choice of words being incorrect: Hilbert's definitions do not define concepts in the proper sense of the word. Korselt also, once more, disagrees with Frege about how to correct the formulations: Korselt thinks, first of all, that Hilbert's definitions do define something as they stand – in his way, Korselt even agrees with Frege that what they define is of higher order than first; but also of higher order than second (as Frege seems to have thought at some point about his proposed reconstructions of Hilbert's definitions) –, namely, relations between classes. At this point Korselt explicitly corrects his own earlier agreement with Hilbert that the axioms serve to define concepts, rather "they are part of definitions of relations [of] classes"[cxxi].

This conclusion addresses one problem Frege raised about Hilbert's foundational work. It answer the question what, if anything, Hilbert's definitions define. They define – rephrasing Korselt's positive conclusion about Hilbert's definitions in terms of more recent meta-mathematics – a class of structures:

> [The Axioms of Hilbert's axiom-groups I through V] define merely the *relation* between certain classes; the concepts *point*, *line*, *plane* etc. are merely relational concepts.[cxxii]

But this response only brings to light the limitations of Korselt's efforts to contribute in a positive way to the philosophical and methodological debate about Hilbert's work: This response leads to further questions – such as, among others, the question what exactly is meant by "relational concepts", and how exactly one should construe the meaning of expressions which occur in axiomatic theories – which Korselt does not discuss.

Korselt designates by 'Λ'), a binary between-relation (which Korselt designates by 'Z'), a binary congruence-relation for lines (which Korselt designates by '$K\Sigma$'), and a binary congruence-relation for angles (which Korselt designates by 'KW')[cxix].

[91] This conception is discussed, for example, by Wilfrid Hodges in his article "Truth in a Structure," who summarises it as follows: "[W]e define a class of structures by giving a set of laws which the structures must obey, these laws are called *axioms* for the class of structures." (Wilfrid Hodges. "Truth in a Structure." In: *Proceedings of the Aristotelian Society* 86.1 [1986], pp. 135–152, p. 136); with reference specifically to geometry he says: "[W]hat kind of object is | defined by a set of geometric axioms? The answer of course is a class of geometric structures." (Hodges, "Truth in a Structure," pp. 139 sq.).

Korselt's "ordered classes of objects" should, I take it, be construed as meaning "classes of objects, given in some order", rather than "classes of objects each (class) equipped with some order". These correspond to tuples of designated properties and relations over domains of structures. Hilbert's axiom-system, as Korselt sees it, defines classes of such tuples, i.e. classes of structures. (If Korselt's explanation is construed in this way, he even seems to localise classes correctly in the hierarchy of classes as classes of classes (which are the structures) of classes (which are the components of structures).)

Summary

I'll end this chapter by summarising what I take to be the most interesting results of my discussion so far: § 68

(1) Frege criticised Hilbert's work on the foundations of geometry ultimately on the basis of the conviction that all expressions used in scientific theories must be associated with a uniquely determined extension. (2) Frege's preferred formulation of this requirement puts it in terms of assigning meaning to expressions, building into his underlying conception of meaning that being associated with a uniquely determined extension is a necessary condition an expression must fulfill in order to be meaningful. (3) Motivating this conviction in Frege's mind is (insofar as there is evidence to know Frege's mind), as I have suggested, the belief that theories must describe something «uncontroversially existing»; (even if only consistency were required, one is forced into exhibiting objects described by the theory because doing so is the only way of establishing consistency Frege recognises).

(4) Korselt analysed this conviction as based on a value judgement – that theories derive their value from the objects they describe, and their importance[92] –, and suggested (instead) that theories should be thought of as having a value of their own, which is based on the complexity of the deductions required to construct them.

(5) Korselt also noted that the Frege's requirement that expressions be associated with uniquely determined extensions presupposes a way of effecting such an association. Korselt proceeded to object that neither axioms nor definitions can serve that purpose, and Frege seems to have no viable alternative available to him.

(6) Definitions cannot serve this purpose for two reasons: (6.1) Definitions can only be formulated on the basis of expressions which were previously defined or accepted as undefined. To avoid an infinite regress of definitions, one must (as Frege urged) accept undefined expressions. (6.2) The problem with undefined expressions is that Frege appears not to have a way of associating a unique extension with them. (6.3) Since definitions can only confer onto the expression defined the kind of meaning the defining expression has, the uncertainty surrounding the undefined expressions propagates to all expressions defined on their basis; and since all defined expressions are ultimately defined in terms of undefined expressions, it propagates to all expressions[93].

[92] This conception of the purpose of (formal) mathematical theories endures. Friedrich Kambartel, for example, emphasised the importance of "material" use of mathematical language (which means, as Kambartel uses the term, speaking about particular objects), citing the reason that the *purpose* of mathematics, and hence their value, lies only in allowing a schematic treatment of certain common aspects of the description of particular objects[cxxiii].

[93] Kripke remarks, in footnote 44 of *Naming and Necessity*, that for any theory operating, as Frege's does, (and in a key role) with the concept of reference "some notion of ostension, or primitive reference, is required to back it up" (Saul A. Kripke. *Naming and Necessity*. Harvard University Press, 1972, p. 97). I take this remark to point to the same issue as I just summarised as (5), and to

(A similar argument presents itself in the case of assigning an extension on the basis of axioms.)

If Korselt's objection is accepted as sound (or at least convincing), it shows that Frege's conception of meaning is untenable. This raises the question what conception of meaning one ought to accept instead, and what kind of meaning (short of uniquely determined extensions) one ought to assign to expressions used in a formal theory.

(7) Korselt suggested that one can associate uniquely determined relations with entire axiomatic theories – in much the same sense as one currently speaks of axiomatic theories as "defining classes of structures" –, but that leaves open the question whether any kind of meaning can be associated with the particular expressions used to build up such an axiomatic theory, and, if so, what kind of meaning they can be said to have.

§ 69 Up until this point I have been concerned with Hilbert almost exclusively «in relief», so to speak; that is to say, with his work only *as it was taken up*, discussed, underpinned or overlaid with philosophical doctrines, criticised and defended by contemporaries between, roughly, 1900–1910. I have, in other words, been mapping out philosophical positions staked out *around* Hilbert's work and in his time by others: I have, in particular, surveyed and discussed one philosophical position, Frege's, according to which Hilbert's foundational ambitions would be ill-fated and ill-founded: neither is Hilbert's work sufficient to exhaustively account for the content of mathematics, as Frege conceives of it, nor is it *the kind of work needed* to provide such an account. (And I have pointed out that it was immediately noted (by Korselt) that Frege's conception of the content of mathematics, conceived of as uniquely the determined extension assigned to each *and every* meaningful expression, was not practicable.)

Now I shall move on to considering Hilbert's own views about his foundational work.

point to a possible remedy, namely, the idea of ostension, or, as Kripke calls it, "primitive reference". This raises the question whether Frege would have found directly referential expressions, i.e. expressions which have reference without sense acceptable, or not.

Chapter 3

Hilbert's Philosophical Position

The preceding chapters have been centred around Hilbert's work on the foundations of geometry. I shall begin my discussion of Hilbert's own views accordingly by discussing what Hilbert said and wrote about and in connection with his work on the foundations of geometry, insofar as it bears on philosophical and methodological questions.

3.1 The Foundations of Geometry

As a starting point for determining what Hilbert's philosophical standpoint was, I want to consider a piece of information found at the very beginning of Hilbert's *Grundlagen der Geometrie*, namely, the epigraph.

Hilbert chose as the epigraph for his *Grundlagen der Geometrie* the first sentence from the last paragraph of the § 70
second sub-sub-section of the second sub-section within the first (of two) main divisions of Kant's *Kritik der reinen Vernunft*[94], which reads: "So all human knowledge thus begins with intuitions, proceeds from there to concepts and ends with ideas"[cxxiv]. Almost nothing has been written about Hilbert's reasons for choosing this epigraph, or about what Hilbert could have meant for it to signal to his readers; and what little has been written about it, so far as I am aware, is ultimately less concerned with understanding the quotation in its role as an epigraph, but rather with "loose analogies" between Hilbert's work and certain Kantian doctrines[95].

[94] In the German editions of Hilbert's book, both the original edition as well as later editions, the source of the quotation is given as "Kant, Kritik der reinen Vernunft, Elementarlehre 2. T[eil] 2. Abt[eilung]". Kant's *Kritik der reinen Vernunft* is divided principally into a foundational section (entitled "Transzendentale Elementarlehre") and a (substantively shorter) methodological section (entitled "Transzendentale Methodenlehre"). The first main division is, in turn, divided into two sub-sections ("Teile") – the first focusing on perception, the other on cognition –, the second of which is, once again, sub-divided into two sub-sub-sections ("Abteilungen"). Hilbert quotes his epitaph from a summary at the very end of the second one of these sub-sub-sections, which is entitled "Transcendentale Dialektik". The exact source, which Hilbert does not provide, is (using the now standard citation conventions) KrV A702/B730.

[95] I have in mind an (unpublished) article by Michael Hallett and Oran Magal. *The Ideal in Hilbert's Grundlagen der Geometrie*. Draft. 2015, which focuses on presenting "loose analogies" (p. 18) as well as "principal disanalogies with Kant" (p. 19) and his position on the whole. However, while this article begins with a discussion of the epigraph, it then concentrates not on the epigraph itself, or on Hilbert's understanding of Kant, but rather on Hilbert's ideas about so called "ideal elements" as presented in Hilbert's 1925 lecture "on the infinite". In particular, Hallett & Magal set out to compare Kant's doctrine of the regulative use of ideas and Hilbert's use of ideal elements. This particular analogy had been previously discussed in a handful of articles published in 1993-94 by Ulrich Majer (Hallett & Magal acknowledge the overlap with Majer's work on p. 3fn4). The epigraph is only explicitly discussed in one of Majer's articles, "Hilbert, Reichenbach und der Neu-Kantianismus," where Majer calls it the "*Leitmotiv* seiner [i.e. Hilbert's] eigener

I want to shed some light on why Hilbert chose this particular quotation as his epigraph from two angles: By considering the context of the quotation *within* Kant's *Kritik der reinen Vernunft*, and by considering the intellectual environment of Hilbert's book in the 19th century tradition of geometry and philosophical reflection on geometry. Neither of these perspectives has, as far as I am aware, been explored so far at any length or depth[96].

3.1.1 The Textual Context of the Epigraph

The sentence Hilbert quotes is taken, as I said, from the *last paragraph* of the "Transzendentale Elementarlehre". It § 71
stands there as the opening sentence in a final summary of Kant's discussion up until this point.

By mentioning "intuitions", "concepts" and "ideas" it recalls discussions also from other sections earlier in the *Kritik der reinen Vernunft*: Intuitions had been the subject of discussion in the first sub-section of the "Transzendentale Elementarlehre", the "Transzendentale Ästhetik"; concepts in the second sub-section's first sub-sub-section, the "Transzendentale Logik"; and ideas in the second sub-section's second sub-sub-section, the "Transzendentale Dialektik", at the very end of which stands the sentence Hilbert uses as the epigraph. The mention of "intuitions" and "concepts" makes clear, in my opinion, that this sentence summarises not only the sub-sub-section entitled "Transzendentale Dialektik", but rather the entire so-called "Transzendentale Elementarlehre" (which, given the disproportionate division of the work, takes up of approximately 3/4 of the *Kritik der reinen Vernunft*)[97].

As a first step towards making sense of the epigraph, I will now briefly consider the theory I take Kant to § 72
summarise by means of the sentence Hilbert quotes, i.e. what one may call Kant's "theory of knowledge".

Within Kant's theory of knowledge all three items mentioned – intuitions, concepts and ideas – are indispensable for knowledge. Kant asserts the indispensability of the first two by claiming, at the outset of the *Transzendentale Logik*, that neither intuitions, nor concepts amount to knowledge in a proper sense without the other[cxxvi]. For Kant to say that all knowledge "begins with intuitions, proceeds from there to concepts" means, I take it, that knowledge in the proper sense, as Kant conceives of it, requires both an intuition and a concept to be somehow

Untersuchungen" (Ulrich Majer. "Hilbert, Reichenbach und der Neu-Kantianismus." In: *Hans Reichenbach und die Berliner Gruppe*. Ed. by Lutz Danneberg, Andreas Kamlah, and Lothar Schäfer. Vieweg, 1994, pp. 253–273, p. 255); but since this article lays out all the same observations and considerations as Majer's other articles of the time (indeed, a number of formulations occur identically in all of them), I will treat all of Majer's articles as having some bearing on the epigraph.

[96] Jeremy Gray comments on the epigraph in this spirit (see: Jeremy Gray. *Plato's Ghost. The Modernist Transformation of Mathematics*. Princeton University Press, 2009, pp. 180 sq.) calling it "a very precise hint to the educated reader of 1899 about Hilbert's approach to geometry", but then explains his interpretation – "that Hilbert was going to consider the regulative rules governing geometrical concepts" – only in an exceedingly compressed manner and without anchoring his reading in the sources.

The work presented in the articles I mentioned in the previous footnote has, to my mind, a predominantly «speculative» character, that is to say, it presents parallels which may strike us as such nowadays without offering any reasons to believe that *Hilbert himself* saw these particular parallels between Kant's work and his own (nor that they had any importance for his choice of epigraph), or, at any rate, not at the time he chose the epigraph for his 1899 *Grundlagen der Geometrie*: the central piece of evidence both Hallett & Magal and Majer rely on is a quotation from Hilbert's lecture "Über das Unendliche" delivered a quarter century (!) after the publication of the *Grundlagen der Geometrie*. Consequently none of these articles offers a plausible *historical* account of the quotation in its role as an epigraph, i.e. an account of what *Hilbert* may plausibly have meant by quoting this particular sentence, given the sentence's original meaning and the state of the philosophical discussion of geometry, and Kant's philosophy of geometry, in Hilbert's own time. This is what I intend present here. In fact, the views Hilbert expresses in that lecture represent only a very late stage of his career (see § 189), and should not be relied on to characterise Hilbert's earlier position, nor be (mis-)taken for his mature views on the whole.

[97] In the long, summary paragraph of which Hilbert's epigraph is the opening sentence, Kant goes on to refer explicitly back to "the critical investigation [...] in the transcendental analytic" (i.e. the first sub-sub section of the so called "Transzendente Logik" preceding the so called "Transzendentale Dialektik", where the epigraph is to be found)[cxxv].

brought together. I do not want to discuss this matter in any detail, since it is mainly the last part of the quotation which I think is significant for understanding what Hilbert may have intended his epigraph to signal to his readers.

The role of ideas within Kant's theory of knowledge is – unfortunately – harder to grasp. § 73

Kant distinguishes (earlier in the "Transzendentale Dialektik") what he calls the "constitutive use of ideas" from their "regulative use" and asserts that only the latter is legitimate[cxxvii]. Making regulative use of ideas in the technical sense Kant attributes to this phrase amounts, roughly speaking, to conceiving the various objects one has knowledge of *as if* they were part of one overarching whole, of which no proper knowledge is attainable[cxxviii].

Ideas in Kant's terminological sense are, in on respect, like concepts, insofar as they, too, contain characterisations of certain objects, but are, in another respect, unlike genuine concepts because it is in principle impossible to bring them together with a «matching» intuition. I think one ought, therefore, not to speak of "objects grasped by ideas", because this way of speaking would already (and falsely) suggest that there really are potential objects of knowledge (i.e. objects of which there could also be a «matching» intuition) grasped by the ideas, which Kant denies. For this reason I will instead use the somewhat cumbersome but, to my mind, appropriately qualified formulation "objects purportedly grasped by ideas", which, as I mean for it to be understood, expresses the similarity between ideas and concepts – namely, that both have, anachronistically speaking, an intentional object –, while also expressing what Kant takes to be the characteristic feature of ideas, distinguishing ideas within the broader class of concepts, namely, that the intentional objects of ideas are for reasons of principle not even possibly objects of knowledge and thus, from the standpoint of Kant's critical idealism, not objects in the proper sense.

It seems to me Hilbert articulates an understanding of "idea" different from Kant's in the passage from *Über* § 74 *das Unendliche* central to the articles I mentioned in footnote 95. Hilbert says an idea in Kant's sense "surpasses all experience and supplements the concrete in the interest of completeness"[98]. While I find the first characteristic, that ideas surpass experience, unobjectionable as an interpretation of Kant's views, it seems to me that the second characteristic Hilbert mentions, that Kant held ideas to supply the grounds for *supplementing* domains, strikes me as a at the very least contentious paraphrase of Kant's views. As I understand Kant, he is urging, to the contrary, that one must *not* take ideas to licence the assumption that every element projected by the idea of the whole must, therefore, exist. This is, I take it, the main consequence of Kant's distinction between constitutive and regulative uses of ideas and of Kant's assertion that only the latter is justifiable[cxxix]. As far as I am aware, Hilbert never mentions the distinction between a constitutive and a regulative use of ideas either in *Über das Unendliche* or elsewhere; and the interpretation of Kant Hilbert puts forward in *Über das Unendliche* suggests that Hilbert either

[98]The translation is quoted from: Ulrich Majer. "Hilbert's Methode der Idealen Elemente und Kants regulativer Gebrauch der Ideen." In: *Kant-Studien* 84.1 (1993), p. 54; the same passage is quoted by Hallett & Magal, p. 2. The lecture was originally published in *Mathematische Annalen* 95, pp. 161-190 and later re-printed as appendix VIII of the 7th edition of the *Grundlagen der Geometrie*, where the quotation appears on David Hilbert. *Grundlagen der Geometrie*. 7th ed. Teubner, 1930, p. 288; (in the original publication, the quotation appears on p. 190). The same remark appears also in the (substantively longer) notes for a lecture course from 1924/25 of the same title recently published in David Hilbert. *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*. Ed. by William Ewald and Wilfried Sieg. Vol. 3. David Hilbert's Lectures on the Foundations of Mathematics and Physics, 1891-1933. Springer, 2013, p. 755.

did not appreciate its import or knowingly chose to disregard it. In either case, the conception of ideas Hilbert attributes to Kant in 1925 would on the whole differ substantively from the conception Kant himself arguably held.

It seems to me that in 1925 Hilbert is rather *reading back into Kant*'s terminology his own doctrine of the adjunction of ideal elements by interpreting Kant's talk of "unity" in the sense of "uniformity (at maximal extension)"[99], rather than, as I suggested Kant meant it, in the sense of "systematisation" (with no claim to completeness)[100].

Kant holds that there are, in particular, three overarching wholes purportedly grasped by what Kant calls § 75
(respectively) the "psychological" and the "cosmological idea" which, Kant goes on to say, are in turn all unified in a third, called the "ideal of pure reason". Kant identifies objects purportedly grasped by these ideas with the self, the infinite universe and the deity steering the chain of events (respectively)[101].

The regulative use of ideas can serve as a corrective force in scientific inquiry: I would be deterred, for example, from explaining the same natural phenomenon at one time in one way and in a different way at another time *because* I am bound, by the regulative use of the idea of one all-encompassing, infinite universe, to regard both occurrences of the phenomenon as taking place within one overarching, internally complex whole, which functions according to uniform regularities and in such a way that like regularities unfailingly govern like events[cxxx]. I am thus bound to seek a uniform explanation of all occurrences of a certain kind of phenomenon because only such an explanation can adequately describe the overarching whole within which I am bound to take these events to occur.

Ideas, and their regulative use, thus contribute, according to Kant, to knowledge not by adding new items of knowledge but by guiding inquiry towards developing increasingly unified theories connecting knowledge together

Indeed, Kant re-iterates this claim in the paragraph immediately before Hilbert's epigraph:

> So pure reason contains [...] nothing but regulative principles which mandate greater unity than empirical cognition can reach, but precisely because these principles remove the goal so far from any approximating of it, they elevate the coherence of empirical cognition with itself in a systematic unity to the highest degree [...].[cxxxi]

The purpose of ideas within Kant's theory of knowledge consists, according to this passage, in bringing about greater "systematic unity" of knowledge. As I understand this, and similar passages, Kant attributes to the ideas primarily the function of determining a systematic organisation of knowledge already attained, where each item of knowledge has a precisely determined place due to its (more or less remote) dependence on other items of knowledge, and

[99] In an effort, as Majer puts it, "die Regeln des Verstandes in einem gegebenen Gebiet durch Erweiterung desselben so gleichmäßig, übersichtlich und einfach und überdies so allgemein wie überhaupt nur möglich zu gestalten" (Majer, "Hilbert's Methode der Idealen Elemente und Kants regulativer Gebrauch der Ideen," p. 66).

[100] It seems to me therefore that Majer is wrong to accept Hilbert's interpretation of what Kant means by "idea", as "obviously right", as he does on p. 66 in "Hilberts Methode der Idealen Elemente"; in "Hilbert, Reichenbach und der Neu-Kantianismus" Majer qualifies that Hilbert's interpretation is right "jedenfalls in erster Näherung", though without clarifying how Hilbert's proposal should be adjusted to accurately represent Kant's intended meaning and how it differs from Hilbert's interpretation. It seems to me furthermore that this interpretation of Kant, which, as far as I know, expresses in 1925 for the first time, also obscures those aspects of Kant's work whose appeal to Hilbert I believe his choice of epigraph for the *Grundlagen der Geometrie* is a testament to (see § 189).

[101] That Kant holds such a determinate view about the number and character of ideas seems to me a stumbling block for any suggested analogies between Kant's views about ideas and Hilbert's conception of ideal elements: Hilbert held that it might be advantageous for studying any given domain to adjoin specific ideal elements to it, and different kinds of ideal elements for different domains; Kant, by contrast, seems to have held that the number and kind of ideas in his sense is fixed, once and for all, by the nature of the specific cognitive capacities human beings are endowed with. Hallett & Magal also note this disanalogy between Hilbert's views and Kant's in note 4 on p. 3 and, at greater length, on p. 20, but seem to think it does not undermine the hypothesis that there is a connection between Hilbert's talk of ideal elements and Kant's conception of ideas.

the dependencies of other items of knowledge on it. What appear to be knowledge of separate objects and events is integrated to constitute what one might anachronistically call a "theory", where every piece of knowledge has a fixed position relative to all others incorporated in the same system of knowledge. As I understand Kant's doctrine, ideas can so unify knowledge because in their light each bit of what seemed like knowledge of a separate object up until that point comes to be regarded by the human being as knowledge of *only a part* of an overarching whole object; theories are thus unified in Kant's view as I understand it because they constitute as a whole an attempt at knowing *a single*, internally highly complex *object*: the infinitely rich and vast universe governed by a rational deity who assured that it operates according to regularities which are as simple and uniform as can be.

§ 76 It seems plausible to me in light of the kind of work presented in the *Grundlagen der Geometrie* to attribute to Hilbert a sympathetic stance towards this aspect of what one may consider Kant's (nascent) philosophy of science. As I want to suggest in the remainder of this section, Hilbert's investigations, and his use of the axiomatic method in the *Grundlagen der Geometrie* serve mainly the purpose of *systematising* geometry. In the short introductory section, at the start of which one finds the Kantian epigraph, Hilbert describes his own work as follows:

> The present investigation is a new attempt to set up a *simple* and *complete* system of independent axioms und to deduce from them the most important propositions of geometry in such a way that the significance of the various axioms and the bearing of the consequences to be deduced from individual axioms comes clearly to light.[cxxxii]

Hilbert's emphasis here falls squarely on the task of systematising geometric knowledge – rather than, for example, on expanding it by establishing new results or describing new constructions –, which fits neatly into the mould prepared by Kant's philosophy of science, as I just proposed to understand it.

Hilbert's emphasis that the investigation of an axiom system must clarify the "significance" of axioms and "bearing" of particular results, makes especially clear, I think, that finding the axioms and establishing their independence from one another is *only a first step* towards an overarching project of organising all (derived) theorems in a system (structured according to deductive dependence).

Systematising geometry in the sense I mean for that phrase to carry contrasts, to one side, with furthering the development of geometry by deriving new geometrical results, and, to the other side, with stringently developing a particular geometry in full detail rigorously from a particular choice of axioms. Rather, as I have come to conceive of his overarching project, Hilbert is interested in establishing, anachronistically speaking: meta-geometrical results about the deductive (in-)dependence of existing geometrical assumptions and results, and in formulating and comparing various different, rather than developing at length a single axiom system (as, e.g., Pasch had done)[102].

§ 77 This understanding of Hilbert's aim suggests a reason for Hilbert to choose as the epigraph for his book the

[102] William Ewald. "Hilbert's Wide Program." In: *Logic Colloquium 2000*. Ed. by R Cori et al. 2005, pp. 228–251, pp. 239 sq. likewise stresses that Hilbert is asking, and answering, questions which "were foreign to the geometric tradition", even to those exponents of it which were nearly Hilbert's contemporaries, like Pasch. Especially in contrast to Pasch it is worth underscoring the aspect of Hilbert's work I just emphasised: Hilbert was interested in formulating and developing not just a variety of models, as Ewald (rightly) highlights, but also a variety of substantively different *axiom systems* and seems to have been not at all interested in even looking for ground upon which to decide which of them to regard as definitively true.

particular quotation from Kant that Hilbert in fact chose: Hilbert may intend, by means of this quotation, to claim for himself, and his *Grundlagen der Geometrie*, the place of being the first one to reach the level of ideas (in the Kantian sense) within the sphere of geometry, i.e., to be the first to bring into clear view the particular ideas which serve to unify all geometric knowledge into a single overarching whole: systematic geometry[103].

If this description of Hilbert's ambitions is accurate, the epigraph can be understood as intending to make a historical claim about the development of geometry and about the place Hilbert sees for his work in it – namely, that he, Hilbert, is the first to give a systematised presentation of geometry –, rather than a theoretical claim about the status of geometric knowledge. That this was Hilbert's intention strikes me as not implausible in light of the fact that one often finds such developmental claims articulated in prefatory remarks.

§ 78 But I believe it is also possible to articulate a sense in which Hilbert may use the epigraph to make not merely a claim about the novelty of his project, but also a theoretical claim about the status of geometrical knowledge. I will now sketch such a proposal: Hilbert could have conceived of the primitives of his geometrical theory as ideas in Kant's sense as (i) purporting to grasp objects never found in experience as the concepts project them to be (but rather only in approximations), and (ii) serving primarily the systematisation of proper geometrical knowledge (by classifying it according to how it approximates the objects projected by the primitives).

§ 79 I begin with an observation: That Hilbert feels compelled to separately establish the consistency of his geometric axiom system – and of any other axiom system – by "exhibit[ing] a geometry in which all axioms of the five groups are satisfied"[cxxxiii] suggests that Hilbert believes one cannot have «direct experience» of the objects – points, lines, planes – the primitive concepts purport to grasp; indeed, one cannot take for granted that there are such objects at all. Rather, one must find objects corresponding to these primitives in other domains independently of the axioms.

Hilbert's use of models can, I think, be understood in a similar fashion.

That the particular model Hilbert uses for his consistency proof (in § 9 of the *Grundlagen der Geometrie*) is an analytical model, based, ultimately, in number theory, may be taken to suggest that Hilbert finds the objects of analytic geometry to be the most «uncontroversially real» geometrical objects. (This conviction would be in line with the 19th century trend of reducing various portions of mathematics to basic arithmetic, which is held to be fundamental and uncontroversial, by way of set-theoretical constructions.)

But the pairs of numbers, which serve as points in the model Hilbert designates "Ω"[104], are, compared to the objects purportedly grasped by Hilbert's primitive concepts, overdetermined, since they have certain features not

[103] It may be worth noting that this project seems to have arisen in geometric research also independently of Kant. One finds it, for example, articulated already by Lazare Carnot in his *Géométrie de Position* from 1803, where Carnot writes, on the topic of "elementary geometry" (p. xxxiii): "On voit par-là, qu'il ne s'agit pas de grossir les élémens ordinaires d'un grand nombre de nouvelles propositions, quelque curieuse et subtiles qu'elles puissent être, mais de parvenir à la solution d'un problême général qui les renferme toutes comme cas particuliers dans ses développemens, et d'ou elles dérivent par une simple combinaison des formules.".

[104] Hilbert's model of choice, Ω, is two-dimensional because Hilbert provides only a consistency proof of (two-dimensional) plane geometry, presumably intending to illustrate a general approach without having to deal with too many particular complications[cxxxiv]; David E. Rowe. "The Calm before the Storm. Hilbert's Early Views on Foundations." In: *Proof Theory. History and Philosophical Significance*. Ed. by Vincent F. Hendricks, Stig Andur Pedersen, and Klaus Frovin Jørgensen. Kluwer, 2000, pp. 55–93, p. 70 suggests that Hilbert may have been motivated in his choice of model by wanting to avoid becoming entangled in difficulties surrounding "the real number continuum" and for that reason chose "a smaller, indeed countable model that satisfies all of his original axioms".

mandated by the geometrical axioms due to their original numerical properties.

The objects in the domain of Ω are, so to speak, «approximations» of what the objects of «pure» geometry would be like. I chose the metaphor of "approximations" deliberately[105], to highlight a certain analogy between Hilbert's use of models and Kant's doctrine of the regulative use of ideas.

§ 80 Generally speaking, a model for an axiom-system exhibits objects constructed so as to be exhibit the properties and relations described in the axioms. But because these objects are constructed based on previously «available» objects (such as, paradigmatically, the natural numbers), these objects will invariably also have other properties and stand in relations not described by the axioms (and, in general, quite unlike those properties and relations). No model therefore consists of objects which exhibit all *and only* the properties and relations described by the axioms. In this sense, all models of, say, geometry do not comprise in their domain *the* objects of «pure» geometry, because they always have some property or relation which are not «purely as described» by the axioms.

These models fall short of exhibiting objects which coincide with the those described completely by the axioms and them alone, just as Kant saw all, e.g., natural sciences falling short of coinciding with knowledge of the universe as purportedly grasped by the idea of the whole spanning all the physical world: Kant emphasises in his discussion of what he calls the "antinomies of pure reason" – i.e. certain problematic arguments in philosophical cosmology arising, according to Kant's analysis, from mistakenly using certain ideas (e.g. that of the universe as a whole) constitutively whose only justifiable use is regulative –, that although regulative ideas cannot themselves give rise to knowledge, they set up a trajectory for inquiry to follow in order to gain increasingly (but never fully) adequate knowledge of the matter at hand[cxxxv]. I think Kant also has in mind a situation where the object purportedly grasped by an idea is approximated from the side of overdetermination, because in the process of scientific inquiry interrelated phenomena will initially seem to be related by more complicated regularities, which are then simplified and unified as far as possible in an effort to approximate the idea[cxxxvi].

Modifying Kant's formulation in the passage quoted in endnote cxxxv, one might say that axiom systems provide «regulative» guidelines for *what kind of model-constructions we ought to carry out* with objects at our disposal, but that they, themselves and on their own, just like Kant's ideas, do not give us any objects.

In this sense one can think of Hilbert's primitives as concepts purporting to grasp objects never given in their «pure» form – exactly as the axioms describe them – but only ever «approximated» by way of models[106].

§ 81 The primitives of an axiom system, like ideas in their regulative use as Kant describes it, can moreover be used to classify various structures appropriate to the language in which an axiom system is presented according to which axioms are true in these structures. In this sense the primitives of an axiom system also contribute to the systematisation of the objects constituting the domains of the different structures.

§ 82 Along these lines one can, I believe, plausibly understand Hilbert's epigraph as also making a theoretical claim

[105] Majer suggests the metaphor of vanishing points (Majer, "Hilbert, Reichenbach und der Neu-Kantianismus," p. 67).

[106] It is worth pointing out that Kant emphasises once again in the paragraph immediately preceding the sentence Hilbert uses as the

about the metaphysical stratification of the objects[107] the axiomatic method in mathematics involves into three tiers: There are, first, objects taken to be «uncontroversially real» (in this case: numbers), analogous to intuitions in Kant's sense; second, there are constructions applied to the objects in the first tier (in this case: to define (e.g. by relying on set-theoretical means) the elements of the domains of various structures suitable for interpreting Hilbert's axioms), analogous to concepts in Kant's sense; and, finally, there are the «pure» objects characterised by the axioms, analogous to ideas in Kant's sense, because there remains an unbridgeable gap between the models one can construct, on the one hand, and, on the other hand, the «pure» objects one can describe in the abstract.

§ 83 I have now described two ways of understanding the sentence Hilbert quotes from Kant's *Kritik der reinen Vernunft* in its role as the epigraph of the *Grundlagen der Geometrie* based mainly on the sense the sentence arguably carries in its original context, as a summary of Kant's theory of knowledge. Summarised briefly, I suggested:

First, that Hilbert may have meant to appropriate this sentence as a historical claim to situate his work of geometry within the development of geometry and emphasise its special status as the first work setting out, and to a remarkable extent also accomplishing – by means of the axiomatic method – the systematisation of geometry, a task set out in general terms by Kant in the section from which Hilbert takes his epigraph.

My second, more speculative suggestion offered a way of understanding Hilbert's use of the quotation from Kant in roughly the same way as it was originally meant, to make a theoretical claim about the different kinds of objects involved in mathematics treated by means of the axiomatic method.

While I offered the second suggestion above in a speculative fashion, it is not as remote from historical reality as it may at first seem. In fact, there is a precedent for the construal I suggested Hilbert may be using the epigraph to indicate – that primitive concepts are ideas – close to Hilbert's own time, which I want to turn to next.

3.1.2 The Historical Circumstances of Hilbert's Book

§ 84 In 1877, still at the start of his career, Benno Erdmann published a book entitled "*Die Axiome der Geometrie*". This book is primarily an effort to discuss recent developments in geometry, especially – though Erdmann is well-informed and cites a variety of other authors – those presented in the writings of Bernhard Riemann and, mainly, of Hermann von Helmholtz, whom Erdmann had studied with in Berlin, from a philosophical point of view (as its subtitle, "Eine philosophische Untersuchung der Riemann-Helmholtz'schen Raumtheorie", indicates).

That Hilbert knew Erdmann's book can be regarded as certain since he listed it in a brief bibliography of relevant philosophical literature he included in lecture notes of a course he taught on geometry: It is referred to in Hilbert's 1894 lectures entitled, like his later book, "Grundlagen der Geometrie"[108], when Hilbert comments on "the views of

epigraph (which I quoted above in endnote cxxxi) that knowledge in the proper sense can only ever approximate ideas.

[107] I first encountered the metaphor of "strata of being" in Michael Thompson's book *Life and Action* a few years ago, and it has come to seem to me since then as more serviceable than the metaphor of divisions between kinds of objects, since it suggests a picture where all things are broadly the same in nature, but some of them exhibit distinctive textures, colouring, and so on.

[108] See: David Hilbert. *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*. Ed. by Michael Hallett and Ulrich Majer. Vol. 1. David Hilbert's Lectures on the Foundations of Mathematics and Physics, 1891-1933. Springer, 2003, p. 120.

the remaining philosophers", meaning: philosophers other than Lotze, whose dismissal of non-Euclidian geometry for philosophical reasons Hilbert had just rejected as "foolish"[109]. It stands to reason, I think, that the brief list of references which follows contains philosophical work Hilbert *approves of enough* to explicitly bring it to his listeners' attention (which presupposes that he knows its contents at least to some extent).

Seeing how Hilbert never discusses any details of Erdmann's book, it remains hard to estimate how thoroughly Hilbert read Erdmann's book, and also whether he thought of it especially highly.

Hermann von Helmholtz saw in Erdmann's book a mathematically competent discussion of the issues, but found some of Erdmann's remarks about the philosophical significance of the mathematical facts obscure[110].

Hilbert flags this discussion from the 1894 lectures at the end of which the reference to Erdmann occurs for inclusion also in his 1898 lecture notes[111] in connection with the issue of whether theories describe experience absolutely precisely, but in the passage corresponding to this discussion in the version of the lecture notes to be made publicly available, no literature is mentioned, so one can presume that Hilbert did not repeat his list of philosophically relevant literature in 1898-99. In 1902 Hilbert mentions a book by Otto Hölder from 1900 as "the best source for the philosophical side of the question"[cxxxvii].

3.1.2.1 Erdmann's Revisions of Kantian Doctrine

Even though Hilbert mentions Erdmann's book only once and never with explicit approval, I believe that Erdmann's work can be of use in sharpening the contours of the philosophical standpoint that may be ascribed to Hilbert at the stage in his career and intellectual development to which his *Grundlagen der Geometrie* belong; not because there is evidence that Hilbert studied it thoroughly, but because in it certain ideas and arguments find expression which Hilbert may independently have come to entertain, and do so more clearly and at greater length than one ever finds them expressed in Hilbert's own writings. I will in this sub-section use Erdmann's book accordingly, as a typical expression of the philosophy of geometry towards the end of the 19th century.

Whatever Hilbert may have thought of Erdmann's book, it is of interest for my purposes, in any case, because Erdmann explicitly makes the claim that the fundamental concepts of geometry are ideas:

> The foundations of geometrical construction are concepts which not only abstract away from all inessential

Erdmann's book is also the only philosophical work Toepell mentions, albeit only very briefly, in his developmental study, where he states that Erdmann's book was of interest to Hilbert because of his discussion of the "innere[n] Zusammenhang zwischen geometrischer Theorie (Axiome) und Erfahrung" (Michael-Markus Toepell. *Über die Entstehung von David Hilberts "Grundlagen der Geometrie"*. Vol. 3. Studien zur Wissenschafts-, Sozial- und Bildungsgeschichte der Mathematik. Vandenhoeck & Ruprecht, 1986, p. 12), but does not discuss in any detail which portions of Erdmann's book he thinks may have especially piqued Hilbert's interest.

[109] Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*, p. 119.

[110] Helmholtz so evaluated Erdmann's book when Erdmann submitted the manuscript (!) as the main sample of his academic work (along with his book on Knutzen, an article on Kant and a review of a book by Eduard von Hartmann). The list of writings Erdmann submitted, as well as Helmholtz' evaluation of the manuscript on geometry are found in the files concerning the Habilitation of Benno Erdmann at the university in Berlin which are held by the Universitätsarchiv der Humboldt-Universität zu Berlin on sheet 173 of Philosopische Fakultät. "Benno Erdmann." Signatur-Nr.: 1209. Universitätsarchiv der Humboldt-Universität zu Berlin. Erdmann's petition for Habilitation, submitted on 29/11/1876, was subsequently approved by the Philosophische Fakultät on 21/12/1876 (for this date, and other significant dates in Erdmann's career, see his personnel-file Philosopische Fakultät. "Personalakte Benno Erdmann." Signatur-Nr.: E082. Universitätsarchiv der Humboldt-Universität zu Berlin, sheet 2-3).

[111] See: Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*, p. 283.

> features of particular spatial intuitions, but even alter features common to all spatial intuitions in a way which makes any particular intuition, e.g. of a line or a triangle not even an adequate visualisation of the concept. The definitions geometry develops are thus, strictly speaking, not intuitions, not abstract concepts, but ideas.[cxxxviii]

§ 85 Being aware of Erdmann's view even to an imperfect extent brings into focus how the (philosophical) discussion about geometry had, around the time Hilbert entered the field, drifted away from Kant's position to the point of explicitly finding fault with it and seeing it as, in some respects, superceded by more recent developments.

In several respects Erdmann's views diverge – and substantively – from the position Kant arguably held: Unlike Kant, Erdmann holds that geometry is based not (only) in intuition but also fundamentally involves concepts. Indeed, Erdmann refers to the need to modify this aspect of Kant's doctrine in the discussion leading up to his claim that geometry deals with ideas. There Erdmann points out that a modification of Kant's views was required, in particular, in order to set up a general concept of space like that articulated by Riemann which comprises several, substantively different kinds of spaces, i.e. with the possibility of non-Euclidean geometries:

> The demand [for a general definition of space under which different spaces are contained as special cases] stands in conflict, it would seem, with the psychological fact that our spatial intuition, insofar as it should be seen as immediately given, must be further determined as *spatial intuition unique in its kind*. The truth of this classification does not admit any doubt in the wake of Kant's groundbreaking investigations, which, at least in this point, have withstood all attacks. However, it is just as certain that it does not contain the full truth. Mathematical considerations, in particular, show that space can, this original character notwithstanding, also be grasped as a concept, which fits into a great series of kindred concepts as a precisely determined member.[cxxxix]

I will discuss the position Erdmann takes up in more detail later on in this sub-section. For now I want merely, and mainly, to highlight his readiness to openly find fault in Kant's position.

Erdmann takes issue not only with the claim that the forms of intuition are uniquely determined, but also disagrees explicitly with Kant about whether they can be investigated a priori:

> For Kant experience in the objective sense, as the sum of affections by things, has merely occasionalistic meaning: it is the occasional cause which effects the initiation of aprioristic forms absolutely independent of it. For the physiology and psychology of our time, by contrast, which is aided both by the epistemological investigation of the concept of reciprocal interaction as well as the biological theory of psychological development, it has the meaning of a factor which determines the knowledge thus produced equally in form and content. Any attempt to still maintain Kant's doctrine of the aprioricity of the subjective factor of knowledge which is independent of all experience is therefore a lost cause from the start. We require a further development of critical idealism, which, as it cedes to the newly discovered facts their due leads to the empiricist theory.[cxl]

§ 86 These two passages brings into focus what I take to be the central reason for Erdmann's divergence from Kantian doctrine: Erdmann sees himself not merely as propounding an alternative philosophical view, but rather as duly correcting and developing Kant's view in the wake of a shift in the available facts that need to be accommodated by any philosophical position[112]. One such fact is the one Erdmann mentions in the above passage: the recent advance in understanding the physiological processes involved in perception and cognition; another fact Erdmann finds himself forced to take into account is the development of coherent systems of non-Euclidean geometry.

[112] Indeed, Erdmann explicitly acknowledges that, in his view, philosophy is facing "a changed" situation than did Kant's system "from which we are separated by a rich intellectual development of a hundred years" and so he deems it "the most obvious obligation even for the most devoted students of Kant's to apply the doctrines of his master [...] only after thoroughgoing critical examination"[cxli].

Erdmann's account of knowledge as the result of the interaction of two distinct factors, which I will in detail later, in principle, allows for two sources of change in any piece or body of knowledge either as a result of a change in the physical world, or as a result of a change in the psychological processes coordinated with the concepts a given human being employs[113]. I take it, the legitimacy of non-Euclidean geometry within the framework Erdmann sets up is guaranteed by the latter possibility, and that Erdmann has this in mind when he speaks of grasping space *as a concept, which fits into a great series of kindred concepts.*

Since, within Erdmann's framework, what Kant called forms of intuition enjoy no special standing – they are, rather, (acquired) items of knowledge like any other[cxliv] – non-Euclidean geometry can be accounted for as the result of changing what Erdmann called the fundamental "construction concepts"[114]. However, the fact that the physical world human beings inhabit happens to be this particular world, rather than any other, and they happen to use certain concepts, rather than any others, the forms of intuitions human beings *in fact* possess are, according to Erdmann, those underlying Euclidean geometry, although, as Erdmann stresses, this is an empirical discovery[cxlvi].

In the concluding section of the book, Erdmann emphasises, once again, his divergence from Kant. Although § 87
Erdmann does not mention Kant's name explicitly here, I take it the fashion in which Erdmann describes the position he opposes in the following passage, as "necessary, universal and unchangeable in the absolute, rationalistic sense", echoes the earlier descriptions of "aprioristic forms absolutely independent of [experience]" clearly enough to believe that Erdmann has Kant's position in mind (perhaps among others)[115] in this passage:

> We are now able to fully answer the question which serves as the point of departure for our investigation. The axioms of geometry can be called necessary, universal and unchangeable not in the absolute, rationalistic sense. They are not single-handed productions of a mental activity, which confront experience from the first arbitrary occasion with a closed series of finished forms, not eternal truths, which prescribe unalterable laws to an experience, of which they are entirely independent, which, indeed, they themselves manufacture. They are a product of a mutual interaction of two equally essential determining causes, one of which lies within the depths of our mind, the other in the nature of things, and they yield judgements about facts, which express in intellectual terms whatever experience has thus far become accessible to our senses.[cxlix]

Yet, this passage also brings into focus a noticeable harmonising streak running through Erdmann's work: Instead of rejecting outright the idea that there is a necessary form of intuition, Erdmann chooses to distinguish different interpretations of the claim, one of which – the "absolute, rationalistic sense" – he opts to reject, while keeping open the possibility of accepting the formulation of the claim if it is understood in a different sense.

[113] Helmholtz had urged a similar point in connection with non-Euclidean geometry specifically. One can, as Helmholtz observed (in his "Über den Ursprung und die Bedeutung der geometrischen Axiome"), speculatively explain in two ways how somebody could have experience of a non-Euclidean space: Either such experience results from in fact inhabiting a different space and measure it using a Euclidean metric[cxlii], or from human beings applying measures other than the Euclidean metric to Euclidean space[cxliii]. Erdmann refers to Helmholtz' discussion on p. 91 to motivate his theory of experience, which I will discuss in § 95: While Helmholtz builds in his discussion on the fact that measuring, in practice, involves measuring instruments and mechanical processes and so any deviation of experience from Euclidean geometry can be chalked up to a deviant mechanics, Erdmann builds on the observation that concepts have physiological manifestations and any deviation in spatial experience from Euclidean space may be due to them.

[114] This piece of terminology may be echoing a formulation Riemann had used in his *Vortrag*, where he says that geometry presupposes "the concept of space and the first fundamental concepts for constructions in space"[cxlv].

[115] To make the kinship between these formulations more apparent, it may help to recall that Kant suggested defining "a priori" in terms of "necessary" and "strictly universal" in the *Kritik der reinen Vernunft*[cxlvii].

Erdmann also explicitly connects the epithet "rationalist" with Kant's view, and notes that according to such a view, non-Euclidian geometry could not be given a place as a legitimate form of geometry[cxlviii].

An almost jarring example of this streak can be found in Erdmann's discussion of Kant's distinction between "a priori" and "a posteriori", whose characterisation Erdmann also feels the need to correct. One cannot, within Erdmann's framework, distinguish between knowledge which does not depend at all on experience, and knowledge which does, because, as Erdmann sees it, *all knowledge* depends on experience. Still, Erdmann goes to some length to re-interpret Kant's terminology, instead of opting to dismiss it altogether. However, the way Erdmann re-interprets the distinction – as a "contrast of perspective"[cl] –, arguably undermines Kant's motivation for introducing it: Erdmann suggests to regard knowledge as a priori insofar as its content is held to have been as determined by psychological processes, and as a posteriori insofar as its content is held to have been determined by physical processes; however, since all knowledge, for Erdmann, is the result of a process of reciprocal interaction between psychological and physical factors and no knowledge can come about without including both, *all knowledge is both a priori and a posteriori* in Erdmann's sense. There is, thus, no longer a special problem, like the one Kant had been driven by, to account for the very possibility of synthetic a priori knowledge[116].

More important than the position Erdmann takes with respect to Kant's doctrine or to the question whether § 88
Euclidean geometry happens to be *the* geometry of experience, in my view, is the *manner of engagement* with Kant Erdmann's work exemplifies. Erdmann has no qualms, as I have now illustrated at length, explicitly faulting Kant where and when Erdmann thinks – as he not infrequently does – Kant was mistaken, or to (radically) re-interpret Kantian terminology in ways that agree with Erdmann's own point of view, while being entirely estranged from Kant's: Erdmann's re-conception of the "a priori"/"a posteriori"-distinction I just mentioned is one example, Erdmann's conception of the process of reciprocal interaction involved in perception, for whose product Erdmann still retains (e.g. in the passage quoted in endnote clxi) the Kantian technical term "intuition" is another.

Appreciating the fact that "a hundred years of rich intellectual development"[cxli] separate Erdmann's work from Kant's is useful also for evaluating Hilbert's occasional philosophical remarks, because it makes clear that even where Hilbert invokes Kantian formulations, one must not assume that the use of these formulations indicates any adherence to the whole, or even large parts of Kant's position, nor to an interpretation of it which is faithful to Kant's intentions, even in cases where Kant's intentions are (at least taken to be) clear.

I have already noted, for instance (in § 74), that the interpretation of Kant's conception of an idea which Hilbert outlined in 1925 arguably diverges substantively from Kant's intentions. This 100-year-distance can also be cited as evidence supporting the hypothesis that Hilbert's use of a quotation from Kant as the epigraph to his *Grundlagen der Geometrie* does not signal any adherence on Hilbert's part to Kant's philosophy of geometry[117].

[116] In passing I'll note a parallel: Peirce maintained (in 1878) that the distinction between a priori and a posteriori was, indeed, not of fundamental importance: "But before asking [the question how synthetic judgements a priori are possible] [Kant] ought to have asked the more general one, 'How are any synthetical judgements at all possible?'." (Charles Sanders Peirce. "The Probability of Induction." In: *Writings of Charles S. Peirce: A chronologial edition. Volume 3: 1872-1878.* Ed. by Christian J. W. Kloesel. Indiana University Press, 1986, pp. 290–305, p. 304).

[117] I find myself in verbal agreement with Hallett & Magal on this point, but I take myself rather to be disagreeing with their underlying estimation of the situation insofar as they appear to treat Hilbert's taking a stand specifically about Kant's philosophy of geometry as the most proximate possibility – since they consider no alternative – of what Hilbert may have seen in Kant, rather than one of the most remote, and to take rejecting it, as I think they are right to, as cause not to search for *any* (other) portion of Kantian doctrine

3.1.2.2 Erdmann's Conception of Geometrical Concepts as Ideas

As I emphasised in the previous sub-section (§ 73), one characteristic hallmark of ideas as Kant conceives of them § 89
is that no intuitions of the objects the ideas purport to grasp are available. With this conception of ideas in mind, Erdmann's immediate reason for claiming that the primitives of geometry are ideas, rather than concepts (which differ from ideas, as I emphasised earlier, precisely on account of there being corresponding intuitions available) or themselves intuitions[118]: Because they are such that no intuition corresponds to them, they are ideas. However, this passage by itself provides no reasons for the claim Erdmann makes about the status of the primitives of geometry.

To shed more light on what position, exactly, Erdmann is here expressing, I will turn to the reasons he adduces § 90
in support of the claim that geometry really deals with ideas. As I will propose to understand Erdmann's position, it results from the confluence of Erdmann's conception of geometry, on the one hand, and his conception of empirical knowledge, on the other. I will take as a point of departure for my discussion of Erdmann's position an observation about the nature of mathematical proof, which Erdmann presents immediately after the remark I just quoted:

> The nature of geometric proof, too, supports this inference. [A proof] consists in nothing other than the reduction of tangled measure-relations, which are stated in the proposition, to simpler measure-relations already familiar; but these point ultimately to the simplest construction-concepts or ideas. Geometric proof, too, thus uses only spatial concepts; since the development of geometry is conditional only on applying these constructive foundations to ever more complicated measure-relations. The particular geometric intuitions are only visual representations of the spatial concepts, which are contained in them. Were it expedient to apply a term to the seeming paradox of this double-nature, one could call the geometric form of space "conceptual intuition".[cli]

After factoring out the complications due to Erdmann's specific view of geometry – Erdmann, following Riemann & Helmholtz, holds that geometry is fundamentally concerned with the investigation of measures applied to figures in space[119] –, a relatively simple basic idea can be discerned: Proofs, Erdmann says, aim to "reduce" particular geometrical facts to other, previously known such facts, and ultimately to certain fundamental facts.

Hilbert may be adopting or at least adapting. (Their argument strikes me, at any rate, as highly dubious: To argue that Hilbert could not possibly subscribe to Kant's views of geometry because Hilbert wants to exclude any "intuitive grasp" of geometrical concepts from consideration while for Kant geometry is centrally concerned with intuition seems to me based on an equivocation between two very different senses of "intuitive" (namely, the sense of "pre-theoretical" relevant to Hilbert with the sense of "based on a particular source of knowledge" relevant to Kant.)) In my opinion, accepting the alternative between Hilbert taking a stand on Kant's philosophy of geometry and there being no direct connection at all as accurate is predicated on too narrow an estimation of what Hilbert could have found relevant and interesting in Kant's work, and, especially, in the section of Kant's book from which he quotes, which is not (anywhere near) the section where Kant articulates his philosophy of geometry; that Hilbert does not quote from the part of Kant's *Kritik der reinen Vernunft* where Kant discusses geometry is also noted by Volkert, who notes that the epigraph "gehört auch nicht zu den einschlägigen Stellen, die man heranzieht, um Kants Philosophie der Mathematik zu erläutern" (Klaus Volkert, ed. *David Hilbert: Grundlagen der Geometrie (Festschrift 1899). Kommentiert von Klaus Volkert*. Springer, 2015, p. 170).

[118] The last of these options may have been Kant's own view of geometry, specifically. Michael Friedman. "Kant's Theory of Geometry." In: *The Philosophical Review* 94.4 (1985), pp. 455–506, p. 470 summarises Kant's views in the claim "that our cognitive grasp of the notion of space is intuitive rather than conceptual or discursive". In Michael Friedman. *Kant and the Exact Sciences*. Harvard University Press, 1992, p. 56, Friedman remarks: "What is most striking to me about Kant's theory,a s it was to Russell, is the claim that geometrical *reasoning* cannot proceed 'analytically according to concepts'—that is, purely logically—but requires a further activity called 'construction in pure intuition.'" Friedman returns to this issue on p. 123 to note that "the pure intuition of space is necessary to make geometrical *concepts* first possible".

[119] In the introductory remarks preceding his "Ueber die Hypothesen, welche der Geometrie zu Grunde liegen," Riemann states as the aim of his investigation "to construct the concept of a multiply extended magnitude on the basis of general concepts of magnitude" and identifies as the eponymous "hypotheses underlying geometry" the "the simplest facts on the basis of which one can determine the measure-relations of a space"[clii]. Helmholtz notes in his "Über den Ursprung und die Bedeutung der geometrischen Axiome" that "all spatial relations known to us are measurable, i.e. they can be reduced to the determination of magnitudes"[cliii], and goes on to explicitly acknowledge Riemann's work as an example of this perspective on geometry.

Rather than dwelling on what, exactly, Erdmann may mean by his talk of reduction, I want to concentrate on a more abstract consideration, which I take Erdmann to presuppose in this passage. In order for some complex claim expressed by using complex concepts to be reducible ultimately to claims in which only primitives occur, the latter have to, themselves, have conceptual content; otherwise they could not adequately recapture the conceptual content of more complicated geometric claims[120]. So if geometrical concepts are ever at play in the development of geometry, then even where geometry seems to be ultimately based only in intuition, it must, in fact, always at least also be dealing with concepts, which are, as Erdmann says: "contained" in these intuitions.

In light of this argument, geometry either is thoroughly conceptual, or, if not that, then it cannot involve concepts at any point in its development, since whatever conceptual content there is somewhere ramifies throughout the deductive structure of geometry. This argument alone, as I am suggesting to understand it, does not establish that geometry *as a matter of fact* does have any conceptual content. Accepting it is compatible with what may have been Kant's view of geometry, that it is intuitive through and through (see footnote 118).

§ 91 A slightly more general line of thought seems to me to hold a certain plausibility also independently of Erdmann's particular assumptions about the function and nature of proofs, when formulated in more abstract terms; that is to say, it seems plausible to me *regardless of* whatever more specific sense one attributes to the key words: If proofs are understood as «deducing by means of logic» (in some broad sense) certain propositions from certain assumptions, both the propositions established as well as the assumptions relied on must be the right kind of thing to feature in logical deductions. If, furthermore, «logic» (in whatever broad sense) is understood as operating on the level of «concepts», then mathematical facts must have conceptual content. Otherwise, the apparent main device for furthering geometrical inquiry – proofs – would be ill-equipped for its purpose; (and also any aspect of a geometrical fact which falls irretrievably outside the conceptual sphere is ill-fitted to figure in geometrical inquiry, in so far as it proceeds centrally by way of proof). I believe it is a strong argument weighing against what seems to have been Kant's position, i.e. the view that (some parts of) mathematics are based entirely and exclusively on intuitions, that from such a point of view mathematical proof would be either superfluous or unfit for its purpose.

§ 92 Independently of this consideration that, given that complex geometrical claims apparently deal with concepts, any primitive basis one which these claims are supposed to rest must already involve concepts, otherwise it cannot furnish a foundation for those complex claims, Erdmann offers one additional reason in support of his claim that the primitives of geometry *in fact* have conceptual content. This second reason, however, consists only in claiming that this assertion requires "no further discussion" beyond simply pointing to analytic geometry[clv].

Gesturing towards analytic geometry is sufficient, I take it, because, as Erdmann sees it, analytic geometry explicitly relies on concepts already at the base level. Therefore, anyone inclined to regard analytic geometry is a viable approach to geometry will thereby grant that geometry proceeds from concepts[clvi].

[120] This same observation was stated with exemplary concision already by Helmholtz, who observed that "no calculation can yield a relation between magnitudes not already contained in the equations constituting the base from which calculation proceeds"[cliv].

In fact, it seems to me this consideration even supports the claim that the primitive basis of geometry is *entirely* conceptual. Assuming that analytic geometry is not taken to be in principle incapable of capturing certain aspects of geometry would entail that all content of geometry is conceptual content.

§ 93 I want to note in passing that Erdmann's views are but one representative of a certain kind of view advocated also by other philosophers belonging to the so called "Neo-Kantian" movement, broadly speaking. For example, analytic geometry plays a pivotal role also in motivating Cassirer's conceptions of geometric concepts: Cassirer in the introduction to *Substanzbegriff und Funktionsbegriff*. Furthermore, the view of geometric concepts both Erdmann and Cassirer espouse – though Erdmann speaks of "construction-concept" ("Konstruktionsbegriff"), while Cassirer speaks of "function-concepts" ("Funktionsbegriffe"), both have in mind as paradigms for the special kind of concept they mean the definitions of geometric objects in analytic geometry – are not altogether dissimilar in substance: Like Cassirer, Erdmann stresses that the concepts of geometry cannot be explained as products of a process of abstraction (and, like Cassirer, Erdmann thinks the same is true of mathematical concepts more broadly), and that adequately accounting for them requires a certain extension of logical theory[clvii].

This affinity between Erdmann's and Cassirer's views points, in my opinion, not to any direct and decisive influence Erdmann may have had on Cassirer's work – in fact, in *Substanzbegriff und Funktionsbegriff* Cassirer cites only Erdmann's *Logik* (using the second, revised edition published in 1907[121]) –, but rather to the fact that both belong to an overarching tradition (of «neo-Kantianism») and both articulate certain ideas about the nature and role of concepts, especially as seen in mathematics, which were in the proverbial air.

Indeed, the entire line of thought I just presented was already put forward concisely by Hermann von Helmholtz himself in his "Über den Ursprung und die Bedeutung der geometrischen Axiome," where he noted:

> The whole execution of a calculation is a purely logical operation; it cannot yield a relation between the magnitudes subjected to calculation which was not already contained in the equations constituting points of departure for the calculation. The newer investigations were for this reason carried out nearly exclusively by means of the purely abstract methods of analytic geometry.[clviii]

§ 94 These two arguments – the first based on the consideration of epistemological aspects of mathematical proof, the second based on the conceptual nature of analytic geometry – do not yet establish Erdmann's claim that the concepts of geometry are *ideas* in Kant's sense of the word. They establishes only a first step towards this goal, namely, that *concepts* play a role in geometry. As I understand Erdmann's overall position, he infers the further conclusion that these concepts are ideas from his conception of empirical knowledge, to which I will now turn.

§ 95 According to Erdmann, experience is the product of a process he labels "reciprocal interaction" ("Wechselwirkung"), which mixes perceptual data and conceptual content. The basic idea underlying Erdmann's conception of experience is the observation that the physiological processes constituting perception depend on both the physical

[121] See: Ernst Cassirer. *Substanzbegriff und Funktionsbegriff. Untersuchungen über die Grundlagen der Erkenntniskritik*. Ed. by Reinhold Schmücker. Vol. 6. Hamburger Ausgabe der gesammelten Werke Ernst Cassirers. Meiner Verlag, 2000, p. 9.

properties of the surrounding world and the physiological properties of the sense-organs[122], which Erdmann assumes are connected in some fashion with certain elements of human psychology[clx]. Perception emerges as a product of the physical world acting on the sense organs, and the sense organs reacting according to their disposition as well as the physiological disposition of the human cognitive apparatus on the whole.

A central tenet of Erdmann's position is that the result of this process of reciprocal interaction does not accurately represent the physical component it incorporates, i.e. the impression from the object; nor does it accurately represent the other, psychological component. The two different factors entering into the process of reciprocal interaction, as it were, «scramble» each other: On the one hand, a human being's peculiar psychological constitution ensures (by way of its physiological manifestations) that external objects and relations cannot simply be «mirrored» in the perceptual apparatus; while, on the other hand, the influence of external objects and relations ensures that perception cannot be exhausted by imagination. Erdmann condenses this conviction into a slogan, according to which whatever human beings perceive of physical things are "mere signs for the things"[clxi]; in addition, Erdmann introduces a pair of technical terms to express his claim: that between intuitions and things there is merely a "correspondence" but never "congruence"[123]. Erdmann goes to some length to underscore that he is making a stronger claim than merely asserting that *one cannot know whether* perceptions are precisely accurate; Erdmann is rather claiming that perceptions *are certainly not accurate*, that they always positively diverge from the objects and facts perceived[clxiii], as well as from the concepts of these objects and facts incorporated in the perception.

That, according to Erdmann, sensory information and psychological processing mutually distort one another constitutes a decisive departure from Kant: Kant never so much as suggests that concepts have physiological manifestation capable of interfering with perceptual mechanisms. Consequently, Kant, as I understand him, maintained that in case an intuition could be found to match a given concept in experience, the resulting knowledge would be of the object grasped by the concept *exactly as the concept grasps it*. Erdmann, as I understand his position, denies neither that there are intuitions or concepts as separate factors of knowledge, but that knowledge is produced by «matching» concepts up exactly with corresponding intuitions; instead, according to Erdmann, knowledge comes about by «meshing» together intuitions and concepts in an inextricable jumble different from both.

§ 96

I take it this view underlies the assertion found in the first passage I quoted from Erdmann's book, that geometrical intuitions are not "adequate visulisation[s]" of geometric concepts. But in light of Erdmann's conception of empirical knowledge, as I just presented it, a much more sweeping claim seems to hold: Within the framework Erdmann sets up, it seems impossible in principle for *any* concept to ever be adequately represented in an intuition because a concept is only one of two factors entering into the process yielding empirical knowledge, each of which

[122] At one point in his discussion, Erdmann equates the distinction between psychological and physical processes with the distinction between "the world of consciousness" and "the world of motion" (fully aware of the vagueness of this contrast)[clix].

[123] These terms must be understood in such a way that congruence entails correspondence, but not conversely, so that correspondence in the relevant sense is compatible with the opposite of congruence. Roughly speaking, an object and an intuition are in correspondence just in case each property of the object can be associated with exactly one feature of the intuition; congruence in the relevant sense would further require that this association is rooted in a *similarity* (in some sense) between properties of objects and features of intuitions[clxii].

invariably distorts the other. If the impossibility of «matching» an intuition precisely to a given concept provides a reason to classify this concept as an idea in Kant's sense, it appears that within Erdmann's framework *every concept turns out to be an idea* in this sense[124], including, in particular, the (fundamental) concepts of geometry.

A way to make some sense of this at least initially startling conclusion, and at the same time a way towards § 97
answering the question whether any part of Erdmann's views or arguments can plausibly be attributed to Hilbert, is to reformulate the chief reason supporting Erdmann's conclusion as follows: Concepts are entirely precise, indeed, they set the standard for precision. Experience, by contrast, is, owing to the noisiness of the human epistemic process, imprecise. Consequently, what a human being comes to know about the real world always and essentially falls short of the precise, «idealised» concepts resorted to in order to describe it.

One finds this view expressed in the following passage:

> It is a process we go through in the same way whenever we construct a theory to account for any empirical data [...]: *the results of any observation hold always only within certain limits of exactitude and under particular conditions; by setting up axioms, we substitute for these results propositions of absolute precision and generality.* In this "idealisation" of empirical data lies, in my view, the proper nature of axioms.[clxv]

The passage I just quoted is taken from an article by Felix Klein. Its significance for my present purposes consists primarily in the fact that Hilbert cites this article, which expresses something like Erdmann's view, in the prefatory remarks of the *Grundlagen der Geometrie* as one of only two "summary and explanatory reports" – the other is the German translation of Veronese's book on the foundations of geometry – of the role of axioms in geometry.

That Hilbert cites Klein's article, in particular, as a report on the role of axioms in geometry indicates that Hilbert may intend to endorse specifically passages like the above where Klein articulates views about the nature and role of axioms. If so, this would establish a plausible connection between Erdmann's views and the philosophical position Hilbert can be presumed to have occupied. Hilbert's endorsement of Klein's account of axioms would make it plausible to ascribe to Hilbert a belief in one of the two key presuppositions underpinning Erdmann's claim that the foundational concepts of geometry are ideas, namely, that no concept which is precisely defined, as the primitive concepts of axiomatic systems are, can ever be accurately represented in experience.

The view I just described, i.e. that geometric concepts impose precision where there is none to be found, was, however, as a matter of historical fact, not original to either Klein or Erdmann. It can be traced back at the very least as far as an article published by Friedrich Ueberweg in 1851, which was entitled "*Prinzipien der Geometrie*" (and based on Ueberweg's Habilitationsschrift). In it Ueberweg distinguished three steps in the development of geometry, the second of which is "idealising our intuition by imputing to it absolute, infinite exactitude"[clxvi]; indeed, it seems likely to me that Erdmann was at least indirectly influenced by Ueberweg's article because, while he does not, as far as I see, cite Ueberweg's article directly (he may, of course, still have read it, regardless), Erdmann does

[124] By accepting this claim about concepts *generally*, Erdmann shows himself to a neo-Kantian philosopher. This thesis seems to reflect the influence of Johann Friedrich Herbart on Neo-Kantianism: Herbart writes in the section of his *Lehrbuch zur Psychologie* dedicated to discussing "logical forms" that concepts "are rather like logical ideals which our real cognition approximates more and more closely"[clxiv]

cite Joseph Delbœuf's 1860 monograph entitled "*Prolégomènes philosophiques de la géométrie*"[clxvii], which contains a translation of Ueberweg's entire article (with some revisions communicated to Delbœuf by Ueberweg himself[clxviii]) as an appendix, and discusses Ueberweg's views on numerous occasions throughout the exposition of his own.

§ 98 The other presupposition which turned out to play a crucial role in justifying Erdmann's claim, as I proposed to understand its justification – i.e. the claim that geometry is a thoroughly conceptual science – can also, I think, plausibly be ascribed to Hilbert, even if the evidence for this is scarcer and thinner: Hilbert goes on to say in the prefatory remarks to the *Grundlagen der Geometrie* that "the task [of formulating axioms and researching their interconnections] amounts to the logical analysis of our spatial intuition"[clxix]. I believe it is significant that Hilbert speaks of the "logical analysis" of spatial intuition, rather than, as Kant had, of the "synthetic determination" of space[clxx]. It seems to me, the reference to logic, and hence (at any rate, within a broadly Kantian framework[125]) to concepts, constitutes a superficially slight but none the less substantive divergence from Kant's view[126] akin to the claim Erdmann derives from his observation of the reliance of geometric proofs on conceptual content. (Speaking of a "logical analysis of intuition" would for Kant, in any case, have been a contradiction in terms, because as I understand Kant's position, intuition and logic are for him two entirely separate branches of mental activity.)

In fact, the introductory remarks to the *Grundlagen der Geometrie* simply reproduce (with some stylistic changes) a formulation already used in the introductory remarks to the official, publicly available lecture notes for the 1898-99 lectures[clxxii]. More to the point, however, is the appearance of this formulation in Hilbert's personal lecture notes for that course, where he appends to it a reference to Pasch[clxxiii].

At the end of the passage Hilbert refers to, Pasch remarks that foundational research aims to "collect the essential pieces completely and set aside the superfluous ones" from among the "expressions and intuitions" scientific developments are initially motivated by[clxxiv]; taking Hilbert's formulation to be inspired by this particular discussion in Pasch's work, as Hilbert's reference seems to suggest it was, makes clear beyond all doubt that Hilbert did not have in mind anything like what Kant had in mind when claiming that geometry is the science of intuition; indeed, Pasch precisely urges that anything not fixed in precise concepts and thus made amenable to figure in rigorous proofs, has no place in scientifically developed geometry.

§ 99 So far my discussion of Erdmann's views, and of the apparent intellectual as well as historical proximity of Erdmann and Hilbert, has served mainly the purpose of elevating the second suggestion I made for reconstructing Hilbert's intentions with his choice of epigraph in the previous section beyond being just a speculative interpretation, to make it a plausible historical hypothesis of what Hilbert, in fact, meant his epigraph to signal.

That Erdmann explicitly held the view that (really all, but, in particular) the fundamental concepts of geometry are ideas in Kant's sense, because they are never realised in experience, and that Hilbert seems to have also held the

[125] I will discuss the precise contours of Hilbert's Kantianism, as I see it, in more detail in section 3.3.5.2.

[126] Bruno Bauch. "Erfahrung und Geometrie in ihrem erkenntnistheoretischen Verhältnis." In: *Kant-Studien* 12 (1907), pp. 213–235 comments on this formulation in a similar fashion. Bauch says that Hilbert's foundational perspective clearly requires "what we were missing in Kant's own work", namely, that "intuition is made accessible to logical analysis and determined logically"[clxxi].

assumptions on which Erdmann's position is based, makes it plausible, to my mind, to attribute to Hilbert the view one finds articulated by Erdmann (and also by Klein): i.e. that the (fundamental) concepts of geometry are ideas in Kant's sense. As I understand Hilbert's early foundational work, he holds a view like Erdmann's, but Hilbert's position, as I see it, also features the other «Kantian streaks», which is less prominent in Erdmann's philosophical theory of geometry: Hilbert uses the ideas which constitute the foundations of geometry in accordance with Kant's doctrine of the "regulative use of ideas": as means of *systematising* geometrical knowledge.

3.1.3 The Contours of Hilbert's Philosophical Position

§ 100

Before moving on, I want to discuss a very general distinction Erdmann draws between different kinds of inquiry any piece of knowledge can be subjected to. I think this distinction is of some use to put a name to what separates Erdmann's investigation from Kant's and it can help draw the contours of Hilbert's position still more precisely. At the beginning of the third chapter of his book, Erdmann distinguishes in very general terms four different kinds of inquiry, which he labels "scientific", "psychological", "logical" and "epistemological". The first is later also said to concern "objective", the latter three "subjective" questions concerning knowledge.

For Erdmann, Kant's investigations seem to belong to the second of these four domains, i.e. to psychology. Erdmann does not say so directly, but there is some indirect evidence that he takes this view. First, in the passage I quoted in endnote cxxxix, Erdmann calls the result of "Kant's groundbreaking investigation" "a psychological result". Second, in the third chapter, Erdmann notes that "psychological hypotheses" are, in principle, independent of (objective) scientific investigations and goes on to mention "the question whether the content of our concept of space is a priori in the nativist sense"[127] as an example of such a psychological hypothesis[clxxvii].

Implicit in this classification of Kant's work is the assertion that Kant's work has no bearing on what Erdmann calls the "scientific investigation" of geometric knowledge. According to Erdmann's general description of his four kinds of inquiry, scientific inquiry is devoted to two questions: What is the content of (in this case: geometric) knowledge, and what systematic place does it occupy within the totality of knowledge?

Hilbert alludes to such a distinction twice in his early lectures on geometry: The first reference occurs in the lecture-notes for Hilbert's 1891 lecture course, where he contrasts the axiomatic approach to geometry with the analytic approach and ascribes to the former "epistemological", to the latter "mathematico-scientific" significance[clxxviii]. The second reference occurs in the passage I quoted in endnote clxxii. For what it's worth, the second of these reference echoes more closely Erdmann's formulation (as quoted in clxxvii), while the first bears a certain affinity to a formulation used by Klein. In the article cited by Hilbert, Klein, in addition to drawing the same distinction

[127] I have given some evidence in footnote 115 that Erdmann sees (or, at any rate, occasionally calls) Kant as a "rationalist"; Erdmann does so explicitly in his discussion of Kant's conception of the a priori[clxxv]. Erdmann applies the label "nativist" interchangeably with "rationalist", and does so in particular in a passage dedicated to the characterisation of Kant's views[clxxvi]. For these reasons I think it is plausible to see this formulation as intended to invoke Kant's position; the formulation that the concept of space is "a priori" does, in any case, invoke that association, independently of whether Erdmann intends "nativist" to apply to Kant.
There is precedent for calling Kant's work "mainly psychological" in Herbart.

between philosophical and mathematical questions concerning geometry, sketches the following project:

> The [...] investigations of the theory of parallels [...] show, first, (...) that the parallel-axiom is not a mathematical consequence of the commonly presupposed axioms, but rather an essentially new aspect of intuition, which was not touched upon in previous investigations, came to be expressed in it. Similar investigations could and should be carried out with respect to every axiom and not just in geometry: one would thereby gain insight into the mutual dependence of the axioms.[clxxix]

Klein frequently distinguishes his work – and his whole approach – from that of Riemann, in particular, and what he sees as Riemann's overindulgence in philosophical speculation[clxxx]. To that end, Klein disavows any implications his theories may have for the "philosophical" or "speculative" questions about the intuition of space, and emphasises, instead, the "mathematical questions" he sees his own work as addressing[128].

I presented the distinction Erdmann draws between scientific and psychological investigations, as well as Klein's distinction between mathematical and philosophical questions, as evidence for the gradual emergence to full and clear recognition in the methodological reflection upon geometry around Hilbert's time of a certain problem for further mathematical research to address, namely, the problem of what Klein calls the "mutual dependence of axioms". In this connection I want to highlight a passage found in the concluding section of Erdmann's book, where § 101
Erdmann sketches in general terms a vision for "a rigorous presentation of geometry":

> The synthetic character of geometrical propositions lies in this: that in each one an expansion of axioms to new complications of construction-concepts takes place. Every proof thus aims, as was mentioned earlier, to reduce the measure-relations stated in the proposition to the simplest measure-relations, as contained in the axioms (associated with the elementary definitions). The task of geometrical development is thus to order the system of propositions in such a way that each subsequent proposition becomes a more complicated presentation of possible measure-relations and to carry out proofs in such a way that no [proposition] can be placed instead of an earlier one without confusing the mutual dependencies. A rigorous presentation of geometry will, therefore, first treat those propositions which depend only on the axioms of congruence, and are thus common to all three geometries of spaces with constant curvature. Then it will focus on those which, like the first 28 propositions of Euclid [...], can be deduced from the axiom of the straight line, and apply equally to our planar space and to the pseudospherical one. Only then can those be added which also require the axiom of angle-sums.[clxxxiii]

Leaving aside, once again, how, exactly, Erdmann intends to structure geometry in detail, the project he sketches on the whole – organising an exposition of geometry along the lines of deductive (in-)dependence – seems to be precisely what Hilbert undertook to carry out in his *Grundlagen der Geometrie*.

The problem I take Klein and Erdmann to be articulating and which both of these writers recognise as immediately contiguous with their own work but not yet accomplished in it *is* the very problem I see Hilbert as taking on (perhaps following not least of all Klein's sketch) in his foundational work: the problem of systematising geometry, which involves, in particular, laying out a (minimal) selection of (mutually independent) axioms, but also exploring the relations of deductive (in)dependence between various axioms and results.

[128] The footnote I cited quoted in endnote clxxx, where Klein laments that "Riemann did not always keep distinct the purely mathematical consideration from considerations of a more speculative character"[clxxx] is found in Klein's second article on non-Euclidean geometries. Klein also draws this distinction in his first article on non-Euclidean geometry, where he professes disinterest in the "philosophical speculations" of Riemann and Helmholtz, contrasting that aspect of the work of Riemann and Helmholtz with what Klein calls the "mathematical results" contained in or emerging from their work[clxxxi] and also in his *Erlanger Program*[clxxxii].

These are, in my view, the contours of Hilbert's foundational work: Hilbert is, above all, a systematiser. This description should be understood in the sense I outlined earlier (in § 76): For Hilbert, the question about the deductive (in-)dependence of mathematical results is as important a question for research as the discovery and justification of new results. It is Hilbert's emphasis on and dedication to pursuing questions concerning the systematisation of mathematics which distinguishes Hilbert from his contemporaries; and, as I will document throughout the remainder of my discussion, does so not only at the time when he prepared and published the *Grundlagen der Geometrie*, but throughout Hilbert's career. Describing Hilbert as first and foremost a "systematiser" is not meant to deny that Hilbert contributed his own share of novel mathematical results, only to describe in outline the stance from which Hilbert did so. Far from detracting from discovery and innovation, as it may at first appear to do, this stance in fact ultimately fosters discovery and innovation in its own way:

> Our task is to systematize the inferences and claims to knowledge made and advanced by previous and contemporary investigators in a variety of fields, and in performing this task we may easily reject some popular types of inference and repudiate some knowledge claims. We [...] attempt to present the history in a way which will conform to a growing account of rationality, an account which tries to expose the most general features of human knowledge and correct human inference.[129]

Using Hilbert's own phrase, I am suggesting to conceive of Hilbert's approach as "thinking conscientiously" always also of the overarching connections tying together the discipline one is thinking about[clxxxiv].

3.1.4 Hilbert's Early Lectures

I have so far relied for my reconstruction of Hilbert's philosophical position mainly on his 1899 booklet, *Die Grundlagen der Geometrie*. Indeed, I began by focusing exclusively on a single detail – the quotation from Kant Hilbert chose as his epigraph –, expounding certain implications gleamed by the light of the historical circumstances in a speculative manner at first, then documenting that Hilbert did in fact accept the claims I set out purely speculatively, or at least accepted claims which effectively commit Hilbert to accepting them. § 102

There are also other sources shedding some light on Hilbert's philosophical position as it was at that stage in his career, namely, the lecture courses Hilbert delivered as part of his regular teaching obligations first in Königsberg, then, from 1895 onward, in Göttingen. None of these lectures are especially richer in indications about Hilbert's methodological standpoint or his philosophical views than the scattered remarks I have seized upon in the *Grundlagen der Geometrie*, which emerged as the culmination of almost a decade of evolving work Hilbert pioneered in his lecture courses[130], and none of them seem to me to go in substance beyond what I have extrapolated up to this point from Hilbert's book. They are, however, of value for my present purposes as sources for independent corroborating evidence – and I have occasionally referred to some of these lectures already in footnotes throughout

[129] Philip Kitcher. *The Nature of Mathematical Knowledge*. Oxford University Press, 1983, p. 98.

[130] The progressive build-up of the material eventually included in Hilbert's 1899 *Grundlagen der Geometrie* is explored in detail by Toepell, *Über die Entstehung von David Hilberts "Grundlagen der Geometrie"* and the editors' introductions in Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*.

the preceding section – affirming the image of Hilbert I have arrived at, namely, that of a systematiser.

§ 103 Hilbert delivered a series of lecture courses in the 1890s, which cumulated in the publication of the *Grundlagen der Geometrie* in 1899, as well as one subsequent lecture course in 1902; after 1902, Hilbert seems not to have devoted any more lecture courses entirely to geometry until 1920, when Hilbert taught a course entitled "Anschauliche Geometrie"[131]; the absence of lecture courses dedicated expressly to geometry should not be taken as an indication that Hilbert ceased lecturing on geometry altogether between 1902 and 1920: Though Hilbert seems to have dedicated no lecture courses mainly and entirely to geometry, the lecture courses dedicated to foundational questions he delivered between 1905 and 1918 all include a more or less extensive discussion of geometry, there presented as a successful case of foundational investigation by means of the methods Hilbert's foundational lectures are dedicated to developing. (I will discuss these foundational lecture courses in detail in the next section.)

Indeed, these lectures also show Hilbert developing his geometrical ideas, albeit only in minor ways compared to the strides he took in the late 1890s. I want to mention just one example, namely: the weakening of the parallel-axiom, which in 1902 is already weakened from stating that "there exists one and at most one" parallel to "there exists at most one" (since the existence claim can be deduced from the other axioms) and in a (presumably later) correction to the 1910 lecture notes one finds Hilbert weakening it even further to say "there exist at most two different lines through a given point not on a line which do not intersect the line" (p. 131).

§ 104 The content and focus of Hilbert's early lectures on geometry in the 1890s changes from year to year, but in the introductory remarks, Hilbert constantly rehearses similar considerations, which I find most clearly articulated in the introductory remarks to the 1894 lecture course also entitled *Die Grundalgen der Geometrie*[132]:

> Among the *phenomena* or *facts of experience* which the observation of nature presents us with is an especially remarkable group, namely, the group of those *facts* which determine the exterior shape of things. These facts are the concern of *geometry*. Every science aims to establish order among the facts which lie in its domain, or, as Kirchoff says, to describe these facts, and so, too, does geometry among those *geometric facts*. This establishment of order or description is accomplished by means of certain *concepts*, which are *to be related* amongst themselves according to the laws of logic. A science is more advanced, in other words, the *architectonic structure [Fachwerk] of concepts* is more advanced to the extent that any given phenomenon or fact can be *assigned a place* more easily. [...] Since, however, not all concepts are deduced by *pure logic*, but [some] rather arise from *experience*, the most important question we will treat in this lecture is that concerning the *basic facts* which suffice for the construction of geometry in its entirety. We must fix these no longer *provable* facts from the outset and call them axioms. [...] The problem for our lecture course can be formulated thus: What are the *necessary* and *sufficient*

[131] A comprehensive list of Hilbert's lecture courses can be found in Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*, pp. 609–623, and in an updated version in subsequently published volumes of the edition of Hilbert's lectures (later versions of the list are updated insofar as they reflect a fuller awareness of the archival sources, the list of courses remains the same). It is further worth noting that towards the end of his teaching career, Hilbert even returned to delivering courses entitled "Grundlagen der Geometire" (first in 1927, then again in 1932 and in 1933-34 (this, incidentally, was also the last course Hilbert taught); for some discussion of these see the editors' introduction to Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*, pp. 6 sq.). The lecture course on "Anschauliche Geometrie", like the lecture courses on the foundations of geometry in the 1890s, yielded a book, namely, the *Anschauliche Geometrie* published jointly with Stephan Cohn-Vossen in 1932.

[132] This lecture course, as well as the other lecture courses on geometry Hilbert delivered between 1891-1902 have been published as Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*. The opening remarks in all the other lecture courses published in that volume are shorter and less detailed than the following. In fact, the opening remarks to the 1898/99 lecture course is merely a compressed reworking of the one from the 1894: In Hilbert's own lecture-notes, the opening remarks consist merely in a reference to the relevant portion of the 1894 lecture notes to be included there[clxxxv]; in the official lecture notes made available to students, a slightly shorter reformulation of the remarks from 1894 takes the place of that reference (quoted in the following endnote). I will discuss the opening remarks to the 1902 lecture course separately below.

> and among themselves *independent* conditions, which one must require of a system of objects [or units] in order that every property of these objects *corresponds* to a geometric fact and conversely, so that by means of this system of things a *complete description* or *ordering* of all geometric facts is possible.[clxxxvi]

Although Hilbert mentions "basic facts which suffice for the construction of geometry in its entirety", i.e. (effectively) axioms, the emphasis in this passage lies squarely on the organisation of geometrical facts, or, as Hilbert says in the above passage, on "establishing order among facts". That is the starting point, the criterion of success in scientific development, as well as the ultimate aim Hilbert outlines in the last sentence of the passage. This emphasis on ordering geometric facts, while the identification and statement of axioms plays a comparatively minor role, corroborates my claim that Hilbert saw *systematisation* as an integral part of his foundational investigation.

(This criterion is arguably more demanding than the requirement that systems of (geometric) axioms must be complete (which I'll discuss in section 3.2.3.1), because it requires not only *that* every result can be accommodated, but, moreover, that one can determine *where* in the deductive structure it belongs.)

In the reformulation of the introductory remarks from 1898-99, Hilbert calls geometry "with respect to its structure a system of sentences"[clxxxvii], which further attests that Hilbert saw the investigation of its deductive structure on the whole, in addition to selecting viable axioms, as essential to geometry.

This passage is, as far as I am aware, the earliest in all of Hilbert's writings, where the metaphor of mathematical theories as "architectonic structures of concepts" occurs[133]. It seems to me plausible in light of the especially pronounced emphasis on ordering results in a deductive structure in the above passage to think that what guided Hilbert's choice of this metaphor of this *particular* style of architecture – rather than merely speaking, e.g. of a "building of concepts" – was the specific visual appearance of buildings-built in such a style, which is marked, especially, by the clear visibility of the various beams which sustain the building[134].

The opening remarks to Hilbert's 1902 lecture (also entitled *Grundlagen der Geometrie*) further support the claim I just made that Hilbert's metaphor of an "architectonic structure of concepts" is meant to highlight, in particular, the systematic organisation of geometric results in a deductive structure. There, Hilbert explicitly coordinates the "ordering" of geometric facts with the "construction of an architectonic framework of concepts":

[133]The German word "Fachwerk" refers to a particular building style, which, as Schlimm reports in his discussion of this passage (without, however, citing a source), is known in English "as half-timbered or Tudor-style houses" (Dirk Schlimm. "Metaphors for Mathematics from Pasch to Hilbert." In: *Philosophia Mathematica* 24.3 [2016], pp. 308–329, p. 316).

It may be worth pointing out that Hilbert was not the first to use this metaphor for theories: One finds the word "Fachwerk" used in this way already in Herbart's *Lehrbuch zur Einleitung in die Philosophie* in a remark to § 13 added in the third edition, where Herbart warns against "getting used to certain architectural structures [Fachwerke] into which everything is supposed to fit"[clxxxviii].

[134]Schlimm's interpretation of the metaphor as conveying Hilbert's view of mathematics as "something thoroughly artificial", as opposed to "something natural, independent of human constructions" (Schlimm, "Metaphors for Mathematics from Pasch to Hilbert," p. 319) seems to me largely to ignore the very sophistication and detail of Hilbert's architectural metaphors, which Schlimm painstakingly documents. Indeed, in spite of Schlimm's emphasis in the introductory remarks that one must "pay careful attention to how the metaphors are actually used in each specific instance" (Schlimm, "Metaphors for Mathematics from Pasch to Hilbert," p. 309) in order to determine which features of the simile are meant to be applied to the subject at hand, when he comes to discussing the import of Hilbert's architectural metaphors (on p. 319), he claims without any discussion of their use or consideration of alternatives that the particular feature of buildings Hilbert meant to apply to mathematics was their artificiality. One therefore has the impression Schlimm's aim consists not primarily in exploring the meaning of the metaphor as it presents itself, but in finding an anchor-point in Hilbert's writings for a pre-conceived notion of "a dramatic change in the way mathematics as a whole was conceived [...] before and after [...] the 'modernist transformation' of mathematics." (Schlimm, "Metaphors for Mathematics from Pasch to Hilbert," p. 310).

> Every science begins when sufficiently many interconnected facts are available. Science, however, emerges only as a result of *ordering* this stuff. Order is established by means of the *axiomatic method*, that is to say, one constructs a *logical architectonic structure of concepts* such that the relations among the concepts also correspond to the relations among the facts to be ordered. In setting up such an architectonic structure of concepts lies a certain arbitrariness (e.g. the place of the concept of continuity, whether at the end or right at the beginning of the axiom-system—yields entirely different approaches) [...].[clxxxix]

§ 105 By emphasising, as I have been, Hilbert's concern with systematisation, I do not mean to deny that Hilbert is also interested in stating axioms and investigating their peculiar properties, such as their independence, consistency and the sufficiency for deriving particular conclusions (i.e. their completeness; to be discussed in section 3.2.3.1).

I mean to stress, however, that foundational work as Hilbert conceives of it is not concerned *only*, or even *mainly* with the construction of any particular axiom-system and the investigation of what has since come to be known as their meta-theoretical properties. Rather, the exploration of the, to indulge Hilbert's architectural choice of metaphor: «load-bearing» deductive connections between various propositions within a given discipline – to figure out how much one needs to assume in order to obtain a certain result, or how little suffices[135] – and the clarification of these connections by means of the construction of a rigorously presented deduction (whether proceeding from axioms or propositions which are, themselves, derived, rather than basic) is as much and as big a part of foundational work as axiomatisation in particular cases. In fact, it seems to me at this early stage in his career, Hilbert regarded the systematic development of geometry by constructing rigorous proofs – which is what, I take it, his programmatic concern with ordering facts amounts to in practice – as main task of his foundational work.

I belabour the distinction I mean to draw between two kinds of foundational work at such length to combat the temptation emanating from the phrase "foundational studies": It is, I think, very tempting to construe foundational work as mainly concerned with something like a «base-layer» of knowledge, and, in Hilbert's particular case, with axioms and their meta-theoretical properties. This temptation ought, I believe, to be resisted as unfaithful to Hilbert's programmatic remarks – though he, and certain (later) remarks of his, may well be part of why this narrower conception of foundational work conceived as concerned exclusively with the primitives holds such sway – and certainly to Hilbert's practice of foundational research in the early stages of his career (but also later on).

To keep in mind this internal articulation of foundational work into a concern with *deductive connections at all levels of a discipline* on one side and a concern with primitives on another side is crucial, in my view, to recognise and appreciate how the focus of Hilbert's work shifted and narrowed over the years and to be able to appreciate the intended import of programmatic methodological and philosophical remarks in Hilbert's later work in the 1920s.

[135] A striking case in point, to my mind, is Hilbert's preoccupation (in chapter III and IV of the *Grundlagen der Geometrie*) with the development of proportion- and measure-theory independently of the Archimedean Axiom. Indeed, if the choice Hilbert makes later about which results from his earlier work to present in the context of subsequent foundational lectures is any indication, this investigation seems to have been a major point of pride for Hilbert, because it is *this portion* of his work he presents in the sections of his foundational lectures in 1913 and 1914 dedicated to the foundations of geometry, rather than, say, his discussion of the independence of the Parallel Axiom; in the third of his notebooks (which I will discuss at length in section 3.3), Hilbert later wrote that "all my geometries and the results following from them lead ultimately to non-Archimedean groups"[cxc].

3.2 Hilbert's lecture courses between 1904-1915

§ 106 The «long decade» between the end of Hilbert's last lecture on the foundations of geometry in 1902 and the beginning of his first lecture focused centrally on mathematical logic in 1917 were a productive time for Hilbert, but his activity during these years concentrated almost entirely on areas other than foundational research: He turned his attention, first (for the rest of the 1900s), to analysis, differential equations and, then (in the 1910s), to mathematical physics before seemingly turning it, in 1917, back to mathematical logic[136].

Hilbert's preoccupation with other areas between 1902 and 1917 explains the paucity of his publication record on foundational issues between 1902 and 1917 to some extent, but not entirely: For instance, Hilbert's work, as part of his concern with mathematical physics, on the foundations of *physics* also falls into this time. However, although Hilbert's work on physics was carried out in the 1910s, it appeared only in the form of preliminary notes at the time and was published in the form of a «full-scale» article only in 1924[137]. Nonetheless, Hilbert completed and published a sizeable body of work – nearly all his contributions to analysis collected in the third volume of Hilbert's collected works stem from this time – and supervised more than 30 doctoral students working on algebra, analysis as well as mathematical physics[138], but next to none concerned with foundational topics[139].

Most significant for retracing the path along which Hilbert's views on the foundations of mathematics, and what I have been calling his "philosophical position" with them, evolved during that time are the notes recording lecture courses and seminars Hilbert held as part of his regular teaching duties and dedicated, under different guises, to foundational reflections[140]. This section will discuss the development of Hilbert's lecture courses on foundational issues up until the winter-term 1917/18, which in several respects marks a fresh start[141].

My aim in this section is threefold:

[136] Peckhaus points out that during this time Hilbert also engaged in university- and wider educational politics with the apparent aim of furthering foundational research as he envisioned it (see: Volker Peckhaus. *Hilbertprogramm und Kritische Philosophie. Das Göttinger Modell interdisziplinärer Zusammenarbeit zwischen Mathematik und Philosophie*. Vandenhoeck & Rupprecht, 1990, pp. 6ff. (sec. 1.2.2, item (2))). The prime examples for this strand of Hilbert's activity, which Peckhaus invokes as illustration of his claim about Hilbert's political agenda, are Hilbert's efforts to secure, first, some form of remuneration and later a permanent position for Zermelo (see Peckhaus' chapter 4, especially sec. 4.3). Peckhaus also discusses briefly Hilbert's efforts in support of Husserl (in sec. 6.2.1), and, at length, Hilbert's later (ongoing and only partially successful) efforts on behalf of Leonard Nelson (chapter 6). Peckhaus also offers a veritable treasure-trove of quotations from unpublished administrative documents and correspondence.

[137] See chapter 1, including the editor's introduction: David Hilbert. *David Hilbert's Lectures on the Foundations of Physics 1915-1927. Relativity, Quantum Theory and Epistemology*. Ed. by Tilman Sauer and Ulrich Majer. Vol. 5. David Hilbert's Lectures on the Foundations of Mathematics and Physics, 1891-1933. Springer, 2009.

[138] See the list of names and topics in David Hilbert. *Gesammelte Abhandlungen. Analysis, Grundlagen der Mathematik, Physik, Verschiedenes, nebst einer Lebensgeschichte*. Vol. 3. Springer Verlag, 1935, pp. 321 sqq.

[139] A seeming exception is Kurt Grelling's dissertation (completed in 1910), which was entitled (see: Hilbert, *Gesammelte Abhandlungen*, p. 433) "Die Axiome der Arithmetik mit besonderer Berücksichtigung der Beziehungen zur Mengenlehre". However, this dissertation was instigated by Zermelo and supervised by Hilbert only because Zermelo was not in a position (institutionally) to supervise it himself[cxci].

Peckhaus, *Hilbertprogramm und Kritische Philosophie*, pp. 143–149 presents a relatively extensive account of Grelling's life, as well as his grim fate: Kurt Grelling was in all likelihood killed in Auschwitz on September 18 1942 (see p. 147).

[140] The importance of these lecture courses as sources for understanding Hilbert's intellectual development was stressed as early as 1987 by Abrusci, who saw in them "a very useful source for the history of Hilbert's foundational researches [providing] far more details than the published papers" (Vito Michele Abrusci. "David Hilbert's *Vorlesungen* on Logic and Foundations of Mathematics." In: *Atti del Convegno Internazionale di Storia della Logica. Le teorie delle modalità*. Bologna: CLUEB, pp. 333–338, p. 335).

[141] Gregory H. Moore. "The Emergence of First-Order Logic." In: *History and Philosophy of Modern Mathematics*. 1988, pp. 95–135, p. 113 expressed a similar view: "Hilbert did not abandon foundational questions after his 1904 lecture at the Heidelberg congress, though he published nothing further on them for more than a decade. Rather, he gave lecture courses at Göttingen on such questions repeatedly—in 1905, 1908, 1910, and 1913. It was the course given in the winter semester of 1917/18, 'Principles of Mathematics and Logic,' that first exhibited his mature conception of logic.".

first, to provide an overview of the sources for Hilbert's interests and activities concerning foundational issues between his work on geometry and his foundational work in the 1920s;

second, to survey those various sources in some detail in order to

finally (and third), isolate the main events, ideas and concerns of Hilbert's work between 1902 and 1917.

More or less extensive notes or manuscripts survive documenting six lecture courses and one seminar[142] Hilbert taught between 1902, when Hilbert last lectured on the foundations of geometry, and 1917 which were devoted – some in substance but not title, some explicitly – to foundational topics[143].

In chronological order, these are: § 107

1. A lecture course entitled "*Zahlbegriff und Quadratur des Kreises*" in the summer-term 1904,

   of which official lecture notes were deposited in the library of the Mathematischen Institute in Göttingen, and, like all unpublished materials relating to Hilbert, are now in the hands of the department for manuscripts and rare prints (Abteilung für Handschriften und seltene Drucke) of the State and University Library of Lower Saxony at Göttingen (Niedersächsische Staats und Landesbibliothek (SUB) Göttingen).

2. A lecture course entitled "*Logische Prinzipien des mathematischen Denkens*" in the summer-term 1905, of which official lecture notes – produced by Ernst Hellinger – were deposited in the library of the Mathematischen Institut in Göttingen. In addition, at least three sets of lecture notes taken privately by students auditing the course are known to exist: (i) by Max Born (now held also at the SUB Göttingen under the signature Cod. Ms. D. Hilbert 558a), (ii) by one Otto Birck (now held by the ULB Bonn, Abteilung Handschriften und Rara, under the signature S 2990:3) and (iii) by Leonard Nelson (now held by the Archiv der Sozialen Demokratie of the Friedrich-Ebert-Stiftung in Bad Godesberg (Bonn) under the signature 1/LNAA000005)[144].

3. A lecture course entitled "*Prinzipien der Mathematik*" in the summer-term 1908,

   of which official lecture notes were deposited in the library of the Mathematischen Institut in Göttingen.

4. A seminar devoted to "Prinzipien der Mathematik" in the winter-term 1908/9,

[142] There may have been other seminars, but only from one such seminar dedicated to foundational topics, held in the winter-term 1908/9, records of the content survive among Hilbert's papers held by the Niedersächsische Staats- und Universitätsbibliothek (SUB) in Göttingen. Some other notes from Hilbert's seminars are held under the overarching signature Cod. Ms. D. Hilbert 570, but all other seminars so recorded are ostensibly devoted to purely mathematical topics. Ewald, "Hilbert's Wide Program," p. 248 mentions "a course on 'mathematical principles' with Noether [in 1917/18], of which no protocol exists".

[143] A full list of Hilbert's lecture courses is included in backmatter of each volume of the edition of *David Hilbert's Lectures on the Foundations of Mathematics and Physics* (edited by William Ewald, Michael Hallett, Ulrich Majer and Wilfried Sieg); the most recent (relative to earlier volumes updated) list is found in volume 3 of the series (*David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*), published in 2013; *not* included in this list are seminars (rather than lecture courses). The SUB Göttingen provides a comprehensive list of Hilbert's papers (here: https://www.sub.uni-goettingen.de/sammlungen-historische-bestaende/handschriften-und-nachlaesse/nachlassverzeichnisse/), including the few sets of notes from Hilbert's seminar in its possession.

[144] This manuscript is not included in the most recent list of lecture notes included in the edition of Hilbert's lectures on the foundations of mathematics and physics; I learned of its existence from Peckhaus, *Hilbertprogramm und Kritische Philosophie*.

of whose opening lecture, outlining topics for presentations to be delivered by students, notes exist in the manuscript MS Cod. D. Hilbert 570/2 held by the SUB Göttingen. The file held under this signature by the SUB Göttingen contains two copies of the text in two separate notebooks, one seemingly Hilbert's «working copy», written by Richard Courant and later revised and supplemented with additions by Hilbert himself, and another, written by Ernst Zermelo, showing no subsequent corrections or additions by Hilbert. The notebooks have been labelled in pencil by a librarian as "Hilbert 570/2:a" (Courant) and "Hilbert 570/2:b" (Zermelo).

I believe the version of the text written by Zermelo was *copied from Courant's version* some time *after* Courant wrote his notes but before Hilbert used his copy actively, and that the version written by Zermelo *is not* an independent set of notes taken by Zermelo on the occasion of Hilbert's presentation.

This hypothesis is supported mainly by the fact that Courant's notes contain one substantial correction – instead of attributing a result to Hermite, it is attributed to Serret – in Courant's own hand, where the corresponding passage in Zermelo's version appears already in its corrected form (i.e. the result is there attributed to Serret and that attribution is not a correction of a prior attribution to Hermite)[cxcii]. I take this change in Courant's notes records a correction Hilbert made to his own oral presentation in class, because if Hilbert had made the change subsequently the correction would be in Hilbert's hand (as other corrections to the manuscript are), not Courant's; (even if Courant changed the name on his own authority, without being prompted by Hilbert, the initial attribution to Hermite can, it seems to me, only be explained as recording what Hilbert in fact said in class). The fact that Zermelo's notes do not contain such a correction therefore suggests that Zermelo was not present at the presentation and contemporaneously taking notes himself; (if the correction is due to Courant, the absence of any correction in Zermelo's version is an even stronger reason to believe that Zermelo did not take his notes at the class, but copied Courant's after the fact).

Other changes in Courant's version by Courant himself are likewise incorporated in Zermelo's original text (without any indication that they correct a prior formulation), as is one apparently subsequent addition in Hilbert's hand (but none of the further additions by Hilbert present in the version written by Courant)[cxciii].

The reason why this second copy was produced – whether Zermelo wanted it for himself, or whether he produced it upon Hilbert's request (say, in order to make a copy available to students while allowing Hilbert to keep his own, where he had noted in the margins who had taken on the task of preparing a presentation on each topic) – and why both seem to have remained in Hilbert's possession, is unclear.

5. A lecture course entitled "*Elemente und Prinzipienfragen der Mathematik*" in the summer-term 1910,

   of which official lecture notes were deposited in the library of the Mathematische Institut in Göttingen;

6. A lecture course entitled "*Elemente und Prinzipien der Mathematik*" in the summer-term 1913,

of which partial lecture notes prepared by Bernhard Baule[145] as well as preparatory notes in Hilbert's own hand exist in a convolute of manuscripts held by the SUB Göttingen as Ms. Cod. D. Hilbert 559[146], as well as partial notes taken privately by a student held also by the university library in Göttingen as Cod. Ms. E. Hecke Df 10 (the notes were not *taken by* Hecke, they just somehow ended up in his possession[147]);

7. A lecture course entitled "*Probleme und Prinzipienfragen der Mathematik*" in the winter-term 1914/15,

   of which preparatory notes in Hilbert's own hand exist in the convolute Ms Cod. D. Hilbert 559, as well as a complete set of lecture notes taken privately by a student (who is not identified in the notes) now held by the library of the Institute of Advanced Study in Princeton[148].

§ 108 With respect to their content, organisation and internal references, these lecture courses and seminar can be tied together into three «bundles» in the following way:

A The 1905 lecture occupies a somewhat isolated position. It exhibits little to no continuity with the other lectures and seminars Hilbert delivered between 1902-1910, but then becomes an important source for Hilbert's 1913 and 1914/15 lecture courses, which, however, are not simply a reiteration of the 1905 lecture: In the lecture courses Hilbert held in 1913 and 1914/15 Hilbert apparently aims to *integrate portions* from the 1905 lecture course as well as from the 1908 and 1910 lecture courses into some new sort of overarching design.

B The lecture courses of 1904 and those of 1908 and 1910 are closely related both with regard to the topics Hilbert covers in them – all three of these lecture courses focus mainly on issues connected to the traditional problem of «squaring» the circle, i.e. constructing by elementary geometric means a measure of the area of a circle of given radius –, as well as the mode of exposition and arrangement of those topics.

   The continuity between these three lectures (and two earlier lecture courses with similar titles from the 1890s: "*Das Problem der Quadratur des Kreises*" and "*Über den Begriff der Irrationalzahl und das Problem*

[145] Abrusci mistakenly attributes them to somebody named "Bauer" (see item 5. in the list on: Abrusci, "David Hilbert's *Vorlesungen* on Logic and Foundations of Mathematics," p. 334); since Baule identifies himself as the writer in the notes themselves, this can really only be due to Abrusci misreading Baule's name.

[146] A full edition of this convolute can be found in appendix B, where it appears with kind permission of the SUB Göttingen.

[147] The catalogue-record of the SUB Göttingen ascribes the notes to somebody by the name of "Heinrich Rickert". Bärbel Mund of the SUB Göttingen informed me in a letter on 27/08/2021 that according to information available to her this Heinrich Rickert was, in fact, the son of the Neo-Kantian philosopher Heinrich Rickert: He was born in 1890 and died in 1917 (for what little information is available about Heinrich Rickert jun., see: `http://d-nb.info/gnd/112317671X`). He had been studying in Göttingen since the summer semester 1912. How these lecture notes fell into Hecke's hands is, according to Bärbel Mund, not known to the SUB.

This manuscript is not included in the most recent list of lecture notes included in the edition of Hilbert's lectures on the foundations of mathematics and physics; I came across it looking through the manuscripts associated with Hilbert by the Kalliope-search engine (`https://kalliope-verbund.info/`; checked 11/5/2022) for personal papers and autographs held by libraries in Germany.

[148] That this lecture is not mentioned by Moore is presumably due to the fact that Moore's list (see endnote 12 of: Moore, "The Emergence of First-Order Logic," p. 130) derives only from a knowledge of the lecture notes held by the Mathematische Institut in Göttingen or found among Hilbert's papers. (This is suggested by the fact that Moore refers (in his footnote 12 at the end of the passage) to the 1913 lecture by the title of the notebook that constitutes MS Cod. D. Hilbert 559 (as "Einige Abschnitte aus der Vorlesung über die Grundlagen der Mathematik und Physik"), rather than by the official title of the lecture course ("Elemente und Prinzipienfragen der Mathematik").) Since no notes of the 1914/15 lecture course survive *there* (and the fact that Ms. Cod. D. Hilbert 559 also contains a few pages of notes for that lecture is easily missed), Moore overlooked this lecture course, of whose existence his primary source of evidence contained only the briefest indication (namely a small packet of notes inlaid in the back of the notebook held under the signature Ms. Cod. D. Hilbert 559 by the SUB Göttingen). This lecture *is*, however, mentioned by Abrusci (Abrusci, "David Hilbert's *Vorlesungen* on Logic and Foundations of Mathematics," p. 334), presumably on the basis of this packet of sheets.

der Quadratur des Kreises" delivered in 1894/95 and 1897/98 respectively, of which Hilbert's preparatory notes survive and are held by the university library in Göttingen under the signatures Cod. Ms. D. Hilbert 542 (1894/95) and 549 (1897/98)) is obscured to some extent by the fact that Hilbert chose an altogether different title for his 1910 lecture, which no longer mentioned the quadrature of the circle, but was rather called "*Elemente und Prinzipienfragen der Mathematik*", and so seems to have more in common with the 1905 lecture, whose title also mentioned "Prinzipien". However, the 1910 lecture is almost identical in scope and disposition, to the 1908 lecture. Indeed, Hilbert had already written on the first page of the 1908 lecture notes: "Bester Titel: Zahlbegriff und Principienfragen in der Mathematik"[149].

In front of the word "Zahlbegriff" Hilbert has written and crossed out the letters "Ir", which presumably were the first letters of "Irrationalzahlen", recalling the title of the 1897/98 lecture course. Hilbert (presumably later) crossed out "Bester" wrote underneath in blue crayon once more: "Bester Titel:" followed (in pencil) by "Principe [sic] u. Fundamente d. Mathematik" and (still later, in slightly but noticeably different handwriting): "oder Probleme und Principienfragen der Mathematik".

These suggested title-changes are impossible to date exactly, but it seems plausible to me to think that at least the first suggested title-change dates back to the time when Hilbert initially read through the notes while they were being compiled or after they were finished; if this assumption is accurate, these pencilled in notes show that Hilbert thought about changing the title of his lecture course on squaring the circle already in 1908 – though they unfortunately give no indication as to why Hilbert thought this change was for the best –, making the eventual change in 1910 seem less abrupt. It seems, in any case, likely that Hilbert came up with at least some of the title-changes before 1910, since he did then deliver substantially the same lecture course under a new title, and would, subsequently, have had little reason to return and revise the 1908 lecture-notes because he had had a complete set of new lecture notes produced for the 1910 course, to which he later refers back to in the preparatory notes for his 1913 and 1914/15 lecture courses..

The 1908/9 seminar also belongs, albeit more loosely, to this bundle. It is not focused centrally on the issue of squaring the circle but its overall ambition and organisation – its focus on principles of mathematics in a wide sense – is in line with that of the lecture courses from 1908 and 1910 (cf. the discussion in § 136).

C The lecture courses of 1913 and 1914/15 are intricately connected. Indeed, the 1914/15 lecture course appears to have been a reworking of the expository ark Hilbert set out to trace in the 1913 lecture course, which Hilbert seems to have been ultimately dissatisfied with (if his revisions to the official 1913 notes are any indication) and so chose to revisit and rework the following year on the basis of his notes for the 1913 lecture course, as well as all kinds of earlier lecture courses, not least of all those delivered in 1910 and 1905.

[149] David Hilbert. "Prinzipien der Mathematik. S[ommer-]S[emester] 1908." (16205y). MI Göttingen, 1908, 1r (though the lecture-notes are paginated by the writer, this page is not, the pagination of the lecture-notes starts on 3r with number "3", counting the (unpaginated) title page (2r) as page 1 and the (empty) 2v as page 2).

§ 109 As I said, Hilbert's publication record on foundational issues in the period between the publication of the *Grundlagen der Geometrie* and the year 1917, when Hilbert turns his attention first to logic and subsequently to foundational issues, so to speak, «full time» is thin. These lecture courses leave a similar first impression: With the exception of the 1905 lecture course, all of them are re-iterations of prior courses and contain, at least upon first sight, no substantive technical innovations of relevance to foundational studies as usually conceived. However, first sight is deceptive in this case and, as I hope the following discussion (especially sections 3.2.2 and 3.4.2) will show, Hilbert had not simply entered a phase of, as it were, «intellectual hibernation» between 1905 and 1917.

In fact, these lecture courses warrant detailed and careful consideration because, upon closer inspection and reflection, they attest in various ways and modes the intensity, focus and evolutions of Hilbert's concern with foundational issues during these years. In the remainder of this section I will discuss, first (in section 3.2.1), Hilbert's work on logic before 1917, focusing, in particular, on the influence of Schröder's work on Hilbert's early logical theory; then I will turn (in section 3.2.2) to what seems to be Hilbert's only sustained attempt to carry out new foundational work, namely, his work towards an axiom system for the arithmetic of natural numbers around 1913 (which, once again, I think shows recognisably the influence of Schröder's work). Finally, I will discuss (in section 3.2.3) in general terms the overall conception of foundational work which seems to underlie these lecture courses, and (in section 3.2.3.1), in particular, what they reveal about Hilbert's views of the status of precisely defined axiomatic theories in relation to the pre-existing mathematics systematised by these axiomatic theories and especially about Hilbert's ideas concerning the question of their completeness.

3.2.1 Hilbert's Early Logical Theory

§ 110 Hilbert's conception of logic, such as it is before 1917, is more or less entirely manifest already in his 1905 lecture course[150]. Indeed, Hilbert did not cover logic at all in the lecture courses Hilbert delivered in 1904, 1908 and 1910. The lecture courses from 1913 and 1914/15 did cover logic, but of the 1913 lectures only Hilbert's own, exceedingly sketchy notes survive (see appendix B, page 290), while the discussion recorded in the set of notes from the 1914/15 lecture held by the Institute of Advanced Study in Princeton (on pp. 208-218 therein) is substantively the same as that found in the 1905 lecture-notes; that is to say, Hilbert states all the same axioms and derives all the same theorems in his 1915 lectures as he did in 1905. Indeed, Hilbert's private preparatory notes for the portion of this lecture course devoted to logic, which have also been preserved (see appendix B, page 296), consist mostly of references to the relevant sections of the official lecture-notes produced by Hellinger of the 1905 lecture course, indicating that Hilbert in all likelihood reused them without substantive changes[cxciv].

[150] Peckhaus has devoted two articles to Hilbert's logical work before the 1917-18 lecture: Volker Peckhaus. "Hilberts Logik. Von der Axiomatik zur Beweistheorie." In: *Internationale Zeitschrift für Geschichte und Ethik der Naturwissenschaften, Technik und Medizin* 3 (1995), pp. 65–86 and Volker Peckhaus. "Logic in Transition. The Logical Calculi of Hilbert (1905) and Zermelo (1908)." In: *Logic and Philosophy of Science in Uppsala*. Ed. by Dag Prawitz and Dag Westerståhl. 1994 (the latter is largely an English condensation of section 3.3. and 4.3 of Peckhaus, *Hilbertprogramm und Kritische Philosophie*). Both of these articles discuss mainly the 1905 lecture.

§ 111 Compared to Hilbert's later treatment of logic starting with his 1917/18 lecture course, Hilbert's early logical theory simply reproduces the basic ideas and results of the so called "algebra of logic": As was standard practice in that tradition, Hilbert uses bits of notation familiar from the algebra of the natural numbers – such as '+' and '·'[cxcv], as well as '1' and '0'[cxcvi] – for the logical operations he deals with, and the axioms Hilbert presents, which he also refers to as "Rechenregeln"[cxcvii], are modelled on the axioms for arithmetic.

For chronological, theoretical & notational and, not least of all, linguistic reasons[151], the works of Ernst Schröder, in principle, seem like the most obvious model Hilbert could have followed. Among Schröder's works, his *opus magnum*, the *Vorlesungen über die Algebra der Logik* are the most prominent: The three volumes of Schröder's *Vorlesungen*, which contain an exhaustive presentation of the state of logical theory at the end of the 19th century, were published in 1890, 1891 and 1895 before Schröder left the work unfinished upon his death in 1902 (though one subsequent volume was edited from his manuscripts and posthumously published by Karl Eugen Müller in 1905[152]). In fact, as Schröder's correspondence with Peirce[153] attests, Schröder initially planned for it to be in two volumes, but was swayed by a renewed exchange with Peirce, who sent to Schröder some of his more recent work on the logic of relations to "cut out of my vol. 2, part II the contingent chapter of the relative logic on account of the size they swelled to, and am greatly occupied with framing them into a third volume"[154].

It may be tempting nowadays, in the wake of the «rehabilitation» and «belated recognition» of Frege as a philosopher and logician of pivotal importance to think of Frege as a possible influence on Hilbert's 1905 lecture course[155]. However, so far as I can see, Hilbert's writings – published and unpublished – give one no reason to think Hilbert ever studied Frege's (technical) work in any detail, or that Hilbert's brief personal interaction with Frege exacted any formative influence on Hilbert's subsequent work or thought about foundational issues or logic (see footnote 234). The most plausible sign of Frege's influence on Hilbert I am aware of is a reference to the *Grundgesetze der Arithmetik*, which suggests that Hilbert read Frege's polemics against formalism (see page 94).

[151] Unlike most of the works belonging to the tradition of the algebra of logic, Schröder's was in German. There are some reasons to think Hilbert preferred reading research-literature in German: Toepell remarks that Hilbert "hat damals [in the 1890s] nicht gern italienisch Geschriebenes gelesen" and that Hilbert knew "italienische Arbeiten insofern sie übersetzt waren" (see: Toepell, *Über die Entstehung von David Hilberts "Grundlagen der Geometrie"*, p. 56). Toepell cites a letter to Hurwitz in support, where Hilbert complains about being sent "viel italienisches [Zeug]" to read. Indeed, there is some reason to believe (see the discussion in footnote 186) that Hilbert may have first read Peano's *Formulaire* in German translation. However, it seems to me the remarks Toepell points to may also be interpreted as a testament of *substantive* discomfort – by interpreting "italienisch" not as "written in Italian" but as "written by contemporary Italian mathematicians" –, rather than linguistic unease. Since Hilbert spent time in Paris, he presumably had a solid command of French, and so may also have read Peano's *Formulaire* in the original. How good a command of English Hilbert had, enabling him to read Russell, Whitehead, Boole or others in the original, I see no solid basis for estimating. (In any event, I believe (see footnote 335) Hilbert first read (about) Russell in French, following up on a reference by Poincaré.)

[152] For an account of Müller's editorial work, and of the subsequent destruction of Schröder's manuscripts, most of which, like Frege's unpublished writings, burned during the bombing of Münster in the Second World War, see: Volker Peckhaus. "Karl Eugen Müller (1865–1932) und seine Rolle in der Entwicklung der Algebra der Logik." In: *History and Philosophy of Logic* 9.1 (1988), pp. 43–56.

[153] Only a part of which, most of them letters from Schröder to Peirce, has been preserved, due to the destruction of Schröder's papers (and Peirce's letters to Schröder along with them) and published by Nathan Houser. "The Schröder-Peirce Correspondence." In: *Modern Logic* 1.2-3 (1990-1991), pp. 206–236.

[154] See the letter dated April 12th 1893 in the Peirce-Schröder correspondence: Houser, "The Schröder-Peirce Correspondence," p. 217.

[155] Peckhaus, for example, maintains that, in particular, Hilbert's correspondence with Frege was "[w]ichtig für Hilberts Einsicht in die philosophischen Implikationen des Axiomatisierungsprogramms" (Peckhaus, *Hilbertprogramm und Kritische Philosophie*, p. 40) and that Hilbert's closing statement that he shall certainly think more about the issues Frege raised was "allerdings mehr als nur eine Höflichkeitsfloskel" (Peckhaus, *Hilbertprogramm und Kritische Philosophie*, p. 46), but points to no examples.

Indeed, the observation that Hilbert's early logical system takes its cues from Schröder's work is common currency among scholars of Hilbert's logic[156]. These observations tend, however, to remain at the level of generalities, that is to say, they never mention any *specific* points of resemblance between Hilbert's early logical system and Schröder's work, nor do they ever discuss which of Schröder's writings Hilbert knew, or became acquainted with, at what point in time[157]. I intend over the following pages to supply these missing specifics, which are worth bringing into focus because Hilbert's engagement with Schröder's work, so far as Hilbert's work bears witness of it, did not proceed down the most obvious path, that is to say, not through Schröder's *Vorlesungen*.

§ 112

Certain striking differences in exposition and detail make it seem unlikely that Hilbert's logical calculus was to any substantive extent inspired by Schröder's *Vorlesungen*: The axioms Hilbert states, for instance, are entirely different from the "Postulates" one finds in Schröder's *Vorlesungen*: Hilbert lists as axioms the associative and commutative laws for '$+$' and '$\cdot$', the distributive law of '$\cdot$' over '$+$', the law of excluded middle and non-contradiction, the conjunctive idempotency of the false (i.e. '$1+1 \equiv 1$', in more recent notation: '$(\bot \wedge \bot) \leftrightarrow \bot$') and the disjunctive unit-law of the false (i.e. '$1 \cdot X \equiv X$', in more recent notation: '$(\bot \vee \phi) \leftrightarrow \phi$'). All of these appear as propositions – with proofs – in the first volume of Schröder's *Vorlesungen* (all of the following pages references are to the first volume): the associative laws are Schröder's Theorem 13 (p. 256) the commutative laws are Schröder's Theorem 12 (p. 255), the distributive law remains implicit as a consequence of Schröder's Theorem 28_+ (p. 311)[158] the laws of excluded middle and non-contradiction are Schröder's Theorem 30 (p. 302), the unit law of the false is Schröder's Theorem 21_+ (p. 271); on the other hand, the principles Schröder sets down as axioms are more akin to the rules of Gentzen's sequent calculus than to Hilbert's logical system: Schröder's Posulate 1, for example, parallels Gentzen's axiomatic sequents: $\phi \Rightarrow \phi$, the (effective) axioms for the binary operations (p. 196) are akin to Gentzen's operational rules: $\chi \Rightarrow \phi, \chi \Rightarrow \psi / \chi \Rightarrow \phi \wedge \psi$ and $\phi \Rightarrow \chi, \psi \Rightarrow \chi / \phi \vee \psi \Rightarrow \chi$.

Indeed, Schröder did not use the word "Axiom" at all for the basic principles of his calculus as presented in his *Vorlesungen*. Instead he spoke of "postulates" and "definitions". By contrast, in his earlier *Operationskreis des Logikkalkuls*, Schröder did speak of axioms[159]. There is some evidence that Schröder changed his mind about

[156] See the remarks quoted below in footnotes 165 and 157, I want to quote one further example from Paolo Mancosu, Richard Zach, and Calixto Badesa. "The Development of Mathematical Logic from Russell to Tarski, 1900-1935." In: *The Adventure of Reason. Interplay between Philosophy of Mathematics and Mathematical Logic 1900-1940*. Oxford University Press, 2010, pp. 5–119, p. 52: "The presentation [of logic in 1905] is influenced mainly by Schröder's algebraic approach.". Michael Hallett. "Hilbert and Logic." In: *Québec Studies in the Philosophy of Science. Part I: Logic, Mathematics, Physics and the History of Science*. Ed. by Mathieu Marion and Robert S. Cohen. Essays in Honor of Hugues Leblanc. Kluwer, 1995, pp. 135–188, p. 155 goes so far as to definitively assert – without evidence, and, I think, unjustifiably – that Hilbert's "calculus of propositions [...] owes much to Schröder['s *Vorlesungen*]".

[157] The editors of Hilbert's 1917/18 lecture course express a similar view of the matter in their introduction: "On the one hand the intellectual influences point to the past (e.g., to Schröder's logical work and the impact of Whitehead and Russell's *Principia*), and, on the other hand, to the future (...). [...]. A great deal remains obscure about the precise intellectual influences on the Hilbert school from these other logical traditions, despite the fact that real progress has been made since the early 1990s." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 48).

[158] Schröder's Theorem 28_+ states a more general claim, namely: '$(a+c)(a+d)(b+c)(b+d) = ab + cd$'. If one takes the special case where $a = b$, one obtains: '$(a+c)(a+d)(a+c)(a+d) = aa + cd$'. Using the commutative law for multiplication, one obtains: '$(a+c)(a+c)(a+d)(a+d) = aa + cd$'. Finally, using the idempotency law for multiplication (Schröder's Theorem $14_\times$ (p. 259)), one obtains the distributive law for '$+$' over '$\times$', '$(a+c)(a+d) = a + cd$'. I take it that Schröder has in mind something along these lines when he remarks in the discussion following the statement of Theorem 28 that it includes the distributive law "as a special case when [...] one of the polynomials to be multiplied is thought of as a monomial or reduces to one"[cxcviii].

[159] As Peckhaus documents in Volker Peckhaus. "The axiomatic method and Ernst Schröder's algebraic approach to logic." In:

the place of axioms in logic as a result of his engagement with C. S. Peirce: In the conclusion of his "A Notation for the Logic of Relatives," Peirce writes that "axioms are mere substitutes for definition of the universal logical relations, and so far as these can be defined, all axioms may be dispensed with. The fundamental principles of formal logic are not properly axioms, but definitions and divisions"[160]. Given how impressed Schröder was with Peirce's work, it seems to me a plausible conjecture that Schröder may have been prompted by this remark to rethink his conception of axioms. Indeed, as the editorial notes to this article report[161], Peirce wrote into the margins of two of his offprints of the article an alternative system of fundamental propositions which includes (among others) exactly the «definitions» given by Schröder in the *Vorlesungen*. Schröder lists Peirce's "A Notation for the Logic of Relatives" in his bibliography (p. 710), along with most of Peirce's other pertinent publications. Peirce may have sent to Schröder offprints of his other (early) publications on logic – the correspondence accompanying them or acknowledging their receipt appears to have been lost – including one of "A Notation for the Logic of Relatives," perhaps even with the alternative primitive basis Peirce seems to have added by hand to more than one offprint he sent to colleagues. In his correspondence with Peirce, Schröder laments not receiving Peirce's "second memoir on the Algebra of logic"[162], which implies that Schröder *had* received a first from Peirce, probably Peirce's 1880 "On the Algebra of Logic," where the same «definitions» appear (and are called "definitions" in § 1 of chapter II[163]).

It is furthermore worth noting that Schröder's sole primitive '$\Subset$' is, conceptually and notationally, the exact analogue to the primitive of Peirce's 1870 and 1880 articles, '$-\!\!<$': both are conceived as combining equality and inclusion, and written by either, in Peirce's case, juxtaposing the equality-symbol (collapsed into a line, presumably to facilitate writing[cc]) and the inclusion-symbol, or, in Schröder's case, superimposing one upon the other.

To anybody comparing Schröder's *Vorlesungen* with Hilbert's early logical system, the complete absence of (any mention of) Schröder's main primitive, the subsumption-relation ('$\Subset$'), will also be conspicuous.

It seems to me the divergences are a solid reason to think that in 1905 Hilbert had no substantive acquaintance with any part of Schröder's *Vorlesungen über die Algebra der Logik*; or, if Hilbert had, all contrary evidence notwithstanding, read Schröder's *Vorlesungen* whole or in part already in 1905, they seem, in any event, not to have been «on Hilbert's mind» when he worked out his 1905 logical system, nor had any substantive influence it.

It can be regarded as certain that Hilbert was acquainted with Schröder's *Vorlesungen* by 1913 because Hilbert

Philosophia Scientiae 1.3 (1996), pp. 1–15, Schröder's views on the nature and status of axioms changed significantly over time. The following remarks about the influence of Peirce on Schröder are meant to supplement Peckhaus' account by pinpointing the probable reason for Schröder's change of mind about axioms. I want to also add, however, that I find the conception of what Schröder calls "formal axioms"[cxcix] contained in Schröder's 1877 *Operationskreis des Logikkalkuls* (and later seemingly abandoned) less confused and closer to Hilbert's own conception than Peckhaus's brief and dismissive presentation (in his section 3.2) makes it out to be.

[160]Charles Sanders Peirce. "Description of a Notation for the Logic of Relatives, resulting from an Amplification of the Conception of Boole's Calculus of Logic." In: *Writings of Charles S. Peirce: A chronologial edition. Volume 2: 1867-1871*. Ed. by Edward C. Moore. Indiana University Press, 1984, pp. 359–429, p. 429.

[161]See: Charles Sanders Peirce. *Writings of Charles S. Peirce: A chronologial edition. Volume 2: 1867-1871*. Ed. by Edward C. Moore. Indiana University Press, 1984, pp. 546 sq.

[162]See the letter dated February 1st 1890: Houser, "The Schröder-Peirce Correspondence," p. 211.

[163]See: Charles Sanders Peirce. "On the Algebra of Logic." In: *Writings of Charles S. Peirce: A chronologial edition. Volume 4: 1879-1884*. Ed. by Christian J. W. Kloesel. Indiana University Press, 1986, pp. 163–209, pp. 182 sq.

mentions the first volume in a brief list of literature included in the lecture notes for the 1913 lecture course[164]. There is no decisive evidence that Hilbert ever read any subsequent volume beyond the first. (On 154, I discuss the only piece of (faint) evidence which suggests that Hilbert may by 1917 have read at least part of the introduction to the second volume.)

§ 113 In light of these observations, one may be inclined to doubt the hypothesis that Hilbert was at all aware of Schröder's work in any detail. Indeed, there are solid reasons one can adduce in support of this suspicion.

The main reason to doubt that Schröder's work served as a significant source for Hilbert's technical work on logic are the various divergences in technical detail between Schröder's work and Hilbert's[165]. Most striking is Hilbert's «inversion» of '+' and '·': While Schröder uses '+' as the symbol for the disjunction, and '·' as his symbol for the conjunction, Hilbert uses '+' as his conjunction-symbol and '·' as the symbol for the disjunction; correlatively, Hilbert also inverts '0' and '1': using '0' where Schröder uses '1', and conversely.

This divergence persists into Hilbert's 1917/18 lecture course, Indeed, even in the *Grundzüge der theoretischen Logik*, published jointly with Ackermann (but based substantively on the 1917/18 lectures), '$X\&Y$' is still called "the logical sum" and '$X \vee Y$' "the logical product" (see the passage quoted in endnote ccxxiii); in Hilbert's 1921/22 lectures the formula '$A \vee B \vee C \vee \cdots \vee \overline{B} \vee \ldots$' is likewise still described as a "product" which "is zero [because] a factor is zero"[cci]. The first to comment on Hilbert's «inversion» of notation, relative to otherwise wide-spread usage, in print was probably Heinrich Behmann in his Habilitationsschrift[166].

I will discuss this notational peculiarity in depth on page 85.

A second cause of doubt surrounding Hilbert's potential source(s) lies in Hilbert's fabled habit of "nostrification", and the observation that one finds suggestive, but skeletal remarks about a logical calculus constructed in analogy with the algebra of the natural numbers in Hermann Schubert's article entitled "Grundlagen der Arithmetik", written for the *Enzyclopädie der mathematischen Wissenschaften* (overseen by Klein & Weber). Schubert's article is the first article of volume 1, published in 1898, and thus a text Hilbert was in all likelihood familiar with.

In footnote 11 of this article, Schubert sketches the fundamental idea of Schröder's logical calculus:

> Also in more general contexts beyond the arithmetical operations these three basic laws [the commutative, associative and distribute law] play a fundamental role, as in formal mathematics, the logical calculus ["Logikkalkül"] and the concept script ["Begriffsschrift"]. (...) In the logical calculus $a + b$ means everything which is either a or b, $a \cdot b$ everything which is at once a and b. For these operations the commutative and associative law holds. Furthermore the distribute law in both forms, whereby $(a + b) \cdot c = (a \cdot c) + (a \cdot c)$ and furthermore $(a \cdot b) + c = (a + c) \cdot (b + c)$. This was brought to attention by *E. Schröder* in his piece "Operationskreis des Logikkalkuls" (Leipzig 1877).[cii]

[164] See the manuscript edited in appendix B, page 271, line 194.
Only two other titles are listed along with Schröder's book there: Hilbert's own *Grundlagen der Geometrie* (in the, at the time, latest, the 4th edition, published in 1913) and Gerhard Hessenberg's "Grundbegriffe der Mengenlehre" (published in 1906).

[165] As Peckhaus succinctly puts it: "Hilbert lehnt sich in seiner Symbolik an die algebraisch-logischen Vorbilder, insbesondere an die Systeme von Boole und Schröder an, allerdings mit bemerkenswerten Änderungen." (Peckhaus, *Hilbertprogramm und Kritische Philosophie*, p. 63). On the basis of this remark one can formulate the doubt I am here entertaining as asking whether the relation between Hilbert's logical theory and that of Schröder anything more than a vague and insignificant affinity in notation.

[166] Published as: Heinrich Behmann. "Beiträge zur Algebra der Logik, insbesondere zum Entscheidungsproblem." In: *Mathematische Annalen* 86 (1922), pp. 163–229, p. 165.

Given that the laws mentioned by Schubert in this footnote are indeed among the axioms underlying the logical system Hilbert presents in his 1905 lecture course, one may regard it as at least in principle possible that Hilbert simply developed his logical system from scratch on the basis of this bare sketch.

Indeed, Hilbert does not cite any previous work on logic in his 1905 lecture course. Hilbert does, however, implicitly acknowledge *having some familiarity* with prior technical developments by making a derogatory remark about the "hitherto overly formal logical calculus"[cciii], but does so without naming any names[167].

§ 114 However, it seems to me the key to understanding the origins of Hilbert's early logical theory lies in another detail of Schubert's article, namely the reference to Schröder's 1877 *Operationskreis des Logikkalkuls*. Following up on this reference and comparing Hilbert's presentation of the logical calculus in his 1905 lecture with Schröder's 1877 booklet[168] – rather than with Schröder's later *Vorlesungen* – reveals a wide and substantive congruence:

- The axioms Hilbert states in his 1905 lecture appear also as axioms in Schröder's 1877 booklet.

  To be exact, the correspondences are as follows:

  – Hilbert's Axiom I is really a rule allowing for the substitution of equals in arbitrary formulas.

    Schröder does not state such a rule, but Schröder also starts by stating axioms for equality, namely reflexivity and transitivity in the form '$(a = c \wedge b = c) \rightarrow a = b$'[cciv].

    I want to note that after stating these axioms Schröder refers to his *Lehrbuch der Arithmetik* (1873) for a fuller discussion of equality and *there* one finds Schröder stating a general substitution-rule exactly like the one Hilbert states as his first «axiom»[ccv], and, indeed (and oddly) one finds Schröder there bemoaning the use of precisely the transitivity-axiom he himself sets down in his 1877 booklet[ccvi]).

  For the remaining axioms, the correspondence is, for the most part, apparent, if one keeps in mind Hilbert's change of notation (compared with Schröder's): Hilbert writes '+' where Schröder writes '·', and '1' where Schröder writes '0', and conversely. I will emphasise the need to take into account the change of notation in order to correctly identify the counterpart to one of Hilbert's axioms in Schröder's booklet by saying that certain axioms and theorems of Hilbert's 1905 system are "in substance", but, owing to the different notation: not "as formulas" the same as certain axioms or theorems of Schröder's *Operationskreis*.

[167] Peckhaus conjectures that "[Hilbert] seems to have in mind above all Ernst Schröder's algebra of logic" (Peckhaus, "Logic in Transition," p. 313) but also acknowledges that "[d]ieses Urteil ist sicher keine adäquate Charakterisierung des mathematisch-logischen Schaffens in der Zeit um die Jahrhundertwende", and, in particular, not of Schröder's work (see: Peckhaus, *Hilbertprogramm und Kritische Philosophie*, p. 62). In a contemporaneous article, Peckhaus goes further and suggests that, in fact, Schröder's overarching "dream" ultimately was "die Darstellung und Analyse der Mathematik mit den Mitteln der Logik" (Volker Peckhaus. "Ernst Schröder und die 'pasigraphischen Systeme' von Peano und Peirce." In: *Modern Logic* 1.2-3 [1991], pp. 174–205, p. 194).

[168] For an account of Schröder's life and work, see Volker Peckhaus. "Schröder's Logic." In: *Handbook of the History of Logic. The Rise of Modern Logic: from Leibniz to Frege*. Ed. by Dov M. Gabbay and John Woods. Vol. 3. Elsevier, pp. 557–609. The *Operationskreis*-booklet is discussed there on p. 563 and pp. 572f. Peckhaus suggests that Schröder developed the theory contained in the *Operationskreis* on the basis of a "quite rudimentary" knowledge of other logic theories, relying mainly on the works of Boole and Robert Graßmann (see: Peckhaus, "Schröder's Logic," p. 572).

  - Hilbert's second as well as his third axiom, which assert the existence in every case of products and sums respectively, are Schröder's axioms 1^0 and $1'$[ccvii];
  - Hilbert's axioms IV-VII (the commutative and associative laws for addition and multiplication) are Schröder's axioms 2^0, $2'$ (associative laws) and 3^0, $3'$ (commutative laws);
  - Hilbert's axiom VIII is (in substance) Schröder's theorem $6'$ (as a formula, it is Schröder's axiom 6^0);
  - Hilbert's axioms IX and X (the laws of non-contradiction and excluded middle, respectively) are Schröder's axioms 7^0 and $7'$; finally, Hilbert's axioms XI and XII are (in substance) respectively a special case of Schröder's theorem 8^0 and an equivalent form (by one commutation) of Schröder's theorem $9'$.

- Moreover, Schröder articulates all the theorems Hilbert goes on to prove; if one discounts omissions by Hilbert, the theorems appear in the same order in Schröder's 1877 booklet as they appear in Hilbert's 1905 lectures.

  Again, to be exact, the correspondences are as follows:

  - Hilbert's theorem (1) is (in substance) Schröder's axiom 5^0;
  - Hilbert's theorem (2) is (in substance) Schröder's theorem 8^0;
  - Hilbert's theorem (3) is (in substance) Schröder's axiom 9^0;
  - Hilbert's theorem (4) is (in substance) Schröder's axiom $5'$;
  - Hilbert's theorem (5) is (in substance) equivalent to Schröder's theorem $8'$;
  - Hilbert's theorem (6) is (in substance) Schröder's theorem 12;
  - Hilbert's theorem (7) is Schröder's theorem 13 and finally
  - Hilbert's theorem (8) is Schröder's theorem 18.

The substantive overlap in both content (i.e. axioms and propositions stated) and arrangement between Hilbert's 1905 exposition of the logical calculus and Schröder's 1877 booklet on the *Operationskreis des Logikkalkuls* leaves little doubt in my mind that *Hilbert developed his early logical systems on the basis of reading Schröder's early booklet from 1877*, the *Operationskreis des Logikkalkuls*, perhaps prompted to seek out *it*, rather than Schröder's *Vorlesungen*, by Schubert's reference[169]. I don't mean to claim that Hilbert simply *adopted* Schröder's theory. Rather, Hilbert *adapted* it, simplified it (mainly by supplying proofs for propositions Schröder had (redundantly) taken as axioms), and disregarded Schröder's theory of inverse logical operations altogether.

Indeed, Hilbert's use of the label "Logikkalkül" is itself evidence for this hypothesis since, as far as I can see, no other logician at the time used this label, or a label which could plausibly be translated this way, for their technical

[169] Hilbert may also have heard about Schröder from Cantor, who mentions the "recently developed logical calculus [Logikcalcül]" to Hilbert in a letter dated 27/1/1900[ccviii]; that Cantor was probably thinking mainly of Schröder's work is plausible in light of a remark found in a letter to Schönflies from around the same time (it is dated 26/6/1899), where Cantor remarks that, due to the existence of Bernstein's proof for the proposition that two sets which are each smaller than the other are equinumerous as an alternative to Schröder's earlier proof, the justification of that proposition "in no way depends on the logical calculus"[ccix]; since Hilbert had a copy of this letter, he would have known of Schröder as the, at least for Cantor: main exponent of the recently developed logical calculus.

work: Peano spoke of "la Logique mathématique", Frege of "Begriffsschrift", Robert Grassmann of "Formenlehre" and "Begriffslehre", Couturat (following Russell (?)) of "logistique", Peirce of "the algebra of logic",…. Indeed, even Schröder uses this label mainly in his 1877 booklet. In the *Vorlesungen über die Algebra der Logik* Schröder also appears to prefer a different label, namely, "identischer Kalkul"[170] – meaning a calculus both of whose binary operations were idempotent –, and, in the second volume for a particular application of the identitary calculus Schröder deems especially important: "Aussagenkalkul"[171].

§ 115 That Hilbert – at least in 1905 – nowhere credits or even so much as mentions Schröder's work on logic is all the more remarkable given how much Hilbert's work on logic, and on foundational issues also beyond logic, seems to owe to Schröder's early work, and even more in light of the fact that Hilbert mentions pioneering work by others freely and frequently throughout especially the 1905 lecture course[172].

The lecture-notes for the 1914/15 lecture course held by the Institute of Advanced Study in Princeton[173], *do* report Hilbert mentioning Schröder and referring his students for "a closer examination" to "Schröder's Handbook of the Logical Calculus"[ccxi]. No work of Schröder's bears this title, but the use of the label "Logical Calculus" ("Logikkalkül") makes it likely that Hilbert is misstating the title of Schröder's 1877 *Der Operationskreis des Logikkalkuls* – or that the writer of the Princeton-notes is writing it down incorrectly –, rather than that of Schröder's *Vorlesungen über die Algebra der Logik*, since, as I note above, Schröder himself later ceases to use the word "Logikkalkul" to describe his work. This reference is significant also for the understanding Hilbert's technical work from 1905 even if it comes from a text written almost a decade later, since Hilbert presented substantially the same logical system in the 1914/15 lecture course as he did in the 1905 lecture course; (cf. page 74): Perhaps Hilbert made, or meant to make a similar acknowledgement in 1905 – e.g. that such an acknowledgement was always part of Hilbert's his preparatory notes (which are not known to have survived); indeed, the acknowledgement in 1914/15 could be based on such a note, and the misstated title the result of Hilbert misremembering the expansion of a compressed reference years later –, but for one reason or another didn't get around to it; (it is possible but exceedingly unlikely that Hilbert made a reference to Schröder in 1905, which Hellinger failed to record in his notes, because none of the other notes taken privately by students mention Schröder's name).

§ 116 Perhaps the most interesting case of Hilbert adapting and redressing a result from Schröder's 1877 booklet is one, where the affinity between Schröder's and Hilbert's work is less apparent on the surface, but still recognisable

[170] See, e.g. in the first volume: Ernst Schröder. *Vorlesungen über die Algebra der Logik. Exakte Logik.* Vol. 1. Teubner, 1890, p. 160.
[171] Note that Hilbert prefers the spelling "Kalkül", while Schröder spells the word "Kalkul". I guess the former spelling derives from the pronunciation of the word "calcul" in French, while the latter derived from the Latin "calculus".
[172] The similarly striking omission of any mention of Wiener's work on geometry in Hilbert's *Grundlagen der Geometrie* is commented on by Rowe, "The Calm before the Storm," p. 65, who cites Weyl in support of the diagnosis that "generosity towards other investigators was not one of [Hilbert']s outstanding traits". Rowe offers a speculative explanation of this "uncharitable attitude", which, Rowe contends, "had nothing malicious about it; Hilbert was just not capable of nor interested in seeing things from anything other than his own point of view" (David E. Rowe. "The Philosophical Views of Klein and Hilbert." In: [1994]. Ed. by Sasaki Chikara, Sugiura Mitsuo, and Joseph W. Dauben. Proceedings of the 1990 Tokyo Symposium on the History of Mathematics, pp. 187–202, p. 192).
[173] As far as I am aware, this manuscript held at the IAS is the only full set of notes recording an exposition of logic by Hilbert after 1905 but before the 1917-18 lecture course). Schröder is also mentioned, along with Frege, Peano and Russell, as a contributor to "the formalisation of logical inference" in the *Grundlagen der Mathematik*[ccx].

when taking a closer look: The technical centrepiece of Hilbert's logical system and the fashion in which Hilbert presents it in his 1905 lecture – the result that every formula of the logical calculus for sentential logic is equivalent to a formula in one of two possible normal forms – also is recognisably prefigured in Schröder's 1877 booklet, in particular, in Schröder's theorem 14 and the following discussion. In his theorem 14 Schröder asserts that "every class b can be expressed linearly and homogeneously by means of another, a"[174] and can be so expressed, either in the form of a sum of products, or in the form of a product of sums[ccxii]. Schröder goes on to say:

> When it comes to the operations relied on to produce this expression we want to assume, first of all, that the symbols a and a_1[175] are connected amongst themselves and with others given independently of them only by means of logical addition and multiplication – which is a special case, to which, however, (as will become clear later) – all conceivable cases are easily reduced. In this case now any sum-expressions appearing, say, as factors can be multiplied out using [the distributive law], and the whole expression can be resolved into an aggregate of monomials. Each one of these monomials can never contain both the symbols a and a_1 at the same time [because of the law of non-contradiction and the law of excluded middle] and can contain each one at most once as a factor [because of the laws of idempotency for logical multiplication and addition].[ccxiii]

I want to briefly comment on two technical details which appear in relatively compressed form in this passage and are, I think, worth spelling out at somewhat greater length:

- Schröder claims that in no component expression both a sentence ('a') and its negation ('a_1') can occur at the same time. Schröder merely cites the results relevant to establishing this claim. I want to briefly spell out the justification Schröder presumably has in mind: According to the results Schröder cites, the logical product of any symbol with its own negation reduces to '0' (in Schröder's, not Hilbert's usage, i.e. the false) and their logical sum reduces to '1' (in Schröder's usage, i.e. the true). Any product having '0' as a factor further reduces to '0', while '1' is simply absorbed into the remaining factors; on the other hand, any sum having '1' as a summand reduces to '1', while '0' is absorbed into the remaining summands.

  In light of these principles, any expression containing both a sentence ('a') and its negation ('a_1') reduces to '1' if it is a disjunction, and to '0' if it is a conjunction. If it, in turn, is a component of a conjunction, it is either absorbed into the other conjunct(s) in the first case or reduces the entire conjunction to '0' in the second case; and if it, in turn, is a component of a disjunction, it either reduces the entire disjunction to '1' in the first case, or is absorbed into the remaining disjunct(s) in the second case.

  In any case, the expression having both a sentence and its negation as components disappears.

- Schröder says that the expression in question in his theorem 14 ultimately takes the form of an "aggregate of monomials". To understand what syntactic form Schröder has in mind, one must first understand what Schröder means by "aggregate" and "monomial". "Aggregate" is, presumably, another word for "(logical) sum" (or perhaps, more generally, for any result of (repeatedly) applying a certain binary operation). The

[174] One finds the idea that the components of a normal-form are "linear factors" also in Hilbert's discussion of normal forms (p. 243f.), with neither Schröder nor Hilbert discussing the precise sense of the phrase "linear factors" in the context of the logical calculus.
[175] In Schröder's 1877 booklet a subscripted '$_1$' appended to any expression is used to form the negation of that expression.

word "monomial", though occasionally used in Schröder's 1877 booklet, is never defined or explained there, likely because Schröder is assuming an understanding of it as part of basic mathematical training (in algebra).

Based on a series of examples Schröder exhibits in his *Lehrbuch*[ccxiv], "monomial" appears to be a relative concept: Schröder there seems to suggest that an expression is a monomial relative to a given operation just in case it is not composed directly by means of that operation; the most telling example is that Schröder considers the expression '$(x + y)^2$' a monomial with respect to addition because "it does not appear in the form of a sum at all", which must mean: not that the addition-operation was never used in the construction of the expression, but that it is *not the main operation* as a result of which the expression "appears".

Since, in the setting of Schröder's theorem 14, there are (by the assumption stated in the first sentence of the passage) only three operations, the claim that the expression in question is an aggregate of monomials effectively requires – assuming that "aggregate" to be a synonym for "(logical) sum" – that it is a disjunction of expressions which are not disjunctions, which must, therefore, be either conjunctions, negations or simply sentence-letters. Since Schröder also requires that "any sum-expressions appearing as factors" be "multiplied out" the "monomial"-components, as Schröder conceives them, will have the form of conjunctions of letters and negations, just like (disjunctive) normal forms of the kind Hilbert envisions.

In light of the evidence I have already produced in support of the claim that Hilbert was modelling the logical theory he presents in the 1905 lecture course on Schröder's 1877 booklet, it stands to reason that Hilbert's normal form-theorem was likewise inspired by something found in Schröder, namely, Schröder's theorem 14.

Indeed, if one re-interprets Schröder's theorem 14 as a claim about propositions, rather than classes (along the lines Schröder himself sketches[ccxv]), it, in fact, readily turns into a normal form-theorem for propositions: that every proposition can be regarded as equal to another *of a certain, specific form*. The continuity between Schröder's theorem 14 – which is, itself, as Schröder duly acknowledges, a novel «take» on Boole's idea of "development"[176] – becomes still more probable upon comparing with Schröder's sketch of a procedure the steps Hilbert outlines in his 1905 lecture in order to obtain what he there calls the "first normal form" (i.e. what is nowadays known as the conjunctive normal form)[ccxvii]. Comparing the details suggests also in this case that Hilbert arrived at his procedure for obtaining the normal form for a given sentence as a result of studying Schröder's theorem 14 (and so, mediately, on Boole's idea of «developing» a formula), in particular[177]:

1. Every one of the "known (binary) connections" available in (spoken) language can be expressed by means of the symbols available in the logical calculus using (only) negation, logical addition and logical multiplication.

[176] In the discussion of his theorem 14, Schröder explicitly cites Boole's development-formula, $f(a) = f(1) \cdot a + f(0) \cdot a_1$, and presents his own theorem and Boole's as alternative methods serving the same end[ccxvi].

[177] Indeed, in the exposition of normal forms for propositional logic in the first volume of the *Grundlagen der Mathematik*, Boole is explicitly mentioned as the originator of the particular method there presented for producing what Hilbert & Bernays call "privileged normal forms" (which is a more specific concept than that of a mere conjunctive or disjunctive normal form, and one for which Hilbert's claim holds that it is, in abstraction of the arrangement of components, uniquely determined)[ccxviii].

This corresponds to Schröder's initial restriction, "that the symbols a and a_1 are connected [...] only by means of logical addition and multiplication" (as opposed, Schröder means, to using also other operations).

2. Next, one can «apply» the negation until it remains to be applied only to atomic formulas[178].

   Schröder's sketch does not invoke de Morgan's laws at the corresponding stage. Indeed, Schröder proves these results only later (as his theorem 18), but does so in service of simplifying the process he sketched following his theorem 14[ccxix], while in his sketch he seems to be presupposing that the initial expression is already formulated in such a way that negation does not apply to complex expressions.

3. Next, one can use the distributive laws until one is left with a conjunction all of whose conjuncts are disjunctions of atomic sentences or negations of atomic sentences.

   This step is mentioned also by Schröder, even using the same (standard) keyword, "ausmultiplizieren", with the effect that, as Schröder puts it: "the whole expression can be dissolved into an aggregate of monomials".

4. Next, one can use the law of excluded middle, the law of reduction of disjunctions with the true as a factor entirely to the true and the law of absorption of the true into the remaining disjuncts to leave out any conjunct containing both a symbol and its negation. (Schröder appeals to those same laws in his claim that in the resulting form no conjunct can "contain both the symbol a and [its negation] a_1 at the same time".)

5. Use the law of idempotency to eliminate, as Hilbert puts it, "higher powers of a proposition"[179].

   Again, Schröder explicitly appeals to the same law for the same purpose.

6. Hilbert then indicates some further possible simplifications which have no obvious parallel in Schröder's sketch.

Comparing Hilbert's sketch of a procedure for obtaining a (conjunctive) normal form with Schröder's sketch for obtaining a "linear and homogeneous expression of one class by another" step by step makes a certain affinity between them apparent: While not all of Hilbert's steps have a parallel in Schröder's sketch, many do.

Even Hilbert's claim that the normal form of a given formula is "uniquely determined (except for the arrangement)" may have its root in Schröder's discussion, which ends with the remark that, once the procedure he outlined has been followed through "the coefficients contain nothing arbitrary any longer" and have "found their determination by way of their derivation from the given expression"[ccxxi]; though Schröder nowhere, as far as I can see, explicitly alleges that the form obtained by way of the method he describes is uniquely determined, this remark,

[178] In Zach's paraphrase of the relevant passage this is the first step: "First one uses de Morgan's laws repeatedly to see that every sentence can be written as sums and products of primitive propositions and their negations" (Richard Zach. "Completeness before Post. Bernays, Hilbert, and the Development of Propositional Logic." In: *Bulletin of Symbolic Logic* 5.3 [1999], pp. 331–364, p. 334). However, the remark Zach is presumably interpreting as a reference to de Morgan's laws – Hilbert does not explicitly cite his theorem 8, which asserts de Morgan's laws, nor any other result – comes *after* a remark emphasising what I presented (for the sake of rendering the parallel with Schröder salient) as the first step: that one can express connective using only negation, conjunction and disjunction.

[179] When Schröder first introduces the laws of idempotency, he remarks that "thus in the logical calculus no powers appear"[ccxx].

and the mention of "determination by derivation" does suggest that Schröder may have believed the result of his procedure to be uniquely determined on the basis of the initial expression to which the procedure is applied.

§ 117 Recognising Schröder's 1877 booklet as the source of Hilbert's early logical system – as I think the parallels I have pointed out provide ample evidence to do – accounts for most of the technical developments Hilbert presents in his 1905 lecture. But three notational changes Hilbert introduces remain unaccounted for: first and foremost, the «inversion» of '+' and '·' already mentioned previously; second, Hilbert's use of '≡' rather than Schröder's '=' for equality; finally, Hilbert's use of an overline as the notation for negation, rather than Schröder's subscripted '$_1$'.

§ 118 The last two are, to my mind, small and insubstantial enough changes to remain without further explanation.

In fact, a marginal note in Hilbert's hand on p. 225 of the official lecture notes compiled by Hellinger for the 1905 lecture course indicates that Hilbert himself abandoned this notation at some later point in time and reverted to using '=', as Schröder did. (This note was presumably added after the lecture course concluded, and after Hellinger produced a clean copy of the entire text, since the '≡'-notation is used throughout the remainder of the 1905 lecture course and all of the independently produced sets of notes for the lecture course I am aware of – by Max Born, Leonard Nelson and Otto Birck – show no indication of a change of notation. Since the notes for the 1914/15 lecture course found in the manuscript held at the IAS at Princeton show no sign of Hilbert using the '≡'-notation, and the 1914/15 lecture course was the first time Hilbert lectured on logic since 1905, the marginal note in the notes for the 1905 lecture course may date from when Hilbert revisited them in 1914/15.)

On the other hand, Schröder later adopted the overline-notation for negation in the third volume of the *Vorlesungen* – in the first two volumes Schröder still uses the subscript-'1'-notation of the *Operationskreis* –, as well as in a note publicising the publication of the third volume in the *Mathematischen Annalen*[180].

§ 119 The first, by contrast, «calls for» an explanation: Since Hilbert was, as I have argued, thoroughly familiar with (at least the first two sections of) Schröder's 1877 *Operationskreis des Logikkalkuls*, and since, furthermore, this «inversion» persists even in Hilbert's 1917/18 lecture course[ccxxii], it seems not to have been simply a mistake, but rather a deliberate choice. There must thus be a reason why Hilbert chose, on the one hand, to retain at least initially the same symbols Schröder used for conjunction and disjunction as well as those Schröder used for the constantly true and the constantly false proposition while, on the other hand, systematically interchanging them.

The most promising starting point for an explanation I known to me is a comment in Hilbert and Ackermann's

[180] Ernst Schröder. "Note über die Algebra der binären Relative." In: *Mathematische Annalen* 46 (1895), pp. 144–158, p. 146. Since Schröder's note appeared in the same issue of the *Mathematische Annalen* as Hilbert's "Über die gerade Linie…", Hilbert may have been aware of the note, but there is no evidence suggesting that he had studied it.

Peckhaus writes that the overline for negation was "damals üblich" (Peckhaus, "Hilberts Logik," p. 76). I confess that I find this claim, which Peckhaus does not support with any citations, puzzling: None of the then prominent logicians (not Schröder, Peano, Frege, Couturat,...) had used this notation, so I cannot think of a plausible sense in which it was "standard at the time"; the earliest use of the overline-notation for negation I am aware of is in Charles Sanders Peirce. "On an Improvement in Boole's Calculus of Logic." In: *Writings of Charles S. Peirce: A chronologial edition. Volume 2: 1867-1871*. Ed. by Edward C. Moore. Indiana University Press, 1984, pp. 12–22, p. 14 (published in 1867), where Peirce presents it as an improvement over the notation of Boole, who had written '$(1-p)$' for the negation of 'p' (one may speculate that Peirce arrived at this «improvement» by suppressing the unchanging '1' of Boole's notation, and, as it were, «moving» the '−' from standing in front of the formula 'p' to above it); and while Schröder acknowledges changing his notation as a result of Peirce's influence in a letter to Peirce (see the postscript to the letter dated March 7 1892: Houser, "The Schröder-Peirce Correspondence," p. 216), Peirce is unlikely to have been Hilbert's source.

Grundzüge der theoretischen Logik. In section 2, where conjunction and disjunction are introduced, Hilbert (and Ackermann) still insist on calling the conjunction "the logical sum", while calling the disjunction "the logical product" on account of "the analogy with algebra"[ccxxiii]. Then Hilbert and Ackermann go on to say:

> By the way, we could equally well have called $X\&Y$ the logical product and $X \vee Y$ the logical sum, and these labels are indeed more widely used in logic. Because there is, in contrast to algebra, a *second distributive law*: $X\&(Y \vee Z)$ äq $(X\&Y) \vee (X\&Z)$.[ccxxiv]

This remark suggests that Hilbert's reason for the inversion is rooted in the «analogy with algebra» somehow, that is to say, in what happens when one treats formulas of the logical calculus as «plain» numerical equations, and, furthermore, that Hilbert's inversion of Schröder's notation for the conjunction and the disjunction has to do specifically with the (first) distributive law (i.e. the distributivity of multiplication over addition).

In order to explain Hilbert's inversion of '+' and '$\cdot$' – using the former as a symbol for conjunction, rather than, as Schröder had, for disjunction, for which Hilbert now uses '$\cdot$' – I begin with two observations: In 1905 Hilbert claims that conjunctive normal forms (i.e. conjunctions of disjunctions of propositional letters or negations of propositional letters) were preferable to the disjunctive normal form (i.e. disjunctions of conjunctions)[181].

Furthermore, in his description of the procedure used to arrive at the first (see page 84), preferred normal form, Hilbert says that one should calculate products with sums as factors just as one would in the algebra of natural numbers. Now, the prescription to distribute multiplication over addition wherever possible can be carried out independently of which connectives one takes '$\cdot$' and '+' to represent, but in order for the resulting formula, where multiplication has been distributed «inward» (towards sentence-letters) as far as possible, to constitute a *conjunctive* normal-form, the symbol '+', which thus emerges as the main connective, must represent the conjunction.

To summarise, I am proposing the following explanation for Hilbert's inversion of '+' and '$\cdot$': For philosophical reasons Hilbert regarded the conjunctive normal form as preferable to the disjunctive normal form. At the same time he wanted the process of obtaining such a normal form to parallel, as much as possible, the process of calculation in the algebra of natural numbers; in particular, the distributive law familiar from the algebra of natural numbers – of multiplication over addition – should play a central role in this process. Because Hilbert wanted the result of this process to be his preferred, i.e. the *conjunctive* normal form, Hilbert was thus compelled to take '+' to represent the conjunction, leaving '$\cdot$' to represent the disjunction (and because the dual distributive law – of addition over multiplication – does not hold in the algebra of the natural numbers, he could not describe such a process leading to his preferred normal form by way of procedures familiar from calculation in the algebra of the natural numbers while maintaining Schröder's notation for conjunction and disjunction).

[181] The reason why Hilbert prefers the conjunctive to the disjunctive normal form are somewhat obscure. Hilbert says that a disjunctive form (unlike a conjunctive normal form) must be "surveyed in its entirety to extract a single positive fact from it"[ccxxv]. The remark found at the corresponding place in the notes recording the relevant lecture from 1914/15 (on p. 217 of the of the notes held by the IAS in Princeton) suggests that Hilbert means only that having the conjunctive normal form entitles one to assert each of its conjuncts, and so is more «deductively rewarding», as it were[ccxxvi]; although Hilbert goes on to note in 1914/15 that "for the purposes of criticism, the second [disjunctive] normal-form would be preferable", presumably because it projects an array of cases to be refuted separately in order to refute a given proposition. In Hilbert & Ackermann's treatment of normal-forms, Hilbert's preference seems to have changed completely: Now Hilbert & Ackermann attribute to the disjunctive normal form "the advantage of a special perspicuity"[ccxxvii].

The inversion of '0' and '1' then has to follow suit to maintain, on the one hand, the customary arithmetic rules of calculating sums and products with 0 and 1 as summands or factors while, on the other hand, ensuring, e.g. that the conjunction of a true and a false statement is calculated as being false: Interchanging '$\cdot$' and '$+$' but *not* '1' and '0' would yield, e.g., '$1 + 0$' for '$(\top \wedge \bot)$', which, by the customary arithmetical rules of calculation, would equal '1', i.e. a true proposition, but should equal '0'. Coherence is maintained by interchanging also '0' and '1', using the latter for a constantly false statement and the former for a constantly true statement.

This account also makes sense of the qualifying remark found in Hilbert & Ackermann's textbook, that, on account of the validity of the second distributive law in the logical calculus, one could just as well take conjunction to be the logical product and disjunction to be the logical sum; to somebody who recognises, as Hilbert first seems to do fully in 1917 (see the discussion in § 209), that the analogy to the algebra of natural numbers is disturbed in this respect in any case, Hilbert's initial reason for inverting '$+$' and '$\cdot$' will no longer seem compelling.

§ 120 In his Habilitationsschrift "Beiträge zur Algebra der Logik, insbesondere zum Entscheidungsproblem," Heinrich Behmann suggests an alternative explanation for why Hilbert may have chosen to invert '1' and '0' (and consequently also '$+$' and '$\cdot$' so as to not completely unhinge the customary arithmetical rules while correctly representing truth-functional connectives). After observing that Hilbert "in a lecture on the principles of mathematics"[182] (i.e., presumably, the 1917-18 lecture, which bore this title) uses the symbols '$+$' and '$\cdot$' differently, Behmann goes on to offer the following consideration to explain Hilbert's inversion of '0' and '1':

> [F]rom the point of view of propositional or intensional logic this second way [of correlating '1' and '0' with truth and falsity] is precisely the more natural just as the first is closer at hand for the class- or extensional logic [...].[ccxxviii]

As I understand Behmann's remark, Behmann is suggesting a different reason for the «inversion» of 0 and 1 than the one I proposed above, namely the following: If one thinks of the letters of the calculus as representing the sense of propositions – as opposed to the class of objects it holds of –, it is "more natural" to use '0' for the trivial proposition, i.e. the proposition which is always correct since it «asserts nothing», and to use '1' for the proposition which is never correct since it «asserts everything at once». Once one has accepted the inversion of '0' and '1', the same considerations as I outlined above urge the further «inversion» also of '$+$' and '$\cdot$'.

§ 121 In Hilbert's «inversion» of the symbols for the binary connectives, in particular, lies the root for another peculiar idea one finds in Hilbert's 1905 exposition of logic, namely, the idea that proofs are, as Hilbert puts it, "factors" of the propositions they establish. In the 1905 lecture, Hilbert speaks of a proof as a "Multiplikator" of a proposition (see p. 248 of the official lecture-notes compiled by Hellinger). In the lecture notes for the 1914/15 course held at the IAS in Princeton, Hilbert describes the process of mathematical inference by means of proofs as "the search for a suitable factor which, multiplied with the premisses, yields the claim"[ccxxix].

[182] See: Behmann, "Beiträge zur Algebra der Logik, insbesondere zum Entscheidungsproblem," p. 165. In addition to the published version, the carbon copy of the original typescript of the thesis with some (mostly insubstantial) corrections by Behmann is preserved among his papers held by the SBB as Nachlass 335, in box 7. The remark about Hilbert is also found in this typescript on p. 2.

Hilbert does not elaborate on this idea at any length, he merely states it[ccxxx]. This idea likely derives from remarks Schröder made about what he considered the main result of his *Operationskreis des Logikkalkuls*, (his Theorem 20^0), but how, exactly, I have not been able to figure out. It seems to me, in any case, that this metaphor of proofs as factors is born, in the first place, from a rather blunt act of syncretism: Seeing that, on the one hand, proofs lead from assumptions to theorems, and, on the other hand, that such factors produce the symbolisation of a theorem when multiplied with the symbolisation of assumptions within Hilbert's algebraic symbolism, Hilbert simply identifies these two things even though there is no obvious substantive reason to identify proofs with what factors in logical products *represent*, i.e. disjuncts. The sense of this metaphor thus remains elusive.

As far as I am aware, this metaphor does not appear in any of Hilbert's lectures or publications on logic after 1915. The last remote but still – however vaguely – discernible echo of this (kind of) metaphor in Hilbert's discussions of logic I am aware of is Hilbert speaking of "a remainder of assumptions in logical deduction" (see the passage quoted in endnote cccvii) in his 1919/20 lectures on *Natur und mathematisches Erkennen*, i.e. of something which cannot be fully factorised into purely logical inferences.

§ 122

It seems to me that this idea of proofs as factors of propositions is, above all, a testament to the paucity of Hilbert's conception of proofs in 1905 (and still in 1915[183]): In identifying proofs with "factors" of the propositions they prove, Hilbert places them within the same «grammatical category» as the propositions themselves: Hilbert seems to be thinking of proofs as simply a handful of propositions taken together.

There is thus no indication that, at this point, Hilbert thinks of proofs as having a specific (syntactic) *structure* of their own, which it might be interesting to consider and describe, and even to manipulate.

Indeed, Hilbert's attempted syntactical consistency proofs (as one finds them recorded in Hilbert's 1904 *Über die Grundlagen der Logik und Arithmetik*) proceed by establishing that *propositions* of certain syntactic forms can never be produced (as expressions) by certain rules (governing the formation of expressions). To be precise, Hilbert in 1904 establishes a claim about individual sentences (namely, that in any theorem of the system he lays out there the terms on each side of '=' must have an equal number of symbols), not about proofs (conceived of as more complicated (syntactic) objects)[ccxxxi]. In this article, Hilbert maintains, rather, that a proof should be "considered as a mathematical object, namely, as a finite set"[ccxxxii], presumably of propositions required for establishing a conclusion; and, although Hilbert adds that these propositions constituting a proof are "linked by propositions expressing that the proof leads from the axioms to the conclusion"[184], it seems that he conceives of the

[183] As I said in § 110, Hilbert's logical theory changed little between 1905 and 1915, and, in particular, these observations and remarks about proofs as factors are re-iterated in the 1915 lecture course.

[184] I want to note in passing at this point that the first serious investigations into what has since become known as "structural proof theory", the work of Paul Hertz in the early 1920s, takes this characterisation of proofs as a point of departure: In his brief lecture from 1921 (published the following year), Hertz sets out to study what he calls "sentences" ("Sätze") which "link elements"[ccxxxiii], and then considered proofs as sequences of such propositions (Hertz defines "proof" in: Paul Hertz. "Axiomensystem für beliebige Satzsysteme I." in: *Mathematische Annalen* 87 [1922], pp. 246–269, p. 250). That Hertz takes Hilbert's early conception of proofs as a point of departure and working out on its basis many of the notions now considered central in structural proof-theory – what Hertz calls a "sentence", for example, is a special case of what Gentzen later calls a "sequent" – may suggest that Hertz' work reignited Hilbert's interest in structural proof theory in the early 1920s. Since Hertz published his work around the same time Hilbert began to develop proof-theory in his lectures, it seems in any event likely that Hertz' work predates Hilbert's own forays into proof-theory.

proofs themselves as simply finite collections of propositions – which could, in theory, be compressed into a single conjunction – rather than as a separate syntactic object with a distinctive syntactic structure.

The shift of attention to constructing a theory focused, in particular, on *proofs*, conceived of as (ordered) sequences of sentences and as having a distinctive structure of their own (rather than other syntactic entities, like propositions), thus appears to be a genuine, substantive innovation of Hilbert's work in the early 1920s.

3.2.2 Hilbert's Reworking of Arithmetic

§ 123 I want to briefly mention a further source of evidence for the hypothesis that Hilbert was, in spite of the lack of explicit acknowledgement, well-acquainted with Schröder's *early* work, and that it served as a point of departure for more than Hilbert's thinking about logic: In 1913 Hilbert seems to have spent a considerable amount of time and energy on developing an axiom-system for a theory of the natural numbers. These efforts are documented in the Cod. Ms. D. Hilbert 559, especially in the portion of the manuscript which contains Baule's lecture notes on the axioms of arithmetic: These show signs of *substantive* and repeated revision to the extent that Hilbert or Baule thought it necessary to write out another clean version of the exposition once the revisions were finished.

This clean copy seems to have served as the basis for the relevant lectures in the 1914/15 lecture course, since the same axioms and theorems appear in the same sequence (except for theorems 1 and 2, which have changed places, in accordance with a correction made in pencil to the clean copy of the axioms in Cos. Ms. D. Hilbert 559) and with the same proofs in the lecture-notes for that course preserved at the IAS in Princeton (on pp. 195-208).

That Hilbert thought these efforts important is attested by the fact that he gave a presentation on the topic to the Mathematische Gesellschaft in Göttingen (on February 10 1914[185]).

§ 124 This work also marks Hilbert's, as far as I can see, first explicit engagement with Peano's work. On a leaf inserted into the notebook held by the SUB Göttingen under the signature Cod. Ms. D. Hilbert 559 one finds the following axioms referred to as "Peano's Axioms": 1, 0 is a natural number, and if n is a natural number, so is $n+1$; 2, if $a+1 = b+1$, then $a = b$; 3, there is no natural number a such that $a+1 = 1$ and 4, the induction-axiom. One finds approximately this list of axioms (in this order) in the *second edition* of Peano's *Formulaire*[186].

§ 125 Hilbert's own axiom system in 1913 bears little resemblance to Peano's, however. Neither the axioms Hilbert formulates, nor the primitives he relies on are the same as those Peano chose to work with. I should add, however,

I hope to explore the place of Hertz in the development of proof-theory in more detail in future work.

[185] See the report of the meetings published in the *Jahresbericht der Deutschen Mathematiker-Vereinigung* 23 (1914), p. 28 of the *Mitteilungen und Nachrichten*-section (which is paginated independently of the substantive part of the *JDMV*). While this report records no details of Hilbert's presentation, I find it plausible to think that Hilbert presented the work documented in the Cod. Ms. D. Hilbert 559, since Hilbert seems to be newly thinking through the topic there and then (in 1913), and, allowing some time after the end of the semester for final revisions puts the completion of that work some time in late 1913, in time for a presentation in early 1914.

I first became aware of these reports through seeing the (partial) list compiled by *The Adventure of Reason. Interplay between Philosophy of Mathematics and Mathematical Logic 1900-1940*. Oxford University Press, 2010, p. 160.

[186] When Hilbert *explicitly cites* Peano in 1917, he does indeed cite the *second* edition of Peano's *Formulaire* (see footnote 187). One possible explanation – and, to my mind, also plausible one – of the fact that Hilbert seems to be relying on the second, rather than any of the subsequent editions, of Peano's *Formulaire* is that this, but no subsequent edition, appeared in an (authorised, partial) German translation (due to Bohlmann and Schepp) as appendix II of *Differentialrechnung und Grundzüge der Integralrechnung* (published in 1899), wherein the axioms of arithmetic are found on p. 353.

that by 1917, Hilbert seems to have adopted Peano's axiom system: One finds him presenting it in section 7-8 of chapter IV of his lecture course on set-theory – held in the Sommersemester 1917 – and explicitly attributing it to Peano; citing, as it happens, the *second* edition of Peano's *Formulaire*[187].

While bearing no obvious resemblance to Peano's axiomatisation, which Hilbert likely studied only after coming up with his own, Hilbert's choice of axioms and primitives in 1913 *is*, upon inspection, not all that dissimilar from *Schröder's* treatment of arithmetic in chapter 2 of his *Lehrbuch der Arithmetik und Algebra* in certain respects:

- Not least of all the label "Axiome der ganzen positiven Zahlen", which Hilbert uses for his axiom system, can be found in Schröder as an alternative to speaking of "natürliche Zahlen"[ccxxxiv].

- Like Schröder's exposition, Hilbert's system focuses on equality, addition and order as primitives, while leaving multiplication, as well as other operations out of consideration, at least initially: In the lecture-notes for the 1914/15 lecture course, where Hilbert also presents this axiom system for arithmetic, Hilbert is reported as saying that "the introduction of multiplication brings with it no further difficulties"[ccxxxv].

  Schröder explicitly introduces multiplication in sections 3 and 4 of chapter 2, after focusing only on addition (and order) in sections 1 and 2 of chapter 2.

- Like Schröder – and unlike Peano, as well as Russell and Frege – Hilbert conceives of the natural numbers as beginning with the number one, rather than zero, which allows him to establish as a theorem (in more recent notation) '$\forall x \forall y(\exists z\ x = y + z \rightarrow x > y)$'[188] (which holds in Peano's system, where it follows from Peano's definition of '$>$', only with the added condition that the increment z is not equal to zero).

As far as I can see, presenting axiom systems for the natural numbers with only addition and order as primitives § 126
(at least for the time being) is rare at the time: Besides Hilbert and Schröder – the only other such system I am aware of is due to Huntington[189] – and with the number one, rather than zero, as a smallest number, with addition as the only binary operation, and, most strikingly, with order as a primitive[190]. While, of course, not by any means

[187] Two sets of lecture notes were eventually deposited in the library of the Mathematische Institut in Göttingen: a handwritten set of notes by Margarethe Goeb, presumably recorded at the lecture, and a typed set of notes, presumably based on the handwritten notes. The reference to Peano's *Formulaire* is found on p. 145 of the typescript (library signature "13903a").

[188] This is proposition 6 in Hilbert's final arrangement of the propositions he derives from his axioms. '$>$' must here be interpreted in the sense of "*strictly* greater", since Hilbert's axiom IV asserts (in more recent notation) '$\forall x \forall y(x > y \rightarrow x \neq y)$'. One finds this theorem also stated by Schröder, who associates it with the idea that "the whole is always greater than its parts"[ccxxxvi].

[189] The system in question is Huntington's "first set of postulate for real quantities" presented in § 1 of his Edward Vermilye Huntington. "Complete sets of postulates for the theory of real quantities." In: *Transactions of the American Mathematical Association* 4.3 (1903), pp. 358–270, p. 360. Like Schröder, Huntington subsequently (in § 2) extends his system to a "second set of postulates for real quantities" (Huntington, "Complete sets of postulates for the theory of real quantities," p. 365), which include also multiplication. There is, in any case, far less overlap in detail between Huntington's axioms and Hilbert's than there is between Hilbert's and Schröder's system; e.g. Huntington includes both the associative and the commutative law for addition among his axioms (axioms 7 and 6 respectively). In Huntington's earlier "Complete sets of postulates for the theories of positive integral and positive rational numbers," order is defined (see: Edward Vermilye Huntington. "Complete sets of postulates for the theories of positive integral and positive rational numbers." In: *Transactions of the American Mathematical Association* 3.2 [1902], pp. 280–284, p. 267).

[190] Felix Klein also proposed what is effectively an axiom system for basic arithmetic (although Klein speaks of "fundamental propositions", rather than "axioms") with order as a primitive in chapter I of part one of the first volume of his lectures on elementary mathematics from a higher standpoint (first published in 1908). The fact that Klein also regards order as a primitive concept aside, his fundamental propositions have less in common with Hilbert's than Schröder's do: Klein considers both addition and multiplication from the outset; his system requires the existence of a neutral element for addition, i.e. the number zero; furthermore, Klein does not include any kind of induction principle among his fundamental propositions (though he mentions the possibility[ccxxxvii]).

a decisive reason, this exclusive likeness does, I believe, make plausible the claim that Hilbert developed his axiom system of arithmetic on the basis of a relatively thorough study of (at least parts of) Schröder's *Lehrbuch*.

§ 127 Schröder announces at the beginning of his chapter 2 that he will offer two separate treatments of arithmetic. The first of these Schröder calls "independent"[ccxxxviii]; he seems to have in mind something like what Hilbert calls the "genetic method", i.e. an informal presentation of the matter along certain lines seen to parallel the historical development of arithmetic[ccxxxix]. The second of these two methods Schröder calls "recursive". He attributes it "mainly to [Hermann] Grassmann" and describes as the response to "a need for a stricter simplification of the presuppositions taken as basic and a greater limitation of the means of inference subsequently used"[ccxl]. At the beginning of his "recursive treatment" Schröder vows "not to presuppose any of our results so far"[ccxli], and then goes on to articulate the presuppositions that strike me as akin to Hilbert's axioms.

Schröder's so called "recursive" treatment of arithmetic doesn't present an axiomatic system as such, since Schröder doesn't explicitly list fundamental assumptions, nor explicitly restrict his means of inference and present proofs with according rigour. It does, however, lend itself to being recast in such a fashion, I think, because it does all those things *effectively*, in all but official styling. I will therefore refer to what Schröder calls his "recursive treatment" as a "proto-axiomatisation" of the natural numbers.

§ 128 There are further similarities in points of detail between Hilbert's axiom system for the natural numbers and the theory one finds Schröder presenting in his *Lehrbuch* in what might be called a "proto-axiomatic" fashion:

- All five of Hilbert's axioms are more or less recognisably formulations in symbolic notation of principles whose fundamental importance one finds Schröder stressing at the beginning of his (proto-)axiomatic treatment of arithmetic beginning in § 6 of chapter two of Schröder's *Lehrbuch*:

  1. Hilbert's axiom I – that 1 is a natural number, and for any given natural number n, $n+1$ is again a natural number – can be seen to recall Schröder's "recursive definition of the natural numbers". Schröder states the latter part explicitly: "Now whenever a is a number from our sequence, so too is $a+1$."[ccxlii]

  2. Hilbert's axioms II ($n+1>1$) and IV (if $b>a$, then $b\neq a$) can be seen as making precise Schröder's assertion that the sequence of natural numbers must be "such that no earlier number ever re-occurs"[ccxli].

  3. Hilbert's axiom III (the transitivity of '$>$') is explicitly stated as a principle also by Schröder[ccxliii].

     It is worth noting Schröder believes this claim can be established, while Hilbert thinks it must be assumed as an axiom along with two other axioms governing the order-relation[191].

  4. Hilbert's final axiom V, the induction-axiom, can also be found in Schröder, in § 7[ccxliv].

[191] I want to parenthetically speculate about the reason why Hilbert took this claim to be an axiom: That may be because Hilbert recognised that the concept of order was implicit already in Schröder's conception of numbers as a sequence "such that no earlier number ever re-occurs" (or, at any rate, in Hilbert's way of making this conception explicit by means of axioms), so that without such an axiom one would not have enough means to establish anything about natural numbers, and certainly not this very claim.

As with the transitivity of '>', Schröder's view concerning the status of the principle of induction remain unclear: Although he doesn't say anything to suggest he thinks it can somehow be established by way of a rigorous argument, he also does not present it as a fundamental *assumption*[192].

- Many of the theorems Hilbert derives from his axioms can also be found explicitly or implicitly in the sections of Schröder's proto-axiomatic treatment concerned with addition as the only operation:

  1. Among the most peculiar technical details of Hilbert's initial attempt of developing an axiom system for the natural numbers, upon which much of Hilbert's subsequent revision focuses, is a proof of the commutative law for addition on the basis of the definition of addition alone. The resulting proof has a gap, where it presupposes the associative law for addition, which, however, in Hilbert's initial axiom system is not available at that point; the result of Hilbert's revisions is, ultimately, to establish the associative law beforehand. This initial arrangement may have been suggested by Schröder's discussion, where the commutative law is labelled "the first law of addition" (in § 2 of chapter two), while the associative law is labelled "the second law of addition" (in § 3 of chapter two).

  2. Four of Hilbert's 12 theorems (namely theorems 5-8 in Hilbert's final arrangement of theorems) are devoted to showing that both order-relations, the (for Hilbert) primitive '>', as well as the (for Hilbert) defined '<', can be defined in terms of addition. The interdefinability of order and addition is explicitly asserted by Schröder[ccxlv], and Hilbert's theorems can be seen as bearing out this claim.

     In the definition of '<' in terms of addition Hilbert states – '$a < b$' just in case (in Hilbert's notation) '$a + c = b$' –, the lack of quantifiers is especially striking, since they would be of great use here to indicate the different roles 'a' and 'b' play in contrast to 'c'; (formulating the definition Hilbert intended in an arithmetical deductive system nowadays one would state the definiens rather as '$\exists z\ x + z = y$').

     Hilbert's apparent lack of familiarity with explicit quantificational notation is another reason, to my mind, to believe that Schröder's early *Lehrbuch*, which is likewise marred by not employing explicit quantifiers, served as Hilbert's point of departure, rather than the work of Peano (or Russell or Frege, all of) who(m) had at that point in time internalised the use of quantifiers (as had Schröder before his death more than ten years before Hilbert's work on this axiom system of natural numbers).

The resemblances between Schröder's *Lehrbuch* and Hilbert's efforts between 1912 and 1915 to articulate an axiom system for basic arithmetic I have pointed out are admittedly of a more remote nature than in the case of Hilbert's early logical theory and Schröder's booklet on the *Operationskreis des Logikkalkuls*. Not only does Hilbert nowhere explicitly refer to Schröder's work, there are also fewer instances of exact likeness; there is thus significantly

[192] Stressing the fundamental role of induction in the rigorous development of arithmetic had been a hallmark of Poincaré's methodological writings, especially in his article "Sur la nature du raisonnement mathématique" (originally published in the *Revue de la métaphysique et de morale* in 1894, later reprinted in *La science et l'hypothèse*), which Hilbert may well have been acquainted with.

less evidence available to weigh against the hypothesis that the convergences of Schröder and Hilbert in substance and styling of their arithmetical theories are due merely and only to the fact that they are both independently codifying a shared «folklore» conception of natural numbers.

One ought not to underestimate, however, the extent to which the work of Schröder, and that of Grassmann, whom Schröder explicitly credits for his pioneering work (see the passage quoted in endnote ccxl), *shaped* whatever «folklore» conception of basic arithmetic Hilbert and others of his generation may have had. I want to point out in this connection that Klein considered Grassmann's *Lehrbuch der Arithmetik* "ground-breaking" when it came to the rigorous axiomatisation of arithmetic, along with Peano's work[ccxlvi].

Hilbert later refers to Helmholtz' article "Zählen und Messen, erkenntnistheoretisch betrachtet" in his discussion of elementary arithmetic in his 1921/22 lecture course entitled "Grundlagen der Mathematik"[ccxlvii]. But Helmholtz, in turn, begins his reflections by acknowledging explicitly the pioneering work of both brothers Grassmann, as well as Schröder's *Lehrbuch*[ccxlviii] with unambiguous approval. Indeed, Helmholtz acknowledges that he himself is "following throughout" the treatments of the brothers Grassmann and that of Schröder.

So I think that, on balance, before this backdrop the similarities between Schröder's work on arithmetic and Hilbert's efforts are sufficiently striking to believe that Hilbert was at least aware of Schröder's work, but also that Hilbert had probably studied (at least parts of) it relatively thoroughly.

Hilbert's familiarity with Schröder's *Lehrbuch* would also be of interest in connection with what I've been call- § 129
ing Hilbert's "philosophical position", because Schröder there propounds – especially in the general, programmatic remarks of Schröder's introductory chapter one – a conception of the natural numbers which bears striking resemblance to the sometimes so called "formalist" conception of arithmetic. This is to say that Schröder formulates and endorses the following three theses (among others): (I) Natural numbers *are* (merely) marks of some sort[ccxlix]. (II) Considered by themselves, these marks have no significance[ccxlix]; they have significance only insofar as the marks are consciously assigned to objects by way of the process ordinarily known as counting[ccl]. (III) Arithmetic consists in the development of a system of relations between such marks according to certain laws[ccli].

One finds Hilbert severely criticising precisely this «formalist» conception of the natural numbers as "most ridiculous and naive" in a brief discussion – including, notably, a reference to Frege's *Grundgesetze der Arithmetik*, presumably to Frege's discussion in volume 2[193] – preceding the presentation of his axiom system of the natural numbers in the lecture notes produced by Baule found in the convolute Cod. Ms. D. Hilbert 559[ccliv]. Although the particular "ridiculous" features of such accounts Hilbert mentions in this brief remark cannot plausibly be attributed to Schröder, whose account is more sophisticated than what Hilbert sketches[194], this remark, and also Hilbert's

[193] In his postcard thanking Frege for the copy of the book Frege had sent him, Hilbert indicates that he has read the critical discussion, and claims to be "generally in agreement with [Frege's] criticism", though he voices reservations about their fairness with respect to Dedekind and Cantor[cclii]. An explicit reference to this discussion (specifically, to "p[p]. 129-130 and p. 131") can be found in Hilbert's 1920 lectures (see: Hilbert, *David Hilbert's Lectures on the Foundations of Physics 1915-1927*, p. 330); Hilbert once again refers to Frege's "right" criticisms "of traditional definitions of irrational number" in his "Neubegründung der Mathematik"[ccliii].

[194] I already stressed this point in § 32, when discussing Frege's critique of Schröder.

acquaintance with Schröder's *Lehrbuch* shows that Hilbert was well-acquainted with the «formalist» conception of arithmetic (and mathematics more broadly) found in the works of such contemporaries as Hermann Hankel, Johannes Thomae or Otto Stolz[195], and that he rejected it (perhaps following Frege).

§ 130 I mention these two facts to stress a simple point: Hilbert's acquaintance with Schröder's work shows that Hilbert knew well what the conception of numbers (among other mathematical objects) held by a number of more or less prominent German mathematicians in the 19th century, which came to be known as "formalism", was; and, as the remarks recorded, e.g. in the notes for the 1913 lecture course show, Hilbert unequivocally rejected this position as naive. It is therefore all the more surprising that Hilbert later appears to have accepted for himself a conception of natural numbers which might fittingly be described as "formalist". Hilbert begins the presentations of arithmetic found in his 1921 lectures on the foundations of mathematics[196] with the following assertion:

> For mathematical knowledge it is best to choose the signs in such a way that relations among cardinal numbers can be displayed by them visually. In this way we can develop number theory as a theory grounded in intuition concerned with certain simple shapes, which we shall call "number-signs". The connection between these number-signs and the cardinal numbers will later emerge in a natural way. Our theory thus deals with shapes given in intuition, of which we presuppose that their form can be always recognised independently of place and time and of the specific manner of their production. Now, the number-signs shall be shapes of the following nature: They consist of a sequence of the signs "1" and "+"; they begin with 1 and end with 1. A 1, unless it stands at the end, is followed by a +; and a + always by a 1.[cclv]

There remains a decisive difference between this and the formalist position Hilbert earlier rejected, even if Hilbert gives expression to it only in a parenthetical remark: That Hilbert speaks of a "relation between these number-signs and cardinal numbers" indicates that, unlike 19$^{\text{th}}$ century formalists, he does not *identify* numbers with certain signs and so Hilbert cannot be truly said to have accepted a formalist conception of arithmetic in this sense[197].

§ 131 Two other details presented in this passage[198] are, however, far more interesting in the present context: First, Hilbert describes number-signs here exactly like Schröder described them in his *Lehrbuch*, as composed of '1' and '+'. In the 1904 lecture, by contrast, Hilbert had described numbers as sequence of vertical strokes only. Hilbert also mentions this possibility in the 1921 lectures but decides against it in order "to accommodate more intimately the familiar habits"[cclvi]. Compare with both of these assertions, Schröder's introduction of number-signs

> To obtain a sign which can express *how many* units are present [...] one displays each of them with a stroke: 1 (a *One*); these Ones are placed adjacently in a line, but linked amongst themselves with the sign + (*plus*), because otherwise, for instance, 111 would be read, according to the habitual designations for numbers, as a hundred

[195] Schröder explicitly refers to Hankel in his partial bibliography at the end of the preface (Ernst Schröder. *Lehrbuch der Artihmetik und Algebra.* Vol. 1 (mehr nicht erschienen). Verlag von B. G. Teubner, 1873, p. VI); later on, in the introductory chapter, Schröder also refers to Johannes Thomae (Schröder, *Lehrbuch der Artihmetik und Algebra*, fn. *) p. 2). Both Hankel and Thomae, and, indeed, the very works Schröder mentions, are targeted by Frege in his raging rejection of formalism, as he conceives of it, in the second volume of his *Grundgesetze der Arithmetik*. Schröder himself does not figure prominently in Frege's critical discussion in the second volume of the *Grundgesetze der Arithmetik*, but his *Lehrbuch* had already received a thrashing earlier, in Frege's *Grundlagen der Arithmetik*.

[196] These, along with the other lectures on logic and the foundations of mathematics Hilbert delivered in the (early) 1920s have recently been edited in Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*.

[197] Ewald, "Hilbert's Wide Program," p. 232 also emphasises this point.

[198] A condensation from this passage served Hilbert as an exposition of this foundationalist viewpoint about number theory in: David Hilbert. "Neubegründung der Mathematik. Erste Mitteilung." In: *Gesammelte Abhandlungen. Analysis, Grundlagen der Mathematik, Physik, Verschiedenes, nebst einer Lebensgeschichte.* Vol. 3. Springer Verlag, 1935, pp. 157–177, pp. 163 sq.

and eleven. In this way one obtains signs like[199]:

$$1+1+1+1+1$$

whose composition can be described by saying: *A natural number is a sum of Ones.*[cclviii]

§ 132 Second, the added condition one may be inclined to overlook as extraneous to the theory proper, that number-signs must "always be recognis[able] independently of place and time and of the specific manner of their production". This formulation echoes one part of the principle Hilbert had in his 1905 lectures labelled "the axiom of thinking".

As it is formulated in the 1905 lectures this principle comprises two separate claims: First, Hilbert presupposes that the human mind is given any objects at all to think about; second, Hilbert presupposes that these objects can be associated with symbols which are such that one is always capable of recognising such a symbol where- and whenever one encounters it as being (a token of) the particular symbol it is.

I shall not discuss the first of these claims in any detail here, but I still want to note in passing that it, too, has a parallel in Schröder, who notes that "it must be admitted as an axiom [...] that there are things at all which are at hand in equal or unequal numbers"[cclix]. In light of this parallel, and the one to the second claim I quote at length below, one can see Hilbert's axiom of thinking as compacting into a single claim the only two assumptions Schröder calls "axiom"s in his *Lehrbuch*. However, I should add that for the first of the two claims merged in Hilbert's axiom of thinking, the likelier source is Cantor, who states the claim that "there are things (i.e. objects of thought" as a fundamental axiom of finite number theory in a letter to Hilbert dated 20/2/1900[cclx] Since Cantor asks in the same letter for Hilbert's opinion on the subject of the arithmetic of finite cardinal numbers, this letter may have prompted Hilbert to think about the matter, leading to his 1904 article, and the remarks about this topic in at the very end of his 1905 lecture course.

Here I want to point out and stress that it also echoes – and so does Hilbert's earlier *axiom of thinking* itself – the principle Schröder introduces in his *Lehrbuch* as "the only axiom", which Schröder formulates as follows:

> The principle in question might be called the *axiom of the stability of signs*. It assures us that in all our developments and inferences the signs stick around in memory – and even more firmly to paper. The number of Ones, which we write down, does not, for example, change while we reposition or rearrange them; they can neither spring up from the paper like mushrooms, nor disappear spontaneously. Quite generally the letters remain what they are while we operate with them; no a transforms into a b; also – if we take the necessary care – no deception can occur by which an x is turned into a u! Without this principle, of which we convince ourselves on the basis of experience by inductive generalisation, all deduction would be illusory, because deductions begins precisely when – after having dressed up the things according to their basic properties in signs – the study of things cedes its place to that of their associate signs.[cclxi]

As Hilbert does in his "axiom of thinking", Schröder here stresses that the ability to always unfailingly recognise signs as the particular signs they are constitutes an indispensable presupposition for his entire investigation.

§ 133 To summarise this excursion into Hilbert's engagement with Schröder's work, I take myself to have (1) established with near certainty that Hilbert developed his early logical theory, which he presents in his 1905 lectures, on the

[199] It is perhaps also worth noting that Hilbert, like Schröder, uses the example of the number-sign '1 + 1 + 1 + 1 + 1', and its decomposition into '1 + 1' and '1 + 1 + 1' to illustrate this conception of number-signs, and the conception of addition based on it[cclvii].

basis of studying Schröder's 1877 booklet *Der Operationskreis des Logikkalkuls*. (2) While I cannot point to what strikes me as equally incontrovertible evidence to support the further claim that Hilbert also studied Schröder's 1873 *Lehrbuch der Arithmetik und Algebra* (and was, indeed, substantively influenced in developing his theory of arithmetic first presented in his 1913 lectures), I do think that the similarities between Schröder's treatment of arithmetic and Hilbert's do also invest this claim with some plausibility; the following particular points of likeness seem to me most strongly to support the claim that Hilbert had seriously studied Schröder's *Lehrbuch*: (a) Hilbert's formulation of the identity-rule in the logical calculus of 1905, which (as I pointed out on page 79) resembled that found in Schröder's *Lehrbuch*, rather than the identity-axiom found in Schröder's 1877 booklet on the *Logikkalkul*, (b) the fact that Schröder was, as far as I can tell, the only one to formulate a theory of arithmetic with order and addition as primitives and that there is, as I have noted in some detail, at least a noticeable affinity between the axioms and results about natural numbers Hilbert presents in 1913 and those found in Schröder's *Lehrbuch*, (c) Hilbert's 1921 conception of number-signs as consisting of '1' and '+' for the sake of "accommodating habits", (d) the idea for the "axiom of thinking", in particular, the condition of recognisability of signs, and its similarity to Schröder's "only axiom". In any case, I hope to have made clear that Schröder's work was of a more decisive importance to Hilbert's development than has, so far, been realised or appreciated.

3.2.3 Principles, Fundaments and Foundations

I want to turn now to discussing on a grander scale Hilbert's conception of foundational studies before the 1920s. As a starting point to this discussion, I will offer some observations about the terminology Hilbert employs.

The titles of the lectures I mentioned in § 106 already show that Hilbert's terminology is far from fixed: Hilbert § 134
lectures on "logical principles" (1905), on "principles" (1908, 1913), on "elements and questions about principles" (1910) or "elements and principles" (1913), and on "problems and questions about principles" (1914/15).

Comparing these with the title of his last geometry lecture (and his 1899 booklet) reveals more diversity yet: In connection with geometry, Hilbert spoke of the "foundations [Grundlagen] of geometry". But the word "foundations" seems to drop out of Hilbert's active foundational vocabulary in the years between 1905-1914, during which Hilbert predominantly speaks about "problems" and "principles" ["Prinzipien"]; indeed, Hilbert's 1917/18 lecture course is still entitled "principles of mathematics", and only later, already well into the 1920s – starting with a lecture course delivered in 1921/22 entitled "laying the foundations [Grundlegung] of mathematics", although the official lecture-notes for this course, produced by Bernays, already bear the title of Hilbert & Bernays' later great collaborative work, "Grundlagen der Mathematik" – does Hilbert appear to return to speaking of "foundations".

While there is no denying the manifest variety in Hilbert's terminological choices, merely observing that the labels Hilbert applies to his foundational investigations change over the years does not answer the question what, if anything, to make of those changes. In my estimation the changing titles are symptoms of changes in more

than terminological preferences on Hilbert's part; although they may not exactly match up in the end – because Hilbert's use of general, methodological terminology is, to an extent, imprecise –, certain substantive differences in how Hilbert views his aims and his material go along with these terminological changes.

In particular, it is, in my view, clear that Hilbert associates with the word "principle" ["Prinzip"] rather different ambitions from what he has in mind when he speaks of "foundations" ["Grundlagen"]. I will now try to bring clearly into focus the substantive distinction I see underlying this particular terminological difference.

It emerges most clearly in the introductory remarks of Hilbert's lecture courses in 1908 and 1910. § 135

Hilbert began both of these courses by distinguishing different approaches to foundational investigations.

After briefly discussing the foundations of arithmetic and geometry – in connection with the latter the axiomatic method is briefly discussed in general terms – in the introductory remarks of his 1908 lecture course, Hilbert goes on to say that in the present lecture course he is interested in principles in a different sense[cclxii], namely, in the sense of "core ideas" of particular disciplines, and of mathematics as a whole[cclxiii].

In the 1910 lectures Hilbert explicitly draws the distinction implicitly drawn in the 1908 lectures between, on the one hand, the concern with particular ideas central to certain disciplines and the investigation of how these ideas are developed in those disciplines, from the concern with basic assumptions from which a discipline can be built up in its entirety[cclxiv]. There, Hilbert proposes to use the word "principle" exclusively for the former concern[cclxv], while using the word "fundamentals" ["Fundamente"] when speaking about the latter[200].

By calling his lecture courses something to do with "principles", as Hilbert proposes to use this word, rather than fundamentals or foundations, Hilbert thus associates them with *a different project* than his earlier work on the foundations of geometry, arithmetic and the axiomatic method was in service of. In the lecture courses between 1908 and 1914, Hilbert professes no interest in isolating and investigating especially simple or basic assumptions necessary (and sufficient) for the construction of precisely delineated theories spanning an entire field, rather, he is interested in clarifying the central concepts and argumentative patterns deployed in particular disciplines.

Indeed, the seminar Hilbert gave in 1908/9 suggests that, at the time, Hilbert treated his concern with "princi- § 136
ples" of mathematics as a sort of catch-all vessel for considering a variety of issues and approaches, with a special emphasis placed on clarifying deductive connections: While there is no apparent relation with regard to the mathematical content of the various presentation-topics Hilbert outlines in his opening lecture (recorded in the Cod Ms. D. Hilbert 570), he repeatedly stresses that the presenter's task will be to clarify the deductive structure of the argument in such a way as to bring the core ideas and argumentative patterns clearly to the fore[cclxvi].

Clarifying deductive relations is not just valuable in and of itself, but serves also to facilitate further foundational § 137
study: Hilbert stresses that, before one can undertake foundational investigations of the kind he undertook in the

[200] Schlimm previously observed that Hilbert (in other writings) uses both "Grundlagen" and "Fundamente" (see: Schlimm, "Metaphors for Mathematics from Pasch to Hilbert," pp. 317 sq.), but does not discuss the question whether in Hilbert's usage these terms carry different meanings, or as synonyms. The passage Schlimm points to – taken from the opening remarks of an earlier lecture by Hilbert on the quadrature of the circle – is, in fact, a «precursor» of the passage in the 1910 lecture I just referred to. It thus seems that in the course of time Hilbert came to associate a substantive distinction with the different piece of terminology he uses.

Grundlagen der Geometrie, one has to possess a thoroughgoing acquaintance with the relevant discipline.

The motivation for this concern with principles, so far as it emerges from Hilbert's own remarks, thus seems to be didactic. In his preparatory notes for the 1914/15 lecture course (preserved in Cod. Ms. D. Hilbert 559, see 293 in appendix B), Hilbert explicitly raises the thorough acquaintance with mathematics required for appreciating foundational investigations as a serious "pedagogical" difficulty (as opposed to their "material" difficulty, i.e. the substantive difficulty of the issues) facing foundational studies, and offers as an adequate solution of this difficulty an exposition of mathematics which highlights the especially the "principal aspects"[cclxvii].

A similar formulation is found in the 1910 lectures, see the passage referred to in endnote cclxxiii. The focus on what Hilbert calls "principles" in the specific sense I just discussed, thus serves also the purpose of conveying such an acquaintance to his students, and, in this way, to serve as a preparatory exercise for foundational investigations.

§ 138 By focusing on select exemplary questions and issues, as Hilbert's 1908/9 seminar and his lecture courses on the quadrature of the circle do, they bear a certain kinship in ambition and design to Klein's lectures on elementary mathematics from a higher standpoint[cclxviii]. How much Hilbert knew of the content and disposition of Klein's lectures is hard to estimate; in any case, Klein first lectured on elementary mathematics in 1905[cclxix], a year after Hilbert lectured on the quadrature of the circle in 1904. So Hilbert and Klein seem to have developed their respective overview-lecture courses at least in parallel; and yet, when Hilbert lectured (twice more) on the quadrature of the circle after Klein's retirement, once in 1924/25 and again in 1928/29, Hilbert appears to be consciously taking up Klein's mantle, declaring in his opening statement that a survey course of elementary mathematics is of interest not just for teachers in training[cclxx], presumably a references to Klein's focus on the education of teachers (which but Klein, as I pointed out, had used as a point of departure, while Hilbert had not expressly mentioned the education of teachers in previous iterations of his lecture course on the quadrature of the circle). This sheet[201] also records a rather «Kleinian» subtitle for the course, namely: "(Elementarmath[ematik] nach höheren Methoden)".

§ 139 In all the lecture courses delivered between 1905-1915, Hilbert projects a complete treatment of the foundations of mathematics to be delivered at a later date: In 1908, Hilbert speaks of an integrated exposition of the entirety of mathematics as an idealised ultimate goal of the investigation of principles "on a logical monistic basis"[cclxxi].

The cryptic formulation of a, in some obscure sense, "monistic" mathematics also occurs in the 1910 lecture, where Hilbert seems to use it to stress the uniformity of mathematics as he sees it[cclxxii].

In the preparatory notes for the 1914/15 lecture course, Hilbert then explicitly promises "the systematic construction of the foundations of mathematics" for "a future lecture course"[cclxxiii]. That Hilbert explicitly defers the proper treatment of foundational issues to a later lecture indicates that he does not regard any of these lecture courses, themselves, as foundational in the same sense as, for example, his *Grundlagen der Geometrie* was, and the *Grundlagen der Mathematik* published later collaboratively with Bernays will, once again, be.

It is worth noting here, however, that while Hilbert throughout the 1910s projects a "systematic construction of

[201] This remark is found on a sheet (later) bound into the 1904 lecture-notes, seemingly arbitrarily, on p. 105.

the foundations of mathematics" to be presented in subsequent lecture course (see the passages quoted in endnote cclxxiii) in his opening remarks to his lectures on the quadrature of the circle, when Hilbert lectures again on the quadrature of the circle in 1924/25 and 1928/29, he does not, as one might expect, refer to his lectures on the logic and (finitist) proof theory as carrying out this project, but rather declares it impossible to give a full systematic exposition of the foundations of mathematics in any lecture course[cclxxiv].

I stress this aspect of Hilbert's lectures between 1905 and 1915 to point out that Hilbert's conception of foundational work is at times broader than one might think on the basis of both Hilbert's earlier, as well as his later work dedicated to foundational topics. One can, I think, distinguish three senses of foundational work Hilbert undertook at different stages of his career, ranging from a very narrow to an exceedingly wide conception: § 140

- The narrowest sense of foundational work is that projected by Hilbert on occasion in these lectures, which later becomes the focal point of his work in the 1920s, namely, the articulation and development of a particular viable system of basic assumptions on which the entirety of mathematics could, in theory, be constructed.

- The widest sense of "foundational" is that attested by the approach Hilbert takes in these lectures. In the specific (semi-technical) sense attributed here to the investigation of what Hilbert calls "principles", Hilbert regards as contributions to foundational work in this wide sense any kind of investigation of connections among mathematical ideas, where the concern is with "propellant ideas" (as Hilbert puts it in 1910, see endnote cclxxiii), rather than precisely stated axioms or theories.

In the abstract, the two poles between which I see Hilbert's foundational work moving – at times closer to one and at other times closer to the other – can, I think, be described as follows: There is, to one side, the investigation of mathematical ideas, which is what I called foundational work in the *widest* sense; on the opposite side, there is the investigation of mathematical theories by means of *precisely defined and limited deductive* techniques, which is what I called foundational work in the *narrowest* sense. Both of these projects of inquiry, I want to suggest, constitute foundational work as Hilbert conceives of it. Hilbert's early (geometrical) work, as I see it, represents a median between these two poles, whose contours are worth describing on its own terms:

- While Hilbert is concerned in his early geometrical work with the articulation and development of a particular geometrical axiom-system, and the investigation of its meta-theoretical properties, as would be the task of foundational work in the narrowest sense I just outlined, Hilbert is also, and no less concerned with the investigation of deductive connections between various geometrical results[202], some of which are far from basic and whose investigation is in certain cases indifferent to the particular geometrical axiom-system presupposed.

[202] Peckhaus once suggested distinguishing between what he calls an "axiomatic heuristic" in contrast to "the axiomatic method itself" (Peckhaus, "Hilberts Logik," p. 71), though he does not elaborate at any length what he takes the distinction to consist in. Still, it seems to me he may have had in mind a similar distinction as I mean to draw by speaking of foundational work in the narrowest sense ("the axiomatic method itself") in contradistinction to some wider sense ("axiomatic heuristic"). Vito Michele Abrusci. "Per una caratterizzazione del programma hilbertiano." In: *Rivista di filosofia* 66.3 (1975), pp. 315–338, p. 319 stresses in a similar vein that proving results within axiomatic theories is as important to foundational studies as devising new axiomatic theories.

Common to all these senses, and so the overall characteristic of foundational work, is the *focus on deductive connections*, whether looked at through the lens of a precisely defined deductive system, along the trails charted by meta-theoretical questions, or as encountered in the wild of mathematical landscapes. In each case, to concern oneself with the foundations of mathematics is to be concerned with the deductive connections between the various bits and pieces of mathematics. This concern can take the form either of investigating the organisation of mathematics as a whole or certain of its disciplines seen on a grand scale (as in what I just labelled the "widest sense" of foundational work), or of developing precisely defined (axiomatic) theories to the point of establishing in within such a framework certain key results – as identified by way of a foundational investigation in the widest sense of the relevant discipline – (in accordance with what I called the "narrowest sense" of foundational work), or of seeking to systematically survey various different but related axiomatic systems, investigating their meta-theoretical properties and comparing them among themselves (that is doing foundational work in what I called the "median sense").

In a word, the basic, constant sense of "foundational investigations", as Hilbert seems to be pursuing them in practice, is what I have called "systematisation" in section 3.1. The discussion in this section is meant to substantiate the further claim that Hilbert's work aimed at systematisation not just in its earliest stages, but also at least through the 1910s, and to articulate a sense (of foundational work "in the narrow sense") in which Hilbert's work of the 1920s can equally be understood to serve this same purpose[203].

§ 141 It seems to me the distinction between foundational work in the widest and in the narrowest sense, in particular, can be used to explicate the distinction sometimes drawn and often simply presupposed between "formal" and "informal" arguments and theories: Insofar as any clear sense can be made of this distinction, it reduces to the distinction between, on the one hand, working within a framework where the available deductive resources are precisely (and recursively) defined and, on the other hand, working within a framework where the standards for admissibility of argumentative patterns and assumptions remain indefinite and flexible.

A similar explication of the distinction was suggested by Lakatos, who remarked that

> in an informal theory there really are unlimited possibilities for introducing more and more terms, more and more hitherto hidden axioms, more and more hitherto hidden rules in the form of new so-called "obvious" insights, in a formalized theory imagination is tied down to a poor recursive set of axioms and some scanty rules.[204]

I want to stress, as Lakatos also already rightly stressed[205], that one ought not to conceive of «informal» arguments

[203] Like Ewald, "Hilbert's Wide Program," p. 248 I am thus attributing to Hilbert a "wide program", which I am, however, inclined to describe not in terms of "the investigation of the logical scaffolding of axiomatic theories" (p. 244), but rather in terms of "the systematisation of knowledge" (both framed in axiomatic theories, but also, and just as crucially, investigated independently of any particular axiomatisation). That investigating deductive connections not only in particular axiom-systems but also independently of axiomatisation plays a central role in Hilbert's overall project has also been stressed by Hallett, "Hilbert and Logic," p. 136: "[Hilbert] is clear that an axiomatic system does not have its origin *ex nihilo*. It begins, rather, with an assemblage of what he calls 'facts' [Tatsachen], more or less established; the task then is to investigate what follows from what, and what is dispensable and what essential."

[204] Imre Lakatos. "What does a mathematical proof prove?" In: *Mathematics, Science and Epistemology. Philosophical Papers Volume 2*. 1978, pp. 61–69, p. 68.

[205] "But to call this sort of proof an informal proof is a misnomer and a misleading one. It may perhaps be called a quasi-formal proof or a 'formal proof with gaps' but to suggest that an informal proof is just an incomplete formal proof seems to me to [make; the original mistakenly prints the ungrammatical phrase 'to be to make'] the same mistake as early educationalists did, when, assuming that a child was merely a miniature grown-up, they neglected the direct study of child-behaviour in favour of theorizing based on simple analogy with adult behaviour." (Lakatos, "What does a mathematical proof prove?" p. 63).

and theories as «degenerate» versions of «formal» arguments or theories, but that just as one can give a positive account of precisely defined deductive systems, one ought to study and give precise *positive* accounts of how «informal» arguments and theories function and evolve. I also concur with the implication of Lakatos' remark that "informal" is a misnomer that should be retired (and "formal" along with it).

I propose to speak instead of "definitively regimented theories (in deductive systems that have been stated explicitly)" instead of "formal" and of "open-standard theories" instead of "informal"; the idea underlying this proposal is, as I said, that the essential distinguishing mark is whether what qualifies as a full and valid argument is explicitly fixed once and for all, or left open, potentially allowing for an extension of the standards to cover further arguments at any given point. The terminology I suggest also underscores that there is really no exhaustive distinction here, because many different kinds of standards can be distinguished, like so called "open" guitar-tunings.

The distinction I drew between different senses of foundational work, especially what I called "foundational work in the widest sense", i.e. the concern with particular ideas which are, in some sense, «central» to entire disciplines, can, I think, also throw some light on the conception underlying Hilbert's talk of completeness before 1920. § 142

3.2.3.1 Hilbert's Early Conception of Completeness

From very early on in his work, Hilbert stresses, whenever a suitable opportunity presents itself, that axiom systems ought to be complete: He does so in the introductory remarks to the *Grundlagen der Geometrie*, which I quoted in full earlier (on page 49), where he describes his undertaking as the search for a "complete system of axioms"[cxxxii]. In his description of the axiomatic method in general terms in his programmatic article *Über den Zahlbegriff*, Hilbert speaks of "the necessary task of demonstrating the *consistency* and *completeness* of the axioms"[cclxxv], and further characterises the latter task as showing "that the system of axioms suffices for the proof of all propositions [belonging to the relevant discipline]"[cclxxv]. Likewise in the 1905 lecture course, Hilbert stresses that ideally an axiom-system is "an architectonic structure [where] all phenomena of the relevant domain find a place"[cclxxvi].

More precise formulations of what Hilbert means by requiring, or asserting, the completeness of a given axiom system are hard to find; indeed, in his early work Hilbert does not have more to offer than this seemingly vague criterion, that in order to be complete an axiom system must supply means to derive the discipline in question in its entirety, that is to say, to derive all results falling within the field of the discipline; and while Hilbert often and at length discusses the other two of the three big meta-theoretical questions he recognises in his early work – independence (of individual axioms and results from one another) and consistency (of entire axiom-systems) –, he never discusses at any length the question whether an axiom-system he is dealing with is complete[206].

The second of the two formulations of a criterion for completeness I just gave – the one characterising completeness by the derivability of all results in the relevant discipline – is somewhat more definite than the first but still

[206] An example of this can be found in the discussion of Peano's axiom-system for the natural numbers Hilbert offers as part of his 1917 lecture on set-theory. There, Hilbert asserts that the axioms he sets out are, in fact, independent, consistent and complete[cclxxvii]. Detailed independence proofs follow, as well as a lengthy discussion of consistency[cclxxviii], but completeness is never mentioned again.

leaves wide open what, exactly, completeness requires; in particular, whether Hilbert has in mind the derivability only of all *presently known* results, or also of *true* results presently unknown (and if the latter, how one could possibly establish the completeness of an axiom system with respect to results not known to be true)[207].

§ 143 It seems to me Hilbert's concern with foundational work in what I called the "widest sense", i.e. his concern with finding (singular) «central ideas» of given disciplines, lends itself to further spelling out what Hilbert may have had in mind when invoking as a criterion for completeness that one must be able to derive the discipline in its entirety from a complete set of axioms. Having identified certain results as «central» to a given discipline allows one to re-cast Hilbert's vague criterion of completeness in the more definite form: A theory qualifies as complete just in case these (select few) «central» results can be derived in it. Since these have been stated explicitly, and are relatively few in number, re-casting the question of completeness in this fashion makes it vastly easier to address[208].

§ 144 This proposal is more than just a speculative development stitching together disparate remarks. I believe it can help explain why Hilbert saw himself justified in making the occasional completeness claims he explicitly makes (though, as I said, Hilbert does not himself lay out at any length his reasons for thinking them justified).

So, for example, in the 1914/15 lecture course, as recorded in the lecture notes held by the Institute of Advanced Study in Princeton: Hilbert comments on his axiom system for geometry that "one can reconstruct Euclidean geometry in its entirety using these axioms"[cclxxix]. Hilbert goes on to discuss the consistency and independence of his axioms as the "second" and "third" question one can ask about axiom systems[cclxxx], which suggests that his remark is to be understood as a declaration that, in Hilbert's view, his system of axioms is, indeed, complete; in fact, Hilbert refers here to his *Grundlagen der Geometrie* as *containing* the *full* development of geometry in its entirety. The reformulation of Hilbert's criterion of completeness in terms of «central» results can help make sense of this confident self-description. In his *Grundlagen der Geometrie* Hilbert deduces from his axioms certain results he deems to be of central importance – e.g. Pascal's Theorem in section 14, Desargues' Theorem in section 22 –, and even stresses that one could turn the deductive structure thus laid out on its head, so to speak, and develop (large portions of) geometry in its entirety on the basis of such «central» results taken as axioms instead of Hilbert's own axioms[209]. Given that the *Grundlagen der Geometrie* proceed in this way and succeed in deducing from the axioms

[207] The rather indefinite nature of Hilbert's early conception of completeness has been noted, e.g. by Erich H. Reck and Steve Awodey. "Completeness and Categoricity. Part I: Nineteenth-century Axiomatics to Twentieth-century Metalogic." In: *History and Philosophy of Logic* 23 (2002), pp. 1–30, p. 11, who observe that "it never becomes entirely clear whether [Hilbert] means [what Awodey & Reck call 'relative completeness'] or [categoricity] by the intended 'completeness' of his system of axioms.".

[208] That Hilbert's conception of completeness (before 1918) turns on a comparison of the theorems derivable within a narrowly regimented theory and those established in an open-standard setting has been recognised clearly by Abrusci, "Per una caratterizzazione del programma hilbertiano," pp. 316 sq., who emphasised that completeness for Hilbert was a matter of deriving "tutte le proposizioni conosciute della teoria e non necessariamente tutte le proposizioni conoscibili di essa".

Reck and Awodey, "Completeness and Categoricity," p. 9 also recognise a conception of completeness like this, which they call "relative completeness" (their definition 5 on p. 4), and credit Wilfried Sieg (in footnote 10) with pointing out to them "the historical importance of relative completeness, especially in connection with Hilbert".

What has, so far as I am aware, not been recognised so far is that and how Hilbert's concern in the years between 1904 and 1915 with the identification of «central» results may have been linked in Hilbert's mind also with his conception of completeness.

[209] In section 30 of the *Grundlagen der Geometrie*, Hilbert establishes (as proposition 35) that Desargues' Theorem can, on its own, be used as a replacement of the entire group of spatial axioms[cclxxxi]. This claim constitutes a completeness result, of sorts, in the spirit of the conception of completeness I have been outlining: that the derivability of select few «central» results suffices for the derivability of all theorems (of a certain general character).

Hilbert formulates several «central» results of geometry (identified as such in an open-standard setting), Hilbert can assert in 1914/15 – with his more developed views about «central» results in mind – that his *Grundlagen der Geometrie* contain a complete development of geometry *because* he had succeeded in deriving what he saw in light of reflection upon the deductive connections of geometry investigated in an informal setting as the «central» results.

So, too, in the 1917 lectures on set-theory, whose (lack of) discussion of completeness I noted in footnote 206: The closest Hilbert comes to offering something that can be construed as an attempt of a completeness proof for the axioms for natural numbers he presents there is at the start of the section 8 of chapter IV, where Hilbert sets out to show that "the axiomatically postulated whole numbers are the numbers we are familiar with"[cclxxxii]. If this really is Hilbert's discussion of completeness, as the pages prior had contained his discussion of independence and consistency, this suggests that completeness, as Hilbert conceives of it at this point, is a question of establishing a few results previously recognised as «central» to the discipline at hand as seen in an informal setting.

Indeed, it seems to me that Hilbert's remarks in the 1914/15 and 1917 lectures support the hypothesis that Hilbert's concern with «central» results not only *can be* but in fact *was connected* also in Hilbert's own mind with the issue of addressing and making more tractable the question of completeness for axiom systems.

§ 145 In the preparatory notes for his 1913 lecture course Hilbert even claims that every mathematical discipline has a *single, unique* «central» result and that axiomatisation constitutes, as it were, a "logical factorisation" of this central result into its elements[cclxxxiii]; this algebraic metaphor recalls Hilbert's earlier suggestion that proofs can be regarded as «factors» (in some sense) of the propositions they establish (which I discussed earlier in § 121). The *other factor* of any given result, beside its proof, is already there identified with the axioms on which the result rests. However this metaphor is to be understood, exactly, it suggests that the main purpose of any given axiom-system, as Hilbert sees it, consists in the derivation of a single (!) «*central*» result.

Although not explicitly stated in general terms, Hilbert also seems to presuppose the idea that for any given discipline there is a *single* «central» result in 1917 when delivering the address entitled "Axiomatisches Denken", where Hilbert speaks of "prevalent propositions" basic to the construction of theories. Hilbert's examples reveal that he apparently has in mind not a plurality of axioms but rather a *single central result* for any given theory; e.g. for geometry "the proposition of the linearity of an equation in the plane and of the orthogonal transformation of the coordinates of points"[cclxxxiv]. Hilbert does not discuss why this particular result should be regarded as central for geometry. Indeed, as far as I can see, Hilbert never discusses the question how one would decide which result(s) to regard as central for any given discipline either in general terms or with respect to a particular case. The fact that Hilbert here adduces a number of central results without comment suggests, however, that he thought, even if there was no general uniform method for identifying central results of given disciplines, any competent mathematician would be able to recognise the centrality of certain results somehow in each particular case.

§ 146 I want to note in passing that, far from a new awakening of interest in foundational work in Hilbert's mind, after

years of focusing on other matters, "Axiomatisches Denken" thus seems to constitute a condensation of Hilbert's preoccupation with "principles" in his foundational lectures during the 1910s.

Even what is often cited as the central innovative idea of this address, that one can distinguish "strata of axioms" located at varying depth, already appears (earlier) in the third volume of Hilbert's so called "Wissenschaftlichem Tagebuch", where Hilbert already speaks of "strata of axioms" ("Axiomschichten")[cclxxxv], and even, as in "Axiomatisches Denken" of "deeper" and "higher strata of axioms"[cclxxxvi] (though the dating of its entries is uncertain, as discussed at length in section 3.3.1, these entries date from the early 1910s at the latest). I will cover some further examples of earlier reflections incorporated into "Axiomatisches Denken" in § 180.

At the bottom line, I take this connection between Hilbert's sketchy account of completeness in his early work and § 147
the concern with what Hilbert calls "principles" of various mathematical disciplines to underscore the importance within Hilbert's overarching conception of foundational work of what I called "foundational work in the widest sense": Without foundational work in the widest sense, that is to say, without identifying what Hilbert in 1910 calls "principles", i.e. the central ideas of (particular) mathematic(al discipline)s, one would not know which assumptions to enshrine as axioms[210]; and without having proposed particular axiomatisations, one lacks the requisite ideas for systematising a discipline. Referring back to the epigraph of the *Grundlagen der Geometrie*, one might describe Hilbert's overall approach as follows: All mathematical knowledge begins with taking in the sights, i.e. gaining a serviceable overview of a given discipline («intuition»), then proceeds to enshrining various assumptions sufficient to yield certain central results in axiom systems («concepts»), and finally investigating and comparing axiomatisations and thus systematising the knowledge of the discipline acquired hitherto («ideas»).

To appreciate that this conception of completeness is no less viable than the one nowadays usually adopted[211],

[210] In the abstract this same point has, I take it, already been made by Lakatos, who wrote "we should speak of formal systems only if they are formalizations of established informal mathematical theories. [...] There is indeed no respectable formal theory which does not have in some way or another a respectable informal ancestor." (Lakatos, "What does a mathematical proof prove?" p. 62).

[211] That Hilbert's early conception of completeness is somehow deficient is implicit, e.g., in the article by Awodey & Reck, who consider the concept of so called "relative completeness", which they attribute to Hilbert, of interest mainly "[f]or historical purposes" (Reck and Awodey, "Completeness and Categoricity," p. 4). Mancosu writes that in Hilbert's foundational work prior to the 1920s "[t]he meaning of completeness at this stage is still vague" (Mancosu, *The Adventure of Reason*, p. 127).

In the same vein, Richard Zach's claim that in Bernays' Habilitationsschrift "completeness is formulated as a precise mathematical question to be answered for a system of axioms" after "Hilbert had formulated completeness as the [imprecise] question of whether the axioms suffice to prove all 'facts' of the theory in question" (Zach, "Completeness before Post," p. 340).

Zach furthermore asserts that Hilbert's later conception of completeness, which he credits Bernays with formulating clearly for the first time, "arises directly out of the completeness axioms of Hilbert's earlier axiomatic system". I am inclined to agree, to the contrary, with Awodey & Reck, who observe that there is no straightforward path leading from Hilbert's earlier axiom of completeness to his later conception of completeness as a meta-theoretical property given that "the Axioms of Line Completeness itself mentions points in a geometric space, not formulas in the corresponding language" (Reck and Awodey, "Completeness and Categoricity," p. 12; a similar cautionary note is sounded by Ulrich Majer. "Husserl and Hilbert on Completeness. A Neglected Chapter in Early Twentieth Century Foundations of Mathematics." In: *Synthese* 110 (1997), pp. 37–56, p. 52). Zach re-affirms his view in "The Development of Mathematical Logic from Russell to Tarski, 1900-1935," though he acknowledges there (in footnote 58, which contains a reference to the article by Awodey & Reck, that "[t]his connection [...] is tenuous" (the footnotes to the article appear as endnotes in Mancosu, *The Adventure of Reason*, p. 497)). The two strongest reasons against seeing any close connection between Hilbert's completeness-axiom and his later conception of completeness as a meta-theoretical property are, in my view: first, the one emphasised by Awodey & Reck, Majer and, not least of all, Hilbert himself[cclxxxvii], i.e. that the axiom concerns domains of models for theories while the meta-theoretical property is one theories, not their models, may have, or fail to have, and the requisite transference to "a theorem | *about* the system instead of an axiom *in* the system", as Zach nonchalantly puts it (Mancosu, Zach, and Badesa, "The Development of Mathematical Logic from Russell to Tarski, 1900-1935," pp. 54 sq.), is not, to my mind, trivial; second, if Hilbert really were somehow developing his later conception of completeness as a meta-theoretical property out of his earlier axiom, then the question arises, why Hilbert sees having the meta-theoretical property as a fact about theories *to be proved*, rather than axiomatically postulated. It seems more plausible to me, where Hilbert's personal development is concerned, to see the meta-theoretical completeness of logic as the fulfillment of Hilbert's early

I believe it will be useful to recall and discuss the idea of completeness at some length in the abstract.

Interlude on Completeness and Maximality

§ 148 As I see it, three main – and different! – uses of the word "completeness" can be distinguished in mathematical logic as it has developed since the time of Hilbert:

- The word "completeness" is used to record the fact that every consequence one can derive by some indefinite means of argument can also be derived within a precisely defined deductive system.

  In that event, the deductive system is called "complete". One ought, strictly speaking, to add that the deductive system is called "complete" *relative to* a certain account of indefinite consequence, but because model-theoretical accounts of consequence have become accepted as the default, no such specification is usually made and it is simply understood that the relevant «standard» are consequences in the sense of model-theory; the concept of a model-theoretic consequence is indefinite not in the sense that it is not precisely defined, but that it does not definitively fix means of argument which are admissible for deriving consequences. Completeness in this sense is thus a question of whether fixed means of arguments can be definitively stated by means of which all sentences which are taken to «follow from» certain assumptions (e.g. because they are bound to be true, or satisfied, whenever the assumptions are) can be derived from these assumptions by the selection of narrowly regimented means of argument available within a particular deductive system.

  Since "completeness" in this usage is a matter of whether, or not, every consequence deducible in the reference account of consequence is also deducible in a precisely defined deductive system, I will speak of "deductive completeness" when I intend to invoke specifically this usage of the word.

- The word "completeness" is used to record the fact that every sentence which is true in some open-ended sense is deducible within a precisely defined theory. If it is so, the theory is called "complete".

  One ought, again, to add, strictly speaking, that the theory is called "complete" *relative to* both (a) a certain informal account of truth and (b) a particular selection of truths, which the theory is «expected» to account for. But, again, because the model-theoretic account of truth has become accepted as the default, no explicit specification of an account of truth is usually made and it is simply understood that the relevant «standard» are the truths of a given model in the sense of model-theory.

  Since "completeness" in this usage is a matter of whether, or not, every truth about a particular structure is «captured» by the theory in question, or, speaking more loosely, whether the theory «adequately describes» the structure in question, I will speak of "descriptive completeness" (relative to a particular structure) when I intend to invoke specifically this usage of the word.

ambition to somehow prove that, in mathematics, there "can be no ignorabimus", which, indeed, Hilbert did come to see as something his proof-theory enabled him to prove (see section 3.3.5.1 for discussion).

The two uses of the word "completeness" are similar, but distinct; and both play key roles in meta-logical investigations: deductive completeness is the concept at issue in the result known as "(Gödel's) completeness theorem", while descriptive completeness is the concept at issue in the result known as "(Gödel's) incompleteness theorem".

Both uses of "completeness" have in common that either one turns on the answer to what looks, in the abstract, to be the same central question: whether a precisely defined concept – consequence in a logical deductive system and theoremhood in a theory – «completely recapture» some presupposed (indefinite) «standard» conception[212].

Meaningfully posing a completeness-question thus requires having at least one precisely defined deductive system at hand, whose completeness is at issue; this «standard» need not, itself, be framed within a definitive deductive system, though it can be (e.g. one can simply ask whether one precisely defined theory completely recaptures the theorems of another); the customary model-theoretic frameworks are what I would call "semi-open": A precise definition of truth is given, but usually on the basis of an account of truth for atomic sentences, of which no definitive account is given; it is simply presumed that one knows which atomic sentences to be true.

In both cases, the question turns, more specifically still, on whether a certain class of sentences, whose elements are determined by indefinite means – a notion of consequence which, like that of a model-theoretic consequence, does not fix particular means of argumentation, or a notion of truth which, like that of truth in a structure, presumes the base truths to be given –, is precisely the same class – of consequences derivable in a precisely and recursively defined deductive system from given assumption or theorems provable (in a deductive system) from precisely and recursively defined descriptive axioms – of sentences as can be derived by precisely defined means of inference.

The two uses of "completeness" I have listed differ with respect to their range of applicability:

The first applies to deductive systems (i.e. means of drawing consequences from arbitrary sets of assumptions), the second applies to deductive theories (i.e. means of establishing theorems *by means of a particular, fixed deductive system* on the basis, in addition, of particular, *fixed* assumptions); technically speaking, a deductive system is characterised by its «logical» axioms and inference rules, while a deductive theory is characterised by the underlying deductive system *and* its peculiar «non-logical», or as I called them "descriptive" axioms.

Seeing how the relevant «standards» are usually defined within (first-order) model-theory, these two uses of the word "completeness" differ furthermore because the descriptive completeness invokes only a single model of particular interest to the investigation at hand (such as the standard model of arithmetic), while deductive completeness invokes (by way of the concept of a model-theoretic consequence) *all* suitable models.

- The word "completeness" is also used (though not as universally) to record the fact that a given theory has

[212] Awodey & Reck also observe that "the main ingredient" of formulating question concerning completeness "in a clear and fruitful way" is "a precise and purely formal notion of deductive consequence" (Reck and Awodey, "Completeness and Categoricity," p. 19). I want to add to this quote, however, that in my opinion the qualification that it must be a "*purely formal* notion" adds nothing of substance to the diagnosis. The key issue to my mind is whether deductive consequence is defined *precisely*; indeed, when Awodey & Reck summarise their discussion up until that point once more in footnote 60 (on the same page), they themselves focus only on *precision*, no longer mentioning "formality". As a matter of fact, whether the concept of deductive consequence at issue is defined independently of a particular interpretation – if that's what "purely formal" means –, seems irrelevant, indeed, counterproductive: For completeness-questions to arise and to be answered one must compare a deductive system or theory with particular interpretations.

> for any sentence in the relevant language as a theorem either that sentence or its negation (but not both).
>
> I will not use the word "completeness" in this connection at all. I will speak, rather, of a theory being "maximal (while consistent)" in case it has, for every sentence in its language, either the sentence or its negation (but not both) as theorems; the associate meta-theoretical property I will call "maximality" ("maximal-consistency").

My motivation for not using the word "completeness" in this connection is that the word "completeness", even if one restricts the word "completeness" to the first two usages I listed – I take these two senses to be the core usages of the word –, is already used ambiguously, and one is hence ill-advised to use the same word in yet another, substantively different way; and "maximal-consistent" is already in use in this sense.

My reason for not applying the word to this particular fact is its remoteness from the other uses of the word in one, in my view, key aspect: Unlike the previous two uses of the word "completeness" I listed, maximal-consistency is not *relative* to a «standard» presupposed in the background. There is nothing about which one can ask whether it is «completely recaptured» by the theory in question; in other words, there are no means of determining the relevant class of sentences *independently of the deductive theory* whose maximality is at issue[213].

But, whether one feels inclined to use the word "completeness" for sets of sentences which are maximal in a given language (while consistent) is naturally a question of terminological preference only.

However, I believe there is also a substantive point at issue here, which I will now try to bring into focus. § 149

As the «standards» for descriptive completeness have come to be construed (within «classical» (first-order) model-theory in the vein of Tarski's definition of satisfaction in a structure), maximal-consistency has come to be seen as «part and parcel» of descriptive completeness. The class of sentences true in a particular structure is maximal while consistent on account of the definition of satisfaction and truth within a Tarskian framework.

One ought to recognise, first of all, that this development took place after Hilbert's time, and rests on certain assumptions about *what* precisely defined deductive frameworks and theories *are for*. In accepting maximal-consistent classes as a «standard» one ties the importance of precisely defined deductive theories to their *exhaustiveness*[214].

Whether one inclines towards this view of the purpose of precisely defined theory or not, one must, I think, recognise that in accepting it one is making a particular choice about what precisely defined deductive systems and theories are for, and what they are meant to accomplish[215].

[213] A similar conception of the root "notion" from which the various precise concepts of completeness stem was articulated by Georg Kreisel in his early article Georg Kreisel. "On the concepts of completeness and interpretation of formal systems." In: *Fundamenta mathematicae* 39 (1953), pp. 103–127, pp. 119 sq.: "[Maximal-consistency is] a rather sophisticated development of a much vaguer notion of completeness, which is concerned with | the complete characterisation of some notion by a formal system.".
Kreisel's article also offers what may be a helpful terminological distinction between completeness as an "internal property" (p. 119) of a system and completeness as the property "of an interpretation" (p. 120), i.e. of how two systems relate to one another; Kreisel himself focuses mainly on the latter; see footnote 215 for some further discussion of Kreisel's conception of completeness.

[214] That such exhaustiveness is furthermore characteristic also for the *meaningfulness* of sentences and that one can therefore identify model-theoretic concepts with «semantic» concepts and the question of completeness with the question of «matching syntax and semantics» is another assumption – this one about the «nature» of meaningfulness –, whose contentiousness seems to me underappreciated.

[215] I take this to also be the position underlying Kreisel's article mentioned in footnote 213: Kreisel expresses the underlying idea of the general conception of interpretation he develops in that article as "study[ing] a *given* formal system $\mathfrak{F}$ by getting 'information' about a familiar system F from proofs in $\mathfrak{F}$" (Kreisel, "On the concepts of completeness and interpretation of formal systems," p. 104). Two aspects of Kreisel deserve special emphasis: (I) Kreisel does not so much as mention the idea that any of the systems in question in some

This choice can be made in various different ways, depending on one's aims and ambitions. It seems to me, in any case, Hilbert's early conception of completeness is one example of a different, but in being different no less sensible answer to this question, given Hilbert's ambitions at the time.

If the axiomatic method served mainly the purpose of «systematising» known mathematical developments – as I have argued it did for Hilbert at the very least up until 1918 – in the double sense of (a) bringing to light the conceptual (de)composition of the notions involved (into particular primitives) and the deductive stratification of entire (sub)disciplines, and (b) simplifying the state of things thus brought to light both for the practitioner and the learner, and cannot proceed without at the same time practising mathematics and studying it in an open setting, one has no reason to expect of any axiom system a means for deciding also *all further* questions beyond those already decided in an informal setting; axiomatisation is meant to supply only a precise reconstruction and systematisation of a certain portion of the deductive structure of preconceived mathematics. Deriving results previously recognised as «central» spells success, exhaustiveness of all true claims is neither required, nor desired[216].

In light of this conception of the purpose precisely defined deductive theories serve, accepting an exhaustive «standard» for descriptive completeness seems decidedly implausible[217]; and it certainly seems implausible to measure Hilbert's claims about completeness by such a standard, which, I have argued, presupposes a different conception of the purpose formulating definitively regimented axiomatic theories ultimately serves.

To accept as the «standard» account of truth one where maximal-consistency is «built in», takes the purpose of the development of axioms systems to be something other than «factorising» certain «central» known results, i.e. of rendering explicit the assumptions on which these results depend and the means and ways by which the results are derived from these assumptions. Accepting a maximal-consistent class as the «standard» takes the purpose of developing axiom systems, rather, to consist in supplying some manageable means of deduction which allow one to *exhaust* the class not only of all *known* results, but furthermore of yet unknown results.

I want to note that Hilbert's ambitions arguably shift over time from systematising and reconstructing mathematical facts which are taken for granted as substantively unproblematic in order to investigate and refine their formulation and deductive dependencies to justifying certain portions of mathematics by providing for axiomatisations of them consistency proofs relying only on very limited ("finitist") means of argument. § 150

I believe that Hilbert's early conception of completeness is adequate for his purposes *at the time*, which I take to

sense *exhaust* everything there is to be known about a particular (sub-)discipline (of mathematics); indeed, he rejects the claim that his conception somehow exhausts what might possibly constitute an interpretation as spurious in his footnote 5. (II) Interpretations are relations between two theories; Kreisel does (I think: rightly) not accord a special status to the system $\mathfrak{F}$ *in which* another is interpreted and whose properties the interpretation of a (usually more easily manageable) system in it is meant to serve as an aide to studying.

[216] Michael Detlefsen. "Completeness and the Ends of Axiomatization." In: *Interpreting Gödel*. Ed. by Juliette Cara Kennedy. Cambridge University Press, 2014, pp. 59–77, p. 61 discusses the same two conceptions of what axiomatic theories are for under the name "descriptive completeness" (aiming "to bring logical organization to a presumed body of 'facts'") and what he calls "Gödel-completeness" (maximal consistency). Detlefsen makes about Gödel in comparison with Russell the same point I am making about the model-theoretic conception of completeness in comparison to Hilbert's early conception of completeness, and provides some historical background; in p. 73fn18 Detlefsen voices reservation about Hilbert's acceptance of maximal consistency as a goal even in the 1930s.

[217] Detlefsen, "Completeness and the Ends of Axiomatization," p. 77 likewise comes to this conclusion that maximal consistency (his "Gödel-completeness") neither is itself "a compelling ideal" nor a requirement for any of the ideals Detlefsen identifies as compelling.

be limited to systematisation (and where the issue of consistency arises, Hilbert is content to resolve it by exhibiting models (usually based on arithmetic)), and that one could, in principle, be content pursuing only such ambitions. The same conception of completeness is no longer adequate for Hilbert's later purposes because, as Poincaré observed in 1905, in order to justify the acceptability of mathematical concepts by means of a consistency-proof for an axiomatisation of them, one must be sure that, when giving the syntactic consistency proof, one is considering *all consequences* anyone can ever, in principle, derive about the objects in question, not just some of them[218]. At that point, when Hilbert's ambitions shift (from 1920 onwards) in the direction of proof-theoretic consistency proofs, Hilbert's early conception of completeness ceases to be adequate to his new and changed ambitions.

Gödel's incompleteness result in 1931 thus jeopardises Hilbert's *later* ambitions, though how seriously depends on the exact characterisation of Hilbert's ambitions and, as Gödel himself noted, on what one takes to be the epistemologically acceptable means to be used within proof-theory for the purpose of justifying various portions of mathematics[cclxxxviii]; in a way, Gödel's results thus bear out on a technical level what Poincaré claimed in 1905 for philosophical reasons, that for every axiom-system one must admit "a new act of intuition"[cclxxxix].

§ 151 While the decidability of every mathematical question is an ideal close to Hilbert's heart, there is, in my view, no evidence to suggest that Hilbert regarded precisely defined deductive systems as the main means to reach this goal. Heinrich Behmann presented the idea that precisely defined deductive systems can, and ought to, serve the purpose of providing means for deciding as of yet undecided questions as a new and revolutionary idea in 1922[ccxc].

Behmann does not credit Hilbert with playing any part in raising the decision problem in general terms,[219] neither in his 1922 article "Beiträge zur Algebra der Logik, insbesondere zum Entscheidungsproblem" – where Behmann emphasises the pioneering efforts of Schröder instead (although Behmann is apparently aware (see footnote 166) of Hilbert's work, up to and including Hilbert's 1917/18 lectures on logic) –, nor in § 49 of his 1918 dissertation, where Behmann first seems to grasp the kernel of what he would go on to call the "decision problem"[ccxci].

§ 152 On the other hand, for *deductive* completeness an «exhaustive standard» *is* plausible: It is a desirable feature *of a deductive system* that it can, in principle, deliver *any and every consequence* derivable from given assumptions[220].

§ 153 A seeming downside of Hilbert's conception of (in Reck & Awodey's terminology: "relative") completeness, as I proposed to understand it, is that it renders (descriptive) completeness, so to speak, «fragmentary»; that is to say,

[218] Henri Poincaré. "Les mathématiques et la logique [I]." in: *Revue de métaphysique et de morale* 13 (1905), pp. 815–835, p. 834 says: "Il ne suffit pas de comparer *certains* conséquences du principe [Poincaré is talking specifically about the principle of arithmetical induction but making, I take it, a general point], il faudrait les comparer *toutes*.".

[219] Richard Zach maintains that "[t]he origin of the decision problem in Hilbert's work is no doubt his conviction [...] that every mathematical problem has a solution" (Mancosu, Zach, and Badesa, "The Development of Mathematical Logic from Russell to Tarski, 1900-1935," p. 62). This seems rather doubtful to me – at any rate, far from certain – because I see no reason to think that when Hilbert asserted, as Zach quotes him saying, "that there is no *ignorabimus*", he meant to assert the existence of a *uniform* method to find a solution to any given problem; rather, I take Hilbert to voice his conviction that *for any given problem there exists a method* by means of which it can be solved or proved unsolvable, which is not only a different thesis, but also in no obvious way suggests the decision problem as Behmann later formulated it, i.e. the problem whether *there exists* a *single*, uniform method to solve *all* problems.

[220] In section 48 of Carnap's *Logische Syntax der Sprache* Carnap points out that its maximality is a significant advantage – rendering it a suitable «reference» – of the concept Carnap calls "Folge" (comparable to the model-theoretic concept of consequence)[ccxcii], which Carnap had defined earlier in the book (in section 14) as a stronger counterpart to what he called "derivation" ("Ableitung")[ccxciii], which is roughly what I have been calling "deducibility in a precisely defined deductive system".

one cannot formulate a general criterion for completeness, since for any given theory (at any given point in time) the derivation of different «central» results may be required in order for it to qualify as complete.

Note, however, that one is not in an altogether different position when accepting the model-theoretic framework: Although descriptive completeness relative to a structure can be defined uniformly and in general terms by the criterion that the set of theorems of the theory must be identical to the set of truths in the structure, the apparent generality of this definition is, so to speak, «hollow»: *Which particular atomic sentences* are true also differs from structure to structure and is usually, as I already noted in footnote 212, presupposed to be «intuitively clear».

That this presupposition is contrary to the spirit of a precise theory, and that the ultimate philosophical challenge lies in stating precise and, most importantly, practical (few, conspicuous, striking,...) principles – an at most recursively defined (if not finite) class of axioms – by means of which one can decide the truth of *atomic* sentences, first occurred to me when I read the concluding chapter of Davidson's *Truth and Predication* for the first time in 2016. I have since been unable to pinpoint the place which brought this idea to my mind (and so have no grounds for attributing it to Davidson, even though it *feels to me* like a distinctively «Davidsonian» idea). The closest to a passage where Davidson can be construed as making something like this point I have been able to find is Davidson's distinction of "axioms of two sorts" involved in Tarski's theory of truth:

> First, there is an axiom for each sentence with an unstructured predicate (its spaces filled with variables or names) specifying the conditions under which that sentence is satisfied by a particular sequence. There will be a finite number of such axioms, since the basic vocabulary of any language must be finite. Second, there will be axioms recursively characterizing the satisfaction conditions of sentences built up from simpler sentences by the operations of negation, alternation, and the other sentential connectives, and, of course, the quantifiers.[221]

§ 154 Davidson does not say much about the first sort of axioms he mentions here (or in any other passage I have been able to locate), beyond claiming that they are integral to any satisfactory Tarskian account of truth—and hence, in Davidson's view, meaning, so far as one can sensibly speak of "meaning". Davidson's remark, as I have come to understand it, projects a task whose importance, and difficulty, can hardly be underestimated: At the basis of a satisfactory account of meaning (along the lines Davidson envisions) for any language must lie an axiomatisation of the primitive expressions in the language which is simple and conspicuous enough to be practically useful for settling the question of the truth of atomic sentences (by deducing them, or their negations, from these axioms)[222].

This view of meaning strikes me, upon reflection, as perfectly suited to make sense of the claim Hilbert appears (at least early on) disposed to espouse: that axioms governing the primitive expressions in a language serve, somehow, to determine «the meaning» of these expressions. The view I am attributing to Davidson – although it may not be Davidson's view, but merely inspired by his work – shows *how* axioms may do so. They key idea, as I mentioned in abstract terms already in §§ 36 and 68, lies in *rethinking one's conception of meaning*: Determining the meaning

[221] Donald Davidson. *Truth and Predication*. Harvard University Press, 2005, p. 160.

[222] Though the practicality of the system of axioms of the first sort remains largely implicit in the above passage, I take it that this is what Davidson's requirement of a *finite* "basic vocabulary" recalls by way of recalling Davidson's stance on the requirements for a language to be learnable (as presented in: Donald Davidson. "Theories of Meaning and Learnable Languages." In: *Inquiries into Truth and Interpretation*. Second Edition. Oxford University Press, 2001, pp. 3–15).

of an expression, according to the view I am proposing (on the basis of Davidson's work) here, does not require, as Frege held, associating expressions with uniquely identified objects as "semantic values" (which they either name, or have as truth-values) but to *articulate explicitly certain core assumptions guiding their use* – formulating certain paradigmatic sentences containing these expressions as essential components –, in light of which one can settle the question of truth for a significant number of sentences involving the expression at issue.

Understanding the meaning – in a slogan – consists in systematically understanding the objects themselves.

§ 155 If one takes seriously the idea Hilbert's work seems to lend expression to, that axioms can somehow fix the meaning of the terms so axiomatised, one is, it seems to me, at the same time bound to abandon the idea of the exhaustiveness of meaning (in the sense I mentioned in footnote 214). The main, and, indeed (it seems to me) decisive reason lies in the incomplete and incompleteable nature of axiom systems. If the meaning conferred upon a primitive, sub-sentential expression (term or predicate) by an axiom system either consists in or decisively depends on whether the axiom system settled certain sentences in which the term or predicate appears one way or another by proving it or its negation – note, by the way, that the account of meaning I am sketching here on the basis of Davidson's work is strikingly similar to the one I attributed to Korselt in § 50 –, cases are bound to arise where a relevant sentence is undecided by the axiom system at hand, since, in light of Gödel's incompleteness theorem, roughly put, the combination of both «manageability» of the axiom-system and maximal-consistency is not to be had even for relatively simple kinds of theories, and much less for more complicated ones.

Gödel's incompleteness theorem for arithmetic is by no means the only one which has this implication for the theory of meaning. Think, for example, also of Gödel's and Cohen's results establishing the independence of the Continuum Hypothesis from the standard axiom systems for set-theory; indeed, in that case it strikes me as far more plausible than in the case of the sentences constructed by Gödel (or Rosser), which turn out to be independent of Peano's axioms, that settling the particular sentence which turns out to be independent of set-theory has (even substantive) bearing on «*what we mean*» by, e.g., "cardinal number" or "set".

That is to say, in sum: There will always be sentences like "Frege's pocketwatch is a Euclidean point.", the question of whose truth or falsity is not settled by the axioms one accepts as determining the meaning of, in this case, the expression "Euclidean point". The response to such examples should, I am urging, not be (dis)qualification (either of the sentences as viable counterexamples or) of the theory, but acceptance: Meaning is fixed for any expression always only so far as the presently salient practical purposes require, to be expanded upon demand.

> [Higher mathematics] is the conceptual edifice raised by mankind in order to make sense of the world. if, then, the search for absolute truth in mathematics is replaced by the progressive clarification and improvement of human understanding, incompleteness ceases to be a bug-a-boo[223]

[223] Solomon Feferman. In: *In the Light of Logic*. Oxford University Press, 1998, pp. 3–27, p. 19.

3.3 Hilbert's Scientific Diary (and other Unpublished Sources)

§ 156 There is another key source for Hilbert's views and ideas at the stages of his career I have been focusing on, which has not yet received the scholarly attention its extent and richness warrants: Hilbert's so called "scientific diary". This title collectively designates three black notebooks held, along with Hilbert's other papers, by the SUB Göttingen under the library signature "Cod. Ms. D. Hilbert 600/1-3"[224].

The entries in these notebooks – written mostly by Hilbert himself, but sometimes also by his wife – range from mere references to publications Hilbert meant to study, through aphoristic reflections on the nature of mathematics and mathematical practice, occasional self-reflective remarks, as well as ideas for results to prove, proofs to review or refine, to lengthy and detailed continuous sketches perhaps intended for inclusion in publications or lectures.

I am aware of at least one instance where one still finds a version of a remark found in the notebook (namely remark I-243, p. 82) among Hilbert's papers written on the back of what is clearly scrap-paper – in this case, a discarded banking-form – as Cod. Ms. D. Hilbert 681, sheet 19. The likely explanation for this remark existing thus in two versions is that Hilbert first wrote down the remark on the sheet of scrap-paper and later asked his wife to copy it into the notebook (since the entry there is in Käthe Hilbert's handwriting; though I see no reason to think that Hilbert did not also copy entries into the notebook himself).

The fact that in the third volume a handful of entries appear more than once also suggests that Hilbert did not always *compose* the entries directly in the notebook, but rather composed them (at least occasionally) in some other format first and then copied them into the notebooks subsequently (or asked his wife Käthe to do so). Some entries are simply *written on* separate pieces of note-paper which have been *glued into* the notebooks.

§ 157 There is, as far as I can see, no reason to think that further notebooks existed at some point but have since been lost or destroyed. That these three notebooks are indeed *all* there ever was is suggested mainly by the overview inlaid in front of the first volume, where Hilbert himself retrospectively highlights certain entries and themes of importance to him: There Hilbert references only "three black notebooks". If Hilbert had continued keeping similar notebooks, he would presumably have included entries from them in this retrospective overview as well.

I believe that Hilbert proceeded, subsequently, to record his thoughts either on separate sheets, or in notebooks dedicated to the preparation of his lecture courses (i.e. Hilbert's so called "Kolleghefte") or, as he had at his disposal more and more clean lecture notes prepared by his collaborators, in the form of additions, revisions or marginalia to these sets of lecture notes instead of keeping dedicated notebooks[225]. Indeed, Hilbert's development of an axiom system for the theory of the natural numbers in 1913, as documented in the convolute held by the SUB under the

[224] As of September 2021, digitisations of these notebooks are available through the web-site of the Göttingen digitisation-centre (GDZ): https://gdz.sub.uni-goettingen.de/volumes/id/DE-611-HS-3180969 (last checked Oct. 30 2021).
A full edition of these notebooks is projected for the sixth and final volume of the edition of *David Hilbert's Lectures on the Foundations of Mathematics and Physics, 1891-1933* edited by Ewald, Hallett, Majer and Sieg.

[225] In his brief introduction to Hilbert's 1919/20 lecture David Rowe highlights the (exceptionally) "rich and harmonious balance between teaching and research [Hilbert attained] during his first two decades in Göttingen" (David Hilbert. *Natur und mathematisches Erkennen. Vorlesungen, gehalten 1919-1920 in Göttingen*. Ed. by David E. Rowe. Birkhäuser, 1992, p. viii), which I think enabled Hilbert to record his latest ideas in a timely and, to him, satisfactory manner in other venues than dedicated notebooks.

signature Cod. Ms. D. Hilbert 559 is a prime example of what I have in mind: One of Hilbert's collaborators produced notes for lectures Hilbert gave, which Hilbert went on to revise substantively, then asked for another copy of the revised notes to be produced, which Hilbert then used as the basis for further work and lectures.

Further evidence of the changed practice of note-keeping I just described exists in the form of sheets which have been filled in the same style as the notebook, i.e. with short entries separated by a horizontal line, dating – judging by the topics – from the late 1910s and the 1920s among the papers held by the SUB, e.g. in the convolute Cod. Ms. D. Hilbert 601 – where one finds (among many other things) an extended discussion of Reichenbach's & Schlick's work (sheet 1), a programmatic rejection of Brouwer's position (sheet 20) and a note concerning proofs of claims which are themselves about proofs (sheet 23) –, and likely also in other convolutes grouped by the SUB under the heading "Calculations and Notes about various mathematical problems" (signatures Cod. Ms. D. Hilbert 600-681).

3.3.1 Dating the Notebooks and their Content

None of the entries in Hilbert's notebooks are dated, making it in principle impossible to determine with certainty, § 158
rather than as a matter of conjecture, the precise date of composition for individual entries.

In order to estimate approximate dates, one must rely on indirect evidence: References to publications, lectures and letters can be relied on at least for establishing that an entry – and, since new entries seem to have been generally added after older ones, all those following it – must have been written *after* a certain date.

The second and third volume give rise to an additional difficulty because Hilbert seems there to have maintained two series of entries – one starting in front and mainly mathematical in content; the other starting in back and mainly philosophical in content – in parallel. Each series appears to have been written in sequence, so the principle that an entry appearing later in the sequence was written after any entry appearing before it in the sequence still applied; estimating the relative date of entries not found in the same sequence involves an additional layer of difficulty, however. The hypothesis that entries concerned with the same topic are for that reason likely contemporaneous can be relied on to establish some entries in different sequences as contemporaneous – and very occasionally the very same entry has even been copied into the notebook twice, once in each sequence –, and the relative date of other entries in close proximity to these on the basis of such a determination.

On the other hand, the time-*span* covered by each of the three notebooks can be estimated quite exactly because § 159
Hilbert dated the first and second notebooks *themselves*, that is to say, Hilbert recorded the date when he acquired (or started using) each notebook on its first page: Hilbert acquired the first notebook "in the winter of 1885/86 in

Leipzig"[226], and the second on "12 November 1892"[227].

Since Hilbert seems to have made at least some additions to the first notebook *after* he already started writing entries in the second[228], these dates suggests that the first notebook spans approximately the years 1885-1892; indeed, since one entry found relatively early in the first volume (on p. 12) apparently was written after Hilbert's encounter with Weierstrass during Hilbert's 1888 travels[229], most entries were likely written between 1888-1892.

That Hilbert still added to the first after starting to use the second notebook can be explained by observing that § 160
this addition, where Hilbert refers to the second volume, belongs to a list of "dissertation topics" (pp. 128-133).

Hilbert seems to have set aside the last few pages in the first notebook for recording dissertation topics to propose to potential doctoral students. So, when Hilbert ran out of space for genuine entries in the first notebook (on p. 126), he presumably started recording new entries in what became the second notebook, while still actively maintaining the list of dissertation topics in the back of the first volume.

Indeed, only the first notebook contains a list of dissertation topics, which suggests that Hilbert may have used the list in the first volume for a long time after he no longer used the first volume actively as a notebook.

At the end of the second volume (pp. 109-113) one finds two further lists, one of "lectures already delivered" (pp. 109-110) and one of "lectures yet to deliver" (pp. 112-113), which likewise were probably also actively maintained also at least for a little while after Hilbert no longer used the second volume actively as a notebook.

The list of lectures already delivered initially consisted of 19 entries (all written by Käthe Hilbert at the same distance to the edge of the page), to which 12 were subsequently added (at varying distances to the edge of the

[226]This date is found on the first page of the notebook. This page is not numbered, but all subsequent (odd-numbered) pages are, starting on the following recto-page with the number "1". This date, though written in Hilbert's hand, was likely added later, since, unlike the first handful of entries in the notebook, the date is *not* written in the older form of German longhand known as "Sütterlin", which Hilbert used early on in his life, as the two notebooks preserved among the papers held by the SUB from Hilbert's school-days and the lecture-notes preserved from his university student-days attest. Indeed, it is perfectly possible that the date was added *much later*, perhaps along with a poem about Göttingen – which must be later, since Hilbert was still living in Königsberg in 1885, when he reports acquiring the notebook – written on the back of the same page (which one also finds quoted on p. 70 of Hilbert's address-book (Cod. Ms. D. Hilbert 756:2), there attributed to somebody called "Tesdorpf" (?)), and/or along with a sheet inlaid in the front of the first volume, where Hilbert points to what he regarded as the most important passages throughout *all three* notebooks.

[227]This date is also found on the first page of the notebook, which is, again, not numbered (although subsequent pages, starting with the next recto-page, are). Seeing that a precise day is specified here suggests this date was likely added contemporaneously (since it is unlikely Hilbert would have remembered the precise *day* later), though, unfortunately, Hilbert does not say in the second notebook whether this day is merely the date when he purchased the notebook, or the (possibly later) day he started writing entries in it.

[228]On p. 128 of the first notebook Hilbert refers to "the first few leaves" of "black notebook II". That "black notebook II" means the second volume of the scientific diary can be regarded as certain, since Hilbert uses the same designation on his overview-sheet inlaid at the front of the first volume. For details, see the discussion in § 160.

[229]On p. 12 (entry I-66) Hilbert takes issue with a position he explicitly attributes to Weierstrass, namely that "the order of a function in (homogeneous) variables is too insignificant to base theories on it"; Hilbert reports Weierstrass taking the position that "the degree, i.e. the order of a curve [is] something too insignificant to base a theory of reality-questions on it"[ccxciv] on the page numbered (by an archivalist) 1/6v of the small diary Hilbert kept during his travels (Cod. Ms. D. Hilbert 741:1)[ccxcv].

The label "reality-questions" ("Realitätsfragen") appears several times in Hilbert's scientific diary, and in an entry on p. 48 of the first volume Hilbert mentions a (by that point) "old 'manuscript'" about reality-questions. I have not succeeded in finding or identifying such a manuscript among Hilbert's papers, so I find myself reduced to reporting Hilbert's bibliographic references (entry I-114 (p. 28), and Cod. Ms. D. Hilbert 741:14) to Klein (both), Kronecker (I-114), (Walther von) Dyck (I-114), Friedrich Meyer (I-114), Maxwell (741:14), Sylvester (741:14), Möbius (741:14) as a (slim) basis for guesswork as to what Hilbert may have meant by that label; judging by the references Hilbert associates with the topic, the manuscript was, in spite of the title, likely not philosophical in nature.

pages; the last 5 in Hilbert's own handwriting)[230]. Hilbert seems to have stopped maintaining this list in 1905[231], which agrees with the end-date for Hilbert's use of the second volume as a notebook (allowing for him to continue maintaining the list of entries for a handful of years after no longer actively using the notebook) and the start-date for Hilbert's use of the third notebook I am about to suggest.

§ 161 Based on the date written into the front of the second notebook, it spans the years between 1892 until approximately 1903, because the second remark written in the third volume (on p. 5) refers to "my Gauss-Festschrift", i.e. the first edition of the *Grundlagen der Geometrie*, which was published in 1899. Since it refers to the book as the "Gauss-Festschrift" this entry likely pre-dates the re-publication of Hilbert's book under the title "Grundlagen der Geometrie" in 1903: Hilbert later used the phrase "meine Grundlagen" as the default way of referring to his early book[232]. Hilbert thus seems to have begun writing entries into the third volume around 1902 – give or take a year or two – and continued to do so *at least* until 1912, as an entry on p. 97 of the third volume attests, where reference is made to a letter "from Einstein to Sommerfeld", whose date is explicitly given as "29.10.1912"; because this entry appears on the fourth-to-last page of the notebook, one may – on the assumption that Hilbert saw this letter to Sommerfeld soon after Sommerfeld received it – take it also as evidence that Hilbert wrote most of the entries in the third, and final, notebook *before the end* of 1912, and very few, if any, after.

§ 162 In the back of the third volume one finds a few loose leaves with entries in a similar style as those found in the notebook, which are clearly of a *much* later date and not originally part of this notebook (indeed, they are considered by the SUB a semi-separate item, which is assigned the library-signature "Cod. Ms. D. Hilbert 600:3, Beilage"), since among these remarks is a list of "Hitler's accomplishments", which reads (in full): "abolishment of the many German states, accommodation of the 5 million jobless persons (or at least reduction to 1 million)[,] setting aside the indignities of the Versailles treaties, introduction of the universal military service" (p. 2^v of the Beilage). It is not clear whether *all* the entries of the Beilage date from the 1930s, or whether some were written at an earlier date and simply happened to be on the pieces of paper where Hilbert proceeded to add entries in the 1930s, but there is no compelling reason to assign an earlier date to any of these entries.

[230] That the list can be so divided into «original» and subsequent entries is plausible for two reasons: (a) this divide – between a lecture on "Algebraische Zahlen und Functionen." (likely the lecture from the winter-semester 1890/91) and "Grundlagen der Geometrie. (nur ausgearbeitet, *nicht* geha[lten)]" (which happened in the summer-semester 1893) agrees approximately with the acquisition- or start-date for the volume found in the front, and (b) the first 19 entries are not in chronological order (though all can be seen as corresponding to some lecture Hilbert delivered before 1892), while the last 12 are more or less in chronological order (but not complete; e.g. Hilbert struck out "Analytische Geometrie" from the list of lectures yet to deliver, presumably when he delivered such a lecture in the winter-semester 1894/95, but never added it to the list of lectures already delivered).

[231] The last entry in the list of lectures actually delivered is for a lecture on "Variationsrechnung", which is likely the one Hilbert held in the winter-semester 1904/5, because it occurs in the list after an entry for the lecture on "Fokaleigenschaften der Curven u. Flächen 2^{ter} Ordnung.", on which topic Hilbert lectured only once, in 1897/98; between this entry and the one for "Variationsrechnung" are two entries: one for "Analytische Mechanik" (presumably the lecture in the winter-semester 1898/99, the first lecture on mechanics Hilbert delivered) and "Grundlagen der Euklidischen Geometrie" (probably the lecture in the summer-semester 1902).

[232] One finds such a reference already in Hilbert's 1902 lectures, where the first edition of the book is referred to (in the brief bibliography given as part of that lecture's opening remarks) as "Grundlagen der Geometrie, Festschrift zur Feier der Enthüllung des Gauss-Weberdenkmals, 1899", and a footnote then refers to it briefly as "Meine Grundlagen" (see: Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*, p. 542). This reference suggests that the end-date for the second volume and the start-date for the third can perhaps be assumed as falling somewhere closer to 1900. One possible anchor point might be the publication of the French translation of Hilbert's book under the title "Les principes fondamentaux de la géometrie" in 1900.

The entry where Hilbert writes with (surprising) appreciation about Hitler stands in sharp contrast to other entries about the NS-regime one finds in an address book (Cod. Ms. D. Hilbert 756:2), which Hilbert seems to have also used as a diary for a while in the 1930s, culminating in the bleak pronouncement that "Everything gets worse! Göttingen's scientific significance waned (1934)[.] Emmy Nöther died (1935) in America."[233].

§ 163 If I am approximately right about the start- and end-dates of the three volumes of Hilbert's so called "scientific diary", they cover roughly the first 25 years of Hilbert's scientific career – from 1885 (mainly from 1888 onwards) until approximately 1913; the first notebook covering, roughly, the first 5 years of this time, the second and third then each around 10 years –, during which Hilbert recorded various thoughts and ideas in these notebooks.

In spite of the fact that these three notebooks thus cover only about half of Hilbert's career, and, in particular, *do not* cover the years Hilbert dedicated (from 1917 onwards) to the development of logic, proof-theory and the foundations of mathematics, the contents of these three volumes are of interest not least of all for what they reveal about Hilbert's philosophical position. This section is dedicated to highlighting those entries most worth noting in relation to Hilbert's foundational work and philosophical interests, broadly speaking.

§ 164 My exploration of the material provided by Hilbert's scientific diary will be organised around Hilbert's discussion of various people – Frege, Clebsch, Klein, Kronecker, Kant,...–, whom Hilbert associates with certain methodological and philosophical views, with respect to which Hilbert positions himself and thus shapes his own philosophical position about such issues as meaning, decidability, and the nature of mathematics and mathematical problems. This discussion will give me the opportunity to highlight, reiterate and finally (in §§ 204–207) compile in a summary fashion my main claims about Hilbert's foundational work and thought before the 1920s.

I want to begin, as I did in chapter 1, with Frege.

I will refer to the three notebooks either as Hilbert's "scientific diary" or simply as "the notebooks", and cite them – with kind permission of the SUB Göttingen – by way of the volume-number (as a capital Roman numeral) and the page-number according to the pagination found in the volumes themselves (where, because only odd pages are numbered, while even page-numbers must be supplied), e.g. as "Scientific Diary II, p. [40]"; when citing from the second or third notebook, I will also specify whether I am citing the sequence of entries starting in front (e.g. "IIIf"), or the sequence of entries starting in back (e.g. "IIIb").

3.3.2 Frege and Meaning

§ 165 Hilbert engages with Frege at some length on two occasions (both of them in the third volume) in his scientific diary. The more substantive of these entries is more decisively dismissive of Frege's ideas than the occasional

[233] See p. 71^v of the address book held by the SUB under the signature Cod. Ms. D. Hilbert 756:2.
The handful of entries about the 1930s found (on pp. 71^v-73^v; the entries may be written back-to-front) in the address-book, where Hilbert comments, among other issues, on the sterilisation of Jewish people with disbelief. These entries are in line with Reid's conjecture that Hilbert "continued to retain the deep Prussian faith in the law" (Constance Reid. *Hilbert*. Springer, 1996, p. 204), although they suggest, contrary to the picture painted by Reid, that Hilbert «saw what was going on» and found it deplorable.

remarks of vague acknowledgement one finds in later lectures[234], and also harsher than Hilbert seemed in his personal correspondence with Frege. The other one of the two entries concerned with Frege records, as Hilbert writes, an excerpt "from a postcard to Frege", which reads:

> My opinion is precisely this, that a concept is logically determined only by means of its relations to other concepts. These relations, formulated in certain assertions, I call axioms and so I arrive at the view that axioms (perhaps after the assignment of names) are definitions of the concepts. This conception is not one I came up with just for fun, but rather I saw myself urged to adopt it by the demand for rigour in logical inference and in the logical construction of a theory. I have become convinced that in mathematics and the natural sciences subtler matters can only be treated rigorously in this fashion, otherwise one only turns in circles.[ccxcix]

The excerpt recorded in this entry apparently stems from the postcard labelled "XV/8" by the editors of Frege's collected correspondence, dated Sept. 22 1900, with whose second paragraph it coincides almost entirely[235].

Less clear than the date of the actual postcard is the date of this entry: Though Hilbert may have recorded the excerpt in the notebook contemporaneously, he may also have copied it into the notebook from a separate draft later, e.g. when, in connection with Frege's later articles criticising him, Hilbert found himself compelled to think about Frege once again. In fact, given the start-date for Hilbert's use of the third volume (around 1903), it seems unlikely that Hilbert copied his postcard to Frege into the notebook when he sent it to Frege in 1900.

More noteworthy than this little summary of Hilbert's own views on mathematics and axioms, which is, in any case, already familiar through the edition of Frege's correspondence, is the following entry (also found in the third notebook on pp. 76f.), where Hilbert appears to prepare a response to (a version of) Frege's *third and final* article on the foundations of geometry published in 1906. As far as I am aware, this entry is the only piece of evidence available to support the claim that Hilbert actually read Frege's articles – or for that matter took any notice of them at all –, though, as the passage makes unmistakably clear, Hilbert was not persuaded:

> Frege's [example] "Ellaj bazet at least 2 Technizek" shows only Frege's ability to come up with senseless sentences, but nothing against my theory. Because my sentences are always of the form "... is coordinated with ...", where the concept "is coordinated" is presupposed to be familiar and determined before all logic, as, indeed, it must be. Because the ability of our mind to relate things to things must be assumed beforehand. Frege thus betrays that he has completely misunderstood the sense and purpose of my foundational efforts. Naturally one can use other words instead of "point", "line", "plane", "between" etc., but that is nothing new. That "lies on", "passes through", "contains" are not intended to mean anything other than the "being coordinated" mentioned above, I have explicitly noted time and time again. I might perhaps later note something like the following underneath

[234] E.g. the mention of "the thoroughgoing investigations of Frege" in David Hilbert. "Axiomatisches Denken." In: *Gesammelte Abhandlungen. Analysis, Grundlagen der Mathematik, Physik, Verschiedenes, nebst einer Lebensgeschichte*. Vol. 3. Springer Verlag, 1918, pp. 146–156, p. 153, which, though seemingly a favourable mentions, does not cite any specific accomplishment of Frege's; in fact, in "Axiomatisches Denken" Frege is mentioned only as a precursor to Russell. Likewise in Hilbert's "Neubegründung der Mathematik," "the acute investigations of Frege" are mentioned, again without any discussion of details (and assimilated to Dedekind's work), as inspiration for the "deep theories of Zermelo and Russell"[ccxcvi]. This tendency of merging Frege's work with Dedekind's continues throughout Hilbert's lectures in writings in the 1920s, e.g. in *Über das Unendliche*[ccxcvii]. As far as I can see, the only technical innovation ever credited to Frege is the use of negation and implication as primitives of propositional logic (e.g. in Hilbert's 1917/18 lectures on logical theory, see: Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 232). Hilbert's appreciation of Frege's work is, in my opinion, most aptly summarised in a remark found in § 1 of the *Grundlagen der Mathematik*, where (Hilbert and) Bernays claimed that "the failure of Frege's project" proved more inspiring than Frege's actual work[ccxcviii].

[235] There is only one minor difference between the text of this notebook-entry and the text found in the edition of Frege's correspondence (which constitutes some, though rather weak, evidence that Hilbert did not, at any rate, copy the text *directly from the postcard* he sent to Frege), namely, that in the postcard Hilbert actually sent to Frege, which has been preserved in its original and is today held by the Staatsbibliothek Berlin, wrote "treated with certainty" ("mit Sicherheit" (see the text of the postcard in Frege, *Wissenschaftlicher Briefwechsel*, item XV/8, p. 79)) instead of "treated rigorously" ("mit Strenge").

> the text: The example of Frege's "Anej bazet at least 2 Tezhnizek" shows only Frege's abilities to come up with senseless sentences, but nothing against my construction of geometry: Because this grouping of syllables of Frege's does not have the form of my assertions "... is coordinated with ...". My conception agrees exactly with that of Hertz (Introduction to Mechanics). What I am calling objects of thought are the "images" of Hertz, the "signs" of Pringsheim, Thomae etc.…
>
> "Frege's distinction between 'Axioms' and 'Definitions' is just chaff.[236]„[ccc]

Since this entry presupposes an acquaintance with Frege's third article, or at least a draft thereof, where a (slightly different) version of the example Hilbert is here focusing on, appears for the first time[237], this entry was presumably written around the time of publication of that article, i.e. around 1906, which would also fit in well with what I have said about the time-period covered by the third notebook, i.e. approximately 1902-1913.

It is not known, as far as I am aware, whether Frege sent Hilbert either drafts or offprints of his articles – if he did so, these drafts or offprints, and any correspondence accompanying them or Hilbert's acknowledgments of receipt, such as the postcard Hilbert sent to Frege to acknowledge receiving a copy of the second volume of Frege's *Grundgesetze der Arithmetik* (item XV/9 of Frege's collected correspondence), have not survived –, or whether Hilbert became aware of them only when reading, as presumably he did routinely, the *Jahresbericht der Deutschen Mathematiker-Vereinigung*, where Frege's articles appeared.

§ 166 Be that as it may, the second entry constitutes, as far as I can see, the most explicit record of Hilbert's views on his exchanges with Frege, and shows Hilbert to have been entirely dismissive of Frege, claiming that Frege "has completely misunderstood the sense and purpose of my foundational efforts"; and also expressly rejecting the distinction between axioms and definitions Frege had gone to great length to urge Hilbert to accept, or, at any rate, to fault Hilbert for not accepting. I see in this second entry a fair amount of corroboration of my suspicion voiced earlier (in footnote 155) that Frege's influence on Hilbert was, in fact, minimal: As this entry shows, *even if* Hilbert thought at any length about Frege's work – which this *single* entry does not suggest that he did –, Hilbert came to the conclusion that Frege did not raise a sound objection but had simply misunderstood Hilbert's project. (How one would adjudicate the substance of the controversy nowadays is, of course, another matter – though I am inclined to take the same view – and I merely want to claim that this is how *Hilbert* came to see it.)

§ 167 In the last entry I quoted, Hilbert speaks of "the ability of our mind to relate things to things", presumably echoing Dedekind's *Was sind und was sollen die Zahlen?*, where Dedekind had proclaimed that without "the ability to relate things to things, to make one thing correspond to another, or to map one thing onto another thing thinking is altogether impossible"[ccci]. Indeed, other entries found in Hilbert's notebooks reveal how impressed Hilbert appears to have been by this particular philosophical claim of Dedekind's from early on, and then throughout his career[238].

[236] It is not clear why Hilbert enclosed this sentence in quotation marks. Perhaps they are meant to flag this particular formulation for use in the event that Hilbert ever decided to respond in print to Frege's article, or discuss it in lectures. One finds the metaphor of "chaff" ("leeres Stroh") also, alongside other words used as metaphors for idle or pointless pursuits ("phantoms" and "carousel-riding"), on the sheet numbered (by an archivalist) "21" in the convolute held by the SUB under the signature "Cod. Ms. D. Hilbert 681", where Hilbert describes his own ambition, by contrast, as "taking away from philosophy the domain of foundational studies".

[237] The example Hilbert attributes to Frege in this entry is not the example one finds in Frege's article, though it is unmistakably of the same kind as Frege's nonsense-example "Each Anej bazet at least two Ellah.". Where Hilbert is taking his alternative example from – whether from a draft of the article, or from correspondence with Frege (since lost), or simply from misremembrance – is unclear.

[238] Stressing Dedekind's influence on Hilbert has been one of the hallmarks of Wilfried Sieg's historical work, see, e.g., Wilfried Sieg and

In the first notebook Hilbert goes as far as saying that *every* mathematical proposition ultimately takes the form of a claim about mapping one thing to another:

> Every mathematical problem can be reduced to the following question: A law is given, by means of which to any given non-terminating sequence consisting only of zero and 1 [...] a certain other such sequence [...] can be constructed (by means of calculatory operations: seek the greatest whole, a divisor etc. no playing dice…).[cccii]

This view of mathematical propositions is reaffirmed in the second notebook:

> Every proposition in mathematics can be formulated positively as an existential proposition of the following form: To any system of numbers of this or that property a system of numbers of this or that property can be coordinated. The first system represents the given, constant, the second system represents the sought, variable, arbitrary. The properties are expressed again in such coordinations, and so the whole building rises up.[ccciii]

The conviction expressed in these two entries presumably underlies the entry about Frege I quoted before: Frege's nonsense-example, according to Hilbert, does not express a claim about the coordination of objects, and for that reason – because, in Hilbert's view, all mathematical propositions must express claims about coordination – it fails to express a mathematical proposition; it is an example only, as Hilbert says, of senselessness.

The reason for this failure, so far as the entry I quoted sheds light on Hilbert's train of thought, seems to lie precisely in Frege's claim to use words with which *no preconceived sense whatever* is yet associated and assign to them a sense *only* by stating a sentence supposed to supply an axiomatic characterisation of the terms involved. That, Hilbert seems to imply, cannot be done, and was, at any rate, not what he, Hilbert, intended.

Rather, Hilbert claims, he *did*, in his presentation of the axioms of the geometry, presuppose something about the meaning of his primitives, but something which, according to Hilbert (echoing Dedekind) *one cannot help but presuppose*, namely, that they express a coordination of objects, the specific «pattern» of which – i.e. what is coordinated how with what else – is then specified by the axioms[239]. But, so Hilbert's rejection of Frege as I understand it seems to imply that in order to state and appreciate axioms one must understand first and foremost (and by some primitive cognitive faculty always presupposed) that one is looking at claims about coordination[240].

Dirk Schlimm. "Dedekind's analysis of number. Systems and axioms." In: *Hilbert's Programs and Beyond*. Oxford University Press, 2013, pp. 35–72, p. 61, Wilfried Sieg. "Methods for real arithmetic." In: *Hilbert's Programs and Beyond*. Oxford University Press, 2013, pp. 73–90, p. 75, Wilfried Sieg. "Relative consistency and accessible domains." In: *Hilbert's Programs and Beyond*. Oxford University Press, 2013, pp. 299–326, p. 306, where Sieg summarily states: "The very beginnings of the Hilbertian program can be traced back to these foundational problems in general and to Dedekind's proposed solution in particular." (the same sentence, and, indeed, much of the surrounding paragraph appears (almost word for word!) also in: Wilfried Sieg. "Hilbert's Programs: 1917-1922." In: *Hilbert's Programs and Beyond*. Oxford University Press, 2013, pp. 91–127, p. 95).

It seems to me this entry (along with the ones I go on to quote) substantiates Sieg's claims about the impact of Dedekind's work on Hilbert, which Sieg seems to have arrived at by comparing the theories of Dedekind and Hilbert in the abstract. As far as I can see, Sieg never points to any explicit references or unmistakable allusions (such as in this passage) to Dedekind's foundational work – as opposed to Dedekind's work in algebra – in Hilbert's early work; to my mind, the (almost) complete absence of passages testifying to explicit engagement of Hilbert's with Dedekind's foundational work may be taken to suggest that, on the whole (and contrary to the thrust of Sieg's claims), Hilbert did not engage at any length or detail with Dedekind's *foundational* work.

[239] This manner of speaking of axioms as fixing a pattern was first used as a central metaphor for how axioms function by Davidson, who so described the effect of Tarski's axiomatic characterisation of truth in *Truth and Predication*: "In his appeal to Convention-T, Tarski assumes, as we have seen, a prior grasp of the concept of truth; he then shows how this intuition can be implemented in detail for particular languages. The implementation requires the introduction of a referential concept, a relation between words and things, that is, some relation like satisfaction. The story about truth generates a pattern in language, the pattern of logical forms, or grammar properly conceived, and the network of semantic dependencies." (Davidson, *Truth and Predication*, p. 35).

[240] Another prominent contemporary reviewer of Hilbert's *Grundlagen der Geometrie*, in contrast to Frege, was remarkably attuned to

A similar presupposition about a slim but definitive basis in meaning presupposed when stating axiom-systems is, it seems to me, implied also in a remark found in Hilbert's 1919/20 lectures, where Hilbert says that when proceeding axiomatically one "characterises [things] by relational properties"[ccciv]: One must *recognise that* the axioms make claims about relations in order to recognise them as axioms. Indeed, Hilbert held that some meaning must be associated with the expressions of a precisely defined theory throughout the remainder of his career, although, as I will discuss in § 172, he appears to have changed his mind about *what meaning* that was.

3.3.3 Clebsch and the Essence of Mathematics

Other entries in the notebooks also show Hilbert criticising others among his contemporaries besides Frege for not § 168
duly appreciating the role of the human cognitive faculty in mathematics as Hilbert saw it.

In this subsection I want to discuss Hilbert's criticism of a rather broad group – including, among others, Kummer, Kronecker, and perhaps, by association, even Klein – in this way. But the scattered critical remarks are of interest also because Hilbert develops his own positive conception of mathematics alongside these criticisms and in explicit contradistinction to those mathematicians whom Hilbert derisively calls "indexexers":

> Mathematics is perfect at every point which lies out of the reach of the indexexer and his torture.[cccv]

Hilbert frequently opposes this approach, as he does in this remark, without mentioning the names of anybody whom he considers to be part of the group he collectively derides as "indexexers" and, indeed, without saying what fault this label is meant to bring into focus[241]. I will address both of these uncertainties in order, beginning with the question whom Hilbert reckons among this group he appears to strongly oppose here. This question can be answered by simply collecting the names occasionally mentioned in connection with some of the same phrases used to describe this type of mathematician. Chief among them is the names of Clebsch, and his "school":

> Because of the school of Clebsch and Jordan with its jarring formalism and its wasteful $x$ing mathematics as a whole, the most noble science, runs the risk of being mistrusted by thinking people and to sink into a thoughtless game which [...] contributes not in the slightest to the development of the human mind[242] What would Gauss

what Hilbert's scientific diaries reveal to have been Hilbert's underlying philosophical position, namely Henri Poincaré. "Les Fondaments de la Géometrie." In: *Bulletin des sciences mathématiques* 26 (1902), pp. 249–272, p. 252, who writes: "Voici les réflexions que doivent nous inspirer ces énoncés [viz. the first group of Hilbert's axioms]: les expressions, *être situé sur*, *passer par*, etc., ne sont pas destinées à évoquer des images; elles sont simplement des synonymes du mot *déterminer*. Les mots *point*, *droite* et *plan* eux-mêmes ne doivent provoquer dans l'esprit aucune représentation sensible. Ils pourraient indifféremment désigner des objets d'une nature quelconque, pourvu qu'on pût établir entre ces objets une correspondance telle qu'à tout système de deux des objets appelés *point* correspondît un des objets appelés *droits*, et un seul."; a similar remark occurs also in Poincaré, "Les mathématiques et la logique [I]," p. 816.

[241] The neologism "indexexers" ("Indexixer") Hilbert uses here expands another similar word Hilbert had used earlier in the notebooks (I will quote some examples), where, in a similarly pejorative sense, he had spoken of "exing", i.e., I take it: of [people who are] operating too freely with 'x's (and, presumably, other letters standing in, pars pro toto, for all kinds of formulas, on which the mathematicians Hilbert opposes rely, in his view, to readily, frequently and needlessly). Using the compound "Indexixer" adds the overuse of indices to the charges implied in the label. (I want to note parenthetically that the verb "ixen" does nowadays appear in the standard German dictionary, carrying, among others, the sense "to replace other letters with an x", see https://www.duden.de/node/147755/revision/502007 (checked 16/11/2021), though this sense likely stems from the practice of using "x"s as deletion-marks when typewriting.)

[242] In another remark, not much later in the first notebook, Hilbert claims that "by that superficial manner of phantasising introduced by the school of Clebsch any deeper mathematical innovation is paralysed"[cccvi].

> say. In any case it is plain to see that by means of such an extraneous tool as symbolism nothing can be accomplished. One must, after all, bring ideas into the mix. From nothing nothing comes to be[243].[cccviii]

One may assume that the phrase "the school of Clebsch" covers, besides Clebsch himself, at least also Lindemann, who, after Clebsch' early death, published notes of Clebsch' lectures[244]. In any case, Hilbert also explicitly associates other mathematicians unrelated to Clebsch with this tendency of a «overly formalistic» approach he opposes.

Perhaps the most prominent among the names Hilbert associates with this tendency are that of Jakobi, whose "manic calculating" ("die Jakobische Rechnerei") Hilbert mentions as an example of "symbolic calculation and *x*ing"[cccix], and Kummer, of whom Hilbert says that he (along with Jakobi) was "the greatest calculator" (and adds, thus implying what seems to me like a modicum of respect that Kummer (and Jakobi)[245] "will always remain that"[cccxi]. Hilbert also reproaches "Kronecker[246][,] Netto and Kneser" for presenting "the clear, simple, natural thoughts of Galois, which even a child could understand, [as] painfully stretched, clogged and smeared into formulas"[cccxiii].

All of these passages suggest that for Hilbert the overestimation of the value and use of *formulas* is at the heart of how Hilbert characterises the position he opposes; and, indeed, one finds Hilbert expressing critical views about the (over-)use of formulas in mathematics in a number of entries like the following:

> The formula (calculation) is either as an example or as a tool (e.g. in the theory of power-series[247]) justified but it is never a mark of progress or an end in itself. Just as the development of means must never make one forget the purpose, the real aim, so, too, the arrangement of formulas [must never make one forget] the thought guiding and driving it.[cccxv]

And:

> The formula must be our slave, not we slaves to the formulas![cccxvi]

In formulations like these, the relevant contrasting position Hilbert seems to reclaim for himself, also takes shape: § 169
As Hilbert sees it, mathematicians should not focus on formulas or calculations, but on what Hilbert vaguely calls "thoughts" or (in the previous entry I quoted in full) "ideas". A similar remark, contrasting "exing" with thinking and decisively rejecting the latter in favour of the former appears in the second notebook:

[243] This aphoristic turn of phrase appears again in chapter 2 of Hilbert's 1919/20 lecture, where Hilbert remarks that mathematics must be based on substantive assumptions because "from nothing nothing can be deduced"[cccvii].

[244] This remains a (however plausible) conjecture since Lindemann's name is never throughout the notebooks mentioned explicitly in this connection. However, I will add that, as far as I am aware, Hilbert only ever mentions Lindemann's name twice in all three notebooks (both mentions occur early on in the second notebook, recording ideas Hilbert had in connection with a presentation Lindemann gave). For a more detailed discussion of Clebsch' school, see David E. Rowe. "Klein, Hilbert, and the Göttingen Mathematical Tradition." In: *Osiris* 5 (1989), pp. 186–213, p. 188, though none of the other members of that school are associated with Clebsch by Hilbert in his notebooks; some of them (e.g. Max Noether, Felix Klein, Paul Gordan) are occasionally discussed, but not as followers of Clebsch.

[245] That Hilbert esteemed Jakobi (and Kummer) more highly than Clebsch is made explicit by a remark in the third notebook, where Hilbert calls Jakobi "a true geometer", and places him on the same level as Weierstrass in this regard, while explicitly contrasting him with "Clebsch, who with his monstrous manipulations [of formulas] was a one-sided analyst" (III/b-2, p. 137; this remark recalls the remark from the first volume[cccx], which was also concerned with the comparison and contrast of Jakobi and Weierstrass).

[246] That Hilbert thought Kronecker, in particular, overly reliant on formulas is further attested by another entry, where Hilbert writes that "Kronecker's mathematics is in part a dull, extrinsic mess of formulas, and in part indefinite sentimentality"[cccxii].

[247] Worth noting in connection with this example is an entry in the third notebook, where Hilbert observes: "How little the formal, analytic expression represents is something the Dirichlet series teaches us, since one can sum it up in finite contained form but still not decide with the aid of that expression whether it is equal to 0."[cccxiv]

> Where the *x*ing begins, thinking ends. We must always remember that the letters signify concepts and that the signs indicate the connection of concepts.[cccxvii]

Hilbert seems to hold this position from very early on. Indeed, one finds it already implied when he first formulates his criticism of the over-reliance on formulas on p. 34 of the first notebook, where Hilbert writes:

> Formulas can only describe thinking (thoughts), not replace it or spare one having to do it: automated production [in contrast to] manual labour[cccxviii]

§ 170 This entry may, in fact, mark the beginning of Hilbert's reflections on the ills of formalism and the nature of mathematics, properly conceived. Not just because these issues make their first appearance in this entry, but because the particular formulation Hilbert uses here, that formulas "spare one having to [think]" ("das Denken ersparen") echoes a formulation Hilbert uses in one of his reports of encounters with prominent mathematicians during his 1888 travels: In his brief report of their encounter Hilbert ascribes to Thomae the "principle" that "one must be able to write everything down and then substitute and carry out calculations", and comments: "He [Thomae] wants to be spared thinking."[cccxix]. Since, as I pointed out earlier (in footnote 229), there is at least one other clear case where an entry early on in the first notebook shows Hilbert reflecting on what he heard from other mathematicians he encountered during his 1888 travels (in that case: Weierstrass), it is not out of the question that in this case, too, the similarity of formulations indicates continuity of reflection between Hilbert's encounter with Thomae in 1888 and his critical attitude towards formalism, as expounded by Thomae in person[248].

§ 171 Be the beginnings what they may, over time, the conception of mathematics expressed in these remarks seems to have evolved in such a way as to make Hilbert sceptical also of the demand that mathematicians should be guided in their research by the criterion of applicability of their results in the natural sciences, which Hilbert came to reject in favour of «self-sufficient thinking», as the following entry from the second notebook shows:

> One often hears complaints about mathematics from those who want to apply it. They demand, fundamentally, that mathematics should spare them thinking or make it superfluous; but mathematics teaches precisely the opposite and everywhere urges one to think more than is needed for the practical purposes momentarily at hand.[cccxx]

One finds another entry strongly opposed to orienting mathematical research by applicability in the third volume:

> It serves well as a counterbalance to the pursuit of mathematics for the sake of application that it should also keep close to philosophy and concern itself with the principles of logic. It is, in fact, an existential question for mathematics: either it will rule both logic and the natural sciences or it will not rule at all.[cccxxi]

The conviction that mathematics should not be guided in its developments by the applicability of mathematics in the natural sciences also appears to have led Hilbert to taking a more critical stance towards Felix Klein. In an entry found in the third volume Hilbert writes: "Klein wants to make mathematics the empress in the world of reality, I in the world of ideals."[cccxxii] (I take it, Hilbert is using the word "ideals" here in its algebraic sense.)

[248] There is yet another potential case of such continuity between Hilbert's 1888 travels and the notebooks, namely, the beginning of Hilbert's critical engagement with Kronecker (on p. 8 of the first notebook, four pages before the entry referencing Hilbert's encounter with Weierstrass, which I discussed briefly in footnote 229), which I will discuss in § 174.

Not much earlier in the third volume one finds other entries which show Hilbert being more explicitly critical of Klein, both as a mathematician and as an organiser or political actor, on p. 127, where Hilbert begins a series of entries with the heading "Polemics against F[elix] K[lein]'s direction in mathematics and science on the whole" and goes on (under this heading) to make a note to himself to tell "the minister" that "we must not be overtaken by other nations [...] and not sink to the state of the English or the Americans"[cccxxiii] .

In the following entry, Hilbert appears to criticise Klein's mathematical work (at this late stage in the Klein's career)[249] for not "carrying anything through properly himself" but rather resting content with "the cheap reference to methods which exist only in ideas and have yet to prove themselves"[cccxxiv]. Hilbert goes on to remark in III/b-42 (without explicit reference to Klein, but sandwiched between two entries explicitly about Klein) that, in general, "it matters as much, *how* one does something, as *what* [one does]"[cccxxv].

At that point in time Hilbert seems to have come to regard his axiomatic method as a culminating synthesis of § 172
sorts, i.e. as a framework where one could indulge in *meaningful* (rather than gratuitous) calculation:

> The essential mark of modern mathematics, which will emerge increasingly clearly in the future is: one strives to understand intrinsically, while earlier one made discoveries by means of formal processes, which strike us nowadays as lucky accidents. The latter is nowadays completely out of the question: one wades in the sand and feels miserable when one calculates around waiting for good fortune. Today the idea is the main thing, the idea to be described in words, which regulates the connections and delivers our intuition. First in Riemann's theory of functions, then number theory[,] differential equations (Lie etc.)[;] the pinnacle is the axiomatic method, which, as it were, systematically excludes guessing, i.e. good fortune by taking into account all combinations, all logically consistent systems of conditions and making [success] depend on ordering the entire material at hand according to the salient aspects of the axiomatic method.[cccxxvi]

And:

> What Gordan and Klein want, to reduce everything to 7 operations, that is exactly the intention of my axiomatic method, so that one can prove every fact in mathematics by means of peas, check it and also discover any error. But the motivation[,] the application of my theory is altogether different, more general[,] more intellectual. Those who reject everything except calculation, who deem calculation special and reflection, concept-formation illicit, the Kroneckerian fanatics of finitude, should, of course, have the rug pulled out from under them by my method.[cccxxvii]

In the second of the two entries I just quoted, that his axiomatic method was, on the one hand, a continuation of the calculatory approach (according to which "every fact in mathematics [can be proved] by means of peas"), while, on the other hand, being "in general more spirited" ("[in der Anwendung] eine allgemeinere geistigere"). The continuity with an approach focused on calculation, as Hilbert seems to see it, lies in the combinatorial character

[249] The relationship between Klein & Hilbert has been discussed at some length in section II of "Klein, Hilbert, and the Göttingen Mathematical Tradition" (1989), and in "The Philosophical Views of Klein and Hilbert" (1994). At the end of the second article, Rowe concludes "that Hilbert, like Klein, was a spokesman for mathematical culture in the broadest sense of the word, and to me the parallels between their thought and their careers are much more striking than the qualities | that distinguish them." (Rowe, "The Philosophical Views of Klein and Hilbert," p. 201). The criticisms Hilbert expresses in the following entries of his notebook, however, speak more to one of the points of difference Rowe highlights, namely (Rowe, "The Philosophical Views of Klein and Hilbert," p. 190) that, while "neither Hilbert nor Klein was principally a productive talent [...] and the most distinctive difference between them, to my mind, lies not in their attitudes toward 'modernism' but in their orientation toward the achievements of the preceding generation. This issue not only reflects Hilbert's depths, as opposed to Klein's relative superficiality, it also relates to [...] the issue of modes of communication and presentation within the social structure of mathematics.".

of Hilbert's axiomatic method, as Hilbert stresses in the first of the two entries I just quoted, by emphasising that his axiomatic method "takes into account all combinations, all logically consistent systems". Less clear is Hilbert's contrary assertion that his method somehow goes beyond merely "mak[ing] discoveries by means of formal processes". Clarifying the difference Hilbert sees between his axiomatic method and the kind of formalism he consistently opposes throughout the notebooks seems to me crucial for understanding Hilbert's position.

I want to propose the following interpretation of Hilbert's cryptic manner of speaking of the "in general more spirited" character of his method, and, along with it, of how Hilbert saw his approach as differing from formalism. Basic to my proposal is the idea I stressed in connection with Hilbert's criticism of Frege in the third notebook: Hilbert appears to have believed – recall, especially, the passage quoted in endnote cccxxviii, and the passages quoted in connection with it on page 125 – that there are certain operations which are fundamentally engrained in the human mind as it is; (although, as I emphasised, the list of these operations Hilbert worked with changed over time, *that there was (only) a handful of such operations* seems to have been a long-standing conviction of Hilbert's).

The difference between his axiomatic method and the formalism he opposes can then be characterised as follows: Hilbert saw his axiomatic method as *based on such fundamental operations*; the primitive constituents of the expressions which constitute axiomatic theories all correspond to such fundamental operations (as revealed by some kind of philosophical reflection): "coordination" in the sense of Dedekind early on, later on perhaps the primitives of the syntactical meta-mathematics Hilbert thought of as foundational (see the following passage).

By contrast, the formalists he opposed simply came up with formulas as it suited them – using letters at pleasure, as Hilbert's derogatory label "indexexers" implies –, without reflecting on the question whether to the operation-symbols they employed any fundamental operations actually correspond. They thus proceed without "thinking" in a double sense: in the sense that they do not engage in philosophical reflection on the status of the concepts their expressions correspond to, and also in the sense that (for this reason) the expressions they operate with may fail to correspond to any real operation of the human mind.

That Hilbert held on to this idea that there were fundamental operations of thinking with which the primitives of precisely defined systems were somehow associated throughout the remainder of his career – although the list of what Hilbert took to be the fundamental operations of thought changed over time – is attested furthermore by the very last entry in the Beilage to the third notebook[250], which reads:

[250]This entry corroborates, as do several others, Michael Hallett's claim that Hilbert saw his foundational work also as an exploration of "the actual workings of the mind" because Hilbert believed "that a (deductive) thought process can be resolved formally into finitely many atomic units" (Michael Hallett. "Hilbert's Axiomatic Method and the Laws of Thought." In: *Mathematics and Mind*. Ed. by Alexander George. Oxford University Press, 1994, pp. 158–200, p. 187). This article is the only one I am aware of to take into account (if only very selectively) evidence from Hilbert's so-called "scientific diary" (in endnotes 22, 40 and in the main text on p. 182).

I want to make two remarks about Hallett's reliance on the notebooks: First, Hallett's statement in endnote 40 that "Hilbert paginates in reverse" strikes me as odd: The notebooks are paginated continuously from front to back, but, as I already noted, in the second and third notebook *two series of entries* are found: one (predominantly mathematical in content) starting in front, the other (predominantly philosophical in content) *starting in back*, so what is "in reverse" at the point Hallett is referencing is not the pagination but Hilbert's mode of adding entries. Second, while I am inclined to accept the dates proposed in endnote 40 ("between 1905 and 1910" for entries in the third volume) as approximately accurate, the date conjectured in endnote 22 (and presupposed on p. 182) of "a data [sic] around 1904-1905" seems to me too early for the entry in question, especially because (in light of the reverted order of entries) an entry on p. 101 would be (significantly) *later* than entries on p. 132-133 dated in endnote 40 between 1905 and 1910.

> Our rules: substitution, abbreviation, append arguments also to propositions[. Propositions] are complete and no extension is possible. Because our thinking is exhausted by these actions and is finite (no magic!)[cccxxviii]

§ 173 As I see it, this attitude underlies also Hilbert's belief that a consistency proof for an axiomatic theory is sufficient for demonstrating the existence of the mathematical objects the theory purports to describe. This confidence can be explained by, once again, referring to Hilbert's conviction that his axiomatic method was duly based on what *as a matter of fact were* the fundamental operations of the mind. Because the primitives in axiomatic theories, as Hilbert conceives of them, all correspond to fundamental operations of the mind, there can be no question that the sequences of operations prescribed by any expression composed of those primitives can be carried out, if only the operational prescriptions are, all in all, consistent. So it suffices to show for any given (collection of) formula(s) that the sequence(s) of applications of the fundamental operations prescribed by the formula(s in the collection) can, in principle, be carried out, i.e. that it is consistent[251].

This justification of consistency proofs as sufficient for existence claims – that, because one is dealing with operations that, constituting the fundamental inventory of the human mind, can always be executed and do always yield results as long as the prescriptions are coherent, one always arrives at objects by in fact carrying out the operations as prescribed – seems to me plausible *given three assumptions*: *that* certain operations are, in fact, unalienably fundamental to the workings of human cognition; that the only condition prescriptions for cognitive operations must fulfil is consistency; and that cognition alone is sufficient for objective existence. The second one of these three assumptions was expressly stated already by Kant himself[cccxxix], who called the principle of non-contradiction a "conditio sine qua non of knowledge", and was uncontroversially accepted at least until Hilbert's time[252]. The first and especially the third of these assumptions are, and were already in Hilbert's time, highly controversial. They were, however, widely shared among (post- and neo-)Kantian philosophers of a, broadly speaking: «idealist» persuasion[253]. Indeed, it seems to me that Hilbert himself had certain unmistakably «idealist» leanings, and that

[251] A remark made by Rowe seems to me to point in a similar direction: Rowe, "The Calm before the Storm," p. 75 says that "the consistency of the [models Hilbert described in his *Grundlagen der Geometrie*] followed from the consistency of the operations performed in generating the corresponding algebraic number fields", which suggests, as I am suggesting, that Hilbert's general strategy for consistency proofs lay in somehow accounting for problematic portions of theory object in terms of unproblematic *operations* used to generate such objects. Indeed, the consistency proofs Hilbert (and Bernays) outlined in more or less detail in Hilbert's lectures during the early 1920s bear out this conjecture, because they assign a central role to the use of recursively defined operations.

[252] Friedman, *Kant and the Exact Sciences*, p. 127, in fact, ascribes to Kant himself a view like the one I am suggesting to attribute to Hilbert. Friedman arrives at the following interpretation of Kant's position: "The rigorous representation of mathematical concepts and propositions requires schemata: constructions in pure intuition. In the case of arithmetic we are given the general capacity for successively iterating any operation in time; in the case of geometry we are given in addition certain fixed, specifically spatial operations as input: the constructions underlying Euclid's geometry. We can only think the propositions of mathematics by, as we might now put it, *presupposing* the required constructions, and it follows, as we have seen, that the true propositions of mathematics are then necessarily true. [...] The truth of the true propositions of mathematics follows from the mere possibility of thinking or representing them, and this is the precise sense in which we know them a priori." As Friedman goes on to say (on p. 128) there is for Kant a distinct sense of "possible", more demanding than logical possibility (i.e. consistency), in which, e.g. non-Euclidean geometry is not possible; it seems to me Hilbert, as I am proposing to understand his position, also recognises such a more exclusive sense of possibility insofar as he thinks not all primitives correspond to *fundamental* cognitive operations (though Hilbert will draw the lines differently than Kant (as reconstructed by Friedman) and think of non-Euclidean geometry as eminently possible also in this more demanding sense.

[253] I am thinking of such figures as the members of the so called "critical idealism" of the Marburg school of Neo-Kantianism (Hermann Cohen, Paul Natorp and Ernst Cassirer), but also, more proximately to Hilbert, of Günther Thiele, who was professor for philosophy in Königsberg when Hilbert studied there and whom I shall talk about at greater length later (in subsection 3.3.5.2). These figures qualify as *post-* or *neo*-Kantians, as I use these labels, on account of accepting the *third* of the three assumption I have formulated, which Kant himself rejected. A key fact to keep in mind when thinking about Hilbert's philosophical position is that accepting such an «idealist» assumption was, certainly in Hilbert's youth but also for most of his career, a live, even mainstream-position in academic philosophy.

Hilbert's equation of consistency and mathematical existence[254] is a testament to this fact.

3.3.4 Kronecker and Decidability

Another feature of the last entry I quoted, which one learns to recognise increasingly as typical the more one reads § 174
of Hilbert's scientific diary, is that Hilbert brings up a position associated with the name "Kronecker" seemingly only for the purpose of explicitly and harshly rejecting it, as also in the following entry:

> Of Kronecker's general views one can perhaps say that the truth, which usually lies in the middle, in this case lies entirely to one side, namely to the side opposite Kronecker.[cccxxx]

Hilbert's relationship with Kronecker, so far as one finds it documented in the notebooks, may be best described as "thoroughly ambivalent": Hilbert seems, on the one hand, to have spent a considerable amount of time thinking about Kronecker's claims and views while, on the other hand, going back and forth on whether to (partially) accept or reject them (and in what form and to what extent). Some of the entries where Kronecker's name is mentioned border on crude insults, bluntly delivered, like the following entry from the third notebook, which reads only:

> Kronecker was a reactionary.[cccxxxi]

Indeed, one entry in the first notebook provides some ground for speculating that Hilbert's animosity towards Kronecker was not entirely academic, but also, at least initially, to some extent personal: Hilbert records (on pp. 53f. of the first volume) reports he has received "from different sources" (of which he names "Hensel" and "Gutzmer") that "Kronecker himself supposedly said, when Gutzmer was present" that "my investigations were only an extension of Kronecker's ideas to invariant theory"[255]. These reports lead Hilbert to the – relative to his normal calm and factual tone – rather forceful assertion[256] that the relevant

> methods and results are my property (...) and I will let nobody take a jot of them, whoever he may be. Rather I believe it goes without saying that as a matter of honour once I have said so every upstanding person will contribute to the elimination of this obvious error.[cccxxxii]

But whatever opinion Hilbert had of Kronecker personally, Kronecker's views seem nonetheless to have exerted § 175

[254] Indeed, on the overview sheet inlaid at the start of the first notebook, one finds, as one among the ideas and themes Hilbert chose to highlight, an equation in the narrow sense, i.e. one finds Hilbert writing there literally "Existenz = Widerspruchsfreiheit".

[255] That Kronecker really did make such a claim seems plausible also in light of Hilbert's (earlier) report of his encounter with Kronecker in Berlin in 1888, where Hilbert reports Kronecker saying that "[...] with his characteristic everything was done (work from the year 1878). Harnack's work, too, as well as my future investigations were [according to Kronecker] in theory already contained therein." (Cod. Ms. D Hilbert 741:1, p. 2^r). Hilbert's report of his conversation with Kronecker reads, I want to say, like Hilbert felt that Kronecker was overall dismissive of Hilbert and his work; take, for example, Hilbert's report of the distinction Kronecker drew between "true invariants" and "literal invariants" (and Kronecker's claim that only the former were genuinely of any interest), which seems to have so preoccupied Hilbert that he went on to discuss it (as is reported on p. 6^v) with Weierstrass. Rowe remarks that Kronecker's remarks about invariant-theory must have felt for Hilbert "wie eine kalte Dusche": David E. Rowe. "David Hilbert (1862-1943)." In: *Die Albertus-Universität zu Königsberg und ihre Professoren. Aus Anlaß der Gründung der Albertus-Universität vor 450 Jahren.* Ed. by Dietrich Raschning and Donata v. Nerée. Vol. XXIX. Jahrbuch der Albertus Universität. Duncker & Humblot, 1995, pp. 544–552, pp. 545 sq.

[256] Rowe, "Klein, Hilbert, and the Göttingen Mathematical Tradition," p. 196 reports another instance where "Hilbert's self-confidence and plucky character" revealed itself when Hilbert defended the merits of his own work, in the case detailed by Rowe: against the criticisms Paul Gordan made of Hilbert's early work in invariant theory.

a substantive influence on Hilbert's thinking[257]. The most recognisable «Kroneckerian streak» in Hilbert's own thinking is Hilbert's eventual acceptance of a version of Kronecker's methodological maxim of decidability.

3.3.4.1 "Kroneckerian Decidability"

I want to stress both qualifications I added in the previous sentence: that Hilbert came to accept *only a modified version of* Kronecker's maxim of decidability, and did so only *later*. In fact, throughout the time documented by the notebooks, Hilbert appears to have rejected Kronecker's methodological demand, as the last entry I quoted in the previous section from the third notebook, where Hilbert says that "the Kroneckerian fanatics of finitude should have the rug pulled out from under them by my [axiomatic] method". That passage does not really explain what contrary methodology Hilbert is ascribing to Kronecker and his followers by calling them "fanatics of finitude".

Other passages, like the following, give indications to that end which are clearer, if only somewhat: § 176

> Concerning the finitude of executable operations in mathematics. Kronecker's view that the proof of this finitude is what provides a secure grounding for a concept is illogical. One could rather say that this proof is necessary so that anything falls under the concept.[cccxxxiv]

Hilbert here ascribes to Kronecker the position – albeit merely in order to immediately reject it himself – that the legitimacy of any concept depends on the "finitude" of the operations involved. What issues about "finitude" Hilbert takes Kronecker to have been concerned about, remains unclear in this passage[258].

As Hilbert portrays him in the notebooks, Kronecker appears to have held two distinct positions, which Hilbert does not always distinguish clearly. One is the position expressed in the passage, according to which Kronecker seems to have required "the finitude of executable operations", i.e. that the (calculatory) operations one relies on in mathematics are finite in something like the sense that each one can be completed within a finite period of time. It seems to me one may see in this idea a precursor to what later came to be called "effectiveness", i.e. the idea that the results of certain operations can be arrived at by relatively restricted calculatory means within limited time.

In the following passage, Hilbert attributes to Kronecker a (to my mind) slightly different claim, namely, that any legitimate mathematical calculation must involve only *a finite number* of elementary operations:

[257] Perhaps a perfect illustration of Hilbert's in this way ambivalent relationship with Kronecker lies in the fact that the report of his conversation with Kronecker is by far the longest of all reports found in the small journal held by the SUB under the signature "Cod. Ms. D. Hilbert 741:1": it spans four full pages, while most others are confined to a single, not infrequently half-empty.

In the first volume of the *Grundlagen der Mathematik*, Kronecker is described as "the first to emphasise the demands of the finite standpoint"[cccxxxiii], signalling, perhaps, a near total reversal in Hilbert's estimation of Kronecker. (However, since Hilbert notes in his preface that Bernays effectively wrote the *Grundlagen der Mathematik*, whether such points of detail reflect Hilbert's (changed) views as they were in the early 1930s, or are rather an expression of Bernays' views about the development of the discipline, remains uncertain.

[258] Mathieu Marion. "Kronecker's 'Safe Haven of Real Mathematics'." In: *Québec Studies in the Philosophy of Science. Part I: Logic, Mathematics, Physics and the History of Science*. Ed. by Mathieu Marion and Robert S. Cohen. Essays in Honor of Hugues Leblanc. Kluwer, 1995, pp. 189–215, p. 193 has emphasised that, however the issues are spelled out, the claim that Kronecker regarded "finitude" (in whatever sense) as *necessary* for the admissibility of concepts misconstrues Kronecker's position. Rather, Kronecker held that «finitary» concepts were *preferable*, but allowed for concepts with no «finitary» definitions to be admissible, if "only as a last resort" (as Edwards puts it when making the same point in Harold M. Edwards. "Kronecker's Place in History." In: *History and Philosophy of Modern Mathematics*. Ed. by William Aspray and Philip Kitcher. University of Minnesota Press, 1988, pp. 139–144, p. 144).

> Kronecker's requirement that everything must be carried out by way of a finite number of procedures is unclear e.g. let $F(a,b)$ be [either] $= 0$ or the smallest prime number of the form ax^3+b. Still it is not possible to decide whether ax^3+b denotes a prime number, even though only $F(a,b)$ operations would be required to that end.[cccxxxv]

Nothing determinate is asserted by the claim ascribed to Kronecker in this passage about the «operational finitude» *of each operation* itself, i.e. their effectiveness. Indeed, I take it this is precisely the problem Hilbert is raising in this passage: As long as no further conditions are imposed on the internal composition of F – e.g. on *which* (kinds of) operations may be used –, other than that the instructions for calculating F must involve only finitely many operations, cases like Hilbert's example here can arise: If Kronecker's demand that only finitely many operations are needed to calculate a given function is taken also to be sufficient for decidability, functions which cannot, by intuitive standards, be calculated mechanically qualify as decidable by including among the finite *number* of operations needed, for instance, *single* instances of such «procedurally opaque» *kind* of operations as appealing to chance by casting a die, or taking into account the result of deciding an undecidable question[259].

§ 177 This formulation of the problem suggests a solution: further conditions must be imposed on *the kind of operations* which are admissible in the construction of instructions for functions which are to be decidable.

As I said in the discussion at the end of the previous subsection, Hilbert seems to have eventually taken precisely such a position (as the passage quoted in footnote cccxxviii from the (later) Beilage to the notebooks shows), and with it accepted (a more precise and restrictive version) of Kronecker's methodological principle: That all thinking is exhausted by carrying out (in some finite sequence) a small number of *those select few operations* which happen, as a matter of fact, to be engrained in the fundamental workings of human cognition.

The claim that the fundamentally admissible operations are *limited in number* – and, furthermore, that they may even be *few in number* – is yet another claim. As far as I can see, Hilbert never explicitly attributes this claim to Kronecker, although he does attribute it to "Gordan and Klein" in the passage I quoted in endnote cccxxvii.

§ 178 There is some evidence that Hilbert changed his mind about the importance of Kronecker's idea of decidability *at some point in the late 1910s*: As has been noted by scholars, Hilbert's lectures in the late 1910s and early 1920s contain lengthy sections where Hilbert discusses Kronecker's views explicitly (though not altogether favourably).

I want to concentrate, instead, on another piece of evidence found on a separate sheet in the convolute held by the SUB under the signature "Cod. Ms. D. Hilbert 601", which – for reasons I will lay out in section 3.4.2 of the epilogue when discussing the rest of this «entry» – I believe to date from some time in the late 1910s (but which is an interesting assertion regardless of the time when Hilbert wrote it down)[260]:

> Language alone without experiment is incomprehensible. Therefore Kroneckerian decidability must also be present.

[259] The discussion on pp. 199f. of the official lecture notes for the 1905 lecture course prepared by Hellinger reveal that Hilbert saw a substantive difference between these two alternatives: Hilbert there regards citing the solution of a yet-unsolved problem as a legitimate means for defining a number, while casting a die is rejected as "not a determinate mathematical law".

[260] The following remark is found on a sheet in the convolute Cod. Ms. D. Hilbert 601: "Sprache allein ohne Experiment ist unverständlich. Also Kroneckersche Entscheidbarkeit muss hinzukommen.". The sheet in question has been numbered "31/32" by an archivist; the double-number is due to the fact that the sheet has been folded (like a greeting card).

In order to interpret this aphoristic remark as it is likely meant, one must not understand the word "experiment" too narrowly: In light of several entries found in Hilbert's notebook, it is unlikely that he had in mind mainly, or even primarily experiments in the sense of, say, experimental physics here. While Hilbert does occasionally in the notebooks reflect on the role of experiments in physics, he also uses the word "experiment" in a broader sense. § 179

In the third volume of the notebooks, for example, one finds an entry Hilbert speaks of "so to speak experimenting on paper" with sentential connectives:

> The points, lines and planes of my geometry will never be anything other than objects of thought and never have anything to do with real points, lines, planes. The situation is entirely different with my things "or", "and", "not". That is a spice unique in all of science which lends these investigations about thinking a peculiar allure that here suddenly the "and", "not", "or" have the meaning of the real "or", "not", "and", since we, as it were, experiment with these things on paper, while we never could experiment with points, lines, planes. This is the region where the line between, broadly speaking, sensual experience and purest thought intermix and one does as a logician the same thing as the geodesist does in geometry, the experimentor in electric theory, namely to experiment with the things "or", "and", "not" on paper.[cccxxxvi]

The idea that expressions are invested with meaning on account of «experiments» reappears here when Hilbert speaks of the sentential connectives of propositional logic having "the meaning of the real [sentential connectives]". The idea seems to be that because the sentential objects can be used directly – in the process of writing formulas –, one can pay attention, as one would in a physical experiment, to how these uses «behave» in certain settings and observe that when the sentential connectives of propositional logic are used, the results «behave» like the products of using certain sentential connectives of whichever spoken language[261]. But that is only possible because (and as long as) the act of writing down formulas takes a finite amount of time and involves a finite palette of letters and ways of combining them (failing that, it would not be possible to wait out the process to see how the product «behaves»). This leads Hilbert to accept Kronecker's principle of decidability, after all.

A similar remark can be found in the first volume of the *Grundlagen der Mathematik*, where the idea of "carrying out thought-experiments with visually present objects" is, in fact, used to characterise the central piece of philosophical terminology, "the 'finite' standpoint". That this characterisation of the finite standpoint owes at least some central traits to Hilbert's reflections on Kronecker is further suggested by the mention of "the executability, as a matter of principle, of processes" as one central feature of the finite standpoint[cccxxxvii].

Brief Interlude on the History of the Decision Problem

I want to emphasise that the sense of "decidability" at issue here is *broader* than the sense of "decidability" at issue in what Heinrich Behmann in 1922 called the "decision-problem" ("Entscheidungsproblem"). What I have labelled "Kroneckerian decidability" concerns, anachronistically speaking, the effectiveness of *all operations* relied § 180

[261] As I understand the contrast to geometry Hilbert is gesturing at in this passage, it consists in the fact that we cannot use the objects of geometry in a similar manner. Geometry is, as Hilbert says in the preface to the *Grundlagen der Geometry* the logical analysis of what Kant called the form of intuition of space and thus geometry depends on something, which it is not in our power to deploy entirely at will: Human beings can only manoeuvre themselves (and their surroundings) into a state and position where they expect certain shapes to appear to them, or, at best, construct in accordance with certain concepts some appearance in what Kant called "pure intuition", but they cannot simply manipulate the forms of intuition themselves.

on in mathematics (and, in the version Hilbert came to accept, also that these operations are finite in number, and that any given problem requires only using finitely many of them finitely often).

In sum, three distinct ideas are thus combined in the version of Kroneckerian decidability Hilbert comes to accept: (a) that there are *finitely many* (indeed, relatively few) *fundamental operations* at work in all of mathematics (or, at any rate, that mathematics can be so reconstructed as to take such a form); (b) that any operation (fundamental or defined) can be carried out in *finitely many steps*; and (c) that each individual step is, in some not precisely defined but rather narrow sense: «effective», i.e. does not, for instance, appeal to unsolved problems being solved.

§ 181 The decision-problem as articulated by Behmann, by contrast, revolves around the *more specific* issue whether the *specific* operation of exhibiting a (within a given axiom-system) viable proof for any given proposition is effective.

Hilbert appears to be well aware of this difference when, in the 1921/22 lectures, as recorded by Kneser, he formulates Behmann's decision-problem not as the problem of deciding *any given question* by a finite procedure (which is Kronecker's requirement), but whether "one can by way of a finite procedure decide whether a given formula is correct"[cccxxxviii]. Though Behmann is not mentioned in the discussion of the decision-problem in the *official* notes to Hilbert's 1922/23 lectures (where Hilbert first lays out the decision problem), his name appears in *Kneser's* personal notes of that lecture and thus was presumably mentioned by Hilbert in the lectures[cccxxxix].

§ 182 Behmann's decision-problem has, as far as I can see, no discernible roots in Hilbert's earlier work[262]. It was Behmann's original invention (see § 151 for what I take to be its roots in Behmann's dissertation); and if it depends on earlier work in logic, then it depends chiefly on the work of Schröder (especially Schröder's *Vorlesungen*)[263].

The most plausible candidate for a root of Behmann's decision problem in Hilbert's earlier work would, in my view, be Hilbert's discussion of normal forms in the 1905 lectures. But because Hilbert, at that time, conceived of his logical system not as a means for analysis the content of mathematical propositions, but rather only of *inferences*, it seems unlikely that he would have regarded the production and investigation of normal forms as a means of deciding upon the truth of individual propositions[264].

When Hilbert speaks of decidability in his 1918 article "Axiomatisches Denken" or his 1900 address "Mathematische Probleme" by discussing (in "Axiomatisches Denken") the "problem of *decidability* of a mathematical question by means of a finite number of operations"[cccxliii] and in the formulation of his 10^{th} problem in 1900 of "*exhibiting a procedure by way of which one can decide through a finite number of operations, whether an equation*

[262] Since Behmann played a key role in articulating for the first time the decision-problem in the second, narrower sense, I find nothing "curious" about the fact "that Hilbert does not mention the decision problem in the 1917/18 lectures" (as the editors remark in the introduction to: Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 42).

[263] Behmann himself emphasises Schröder's influence in the historical account of the discovery of his decision problem Behmann gives his first letter to Russell (dated 8/8/1922, accompanying an offprint of Behmann's 1922 article), where Behmann highlights the importance, in particular, of what Schröder called the "Eliminationsproblem" for his own work).

[264] Indeed, in *Grundlagen der Mathematik*, the sentential calculus is still introduced as serving mainly the purpose of "obtain[ing] a formalisation of logical inferences"[cccxl], and it is only with the predicate calculus that "the relation of *subject and predicate*" is said to be adequately appreciated[cccxli]. Frege had already observed, when compared his logical theory and Boole's, that Boole's very notation prevented him from even representing "conceptual content" adequately[cccxlii]. Frege does not venture any positive description of what it was that Boole, in contrast to Frege himself, *was* trying to represent and investigate. I am suggesting that it was mainly argumentative patterns, not the internal syntactic or conceptual structure of particular propositions.

in whole rational numbers is solvable"[cccxliv], the formulations Hilbert employs strikingly resemble the formulation Hilbert uses to characterise Kronecker's principle in the passage I quoted in endnote cccxxxv.

The problems about decidability Hilbert is discussing in 1900 and 1918 seem to be problems of whether certain results can be arrived at through a finite series of (certain kinds of) operations or whether certain concepts can be defined in terms of effective operations. Hilbert is not thinking about decidability in the specific sense central to Behmann's decision problem, i.e. the effectiveness specifically *of the provability-relation*.

Whether every mathematical problem can, in principle, be settled – as Hilbert was convinced it could – is yet § 183
another altogether different question with altogether different origins, which I will discuss in section 3.3.5.1. At least as Hilbert formulates it initially, say, in his 1900 address delivered in Paris, this conviction does not take a stand on whether every problem can be settled *in an effective manner*.

When Hilbert spoke about the solvability of all problems he meant, as I am suggesting to understand Hilbert's views, the solvability in principle by *any* sort of method[265], while when he spoke about the decidability of individual problems (by a finite number of operations) he meant to restrict attention only to (in some sense) constructive methods. In neither case was Hilbert thinking of the (constructive) decidability *specifically of the provability-property* for a precisely defined axiom system. To articulate that *specific* decidability question as a central problem within foundational studies was Behmann's original feat[266].

3.3.4.2 Hilbert's Conception of Number

Kronecker also exerted influence of another kind on Hilbert's thought. Hilbert was moved to accept certain claims § 184
by a desire to reject certain claims he attributed to Kronecker. One example of this negative yet formative influence Kronecker exerted on Hilbert is the philosophical conception of numbers Hilbert came to adopt, seemingly mainly in order to refute Kronecker's claims about (irrational) numbers, which Hilbert thought untenable.

A few entries early in the first notebook show Hilbert working out for himself a conception of irrational numbers § 185
starting from a strongly felt urge to oppose Kronecker, who wants "to squeeze [everything] through the eye of a needle", and his "completely arbitrary"[cccxlv] exclusion of irrational numbers.

Hilbert comes to the conclusion that "number is the fictional bearer of a rule, a product of thought, in other words, a moving cut"[cccxlvi] and, so, in particular, the irrational numbers "have nothing infinite (which is mathematical nonsense anyway) about them, but rather the fundamental sequence is only the bearer of a finite law, i.e. a_n must be found for a finite n always by way of a finite number of operations"[cccxlvii].

[265] The remark in Rowe, "The Calm before the Storm," p. 85 that the "underlying tension [is] between transcendental and constructive arguments" seems to me to accurately describe the issue Hilbert's conviction of the solvability of every problem leaves open. (Rowe, however, also maintains on p. 84 that Hilbert had in 1917 started "thinking about the so-called *Entscheidungsproblem*".)

[266] In light of the difference between Kroneckerian and «Behmannian» decidability, I find Sieg's remark that "[c]learly, the general problem of mechanically deciding mathematical questions had been mentioned already [before the winter term of 1922/23]" (Sieg, "Hilbert's Programs: 1917-1922," p. 111) misleading: Hilbert was previously concerned only with Kroneckerian, not Behmannian decidability; Hilbert was concerned with *effectiveness* only insofar as he thought about whether mathematical operations are given effective definitions, not insofar as he thought about whether *proofs* can be exhibited in an effective manner for a given proposition.

That Hilbert regarded this idea as a definitive answer to the doubts raised about "the existence of irrational numbers" is stated clearly in another entry later in the first notebook, where Hilbert writes:

> The controversy about this is without object given this definition: irrational number equals bearer of a law, i.e. if I am subsequently handed a law I can make all of my claims about it.[cccxlviii]

Hilbert goes on to spell this conception of irrational numbers out in more detail one page later:

> With respect to the concept of irrational number one must say: If a *sequence-law* is given, by means of which for any number n a number a_n can be found etc… Sequence-law is there to be defined as a system of calculatory operations (addition and subtraction of whole numbers suffices, since everything else can be reduced to this), which enable me in every case, using only a finite number of applications of subtracting and adding, to find for n the number a_n. This definition suffices, in any case, whenever a thing is proposed to decide on the basis of this definition whether the proposed thing is a sequence-law or not.[cccxlix]

This idea of number "as the fictional bearer of a rule" smacks of Kantianism. While Kant's views of the nature § 186
of number are, even by the dim standards of Kantian doctrine, obscure[267], one finds Kant making remarks which suggest such a conception of number, and in Hilbert's notebook other entries in relatively close proximity to this proclamation on the nature of number which suggest that, in fact, Hilbert was taking his cues from Kant, and so, that Hilbert can truly be considered – at a deeper level than has so far been appreciated[268] – a Kantian.

3.3.4.3 Kant's Account of Number, and Hilbert's Adoption of it

In a brief and compressed discussion of the foundations of mathematics found in the second main division of Kant's § 187
Kritik der reinen Vernunft, which is entitled the "transcendental doctrine of method", Kant states that

> the only synthetic knowledge a priori of things in general can be nothing more than the mere rule of synthesis of those things which may be given a posteriori in perception[ccli]

The paragraph from which this remark is taken begins with Kant emphasising how the basic elements of mathematics – which Kant identifies here as shape and number – can be "constructed a priori"[cclii]. Whatever the details, Kant clearly commits himself here to an account of number in which the idea of a rule (of synthesis) plays a central role.

[267] As noted by Daniel Sutherland. "Kant on Arithmetic, Algebra, and the Theory of Proportions." In: *Journal of the History of Philosophy* 44.4 (2006), pp. 533–558, p. 533, the reason for this obscurity may be the fact that Kant nowhere "gives a sustained and focused account of his mature views on mathematical cognition, forcing readers to glean what they can from disparate contexts". The scholarship on Kant's philosophy of mathematics is only of limited use for my present purposes, since it is devoted largely to either the role of what Kant calls "intuition" in his account of mathematics, or the role of Kant's account of mathematics in his theory of knowledge overall; the scholarship on Kant's philosophy of mathematics thus focuses on exploring *epistemological* issues about mathematics, and does not dwell in detail on how Kant may have wanted to *formulate* mathematical concepts, results or theories, and how Kant would have reconstructed some of them, and why. Kant's account of number is discussed only in pursuit of these other purposes.

[268] In my view, the supposedly Kantian streaks in Hilbert's thought pointed out so far, are in fact and in truth, fainter than they have been taken to be. As I discussed earlier (in footnote 96), the articles by Ulrich Majer, and Hallett & Magal build centrally on a reference to Kant in Hilbert's *1925 lecture* on the infinite; they are thus by design not about Hilbert's engagement with Kant's works *before 1925*. What is more, it seems to me that Hilbert's reference to Kant in 1925 itself does not accurately reflect either the impact of Kant's work on Hilbert's thinking overall, nor the origins of Hilbert's thinking about ideal elements in particular; (I want to point out in this connection that the discussion of ideal elements in the final chapter of Hilbert's 1919/20 lectures on *Natur und mathematisches Erkennen* make no mention of Kant's views of ideas, even though Kant is mentioned several times (on pp. 4, 16 and 63) in the course of the lecture). So far as the notebooks shed light on Hilbert's thinking about ideal elements, entries like the following suggest that this idea is rooted not in Hilbert's engagement with Kant (which is relatively amply documented in the notebook, as I will lay out in the following subsection, but never ties in with thoughts about ideal elements), but rather in the impression made upon Hilbert by the algebraic work of Dedekind; in entry Hilbert writes: "Just as Lie did with the concept of a group, I want to apply the concept of an ideal to all mathematical disciplines [...]"[cccl] (a similar claim about the central status of ideals in mathematics, as Hilbert conceived of it, was found in the entry I quoted earlier (in endnote cccxxii) where Hilbert positions himself in opposition to Klein).

The following entry, which can be found two pages after the entry about the concept of number, records in a very compressed form the engagement with Kant which, as I'll go on to suggest in the remainder of this subsection, lead Hilbert to the conception of number as the "fictional bearer of a rule". This subsection is dedicated to proposing a reconstruction of the chain of thoughts which may have led Hilbert from a reading of Kant's *Kritik der reinen Vernunft* to his conception of number. On p. 28 of the first notebook Hilbert writes:

> [The] concept of whole number is necessarily based on space and time. [The] concept of continuity is simpler than the concept of whole number. Kant is right.[ccliii]

While rather cryptic, this entry clearly shows that Hilbert's reflections on the nature of number at the time were, in fact, closely tied up with his engagement with Kant. Indeed, one can interpret Hilbert's mention of number being "based on space and time" here as a paraphrase of what Kant goes on to say about the concept of number in the transcendental doctrine of method: Kant there goes on to recall his distinction between forms of intuition and matter given in intuition[ccliv], and, on this basis, makes the following claim:

> With respect to the former [i.e. the forms of intuition, space and time] we can determine our concepts in intuition a priori by creating objects in space and time for ourselves by uniform synthesis by viewing them merely as quantities; [...] this is called the use of reason by construction of concepts [...].[cclv]

Kant picks up the label "use of reason by construction of concepts" once again (only slightly modified) at the end of the paragraph and there connects it to the concept of a number in the following passage:

> But to determine an intuition in space a priori (shape), to divide time (duration), or just to grasp the general [pattern] of synthesis of one and the same [thing] in time and space, and the magnitude of any appearance whatever (number) emerging from this [general pattern of synthesis], that is the business of reason by construction of concepts, and is called *mathematical*.[cclvi]

Interpreting Hilbert's cryptic remark about "the concept of whole number" as paraphrasing something like this particular Kantian doctrine – where number is something "emerging from the general pattern of synthesis" – can also explain why Hilbert follows it up with a remark about the concept of continuity being "simpler" than that of number: Whatever the details of Kant's account of number in the transcendental doctrine of method, number appears there – unlike what Kant calls "shape" and "duration" – as not directly based on "constructions" in space and time, but rather as "emerging from" such a construction somehow[269]. In contrast to how Kant apparently sees

[269]Earlier in this discussion (in the passage I quoted in endnote ccclii), Kant distinguishes constructions of concepts "alongside their quality (their shape)" from such which construct "merely their quantity (the mere synthesis of the equal and manifold ['das Gleichartigmannigfaltige'])", which likewise suggests that the construction of number Kant has in mind is somehow separate from simply constructing specific shapes in space or time. According to the interpretation offered by Friedman, *Kant and the Exact Sciences*, p. 119, which I think significantly clarifies this issue, in particular, the difference between arithmetic and geometry is "that geometrical construction necessarily starts from [...] given operations [...]; the serve as given fixed inputs,a s it were, for our general capacity successively to iterate any operation (...). In algebra and arithmetic, by contrast, we specify no such fixed inputs and concern ourselves only with the form of successive progression common to any and all iterative procedures.":

Daniel Sutherland has articulated a way of drawing this distinction using Kant's distinction between a "quantum" on the one hand and a "quantitas" on the other hand. Key to the distinction Sutherland draws is that quantitates, unlike quanta, are *determinate* magnitudes, that is to say: In case of the latter, but not the former, the question "How much, exactly?" has an answer. Since determination can, within Kant's framework, only be effected by the use of concepts, quantitates are distinguished from quanta because quantitates depend on cognition while quanta do not (Daniel Sutherland. "The Role of Magnitude in Kant's Critical Philosophy." In:

whole number, Kant does seem (as certain remarks found in an earlier section of the *Kritik der reinen Vernunft* indicate) to regard continuity as something that adheres directly to anything which appears in space or time; indeed, on the basis of the following passage one may surmise that the added conceptual complexity involved in the concept of a whole number, as Kant sees it, is that it presupposes a fixed unit[270], while continuity, which Kant also calls "fluent magnitude"[ccclviii] and apparently considers the primary sense of "quantity", does not:

> Any appearance whatever is a continuous magnitude [...]. Because any number must be based on a unit, so an appearance as a unit is a quantum and as such always a continuum.[ccclix]

I want to briefly sum up this digression into abstract Kantian doctrine from back to front: Given that Kant regards continuity as a property of all appearances, Hilbert's claim that continuity is conceptually simpler than the concept of a whole number makes sense only if, as Hilbert understands Kant, whole number is not directly a property of appearances. Kant presents just such an account of whole numbers in the transcendental doctrine of method, where Kant claims – whatever the details – that whole numbers emerge from grasping "the general [pattern] of synthesis", which I take it is also what Kant called "the mere rule of synthesis" in the first passage I quoted from the transcendental doctrine of method. I want to suggest that something like this chain of thoughts lies behind Hilbert's account of number as the "fictional bearer of a rule"[271], i.e. (as I understand Kant's theory), the object produced by acting according to "the mere rule of synthesis", which, because synthesis is *in actual fact* only ever carried out when some *particular* object is synthesised on the basis of sensations, is, in this sense, "fictional".

I should add that even if the entries I quoted from Hilbert's scientific diary in § 185, where Hilbert calls irrational numbers as "bearers" of (finite) laws represent Hilbert's conception of (irrational) numbers *at the time*, it is likely that he abandoned it around 1905 at the latest, since it is precisely such a conception of irrational numbers, which leads Julius König to claim to have refuted the continuum hypothesis (see the discussion in § 216, especially the letter from Cantor referred to in footnote 325); at the same time, *that* König took such a conception of irrational number for granted as not especially contentious makes it plausible, to my mind, that Hilbert subscribed to such a conception early on since it appears to have been more or less common currency in the late 19th century[272].

The entry I quoted at the start of this digression, which ends with Hilbert assertion that "Kant is right" is one § 188
of a series of entries seemingly documenting Hilbert's engagement with Kant's views on mathematics (among other

Canadian Journal of Philosophy 34.3 [2004], pp. 411–442, p. 434). In particular, the constitution of a quantitas involves, according to Sutherland, "The Role of Magnitude in Kant's Critical Philosophy," p. 432, "all three quantitative categories – unity, plurality, and totality" and "the schema corresponding to these categories is [natural] number". Since Kant elsewhere suggests that concepts generally, and categories in particular, are manifestations of "rules"[ccclvii] there a connection emerges between the account of number Sutherland attributes to Kant and Hilbert's account of number as merely "the bearer of a rule".

[270] See, once again: Sutherland, "The Role of Magnitude in Kant's Critical Philosophy," p. 428.

[271] I want to mention in passing a conjecture put forward by Sutherland, "Kant on Arithmetic, Algebra, and the Theory of Proportions," 557fn82, who claims that the views on arithmetic of Thomae, Lipschitz and Schröder were "were likely influenced by Kant's views and Schultz's defense of Kant" (unfortunately without discussing this connection in any detail).

[272] One might speculate, in the same vein as Sutherland's conjecture I cited in footnote 271, that the spread of such a conception which is more or less recognisably akin to Kant's is a testament to the influence of Kant's philosophy on 19th century mathematics; or, to the contrary, that Kant himself resorbed this conception of number from philosophically inclined mathematicians of his day and that König and others did so from people in the same tradition for more than a century after Kant. The latter hypothesis strikes me as more plausible, but I can state it here only as a conjecture because I have not had the time to study, e.g., Wolff, Euler, Kästner or the (somewhat later) Ohm, nor am I aware of any other evidence to *decisively* support this conjecture.

things), or, at least, mentioning them. I want to consider all of them in some detail now. Of these entries four are found in the first, two in the second and two in the third notebook (and two in the "Beilage") .

3.3.5 Kant and the Promise of Knowledge

In fact, the aspects of Kant's work which appear, in the different entries, to capture Hilbert's attention vary from notebook to notebook, that is to say, through the course of time.

One may speculate about whether Hilbert re-read (different parts of) Kant's work at certain points, or wrote about it from (increasingly distant) memory; another fertile field for speculation, in my opinion, surrounds the question how much Hilbert knew of the (by his time already sizeable) literature of Kant-commentaries, and, if any, which. I see no firm grounds for deciding these questions—which is not to say that I regard such questions as not worth asking or answering, I can just think of nothing in any way enlightening to say, myself. So I will content myself having posed these questions and focus, in this sub-section on surveying what Hilbert says throughout the notebooks about different aspects of Kant's writings and discussing some of them in some detail.

- The entries mentioning Kant in the first notebook show Hilbert interested first in the concept of number, as I have just discussed at some length, and subsequently in the status of mathematical propositions and, in particular, in the question whether they presuppose experience or not[273].

  I will discuss the entries reflecting on mathematics generally, and the role of experience, at some length after surveying the entries where Kant is mentioned in subsequent volumes of Hilbert's notebooks.

- In the second notebook one finds Hilbert comparing the theory of limits to Kant's theory of ideas, writing: § 189

  > The mathematical limit is, what in life and in philosophy are the ideas (according to Kant). [The] mathematical limit is the precise physical law, which only exists within us. We act as if this law held. That is necessary for the sake of simplicity.[cccl x]

  This passage shows that Hilbert, following Kant, conceived of the objects ideas purport to grasp – roughly along the lines I sketched earlier in § 73 – as not a part of the objective, «real» world, but rather, as objects of a pretence entertained "for the sake of simplicity".

  In combination with the discussion found in the last chapter of Hilbert's 1919/20 lectures, this remark suggests that Kant's conception of an idea is linked in Hilbert's mind with the *idealisation* (in the sense discussed in § 97), not with the reliance on *ideal objects*, which Hilbert there explicitly distinguishes from idealisation[ccclxi]. Although, as I said in footnote 268, Kant is not mentioned at all in the chapter of the 1919/20 lectures where Hilbert discusses the two sense of "ideal", Hilbert there uses the concept of a *limit*, which he linked with

[273] In a slight break of the pattern I am suggesting, where Hilbert's interest in aspects of Kant's work evolves over time, something like this concern seems to resurface in the third notebook, but does so in a more incidental fashion, where (on p. 95) Hilbert mentions "Kant's fundamental question: How is pure mathematics possible?", without, however, saying anything more about Kant's answer to this question, nor about Hilbert's own answer to it at the time he was writing this entry (III/b-171) in the third notebook.

Kant's conception of an idea in the notebook-entry above, as a metaphor for idealisation in physics, speaking of "the ultimate goal of a complete mathematical description of reality" as "a limit-ideal"[ccclxii].

It seems to me Hilbert's appeal to Kant's conception of idea in the 1925 lecture on the infinite was likewise meant to elucidate that status of the infinite as an *idealisation*: The claim which leads Hilbert to invoke Kant, that the infinite "does not occur in nature", is in any case more reminiscent of what Hilbert labels "idealisation" in the 1919/20 lectures than of what he there labels "ideal objects"[274]. However, I should say that in his 1925 lectures on the infinite, Hilbert appears, to an extent, to be running together attributes he used in 1919/20 to describe idealisation and attributes he used to describe ideal elements

I said above that there are two entries mentioning Kant in the second notebook, as, indeed, there are, but only this one is substantive. In the other entry[ccclxiii] Kant's name is mentioned only as that of somebody who "used mathematics as a point of departure", but nothing substantive is said about Kant's ideas or doctrines.

- In the third notebook, Hilbert shows interest in Kant's so called "antinomies" of reason, which is in keeping with Hilbert's broader concern with paradoxes of different kinds from (at least) 1905 onwards:

  > The Kantian antinomies appear as simple errors in calculation within the logical calculus. The question is thus only this: Where is the mistake? So it is with all antinomies, with which even ordinary life is rife, where one likes to trivially say that everything has two sides to it.[ccclxiv]

  This passage also shows that Hilbert saw the logical calculus as a means of «solving» paradoxes by showing their seemingly stringent arguments to be fallacious. I will return to this topic in the epilogue.

- The two remarks found on the loose leaves at the back of the third notebook, i.e. in the Beilage to the notebooks, attribute to Kant the invention of "impossibility-claims", so Hilbert writes:

  > Kant was the first to make impossibility-claims: "It is impossible to prove that god exists" and "It is impossible to prove that god does not exist" are 2 independent claims.[ccclxv]

  In the last entry found in the (Beilage to the) notebooks where Kant is mentioned, Hilbert stresses the fundamental importance of Kant's work for the development of philosophy:

  > The Kantian question, how is pure mathematics possible? is the first member in a chain of questions, which leads us to that higher standpoint, where philosophy will become [an] exact science.[ccclxvi]

[274] Contrary to what Majer and Hallett & Magal suggest, I thus want to maintain that Hilbert's claims about ideal elements are not rooted in Hilbert's engagement with Kant. I am sympathetic, by contrast, to the view presented by Hourya Benis-Sinaceur. "Du formalisme à la constructivité: le finitisme." In: *Revue Internationale de Philosophie* 47.186 (4 1993), pp. 251–283, p. 271 that Hilbert's use of the distinction of real/ideal is "un usage analogique" (the analogy being, as Sinaceur notes (p. 263) with Kummer's theory of ideal numbers, not Kant's conception of an idea (or an ideal) and is meant "à indiquer (par une comparaison au fond inadéquate) une différence dans le degré d'évidence des objets mathématiques" (p. 271). Indeed, as I take Sinaceur's remarks about the analogical usage of the distinction between "real" and "ideal" to imply, I am not inclined to think Hilbert held a "theory" of ideal elements in any firm sense of "theory", e.g. the sense in which I will speak about "Hilbert's theory of problems" in section 3.3.5.1 (though I don't mean to deny that one can (re-)construct such a theory on the basis of Hilbert's remarks).

> Indeed, Hilbert seems to have intended at roughly the time this entry was written (as another entry in the Beilage attests) to write a book tentatively entitled "Philosophy on the Basis of the Natural Sciences"[cccLxvii]. Needless to say, no such book ever appeared and, as far as I am aware, no recognisable drafts of it have survived among Hilbert's papers, if, indeed, any were ever produced.

§ 190 Among all the entries one finds in Hilbert's notebooks, where Kant is mentioned, the most interesting from a philosophical point of view, and also the most extensive, are the four entries found in the first notebook. I have already discussed the first one, where Hilbert claims that "Kant is right" about the concept of a whole number, at some length in the previous subsection. The remaining three entries in the first volume mentioning Kant read:

> In all other sciences [there is] matter, of which one knows not whence it comes, one knows only that it is there. Here is pure thought, pure philosophy. Kant was the first to feel this, but did not prove it; he has begun something, but not finished it.[cccLxviii]
>
> Read pure geometry: (1) Axioms of Geometry (Helmholtz, Grassmann, Riemann, Kant etc.[)] Which are the necessary postulates taken from intuition, on the basis of which everything later comes to be out of logic (schematism). What corresponds to the whole numbers in analysis, which do not suffice for the whole construction, once one has taken their sequence-laws from intuition?[cccLxix]
>
> Weierstrass is the Euclid of analysis, because he set down rigorously the foundations and presuppositions and reconstructed on their basis the whole proud construction of science. The presuppositions are nothing other than calculating with whole and positive numbers, which intuition teaches. Compare Kant, who held these facts to be a priori. One cannot reduce them further. That is to say, the claims of mathematics are certain in the same manner as calculating with whole numbers.[cccLxx]

These entries show Hilbert substantially accepting a Kantian view not just about the concept of whole numbers, but of mathematics overall. Before discussing at some length the shape of the Kantian view Hilbert may be accepting, whose outline remain rather hazy between Hilbert's blanket admission that "Kant is right" and the more reserved estimation (in the entry immediately after) that "[Kant] has begun something, but not finished it", I want to discuss one shining example of how deeply Kant's influence ran through Hilbert's thought.

3.3.5.1 Hilbert's Theory of Problems

§ 191 In close proximity to the first remarks where one finds Hilbert engaging with Kant's views early on in the first notebook, one finds the following entry:

> Assuming that there were no practical difficulties, is the human mind capable of solving all questions it poses for itself? Must it be possible to come to a decision about the quadrature of the circle, is there a well-defined question referring to things given to the senses which must have an answer and yet cannot be answered. Can one give a general decisive proof that all knowledge must be possible?[cccLxxi]

Some of the terminology Hilbert uses in this entry is unmistakably philosophical: The question Hilbert ultimately poses concerned the possibility of "knowledge" ("Erkenntnis"); before that he asks whether there can be a question concerned with "things given to the senses" ("sinnliche Dinge"), to which the human mind ("der menschliche Verstand") may be incapable of answering. It seems to me, furthermore, there is a good reason beyond the philosophical roots of the terminology to think that this entry continues Hilbert's engagement with Kant, in particular.

One can, I think, explain why Hilbert raises the case of a question about "things given to the senses" as potentially problematic if one views this discussion as situated before the backdrop of Kant's theory of knowledge, to which Hilbert had alluded in the entry discussing Kant a few pages earlier in the notebook, in the passage quoted in endnote ccclxviii. There Hilbert reports that Kant was the first to "feel", as Hilbert puts it, that while "all other sciences" relies on "matter", "[h]ere is pure thought". While Hilbert does not specify how the local metaphor is to be spelled out – i.e. where "here" is supposed to be –, in Kant's theory of the sciences there is only one science which does not incorporate any "matter" (i.e. anything given by the senses), namely, mathematics.

Kant articulates this aspect of his philosophy of mathematics on two occasions in the second half of the *Kritik der reinen Vernunft*, namely: in the section on the so-called "antinomy of reason" – the longest sub-section of the so called "transcendental dialectics"[275] – and in the "Transcendental Doctrine of Method", where Kant calls "mathematics a shining example of pure reason extending itself by its own means without the aid of experience"[ccclxxiii].

This singular, special character of mathematics as a "pure science of reason" is what Kant emphasises in sub-section 4 of the section on "the antinomy of pure reason", where Kant clarifies that pure mathematics is the only such science in the theoretical domain (the other such sciences are what Kant there calls "pure ethics", located in the practical domain, and what Kant calls "transcendental philosophy", presumably conceived as some sort of overarching science of reason)[ccclxxiv]. The hallmark of such a pure science of reason, as Kant characterises them in this sub-section, consists, precisely in that feature Hilbert focuses on in the entry I quoted[276], namely, that "their nature brings with it that every question which occurs in the[se sciences] must without fail be capable of being answered because the answer arises from the same sources as the question"[ccclxxvi].

It is, according to Kant's philosophy of mathematics, *because* mathematics does not rely on external matter given to the senses that it is capable of solving any problem that can be posed within its domain, and, indeed (as Kant himself already stresses), called upon to actually do so. It seems to me no accident that this summary paraphrase of Kant's view of mathematics bares a striking resemblance to Hilbert's famous dictum included in Hilbert's 1900 address to the international mathematical congress that "in mathematics there is no ignorabimus":

> This conviction of the solvability of any mathematical problem is a strong motivation while at work; we hear within ourselves the constant call: *There is the problem, seek its solution. You can find it by means of pure thought; because in mathematics there is no Ignorabimus!*[ccclxxvii]

The resemblance between this dictum and Kant's philosophy of mathematics is no accident, I contend: *Hilbert*

[275] That Hilbert studied Kant's so called "transcendental dialectics" closely is plausible not just because, as I discussed earlier in section 3.1.1, he took from there his Kantian epigraph for the *Grundlagen der Geometrie*, but also because of the remark found in the Beilage to the third notebook, where Hilbert alludes to Kant's discussion of proofs of the existence of god, which also forms part of the so called "transcendental dialectics". Further evidence that Hilbert studied, in particular, the section on antinomies is provided by the entry I quoted from the third notebook (p. 93), where Hilbert claims that "[t]he Kantian antinomies appear as simple errors in calculation within the logical calculus". It is worth noting that on the page before this remark one finds Hilbert substantively reiterating the Kantian view of mathematics he claims Kant was the first to "feel". So Hilbert writes on p. 94 of the third notebook: "Mathematics is thinking, pure thinking, nothing more, i.e. determinate, correct thinking, thinking in its purest form."[ccclxxii].

This re-affirmation, in combination with the remark about antinomies as calculation mistakes may indicate that Hilbert was, at this point, re-reading (parts of) Kant's *Kritik der reinen Vernunft*, or at least recalling them.

[276] When labelling mathematics a "pure science of reason", Kant goes on to illustrate this status of mathematics by alluding to the same example of a mathematical question Hilbert invokes also: Lambert's proof of the impossibility of squaring the circle ([ccclxxv]).

convinced himself of it as a result of reading Kant. One finds the process documented by the entries in the first of Hilbert's notebooks initially about Kant and then, subsequently, about the solvability of mathematical problems. Indeed, Hilbert's later famous dictum already appears fully formulated in the first volume of Hilbert's notebooks (and thus, taking into account the uncertainty surrounding the dating of these entries) «a good decade» before Hilbert announced it to the world in 1900. On p. 72 of the first notebook, Hilbert wrote:

> Mathematics is the only science for which there is no ignorabimus but rather we must say with the greatest expanse and with respect to all problems, however difficult: *noszemus.*[cccLxxviii]

This «missing link» between the entries concerned with Kant and this entry, where, like in Hilbert's 1900 address to the mathematical congress, Kant is no longer mentioned, is the second half of the entry I already cited from earlier in endnote cccii. After claiming that every mathematical problem takes the form of a claim about the coordination of objects, Hilbert goes on there to articulate the following general claim about all mathematical problems:

> Every mathematical problem is solvable. Anything the human mind can reach (by means of pure thinking without matter) can also be resolved. There is only one problem[277]. (e.g. quadrature of the circle. [...])
> The assumption of the possibility [of the solvability] is taken for granted from the outset.[cccii]

One finds here both the philosophical terminology Hilbert used in the entries documenting his engagement with Kant (as well as Hilbert's, and Kant's, default example of the quadrature of the circle), the formulation of the conviction one finds in the previous entry I quoted – i.e. that all problems are solvable – and even the compressed (Kantian) rationale later given (in even more compressed form) also in the 1900 address, namely, that the solvability of every mathematical problem is owed to the fact that mathematics is based on "pure thought [without matter]".

Surveying the entries I have been discussing, which lead, it seems to me, almost continuously from Kant's discussion of mathematics as a "pure science of reason" to Hilbert's dictum in the 1900 address *clarifies* the latter substantively by shedding light on the reason *why* Hilbert takes every mathematical problem to be solvable.

As these passages reveal, this is not a naively held article of faith for Hilbert, but rather a consequence of Hilbert's acceptance of a broadly Kantian theory of science in general, and mathematics in particular, according to which, because mathematical knowledge does not depend on any "matter", all relevant tools for solving mathematical problems, which reside in what Hilbert, following Kant, calls "pure thinking" ("reines Denken") are always at hand and, with them, so is the solution to every mathematical problem.

A much later entry (in the third volume) (on p. 104) furthermore reveals that this dictum later become linked in Hilbert's mind with his proof-theoretical efforts, which, Hilbert thought, would allow him to give a rigorous proof that every problem is solvable: "That there can be no ignorabimus in mathematics is probably provable by means of my theory of logic-arithmetic."[cccLxxix]; in a later entry one finds Hilbert using the phrase "proof of all proofs"[278]

[277] To make sense of this claim recall that this is the entry I discussed in § 167, where Hilbert claims that all mathematical claims are claims about co-ordination. All mathematical claims being thus of one kind, there is only one general problem each gives rise to different instances of, namely, the problem whether certain things are, in fact, co-ordinated with certain others.

[278] The phrase "proof of proofs" can also be found on sheet 22 in the convolute Cod. Ms. D. Hilbert 601, which, because they contain a lot of the same formulations as one finds on pp. 95f. of the third notebook, may date from roughly the same time.

in what seems like this sense: "Proof of all proofs: that one must always be able to find a proof!"[cccLxxx].

Accepting that every mathematical problem can be solved was for Hilbert only the beginning of a process of § 192
reflection on the nature, structure and order of (mathematical) problems generally[279], which he would pursue with varying intensity for years to come. The first entry where one finds such reflections occurs already in the first notebook in between the entries documenting Hilbert's engagement with Kant and the first entry where Hilbert formulated his later famous dictum, on p. 52f., where in a relatively long entry, Hilbert, so to speak, picks up where his engagement with Kant left him, i.e. having accepted the solvability of every problem:

> Mathematics has the advantage over other sciences that one can definitively say whether a question is dealt with. [...] For mathematics the danger lies in arbitrariness getting ahead of itself – corresponding to the fact that it is the mind by itself which sets the task, solves it and critically evaluates and values it according to its own taste and without external standards: Strive for large striking aspects and the prescription one imposes onto oneself as a duty, to view and compare every result with respect to the science as a whole. This yields a better way of working through the available stuff than if everyone just keeps digging through their own trash, which nobody else cares about. [...] Not dealing with particular questions is what matters (that is only an overarching motivation, a looming goal, exciting bait) but that the proud building of science rises before our mind's eye harmoniously and unified, as from a single block of stone[280]. A question F, for which the next superordinate question F^2 has been answered with yes, is to be esteemed less highly – perhaps a criterion for the importance of the question. In any case, there is a great difference even where the next superordinate question F^2 has been answered, whether I show only that something is possible by a finite number of processes (as with my proof for the finitude of the invariants of ternary forms) or whether I really state an upper limit for the number of processes, so that it is only a question of time and investing labour. Furthermore there is still the question whether there are problems in mathematics which cannot be dealt with in a predetermined small amount of time? etc.[cccLxxxii]

This entry appears like a kind of kernel for Hilbert's subsequent remarks discussing problems in general terms: All kinds of different questions about mathematical problems are raised here in no discernible order and without expressly separating one from the other. Hilbert later discusses some of them further, separately. Broadly speaking, three tracks can be distinguished in the series of entries where Hilbert discusses problems in general terms:

- Questions like the first issue raised, concerning the selection of problems. § 193

  > Problems must be difficult and simple, not easy and complex, so that one stands clueless before them at first – not such that keeping all presuppositions and conditions in mind already strains memory. (p 55)

  The entries where Hilbert seems concerned with the direction of mathematics, on account, not least of all, of how mathematicians spend their working hours are remarkable because they often take the form of similes or metaphors likening mathematics, and mathematicians, to various other fields, and characters, e.g. miners:

  > Many mathematicians are like miners, who dig deeper in the shafts, even though the way down to them has already caved in, so that no fresh air and no new life-force can be brought down to the place of work: they accomplish nothing for the science on the whole and finally come to a miserable end.[cccLxxxiii]

  Or mountaineers:

[279] Hilbert speaks of "the theory of questions posing problems" ("Theorie von Problemstellungen") in the third notebook[cccLxxxi].
[280] Hilbert's original metaphor refers to the production of sculptures by means of moulds: Hilbert says the building of science should seem as if it required only a single cast ("wie aus einem Guss").

> Whoever loves to undertake hikes in science and the fresh and healthy air should seek out the bright heights of arithmetic[cccLxxxiv]

Or those too easily pleased with less than is their due:

> When it comes to the selection of problems one must never make applicability a decisive consideration. Who would, after all, sell the right of the first-born due to the scientifically self-sufficient for the mess of pottage[281] of even a successful physical application or inspiration.[cccLxxxv]

- Then questions in the same vein as the remark about "superordinate problems" ("Oberfragen") to given § 194 problems, and, more broadly, about the order in which problems ought to be raised to be solved.

  Hilbert appears in several entries to have held the view that mathematical problems stood in some fixed order, and that this order was – at least often, perhaps generally – the inverse order of cognitive accessibility. E.g. on p. 50 (two pages before he introduces the terminology I translated "superordinate question" ("Oberfrage", on p. 51 in the entry immediately preceding the lengthy entry I quoted above):

  > Whenever a mathematical problem, I mean a question A has been posed, then one can deal first with the question whether this question A can be dealt with by way of a finite number of operations or not. Call this question B. From this in the same way question C, D,…. The current state of the science determines where this chain of questions breaks off. E.g. question A: Is there a C_6 with 11 separate ovals. can so far not be answered. But the question B stemming from this one can be. It is to be answered in the affirmative.[cccLxxxvi]

  Hilbert later presented at least this bit of his general reflections to the Göttingen mathematical colloquium: A relatively extensive report of a presentation by Hilbert on December 21 1910 about "mathematical problems", and about the "superordinate problem" of the solvability of a given problems is published in the *Jahresberichte der Deutschen Mathematiker Vereinigung*[cccLxxxvii].

  This idea of questions "stemming" from others subsequently gives rise in Hilbert's mind to the idea of an immediately superordinate question, which, Hilbert appears to claim, *must* be solved beforehand[282]:

  > One must think through a matter until one can say quite clearly why one is making no headway, i.e. which prior work, which can be carried out but is, for the time being, too difficult, must be dealt with beforehand in order to solve the problem posed, i.e. one must at least solve the next superordinate question $B = F^2$.[cccxc]

  Later, in the entry I referred to in footnote 279, Hilbert seems have generalised the idea of a «natural order» among problems to comprise not only what he called "superordinate questions" (i.e. questions asking,

[281] I am grateful to Gaëlle Fiasse and Stephen Menn, both of whom independently made me aware that this remark alludes to the story of Esau selling his birthright in Gen. 25:29–34.

[282] A remark found towards the end of the third notebook indicates that Hilbert did not continue to hold such a rigid view about the priority of superordinate questions. He writes (on p. 98 of the third notebook): "There are problems where the superordinate question is probably most easily answered by solving the problem itself[.] E.g. $p - q = 2$, where p and q are prime numbers, can be solved in infinitely many ways; but also to the contrary [i.e., I take it: cases where the superordinate problem is more easily solved than the problem itself], e.g., to exhibit the $10^{(10^{10})}$th numeral in the decimal expansion of π."[cccLxxxviii].

Connecting this entry with the entries in the first notebook is the example Hilbert used there of "a C_6 with 11 separate ovals", which reappears on p. 87f. of the third notebook as "the question whether a surface of 4th order with 11 separate ovals exists", of which Hilbert says there that it is "of the kind that the assorted superordinate question, i.e. the question whether that problem is decidable by a finite number of steps, is to be answered in the affirmative but these problem are [also] of the same kind as the problem of finding the $10^{(10^{10})}$[th] numeral in the decimal expansion of π^π"[cccLxxxix].

specifically, whether other questions can be solved, and how much calculatory work is required to that end), but also all kinds of (unspecified) "easier problems":

> Where the theory of questions posing problems is concerned, one can easily pose problems of any difficulty in number theory, which suggest no starting point. In that case one must first seek out the immediately adjacent easier problems.[cccLxxxi]

A few pages later, Hilbert continues the same line of thought as follows:

> The entirety of thought consists in finding simpler problems than presently posed[;] so, in a way, every problem is a problem of the calculus of variations, since the crucial issue is to find the problems which is the easiest among those not yet solved with the added condition that it must be required for the solution of the problem presently posed.[cccxci]

Finally, Hilbert offers the following analogy:

> Often, when a problem thoroughly resists one's attempts of solving it, that is because one has not yet solved another, more proximate one: Were one to attempt to take off one's socks and then take off one's boots it will be impossible, but in reverse order it can quite easily be done.[cccxcii]

- Finally questions like the last two raised, i.e. whether problems can be solved by way of finitely many steps. § 195

  This idea makes its first appearance in this general form[283] in the entry on p. 37 of the first notebook, from which I have already quoted twice. In between the two passages I have quoted, Hilbert asserts:

  > On should decide by a finite number of operations whether in any series there appears a 0 or whether all series consist of ones. I claim: Every such decision is possible by way of a finite number of operations (calculations). That is to say, there is no law for which this decision is not possible by way of a finite number of operations.[cccii]

Although many of the remarks where Hilbert discusses problems in general terms occur in close proximity to the § 196 remarks I have been discussing, which, I have suggested, document Hilbert's process of convincing himself, starting from Kant, of the solvability of all mathematical problems, Hilbert returns to these issues in both subsequent volumes of his so called "scientific diary" and there articulates something that increasingly takes the form of an abstract theory of problems. Early in the second volume one finds the following entry:

> Formulate a theory of calculation, i.e. investigate what the advantages of familiar arithmetical formulas and sentences (...) for calculating certain problems consist in and how one can estimate these advantages [themselves] by means of calculation. (Maybe by introducing certain elementary calculatory operations (addition, multiplication, exponentiation) as units and determines the number of units to be applied in different paths one treads in calculating. How many calculatory units, for example, are required to determine the so-and-so-manieth position in the decimal expansion of π? What is the quickest way of calculating $n!$? [...][cccxciv]

All of these entries, scattered throughout all three notebooks and thus attesting to an ongoing concern threading through many years, underscore once more and strongly how central the reflection on the internal organisation of mathematics and the connections between particular problems and results were for Hilbert.

[283] Earlier in the first notebook Hilbert raises the specific question to "prove that one can decide by a finite number of operations, how many, if any, integral solutions $\phi(x', y, \dots) = 0$ with integral coefficients has"[cccxciii].

Perhaps the most forceful testament to the importance these reflections held for Hilbert is provided by an entry found early on in the third notebook, where Hilbert reports that he had intended to include the task of developing a theory of problems "as the 24th problem of my Paris address"[284].

The following entry is also of some interest because it shows the sketches for a theory of problems found earlier in Hilbert's notebooks having been reconfigured into something more akin to what Hilbert would later develop under the label of "proof theory", although the emphasis and motivation falls on different issues here from where Hilbert later places it in his proof theory: Picking up on the idea sketched in the previous entry I quoted – of introducing certain "calculatory units" – Hilbert suggests using it in order to measure the complexity of proofs[285]:

> As the 24th problem in my Paris address I wanted to pose the question: [to find] criteria for the simplicity, or rather to give a proof of the greatest simplicity of certain proofs. In general, to develop a theory of methods of proof in mathematics. There can under given conditions only be one simplest proof. Generally, whenever one has two proofs for a proposition one must not rest until one has reduced each to the other or recognised precisely which presuppositions (or aides) have been used in these proofs: When there are 2 paths, one must not just walk these paths or search new ones, but chart the all the land which lies between them. [...] Since every process of adding is an application of the commutative law of addition – [and] that always corresponds to geometric propositions or logical inferences, one can count those and so decide, for example, when proving certain propositions of elementary geometry (Pythagoras, or about the special points in a triangle) very well which proof is the simplest.[cccxcv]

Hilbert's ongoing preoccupation with formulating a general theory of mathematical problems shows how deep Kant's influence runs through Hilbert's work: Hilbert's famous pronouncement that in mathematics there can be no lasting ignorance grew, as I take the entries from the first notebook to document, out of Hilbert's engagement with Kantian doctrine. But Hilbert's engagement with Kant is not limited to occasional inspiration. Hilbert also took an interest in decidedly philosophical questions such as whether (a certain portion of) mathematics is a priori.

3.3.5.2 Hilbert's Kantianism

Two other unpublished manuscripts found among the papers held by the SUB Göttingen testify to this aspect of § 197
Hilbert's interest in Kant: On the one hand, there is sheet 1 of the convolute held under the signature "Cod. Ms. D. Hilbert 601", which is entitled "Reichenbach: Relativitätsth."[286] and discusses at some length the impact of accepting the theory of relativity on Kant's doctrine of the aprioricity of knowledge in general and of mathematics in particular, coming ultimately to the conclusion that "Kant is thus refuted".

On the other hand, there is the manuscript held under the signature "Cod. Ms. D. Hilbert 606", which likely dates from the mid 1880s and records a discussion of Kant's theory of arithmetic concluding with the vindication of Kant's position. Merely recounting the basic facts about these two manuscripts as I just did suggests: (I) that

[284] I want to note, parenthetically, that on the assumption that Hilbert made this entry not too long after delivering his Paris address, it, and that fact that it occurs early on in the third notebook, provides a reason to think that the beginning of Hilbert's use of the third notebook ought to be dated closer to the year 1900 than to the year 1903.

[285] On the sheet inlaid in front of the first notebook, where Hilbert highlights various entries and topics, one finds a neat question-form formulation of Hilbert's unofficial 24th problem as "Is there a simplest proof?" ("Gibt es einen einfachsten Beweis?"). Hilbert raises this question explicitly in his 1918 talk "Axiomatisches Denken" (see the passage quoted in endnote cccxliii).

[286] Presumably the work in question is Reichenbach's 1920 monograph entitled "*Relativitätstheorie und Erkenntnis apriori*"; in any case, the majority of the sheet is dedicated to a discussion of Moritz Schlick's *Allgemeine Erkenntnistheorie* (published in 1918).

Hilbert held, or – depending on how one is inclined to think about the status of Cod. Ms. D. Hilbert 606 – at least sympathised with a Kantian conception of arithmetic early on in his life (likely starting in his student days in the 1880s, perhaps inspired by one of his teachers) up until (II) in the late 1910s, when his growing appreciation of the (philosophical ramifications of the) theory of relativity induced in him a change of mind.

I will now report the contents of both manuscripts in some detail and offer some conclusions on that basis.

The argument Hilbert rehearses in Cod. Ms. 601 is more sophisticated than claiming that the theory of relativity had shown non-Euclidean geometry to be empirically correct and thereby refuted Kant, who held Euclidean geometry to be the one and only empirically correct geometry. Rather, Hilbert is lead to the conclusion that Kant has been refuted by the following argument: Kant holds a change of the forms (of intuition and cognition) which shape experience to be impossible, but the theory of relativity showed Kant's conception of space to be incoherent and so a change of mind is needed which would be in principle impossible in Kant's framework[cccxcvi].

The manuscript held by the SUB Göttingen under the signature "Cod. Ms. D. Hilbert 606" likely dates from around 1885 (since it speaks of Frege's 1884 booklet on the *Grundlagen der Arithmetik* as "recently published") and may be a (partial) set of lecture notes taken by Hilbert of lectures about Kant's theory of mathematics given in all likelihood by Günther Thiele, who at the time held one of two professorships for philosophy at the university of Königsberg, where Hilbert studied. Whatever its nature – whether it contains lecture notes or a, more or less, original composition –, the manuscript shows how much philosophy – and how much of the 19$^{\text{th}}$ century controversy surrounding Kant's views on mathematics – Hilbert was, at one point, at least (remotely) aware of. § 198

A Brief Introduction to the Philosophy Department at Königsberg in the 1880s

Günther Thiele took up one of the two "ordentliche" professorships at the start of the winter-semester 1882/83[287]. § 199

The other professorship was at the time held by Julius Walter, whose research and teaching activities focused mainly on the history of (ancient) philosophy and on aesthetics. Although Walter regularly lectured on logic (e.g. in the summer-semester 1884), and in the winter-semester 1886/87 lectured on "history of modern philosophy" ("Geschichte der neueren Philosophie") and in every term, as was customary for professors, offered "Philosophische Übungen", the topics of which varied and were only in exceptional cases (e.g. for Thiele's Übungen in the Sommer-Semester 1890, which were cancelled before the end of the semester for lack of attendants) recorded in the official course-lists of the university, it seems to me unlikely that Cod. Ms. D. Hilbert 606 records a lecture by Walter – though I cannot exclude the possibility outright since I do not know anything about Walter's 1886/87 lectures, nor about the topics of his Übungen, where he could have discussed Kant's views on mathematics – because Kant's philosophy of mathematics was Günther Thiele's speciality: Thiele's dissertation (submitted to the University of Halle in 1869) was dedicated to the question "How are the synthetic judgements of mathematics possible?".

[287] For an account of Thiele's appointment at Königsberg, see: Christian Tilitzki. *Die Albertus-Universität Königsberg. Die Geschichte von der Reichsgründung bis zum Untergang der Provinz Ostpreußen.* Vol. 1, 1871-1918 [volume 2 remains unpublished]. Akademie Verlag, 2012, pp. 86f., and for an overview of Thiele's career, see the entry on Thiele in Tilitzki's "Catalogus Professorum", p. 633.

In principle, Hilbert's time as a student at the university of Königsberg also overlapped for about a year-and-a-half with the tenure of Thiele's predecessor, Richard Quäbicker (who, after years as an außerordentlicher Professor was named the successor of Karl Rosenkranz in 1880, but committed suicide on Mai 31st 1882[288], but Quäbicker could not possibly be the source for the text of Cod. Ms. D. Hilbert 606, since, by the time Frege's *Grundlagen der Arithmetik*, which is mentioned in the manuscript as "recently published", appeared, Quäbicker was dead.

Quäbicker may still have been of some interest to Hilbert because in the winter-semester 1880/81 (Hilbert's first semester at the university) Quäbicker lectured on "Philosophy on the Basis of the Natural Sciences" (like the title of Hilbert's later book-project; see page 137). However, owing to the fact that Quäbicker published almost nothing, the contents of this lecture are likely lost entirely in an unrecoverable past.

In any case, it is not known whether Hilbert attended either Quäbicker's, Thiele's, or Walter's lectures[289].

§ 200 The nature of Cod. Ms. D. Hilbert 606 and the circumstances surrounding its production thus remain obscure. I think that, on balance, of the considerations I will lay out below, the reasons against seeing Cod. Ms. D. Hilbert 606 as an original composition by Hilbert outweigh the reasons to do so, and so I will proceed on the assumption that the manuscript records somebody else's thoughts—with the following caveat: The manuscript contains several footnotes which I take to record Hilbert's personal views about particular issues addressed in the manuscript, *even though* I am assuming that the main text of the manuscript does not; indeed, and, of course, in case the manuscript *is* an original composition by Hilbert, recording Hilbert's own opinions, all the better for my purposes of using it as a source to for inferences about *Hilbert's* philosophical stance.

The strongest reason in favour of ascribing the whole text to Hilbert, in my view, are the substantive revisions and changes of formulation throughout large parts of the manuscript, which are hard to reconcile with the hypothesis that Hilbert is merely recording somebody else's words. The main reason to doubt that this is an original composition by Hilbert is the fact that one finds in this text a broader and more thoroughgoing discussion of philosophical sources – ranging from Kant's own various works, through the critical comments about Kant made by Herder and Hegel, to F.A. Lange and Friedrich Ueberweg, and finally Hilbert's near-contemporaries Robert Zimmermann and Frege – than one finds in any other publication or (unpublished) manuscript of Hilbert's.

To put the matter into proper perspective, think, for example, of Hilbert's 1919/20 lectures on *Natur und mathematisches Erkennen*, which are, by Hilbert's standards, quite philosophical *in ambition*. In substance, however, these lectures still concentrate on mathematical details. By contrast, Cod. Ms. D. Hilbert 606 is *entirely and*

[288] See: Tilitzki, *Die Albertus-Universität Königsberg*, p. 86; I have not been able to corroborate Tilitzki's report that Quäbicker committed suicide: the university-files I have seen leave the manner of his death unspecified and speak only of his "sudden passing".

[289] Blumenthal mentions in a footnote (Otto Blumenthal. "Lebensgeschichte [David Hilberts]." In: David Hilbert. *Gesammelte Abhandlungen. Analysis, Grundlagen der Mathematik, Physik, Verschiedenes, nebst einer Lebensgeschichte*. Vol. 3. Springer Verlag, 1935, pp. 388–429, p. 389n1) that he possessed a list of all lectures Hilbert attended as a student of the university of Königsberg. Since unfortunately Blumenthal did not reproduce this list in full, and the lists of attendees for lectures held in Königsberg during those years, which were archived as part of the files involved in processing the payment of lecturers by the university's so called "Quästur", seem – save for one or two preserved by chance – not to have survived the collapse of Eastern Prussia, it is no longer possible to gain a full view over which lectures (in philosophy, mathematics, or any other subject) Hilbert did or did not attend at the university of Königsberg.

exclusively focused on philosophical issues; technical, mathematical details are not touched upon *at all*[290].

Of particular interest for my present purposes is a footnote on the page numbered "3", which reads: § 201

> Geometrical propositions, too, belong, in my opinion, to this group, since the properties of our space in contradistinction to other conceivable manifolds can only be extracted from experience[;] compare Riemann's *Collected Works*[:] "On the Hypotheses which underlie Geometry". Accordingly, the Kantian claim of the aprioristic nature of mathematical judgements will have to be restricted to arithmetical [judgements].[cccxcviii]

A substantive reason to think that *at least this* footnote records Hilbert's own views is that the text recorded in this manuscript never uses the first person – it always expresses itself «neutrally» in phrases like "The investigation so far undertaken shows..." – while this footnote claims to state how things are "in my opinion".

While rejecting the claim made by Kant about the status of geometry (as, like arithmetic, based somehow on a "construction of concepts"), and thus diverging from Kant's own position with respect to these issues, this footnote – assuming it expresses Hilbert's own view – shows Hilbert explicitly accepting "the Kantian claim of the aprioristic nature of *arithmetical* judgements"[291]. That is, in any case, the position at which the whole manuscript eventually – after an extended discussion and refutation of criticisms of Kant – arrives at (on page 3'^{v}), where the final result of the discussion recorded in the manuscript is summarised as follows:

> As has by now been justified repeatedly we are completely in line with respect to [the question about the nature of arithmetical judgements] with Kant's conception, which declares the sentences $7+5=12$ to be synthetic. The higher arithmetical and algebraic operations also rest essentially on the same constructing and thinking [rooted] in sensual intuition, as does the lower kinds of calculation[292].[cd]

Comparing this summary with the third of the three entries discussing Kant in the first notebook (see § 190) reveals, in any case (that is to say: also in case not even the footnotes in Cod. Ms. D. Hilbert 606 can be attributed to Hilbert himself), that Hilbert was inclined to accept both of the claims made here.

Another aspect of the philosophical position implicit in this entry, as well as the second one I quoted there is that the basic principles of mathematics are said to be something "taken from intuition"[ccclxix] or as something "which intuition teaches"[ccclxx]. As I understand these compressed formulations, they do not mean to assert that arithmetic is based on intuition in any firmer sense than what Kant calls "the use of reason by construction of concepts" (see the discussion on page 133); I believe, based on my understanding of Kantian theory, that the reference to "schematism" in the second entry I quoted (in endnote ccclxix) further supports this interpretation.

[290] I want to cite, in this connection, an entry from the second notebook, where Hilbert expresses his discomfort with discussing subject matters he did not thoroughly grasp: "Talking about general topics I am not fully in charge of is not a pleasant state for me and so I enter into this state only in an extreme emergency."[cccxcvii]. If Hilbert held an attitude like this throughout his life, it would be hard to imagine him *composing* a manuscript so completely philosophical in character as Cod. Ms. D. Hilbert 606.

[291] One may speculate about why Hilbert was inclined to accept the aprioristic and synthetic nature of arithmetic. In this connection I want to cite an observation made by Charles D. Parsons. "Kant's Philosophy of Arithmetic." In: *Mathematics in Philosophy. Selected Essays*. Cornell University Press, 2005, pp. 110–149, p. 131, that Kant was moved to declare arithmetic synthetic because it involved existence-claims which, according to Kant's conception of existence, could not possibly by analytic. One may speculate that because in Hilbert's view arithmetic was the one mathematical discipline that must be presumed to have a realisation, while realisations of other disciplines can be constructed on the basis of numbers, it involved ineliminable existence presuppositions, and must therefore be synthetic. Friedman, *Kant and the Exact Sciences*, p. 117 remarks, in the same vein, that Kant regarded mathematics as synthetic because "I need assurance that a number [...] can be constructed (for us, that it exists)".

[292] One reason to think that this manuscript bears the marks of substantive influence by Thiele is that one finds Thiele asserting this claim that all mathematical operations alike are based "on the same constructing in sensual intuition" in his 1878 *Grundriss der Logik und Metaphysik*, where Thiele says that "the higher forms of operations involve no new categories"[cccxcix].

§ 202 To clarify how Hilbert seems to think of the role of intuition in mathematical theorising, and contextualise his views in what seems to me their proper historical context, consider the following remark:

> Needless to say intuition has no significance for abstract thought. *Convincing oneself of the truth* of judgements is based entirely on the essence of concepts and is that not only in metaphysics, but even in mathematics. Whoever cannot get away from the intuition which accompanies thought should study absolute geometry, or the investigations of Riemann and Helmholtz about the facts underlying geometry. These investigations are, as we may put it, the endeavour of factorising the fundamental intuitions of geometry into *concepts*, to make its basic doctrines, which are otherwise taken to be self-evident, appear in the style of abstract mathematical (analytic) thought as assumptions not capable of further proof; they are, in fact, the beginning of an attempt to make apparent the scaffolding of categories ["Kategoriengerüst"] which is fundamental to spatial intuition.[cdi]

In substance, I find this passage strikingly reminiscent of Hilbert's methodological aphorism from the introductory remarks of his *Grundlagen der Geometrie*, which I quoted earlier (in endnote clxix), i.e. that the task of geometry consists in "the logical analysis of spatial intuition"; the last sentence of this passage even resembles Hilbert's own formulation some, or rather, a mixture of that formulation with Hilbert's metaphor of theories as architectural structures of concepts. This quotation, however, is not taken from any work or manuscript of Hilbert's, but rather from Günther Thiele's *Grundriss der Logik und Metaphysik* publsihed in 1878[293].

In fact, Thiele's *Grundriss der Logik und Metaphysik* is in its entirety an effort to "make apparent the scaffolding of categories" which enter into the construction (by any given subject) of the objective world (experienced by that subject). Thiele eventually comes to the conclusion that all objectivity is from a philosophical point of view to be accounted for as the result of deploying certain categories in pursuit of the systematisation of the various impressions human beings have of whatever surrounds them. In § 85 of his *Grundriss* Thiele writes:

> The variety and persistence of the objective world must be *recognised* for what it is: [it is] sensation systematised and made self-sufficient, and the dynamic whole must through a strengthening of its unity and the possibility of internal variety and liveliness compatible with that [unity] find stability in itself [...].[cdii]

§ 203 I quote this summary statement not as the starting point for a detailed discussion of Thiele's metaphysics, but to underscore the resemblance of Thiele's views with certain methodological principles Hilbert appears on occasion disposed to accept. This particular passage brings into focus two further parallels, beyond the agreement over the role of intuition for geometrical theory construction which came to light in the previous passage I quoted from Thiele: The first of the parallels I see lies in the fact that Thiele – like Hilbert, as I proposed to understand his approach in section 3.2.3 – sees the role (mathematical) concepts play as "systematising" something that is given antecedently (sensation in Thiele's case, facts of knowledge for Hilbert).

The second parallel between Hilbert's views and Thiele's is less apparent on the surface, but it will, I hope, come more clearly into view after saying a little more about what Thiele means by "the dynamic whole": Thiele is

[293] That Hilbert was acquainted with this book can be regarded as certain for the same reason as I cited (in footnote 108) to document Hilbert's acquaintance with Erdmann's book on the foundations of geometry: Hilbert mentions Thiele's book in a short list of references to philosophical works commenting on geometry. In light of the fact that Thiele's book, unlike Erdmann's, is not *focused* on geometry, or even on mathematics, it seems to me Hilbert must have, in referring to Thiele's book, been thinking especially about the section containing the above quotation (§ 17), which is one of the few sections (along with § 64) where Thiele discusses geometry.

of the view that any human being will eventually be forced – by the dynamics inherent in the process of acquiring more and more refined knowledge, which Thiele's book is dedicated to tracing (presumably modelled on Hegel's *Phänomenologie des Geistes*) – to abandon the conception of the world as a system of independent objects which occasionally interact with one another and come to accept, instead, "that [any two objects and force between them] are only isolated, parts of an overarching dynamic whole, which has a single essence, in and by which [the force between any given things] has its basis and determination"[cdiii].

One finds Hilbert expressing a similar view – that, ultimately, i.e. in light of a fully developed science – the world would appear completely unified in the third notebook, where Hilbert writes:

> We carry unity into nature by seeking to rule a certain amount of facts (e.g. geometric facts) by means of *one* system of axioms; [...] in truth there is only a single science, everything is connected.[cdiv]

I do not want to claim that Hilbert is *taking* this view from Thiele (I am, in fact, inclined to think so, but have not found definitive evidence to support this hypothesis)[294]. These parallels serve me, rather, to make a broader point: I take them to show how *entrenched Hilbert was in the philosophy of his time*, from which at the very least Hilbert's conception of geometry as a "logical analysis of spatial intuition" certainly derives (and in this respect I take the influence of Thiele, in particular, to be very likely), as well as potentially also on Hilbert's conception of the task of science and mathematics – as systematising and unifying disparate facts – more broadly[295].

Summary

The outlines of Hilbert's early foundational project were, as the epigraph of Hilbert's 1899 *Grundlagen der Geometrie* § 204 signals, informed by Kant's views on scientific methodology.

Hilbert seems to have been struck by various remarks Kant makes about mathematics and the sciences *later* in his *Kritik der reinen Vernunft*, and appears to have studied the parts entitled "transcendental dialectics" and "transcendental doctrine of method", in particular, rather closely: The first eventually served Hilbert as the source for his chosen epigraph at the start of his *Grundlagen der Geometrie*, while from the so called "transcendental doctrine of method" Hilbert seems to have adopted his philosophical conception of number[296]; and, I believe, it is

[294] Another plausible influx from Thiele into Hilbert's notebooks, which also isn't clearly recognisable as such, is Hilbert's claim in the first notebook that "Kant has started something, but not finished it"[ccclxviii], which is in perfect agreement with Thiele's summary judgement about Kant's philosophy in his earlier monograph *Kant's intellektuelle Anschauung als Grundbegriff seines Kriticismus* (published in 1876), where Thiele concludes that Kant's conception of a "thing in itself" is evidence that Kant "did not submerge himself deeply enough in the spirit of [his] critical philosophy"[cdv].

[295] Last but not least I want to point out that the conception of irrational numbers (as "fictional bearers of a rule") Hilbert can be seen adopting in the first notebook was also articulated by Thiele in his *Grundriss der Logik und Metaphysik*, where he wrote: "An irrational number has *a completely determinate value* [...] which is *exhaustively* defined only by either the law governing the formation of a series or by the expression which is developed in an infinite sequence"[cdvi].

[296] This discussion thus bears out a remarkable prescient conjecture by Abrusci, "Per una caratterizzazione del programma hilbertiano," p. 317, who emphasised three sources for Hilbert's conception of mathematics: Hilbert's mathematical work, Hilbert's preoccupation with the issue of truth in mathematics, *and* "le influenze filosofiche presenti nell'ambiente mathematico tedesco della fine dell'800 e dell'inizio del'900". In Vito Michele Abrusci. "Hilbert e i fondamenti della mathematica. Il methodo assimatico." In: *Le matematiche* 55 (Supplement 1), pp. 127–142, p. 136, Abrusci notes the similarity between Hilbert's position and (Neo-)Kantian positions, which Abrusci sees in the presupposition that mathematical knowledge is a fact to be accounted for (as opposed to being corrected according

also on account of the influence of Kant's conception of mathematics as a pure science of reason (as I discussed in § 191) that Hilbert adopted the conception of mathematics itself which he expresses in the third notebook:

> Mathematics is thinking, pure thinking, nothing more, i.e. determinate, correct thinking, thinking itself[ccclxxii]

This conception of mathematics as "pure thinking" can, it seems to me, neatly be described, after everything I have said, by stating two philosophical assumptions, which I believe the so called "scientific diary" shows Hilbert accepted, and relied on as much positively in the development of his own position – about number, about mathematical problems, about mathematics as a whole – as negatively in his criticism of formalism, Frege and Kronecker:

- There are fundamental cognitive operations engrained in the human mind.
- Mathematics ought to be be based completely and exclusively on these fundamental operations[297].

One finds it expressed in the following passage from Hilbert's 1928 "Die Grundlagen der Mathematik": § 205

> The formula-game [of mathematics] proceeds according to certain determinate rules, in which the technique of our cognition expresses itself. These rules constitute a closed system, which can be discovered and stated in a final form. The basic idea of my proof-theory is nothing other than describing the activity of our mind, to start protocollising the rules according to which our thinking in fact proceeds.[cdvii]

To the extent that Hilbert accepted the two assumptions I just stated, Hilbert can, it seems to me, be rightly considered to have been a logicist. The sense of the word "logicist" I have in mind is somewhat broader than the sense usually associated with the label "logicist" when applied to, say, Frege or Russell.

Indeed, the passage I just quoted brings clearly into focus what I take to be the decisive difference between what I am suggesting to call Hilbert's "logicism" and that attributed, for instance, to Frege, Dedekind or Russell: Unlike Frege, Dedekind or Russell, Hilbert never in his published work – and, apart from the early notebook entries I discussed in section 3.3.2, also not in any unpublished manuscripts I am aware of – espoused *a single list* of fundamental cognitive operations *as definitive* either of the subject-matter of logic or of the operations underlying mathematics[298], but he saw it as an vital part of mathematical work throughout his career to investigate the «logical» *deductive structure* of mathematical theories, simplify and systematise it, and as the result of such an investigation identify the fundamental cognitive operations at work in mathematics[299]; and it will be recalled

to philosophical principles, as, according to Abrusci, the intuitionists intended): "e dunque non meraviglia che Hilbert negli anni venti abbia espresso (su stimolo di Bernays) chiare posizioni kantiane o neo-kantiane". While I concur that Bernays had strong philosophical leanings, and that these very likely had an influence on Hilbert's later writings, I have been working throughout my discussion of Hilbert in this chapter to substantiate this claim in a broader sense than Abrusci apparently intends: Hilbert *himself* had – and from very early on in his career – *his own* (neo-)Kantian leanings (and I believe these were not in every respect congruent with Bernays').

[297] In his article "Hilbert's Epistemology," Philip Kitcher likewise localises a close kinship between Kant and Hilbert because "[t]hey identify a process which we can recognize as occurring in our mental (mathematical) life and hypothesize that this process is the basis of our mathematical knowledge" (Philip Kitcher. "Hilbert's Epistemology." In: *Philosophy of Science* 43 [1976], pp. 99–115, p. 111).

[298] Abrusci, "Per una caratterizzazione del programma hilbertiano," p. 328 emphasises similarly that while Hilbert's overall approach to foundational issues remains fairly constant throughout his career, "mutevole è la posizione di Hilbert sul probleme degli stumenti e delle evidenze" by which foundational work is to be accomplished. Abrusci concludes (p. 335) "Il finitismo è per Hilbert, comunque, sempre il punto minimo di stasi nel regresso dei mezzi da usare nelle prove di non-contraddittorietà.".

[299] It seems to me that this is approximately what Michael Detlefsen labelled "Hilbert's [later] formalism" in his eponymous article, where he concludes thus: "What emerges from all this is an idealistically oriented formalism that is committed to a search for the

that the logicism of Frege became embroiled in trouble mainly because of the *particular logical operations* (e.g., unrestricted class-comprehension) to which they held all of mathematics to be reducible.

It seems to me one can rely on passages such as the one I just quoted to attribute to Hilbert himself what Georg Kreisel later called "the modified Hilbert-programme", which Kreisel describes as follows:

> [I]nstead of having a single kind of elementary reasoning whereby we understand the use of transfinite symbols, there will now be methods of reasoning involving a hierarchy of conceptions such as, e.g. more and more abstract conceptions of a "construction", and we have a hierarchy of Hilbert programmes of *discovering the appropriate complex of such methods which is needed for understanding the use of transfinite symbols in given systems*[300]

In fact, the above passage also testifies to the fact that not even Hilbert's later so called "finitist standpoint" § 206 constitutes a renunciation of this basic philosophical position: Hilbert came to hold only that one should focus on cognitive operations *whose results were accessible to intuition*, but he still held that the operations in question were *cognitive* in nature. In this connection I want to quote from Hilbert's 1925 lecture "Über das Unendliche":

> And has content-reliant logical inferring anywhere deceived us or left us hanging in cases where we applied it to real things or events? No—content-reliant inferring is indispensable. It was deceptive only when we admitted abstract concepts at pleasure; we applied content-reliant inferring inadmissibly then, i.e. we have not taken duly into account a necessary precondition for the application of content-reliant inferring. And by recognising that such [preconditions] exist and must be duly taken into account we find ourselves in agreement with the philosophers, especially with Kant[301]. Kant already taught – and indeed this constitutes an integral component of his doctrine – that mathematics has safe content independently of all logic and can thus never be given foundations by logic alone, which is why the attempts by Frege and Dedekind were bound to fail. Rather as a precondition for the application of logical inferences and for the execution of logical operations something is already given in representation: certain extralogical concrete objects, which are there as immediate experiences before all thought[cdx]

What I find striking about this passage is the formulation also found in the text for Hilbert's 1924/25 lecture course: that the extra-logical objects presupposed for correct thinking are given "as immediate experiences" ("als unmittelbare Erlebnisse"). This formulation is striking because of its philosophical character, but more striking because, upon reflection, it makes little sense: One would rather expect the concrete extra logical objects to be *the objects* of certain experiences (sense-perceptions), not these experiences themselves. Now, it is possible that this is just a «slip of the pen» or an inaccurate formulation, but the fact that it appears unchanged in the 1925 lecture after it already appeared in the text of the 1924/25 lecture course suggests that it was not either kind of mistake[302].

'forms' of *thought*." (Michael Detlefsen. "Hilbert's Formalism." In: *Revue Internationale de Philosophie* 47.186 [4 1993], pp. 285–304, p. 302). I want to emphasise, to ward off misunderstanding arising from projecting into this conclusion a quite different but, I believe, more wide-spread sense of "formalism", which is so called not because of the centrality to it of "*forms* of thought", but of *formulas* assimilates the position to nominalism and considers as central the question whether the objects of investigation are «concrete»; where the word "Formalismus" occurs in Hilbert's own writings, it does so exclusively in this sense[cdviii]. Indeed, even Detlefsen feels compelled to remark in such a spirit that the "forms of thought" he is speaking about "are of a fundamentally syntactic character" (p. 303), but, as far as I can make it out, "syntactic" in Detlefsen's usage means not "concerned with written expressions", but rather "precisely defined", though this is far from clear: The idea is introduced on p. 298 to paraphrase Hilbert's demand that "logical relations" be formalised. "Formalised" there is opposed to "being contentually operated with" and, in effect, seems to call for no more and no less than the statement of precise rules governing the mechanical use of logical connectives.

[300] Georg Kreisel. "Hilbert's Programme." In: *Dialectica* 12 (3-4 1958), pp. 346–372, p. 349.

[301] This passage, like much else (especially in the first half) of the 1925 lecture, seems to stem in part from a passage in Hilbert's 1924/25 lecture-coures "Über das Unendliche", where Hilbert likewise speaks of "necessary preconditions for logical inferring" and goes on to speak, as here, of "certain extra logical concrete objects" given as "immediate experiences", but this mention of Kant has no parallel in the text of the lecture course[cdix].

[302] The formulation, in fact, *also* appears in a more condensed reworking of the above passage incorporated into the opening remarks

§ 207 Instead I want, in closing, to point out one way of making sense of the formulation as it stands, and at the same time re-enforce a historical conjecture I made in the last section: In his early *Kant's Intellektuelle Anschaung als Grundbegriff seines Kriticismus*, Günther Thiele developed a theory of what he calls "intellectual intuition", according to which the objects of such intuition are indeed identified with the experiences themselves:

> Only then is an intuition truly such that "*it itself* guarantees the existence of the object of intuition" when nothing more must go along with this intuition, *no creative power external or internal to it* need be active in order for it to be a subsisting object, when the intuition *immediately as such* is a real existing object, when it itself immediately is its object, when being and knowledge are identical[cdxii]

I want to suggest that Hilbert may in the 1920s be thinking of intuition along the lines sketched decades earlier by Thiele (because, as I suggested in the face of the manuscript Cod. Ms. D. Hilbert 606, Thiele shaped the philosophical views of the young Hilbert); and that, consequently, one ought to be very careful about defaulting to an interpretation of Hilbert's use of the word "intuition" as "perception" (and of the word "concrete" as coextensive with "physical"). As far as I can see, Hilbert never applies to these "concrete extra logical objects" predicates like "sensible" ["sinnlich"] or "perceptible" ["wahrnehmbar"] or visible ["beobachtbar"], so there is, in fact, no decisive textual grounds for taking Hilbert to mean that these objects are material objects of sense-perception, except by begging the question and supposing that this is what "concrete" must mean[303].

This misstep is especially tempting because Thiele's conception of intellectual intuition allows one, unlike Kant's own conception of intuition[304], to make all the same claims about it(s objects) as one would make about perceptual objects, without, however, having to deny that these objects are *products* of the human mind (just not of cognition, but rather of certain other operations of the human mind).

This brings me to the conclusion of my discussion of Hilbert. To summarise it in the spirit I have been conducting it I shall quote from the introductory section of Thiele's later study of Kant, where Thiele summarises his views on philosophy as follows (and proceeds, in a footnote, to add a string of references to his earlier *Grundriss der Logik und Metaphysik*, of which this passage thus appears to be a retrospective summary):

> The Object and content of philosophy is not just logic, [philosophy] must rather investigate *reality overall*, and must in each specific science proceed from the *factual*, it must attempt to comprehend *what is given in experience* in a unified whole. But processing the factual into a theory is not possible without the categories, i.e. not without

of Hilbert's 1928 Hamburg lecture "Die Grundlagen der Mathematik," where Hilbert once again speaks of "certain extralogical concrete objects which are there intuitively as immediate experiencesbefore all cognition"[cdxi].

[303] In an, in my opinion, inspired (if historically slapdash) article, Michael Detlefsen noted notes that "Erlebnis" "was widely used by the post-Kantian idealists"(Michael Detlefsen. "Constructive Existence Claims." In: *The Philosophy of Mathematics Today*. Ed. by Matthias Schrin. Oxford University Press, 2005, pp. 307–335, p. 326n38) and maintained, as I maintain, that Hilbert's usage of this term "is perhaps some small further confirmation that Hilbert did not see finitary intuition as reducible to sensory intuition".

To reinforce the abstract philosophical point I am making, I want to quote Peirce's definition of intuition as "the opposite of discursive cognition" and "premise not itself a conclusion" (Charles Sanders Peirce. "Questions Concerning Certain Faculties Claimed for Man." In: *Writings of Charles S. Peirce: A chronologial edition. Volume 2: 1867-1871*. Ed. by Edward C. Moore. Indiana University Press, 1984, pp. 193–211, p. 193), which leaves open that intuitions are cognitions *of some sort*.

[304] As Jaakko Hintikka. "Kant's 'New Method of Thought' and his Theory of Mathematics." In: *Knowledge and the Known. Historical Perspectives in Epistemology*. Kluwer Academic Publishers, 1991, pp. 126–134, p. 130 has pointed out, intuition in Kant's sense must not be identified with perception; (this observation is re-affirmed by: Parsons, "Kant's Philosophy of Arithmetic," p. 145). Sutherland, "The Role of Magnitude in Kant's Critical Philosophy," p. 435 emphasises that Kant sets up his philosophy *of mathematics*, specifically, so that it depends on "the homogeneity of intuition in general, and not the more particular properties of space and time".

> the logical forms of operation of the mind, nor is the formation of our objective world possible on the basis and by means of only the initially given world of sensation. These forms of operation do not have *just external, formal* significance, rather they are of principal and thoroughly determinative value for the nature of things just as much as for the *content* of scientific teachings: so, in particular, those presuppositions which no science can do without, which even mathematics requires, and which are at work not just at the start but throughout the whole of any scientific edifice, [those presuppositions] must be conceived as consequences of the essence of the human mind, as expressions of categorial activity.[cdxiii]

It seems to me this passage may just as well be summarising Hilbert's overall methodological stance, as I have been describing it: that mathematics was, through and through, ever and always, the work of thought.

3.4 Developments leading into the 1920s

I have been discussing Hilbert's philosophical position as it was, judging by the evidence available, in the time § 208
before Hilbert embarked on the work for which he is among philosophers best known: the development of what Hilbert called "proof-theory" and "finitism" beginning in the 1920s and 30s. I have said little to nothing about the relation between Hilbert's «earlier» work, which my discussion throughout this dissertation has focused on, and Hilbert's «later» work. This section – an epilogue of sorts – is dedicated to questions about the continuities between the period in Hilbert's life and thought I have been focusing on to the subsequent period during which Hilbert's work revolved around proof-theory and so called "finitist reasoning". Of course, a single section can hardly balance-out the one-sidedness of my interest so far, and is not intended to[305]. I want only to touch now, in closing, on three points of detail – central though I think they are – and leave the task of comparing, contrasting, relating and potentially reevaluating Hilbert's later work in light of a fuller understanding of his earlier work and ideas to future work (of my own, perhaps, or others). I will discuss the following three issues (in the following order):

- Hilbert's reception of Schröder's *Vorlesungen* (and Russell's works) in his 1917 lecture-course.
- Hilbert's evolving views about the puzzle known as "Richard's paradox" after 1905, which is one of the few developments of Hilbert's foundational view documented at significant length in the lecture notes from the 1910s. It seems to me, based on this evidence, that Hilbert's thinking about this puzzle, in particular, played a role in bringing Hilbert's attention back to logic in 1917 and in preparing what I see as the major technical innovation in Hilbert's 1917/18 lectures in comparison to his earlier presentations of logical theory.
- The potential first roots of Hilbert's later claims about finitist reasoning in his earlier work, in particular, in some earlier remarks concerning the justification of Zermelo's axiom of choice.

[305] The historical importance of Hilbert's foundational work of the 1920s and its appropriate place on a full list of Hilbert's accomplishments tends to be unduly overestimated by many whose interest mainly in philosophical aspects of Hilbert's work, rather than Hilbert's mathematics. This disproportional interest among philosophers in Hilbert's later work more than justifies, in my view, at least one dissertation dedicated to Hilbert's earlier foundational work. Hilbert himself is not blameless in this regard: Starting the early 1920 he pursued and advocated his foundational views with a fervour which, as David Rowe observed, was "almost insane"[cdxiv]. Indeed, it seems to me Rowe has a point insofar as one ought not to take Hilbert's own claims about the importance of his later foundational work at face value.

3.4.1 Hilbert's reception of Schröder and Russell leading up to 1917

§ 209 In section 3.2.1, I argued that Hilbert initially developed his logical theory in the form Hilbert presented in his 1905 lectures, by studying and reworking Schröder's early booklet entitled "*Der Operationskreis des Logikkalkuls*" (1877) and that, in light of the substantive differences in disposition and notation, Hilbert in all likelihood was not familiar with Schröder's *Vorlesungen* at the time. That Hilbert *later* read at least the first volume of Schröder's *Vorlesungen* can be regarded as certain because, as I also mentioned earlier (in footnote 164), Hilbert explicitly refers to the first volume of Schröder's *Vorlesungen* in his 1913 lecture-course (as recorded by Baule). Since Hilbert can thus be assumed to have been familiar with (at least the first volume) of Schröder's *Vorlesungen* when he turned his attention back to logical theory for his 1917/18 lecture-course, the question arises whether this acquaintance with Schröder's later work shaped Hilbert's «mature» logical-theory, and, if so, how it did so.

All in all, Hilbert's engagement with Schröder's «mature» logical theory appears to have left only vague traces in Hilbert's 1917/18 lectures on logical theory, of which I want to highlight the following two:

- When discussing the distributive law of the disjunction over the conjunction, which is one of Hilbert's axioms in 1917/18 as it had been in 1905 (and in Schröder's 1877 booklet), Hilbert points out that the principle of duality leads one to the assertion of "a second distributive law [of conjunction over disjunction]", in which "the divergence of the logical calculus from the formalism of arithmetic comes to light especially clearly"[cdxv]. Though, given the fact that Hilbert is presupposing all the same axioms for his sentential calculus in 1917/18 as he did in 1905, this second distributive law was already a theorem of Hilbert's earlier calculus, it was not mentioned in the (small) selection of theorems Hilbert presented in his 1905 lecture, nor did Hilbert there comment on it as a marked point of disanalogy between logic and arithmetic.

  Schröder states the second distributive law in his 1877 booklet (and even provides a brief proof) but immediately marginalises it as "a proposition of no essential use to us in the sequel"[cdxvi] and does not comment on the fact that no analogous distributive law holds in arithmetic (which may be why Hilbert doesn't, either).

  In 1890, in the first volume of the *Vorlesungen über die Algebra der Logik*, Schröder's attitude has changed significantly. Half of an entire «lecture» (the sixth, § 12) is dedicated to the discussion of both distributive laws[306]. In the course of this discussion Schröder comments on the second distributive law as follows[307]:

[306] In contrast to his 1877 booklet, where Schröder stated two (commuted) forms of each distributive law (e.g. both '$a(b+c) = (ab)+(ac)$' as well as '$(b+c)a = (ba)+(ca)$' as forms of the first distributive law (Axiom 6^0) in 1877)), Schröder only states one form of each distributive law in the first volume of his *Vorlesungen*. But he comments, presumably with his 1877 presentation in mind, that "the commutative law permits us to arbitrarily rearrange members and factors of proved formulas and so give them a different form"[cdxvii].

[307] In that discussion Schröder also reports that he believed he had discovered the second distributive law in his 1877 booklet, but that he later found out that Peirce had already stated it before him[cdxviii] (as early as 1867 in Peirce, "On an Improvement in Boole's Calculus of Logic," p. 13, where Peirce calls "logical addition and logical multiplication [...] doubly distributive" and goes on to explicitly state both distributive laws stated by Schröder in his 1877 booklet; that only one holds also in arithmetic is implicitly noted in Charles Sanders Peirce. "Upon the Logic of Mathematics." In: *Writings of Charles S. Peirce: A chronological edition. Volume 2: 1867-1871*. Ed. by Edward C. Moore. Indiana University Press, 1984, pp. 59–69, p. 64 (where Peirce asserts that several theorems *except* the distribution of addition over multiplication hold "also with arithmetical multiplication")). Schröder's discovery of Peirce's work can be dated rather precisely to between 1877 and 1879, when Schröder published a brief report on Peirce's work as an addition to a prior report on his *Operationskreis* in a survey-volume of recent mathematical work published in 1879. It seems, according to the brief account Schröder

> Of these two propositions the latter [the second distributive law] is of some value for the identitary calculus. It can be shown, however, that the application of Th. 27_+) [i.e. the second distributive law] can be avoided altogether, and that one can make do with the [first] distributive law $27_\times$).
> In arithmetic the formula 27_+) is *not* valid; here [in arithmetic] multiplication and addition are related only *one-sidedly*: multiplication is related distributively to addition, but not conversely. *In the identitary calculus*, by contrast, *addition and multiplication are related in a reciprocally distributive manner.*[cdxx]

Hilbert's comment on the special status of the distributive law thus enters into his 1917/18 discussion probably as a result of his reading of Schröder's *Vorlesungen*. Of course, the observation that the second distributive law is not a valid formula in arithmetic is obvious, and Hilbert could well have made it on his own. But it seems to me that in combination with stressing that one is lead to the second distributive law by the overarching dualism of the calculus, it is not implausible to regard Hilbert's remark as an echo of Schröder's.

- Another trace of Schröder's logical theory in Hilbert's 1917/18 lectures is the introduction of the systematically ambiguous use of the identity symbol '=' to denote, on the one hand, "predicate-equations" and on the other hand "propositional equations"; specifically, to address the issue of wanting to be able to state *within the system* not only a certain equation concerning, e.g., the partition of the universe into three classes, but to state *that* this partition is true (or false)[308]. Hilbert motivates this double use of '=' as follows:

> This consideration leads to the construction of a combined calculus, whose relations are propositional equations, in which the particular propositions 0, 1 as well as predicate-relations feature as members (which, for the sake of the clarity of the notation are to be placed between parentheses). The interpretation of the formulas results without difficulty from the meaning of the predicate-equations and the propositional equations. For example the formula
>
> $$(X + Y = Z) = 0$$
>
> asserts that the predicate-relation $X + Y = Z$ is the case;
>
> $$(X = 1) + (Y \to Z) = 1$$
>
> means that the relations $X = 1$ and $Y \to Z$ cannot both be satisfied; [...] The identically valid relations of this combined calculus have the meaning of propositions in the predicate-calculus and so we obtain in the form of such identities representations of definitions, axioms and propositions, which we could up until now only express by means of language.[cdxxi]

This practice of using the identity symbol with systematic ambiguity for, anachronistically speaking, both the relation of equality between objects of the domain as denoted by (complex or simple) terms of the notation and for something like a valuation-function over sentences about the domain, goes back to Schröder, who introduces it in the first section of the *second* volume of his *Vorlesungen*:

> Whenever we deal with an assertion *within our calculus*, regardless of whether we introduce a single letter to designate it, or exhibit it with its full content, in detail (enclosed in parentheses), it should be taken in

gives there, to have been Peirce who first became aware of Schröder's work and then sent to Schröder offprints of his articles on logic, including "On an Improvement in Boole's Calculus of Logic" "from which [Schröder] recognise[d] that with respect to some essential points in my booklet, especially related to noticing the double distribuitivity [...] the priority belongs to Mr. Peirce" [cdxix].

[308] Bear in mind that, as discussed earlier (in § 119), Hilbert «inverted» '1' and '0', using the former for a constantly false sentence and the latter for a constantly true proposition, while Schröder uses '$= \dot{1}$ ' to express truth and '$= 0$' to express falsehood.

> the sense of the *duration of its validity*[309]. | For the above examples [of "$2 \times 2 = 4$" (abbreviated 'a') as an always true and "$2 \times 2 = 5$" (abbreviated 'b') as an always false assertion] we may thus say that $a = \dot{1}$ and $b = 0$. Instead – as we just allowed ourselves – of writing: $(2 \times 2 = 4) = \dot{1}$, one will usually more briefly simply set down the assertion $2 \times 2 = 4$, or a itself. Whereas the falsehood of the assertion that 2×2 equals 5 can, for now, not be expressed more simply than by writing: $(2 \times 2 = 5) = 0$.[cdxxii]

Note, in particular, that Hilbert is not only adopting Schröder's practice of ambiguous use of '=', but also Schröder's «safeguard»-convention of enclosing what Hilbert calls "predicate-equations" between parentheses.

As far as I can see, Hilbert's adoption of this practice of ambiguous use of the identity symbol is the most transparent case of resemblance between Hilbert's «mature» logical theory, as first developed in the 1917/18 lectures, and Schröder's «mature» logical theory, as found in his *Vorlesungen*.

It is, incidentally, also the only evidence I am aware of to support the hypothesis that Hilbert read *more than just the first volume* of Schröder's *Vorlesungen*; though, given that Schröder introduces this practice as part of his opening remarks in the second volume of the *Vorlesungen*, Hilbert's adoption of it is not evidence that Hilbert read *much more* of Schröder's *Vorlesungen* beyond the first volume.

The influence of Schröder's *Vorlesungen*, such as it is, does not, in any case, reach far into the 1917/18 lectures: Immediately after introducing what Hilbert calls the "combined calculus", Hilbert moves on to discussing its insufficiency and moves on to search for "a new kind of logical symbolism"[cdxxiii], which is quickly found in the representation of assertions in the form of functional expressions in the style of Russell and Frege.

Within this new notation, Hilbert explicitly adopts Schröder's recommendation of simply "setting down" an assertion itself, instead of explicitly equating it to '$\dot{1}$' – or, given Hilbert's, compared to Schröder: inverted use of the propositional constants, to '0' –, and simply setting down the negation of an assertion, formed by using the negation-overline, instead of equating it to '0' (or, for Hilbert, to '1')[cdxxiv]. From that point on, along with the notation adopted from Schröder, so too, does Schröder's theoretical influence, as far as I can discern it, fade.

§ 210

It is eclipsed by the influence of the work of Russell (and Whitehead), which emerges, it seems to me, as the main source for fresh technical developments found in the 1917/18 lectures[310].

This observation raises as the key question for the understanding of the evolution of Hilbert's logical theory in the late 1910s the question what the form and the extent of Russell's influence on Hilbert's technical work was.

This question was, as far as I can see, first clearly stated by Sieg[311], but it remains, as Sieg writes in the introduction to the recently published 1917/18 lectures on logical theory largely open to this day (see the passage

[309] In place of what Frege termed "truth-values", Schröder assigns to any proposition what he calls a "duration of validity" ("Gültigkeitsdauer"), which is either $\dot{1}$ or 0. In order to distinguish his notation for the duration of total validity (= truth) from the notation for the domain as a whole, Schröder introduces the symbol '$\dot{1}$', i.e. a '1' with a dot above it, for the duration of total validity while reserving the bare '1' for the domain. Hilbert, as far as I see, does not adopt Schröder's '$\dot{1}$' (though, I should say, that I am relying for the text of the 1917/18 lecture entirely on the edition in *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933* and have not checked the underlying manuscripts, because a dot above a '1' may easily be missed).

[310] Paolo Mancosu observes: "Determinant for Hilbert's approach during [the 1910s] is the influence of Russell and Whitehead's *Principia Mathematica*." (Paolo Mancosu. "Hilbert and Bernays on Metamathematics." In: *The Adventure of Reason. Interplay between Philosophy of Mathematics and Mathematical Logic 1900-1940*. Oxford University Press, 2010, pp. 125–158, p. 129).

[311] See: Sieg, "Hilbert's Programs: 1917-1922," p. 101.

from the editors' introduction referred to in footnote 157). I mention it here not with the intention of providing a full answer to it. I want only to reaffirm its importance and offer some observations which I think are relevant to answering it. (I am inclined to believe that at least part of the reason why this question has remained largely unanswered is the fact that accurately gauging the impact Russell's work had on the technical aspects of Hilbert's logical theory[312] requires a deeper and deeply detailed mastery in turn of the technical side of Russell's work than most – my (present) self included – possess or are inclined to acquire.)

§ 211 In an article entitled "Between Russell and Hilbert" Paolo Mancosu has drawn attention to the work of Heinrich Behmann as one of the earliest (preserved) documents of the reception of Russell in Göttingen[313].

Mancosu (in my opinion: rightly) emphasises that – but contrary to what the title of his article seems at first to suggest – that he is "not making here a priority claim on behalf of Behmann" but rather "highlighting, through Behmann's work, ideas which seem to have been shared by Hilbert and his immediate co-workers"[314].

Indeed, it seems to me that it would be decidedly implausible to ascribe to Behmann a pivotal role, i.e. to believe that Behmann was responsible for *first introducing* Göttingen, and Hilbert in particular, to Russell's work, nor a mediating one, i.e. to believe that Hilbert became acquainted with the technical details of Russell's work by reading, e.g., Behmann's 1918 dissertation. In fact, none of the evidence Mancosu presents or refers to in his article suggests this; nor is there, as far as I can see after having read through large parts of Behmann's papers archived at the Staatsbibliothek in Berlin (SBB), any still unpublished manuscript of Behmann's which would do so.

§ 212 This report of my impression of what Behmann's papers have to offer *when it comes to settling this particular question* in Hilbert scholarship should in no way be taken as asserting or even implying that Behmann's unpublished manuscripts are not worth studying. Indeed, I think the opposite is the case. Behmann remains an underestimated and undeservedly understudied figure in Hilbert's vicinity, whose published and unpublished work contains much of historical and theoretical interest[315]. Indeed, many writings well worth studying remains unpublished.

[312] I am inclined to agree with Richard Kahle. "David Hilbert and *Principia Mathematica*." In: *The Palgrave Centenary Companion to* Principia Mathematica. Ed. by Nicolas Griffin and Bernard Linsky. Palgrave Macmillan, 2013, pp. 21–34, p. 29 in thinking that the influence of Russell's (and Whitehead's) work on Hilbert was limited to technical aspects of logical theory (Kahle mentions, in particular, type-theory and the propositional calculus) and did not extend very far or deeply into philosophical matters.

[313] Much of the source material presented in Paolo Mancosu. "Between Russell and Hilbert. Behmann on the Foundation of Mathematics." In: *The Adventure of Reason. Interplay between Philosophy of Mathematics and Mathematical Logic 1900-1940*. Oxford University Press, 2010, pp. 159–175 as well as the considerations based on it are also included (some word for word) in a slightly later article of wider scope entitled "The Russellian Influence on Hilbert and His School" (there in sections 4 and 7). Mancosu stresses the following two points of contact between Behmann's reception of Russell and Hilbert's later work in both articles: (i) the use of an analogy with the ideal elements of, e.g., projective geometry (see Mancosu, "Between Russell and Hilbert," pp. 163, 172 and Paolo Mancosu. "The Russellian Influence on Hilbert and His School." In: *The Adventure of Reason. Interplay between Philosophy of Mathematics and Mathematical Logic 1900-1940*. Oxford University Press, 2010, pp. 176–198, pp. 181, 198), and (ii) the requirement that some objects be given (in intuition and) independently of cognition (see Mancosu, "Between Russell and Hilbert," p. 173 and Mancosu, "The Russellian Influence on Hilbert and His School," p. 198. With respect to the latter, I want, however, to remark that the formulations Hilbert uses to formulate the requirement for objects given independently of cognition, e.g., in his "Neubegründung der Mathematik" are more reminiscent of Hilbert's 1905 "Axiom des Denkens" (and so of Schröder, see the discussion in § 132) than of formulations used by Behmann; in particular, Hilbert, as far as I can see, does not speak of an "individual", which is a central term for Behmann.

[314] Mancosu, "Between Russell and Hilbert," pp. 172 sq.

[315] Mancosu himself has highlighted and discussed another unpublished manuscript (and some letters related letters) by Behmann "applying to proofs [...] graph-theoretic and topological considerations [for what] might very well be the first time" in Paolo Mancosu. "On the Constructivity of Proofs. A Debate among Behmann, Bernays, Gödel, and Kaufmann." In: *The Adventure of Reason. Interplay between Philosophy of Mathematics and Mathematical Logic 1900-1940*. Oxford University Press, 2010, pp. 199–216, (the remark just quoted is found on p. 205), and together with Richard Zach, recently published Paolo Mancosu and Richard Zach. "Heinrich Behmann's

I want to present here at least a brief overview of some of them:

- A (complete) manuscript of an introductory lecture-course on logic Behmann delivered in 1922/23, which, unlike Hilbert's lectures on logic, begins with a lengthy and thorough study of the (recent) history of logic; and another lengthy manuscript of a lecture-course on logic and the decision problem – distinct from the manuscript published by Mancosu & Zach – Behmann delivered the following year;

- Behmann's correspondence with, among others, Ackermann, Bernays, Russell, Ramsey, Scholz;…

  Especially worth publicising seems to me Behmann's correspondence with Alfred Boskovitz, the obscure figure whose help correcting errors in the *Principia Mathematica* is explicitly acknowledged (as is that of Behmann) by Russell in his Introduction to the Second Edition (p. xiii fn†), where Russell misspells the name "Boscovitch"): Behmann received and kept a number of lengthy letters from Boskovitz, along with two substantive technical manuscripts (!) Boskovitz entrusted to Behmann, one of which contains a rigorous development of Hilbert's geometry in the framework of the *Principia Mathematica*; after the war Behmann tried to find somebody to take Boskovitz' manuscripts into due consideration, but without success.

  The last letter Behmann received from Boskovitz is dated 29/11/1942; an entry in the Yad Vashem Central Database of Shoah Victims' Names (ID 13780029) reports (based on information submitted by Boskovitz' nephew) that Alfred Boskovitz died of starvation "ca 22 January, a few days after liberation of the [Budapest] Ghetto on January 18, 1945"[316]; the biographical data recorded there also offers an authoritative correction to Mancosu's mistaken claim that Boskovitz was Russian[317]: Boskowitz was born, and died, in Budapest.

§ 213 The following two considerations seem rather to favour the hypothesis that Hilbert became acquainted with Russell's work independently of Behmann: Hilbert appears to have been mainly influenced by Russell's technical logical work; Russell's attempts to resolve the various paradoxes by appealing to what Russell called the "vicious-circle principle" seem never to have impressed Hilbert all that much. By contrast, Behmann's work on Russell focused almost entirely on Russell's solution of the paradoxes.

In any case, Behmann never dwells on technical details of Russell's *logical* theory at any significant length[318], so whatever Hilbert knew of Russell's logical theory could not have come from Behmann's work on Russell.

These differences in what Behmann mainly discusses and what had the most impact on Hilbert's «mature» logical theory almost necessitates, it seems to me, the assumption that Hilbert was acquainted with Russell's work independently of reading and supervising Behmann's dissertation (or hearing Behmann's oral presentations).

1921 lecture on the decision problem and the algebra of logic." In: *Bulletin of Symbolic Logic* 21.2 (2015), pp. 164–187.

[316] See: https://yvng.yadvashem.org/nameDetails.html?itemId=13780029 (checked 3/12/2021).

[317] Mancosu, "Between Russell and Hilbert," p. 160.

[318] Mancosu acknowledges in both of his articles that "Behmann's work skips over the logical parts of *Principia* to focus instead on the mathematical developments more immediately related to the construction of arithmetic and of Cantorian set theory" (Mancosu, "Between Russell and Hilbert," p. 164, repeated verbatim in Mancosu, "The Russellian Influence on Hilbert and His School," p. 182). The same is true of Behmann's two lectures on Russell's theory edited in appendix D.

In fact, there is even a reason to think that Hilbert had familarised himself with Russell's work *before* Behmann even started working on his dissertation: Behmann reports to Russell that Hilbert had (competently) advised him on how to approach writing his dissertation and how to shape the topic[319], which seems to imply that Hilbert had some ideas of his own about what was important and in need of further discussion in Russell's work.

Consequently, it seems to me that any consideration of Behmann's work provides no clues as to when and how *Hilbert* himself *first* seriously studied Russell's work, and which writings of Russell's, in particular, Hilbert studied. I will come back to this question at the end of the following sub-section .

3.4.2 The evolution of Hilbert's logical theory

One, if not the main technical innovation in Hilbert's 1917/18 lectures on logical theory, in my view consists in § 214
the introduction of precisely defined languages. In 1917/18 Hilbert appears, for what I take to be the first time, interested in analysing particular mathematical propositions within a logical system, while, in contrast, Hilbert's earlier forays into logical theory served mainly the purpose of verifying inference patterns[320].

Another crucial technical innovation, if not in the development of logic on the whole, certainly in the development of *Hilbert's* logical theory, I want to briefly highlight is Hilbert's long overdue appreciation of quantifiers: As I pointed out on p. 92, quantifiers are absent not only from Hilbert's exposition of logical theory in 1905 (or, for that matter, 1915), but also from Hilbert's presentation of an axiom-system for the natural numbers, even though by that point Hilbert had studied the work of Peano in detail. Indeed, still in the discussion of the axiom system for the natural numbers included which is part of Hilbert's 1917 lectures on set-theory (which I discussed already in footnote 187), quantifiers are absent: Hilbert stills writes, for example the definition of the '$<$' as "If $a = b + c$ then $b < a$.", in the sense of '$\forall x \forall y(\exists z\ x = y + z \rightarrow y < x)$', using parameters indifferently in place *both* of an existentially quantified variable ('c' must be construed as existentially quantified) and universally quantified ones ('a' and 'b').

In connection with the introduction of precisely defined languages, I want to highlight one last string of developments threading through the unpublished manuscripts and lecture notes for lecture-courses Hilbert delivered between 1905 and 1917. Throughout this time Hilbert appears to be working towards what he could regard as a satisfactory solution of what Hilbert calls "Richard's paradox", and to change his mind significantly about both how to formulate the paradox in the first place and also about how to finally resolve it.

In fact, Hilbert applies the label "Richard's paradox", which, as far as I can see, makes its first appearance in § 215
the lecture notes for Hilbert's 1910 lecture-course (there on p. 157), to two slightly different puzzles:

- All numbers can, it seems, be described using words, but there are, on the other hand, at most countably

[319] The relevant portion of Behmann's letter to Russell is quoted in full by Mancosu, "Between Russell and Hilbert," p. 164.

[320] See the discussion in footnote 264. The distinction between what Hilbert uses his logical theory for aligns roughly with the distinction between "logic as calculus" and "logic as language" as articulated by Jean van Heijenoort: In 1905 Hilbert was interested, as van Heijenoort puts it, in "algebraic relations between sentences", while in 1917/18, Hilbert became interested also in "the symbolic rewriting of whole tracts of scientific knowledge, perhaps of all of it" (Jean van Heijenoort. "Logic as calculus and logic as language." In: *Selected Essays*. Bibliopolis, 1985, pp. 11–16, p. 12).

> many distinct descriptive phrases one can formulate in any spoken language, such as German or English, so it would follow that there are at most countably many irrational numbers, which is false.

Hilbert discussed this puzzle already his 1905 lecture-course, although Hilbert (understandably) calls it "Richard's paradox" only in 1910 (Richard published his formulation only in June 1905). In his 1910 lecture-course Hilbert additionally presents another puzzle, which Hilbert bills as a modification *still of Richard's paradox* and which he takes to show "that the mathematical concept of infinity is certainly not crucial to [Richard's] paradox"[cdxxv]:

- Consider the phrase "the least number not denotable by a term consisting of less than, say, 100 symbols".

  This description seems to (paradoxically) denote precisely the number in question by means of a description consisting of less than 100 symbols.

In both the 1905 and the 1910 lecture-course, Richard's paradox is discussed at the very beginning of the discussion of logic, where Hilbert motivates the need for a precise investigation of mathematical inference by raising a handful of paradoxes arising from insufficient regimentation of inference patterns (in Hellinger's official lecture notes for the 1905 lecture-course one finds it on pp. 199-203; in the 1910 lecture-notes it is found on pp. 157-158).

That Hilbert does not speak of "Richard's paradox" in 1905 is unsurprising: Richard's publication – a letter to § 216
Louis Olivier, the editor of the *Revue générale des Sciences pures et appliquées* – appeared only in the summer of 1905 (in the *Revue* itself[321]). For chronological reasons, therefore, the first puzzle Hilbert presents in 1905, which he in 1910 calls "Richard's paradox", *cannot* possibly derive from Richard's publication. Hilbert's first puzzle derives, rather, from Julius König's attempted refutation of Cantor's continuum hypothesis in (and after) 1904, specifically from König's considerations about "finitely defined elements of the continuum", which first appeared in print in the form of a brief note published in the *Mathematische Annalen* in 1905, where König wrote:

> An element of the continuum shall be called "finitely defined" if we can, relying on a language suitable for fixing our scientific thinking, exhibit within finite time a procedure (law) which conceptually differentiates that element of the continuum from every other [element of the continuum], or – in other words – for an arbitrarily chosen k yields the existence of one and only one associate number [in the infinite Cantorian fundamental-sequence which converges to the element of the continuum in question] a_k.[cdxxvi]

König goes on to assert, like Hilbert assumes in his formulation of his first puzzle, that, owing to the finitude of the alphabet in any given spoken language, at most countable many finite definitions can be formulated[cdxxvii]. Consequently, assuming that the continuum is non-denumerable, some of its elements must fail to be finitely definable[cdxxviii]. From these starting points, König goes on to derive the conclusion that the continuum cannot be well-ordered. König argues ultimately that the assumption that the continuum *can* be well-ordered entails that every element of the continuum is finitely definable. In detail, König's argument runs as follows: Assuming the

[321] Jules Richard. "Les principes des Mathématiques et le problème des ensembles." In: *Revue générale des Sciences pures et appliquées* 16.12 (1905). Ed. by Louis Olivier, pp. 541–543 (the relevant issue of the *Revue* is dated June 30th 1905), where Olivier, to whom Richard's letter is addressed, quotes the letter in full, then adds further elaboration of his own. Richard's letter was re-published the following year in the *Acta Mathematica* as Jules Richard. "Lettre à Monsieur le rédacteur de la Revue Générale des Sciences." In: *Acta mathematica* 30 (1906), pp. 295–296, where Richard's letter is published as is, without any further elaboration or comment.

continuum to be well-ordered, and supposing furthermore that there exists an element of the continuum which is not finitely defined, there must exist a first element which is not finitely defined. This element must, because of the assumption that the continuum is well-ordered, be associated with an ordinal number, which must, itself, not be finitely definable (otherwise the element in question can be finitely defined as the element corresponding to that finitely defined ordinal). However, the phrase "the least ordinal larger than the ordinals associated with elements of the continuum which are smaller than the least not finitely definable element in the assumed well-ordering of the continuum" would finitely define that ordinal, contradicting the impossibility of finitely defining it, and consequently the supposition that there is an element of the continuum which is not finitely defined[cdxxix].

I cannot say for certain whether Hilbert encountered these considerations of König's for the first time when reading (proofs of) the note published in the *Mathematische Annalen*, or knew of them before by hearsay (or even directly from König). It is possible that König presented some of these ideas already at the International Congress for Mathematics in 1904 (which Hilbert, and also Cantor attended) but then omitted them from the published version of his talk along with other remarks he made in his effort to refute Cantor's continuum-hypothesis[322]; but it is also possible that König's 1905 considerations about finite definability represent his subsequent effort to "save what can be saved"[323] from his 1904 efforts. Either way, it seems safe to assume that when Hilbert's 1905 lecture-course began – and certainly towards the end of the lecture-course – Hilbert would have become acquainted with these considerations of König's, because König's 1905 note in the *Mathematische Annalen* appeared just in time for Hilbert's 1905 lecture-course in the Sommersemester 1905: in the first fascicle, published in March 1905[324].

That Hilbert knew, and thought about these considerations of König's note, is beyond doubt not only because Hilbert was at the time already one of the editors of the *Mathematische Annalen* but because Hilbert explicitly discussed König's considerations concerning finite definability in correspondence with Cantor[325].

Seeing how the first version of Richard's paradox Hilbert discusses derives, in substance, from König's work makes it seem somewhat surprising that Hilbert in 1910 proceeds to call it "Richard's paradox" (rather than, say, "König's paradox"). Hilbert's reason for attributing the paradox to Richard, rather than König, likely lies in the nature of Richard's work: Richard presented his own contribution mainly as a refinement of König's considerations (as expounded by Olivier in the *Revue générale des sciences*, see footnote 322). Richard observed (as Hilbert

[322]For a recent account of the events surrounding König's 1904 talk, see: Walter Purkert. "On Cantor's Continuum Problem and Well Ordering. What really happened at the 1904 International Congress of Mathematicians in Heidelberg." In: *A Delicate Balance. Global Perspectives on Innovation and Tradition in the History of Mathematics*. Springer, 2015, pp. 3–24, pp. 12–18. One indication that questions of finite definability may have already been at least mentioned in 1904 is that the account given of König's «result» and Zeremlo's contrary result by Louis Olivier in the *Revue générale des Sciences* (No. 6 (March 30 1905), p. 241) raises as crucial the question "Mais où voit-on qu'une loi ait besoin de pouvoir être explicitement formulée pour exister?".

[323]As Zermelo reports König was trying to do in a postcard to Dehn recently published by Heinz-Dieter Ebbinghaus. "Zermelo and the Heidelberg Congress 1904." In: *Historia Mathematica* 34 (2007), pp. 428–432, p. 431.

[324]The notes taken by one Otto Birck (now housed at the ULB Bonn, see the list of lecture notes in § 107) contain dates for individual lectures. According to Birck's notes, Hilbert presented the paradoxes on Tuesday July 4th 1905 (this date is found on p. 116 of Birck's notes, the next date, July 11th 1905 is found on p. 132 and the contradiction derived from König's work is presented on p. 129).

[325]In a letter dated 8/8/1906 Cantor offers his diagnosis of what goes wrong in König's argument (according to Cantor, König's mistake is to assume that the set of possible definitions is at most denumerable), and also mentions "the note of one Mr. Richard in the latest issue of the Acta mathematica"[cdxxx]. That Cantor provides full bibliographical references to Richard's note suggests that Hilbert was not aware of it previously, and that it was, thus, Cantor who brought Richard's work (or at least Richard's name) to Hilbert's attention.

remarks in 1910) that the concept of a finitely defined number by itself already gave rise to a paradox, regardless of whether – or how – it lined up with the concept of an irrational number, or transfinite ordinals. So Hilbert presumably calls both puzzles "Richard's paradox" to honour Richard's clarification of what the central problem was, and Richard's disentangling of that problem concerning finite definability from set-theoretic notions.

It should be noted, however, that the second version of what Hilbert in 1910 calls "Richard's paradox" is not the paradox formulated in 1906 by Richard himself, either. Richard's own paradox is a variation of Cantor's diagonal argument, applied to the class of all finitely defined numbers: Given a list – Richard speaks of a "tableau" in 1906, accordingly Hilbert in 1910 speaks of a "Tafel" – of all finitely defined numbers, one can construct a number not on this list which, because it is defined finitely in terms of finitely defined numbers, is itself finitely defined, but not included on the list which was supposed to comprise all finitely defined numbers.

What Hilbert offers as the second, clarified version of Richard's paradox, by contrast, does not rely on any diagonal construction. Rather, Hilbert's second version of Richard's paradox bears a certain resemblance to the puzzle known nowadays as "Berry's paradox", which centres around a definition of the least number not definable by a certain kind of expression, which – paradoxically – is defined by that very expression.

This puzzle was, as far as I am aware, first publicised by Russell, who attributed it to one G. G. Berry, in an article discussing Richard's paradox in 1906[326]. Hilbert likely learned of it not from reading Russell's work but from Poincaré, who mentioned Berry's paradox – attributing its invention simply to Russell, rather than repeating Russell's attribution to Berry – in the fifth of his Göttingen lectures[cdxxxi]. The main reason to think that Hilbert is taking this paradox from Poincaré, rather than Russell, lies in one particular detail of the formulation: Like Poincaré, Hilbert in 1910 formulates the paradoxical definition as that of the least number not definable *by less than 100 words*, while Russell speaks of the least number not definable *by less than 18 syllables*[327].

§ 217 In 1910 Hilbert seems to regard neither one of these two puzzles as a genuine paradox, or even a real difficulty. In his discussion, Hilbert dismisses (both versions of) what he calls "Richard's paradox" with a rather brief remark diagnosing the appearance of a paradox as due to relying on the vague idea of a meaning associated with a given phrase[cdxxxii]. Hilbert's estimation that neither version, upon reflection, constitutes a genuine paradox, and his diagnosis that the appearance of paradoxicality arises from the involvement of the extra-mathematical notions of meaning into a purportedly mathematical consideration echoes Richard's own (brief) discussion of his paradox in 1906, when Richard himself already emphasised that "the contradiction is only apparent"[cdxxxiii].

[326] Bertrand Russell. "Les paradoxes de la logique." In: *Revue de métaphysique et de morale* 14 (1906), pp. 627–650, p. 645fn2.

[327] Richard Kahle. "Poincaré in Göttingen." In: *Poincaré, Philosopher of Science. Problems and Perspectives*. Springer, 2014, pp. 83–99, pp. 95 sq. reproduces and discusses a series of marginalia Hilbert added to the lecture notes for his 1905 lecture course, which show that his thinking about Richard's paradox, in particular, was significantly impacted by Poincaré's discussion in the fifth talk Poincaré delivered in Göttingen: The formulation of the paradox appealing to definability in less than 100 words appears in these marginal notes for the first time and thus seems to have come to Hilbert's attention through Poincaré's sketch of it.

(I want to briefly supplement Kahle's transcriptions in two places: The word Kahle could not read in the last line of the marginal note reproduced on p. 95 is "Richter", so the translation should read: "Arbitrariness of the judge"; and, as the presentation of the definition reproduced on p. 96 in Hilbert's private lecture-notes (i.e. Cod. Ms. Hilbert 559, see endnote cdxxxvi) shows, the word Kahle could not read in his figure 4 is the (deliberately) nonsensical word "lem".

The remarkable fact I want to highlight about Hilbert's development is that, even though Hilbert seems to dismiss Richard's paradox entirely in 1910, he not only continued to discuss it in both of his subsequent foundational lecture courses (in 1913 and 1914/15), but even modified his treatment of Richard's paradox for each subsequent lecture course; in fact, for the lecture course in 1914/15, these modifications take up the majority of Hilbert's preparatory notes preserved in the convolute held by the SUB Göttingen under the signature "Cod. Ms. D. Hilbert 559".

Another document worth highlighting in this connection is the sheet from which I quoted the aphoristic remark about "Kroneckerian decidability" discussed earlier (see p. 128): That remark about decidability is, in fact, framed by two different attempts to resolve Richard's paradox[cdxxxiv]. Since both of these attempts differ from the solution Hilbert ultimately settles on in the 1920s, this sheet and its contents likely date from the late 1910s at the latest.

For his 1913 lecture course, Hilbert noted in Cod. Ms. D. Hilbert 559:

> Resolution [of the first paradox mentioned in the 1905 lecture-notes] in any case by doubting the concept of words or a phrase, which cannot, after all, have a sense independently of an interpretation.[cdxxxv]

While the reference to "an interpretation"[328] presumably recalls what Hilbert in the 1910 lecture notes called "the ambiguous, subjective character of language", the idea that one should somehow "doubt the concept of words" has no obvious precedent in Hilbert's previous discussions of Richard's paradox; and it is this idea that Hilbert expands for his 1914/15 discussion of the paradox. His preparatory notes for the 1914/15 lecture course read[329]:

> We clarify first of all the problematic nature of the concept "definable by a certain number of syllables". Indeed, just recall the very concept of being defined in general.
>
> Is a defined if I say $a = 1$ in case $2^{\sqrt{2}}$ irrational [and a]=0 [in case $2^{\sqrt{2}}$] rational; or if I say $a = 1$ if $2^{\sqrt{2}}$ irrational [and a]=lem (senseless word) in case $2^{\sqrt{2}}$ rational cf. p. 202 [of Hellinger's notes] down the page[.]
>
> Definition of an irrational number by means of a law, but also by means of dice (no!)
>
> How to save myself? State of the science[:] Theory of types Poincare p. 199 Rüssel [sic]. I feel that there is some truth to the theory of types; about it must still be greatly simplified. And there it is required above all to see if forbidding the concept of a number of syllables won't cut off all paradoxes. That is unfortunately not the case. Zermelo's paradox.[cdxxxvi]

In this version of his discussion of Richard's paradox, Hilbert appears to have dropped any and all reference to the uncertainty of the interpretation of words, and focused, instead, on the uncertainty of which (syntactical) entities are admissible as components for definitions in the first place (regardless of what they may subsequently be taken to mean). The strictly regimented «artificial» languages Hilbert introduces in his 1917/18 lectures seem to satisfy the need for an account of what it means for a concept to be defined raised here: With precisely defined languages in hand, Hilbert can respond that "to be defined" means to be formulated using the strictly regimented means of expressions available within a precisely specified language.

[328] Below this remark, Hilbert has written "Frege zugestanden", which is unfortunately ambiguous: (A) Either Hilbert means to say that Frege granted this point, i.e. that expressions can have no sense independently of an interpretation (Hilbert could be thinking of Frege's concession that elucidation is necessary for the conferral of meaning, and less than definite, so a kind of «subjective» interpretation), or (B) Hilbert means to say that this was a point made by Frege, which he, Hilbert, is granting. I incline towards the former reading because in his notes for the third paradox (that of Zermelo-Russell), Hilbert has similarly noted "Dedekind zugestanden", which presumably refers back to the remark in 1905 (on p. 212 of Hellinger's notes) that Dedekind himself granted that Russell's paradox posed a grave difficulty for his foundational project in *Was sind und was sollen die Zahlen?*.

[329] In the Princeton manuscript, which records this lecture, the discussion is recorded at greater length on pp. 164-165.

In his 1922/23 lectures about the motivating role of paradoxes for the development of the logical calculus Hilbert explicitly says that the purpose of developing the logical calculus was the need for "the precisification of language", which arose in the wake of Richard's paradox[330]. The paradox itself is presented in these lectures in *yet another* version[331]: Hilbert now concentrates on "all infinite sequences of numbers made up just using the numbers 0 and 1 and are definable by a mathematical law" and then reduces to absurdity the assumption that these sequences can all be listed by way of a diagonal argument. It is the last clause, that the sequences are supposed to be "definable by a mathematical law" which constitutes the common thread running through all previous versions of Richard's paradox Hilbert presented as well as this one; (note, by the way, the continuity between the focus on binary expansions determined by "mathematical laws" here and the notebook entry quoted in endnote cccii).

Hilbert's incessant reformulation of Richard's paradox aims to distil a «purely mathematical» (rather than philosophical) paradox by excising all non-mathematical concepts: In the case of Richard's paradox, the non-mathematical concept of a definition by syllables of the (German) language is excised in favour of "mathematical law", and the appeal to a certain number of syllables is replaced by a consideration of denumerability.

For my purposes, the details of this new version of the paradox are immaterial. I want to highlight rather what Hilbert says about the methodological import of this paradox as recorded by Kneser:

> The paradoxes call out to us to stop. The second one (Richard) is meant to give us an indication as to how we shall attempt to get ahead. We must formalise the language and precisify it by means of the *logical calculus*, which has often been dismissed by mathematicians and by philosophers not versed in mathematics.[cdxxxvii]

This section was meant to substantiate the hypothesis that the arc Hilbert traces in this passage from Richard's paradox to the development of the logical calculus is not a retroactively constructed motivation, but, as a matter of historical fact, describes the route that (in the middle of the 1910s) lead Hilbert himself back to logic with renewed interest around 1917 and subsequently led to the development of Hilbert's «mature» logical theory.

When Hilbert (at the end of both the 1922/23 and the 1923/24 lectures) claims that Richard's paradox has now been satisfactorily resolved, the technical device he refers to as crucial to this resolution is a precisely defined language (suitably enriched to provide a notation for recursively defined functionals of higher types)[cdxxxviii].

The impact of Hilbert's thinking about Richard's paradox I have been discussing corroborates the hypothesis that Hilbert was lead to think about logic as the result of the emergence of various paradoxes in set-theory. In fact, the discussion in this sub-section suggests that, rather than Russell's paradox or Burali-Forti's paradox or Cantor's considerations about the set of all transfinite cardinals or any of the «classical» set-theoretical paradoxes, Hilbert was, rather, most concerned *from the beginning* with considerations about definability, which first emerged out of König's attempted refutation of Cantor's continuum hypothesis and were subsequently distilled by Richard and

[330] Though the name "Richard" is mentioned neither in the official lecture notes nor in Kneser's notes *when the paradox is first presented*, Kneser records Hilbert later referring back to the paradox simply as "Richard" (see the passage quoted in endnote cdxxxvii).

[331] In the official lecture notes the paradox is presented on Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, pp. 530 sq. In Kneser's notes on p. 602.

Berry into the paradoxes that now bear their names. Taking Hilbert's forays into logic to be motivated primarily by wanting to uncover the error in König's considerations, and subsequently to resolve Richard's paradox to his satisfaction, can explain why Hilbert turned to logic *precisely in 1905*, rather than earlier, or later: Hilbert did so following the kerfuffle surrounding König's presentation at the 1904 congress. (By contrast, the emergence of Russell's paradox could not have surprised Hilbert, since Cantor had been emphasising for several years in correspondence with Hilbert that not all conceivable classes are what Cantor eventually called "consistent collections", and, owing to Cantor's assurances that such problems were, on the one hand, to be expected while, on the other hand, posing no substantive difficulties, probably did not trouble Hilbert all that much[332].) And the hypothesis that Hilbert subsequently came to be concerned mainly about a satisfactory resolution of Richard's paradox can explain why Hilbert turned his attention back to logic in 1917, when, after thinking over Richard's paradox again and again between 1910 and 1915, Hilbert resolved that Russell's work offered the best chance at a resolution but still stood in need of substantive refinement, which Hilbert then undertook to provide in his 1917/18 lecture course.

3.4.2.1 Hilbert's First Encounter with Russell

Hilbert's preperatory notes for this 1913 lecture course (preserved in Cod. Ms. D. Hilbert 559, quoted in endnote cdxxxvi) contain the first appearance of Russell's name (albeit misspelled) in Hilbert's work[333]: § 218

> How to save myself? State of the science[:] Theory of types Poincare p. 199 Rüssel [sic!]. I feel that there is some truth to the theory of types; about it must still be greatly simplified.[cdxxxvi]

Given the importance of Russell's work for Hilbert's 1917/18 lecture course on logical theory, this mention of Russell in the context of Hilbert's evolving thoughts about Richard's paradox further supports the idea that these reflections on Richard's paradox were tied to the evolution of Hilbert's logical theory.

When and how, exactly, Hilbert began engaging with (and possibly reading) Russell's work is, in my view, a matter that admits only of speculation: No evidence exists, as far as I can see, which gives any clear and decisive indication as to the exact course of events. Still, Hilbert can, I think, safely be assumed to have known *about* Russell by 1910 at the latest, since Poincaré spoke about Russell's work in a guest lectures in Göttingen[334].

Indeed, all the evidence I have been discussing about the development of Hilbert's thoughts about logic after 1905 suggest that Poincaré played a crucial role in this process. Indeed, one way of explaining the curious misspelling of Russell's name ("Rüssel") is as a reflection of the *francophone* pronunciation of Russell's name by Poincaré[335].

[332] First in a letter dated 26/9/1897 (Georg Cantor. *Briefe*. Ed. by Herbert Meschkowski and Winfried Nilson. Springer, 1991, see:388 sq.), then again in a letter dated 6/10/1898 (see: Cantor, *Briefe*, pp. 394 sq.).
The terminology of a "consistent collections" is introduced in a letter dated 9/5/1899 (Cantor, *Briefe*, p. 399).

[333] That Russell is mentioned here for the first time has also been noted by Sieg, "Hilbert's Programs: 1917-1922," p. 103.

[334] Henri Poincaré. *Sechs Vorträge über ausgewählte Gegenstände aus der reinen Mathematik und mathematischen Physik*. Auf EInladung der Wolfskehl-Kommission der Königlichen Gesellschaft der Wissenschaften gehalten zu Göttingen vom 22.–28. April 1909. Teubner, 1910, p. 47; Russell is discussed in the fifth of the six lectures, which is entitled "On transfinite numbers" and mainly dedicated to the discussion of Richard's paradox.

[335] Kahle, "David Hilbert and *Principia Mathematica*," p. 25 also speculates about the reasons for Hilbert's misspelling of Russell's name: Kahle thinks Hilbert is deliberately assimilating it to a German word as a mnemonic, though Kahle notes (in note 9) that the

In any case, Hilbert's misspelling of Russell's name strongly suggests that by 1913 Hilbert had not *read* anything by Russell, since he had at that point apparently never *seen* the name in print (or only incidentally, without it making enough of an impression on Hilbert to remember how to properly spell it).

The misspelling of Russell's name indicates that Hilbert learned about Russell from Poincaré mainly orally (through Poincaré's lecture, certainly, and perhaps also in other informal discussions taking place during Poincaré's visit to Göttingen) and did not *read* either Poincaré's presentation or critique of Russell's views (e.g. in Poincaré's various article published in the *Revue de métaphysique et de morale*, and subsequently collected in Poincaré's *Science et méthode*), or, at any rate, did not read them very carefully[336].

The reference to "Poincaré p. 199" in the passage above indicates that Hilbert had read *something* by Poincaré, although what remains unclear. Most probably the reference is to Poincaré's discussion note concerning Richard's paradox[337], which is the only publication of Poincaré's on a relevant topic which has a page numbered "199"; and, indeed, Poincaré discusses Russell's views (albeit only very briefly) in the paragraph extending from p. 199 to p. 200. (Another possibility is that Hilbert is referring to page 199 of (the French edition) of *Science et méthode*. While Poincaré does not discuss Russell on *that very page* of *Science et méthode* either, Poincare does mention a "nouveau mémoire de M. Russell" on p. 201[338].) In any case, this brief sketch suggests that it was Poincaré who – personally, or by way of his articles from the 1900s – led Hilbert to study Russell's work.

3.4.3 A potential root of finitism

In light of these sketches, and the ongoing development in Hilbert's thinking about paradoxes and precisely defined § 219
languages they suggest, a fundamental and widely held assumption about Hilbert's development, which I also presupposed at the beginning of section 3.2, should, perhaps be reassessed. It no longer seems like the year 1917 marked a new beginning in Hilbert's intellectual life, when he suddenly turned his attention back to logic and foundational questions. Rather, certain foundational questions seem to be (albeit slowly) gestating in Hilbert's mind from (at least) 1905 onwards. The mere fact that these ongoing reflections left little to no trace in Hilbert's publication record, does not entail that such questions and issues were not on Hilbert's mind.

The development and modification of an axiom-system for the natural numbers (which I discussed in section 3.2.2) independently constitutes ample positive evidence to the contrary. Hilbert's repeated confrontations with

proper pronunciation of Russell's name would really suggest a different word, "Rassel", as such a mnemonic. It seems to me that both the seemingly peculiar choice of mnemonic (if that's what "Rüssel" is—in fact, contrary to Kahle, I don't believe Hilbert deliberately chose to misspell Russell's name for mnemontechnical reasons; I think Hilbert simply did not know how to spell it properly), as well as another question Kahle raises – why Whitehead is not mentioned – point to Poincaré: The reason for the omission of Whitehead's name would be that Poincaré had been engaging only with Russell. And the misspelling of Russell's name one finds in here can be explained by as a phonetic transcription of a native French speaker's – Poincaré's – pronunciation of Russell's name.

[336] Mancosu mentions that "knowledge of Russell's work was certainly disseminated by the polemic with | Poincaré in the *Revue de Metaphysique et de Morale*" (Mancosu, "The Russellian Influence on Hilbert and His School," pp. 177 sq.), but does not pursue the question of Poincaré's mediating role in introducing Hilbert, or others around him, to Russell's work in any detail.

[337] Henri Poincaré. "Réflexions sur les deux notes précédentes." In: *Acta mathematica* 32 (1909), pp. 195–200.

[338] Henri Poincaré. *Science et méthode*. Flammarion, 1908, p. 201. Poincaré is presumably talking about Russell's 1906 "Les paradoxes de la logique" (which was, in turn, written in response to Poincaré's earlier "Les mathématiques et la logique").

Richard's paradox, which show Hilbert trying out different, evolving responses in each iteration. Hilbert's axiom system for the natural numbers likewise evolves step by step throughout the 1910s: Hilbert first develops it in 1913 more or less from scratch – perhaps taking some inspiration from Schröder –, as documented by heavily reworked lecture notes produced by Bernhard Baule. The result of these revisions is then presented in the 1914/15 lectures (which were, perhaps, cause for further modifications) and, finally, the system is once again substantively revised for the discussion of arithmetic in the 1917 lecture course on set-theory.

Now I want to point to one last piece of evidence which casts doubt on the idea of a sharp divide between Hilbert's logical and foundational work before and after 1917/18: Three entries found towards the end of the third notebook (on pp. 100 and 99) show Hilbert reckoning with Zermelo's axiom of choice. These entries are, as far as I am aware, the earliest record of an idea that would later come to stand in the center of Hilbert's proof-theoretic work in the 1920s, namely, that everything which can be proved by relying on certain transfinite principles – of which Hilbert takes the axiom of choice to be an example in these entries – should also be provable without them:

> The choice-postulate, for example, can, when one writes [things down] correctly, (formal application of the rule of writing) never lead to a contradiction by way of experimenting. Mathematical experiment = experiment which never fails[,] always safe and accurate, if only one is careful. What does it mean, to be careful?, to be attentive? apply the prescriptions correctly when experimenting. Does controlling this presuppose an overmind?[cdxxxix]
>
> Prove that if one finds an ordinary proposition (e.g. of number theory or function theory) by means of such a general (transfinite) proposition (e.g. by means of the choice-principle), then that [ordinary] proposition must also be provable directly (e.g. without the choice-principle), i.e. elimination, [replace] indirect, [arrive at] direct proof. In this alone a great value of such transfinite principles would consist.[cdxl]
>
> I want to formulate Zermelo's axiom of choice thus: If M is a set of sets which involves no contradiction, then this [set] does not do so [i.e. not involve a contradiction] either in the event that we add the axiom of choice.[cdxli]

The last entry is especially odd, because, contrary to what Hilbert claims, he offers no formulation of Zermelo's axiom of choice itself. Rather, what Hilbert goes on to formulate seems to be rather a principle *about* the (meta-mathematical properties of the) axiom of choice, namely: That adding the axiom of choice to the axioms which describe a certain set does not render inconsistent the propositions describing the objects in that set, unless the description of those objects already gave rise to an inconsistency before the axiom of choice was added.

When Hilbert comes to discussing the admissibility of his ε-operator, in the early 1920s, he offers the following § 220
informal comment (as recorded by Kneser in his notes for Hilbert's 1922/23 lectures):

> If we have a contradiction, there must, because our thinking is a finite process, be a (finite) proof for it at hand. In case choice is used in that proof, then *the proof itself* must give the choice. It is not being claimed that one can always make the choice; but one can so, without risking a [new] contradiction, always *act* as if it were made. The chosen elements play the same role as, e.g., the ideal elements[339] (∞ far, imaginary)[cdxliii]

While the technical details are vastly more sophisticated than in the above notebook entries, the thought underlying Hilbert's sketched consistency proofs, is the same as expressed in the last of the three entries I just

[339] It strikes me as worth noting in this connection that König already spoke in his 1905 note of elements of sets (in König's case, the continuum), for which there was no finite definition – which could thus not, in practice, explicitly be chosen – but which are still taken to exist as "albeit in a completely new sense: 'ideal' elements"[cdxlii]. Although König, like Hilbert, saw an analogy between ideal elements in geometry and such indefinable elements of sets (which he, like Hilbert, does not spell out in any detail), unlike Hilbert, König also still recognised a significant disanalogy (which, unfortunately, he also does not discuss at any length.

quoted from the third notebook: Hilbert indicates that the ε operator is to be eliminated either in favour of '0' or in favour of the least number such that the formula to which the ε-operator was prefixed is false when that number's numeral is substituted for the variable bound by the ε-operator[340]. This elimination strategy is, I take it, what Hilbert briefly expresses by saying that "the proof itself must give the choice", i.e. the numeral in favour of which variables bound by an ε-operator are eliminated are determined ultimately by the ε-free formulas which occur in the proof in question. Just as in the above diary entry Hilbert said that "an ordinary proposition [established] by means of a general (transfinite) proposition must also be provable without it". The axiom of choice, in particular, can only make explicit choices the remaining axioms already «provide the means» for making, so, as Hilbert writes in the notebook-entry, if the axiom of choice leads to a contradiction that must be because the remaining axioms already «provide the means» to derive a contradiction.

With these three entries from the third notebook, which, among all the material I have discussed, reaches furthest and most directly into Hilbert's later work, this discussion of Hilbert's *early* views and ideas comes to an end. At the same time, here is a starting point for the further work of connecting what I have tried throughout this dissertation to bring to the surface – now in particular that Hilbert apparently thought about the dispensability of certain "transfinite" principles of proof long before he began thinking in depth about a theory of proofs, and for reasons of his own[341] – with what is already widely known about Hilbert's later work, and reappraise it accordingly.

[340] The same basic idea is presented to give a consistency proof of the 'τ'-axiom in David Hilbert. "Die logischen Grundlagen der Mathematik." In: *Gesammelte Abhandlungen. Analysis, Grundlagen der Mathematik, Physik, Verschiedenes, nebst einer Lebensgeschichte*. Vol. 3. Springer Verlag, 1935, pp. 178–191, pp. 185 sq. Hilbert also emphasises here (on p. 185) that "the crucial point is showing that no [new] contradictions result from adding [the 'τ'-axiom] to [a collection of axioms whose consistency has already been established]".

[341] Benis-Sinaceur, "Du formalisme à la constructivité: le finitisme," p. 256 already observed in 1993 that "L'argument avancé au moment où il [Hilbert] adopte le 'principe de constructivité', et selon lequel c'est seulement en poursuivant la voie ouverte par l'axiomatique que l'on respectera pleinement 'les tendence constructives, pour autant qu'elles sont naturelles', n'est donc pas, ou pas seulement, dicté par une stratégie de défense contre les attaques de ses adversaires (en 1922, il s'agit surtout des intuitionnistes Weyl et Brouwer). C'est un argument que exprime une conviction profonde et reflète une pratique constante de son 'criticisme'. A qui a mené à bien l'élimination des axiomes de continuité, la tâche d'éliminer le transfini devait paraître naturelle et réalisable.".

Supplementary Documentation

i. “In der Meinung, die darin behandelten Fragen möchten von allgemeinerem Interesse sein, dachte ich an eine spätere Veröffentlichung [des Briefwechsels mit Hilbert]. Herr Hilbert trägt indessen Bedenken, darein zu willigen, da seine eigenen Ansichten sich seitdem umgewandelt haben. Ich bedauere das, weil der Leser durch den Briefwechsel am bequemsten in den Stand der Fragen eingeführt und mir eine neue Abfassung erspart worden wäre. Es scheinen mir aber auf diesem Gebiete die Ansichten noch so wenig übereinzustimmen und von einer Klärung noch so entfernt zu sein, daß eine öffentliche Aussprache zu dem Zwecke eine Verständigung herbeizuführen, wohl gerechtfertigt sein dürfte. Und so möchte ich denn hier einige Fragen von grundlegender Wichtigkeit behandeln in Form einer Auseinandersetzung mit jener Schrift des Herrn Hilbert. Und es kann dabei gleichgültig sein, ob die angezweifelten Ansichten des Herrn Verfassers [Hilbert] noch jetzt von ihm festgehalten werden oder nicht.” (Gottlob Frege. *Kleine Schriften*. Ed. by Ignacio Angelelli. Georg Olms Verlagsbuchhandlung, 1967, p. 262).

ii. “Ich würde Ihre Schrift nicht für wertvoll halten, wenn ich nicht ungefähr zu sehen glaubt, wie etwa solchen Einwänden die Spitze abzubrechen wäre; aber das wird wohl nicht ohne erhebliche Umgestaltung möglich sein. Zunächst scheint mir jedenfalls eine Verständigung über die Ausdrücke: ‘Erklärung’, ‘Definition’, ‘Axiom’ nötig zu sein, worin Sie von dem mir Geläufigen und auch wohl Hergebrachten stark abweichen, wodurch es mir schwer wird, diese Ausdrücke in Ihrer Darstellung auseinander zu halten und den logischen Bau in voller Klarheit zu erkennen.” (Gottlob Frege. *Wissenschaftlicher Briefwechsel*. Ed. by Gottfried Gabriel et al. Felix Meiner Verlag, 1976, p. 64).

iii. “So geistreich Manches erfunden ist, so halte ich doch das | Ganze für verfehlt und jedenfalls nur mit sehr viel Kritik verwendbar. Bei H[ilbert] scheint mir wie bei vielen Mathematikern ein klares Bewusstsein dafür zu fehlen, was eine Definition leisten kann, und dass das nicht dasselbe sein kann, was ein Lehrsatz leistet, dass also eine Definition nicht für einen Lehrsatz eintreten kann.” (Frege, *Wissenschaftlicher Briefwechsel*, pp. 147 sq.); this is letter XXVII/1 in the editors’ numbering of Frege’s correspondence.

iv. “Wir haben allso *zwei ganz verschiedene Fälle* zu unterscheiden 1. Wir bauen einen Sinn aus seinen Bestandteilen auf und führen ein ganz neues einfaches Zeichen ein, um diesen Sinn auszudrücken. Man kann dies ‘aufbauende Definition’ nennen, wir wollen es aber lieber ‘Definition’ schlechtweg nennen. 2. Schon längst ist ein einfaches Zeichen in Gebrauch gewesen. Wir glauben seinen Sinn logisch zerlegen zu können und erhalten einen zusammengesetzten Ausdruck, von dem wir meinen, dass er denselben Sinn ausdrücke wie jener. [...] Man spricht hier auch wohl von Definition. Zum Unterschied vom ersten Falle könnte man ‘zerlegende Definition’ sagen. Besser aber ist es, hier das Wort ‘Definition’ ganz zu vermeiden, weil das, was man hierbei Definition nennen möchte, eigentlich als Axiom aufzufassen ist.” (Gottlob Frege. *Nachgelassene Schriften*. Ed. by Hans Hermes, Friedrich Kambartel, and Friedrich Kaulbach. Zweite, revidierte Auflage. Meiner Verlag, 1983, p. 227).

v. These are the last sentences of § 55, which, quoted here in its entirety, reads: “Bevor wir prüfen, was hervorragende Mathematiker von den Zahlen, insbesondere den Irrationalzahlen gelehrt haben, wird es gut sein, vorweg einige Grundsätze für das Definiren aufzustellen und zu begründen, die fast von allen Schriftstellern auf diesem Gebiete ausser Acht gelassen werden, damit wir nicht nöthig haben, jedes

einzelne Mal ausführlich darauf einzugehen. Wir haben solche Grundsätze schon im 1. Bd. für die Begriffsschrift aufgestellt; hier soll es sich hauptsächlich um Definitionen in der Wortsprache handeln. Zwischen beiden Aufstellungen werden freilich nur solche Unterschiede hervortreten, welche in der verschiedenen Natur dieser Ausdrucksmittel begründet sind." (Gottlob Frege. *Grundgesetze der Arithmetik*. Vol. 2. Verlag von Hermann Pohle, 1903, p. 69).

vi. "Definitionen nennt man in der Mathematik wohl allgemein die Festsetzung der Bedeutung eines Wortes oder Zeichens. Die Definition unterscheidet sich von allen andern mathematischen Sätzen dadurch, daß sie ein Wort oder Zeichen enthält, das bis dahin keine Bedeutung hatte, nun aber durch sie eine bekommt." (Gottlob Frege. "Über die Grundlagen der Geometrie." In: ed. by Ignacio Angelelli. Georg Olms Verlagsbuchhandlung, 1967, pp. 263–266, p. 262).

vii. Frege's guideline 1 reads: "Jeder aus den definirten Namen rechtmässig gebildete Name muss eine Bedeutung haben. Es muss sich also immer ein aus unsern acht Urnamen zusammengesetzter Name angeben lassen, der gleichbedeutend mit ihm ist, und dieser muss bis auf die unwesentliche Wahl deutscher und griechischer Buchstaben durch die Definition unzweideutig bestimmt sein." (Gottlob Frege. *Grundgesetze der Arithmetik*. Vol. 1. Verlag von Hermann Pohle, 1893, p. 51). (German and Greek letters are two kinds of bound variables: German variables serve as bound variables for quantifiers; Greek letters as bound variables for Frege's class-formation operator; the requirement that the defining expression of an expression must be uniquely determined "up to the inessential choice of German and Greek letters" is thus making an allowance for what would nowadays be called "alphabetic variance" or "bound variable-change".)

viii. Carnap expounds his views on definitions in § 22 of the *Logische Syntax der Sprache*: "Bei streng formalem Verfahren kann die Entscheidung darüber, ob ein vorgelegter Satz eine zulässige Definition eines bestimmten Kalküls ist oder nicht, nur von der Form des Satzes und von den Formbestimmungen des Kalküls abhängen. Auf Grund jener nicht-formalen Forderung würde aber diese Entscheidung abhängig werden von der historischen Feststellung, ob gewisse Sätze früher aufgestellt worden sind oder nicht." (Rudolf Carnap. *Logische Syntax der Sprache*. Springer-Verlag, 1968, p. 58).

ix. This remark is found at the end of § 10, which is concerned with the fundamental syntactic distinction between truth-evaluable expressions (especially sentences) and denoting expresions (especially numerals): "Die wichtigste Eintelung der Ausdrücke ist die in Sätze und Nicht-Sätze. Die häufig vorgenommene Einteilung der Satzteile, die nicht Sätze sind, in Ausdrücke mit '*selbstständiger Bedeutung*' ('Eigennamen' im weiteren Sinne) und die übrigen ('ungesättigte', 'unvollständige', 'synsemantische' Ausdrücke) dürfte mehr psychologisch als logisch bedeutungsvoll sein." (Carnap, *Logische Syntax der Sprache*, p. 25).

That Carnap is here thinking (perhaps among others) of Frege is, in my opinion, beyond doubt on account of Carnap's use of Frege's peculiar metaphor of an "non-satiated" ("ungestättigten") name.

x. "Man darf also ein Zeichen oder Wort nicht dadurch erklären, dass man einen Ausdruck erklärt, in dem es vorkommt, während die übrigen Theile bekannt sind. Denn es wäre erst eine Untersuchung nöthig, ob die Auflösung für die Unbekannte – ich bediene mich eines wohl verständlichen algebraischen Bildes – möglich sei, und ob die Unbekannte eindeutig bestimmt werde. Es ist aber, wie oben schon gesagt, unthunlich, die Rechtmässigkeit einer Definition von dem Ausfall einer solchen Untersuchung abhängig zu machen, die überdies vielleicht garnicht einmal durchführbar wäre." (Frege, *Grundgesetze der Arithmetik*, p. 79).

xi. "Bei einer Fortsetzung dieses Unternehmens wird immer wieder das Bedürfniss von Definitionen hervortreten. Die Grundsätze, die dabei maassgebend sein müssen, sind in § 33 aufgeführt. Die Definitionen sind nicht eigentlich schöpferisch und dürfen es, wie ich glaube, nicht sein; sie führen nur abkürzende Bezeichnungen (Namen) ein, die entbehrt werden könnten, wenn nicht sonst die Weitläufigkeit unüberwindliche äussere Schwierigkeiten machte." (Frege, *Grundgesetze der Arithmetik*, p. VI).

xii. Appended to Frege's brief sketch of principles to follow when defining concepts is a footnote saying: "Man vergleiche meine Grundgesetze der Arithmetik II, §§ 56-67." (Frege, *Kleine Schriften*, p. 265); following the same formulations in the letters is a reference to the first

volume: “Es wäre an der Zeit, meine ich, dass man sich einmal darüber verständigte, was eine Definition ist und leisten soll, und welche Grundsätze beim Definieren demgemäss zu befolgen sind (Meine Grundgesetze der Arithmetik Bd. I § 33).” (Frege, *Wissenschaftlicher Briefwechsel*, p. 62).

xiii. It appears as the first basic principle of definitions in § 56 of the second volume of the *Grundgesetze der Arithmetik*: “1. Grundsatz der Vollständigkeit. Eine Definition eines Begriffes (möglichen Prädikates) muss vollständig sein, sie muss für jeden Gegenstand unzweideutig bestimmen, ob er unter den Begriff falle (ob das Prädikat mit Wahrheit von ihm ausgesagt werden könne) oder nicht.” (Frege, *Grundgesetze der Arithmetik*, p. 69).

xiv. “Wir haben dies im ersten Bande so ausgedrückt: jeder Functionsname muss eine Bedeutung haben. Deshalb also sind alle bedingten Definitionen und alles stückweise Definiren zu verwerfen. Jedes Zeichen muss mit einem Schlage vollständig erklärt werden, sodass es, wie wir sagen, eine Bedeutung erhält. Alles dies hängt auf’s Engste mit einander zusammen und kann als Ausfluss des Grundsatzes der Vollständigkeit der Definitionen angesehen werden.” (Frege, *Grundgesetze der Arithmetik*, p. 78).

xv. A case in point is the opening statement of Frege’s «discussion» of Cantor in § 68 of the second volume of the *Grundgesetze der Arithmetik*, where Frege preaches: “Es ist ein großer Uebelstand, dass das Wort ‘Zahl’ von den Mathematikern schwankend gebraucht wird: bald nennt man die Zahlenzeichen, bald ihre Bedeutungen Zahlen. Jeder mathematische Schriftsteller, der das Wort ‘Zahl’ gebraucht, sollte eigentlich angeben, wie er es meint [attached to this sentence is a footnote, which reads: Wir haben uns für das Zweite [i.e. to designate as ‘Zahlen’ the meaning of numerals] entschieden.].” (Frege, *Grundgesetze der Arithmetik*, p. 80).

xvi. “Der Ausdruck | ‘die Summe eines ersten und eines zweiten Gegenstandes’ stellt sich als Name einer solchen Function [mit zwei Argumenten] dar. Es muss also auch hier für jeden ersten und für jeden zweiten Gegenstand bestimmt sein, welcher Gegenstand die Summe des ersten und zweiten sei, und einen solchen muss es immer geben. Ist das nicht der Fall, so ist auch nicht bestimmt, welcher Gegenstand zu sich selbst addirt Eins ergebe. Mithin bedeuten dann die Worte ‘etwas, was zu sich selbst addirt Eins ergiebt’ keinen scharf begrenzten Begriff, also überhaupt nichts logisch Brauchbares. Und die Frage, wieviel Gegenstände es gebe, die zu sich selbst addirt Eins ergeben, ist unbeantwortbar.” (Frege, *Grundgesetze der Arithmetik*, pp. 75 sq.).

Note, incidentally, that this passage also corroborates what I said about the «native soil» of definitions: Frege begins by considering an expression, but then shifts to considering the function it designates and seems to be concerned with whether the definition of *that function* is complete in the sense of assigning an object as the value of the function for absolutely any (pair of) thing(s) – integers, complex numbers, classes, cars, planets,... – as argument.

xvii. “Diese Forderung kann auch damit begründet werden, dass der Satz vom ausgeschlossenen Dritten gelten muss. Daraus folgt nämlich, dass jeder Begriff scharf begrenzt sein muss, sodass von jedem Gegenstande bestimmt ist, ob er unter den Begriff falle oder nicht. Fände das nicht statt, so gäbe es eben ausser den beiden Fällen ‘a fällt unter den Begriff F’ und ‘a fällt nicht unter den Begriff F’ noch einen dritten, den der Unentschiedenheit.” (Frege, *Nachgelassene Schriften*, p. 168).

xviii. “Betrachten wir nun beispielsweise den peanoschen Satz ‘$u, v\varepsilon K.f\varepsilon vfu.\overline{f}\varepsilon ufv. \supset .\text{num } v = \text{num } v$’, er im Wesentlichen meinem [...] entspricht. Es fehlen bei mir allerdings die beiden peanoschen Bedingungen $u, v\varepsilon K$, ohne dass jedoch deswegen mein Satz falsch wäre. Meine Zeichen sind vielmehr so definiert, dass, ohne die Wahrheit zu stören, für u oder v auch Namen von Gegenständen eingesetzt werden können, die keine Klassen sind” (Frege, *Nachgelassene Schriften*, p. 165).

xix. “Betrachten wir nun einen Satz, der aus unserem allgemeinen Satz durch Wendung hervorgeht, indem wir einen Bedingungssatz verneint zum Folgesätze und den bisherigen Folgesatz verneint zu einem Bedingungssatze machen. [...] Der Sinn wir hierdurch kaum berührt, da der Satz nach der Umformung nicht mehr und nicht weniger Auskunft gibt, als vorher. Da aber die Bedingungen nicht ganz dieselben sind, so sind auch die den *Bedeutungen* der Buchstaben auferlegten Beschränkungen nicht mehr dieselben.” (Frege, *Nachgelassene*

Schriften, p. 166).

xx. "So kommt es also, dass durch erlaubte Umformungen die zulässigen *Bedeutungen* der Buchstaben sich ändern, was die logische Durchsichtigkeit nicht vermehrt." (Frege, *Nachgelassene Schriften*, p. 166).

xxi. "Die Definitionen von 'num u' und der Abbildung ('$f\varepsilon vfu$') müssen also demgemäss abgefasst sein. Da man nun nicht von vornherein wissen kann, in welchen Sätzen diese Zeichen vorkommen werden und welche Beschränkungen dabei den Bedeutungen der Buchstaben auferlegt werden, so sind die Definitionen so anzulegen, dass für jede *Bedeutung* der Buchstaben diesen Zeichenverbindungen eine Bedeutung gesichert sei." (Frege, *Nachgelassene Schriften*, p. 168).

xxii. "Weiter ist wesentlich, dass die Zeichengruppe '$(3 + 2) - 2$' eine Bedeutung hat, auf die man mit dem bestimmten Artikel ('*die* Zahl, welche') und mit dem Demonstrativpronomen ('*Diese* Zahl') hinweisen kann. Auch dies fällt im Rechenspiele weg. Von einer Definition kann hier nicht gesprochen werden; denn der Minusstrich ist hier kein Subtractionszeichen, er bedeutet überhaupt nichts; statt einer Definition giebt es hier nur Regeln für die Handhabung dieses Striches; aber grade eine Regel fehlt, welche für unsern Zweck brauchbar wäre." (Frege, *Grundgesetze der Arithmetik*, p. 122).

Frege had made a similar observation earlier. In § 93 he noted in full generality that definitions in formal theories are rules for handling symbols rather than proper definitions: "Sind in der formalen Arithmetik Definitionen möglich? Jedenfalls nicht solche, welche für arithmetische Zeichen Bedeutungen festsetzen; denn diese [i.e. Bedeutungen] sollen hier ganz ausser Betracht bleiben. Statt der Definitionen | wird es hier Einführungen neuer Figuren geben mit Hinzufügung der Regeln für ihre Handhabung. Wenn also bei Thomae der Ausdruck 'formale Definition' vorkommt, so soll wohl nur dies darunter verstanden werden." (Frege, *Grundgesetze der Arithmetik*, pp. 101 sq.) — Disqualifying such so called "formal definitions" from being definitions is Frege's requirement that definitions assign meaning, not his principle of completeness.

Frege goes on, somewhat surprisingly, to admit that even in a «formal» theory one can state proper definitions in a different, purely syntactic (cf. footnote 18) sense: "In einer Theorie der formalen Arithmetik sind auch eigentliche Definitionen möglich; aber in diesen werden nicht Figuren Bedeutungen beigelegt, die ja ausser Betracht bleiben sollen; sondern es werden in ihnen etwa Ausdrücke erklärt, mit denen man die Lehrsätze dieser Theorie kürzer fassen kann." (Frege, *Grundgesetze der Arithmetik*, p. 102).

xxiii. So Frege says, for example, in an unpublished manuscript entitled "Über Euklidische Geometrie": "In der euklidischen Geometrie sind gewisse Wahrheiten als Axiome überliefert worden. Wer einen Gedanken für falsch hält, kann ihn nicht als Axiom anerkennen; denn ein Axiom ist eine Wahrheit." (Frege, *Nachgelassene Schriften*, p. 183).

xxiv. "Ich habe mit Interesse Kenntnis von Ihrer Festschrift über die Grundlagen der Geometrie genommen, um so mehr, als ich mich selbst früher damit beschäftigt habe, ohne jedoch etwas zu veröffentlichen. Wie natürlich, finden zwischen meinem früheren unvollendeten Versuche und Ihrer Darstellung Berührungen, aber auch mannigfache Abweichungen statt. Insbesondere glaubte ich, mit weniger Urgebilden auskommen zu können." (Frege, *Wissenschaftlicher Briefwechsel*, p. 60).

xxv. Frege writes: "Die Erklärungen des § 4 scheinen ganz derselben Art zu sein, wie ihre Definitionen: es wird darin z.B. gesagt, was die Worte 'liegen in der Geraden a auf derselben Seite von Punkt O' bedeuten sollen, ebenso wie in der darauf folgenden Definition z.B. gesagt ist, was das Wort Streckenzug bedeuten soll. Ganz anderer Art sind wohl die Erklärungen der §§ 1 und 3, bei denen die Bedeutungen der Wörter 'Punkte', 'Gerade', 'zwischen' nicht angegeben, sondern als bekannt vorausgesetzt werden. Wenigstens scheint es so." (Frege, *Wissenschaftlicher Briefwechsel*, p. 61).

Hilbert's definition of a polygon reads: A polygon is a set of lines such that there is an ordered sequence of points each of which, together with the next in the sequence, determines exactly one line in the set, and for every line there are two points of the sequence determining it, and the last point in the sequence is identical to the first. ("Ein System von Strecken $AB, BC, CD, \ldots, KL$ heißt ein

Streckenzug, der die Punkte A und L miteinander verbindet; dieser Streckenzug wird auch kurz mit $ABCD \dots KL$ bezeichnet. [...] | Fällt insbesondere der Punkt L mit dem Punkt A zusammen, so wir der Streckenzug ein *Polygon* genannt und als Polygon $ABCD \dots K$ bezeichnet." (David Hilbert. *Die Grundlagen der Geometrie. The* Festschrift *of 1899*. In: *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*. Ed. by Michael Hallett and Ulrich Majer. Vol. 1. David Hilbert's Lectures on the Foundations of Mathematics and Physics, 1891-1933. Springer, 2003, pp. 407–529, pp. 441 sq.).).

xxvi. "Die Axiome dieser Gruppe [the heading is 'Die Axiomengruppe II: Axiome der Anordnung'] definieren den Begriff 'zwischen' und ermöglichen auf Grund dieses Begriffes die *Anordnung* der Punkte auf einer Geraden, in einer Ebene und im Raume." (Hilbert, *Die Grundlagen der Geometrie*, p. 439).

xxvii. "Nun erklärt sich der Ausspruch des Herrn Hilbert, daß Axiome etwas definieren. Aber ist es damit verträglich, daß die Axiome Grundtatsachen unserer Anschauung ausdrücken? Wenn sie das tun, so behaupten sie etwas. Dann muß aber jeder in ihnen vorkommende Ausdruck schon vollständig bekannt sein. Wenn die Axiome dagegen Bestandteile von Definitionen sind, so werden sie Ausdrücke, z.B. 'Punkt', 'Gerade', enthalten, deren Bedeutung noch nicht feststeht, sondern erst festgestellt werden. Dann ist jedes einzelne Axiome etwas Unselbständiges, ist gar nicht zu denken, ohne die andern Axiome, die derselben Definition angehören. Die Bedeutung des Wortes 'Punkt' wird erst auf S. 19 der Festschrift nach der Absicht des Herrn Hilbert festgestellt. Nun erst drücken die bisher aufgeführten Axiome Gedanken aus, die wahr sind kraft der Definition; aber eben deswegen drücken sie nicht Grundtatsachen unserer Anschauung aus; denn dann gründete sich ihre Geltung eben auf Anschauung." (Frege, *Kleine Schriften*, p. 265).

xxviii. "Keine Wissenschaft ist ganz formal; aber in einem gewissen Grade ist auch die Gravitationsmechanik formal, insofern alle optischen und chemischen Eigenschaften ihr ganz gleichgültig sind. Körper von verschiedenen Massen sind für sie nicht vertauschbar; aber die Verschiedenheit der Körper in chemischen Eigenschaften steht ihrer Vertauschung in der Gravitationsmechanik nicht im Wege. Der Logik gehören z.B. an, die Verneinung, die Identität, die Subsumption, die Unterordnung von Begriffen. Und hierbei duldet die Logik keine Vertauschung." (Frege, *Kleine Schriften*, p. 322). A few lines above this passage, Frege states: "Wie der Geometrie der Begriff *Punkt* angehört, so hat auch die Logik ihre eigenen Begriffe und Beziehungen, und nur dadurch kann sie einen Inhalt haben." (Frege, *Kleine Schriften*, p. 322).

xxix. He says, for example, about the axioms of order: "Hier handelt es sich um die axiomatische Charakterisierung derjenigen Verhältnisse, die wir mit dem Worte 'zwischen' zum Ausdruck bringen." (David Hilbert. *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*. Ed. by William Ewald and Wilfried Sieg. Vol. 3. David Hilbert's Lectures on the Foundations of Mathematics and Physics, 1891-1933. Springer, 2013, p. 66).

xxx. "Wie üblich, definieren wir nun: Zwei Winkel mit gleichem Scheitelpunkt heissen *Nebenwinkel*, wenn sie einen Halbstrahl gemeinsam haben und die anderen beiden Halbstrahlen in einer und derselben Geraden auf entgegengesetzten Seiten des Scheitelpunktes liegen." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 71).

xxxi. In his discussion of the reference of whole sentence, he considers (and eventually rejects) the possibility that sentence as wholes have sense but no reference. In the course of this discussion he observes: "Und Sätze, welche Eigennamen ohne Bedeutung enthalten, werden von der Art sein [i.e. will have sense but no reference]. Der Satz 'Odysseus wurde tief schlafend in Ithaka ans Land gesetzt' hat offenbar einen Sinn. Da es aber zweifelhaft ist, ob der darin vorkommende Name 'Odysseus' eine Bedeutung habe, so ist es damit auch zweifelhaft, ob der ganze Satz eine habe." (Frege, *Kleine Schriften*, p. 148). The first sentence states the contrapositive of the principle I stated: If even one component of a sentence has no meaning, then neither will the sentence. (By contraposition: If a sentence has a meaning, then every essential component of it must as well.). Frege touches on this principle again later on in "Über Sinn und Bedeutung": "Ich habe nämlich das Verhältnis des Ganzen und des Teils vom Satze auf seine Bedeutung übertragen, indem ich die Bedeutung eines Wortes

Teil der Bedeutung des Satzes genannt habe, wenn das Wort selbst | Teil dieses Satzes ist, eine Redeweise, die freilich anfechtbar ist, weil bei der Bedeutung durch das Ganze und einen Teil der andere nicht bestimmt ist, und weil man bei Körpern das Wort Teil schon in anderm Sinne gebraucht." (Frege, *Kleine Schriften*, pp. 150 sq.).

xxxii. This principle is found in section χ_1 of Schröder's introduction: "In der Theorie müssen wir die fundamentale Anforderung der Einsinnigkeit, kraft welcher erst ein Zeichen seiner Bestimmung voll zu genügen vermag, jeweils als erfüllt voraussetzen und dieses Ideal, bevor wir zu Nutzanwendungen schreiten, allemal vorgängig zu erfüllen trachten. Hierzu ist es ausreichend, einen etwa vorgefundenen vielsinnigen Namen (wie man nach früheren sagen kann) zu 'differenziiren', das heisst hier: so viel verschiedene Namen aus ihm zu machen, als in wie viel verschiedenen Bedeutungen er gebraucht werden soll." (Ernst Schröder. *Vorlesungen über die Algebra der Logik. Exakte Logik*. Vol. 1. Teubner, 1890, p. 54); Schröder later mentions "univok" as a synonym for "einsinnig" (see: Schröder, *Vorlesungen über die Algebra der Logik*, p. 69), and since there is (as far as I know) no natural English translation of the latter, I used the translation of the former.

xxxiii. "Hiermit kommen wir auf die Forderung der Eindeutigkeit der Zeichen zu sprechen, die, wie es scheint, von manchen neueren Mathematikern mißach|tet wird [...]" (Frege, *Kleine Schriften*, pp. 292 sq.).

xxxiv. "Ich weiss nicht, wie ich mit Ihrer Definition die Frage entscheiden soll, ob meine Taschenuhr ein Punkt sei. Gleich das erste Axiom handelt von zwei Punkten; wenn ich also wissen wollte, ob es von meiner Uhr gälte, müsste ich zunächst von einem anderen Gegenstande wissen, dass er ein Puinkt wäre. Aber selbst wenn ich das z.B. von meinem Federhalter wüsste, so könnte ich noch immer nicht entscheiden, ob meine Uhr und mein Federhalter eine Gerade bestimmten, weil ich nicht wüsste, was eine Gerade wäre." (Frege, *Wissenschaftlicher Briefwechsel*, p. 73).

xxxv. After quoting Hilbert's definition of "between" as "as certain relation between points on a line", Frege comments: "Hierdurch erfahren wir offenbar die Bedeutung des Wortes 'zwischen' nicht. Die Erklärung ist aber auch noch unvollständig. Sie ist durch folgende Axiome zu ergänzen: [Frege goes on the quote the axioms of order.] Erfahren wir nun hierdurch, wann die Beziehung des Zwischenliegens stattfindet? Nein;" (Frege, *Kleine Schriften*, p. 267). Frege then presents the same analogy as in the letter (a «Hilbert-style» definition of "congruent numbers"), before returning to Hilbert's definitions and presenting his pocket-watch example as a reason to think that one could not figure out what the expressions Hilbert defines apply to: "Wenn wir das Ganze der Erklärungen und Axiome des Herrn Hilbert überblicken, so erscheint es vergleichbar einem Systeme von Gleichungen mit mehreren Unbekannten; denn in einem Axiome kommen in der Regel mehrere der unbekannten Ausdrücke 'Punkt', 'Gerade', 'Ebene', 'liegen', 'zwischen', usw. vor, so daß erst das Ganze, nicht die einzelnen Axiome oder Gruppen von solchen zur Bestimmung der Unbekannten genügen. Aber genügt das Ganze? Wer sagt, daß dies System für die Unbekannten auflösbar sei, und daß diese eindeutig bestimmt seien? [...] Wenn eine solche Lösung des Hilbertschen Systems möglich sei, müßte sie gegeben werden; aber sie ist wohl unmöglich. Wenn wir die Frage beantworten wollen, ob ein Gegenstand – z.B. meine Taschenuhr – ein Punkt sei [...]" (Frege, *Kleine Schriften*, p. 268).

xxxvi. So Frege writes in his second letter: "Bei Ihrer Erklärung des Zwischen liegt die Sache noch ungünstiger; denn Ihre Axiome der Anordnung enthalten auch noch die Wörter 'Punkt' und 'Gerade', deren Bedeutungen ebenfalls unbekannt sind." (Frege, *Wissenschaftlicher Briefwechsel*, p. 73).

xxxvii. "Nun, eine Definition hat doch den Zweck, anzugeben, welchen Sinn man mit einem Worte oder Zeichen verbindet." (Frege, *Kleine Schriften*, p. 106).

xxxviii. "Hieraus folgt nun die Unzulässigkeit des in der Mathematik so beliebten stückweisen Definirens." (Frege, *Grundgesetze der Arithmetik*, p. 70).

xxxix. “Ungünstig für mein Buch ist auch die weit verbreitete Neigung, nur das Sinnliche als vorhanden anzuerkennen. Was nicht mit den Sinnen wahrgenommen werden kann, sucht man zu leugnen oder doch zu übersehen. Nun sind die Gegenstände der Arithmetik, die Zahlen unsinnlicher Art; wie findet man sich damit ab? Sehr einfach! man erklärt die Zahlenzeichen für die Zahlen. [...] Man dichtet ihnen die gewünschten Eigenschaften durch sogenannte Definitionen einfach an. Wie freilich eine Definition statthaben kann, wo gar kein Zusammenhang zwischen Zeichen und Bezeichnetem in Frage kommt, ist ein Räthsel. [...] Es kommt hier darauf an, sich klar zu machen, was Definiren ist und was dadurch erreicht werden kann. Man scheint ihm vielfach eine schöpferische Kraft zuzutrauen, während doch dabei weiter nichts geschieht, als dass etwas abgrenzend hervorgehoben und mit einem Namen bezeichnet wird. Wie der Geograph kein Meer schafft, wenn er Grenzlinien zieht und sagt: den von diesen Linien begrenzten Theil der Wasserfläche will ich Gelbes Meer nennen, so kann auch der Mathematiker durch sein Definiren nichts eigentlich schaffen.” (Frege, *Grundgesetze der Arithmetik*, p. XIII).

xl. “Wenn ich die Arithmetik mit einem Baume vergleiche, der sich oben in eine Mannichfaltigkeit von Methoden und Lehrsätzen entfaltet, während die Wurzel in die Tiefe strebt, so scheint mir der Wurzeltrieb, in Deutschland wenigstens, schwach zu sein. Selbst in einem Werke, das man dieser Richtung zuzählen möchte, der Algebra der Logik des Herrn E. Schröder, gewinnt doch bald der Wipfeltrieb wieder die Oberhand, bevor noch eine grössere Tiefe erreicht ist, bewirkt ein Umbiegen nach oben und eine Entfaltung in Methoden und Lehrsätze.” (Frege, *Grundgesetze der Arithmetik*, p. XIII). I should emphasise that this passage, though it immediately precedes the passage cited in footnote 43, and serves the same purpose (outlining why Frege has no reason to hope that his work will receive the reception he thinks it deserves), they make separate points and thus could have been written independently of each other.

xli. “Eine Besprechung von *Schröders* Algebra der Logik wäre mir selbstverändlich willkommen, wie alles aus Ihrer Feder. Bis jetzt habe ich jedoch noch kein Rezensionsexemplar erhalten. Wenn ein solches eintrifft, können Sie es sofort bekommen.” (Frege, *Wissenschaftlicher Briefwechsel*, p. 48).

xlii. “Durch eine Definition kann man nicht einen Gegenstand mit beliebigen Eigenschaften schaffen, noch einem leeren Namen oder Symbole beliebige Eigenschaften anzaubern.” (Frege, *Kleine Schriften*, p. 210).

xliii. “Solange es ununtersucht gelassen wird, ob es ‘eigentliche’ Gebiete gebe, welche die von den Symbolen 0, 1, ab, $a+b$ geforderten Eigenschaften besitzen, konnten wir sagen, dass unsre Definitionen die | Existenz des Definirten insofern verbürgen, als sie es gewissermassen selber erzeugen oder schöpferisch einführen.” (Schröder, *Vorlesungen über die Algebra der Logik*, pp. 211 sq.).

xliv. “Es wird der Logik gemeinhin zugemutet, dass sie auch erkläre, was unter Definition, Postulat, Axiom und Theorem zu verstehen sei, dass sie also namentlich auch auf die Erfordernisse einer guten Definition näher eingehe, desgleichen auf die Anforderungen, die an den *Beweis* (die ‘*Demonstration*’) zu stellen, durch welche das Theorem als ein solches nachgewiesen werden muss, durch welchen es von einer blossen Behauptung zum Lehrsatz erst erhoben wird. [...] Es erscheint jedoch durch die Anlage, den Plan des ganzen Buches geboten, dass wir uns *an dieser Stelle* auf diese Fragen nicht einlassen, vielmehr uns mit dem Hinweis begnügen, dass die fraglichen Begriffe, soweit sie nicht ohnehin schon Gemeingut sind, einstweilen wenigstens synthetisch erworben, herangebildet werden können an dem Material der aufzustellenden und als solche hingestellten speziellen Definitionen, an der grossen Zahl von mustergültig bewiesenen Theoremen, etc.” (Schröder, *Vorlesungen über die Algebra der Logik*, p. 166).

xlv. “Definition (3^+). Wenn es für gegebene Gebiete a, b und c zutrifft, dass zugleich $a \not\in c$ und $b \not\in c$ ist, so soll — kürzer — gesagt werden, es sei: $a + b \not\in c$.

Mit dieser Festsetzung haben wir definirt: die identische Summe als Subjekt.” (Schröder, *Vorlesungen über die Algebra der Logik*, p. 196).

xlvi. “Sobald wir aber jenen unter die ‘Gebiete’ aufgenommenen Symbolen eine Bedeutung unterlegen, behaupten, dass es der Anschauung zugängliche, wirkliche Gebiete gebe von den bezüglichen Eigenschaften, *fügen wir* unsern Definitionen gewisse *Postulate hinzu*, wir

stellen Forderungen über Gebietsnachweise als allgemein erfüllbare hin, bezüglich deren wir uns lediglich auf die Anschauung zu berufen vermögen." (Schröder, *Vorlesungen über die Algebra der Logik*, p. 212).

xlvii. "Wir werden die Gesetze dieses Kalkuls zunächst (unter Beihülfe der Wortsprache) in der allgemeinen Form mathematischer Beweisführung begründen, für welche seinerzeit die Geometrie des Euklides mustergültig geworden ist, um hernach in einem Rückblicke zu erkennen, dass bei den Schlüssen ebendieser Beweisführung nur die Prinzipien dieses Kalkuls selber angewendet worden sind.

Niemand, der für Reinheit der Methode und Konsequenz des Verfahrens Sinn besitzt, wird sich dem Eindruck der Schönheit und mathematischen Eleganz des damit geschaffenen wissenschaftlichen Systems verschliessen können. Freilich wird man, um diesen Eindruck ganz ungetrübt zu gewinnen, möglichst abzusehen haben von allem Beiwerk der hiernächst zu entwickelnden Theorie." (Schröder, *Vorlesungen über die Algebra der Logik*, p. 159).

xlviii. "Hieraus erhellt, dass aus der *Anschauung* nicht in gleichem Maasse die Überzeugung von der *Gewissheit* unsrer allgemeinen Untersuchungsergebnisse zu schöpfen ist, wie sie sich erreichen lassen wird durch die streng *analytische* Methode, deren wir uns fast immer, jedenfalls in wesentlichen Fragen ganz ausschliesslich bedienen. Kann doch in der That für die Umgrenzung eines Gebiets die Figur immer nur ein *Beispiel* darstellen, während unsre Gebiete irgendwie beschaffen sein, auch aus isolirten Punkten, Linien und getrennten Flächenstücken sollen bestehen dürfen! Mag also auch anfangs — bei unsern grundlegenden Betrachtungen — die Anschauung oft rasch vorauseilen dem durch das Folgende illustrirten modus procedendi, nämlich dem vorsichtigen und zuweilen mühsamen Verfahren des von der Anschauung losgelösten streng deduktiven Schliessens, so wird sie doch später sicher hinter diesem Verfahren zurückbleiben; sie wird ihm bald nachhinken und zuletzt es aufgeben müssen, dasselbe einzuholen. Bei dem Aufbau unsres Lehrgebäudes soll darum die Anschauung nur nebensächliche Verwendung finden, illustrationsweise, um den abstrakten logischen Prozeduren einen Vorstellungsinhalt zu geben; sie soll darin überhaupt nur eine didaktische, erziehende, pädagogische Rolle spielen." (Schröder, *Vorlesungen über die Algebra der Logik*, p. 171).

xlix. Before the passage about the general mathematical sequence of deductions I just referred to, Schröder claims that he will only use one primitive, which he calls subsumption, while all other symbols are introduced by definition: "Den Sinn eben *dieser* Beziehung setzen wir einzig und allein als bekannt voraus. Alle andern Begriffe und Beziehungen, die wir noch in den Bereich des identischen Kalkuls hereinzuziehen haben, werden ausschliesslich aus Beziehungen dieser Sorte, aus 'Subsumtionen' aufgebaut, sodass wir ungeachtet seiner später vollzogenen Erweiterungen und scheinbar grösseren Tragweite doch sagen können, der identische Kalkul beruhe einfach und ganz auf dem Studium der Subsumtionen." (Schröder, *Vorlesungen über die Algebra der Logik*, p. 159).

l. "Die Hauptmasse aber des mit der Theorie des identischen Gebietekalkuls hier zu verflechtenden Beiwerks wird von *sachlicher* Art sein, nämlich aus Nutzanwendungen des Kalkuls für die Zwecke der Logik selbst zu bestehen haben. Von diesen finden wir für gut, einen (ersten) Teil wenigstens gleich neben der Theorie einherlaufen zu lassen, und zwar den Teil, welcher abzielt auf die Verwertung des Kalkuls behufs Einkleidung in seine Zeichensprache zunächst derjenigen Beziehungen, welche zwischen Klassen oder Begriffsumfängen die Wortsprache auszudrücken vermag." (Schröder, *Vorlesungen über die Algebra der Logik*, p. 160).

li. "Begriffe und Sätze oder Formeln des 'identischen' Kalkuls (beziehungsweise des damit verwandten logischen, [...]) werden (überhaupt) die verschiedenartigsten Anwendungen zulassen, Anwendungen, die sich lediglich unterscheiden durch die Deutungsweise, Interpretation der hier als allgemeine Symbole verwendeten Buchstaben, und demgemäss auch der sie verknüpfenden Operations- und Beziehungszeichen. [A list of possible interpretations follows.]" (Schröder, *Vorlesungen über die Algebra der Logik*, p. 160).

lii. "Geblieben sind nur gewisse allgemeine Eigenschaften der Addition, und diese treten jetzt als die wesentlichen Merkmale der Größe hervor. So hat sich der Begriff von der Anschauung allmählich losgerungen und selbständing gemacht. Dies ist um so unbedenklicher als die frühere Anschaulichkeit im Grunde nur Schein war." (Frege, *Kleine Schriften*, p. 50).

liii. “Wenn wir den Größenbegriff, wie gezeigt, nicht in der Anschauung vorfinden, sondern selber schaffen, so ist der Versuch gerechtfertigt, diese Definition so einzurichten, daß sie eine möglichst mannigfaltige Anwendung gestattet, damit der Arithmetik ein möglichst großes Gebiet untertan werde.” (Frege, *Kleine Schriften*, p. 51).

liv. The passage I was quoting from continues: “Ganz allein bezüglich der Null werden wir solchen Nachweis als nicht ausführbar erkennen, und darf ich die Konstatirung des Gegenteils wol [sic] für den Augenblick als ein ‘negatives’ Postulat bezeichnen.”. (Schröder, *Vorlesungen über die Algebra der Logik*, p. 212).

lv. In his discussion of the logical sum and product, Schröder says that, if the sum or product of two given regions does not exist as a “proper” region, an improper region must be adjoined to serve as the interpretation of the sum- and product-expressions: “Auf unserm gegenwärtigen Standpunkt müssen wir noch darauf gefasst sein, dass [das logische Produkt und die logische Summe zweier Gebiete a und b] – je nach den Bedeutungen von a und b – sich als eigentliche Gebiete vielleicht nicht nachweisen lassen, sondern eben als ‘uneigentliche’ unsrer Mannigfaltigkeit zuzuschlagen, zu adjungiren sind.” (Schröder, *Vorlesungen über die Algebra der Logik*, p. 196).

lvi. Schröder discusses the status of adjunct regions in connection with the status of the object used as the interpretation of ‘0’: “Da die 0 in jedem Gebiet enthalten, allen gemeinsam sein soll, so kann sie nur ein ‘leeres’ Gebiet sein, welches kein Element der Mannigfaltigkeit enthält. Trotzdem als ein ‘Gebiet’ derselben charakterisirt, kann die Null auch nichts, was etwa ausserhalb der Mannigfaltigkeit läge, enthalten, sie kann nur dem Begriffe des ‘Nichts’ entsprechen. Nach Adjungirung des letzteren ist in der That dem Sprachgebrauch entsprechend zu sagen: Jedes Gebiet enthält seine eigenen Teile sowie Elemente und sonst ‘nichts’. Das Nullgebiet wird so von den eingeführten das einzige uneigentliche oder fingirte, eingebildete, angebliche Gebiet bleiben.” (Schröder, *Vorlesungen über die Algebra der Logik*, p. 212).

lvii. “In der Tat gibt es z.B. kein Gebiet, das in jedem der Gebiete der Staaten des Deutschen Reiches enthalten wäre. Herr Schröder läßt sich dadurch nicht stören. Die Definition verbürgt ihm die Existenz des Definierten insofern, als sie es gewissermaßen selber erzeugt und schöpferisch einführt (S. 212); freilich sehr gewissermaßen! Dem Symbole 0 hat er durch seine Definition die Eigenschaft zugeschrieben, in jedem Gebiete der Mannigfaltigkeit enthalten zu sein, und somit haben wir nun vermöge dieser | schöpferischen Definition ein Gebiet gewonnen, das in jedem Gebiete unserer Mannigfaltigkeit enthalten ist. Das ist freilich nur ein leeres Zeichen; aber da es die verlangte Eigenschaft hat, haben wir alles, was wir brauchen. So meint wenigstens Herr Schröder und begeht damit einen bei den Mathematikern beliebten Fehler, auf den ich wiederholt aufmerksam gemacht habe, ohne Eindruck damit zu machen; woraus wohl zu schließen ist, daß die Mathematiker ein durch Verjährung wohlerworbenes Recht auf seine Begehung haben.” (Frege, *Kleine Schriften*, pp. 204 sq.).

lviii. “Ich will also zeigen, daß man mit dieser Theorie nie recht Ernst macht und nicht Ernst machen kann, weil sie dann alsbald unnütz wird. Trotz der lebhaften Behauptung, daß die Zeichen leer und daß sie selbst die Zahlen sind, bleibt doch immer im Hintergrunde der Gedanke, daß sie etwas bedeuten, und daß dieser Inhalt des Zeichens eigentlich die Zahl ist. Das wird im Gebrauche des Wortes ‘Zeichen’ angedeutet; denn ist etwas ein Zeichen, was gar nichts bezeichnet und gar nicht den Zweck hat, etwas zu bezeichnen?” (Frege, *Kleine Schriften*, p. 105).

lix. There Frege summarises his critique as follows: “Es wird unnöthig sein, Thomaes Darstellung weiter durchzunehmen. Dieser Versuch einer formalen Arithmetik ist als gescheitert anzusehen schon darum, weil er nicht folgerecht durchgeführt werden kann. Die Zahlfiguren werden zuletzt doch wieder als Zeichen verwendet.” (Frege, *Grundgesetze der Arithmetik*, p. 138).

lx. “Freilich wird hier die schöpferische Macht durch den Zusatz eingeschränkt, dass jene Eigenschaften einander nicht widersprechen dürfen; eine selbstverständliche, aber sehr folgenschwere Einschränkung. Woran erkennt man, dass Eigenschaften einander nicht widersprechen? Kein anderes Kennzeichen scheint es dafür zu geben, als dass sich die fraglichen Eigenschaften an demselben Gegenstande vorfinden. Dadurch wird aber die Schöpfungsmacht, die viele Mathematiker sich zuerkennen, so gut wie werthlos. Denn sie müssen ja nun, bevor

sie eine Schöpfungsthat vollziehen, beweisen, dass die Eigenschaften einander nicht widersprechen, die sie dem zu schaffenden oder schon geschaffenen Gegenstande beilegen wollen; und das können sie wohl nur dadurch, dass sie beweisen, es gebe einen Gegenstand, welcher diese Eigenschaften sämmtlich habe. Können sie aber das, so brauchen sie nicht erst einen solchen zu schaffen." (Frege, *Grundgesetze der Arithmetik*, p. 145).

lxi. "Die Weiterbildung des Zahlbegriffs geschieht nun mittels der Verknüpfungs- und Rechnungsregeln der eben gebildeten ganzen Zahlen. Gewisse elementare, man könnte sagen, intuitiv verbindliche Regeln, mittels deren drei oder mehr der eben gebildeten ganzen Zahlen mit einander verbunden werden, dienen dazu, das Zahlensystem zu erweitern, indem gefordert wird, dass die Aufgabe, nach den Elementarregeln aus zwei Zahlen eine dritte zu bilden, unter allen Umständen ausführbar sei. Wo dies mit den vorhandenen Zahlen nicht möglich ist, da werden neue Zeichen, die dann auch Zahlen heissen, eingeführt. Diese Zahlen sind als rein inhaltslose Schemen aufzufassen, obschon es gelingt, auch diese Zahlen mit realen Objecten so zu verknüpfen, dass manche erst aus dieser Verknüpfung die Existenzberechtigung der Zahlen herleiten möchten. In der reinen Mathematik aber wird die Existenzberechtigung der neuer Zahlen einzig damit begründet, dass sich von ihnen die vom Rechnen mit den ganzen Zahlen abstrahirten Verknüpfungsregeln widerspruchslos ausführen lassen. In wie weit dies durchführbar ist, lehrt die Arithmetik. Dabei ergiebt sich, dass mehr Zahlensysteme möglich als gebräuchlich sind, aber es ist ein erheblicher Unterschied zwischen diesen Systemen, der es uns erleichtert, die angemessene Auswahl unter den möglichen System zu treffen. Das gemeine Zahlensystem kennzeichnet sich nämlich als ein nothwendiges, während die übrigen eben nur möglich sind, vielleicht als überflüssige bezeichnet werden können. Doch darauf wollen wir später zurückkommen." (Johannes Thomae. *Elementare Theorie der analytischen Functionen einer complexen Veränderlichen*. Verlag von L. Nebert, 1880, p. 2).

lxii. In his third lecture, Schröder provides such an interpretation and states, in a closing remark: "Hiermit aber haben wir den Boden der Realitäten betreten. Wir können aus dem anschaulichen Substrat die Gewissheit schöpfen, dass das System unsrer grundlegenden Definitionen und Prinzipien ein in sich konsistentes ist, dass dasselbe Widersprüche nicht in sich bergen kann, seine Teile miteinander verträglich sein müssen." (Schröder, *Vorlesungen über die Algebra der Logik*, p. 216).

lxiii. Where he says: "Welche Mittel hat man nun, um die Widerspruchslosigkeit zu beweisen? Ich sehe keinen andern Grundsatz, der zu diesem Zwecke dienen könnte, als den, daß Eigenschaften, die an demselben Gegenstande gefunden werden, nicht in Widerspruch miteinander stehen. Hätte man aber [einen] solchen Gegenstand, so wäre diese formale Theorie überflüssig." (Frege, *Kleine Schriften*, p. 110).

lxiv. "Es entsteht nun ebenso wie bei der Betrachtung der unendlich fernen Punkte das Bedürfnis, diese uneigentlichen Elemente nicht nur in gleicher Weise wie die eigentlichen zu behandeln, sondern auch vor Augen zu haben. Für die unendlich fernen Punkte der Ebene ist dies leicht bewirkt [...]. Das Entsprechende für imaginäre Gebilde zu leisten, soll im folgenden versucht | werden. Unter einer geometrischen Darstellung der imaginären Gebilde der Ebene verstehen wir demnach eine Art der Zuordnung, vermöge deren jedem reellen oder imaginären Elemente der Ebene ein reelles, anschauliches Element entspricht. Hierdurch wird zunächst der Vorteil erreicht, welcher bei jeder eindeutigen Zuordnung zweier Elementengebiete dadurch entsteht, daß man von bekannten Sätzen durch bloße Übertragung zu neuen Wahrheiten gelangt. Das Besondere dieses Falles aber ist, daß die unanschaulichen Beziehungen imaginärer Gebilde durch anschauliche ersetzt werden." (Frege, *Kleine Schriften*, pp. 2 sq.).

lxv. "Einer der folgenreichsten Fortschritte, welche die analytische Geometrie in neuerer Zeit gemacht hat, besteht darin, daß sie außer den Punkten noch andere Gebilde (Geraden, Ebenen, Kugeln) als Raumelemente betrachtet und durch Koordinaten bestimmt. Man kommt auf diesem Wege zu Geometrien in mehr als drei Dimensionen, ohne den Boden der Anschauung zu verlassen." (Frege, *Kleine Schriften*, p. 94).

lxvi. "Will man die häufig gestellte Frage beantworten, ob eine gewisse Zahl möglich oder unmöglich sei, so muss man sich zunächst über den

eigentlichen Sinn dieser Frage klar werden. Ein Ding, eine Substanz, die selbstständig ausserhalb des denkenden Subjects und der sie veranlassenden Objecte existirte, ein selbstständiges Princip, wie etwa bei den Pythagoreern, ist die Zahl heute nicht mehr. Die Frage von der Existenz kann daher nur auf das denkende Subject oder die gedachten Objecte, deren Beziehung die Zahlen darstellen, bezogen werden. Als unmöglich gilt dem Mathematiker streng genommen nur das, was logisch unmöglich ist, d.h. sich selbst widerspricht. Dass in diesem Sinne unmögliche Zahlen nicht | zugelassen werden können, bedarf keines Beweises. Sind aber die betreffenden Zahlen logisch möglich, ihr Begriff klar und bestimmt definirt und also ohne Widerspruch, so kann jene Frage nur darauf hinaus kommen, ob es im Gebiete des Realen oder des in der Anschauung Wirklichen, des Actuellen ein Substrat derselben, ob es Objecte gebe, an welchen die Zahlen, also die intellectuellen Beziehungen der bestimmten Art zur Erscheinung kommen." (Hermann Hankel. *Vorlesungen über die Complexen Zahlen und Ihre Funktionen. In Zwei Theilen: I. Theil: Theorie der Complexen Zahlensysteme.* Leopold Voss, 1867, pp. 6 sq.).

lxvii. "Ich gehe zum Begriffsumfang über. Das Wort schon deutet darauf hin, daß wir es hier nicht mit Räumlichem und Physischem zu tun haben, sondern mit Logischem. Mittels unserer logischen Fähigkeiten bemächtigen wir uns des Begriffsumfangs, indem wir vom Begriffe ausgehen." (Frege, *Nachgelassene Schriften*, p. 197).

lxviii. Kant first rejects the idea that empirical concepts can be defined in the strict sense of the word, which Kant takes to be "den ausführlichen Begriff eines Dinges innerhalb seiner Grenzen ursprünglich darstellen" (Immanuel Kant. *Kritik der reinen Vernunft.* Ed. by Jens Timmermann. Philosophische Bibliothek 505. Meiner Verlag, 1998, A727/B755)): "Nach einer solchen Foderung kann ein *empirischer* Begriff gar nicht definiert, sondern nur *expliziert* werden." (Kant, *Kritik der reinen Vernunft*, A727/B755) because, as Kant goes on to say, the limits of an empircal concept can never be finally drawn: "Man bedient sich gewisser Merkmale nur so lange, als sie zum Unterscheiden hinreichend sein; neue Bemerkungen dagegen nehmen welche weg und setzen einige hinzu, der Begriff steht also niemals zwischen sicheren Grenzen." (Kant, *Kritik der reinen Vernunft*, A728/B756). Then Kant also rejects that concepts "given a priori" can be defined because their content can never be finally exhausted (which contradicts the requirement of "Ausführlichkeit" Kant imposed on definitions): "Zweitens kann auch, genau zu reden, kein a priori gegebener Begriff definiert werden, z.B. Substanz, Ursache, Recht, Billigkeit etc. Denn ich kann niemals sicher sein, daß die deutliche Vorstellung eines (noch verworren) gegebenen Begriffs ausführlich entwickelt worden, also wenn ich weiß, daß dieselbe dem Gegenstande adäquat sei." (Kant, *Kritik der reinen Vernunft*, A728/B756). Kant concludes from these two rejections: "Also bleiben keine andere Begriffe übrig, die zum Definieren taugen, als solche, die eine willkürliche Synthesis enthalten, welche a priori konstruiert werden kann, mithin hat nur die Mathematik Definitionen." (Kant, *Kritik der reinen Vernunft*, A729/B757).

lxix. "Da also weder empirisch, noch a priori gegebene Begriffe definiert werden können, so bleiben keine andere als willkürlich gedachte übrig, an denen man dieses Kunststück versuchen kann. Meinen Begriff kann ich in solchem Falle jederzeit definieren; denn ich muß doch wissen, was ich habe denken wollen, da ich ihn selbst vorsätzlich gemacht habe, und er mir weder durch die Natur des Verstandes, noch durch die Erfahrung gegeben worden, aber ich kann nicht sagen, daß ich dadurch einen wahren Gegenstand definiert habe. Denn wenn der Begriff auf empirischen Bedingungen beruht, z.B. eine Schiffsuhr, so wird der Gegenstand und dessen Möglichkeit durch diesen willkürlichen Begriff noch nicht gegeben; ich weiß daraus nicht einmal, ob er überhaupt einen Gegenstand habe, und meine Erklärung kann besser eine Deklaration (meines Projekts) als Definition eines Gegenstandes heißen." (Kant, *Kritik der reinen Vernunft*, A729/B757).

lxx. "Ich bin beim Nachdenken über das Definieren immer strenger in meinen Anforderungen daran geworden und habe mich wohl so weit von der Meinung der meisten Mathematiker entfernt, dass eine Verständigung sehr erschwert ist." (Frege, *Wissenschaftlicher Briefwechsel*, p. 72).

lxxi. "[d]ie neue Seite einer Definition darf [nicht] mehr lateinische Buchstaben enthalten als die alte" (Albert Veraart. "Geschichte des

wissenschaftlichen Nachlasses Gottlob Freges und seiner Edition. Mit einem Katalog des ursprünglichen Bestands der nachgelassenen Schriften Freges." In: *Studien zu Frege. Band I: Logik und Philosophie der Mathematik*. Ed. by Matthias Schirn. Problemata 42. Frommann-Holzboog, 1976, pp. 49–106, p. 100; Scholz' list, as published by Veraart, reads "darf mehr" (without the "nicht"), which I take to be a misprint (due either to Veraart or Scholz) on account of Frege's guideline 7 in the first volume of the *Grundgesetze der Arithmetik*: "Es darf also nie auf der einen Seite einer Definitionsgleichung ein lateinischer Buchstabe vorkommen, der nicht auch auf der andern steht." (Frege, *Grundgesetze der Arithmetik*, p. 53))

lxxii. "Das Wort 'Deutung' ist zu beanstanden; denn ein Gedanke, richtig ausgedrückt, läßt für verschiedene Deutungen keinen Raum. Wir haben gesehen, daß die Vieldeutigkeit durchaus zu verwerfen ist, und wie der Schein ihrer Notwendigkeit bei mangelhafter logischer Einsicht entstehen kann." (Frege, *Kleine Schriften*, p. 301); a little later Frege states the problem that the variability of interpretations conflicts with the principle of univocity explicitly and concisely: "Deuten läßt sich vielleicht ein Zeichen oder eine Gruppe von Zeichen, wiewohl die Eindeutigkeit der Zeichen, an der wir unbedingt festhalten müssen, verschiedene Deutungen ausschließt." (Frege, *Kleine Schriften*, p. 303).

lxxiii. "Man wird nicht einfach sagen können 'das Parallelenaxiom'; denn in den verschiedenen Geometrien wird es verschiedene Parallelenaxiome geben. Wenn der Wortlaut derselbe sein sollte, so ist das nur fehlerhafterweise dadurch bewirkt worden, daß man z.B. statt 'Gerade' der A-Geometrie einfach 'Gerade' sagt, wodurch die Verschiedenheit des Gedankeninhalts zwar verhüllt, aber nicht aufgehoben werden kann." (Frege, *Kleine Schriften*, p. 272).

lxxiv. "Diese Definition reicht aus, denn alle|mal, wenn wir irgend ein Ding vorgelegt sehen, kann sich aus dieser Definition entscheiden ob das vorgelegte Ding ein Reihungsgesetz ist oder nicht. Das ist aber meiner Ansicht das wahrhaft einzige Merkmal einer Definition. Danach eine Definition mit negativen Merkmalen nicht fehlerhaft." (Wissenschaftliches Tagebuch I, pp. 39f.)

lxxv. "Eine Definition kann niemals 'falsch' sein auch kann sie niemals unlogisch sein. Denn sie ist etwas rein sprachliches. Demgemäss kann sie unverständlich sein oder mehrfacher Interpretation fähig. Nur in diesem Falle ist sie ohne weiteres fort zu werfen und unbrauchbar. Ausserdem kann das zu definirende Wort sich mit dem sonst üblichen Gebrauch in Widerspruch befinden. Dies ist aber kein wesentlicher Uebelstand." (Wissenschaftliches Tagebuch III, p. 18)

lxxvi. "Ich habe gelesen, dass Wortdefinitionen getadelt werden und als solche hingestellt werden, die eigentlich nicht mehr vorkommen dürften. Es wurde dabei beispielsweise auf eine von mir gegebene Definition hingewiesen; aber es war nicht gesagt, was eine Wortdefinition sei." (Frege, *Nachgelassene Schriften*, p. 232).

lxxvii. Scholz description reads: "(Korseltantwort). Entwürfe, Inhaltsangabe, Reinschrift. [...] Teile davon sind zu Anmerkungen bestimmt. Inhalt: Bedeutung in der Wissenschaft. Begriff. Subsumtion. Satzteile. Sprachliche Schwierigkeit bei 'Begriff'. [...] Gänsefüßchen, Sinn und Bedeutung." (Veraart, "Geschichte des wissenschaftlichen Nachlasses Gottlob Freges und seiner Edition," p. 100).

lxxviii. "Die hier besprochenen Aufsätze Korselts beinhalten wenige originelle Gedanken. Sie sind im wesentlichen Kommentare und Auseinandersetzungen mit Schriften anderer Autoren, vor allem mit Ernst Schröder [...]; Gottlob Frege [...]; Arthur Schoenflies [...]; Heinrich Weber [...]; Theodor Vahlen [...]; Voß [...]." (Volker Peckhaus. "Alwin Reinhold Korselt." In: *Arbeitsberichte aus dem DFG-Projekt Fallstudien zur Begründung einer Sozialgeschichte der Logik* 6 [1986]. Ed. by Volker Peckhaus, pp. 10–24, p. 19). All omissions in the following quotation are of bibliographic references.

lxxix. "In der Dichtung haben die Wörter freilich nur einen Sinn, aber in der Wissenschaft und überall, wo uns die Frage nach der Wahrheit beschäftigt, wollen wir uns nicht mit dem Sinne begnügen, sondern auch eine Bedeutung mit den Eigennamen und Begriffswörtern verbinden; und wenn wir es etwa aus Versehen doch nicht tun, so ist das ein Fehler, der leicht unser Nachdenken zuschanden machen

kann." (Frege, *Nachgelassene Schriften*, p. 128). Frege re-iterates this conviction once more at a later point in the same manuscript: "Und der Dichtung genügt | der Sinn, der Gedanke auch ohne Bedeutung, ohne Wahrheitswert, aber nicht der Wissenschaft." (Frege, *Nachgelassene Schriften*, pp. 133 sq.).

lxxx. "Der Satz hat einen Sinn, und den Sinn eines Behauptungssatzes nennen wir den Gedanken. Ein Satz wird entweder mit oder ohne behauptende Kraft ausgesprochen. Für die Wissenschaft genügt es nicht, dass ein Satz nur einen Sinn habe; er muss auch einen Wahrheitswert haben und diesen nenn wir die Bedeutung des Satzes. Wenn ein Satz nur einen Sinn, aber keine Bedeutung hat, so gehört er der Dichtung, nicht aber der Wissenschaft an." (Frege, *Nachgelassene Schriften*, p. 262).

lxxxi. "Bei einem Gedanken kümmert uns am meisten die Frage nach seinem Wahrsein. Einen wahren Gedanken kann man am bequemsten eine Wahrheit nennen. Eine Wissenschaft ist ein Ganzes verbundener Wahrheiten. Ein von uns gefasster Gedanke drängt immer auf Beantwortung der Frage nach seinem Wahrsein. Die Anerkennung des Wahrseins eines Gedankens oder, wie wir auch sagen können, die Anerkennung einer Wahrheit geben wir kund, indem wir einen Satz mit behauptender Kraft aussprechen. [...] Wer die euklidische Geometrie für wahr hält, wird jedem ihrer Lehrsätze einen Sinn zuerkennen, er wird in jedem ihrer Lehrsätze eine Wahrheit ausgedrückt finden. Damit ist von selbst gegeben, dass er die Begriffswörter 'Punkt', 'Gerade', 'Ebene' als sinnvoll anerkennt." (Frege, *Nachgelassene Schriften*, p. 183).

lxxxii. "Nehmen wir an, es werde die Wissenschaft von diesen Zahlen aufgebaut. Ohne Zweifel muss sie gänzlich verschieden sein von der Wissenschaft, in der man Zahlen gewisse Gebilde nennt, die mit einem Schreibmittel auf eine Schreibfläche hergestellt werden. Auch wenn der Wortlaut derselbe ist, muss der ausgedrückte Gedanke ganz verschieden sein. Nun ist es auffallen, dass die Sätze dieser grundverschiedenen Wissenschaften, deren jede Arithmetik genannt wird, im Wortlaute ganz übereinstimmen." (Frege, *Nachgelassene Schriften*, p. 233).

lxxxiii. "Alle Wissenschaften haben das Bestreben, einfachste gegenständliche reine Lehrbegriffe zu werden, nur stellen sich den meisten ernste Schwierigkeiten in der Auffindung passender Grundaussagen in den Weg, z.B. schon der rationellen Mechanik." (Alwin Korselt. "Über die Grundlagen der Geometrie." In: *Jahresberichte der Deutschen Mathematiker-Vereinigung* 12 [1903], pp. 402–407, p. 404).

lxxxiv. "Die Forderung an eine formale Theorie, daß sie ihren nach Art von Eigennamen oder Begriffsnamen gebildeten 'Figuren' (Namen), z.B. 'Punkt', eine *bestimmte* Bedeutung gebe, ist unzweckmäßig und unbillig, im Gegenteil muß ein reiner Lehrbegriff seinen 'Namen' einen möglichst weiten | Deutungsbereich offen halten, auch wenn eine bestimmte Deutung beabsichtigt ist, um für möglichst viele Erfahrungen gleichzeitig einen Zusammenhang zu schaffen. Sätze desselben Wortlautes sollen wenn möglich nur einmal bewiesen werden, mögen sie auch in verschiedenen Gebieten auftreten. Nur so erreicht man eine Übersicht über Vollständigkeit, Reinheit und Unabhängigkeit der Grundsätze und Grundbegriffe." (Korselt, "Über die Grundlagen der Geometrie," pp. 406 sq.).

As this passage shows, Korselt seems to be using the phrase "formale Theorie" and "reiner Lehrbegriff" interchangeably; (Frege also understood them to be interchangeable, cf. Frege, *Kleine Schriften*, p. 317). I will therefore always translate both into English as "formal theory".

lxxxv. "Das Wesen der Erklärung besteht ja gerade darin, dass sie eine grosse, vielleicht unübersehbare Mannigfaltigkeit durch einen oder wenige Sätze beherrscht. Ihr Wert kann geradezu durch diese Verdichtung und Vereinfachung gemessen werden; er ist 0, wenn die Zahl der Annahmen so gross wie die der zu erklärenden Tatsachen ist." (Frege, *Nachgelassene Schriften*, p. 40).

lxxxvi. "Wer einen logischen und möglichst einfachen Zusammenhang unseres gesamten Erkennens will, darf daher von einem reinen Lehrbegriff nicht von vornherein Gegenständlichkeit verlangen. Mag man ihn dann auch 'leeres Zeichenspiel, nichts bedeutend' und dergleichen nennen, als streng gesetzlicher Zusammenhang von Sätzen hat er keine besondere Würde mehr nötig. Ein reiner Lehrbegriff ist so lange merkwürdig, als die in ihm etwa vorkommenden Sätze nicht zu einem Widerspruch führen, ja, er bleibt auch als widerspruchsvoller

Lehrbegriff interessant, wenn der Widerspruch erst nach einer längeren Reihe von Schlüssen über die Gegenstände des Lehrbegriffs offenbar wird." (Korselt, "Über die Grundlagen der Geometrie," p. 403).

That Korselt speaks of "the objects of the formal theory" strikes me as another symptom of his ambivalence between treating formal theories (as he does earlier in this passage) as simply meaningless; and treating them as meaningful in some looser way than being associated uniquely with a particular interpretation.

lxxxvii. "Nun ist es ganz richtig, dass wir unsere Regeln des Schliessens und die andern Gesetze der Begriffsschrift auch hätten einführen können als willkürliche Festsetzungen, ohne irgend von der Bedeutung und dem Sinne der Zeichen zu sprechen. Diese Zeichen würden dann eben als Figuren behandelt. Was uns als äussere Darstellung eines Schlusses galt wäre dann einem Zuge des Schachspiels vergleichbar [...]. Man könnte jemandem unsere Formeln I bis VI und die Definitionen *A* bis *H* des ersten Bandes als Ausgangspunkte geben – vergleichbar der Grundstellung der Schachfiguren –, ihm die Regeln sagen, nach denen er Umformungen vornehmen dürfte, und nun die Aufgabe stellen, unsern Satz (71) des ersten Bandes von jenem Ausgangspunkt aus zu erreichen; alles diese, ohne dass er eine | Ahnung von Sinn und Bedeutung dieser Zeichen hätte, noch von den Gedanken, deren Ausdruck die Formeln sind. Es wäre sogar denkbar, dass diese Aufgabe ebenso gelöst würde, wie wir es gethan haben. Dass dabei geistige Arbeit geleistet werden müsste, versteht sich von selbst, ebenso wie bei einer ähnliche Aufgabe des Schachspiels, von einer Grundstellung aus zu einer gegebenen Endstellung gemäss den Regeln des Spiels zu gelangen, wobei von Gedanken, die durch die verschiedenen Stellung ausgedrückt würden, keine Rede wäre, und kein Zug als Schluss gedeutet werden könnte. Obwohl also geistige Arbeit geleistet würde, fehlte doch ganz der Gedankengang, der bei uns die Sache begleitet und ihr eigentlich erst Interesse verliehen hat." (Frege, *Grundgesetze der Arithmetik*, pp. 99 sq.).

lxxxviii. "Wenn man fragt, worin denn eigentlich der Wert mathematischer Erkenntnisse liege, so muss die Antwort sein: weniger in dem Was des Wissens als in dem Wie, weniger in dem Wissensstoffe als in dem Grade seiner geistigen Durchleuchtung und der Einsicht in den logischen Zusammenhang." (Frege, *Nachgelassene Schriften*, p. 171).

lxxxix. So Frege says, for example, in an early unpublished manuscript simply entitled "Logik": "Die Logik hat eine nähere Verwandtschaft mit der Ethik. Für diese hat die Eigenschaft 'gut' eine ähnliche Bedeutung wie für jene die Eigenschaft 'wahr'." (Frege, *Nachgelassene Schriften*, p. 4).

xc. "So scheint das Wort 'wahr' das Unmögliche möglich zu machen, nämlich das, was der behauptenden Kraft entspricht, als Beitrag zum Gedanken erscheinen zu lassen. Und dieser Versuch weist, obwohl er misslingt oder vielmehr gerade durch sein Misslingen, auf die Eigenart der Logik hin, und diese scheint hiernach wesentlich von der der Ästhetik und Ethik verschieden. Denn das Wort 'schön' weist doch wohl wirklich auf das Wesen der Ästhetik hin, wie auch 'gut' auf die Ethik während 'wahr' eigentlich nur einen missglückten Versuch macht, auf die Logik hinzuweisen, indem das, worauf es eigentlich dabei ankommt, gar nicht in dem Worte 'wahr' liegt, sondern in der behauptenden Kraft, mit der der Satz ausgesprochen wird." (Frege, *Nachgelassene Schriften*, p. 272).

xci. "Warum sollen die Axiome und Lehrsätze keine Zeichen enthalten, deren 'Bedeutung' nicht vorher feststeht? Da muß man doch wieder fragen: Was 'bedeutet' überhaupt die Aussage, daß die Bedeutung eines Zeichens feststände? Sehr vieles. Entweder: daß man dies Zeichen vorkommendenfalls als dasselbe Zeichen für denselben Gegenstand wiedererkennt, oder. daß man alle oder doch gewisse einfache das Zeichen benutzende Sätze immer als wahre, falsche oder als Definitionen wiedererkennt. 'Der Mont Blanc ist 4800m hoch' wird von vielen verstanden, die den Berg und eine Höhe von 4800m ihr Lebelang nicht gesehen haben, weil sie diesen Satz in bestimmter Weise in den Zusammenhang der ihnen geläufigen Aussagen einzuordnen verstehen. Sollte man sich schließlich über die 'Bedeutung' eines Ausdruckes nicht einigen können, so ist dies nur ein Zeichen dafür, daß sich der eine oder andere mehr Sätze über dies Zeichen oder mit diesem Zeichen zu eigen machen muß. 'Das Zeichen hat keine Bedeutung' wird also heißen: 'Uns sind keine Sätze bekannt, die den Gebrauch dieses Zeichens überhaupt oder in einem gegebenen Gebiete regeln'." (Korselt, "Über die Grundlagen der Geometrie," p. 402).

xcii. Korselt's original remark is longer than the sentence I quoted in translation, it reads: "Die Zeichen einer formalen Theorie haben überhaupt keine 'Bedeutung', die Gesetze derselben geben nur Regeln, denen etwa beabsichtigte Deutungen unterliegen und nicht beabsichtigte unterliegen sollen. Die Regeln und Sätze jedes Spiels, z.B. des Schachspiels, sind dafür das beste Beispiel." (Korselt, "Über die Grundlagen der Geometrie," p. 407). Frege, however, also only quoted the scrap of this passage, which I quoted in translation. He then proceeds to comment on it as follows: "Nun hält [Korselt] offenbar die Hilbertsche Theorie für eine formale. Also kann es in ihr keine Erklärung der Zeichen geben, durch die diesen Zeichen Bedeutungen gegeben würden." (Frege, *Kleine Schriften*, p. 290).

xciii. "Wenn es vielfach unmöglich ist, über die Zugehörigkeit eines Gegenstandes zu einem gewissen Begriffe zu entscheiden, was nutzt dann die 'scharfe Definition' dieses Begriffes? Man weiß nicht einmal, ob die Definition scharf ist. Man muß dann doch fragen: woran erkennt man, daß man den hervorzuhebenden Begriff scharf definiert hat? Doch wohl nur aus den bekannten Verbindungen, in denen der Begriff vorkommt, oder daran, daß sich mit ihm etwas 'machen' läßt, daß er in bestimmter Weise in einem gesetzmäßigen und *deutlichen* Zusammenhange von Bewußtseinserlebnissen auftritt. Was aber 'deutlicher gesetzmäßiger Zusammenhang von Bewußtseinserlebnissen' ist, läßt sich zwar nicht per genus proximum et differentiam specificam *definieren*, ist aber leicht an Beispielen aufzuzeigen und verständlich zu machen. Eine Vorstellung, die in keinem gesetzlichen Zusammenhange mit andern Vorstellungen auftritt, ist belanglos, mag sie scharf oder unscharf sein. Es kommt für den Beweis allein darauf an, richtige Schlüsse zu ziehen und sämtliche benutzte Grundsätze und Schlußregeln zu erkennen. Damit ist die besondere Forderung 'scharfer Begriffe' überflüssig geworden." (Alwin Korselt. "Über die Grundlagen der Mathematik." In: *Jahresberichte der Deutschen Mathematiker-Vereinigung* 14 [1905], pp. 465–388, p. 368).

xciv. "Damit sich hierbei [in the investigation of the foundations of arithmetic] nicht unbemerkt etwas Anschauliches eindrängen könnte, musste Alles auf die Lückenlosigkeit der Schlusskette ankommen. Indem ich diese Forderung auf das strengste zu erfüllen trachtete, fand ich ein Hindernis in der Unzulänglichkeit der Sprache, die bei aller entstehenenden Schwerfälligkeit des Ausdrucks doch, je verwickelter die Beziehungen wurden, desto weniger die Genauigkeit erreichen liess, welche mein Zweck verlangte. Aus diesem Bedürfnisse ging der Gedanke der vorliegenden Begriffsschrift hervor. Sie soll also zunächst dazu dienen, die Bündigkeit dieser einer Schlusskette auf die sicherste Weise zu prüfen und jede Voraussetzung, die ich unbemerkt einschleichen will, anzuzeigen, damit letztere auf ihren Ursprung untersucht werden könne." (Gottlob Frege. *Begriffsschrift und andere Aufsätze*. Ed. by Ignacio Angelelli. 2nd ed. Verlag Georg Olms, 2007, p. X).

xcv. "Die Frage: 'was ist eine Zahl'? birgt schon eine Beschränkung in sich, sie verlangt eine Bestimmung des Gegenstandes 'Zahl' aus seinen *inneren* Beschaffenheiten. Viele Gegenstände, z.B. 'Punkt', können wir aber nicht durch Eigenschaften, sondern nur durch Verhältnisse, nämlich Beziehungen zu andern Gegenständen festlegen. Und so muß hier die Frage lauten: welche Eigenschaften eines Inbegriffes oder welche Beziehungen zwischen Inbegriffen werden durch den Namen 'Inbegriff von Zahlen' angedeutet? Die Antwort hierauf geben diejenigen Grundsätze, in denen das Wort 'Zahl' vorkommt." (Korselt, "Über die Grundlagen der Mathematik," p. 372).

xcvi. "Hilbert geht von drei Arten mathematischer Objekte aus, die Punkt, Gerade, Ebene genannt werden, und von gewissen mathematischen Beziehungen, die diese Objekte betreffen. Ihre nähere Aufzählung ist hier nicht nötig. Nur an zweierlei muß ich erinnern. Erstens wird zunächst nur eine Axiomgruppe eingeführt, nämlich die der Verknüpfung, und zweitens sind es bei jeder Axiomgruppe *nur* die in ihr enthaltenen *Beziehungen*, die den materiellen bzw. den mathematischen Inhalt der Axiome ausmachen. Diese Beziehungen sind es daher, die die Grundlage aller Sätze über die Verknüpfungen der drei Arten von Objekten bilden (wie z.B. für den Desarguesschen Satz); sie bilden aber auch, was ich ausdrücklich anführe, die *alleinige* und *ausschließende* Grundlage aller derartigen Sätze. Dagegen sind die Namen der Objekte, wenigstens soweit die Eigenart der axiomatischen Methode in Betracht kommt, belanglos." (Arthur Schoenflies. "Über die Stellung der Definition in der Axiomatik." In: *Jahresberichte der Deutschen Mathematiker-Vereinigung* 20 [1911], pp. 222–254, p. 225).

xcvii. Skolem says: "Eine 'Menge' bedeutet nach der Axiomatik nicht eine beliebig definierte Zusammenfassung; die Mengen sind nur Dinge,

die mit einander durch gewisse, durch die Axiome ausgedrückte Beziehungen verbunden sind. [...] | Also: *Die axiomatische Begründung der Mengenlehre führt zu einer Relativität der Mengenbegriffe, und diese ist mit jeder konsequenten Axiomatik untrennbar verknüpft.* Die Relativität rührt davon her, dass die Dinge in B eine andere und weit eingeschränktere Bedeutung haben, als das überhaupt Definierbare. Dass diese Relativität mit jeder konsequenten axiomatischen Begründung untrennbar verknüpft sein muss ist klar; denn sie beruht auf den oben erwähnten allgemeinen Sätzen der mathematischen Logik." Thoralf Skolem: Selected Logical Works (ed. Jens Erik Fenstad), p. 143f.

xcviii. "Das obige Beispiel des Euklidischen Punktes [Korselt had claimed on the previous page (p. 406) that it was impossible to assign a unique extension to the concept 'Euclidian point'] zeigt, daß man nicht mit Herrn Frege die Auflösbarkeit jedes Systems von Grundsätzen nach den in ihnen vorkommenden Unbekannten (Grundbegriffen) fordern darf, vollends nicht die eindeutige Auflösung; das liefe auf unbeschränktes Definieren hinaus." (Korselt, "Über die Grundlagen der Geometrie," p. 407).

xcix. "Meine Meinung ist diese. Wir müssen logische Urelemente anerkennen, die nicht definierbar sind." (Frege, *Kleine Schriften*, p. 288).

c. The previous passage I quote about "logische Urelemente" continues: "Auch hierbei stellt sich das Bedürfnis ein, sicherzustellen, daß man mit demselben Zeichen (Worte) dasselbe bezeichnet. Wenn sich die Forscher über diese Urelemente und ihre Bezeichnungen verständigt haben, ist das Einverständnis über das logisch Zusammengesetzte durch Definition leicht erreichbar. Da bei den Urelementen diese nicht möglich sind, muß hier etwas anderes eintreten; ich nenne es Erläuterung. Diese dient also den Zwecken der Verständigung der Forscher untereinander und der Mitteilung der Wissenschaft. [...] Da ohne gegenseitiges Verständnis der Forscher ein Zusammenarbeiten an der Wissenschaft nicht möglich ist, muß man das Vertrauen haben, daß ein solches Verständnis durch Erläuterungen erreicht werden kann, obwohl theoretisch das Gegenteil nicht ausgeschlossen ist." (Frege, *Kleine Schriften*, p. 288).

ci. "Kerry bestreitet das, was er meine Definition von Begriff nennt. Da möchte ich nun zunächst bemerken, daß meine Erklärung nicht als eigentliche Definition gemeint ist. Man kann auch nicht verlangen, daß alles definiert werde, wie man auch vom Chemiker nicht verlangen kann, daß er alle Stoffe zerlege. Was einfach ist, kann nicht zerlegt werden, und was logisch einfach ist, kann nicht eigentlich definiert werden. Das Logischeinfache ist nun ebensowenig wie die meisten chemischen Elemente von vornherein gegeben, sondern wird erst durch wissenschaftliche Arbeit gewonnen. Wenn nun etwas gefunden ist, was einfach ist oder wenigstens bis auf weiteres als einfach gelten muß, so wird eine Benennung dafür zu prägen sein, da die Sprache einen genau entsprechenden Ausdruck ursprünglich nicht haben wird. Eine Definition zur Einführung eines Namens für Logischeinfaches | ist nicht möglich. Es bleibt dann nichts anderes übrig, als den Leser oder Hörer durch Winke anzuleiten, unter dem Worte das Gemeinte zu verstehen." (Frege, *Kleine Schriften*, pp. 167 sq.). A transcription of an earlier draft of this articles survives, and is published in *Nachgelassene Schriften*. The parallel passage there reads: "Man kann nicht alles definieren, ebensowenig, | wie man alle Stoffe chemisch zerlegen kann. Voraussetzung ist in beiden Fällen die Zusammengesetztheit. Man muss sich in vielen Fällen damit begnügen, den Leser durch Winke dazu anzuleiten, unter dem Worte das Gemeinte zu verstehen." (Frege, *Nachgelassene Schriften*, pp. 97 sq.).

cii. "Nachdem wir uns über das Sprachliche verständigt haben, mag es uns leichter werden, das gegenseitige Verstehen auf das Gedankliche auszudehnen, das in der Sprache sich abbildet. Nicht auf das gewöhnliche Verstehn der Sprache kommt es hier an, nicht auf das Fassen der in ihr ausgedrückten Gedanken, sondern auf das Erfassen der Eigenschaft von Gedanken, die ich logische Allgemeinheit nenne. Freilich muss dabei auf ein Entgegenkommen des Andern gerechnet werden, und diese Erwartung kann getäuscht werden. Auch erfordert der Gebrauch der Sprache Vorsicht. Wir dürfen nicht die tiefe Kluft übersehen, die doch die Gebiete des Sprachlichen und des Gedanklichen trennt, und durch die dem gegenseitigen Entsprechen beider Gebiete gewisse Schranken gesetzt sind." (Frege, *Nachgelassene Schriften*, p. 279).

ciii. "Meine Anstrengungen, Licht in die Frage zu bringen, die sich an das Wort 'Zahl', an die einzelnen Zahlwörter und an die Zahlzeichen

geheftet haben, scheinen in einem völligen Mißerfolg geendet zu haben. Doch sind diese Anstrengungen nicht ganz vergeblich gewesen. Gerade durch diesen Mißerfolg können sie zu einer Erkenntnis führen. Die Schwierigkeiten dieser Untersuchungen werden of sehr unterschätzt." (Frege, *Nachgelassene Schriften*, p. 284). On the following page, Frege explains the difficulties further: "Diese Untersuchungen sind besonders schwierig, weil wir uns gerade in ihnen durch die Sprache leicht irre führen lassen, durch die Sprache, die uns doch ein unentbehrliches Werkzeug dabei ist." (Frege, *Nachgelassene Schriften*, p. 285). This description recalls a diary entry, which survives in transcription, where Frege reports: "Ja, bei längerer Beschäftigung mit diesen Fragen kommt man zu der Vermutung, daß der Sprachgebrauch irreführe, daß die Zahlwörter gar nicht Eigennamen von Gegenständen sind und die Wörter wie 'Zahl', 'Quadratzahl' u. dgl. keine Begriffswörter sind, so daß durch einen Satz wie 'Vier ist eine Quadratzahl' gar nicht die Subsumtion eines Gegenstandes unter einen Begriff ausgedrückt wird und also dieser Satz gar nicht aufgefaßt werden kann wie der Satz 'Sirius ist ein Fixstern'. Wie aber dann?" (Frege, *Nachgelassene Schriften*, p. 282).

civ. "Eine für die Zuverlässigkeit des Denkens verhängnisvolle Eigenschaft der Sprache ist ihre Neigung, Eigennamen zu schaffen, denen kein Gegenstand entspricht. Wenn das in der Dichtung geschieht, die jeder als Dichtung versteht, so hat das keinen Nachteil. Anders ist es, wenn es in einer Darlegung geschieht, die den Anspruch auf strenge Wissenschafltichkeit macht. Ein besonders merkwürdiges Beispiel dazu ist die Bildung eines Eigennamens nach dem Muster 'der Umfang des Begriffes a', z.B. 'der Umfang des Begriffes Fixstern'. Dieser Ausdruck scheint einen Gegenstand zu bezeichnen wegen des bestimmten Artikels; aber es gibt keinen Gegenstand, der sprachgemäss | so bezeichnet werden könnte. Hieraus sind die Paradoxien der Mengenlehre entstanden, die diese Mengenlehre vernichtet haben. Ich selbst bin bei dem Versuche, die Zahlen logisch zu begründen, dieser Täuschung unterlegen, indem ich die zahlen als Mengen auffassen wollte. Es ist schwer, einen allgemein üblichen Ausdruck zu vermeiden, wenn man die Fehler, die daraus entspringen können, noch nicht kennengelernt hat. Es ist gar schwer, vielleicht unmöglich, jeden Ausdruck, den uns die Sprache darbietet, auf seine logische Unverfänglichkeit zu prüfen. So besteht denn ein grosser Teil der Arbeit des Philosophen – oder sollte wenigstens bestehen – in einem Kampfe mit der Sprache. Aber vielleicht sind nur wenige sich dieser Notwendigkeit bewusst." (Frege, *Nachgelassene Schriften*, pp. 288 sq.).

cv. So Frege goes on to say in the manuscript entitled "Erkentnisquellen der Mathematik und Naturwissenschaften": "Freilich, wenn man dem Entstehen des Denkens in der Entwicklung des einzelnen nachgeht, kann man das Denken wohl ein unhörbares inneres Sprechen nennen; aber das eigentliche Wesen des Denkens wird dadurch nicht bezeichnet. Kann der Mathematiker nicht auch in Formeln denken? Auch die mathematische Formelsprache ist ein Geschöpf des Menschen wie die Lautsprache, aber von dieser grundverschieden. Jene Eigenheiten der Lautsprache, die, wie wir gesehen haben, zu logischen Fehlern verführen, können hierbei vermieden werden. Doch ist der Einfluss der Sprache so gross, dass es nicht immer geschieht. Wenn wir also davon absehen, wie beim einzelnen das Denken ins Bewusstsein tritt, dagegen das eigentliche Wesen des Denkens betrachten, werden wir es nicht dem Sprechen gleichsetzen können. Dann werden wir nicht das Denken aus dem Sprechen ableiten; sondern das Denken erscheint dann als das Erste, und die an der Sprache bemerkten logischen Mängeln können wir nicht dem Denken zur Last legen." (Frege, *Nachgelassene Schriften*, p. 289).

cvi. "Übrigens will ich die wissenschaftliche Tätigkeit, die in der Erschaffung solcher Wortdefinitionen liegt, keineswegs herabsetzen; nur ihre *mathematische Entbehrlichkeit* und *Unverwendbarkeit* sollte hier betont werden. Werden sie insbesondere für die grundlegenden Beziehungen benutzt (wie z.B. bei Frege) so heißt dies eine axiomatische mathematische Beziehung auf unmathematische Worte von unsicherer Bedeutung zurückführen." (Schoenflies, "Über die Stellung der Definition in der Axiomatik," p. 234).

cvii. Schoenflies criticises "allgemeine Wortdefinitionen [...], die oft nur ein Wort durch andere ihm gleichwertige zu bestimmen vermögen, die überdies nur mit mehr oder weniger | unbestimmten Ausdrücken operieren, und bei denen man niemals sicher ist, daß sich der Leser dasselbe denkt wie der Autor, die sie geschaffen" (Schoenflies, "Über die Stellung der Definition in der Axiomatik," pp. 233 sq.); especially the last clause of this characterisation is reminiscent of what Frege wrote about elucidations in his response to Korselt.

cviii. "Um die Arbeit, die die axiomatische Methode auf einem Wissensgebiet zu leisten hat, allgemeiner zu charakterisieren, muß ich einen

Augenblick die Frage streifen, woher die Begriffe und Beziehungen stammen, die den Gegenstand der mathematischen Forschung bilden. Selbstverständlich teilweise aus der Empirie und teilweise aus der Phantasie. Mathematisch verwendbar werden sie aber insgesamt erst dann, wenn es gelingt, ihnen ein mathematisches Gepräge zu geben, sie mit einem *eindeutigen* vielmehr *völlig neuen* Inhalt zu erfüllen, so daß sie sich *logisch vollkommen* verhalten, und den Aufbau eines *kontradiktorisch* gefügten Wissens gewährleisten. Dies gilt für alle Begriffe, mit denen man operiert, mögen sie grundlegend sein oder nicht, mag es sich um Punkte, Ebenen, Strecken und Winkel handeln, um die Funktionen oder das Kontinuum, um die Menge oder die Begriffe endlich | und unendlich. Ihre eventuelle Reinigung und Umbildung, kurz ihre *Erhebung zu mathematischen Objekten* ist das, was die axiomatische wie überhaupt die mathematische Methode in erster Linie zu leisten hat, und was insbesondere Hilbert für die aus der allgemeinen Empirie stammenden geometrischen Vorstellungen durchgeführt hat." (Schoenflies, "Über die Stellung der Definition in der Axiomatik," pp. 226 sq.).

Schoenflies conception of mathematics – as a science all of whose concepts obey, on the one hand, the law of excluded middle and the law of non-contradiction, and, on the other hand, can always be exhausted by a finite number of sub-concepts (allowing the use of proofs by cases); cf.: Schoenflies, "Über die Stellung der Definition in der Axiomatik," p. 224 – would deserve some attention on its own, but this is not the place.

cix. "Man kann sie [Erläuterung] einer Propädeutik zuweisen. Im System der Wissenschaft hat sie keine Stelle; in diesem wird kein Schluß auf sie gegründet. Jemand , der nur für sich forschte, brauchte sie nicht. Der Zweck der Erläuterungen ist ein praktischer, und wenn dieser erreicht ist, muß man mit ihnen zufrieden sein. Dabei muß auf etwas guten Willen, auf entgegenkommendes Verständnis, auf Erraten gerechnet werden können; denn ohne Bildlichkeit des Ausdrucks wird oft nicht auszukommen sein. Aber von dem Urheber einer Erläuterung kann man immerhin verlangen, daß er selbst bestimmt wisse, was er meine, daß er mit sich selbst im Einklange bleibe und daß er, wenn sich die Möglichkeit eines Mißverstehens auch bei gutem Willen ergibt, bereit sei, seine Erläuterung zu vervollständigen und zu verbessern." (Frege, *Kleine Schriften*, p. 288).

cx. "Die neue Mathematik – oder sagen wir einfach: Herr Hilbert – versteht unter einem Axiome nicht einen eigentlichen Satz, der einen Gedanken ausdrückt, sondern einen uneigentlichen, aus dem beim Schließen vom Allge|meinen zum Besonderen verschiedene eigentliche Sätze hervorgehen können, die dann also Gedanken ausdrücken. [...] Die Hilbertschen Axiome sind dann Teile eines allgemeinen Lehrsatzes, der sinnvoll ist, obwohl jene Teile es nicht sind. Und nur als Teile eines sinnvollen Ganzen sind jene Axiome berechtigt. Sie erscheinen als Bedingungssätze [...]" (Frege, *Kleine Schriften*, pp. 299 sq.). Frege presents the distinction between "eigentlichen" and "uneigentlichen Sätzen" a few pages earlier (see: Frege, *Kleine Schriften*, p. 296); and then, based on it, presents a general definition of "(formal) theory", which serves as the basis for his reconstruction of Hilbert's work: "Ein System von allgemeinen Lehrsätzen, die in ihren uneigentlichen Bedingungssätzen übereinstimmen, kann man eine Theorie nennen." (Frege, *Kleine Schriften*, p. 298).

cxi. "Wenn somit in der axiomatischen Geometrie die der anschaulichen Geometrie entsprechenden Bezeichnungen wie 'liegen auf', 'zwischen' für die Grundbeziehungen gebraucht werden, so geschieht das nur als eine Konzession an das Gewohnte und um die Anknüpfung der Theorie an die anschaulichen Tatsachen zu erleichtern. In Wahrheit aber haben für die formale Axiomatik die Grundbeziehungen die Rolle von *variablen* Prädikaten." (David Hilbert and Paul Bernays. *Grundlagen der Mathematik*. Vol. 1. Springer, 1934, p. 7).

cxii. "Ein Inbegriff b von Sätzen ist ableitbar von einem Inbegriffe a von Sätzen in bezug auf die Vorstellungen x, y, z..., wenn jedem Inbegriff von Vorstellungen, der für x, y, z... gesetzt die Sätze von a wahr macht, auch (nach dieser Einsetzung) die Sätze von b wahr macht." (Alwin Korselt. "Über die Logik der Geometrie." In: *Jahresberichte der Deutschen Mathematiker-Vereinigung* 17 [1908], pp. 98–124, p. 108).

cxiii. "Es ist dies [Abfolge] wohl dasselbe, was Herr Frege auf Seite 426 des fünfzehnten Bandes dieser Zeitschrift 'das Folgen von Gedanken' nennt." (Korselt, "Über die Logik der Geometrie," p. 110).

cxiv. This is how Korselt introduces the term: "Von der Ableitbarkeit der Sätze unterscheidet Bolzano das Verhältnis der Abfolge, das nur zwischen Wahrheiten besteht, und vermögen dessen sich einige derselben zu andern wie *Gründe* zu *Folgen* verhalten." (Korselt, "Über die Logik der Geometrie," p. 109). Korselt then goes on to say: "Doch bleibt es mir nach diesen Entwicklungen [this passage follows immediately Korselt's introduction of the Abfolge-relation and his comparison of it with Frege's 'Folge von Gedanken'] noch zweifelhaft, ob es Wahrheiten gibt, die in diesem Sinne auseinander *folgen*, ohne in dem oben erklärten Sinne (wenigstens durch Zwischensätze, also mittelbar) auseinander *ableitbar* zu sein." (Korselt, "Über die Logik der Geometrie," p. 110).

cxv. "Die Fragen Herrn Freges, was ein logischer Schluss sei, und was der Logik eigentümlich ist, hat Bolzano in seiner Wissenschaftslehre (§ 14, 15, 155, 164) beantwortet. Ein Schluß ist ein Satz, welcher sagt, daß aus einem gewissen Inbegriffe von Sätzen ein anderer Inbegriff von Sätzen ableitbar ist (in dem in § 1 erklärten Sinne)." (Korselt, "Über die Logik der Geometrie," p. 124).

cxvi. "Die Hilbertschen Behauptungen über Unabhängigkeit usw. seiner 'Axiome' sind also sinnvoll und richtig, nur bedürfen sie einiger (durch die vorherigen Entwicklungen hoffentlich gelieferter) Aufklärung. Anstatt von 'ungültigen Axiomen' wird man z.B. von einem Inbegriffe von Sätzen reden, aus dem sich ein gewisser anderer Inbegriff von Sätzen hinsichtlich gegebener veränderlicher Vorstellungen *nicht* ableiten läßt." (Korselt, "Über die Logik der Geometrie," p. 118).

cxvii. "Grundsätze, die für sich selbst sehr einleuchten, pflegt man Axiome, oder wenn sie praktisch sind, Postulate zu nennen. 'Ungültige, falsche Grundsätze oder Axiome' is daher eine widerspruchsvolle (imaginäre) Vorstellung, was auch Herr Frege findet."(Korselt, "Über die Logik der Geometrie," p. 113).

cxviii. "Wie kommt Hilbert dazu, seine Definitionen 'Axiome' zu nennen? Wahrscheinlich hat ihm im Sinn gelegen, daß **axiooma** Forderung heißt, und daß er seine Vorstellung oder ein Verhältniß von Vorstellungen durch *Forderungen* (Bedingungen, Beschaffenheiten) in bezug auf die ihr unterstehenden Gegenstände bestimmte. [...] Man wird [den Hilbertschen Begriff des Axioms] aber fallen lassen, da er, wie Herr Frege zeigt, zu Widersprüchen führt. Es scheint sogar, als ob die Mathematik keine eigenen Axiome im euklidischen Sinne habe und diese nur in der Logik oder in den Erfahrungswissenschaften ihre Stelle hätten. Was bisher in der Geometrie 'Axiom' genannt worden ist, läßt sich immer als Bestandteil einer, wenn auch umfangreichen Definition ansehen. Dies mag Herr Hilbert gefühlt haben, als er schrieb: die Axiome dieser Gruppe definieren den Begriff 'zwischen'." (Korselt, "Über die Logik der Geometrie," pp. 118 sq.).

cxix. Korselt's presentation of his reformulation of Hilbert's axioms is exemplary (and modelled, presumably, on Frege's symbolic reconstruction of a small part of Hilbert's axioms in his 1906 article (see: Frege, *Kleine Schriften*, pp. 304 sqq.)): Before stating the axioms, Korselt explicitly introduces his entire vocabulary: "Wir betrachten gewisse Vorstellungen, bezeichnet mit Λ (Zusammenliegen), Z (Zwischenliegen), $K\Sigma$ (Kongruenz von Strecken), KW (Kongruenz von Winkeln), die, wie auch sonst beschaffen, jedenfalls die unter I bis V angegebenen Verhältnisse haben. Dadurch sind die folgenden Vorstellungen bestimmt: Π_Λ (Punkt), Γ_Λ (Gerade), E_Λ (Ebene), $\Sigma_\Lambda ab$ (Strecke ab), Σ_Λ (Strecke), [...]" (Korselt, "Über die Logik der Geometrie," p. 114).

The sense in which Korselt's four basic relations determine the properties of being a point, a line and a plane is, I take it, that one can (in principle) extrapolate the extension of these properties from the relations given in extension by investigating which objects occur in which positions of those relations how often.

cxx. "Mit den obigen Erklärungen I bis V ist nicht gemeint, daß nur eine Vorstellung Λ, nur eine Vorstellung Π_Λ usw. die angegebenen Bedingungen erfüllen dürfte, sondern es soll nur ein Verhältnis, eine Beziehung von (vier) Vorstellungen definiert werden, also ein Inbegriff von *geordneten* Inbegriffen von Gegenständen. Der Definition genügen *irgendwelche* vier Vorstellungen, falls sie nur, in bestimmter Reihenfolge genommen, die für Λ, Z, $K\Sigma$ und KW angegebenen Eigenschaften und Verhältnisse haben. So kann es z.B. einen unter die Definition fallenden Inbegriff geben, in dem die an der Stelle von Λ stehende Vorstellung eine Beziehung von Zahlen ist." (Korselt, "Über die Logik der Geometrie," p. 118).

cxxi. "So wie der Ausdruck 'jedes A hat (die Beziehung) b zu zwei E' liegt, ist er eine Satzform, ein Ausdruck von unendlich vielen Sätzen, die man erhält, wenn man für A, b, E passende Vorstellungen einsetzt. Meine frühere Ansicht, daß jener Satz eine dieser Vorstellungen definiere, gebe ich auf, meine aber nun, daß er Bestandteil der Definition eines *Verhältnisses* dreier Vorstellungen werden kann, z.B. wenn man sagt: es sei (A, b, E) ein geordneter Inbegriff einer Vorstellung A, einer Beziehung b und einer Vorstellung E derart, daß jedes A usw." (Korselt, "Über die Logik der Geometrie," p. 120).

cxxii. "[Jene Sätze der Axiom-Gruppen I bis V] definieren vielmehr nur das *Verhältnis* gewisser Vorstellungen; die Begriffe *Punkt*, *Gerade*, *Ebene* usw. sind nur Beziehungsbegriffe." (Korselt, "Über die Logik der Geometrie," p. 122).

cxxiii. He writes, in a 1975 article: "Formale Theorien und damit die Untersuchung von Strukturtypen dienen dazu, gleichartige Schlüsse in verschiedenen materialen Theorien ein für allemal vorwegnehmend zu behandeln. Formale Theorien sind damit auch nur dann gerechtfertigt, wenn Anwendungen in relevanten (materialen) Modellen existieren. [...] Über die Anwendung formaler Theorien treten also die von Hilbert aus der Mathematik, ja aus der Wissenschaft überhaupt, verbannten Probleme *materialer* Sprachkonstruktionen und Behauptungen (Aussagen) schließlich wieder auf und bedürfen, wenn formale Theorien nicht spielerisch bleiben sollen, jeweils einer Lösung." (Friedrich Kambartel. "Frege und die axiomatische Methode. Zur Kritik mathematikhistorischer Legitimationsversuche der formalistischen Ideologie." In: *Studien zu Frege. Band I: Logik und Philosophie der Mathematik*. Ed. by Matthias Schirn. Problemata 42. Frommann-Holzboog, 1976, pp. 215–228, p. 224). The term "material" is explained previously as follows: "Im Gegensatz dazu besteht dann eine *materiale* Theoriekonstruktion im Fortschreiten von begründeten (wahren) Behauptungssätzen (Aussagen) zu weiteren begründeten Behauptungssätzen (Aussagen). Formale Theorien, deren zugrundeliegendes (formales) Axiomensystem ein Modell besitzt, mögen *formal interpretierbar* heißen." (Kambartel, "Frege und die axiomatische Methode," p. 221); this passage suggests that what is material about material theories is that they deal with particular objects, about which they make particular (determinately true) claims.

cxxiv. "So fängt denn alle menschliche Erkenntnis mit Anschauungen an, geht von da zu Begriffen, und endigt mit Ideen. Ob sie zwar in Anschauung aller dreien Elemente Erkenntnisquellen a priori hat, die beim ersten Anblicke die Grenzen aller Erfahrung zu verschmähen scheinen, so überzeugt doch eine vollende Kritik, daß alle Vernunft im spekulativen Gebrauche mit diesen Elementen niemals über das Feld möglicher Erfahrung hinaus kommen könne, und daß die eigentliche Bestimmung dieses obersten Erkenntnisvermögens sei, sich aller Methoden und der Grundsätze derselben nur zu bedienen, um der Natur nach allen möglichen Prinzipien der Einheit, worunter die der Zwecke die vornehmste ist, bis in ihr Innerstes nachzugehen, niemals aber ihre Grenze zu überfliegen, außerhalb welcher *für uns* nichts als leerer Raum ist." (Kant, *Kritik der reinen Vernunft*, A702/B730).

cxxv. The passage quoted in cxxiv continues: "Zwar hat uns die kritische Untersuchung aller Sätze, welche unsere Erkennt[A703/B731]nis über die wirkliche Erfahrung hinaus erweitern können, in der transzendentalen Analytik hinreichen überzeugt, daß sie niemals zu etwas mehr, als einer möglichen Erfahrung leiten könne, und, wenn man nicht selbst gegen die klärsten abstrakten und allgemeinen Lehrsätze mißtrauisch wäre, wenn nicht reizende und scheinbare Aussichten uns locketen, den Zwang der ersteren abzuwerfen, so hätten wir allerdings der mühsamen Abhörung aller dialektischen Zeugen, die eine transzendente Vernunft zum Behuf ihrer Anmaßungen auftreten läßt, überhoben sein können; denn wir wußten es schon zum voraus mit völliger Gewißheit, daß alles Vorgeben derselben zwar vielleicht ehrlich gemeint, aber schlechterdings nichtig sein müsse, weil es eine Kundschaft betraf, die kein Mensch jemals bekommen kann." (Kant, *Kritik der reinen Vernunft*, A702 sq./B730 sq.).

cxxvi. "Anschauung und Begriffe machen also die Elemente aller unserer Erkenntnis aus, so daß weder Begriffe, ohne ihnen auf einige Art korrespondierende Anschauung, noch Anschauung ohne Begriffe, eine Erkenntnis abgeben können." (Kant, *Kritik der reinen Vernunft*, A50/B74). This point is reinforced a page later: "Unsere Natur bringt es so mit sich, daß die *Anschauung* niemals anders als *sinnlich* sein kann, d.i. nur die Art enthält, wie wir von Gegenständen affiziert werden. Dagegen ist das Vermögen, den Gegenstand sinnlicher

Anschauung zu *denken*, der *Verstand*. Keine dieser Eigenschaften ist der andern vorzuziehen. Ohne Sinnlichkeit würde uns kein Gegenstand gegeben, und ohne Verstand keiner gedacht werden. Gedanken ohne Inhalt sind leer, Anschauungen ohne Begriffe sind blind. Daher ist es eben so notwendig, seine Begriffe sinnlich zu machen, (d.i. ihnen den Gegenstand in der Anschauung beizufügen,) als seine Anschauungen sich verständlich zu machen (d.i. sie unter Begriffe zu bringen)." (Kant, *Kritik der reinen Vernunft*, A51/B75).

cxxvii. "Auf solche Weise ist die Idee eigentlich nur ein heuristischer und nicht ostensiver Begriff, und zeigt an, nicht wie ein Gegenstand beschaffen ist, sondern wie wir, unter der Leitung desselben, die Beschaffenheit und Verknüpfung der Gegenstände der Erfahrung überhaupt *suchen* sollen. Wenn man nun zeigen kann, daß, obgleich die dreierlei transzendentalen Ideen (...) direkt auf keinen ihnen korrespondierenden Gegenstand und dessen *Bestimmung* bezogen werden, dennoch alle Regeln des empirischen Gebrauchs der Vernunft unter Voraussetzung eines solchen *Gegenstandes in der Idee* auf systematische Einheit führen und die Erfahrungserkenntnis jederzeit erweitern, niemals aber derselben zuwider sein können: so ist eine notwendige *Maxime* der Vernunft, nach dergleichen Ideen zu verfahren. Und diese ist die transzendentale Deduktion aller Ideen der spekulativen Vernunft, nicht als *konstitutiver* Prinzipien der Erweiterung unserer Erkenntnis über mehr Gegenstände, als Erfahrung geben kann, sondern als *regulativer* Prinzipien der systematischen Einheit des Mannigfaltigen der empirischen Erkenntnis überhaupt, welche dadurch in ihren eigenen Grenzen mehr angebauet und berichtigt wird, als es ohne solche Ideen durch den bloßen Gebrauch der Verstandesgrundsätze geschehen könnte." (Kant, *Kritik der reinen Vernunft*, A671/B699).

cxxviii. "So bezieht sich demnach die Vernunft nur auf den Verstandesgebrauch, und zwar nicht so fern dieser den Grund möglicher Erfahrung enthält, (denn die absolute Totalität der Bedingungen ist kein in einer Erfahrung brauchbarer Begriff, weil keine Erfahrung unbedingt ist,) sondern um ihm die Richtung auf eine gewisse Einheit vorzuschreiben, von der der Verstand keinen Begriff hat, und die darauf hinaus geht, alle Verstandeshandlungen, in [A327] Ansehung eines jeden Gegenstandes, in ein *absolutes* Ganzes zusammen zu fassen. Daher ist der objective Gebrauch der reinen Vernunftbegriffe jederzeit *transzendent*, indessen daß der von den reinen Verstandesbegriffen, seiner Natur nach, jederzeit *immanent* sein muß, indem er sich bloß auf möglicher Erfahrung einschränkt. Ich verstehe unter der Idee eine notwendigen Vernunftbegriff, dem kein kongruierender Gegenstand in den Sinnen gegeben werden kann. Also sind unsere jetzt erwogene reine Vernunftbegriffe *transzendentale Ideen*." (Kant, *Kritik der reinen Vernunft*, A326 sq./B384).

cxxix. "Die Vernunft bezieht sich niemals geradezu auf einen Gegenstand [...], *schafft* also keine Begriffe (von Objekten), sondern *ordnet* sie nur, und gibt ihnen diejenige Einheit, welche sie in ihrer größtmöglichen Ausbreitung haben können [...] [A644/B672] Ich behaupte demnach: die transzendentalen Ideen sind niemals von konstitutivem Gebrauche, so, daß dadurch Begriffe gewisser Gegenstände gegeben würden, und in dem Falle, daß man sie so versteht, sind es bloß vernünftelnde (dialektische) Begriffe." (Kant, *Kritik der reinen Vernunft*, A643/B671).

cxxx. "Übersehen wir unsere Verstandeserkenntnisse in ihrem ganzen Umfange, so finden wir, daß dasjenige, was Vernunft ganz eigentümlich darüber verfügt und zu Stande zu bringen sucht, das *Systematische* der Erkenntnis sei, d.i. der Zusammenhang derselben aus einem Prinzip. Diese Vernunfteinheit setzt jederzeit eine Idee voraus, nämlich die von der Form eines Ganzen der Erkenntnis, welche vor der bestimmten Erkenntnis der Teile vorhergeht und die Bedingungen enthält, jedem Teile seine Stelle und Verhältnis zu den übrigen a priori zu bestimmen. Diese Idee postuliert demnach vollständige Einheit der Verstandeserkenntnis, wodurch diese nicht bloß ein zufälliges Aggregat, sondern ein nach notwendigen Gesetzen zusammenhangendes [sic] System wird. Man kann eigentlich nicht sagen, daß diese Idee ein Begriff vom Objekte sei, sondern von der durchgängigen Einheit dieser Begriffe, so fern dieselbe dem Verstande zur Regel dient. Dergleichen Vernunftbegriffe werden nicht aus der Natur geschöpft, vielmehr befragen wir die Natur nach diesen Ideen, und halten unsere Erkenntnis für mangelhaft, so lange sie [A646/B674] denselben nicht adäquat ist." (Kant, *Kritik der reinen Vernunft*, A645 sq./B673 sq.).

cxxxi. "So enthält die reine Vernunft, die uns Anfangs nichts Geringeres, als Erweiterung der Kenntnisse über alle Grenzen der Erfahrung, zu

versprechen schiene, wenn wir sie recht verstehen, nichts als regulative Prinzipien, die zwar größere Einheit gebieten, als der empirische Verstandesgebrauch erreichen kann, aber eben dadurch, daß sie das Ziel der Annäherung desselben so weit hinaus rücken, [A702/B730] die Zusammenstimmung desselben mit sich selbst durch systematische Einheiten zum höchsten Grade bringen, wenn man sie aber mißversteht, und sie für konstitutive Priznipien transzendenter Erkenntnisse hält, durch einen zwar glänzenden, aber trüglichen Schein, Überredung und eingebildetes Wissen, hiemit aber ewige Widersprüche und Streitigkeiten hervorbringen." (Kant, *Kritik der reinen Vernunft*, A701 sq./B729 sq.). My translation presupposes a particular, somewhat unobvious construal of the passage (which, however, makes better sense, to my mind, than any of the more obvious alternatives I see), namely, I take "desselben" in the phrase "der Annäherung desselben" to refer back to "das Ziel", while I take "desselben" in the following clause (in the phrase "die Zusammenstimmung desselben mit sich selbst") to refer back all the way to "der empirische Verstandesgebrauch".

cxxxii. "Die vorliegende Untersuchung ist ein neuer Versuch, für die Geometrie ein *einfaches* und *vollständiges* System von einander *unabhängiger* Axiome aufzustellen und aus denselben die wichtigsten geometrischen Sätze in der Weise abzuleiten, dass dabei die Bedeutung der verschiedenen Axiomgruppen und die Tragweite der aus den einzelnen Axiomen zu ziehenden Folgerungen möglichst klar zu Tage tritt." (David Hilbert. *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*. Ed. by Michael Hallett and Ulrich Majer. Vol. 1. David Hilbert's Lectures on the Foundations of Mathematics and Physics, 1891-1933. Springer, 2003, p. 436).

cxxxiii. § 9 opens with the following statement: "Die Axiome der fünf in Kapitel 1 aufgestellten Axiomgruppen stehen mit einander nicht in Widerspruch, d.h. es ist nicht möglich, durch logische Schlüsse aus denselben eine Thatsache abzuleiten, welche einem der aufgestellten Axiome widerspricht. Um dies einzusehen, genügt es eine Geometrie anzugeben, in der sämtliche Axiome der fünf Gruppen erfüllt sind." (Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*, p. 454).

cxxxiv. He states at the end of his brief exposition: "Die entsprechende Betrachtungsweise für die räumlich Geometrie bietet keine Schwierigkeiten." (Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*, p. 455), which suggests that he take himself to have just illustrated an approach (a "Betrachtungsweise"), rather than provided an entirely satisfactory proof.

cxxxv. "Da durch den kosmologischen Grundsatz der Totalität kein Maximum der Reihe von Bedingungen in einer Sinnenwelt, als einem Dinge an sich selbst, *gegeben* wird, sondern bloß im Regressus derselben *aufgegeben* werden kann, so behält der gedachte Grundsatz der reinen Vernunft, in seiner dergestalt berichtigten Bedeutung dennoch seine gute Gültigkeit, zwar nicht als *Axiom*, die Totalität im Objekt als wirklich zu denken, sondern als ein *Problem* für den Verstand, also für das Subjekt, um, der Vollständigkeit in der Idee gemäß, den Regressus in der Reihe der Bedingungen zu einem gegebenen Bedingten anzustellen und fortzusetzen. [...] Der Grundsatz der Vernunft also ist eigentlich nur eine *Regel*, welche in der Reihe der Bedingungen gegebe[A509/B537]ner Erscheinungen einen Regressus gebietet, dem es niemals erlaubt ist, bei einem Schlechthinunbedingten stehen zu bleiben. Er ist also kein Principium der Möglichkeit der Erfahrung und der empirischen Erkenntnis der Gegenstände der Sinne, mithin kein Grundsatz des Verstandes; denn jede Erfahrung ist in ihren Grenzen (der gegebenen Anschauung gemäß) eingeschlossen, auch kein *konstitutives Prinzip* der Vernunft, den Begriff der Sinnenwelt über alle mögliche Erfahrung zu erweitern, sondern ein Grundsatz der größtmöglichen Fortsetzung und Erweiterung der Erfahrung, nach welchem keine empirische Grenze für absolute Grenze gelten muß, also ein Principium der Vernunft, welches, *als Regel*, postuliert, was von uns im Regressus geschehen soll, und *nicht antizipiert*, was *im Objekte* vor allem Regressus an sich gegeben ist. Daher nenne ich es ein *regulatives* Prinzip der Vernunft [...]." (Kant, *Kritik der reinen Vernunft*, A508 sq./B536 sq.).

cxxxvi. "Es zeigt sich aber, wenn man auf den transzendentalen Gebrauch des Verstandes Acht hat, daß diese Idee einer Grundkraft überhaupt, nicht bloß als Problem zum hypothetischen Gebrauche bestimmt sei, sondern objektive Realität vorgebe, dadurch die systematische Einheit der mancherlei Kräfte einer Substanz postulieret und ein apodiktisches Vernunftprinzip erreicht wird. Denn, ohne daß wir einemal die Einhelligkeit der mancherlei Kräfte versucht haben, ja selbst wenn es uns nach allen Versuchen mißlingt, sie zu entdecken, setzen wir doch voraus: es werde eine solche anzutreffen sein, und dieses nicht allein, wie in dem angeführten Falle, wegen der Einheit

der Substanz, sondern, wo so gar viele, obzwar in gewissem Grade gleichartige, angetroffen werden, wie an der Materie überhaupt, setzt die Vernunft systematische Einheit mannigfaltiger Kräfte voraus, da besonders Naturgesetze unter allgemeineren stehen, und die Ersparung der Prinzipien nicht bloß ein ökonomischer Grundsatz der Vernunft, sondern inneres Gesetz der Natur wird." (Kant, *Kritik der reinen Vernunft*, A650/B678).

cxxxvii. "*Litteratur*. (Wir nennen dabei nur einige Hauptwerke, welche zusammenfassende Darstellungen unseres Stoffes darbieten): [...] 7.) *Hölder*: Anschauung und Denken in der Geometrie, 1899 (darin kann man sich am besten über die philosophische Seite der Frage unterrichten)." (Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*, pp. 541 sq.).

cxxxviii. "Die Grundlagen der geometrischen Construction sind Begriffe, die nicht bloß von den unwesentlichen Merkmalen der einzelnen Raumanschauungen abstrahiren, sondern sogar die allen bezüglichen Raumanschauungen gemeinsamen Merkmale in einer Weise verändern, die jede besondere Anschauung z.B. einer Linie oder eines Dreiecks zu einer nicht einmal adaequaten Darstellung des Begriffs macht. Die Definition der Geometrie entwickeln daher, streng genommen, nicht Anschauungen, nicht abstracte Begriffe, sondern Ideen." (Benno Erdmann. *Die Axiome der Geometrie. Eine philosophische Untersuchung der Riemann-Helmholtz'schen Raumtheorie*. Verlag von Leopold Voss, 1877, p. 37).

Later in the book Erdmann expands on this discussion (explicitly referring to it the page before the following passage): "Die Maßbeziehungen der [geometrischen] Constructionsbegriffe sind deshalb weder tatsächliche Eigenschaften der Körper, noch aus ihnen schlechtweg abstrahirte Begriffe, sondern empirische Ideen; sie verändern die beobachtbaren Eigenschaften der elementaren Körperformen so, dass sie ideale Musterbilder werden, denen alle Wirklichkeit nur beliebig nahe gebracht werden kann, die sie aber niemals zu erreichen vermag." (Erdmann, *Die Axiome der Geometrie*, p. 158).

cxxxix. "Denn die oben gefordete Definition unserer Raumvorstellung kann nur gebildet werden, wenn wir im Stande sind, den Gattungsbegriff zu finden, dem wir dieselbe subsumiren müssen, und die spezifischen Merkmale zu bestimmen, die sie von den anderen möglichen oder etwa wirklichen coordinirten Arten dieses Gattungsbegriffs unterscheidet. Dieser Forderung widerstrebt aber, so scheint es, die psychologische Erkenntnis, dass unsere Raumvorstellung, sofern sie als unmittelbar gegeben anzusehen ist, als eine *einzige Raumanschauung* näher bestimmt werden muss. Die Wahrheit dieser Klassificirung lässt sich nach Kants epochemachenden Untersuchungen, die wenigstens in diesem einen Punkt allen Angriffen siegreich widerstanden haben, nicht | mehr bezweifeln. Jedoch ebenso sicher ist, dass sie nicht die volle Wahrheit enthält. Gerade mathematische Betrachtungen lehren, dass der Raum unbeschadet dieses seines ursprünglichen Charakters auch als ein Begriff aufgefasst werden kann, der sich als ein wolbestimmtes Glied in eine große Reihe entsprechender Begriffe einordnen lässt." (Erdmann, *Die Axiome der Geometrie*, pp. 36 sq.).

cxl. "Für Kant hat die Erfahrung in objectivem Sinne, als die Summe der Einwirkungen der Dinge, nur eine occasionalistische Bedeutung: sie ist die Gelegenheitsursache, welche die Auslösung der von ihr absolut unabhängigen apriorischen Formen bewirkt. Für die Physiologie und Psychologie unserer Zeit dagegen, der sowohl die erkenntnistheoretische Untersuchung des Begriffs der Wechselwirkung, als auch die biologische Theorie der psychischen Entwicklung zur Seite tritt, hat sie die Bedeutung eines Factors, der das Erkenntnisprodukt nach Form und Inhalt gleich wesentlich | bestimmt. Jeder Versuch, Kants Lehre von der Apriorität als des subjectiven, von aller Erfahrung absolut unabhängigen Erkenntnisfactors, trotzdem zu halten, ist deshalb von vornherein aussichtslos. Wir bedürfen einer Weiterbildung des kritischen Idealismus, die, wenn sie den neu gefundenen Tatsachen zugiebt, was ihnen gebührt, eben zur empiristischen Theorie überführt." (Erdmann, *Die Axiome der Geometrie*, pp. 146 sq.).

cxli. "Es sollte doch vielmehr auch dem überzeugtesten Schüler Kants eine selbstverständliche Pflicht sein, die Lehren seines Meisters, die er auch den gegenwärtigen Problemvorstellungen gegenüber für ausreichend erachtet, auf die veränderten Fragen nur nach eingehender kritischer Prüfung zu übertragen; denn nur dadurch lässt sich die Ueberzeugung sicher gewinnen, dass ein philosophisches System, von dem wir durch eine reiche geistige Entwicklung von hundert Jahren getrennt sind, auch in seinen besonderen Theoremen noch den

Forderungen der Wissenschaft unserer Zeit völlig gewachsen ist." (Erdmann, *Die Axiome der Geometrie*, p. 109).

cxlii. "Wir haben allen Grund zu vermuten, daß es uns im pseudosphärischen Raume bald genug ebenso gehen würde, wie es bei einem angehenden Brillenträger nach wenigen Stunden schon der Fall ist. Kurz der pseudosphärische Raum würde uns verhältnismäßig gar nicht sehr fremdartig erscheinen; wir würden uns nur in der ersten Zeit, bei der Abmessung der Größe und Entfernung fernerer Gegenstände nach ihrem Gesichtseindruck, Täuschungen unterworfen finden. Die entgegengesetzten Täuschungen würde ein sphärischer Raum von drei Dimensionen mit sich bringen, wenn wir mit dem im Euklidschen Raume erworbenen Augenmaße in ihn eintreten." (Hermann von Helmholtz. "Über den Ursprung und die Bedeutung der geometrischen Axiome." In: *Schriften zur Erkenntnistheorie*. Ed. by Moritz Schlick and Paul Hertz. Springer Verlag, 1921, pp. 1–24, p. 21).

cxliii. "Wenn wir es zu irgendeinem Zwecke nützlich fänden, so könnten wir in vollkommen folgerichtiger Weise den Raum, in welchem wir leben, als den scheinbaren Raum hinter einem Konvexspiegel mit verkürztem und zusammengezogenem Hintergrunde betrachten; oder wir könnten eine abgegrenzte Kugel unseres Raumes, jenseits deren Grenzen wir nichts mehr wahrnehmen, als den unendlichen pseudosphärischen Raum betrachten. Wir müßten dann nur den Körpern, welche uns als fest erscheinen, und ebenso unserem eigenen Leibe gleichzeitig die entsprechenden Dehnungen und Verkürzungen zuschreiben, und würden allerdings das System unserer mechanischen Prinzipien gleichzeitig gänzlich verändern müssen;" (Helmholtz, "Über den Ursprung und die Bedeutung der geometrischen Axiome," p. 23).

cxliv. The passage continues: "Diese von Helmholtz mit glücklicher Präcision auch in dem Reich der Naturwissenschaften eingebürgerte Ueberzeugung erfolgt jedoch aus diesen Betrachtungen um nichts sicherer, als eine zweite, deren allgemeiner Inhalt nach Kants Untersuchungen unveräußerlichen Eigentum der Psychologie hätte bleiben sollen. Auch die Formen nämlich, in denen sich jenes Empfindungsmaterial ordnet, die räumlichen nicht mehr und nicht anders als die verstandesmäßigen, können nur in Zeichensystem für die Verhältnisse und Beziehungen der Dinge sein. Von den Consequenzen dieser Auffassung hinsichtlich des Begriffs der Apriorität, sowie des Verhältnisses der Anschauungsformen zu den Kategorien sehen wir an dieser Stelle ab, um dieselbe für die Raumvorstellung, deren Betrachtung uns allein obliegt, noch etwas eingehender zu begründen." (Erdmann, *Die Axiome der Geometrie*, p. 94).

cxlv. "Bekanntlich setzt die Geometrie sowohl den Begriff des Raumes, als die ersten Grundbegriffe für die Constructionen im Raume als etwas Gegebenes voraus." (Bernhard Riemann. *Gesammelte Mathematische Werke, Wissenschaftlicher Nachlass und Nachträge*. Ed. by Henrich Weber and Richard Dedekind. Ed. by Raghavan Narashimhan. 1990, p. 304).

cxlvi. "Die Erfahrung kann nicht bloß der äußere, occasionalistische Anlass zur Entstehung des dreifach ausgedehnten Raumes sein, wenn zweifellos ist, dass die besondere Beschaffenheit der Dinge außer uns dazu *nötigt*, gerade drei Dimensionen, nicht mehr und nicht weniger, | zu entwickeln. Das erste Axiom der Geometrie bezieht sich deshalb auf keine notwendige, sondern eine tatsächliche Eigenschaft unserer Raumvorstellung; es ist keine rationale, sondern eine empirische Wahrheit." (Erdmann, *Die Axiome der Geometrie*, p. 145).

cxlvii. "Es kommt hier auf ein Merkmal an, woran wir sicher ein reines Erkenntnis [sic. Kant regularly uses 'Erkenntnis' apparently as if it were grammatically neuter] von empirischen unterschieden können. Erfahrung lehrt uns zwar, daß etwas so oder so beschaffen sei, aber nicht, daß es nicht anders sein könne. Findet sich also *Erstlich* ein Satz, der zugleich mit seiner *Notwendigkeit* gedacht wird, so ist er ein Urteil a priori; [...] Zweitens: Erfahrung gibt niemals ihren Urteilen wahre oder strenge, sondern nur angenommene und komparative *Allgemeinheit* (durch Induktion) [...]. [B4] Wird also ein Urteil in strenger Allgemeinheit gedacht, d.i. so, daß gar keine Ausnahme als möglich verstattet wird, so ist es nicht von der Erfahrung abgeleitet, sondern schlechterdings a priori gültig." (Kant, *Kritik der reinen Vernunft*, B3 sq.).

cxlviii. "Daraus aber geht hervor, dass die Geometrie keine rein apriorische Wissenschaft, sondern die Wissenschaft von den empirischen Maßbestimmungen unseres Raumes ist. Diese Raumvorstellung selbst endlich ist ebenfalls keine apriorische Vorstellung in rationalistischem

Sinne. Denn würde sie in der Tat unabhängig von aller Erfahrung durch die spontane Kraft der Seele erzeugt, wäre sie auch nur die allgemeine Anschauungsform der Receptivität gegen die äußeren Dinge im Sinne Kants, es müsste unmöglich sein, dass wir uns die Vorstellungen anderer dreifach ausgedehnter Mannigfaltigkeiten mit abweichenden Maßbestimmungen anschaulich bilden könnten." (Erdmann, *Die Axiome der Geometrie*, p. 91).

cxlix. "Nunmehr sind wir in den Stand gesetzt, die Frage, welche den Ausgangspunkt unserer Untersuchungen bildete, vollständig zu beantworten. Den Axiomen der Geometrie kommen die Prädicate der Notwendigkeit, Allgemeinheit und Unveränderlichkeit nicht in absolutem, rationalistischem Sinne zu. Sie sind nicht das einseitige Erzeugnis einer geistigen Tätigkeit, die der Erfahrung bei den ersten zufälligen Anlässen mit einer geschlossenen Reihe fertiger Formen gegenübertritt, nicht ewige Wahrheiten, die einer Erfahrung, von der sie gänzlich unabhängig sind, die sie vielmehr im eigentlichsten Sinne selbst machen, unabänderliche Gesetze vorschreiben. Sie bilden das Product einer Wechselwirkung von zwei gleich wesentlich bestimmenden Ursachen, deren eine in den Tiefen unseres Geistes, deren zweite in der Natur der Dinge ruht, sie geben Urteile über Tatsachen, welche die bisher unseren Sinnen zugänglich gewordene Erfahrung (in objectivem Sinn) zu ihrem geistigen Ausdruck bringen." (Erdmann, *Die Axiome der Geometrie*, p. 153).

cl. This passage follows immediately after the discussion quoted in endnote clxiii: "[A]lle unsere anschaulichen Vorstellungen correspondiren den Dingen nicht weniger vollkommen, wie sie von ihnen in allen ihren Elementen absolut verschieden sind. Jeder Teil dieser Vorstellungen daher, ihre Materie wie ihre Form, ist *a priori*, sofern die psychischen Tätigkeiten, die sie erzeugen, in Betracht kommen; jeder dieser Teile aber ist zugleich empirisch oder *a posteriori*, sofern auf die Bedingungen gesehen wird, welche seine Auslösung veranlassen. [...] Der Gegensatz zwischen dem Apriorischen und dem Empirischen in unseren Vorstellungen ist deshalb kein Gegensatz verschiedener Vorstellungsgruppen oder ihres Bildungsprocesses, sondern es ist ein Gegensatz der Betrachtungsweise, der die beiden überall notwendig zu supponirenden Seiten jenes Processes zur Darlegung bringt." (Erdmann, *Die Axiome der Geometrie*, p. 96).

cli. "Auch aus der Natur des geometrischen Beweises lässt sich dies folgern. Derselbe besteht in nichts anderem als in der Reduction der verwickelteren Maßbeziehungen, die in der Behauptung angegeben sind, auf einfachere, bereits bekannte Maßbeziehungen; diese aber weisen zuletzt auf die einfachsten Constructionsbegriffe oder Ideen zurück. Auch der geometrische Beweis benutzt somit lediglich Raumbegriffe; wird doch die Entwicklung der Geometrie lediglich dadurch be|dingt, dass jene constructiven Grundlagen auf immer complicirtere Maßbeziehungen übertragen werden. Die geometrische Einzelanschauungen sind deshalb nur Vorstellungsrepräsentanten der Raumbegriffe, die in ihnen enthalten sind. Wäre es zweckmäßig, die scheinbare Paradoxie dieser Doppelnatur terminologisch auszudrücken, so könnte man die geometrischen Raumformen Begriffsanschauungen nennen." (Erdmann, *Die Axiome der Geometrie*, pp. 37 sq.).

clii. "Ich habe mir daher zunächst die Aufgabe gestellt, den Begriff einer mehrfach ausgedehnten Grösse aus allgemeinen Grössenbegriffen zu construiren. Es wird daraus hervorgehen, dass eine mehrfach ausgedehnte Grösse verschiedener Massverhältnisse fähig ist und der Raum also nur einen besonderen Fall einer dreifach ausgedehnten Grösse bildet. Hiervon aber ist eine nothwendige Folge, dass die Sätze der | Geometrie sich nicht aus allgemeinen Grössenbegriffen ableiten lassen, sondern dass diejenigen Eigenschaften durch welche sich der Raum von anderen denkbaren dreifach ausgedehnten Grössen unterscheidet, nur aus der Erfahrung entnommen werden könne. Hieraus entsteht die Aufgabe, die einfachsten Thatsachen aufzusuchen, aus denen sich die Massverhältnisse des Raumes bestimmen lassen [...]. Diese Thatsachen sind wie alle Thatsachen nicht nothwendig, sondern nur von empirischer Gewissheit, sie sind Hypothesen;" (Bernhard Riemann. "Ueber die Hypothesen, welche der Geometrie zu Grunde liegen." In: ed. by Henrich Weber and Richard Dedekind. Ed. by Raghavan Narashimhan. 1990, pp. 304–319, pp. 304 sq.).

cliii. "Nun haben wir aber noch einen anderen Weg zur wissenschaftlichen Behandlung der Geometrie. Es sind nämlich alle uns bekannten Raumverhältnisse meßbar, das heißt, sie können auf Bestimmung von Größen (von Linienlängen, Winkeln, Flächen, Volumina) zurückgeführt werden. Eben deshalb können die Aufgaben der Geometrie auch dadurch gelöst werden, daß man die Rechnungsmethoden aufsucht, mittels deren man die unbekannten Raumgrößen aus den bekannten herzuleiten hat. Dies geschieht in der analytischen Geometrie, in

welcher die sämtlichen Gebilde des Raumes nur als Größen behandelt und durch andere Größen bestimmt werden." (Helmholtz, "Über den Ursprung und die Bedeutung der geometrischen Axiome," p. 11).

cliv. "Die ganze Ausführung der Rechnung ist eine rein logische Operation; sie kann keine Beziehung zwischen den der Rechnung unterworfenen Größen ergeben, die nicht schon in den Gleichungen, welche den Ansatz der Rechnung bilden, enthalten ist." (Helmholtz, "Über den Ursprung und die Bedeutung der geometrischen Axiome," p. 5).

clv. Immediately before the passage quoted in endnote cli, Erdmann writes: "Erstens nämlich ergiebt sich leicht, dass jene Raumformen, deren gegenseitige Maßbeziehungen die euklidische Geometrie durch die synthetische Methode constructiver Darstellungen entwickelt, zugleich *Grössenbegriffe* sind, deren Verhältnisse sich auch auf analytischem Wege durch Systeme von Gleichungen bestimmen lassen. Schon daraus geht diese Doppelnatur jener Formen hervor, dass alle geometrischen Beweise zugleich Größenbegriffe enthalten und verwerten. Diese zweifache Geltung jedoch ist nur dadurch möglich, dass jene besonderen Formen nicht lediglich Raumanschauungen sind, vielmehr der geometrischen Untersuchung überall als *Raumbegriffe* gelten. Dass dies wirklich der Fall ist, bedarf keiner eingehenden Diskussion." (Erdmann, *Die Axiome der Geometrie*, p. 37).

clvi. Besides the passage I quoted in the previous endnote, the following remark, found immediately after the passage quoted in endnote cxxxix, testifies to the role analytic geometry plays in Erdmann's exposition: "Die Handhabe hierzu bietet die analytische Geometrie, welche lehrt, dass auch die besonderen Raumanschauungen, die den eigentlichen Gegenstand der mathematischen Untersuchung bilden, zugleich Raumbegriffe sind." (Erdmann, *Die Axiome der Geometrie*, p. 37).

clvii. "In einem einzigen Punkte war unsere Untersuchung von den üblichen Deductionen dieser Art unterschieden, aber diesen Punkt hatte unsere Darstellung mit den Erörterungen aller entsprechenden mathematischen Begriffe gemein. Der allgemeinste Gattungsbegriff nämlich, zu dem wir gelangten, der Begriff einer n-fach bestimmten Mannigfaltigkeit überhaupt, ist kein höchster Begriff in dem Sinne der gewöhnlichen logischen Forderungen. Jene Begriffe entstehen durch Abstraction von immer neuen Merkmalen, dieser Begriff ging aus der unbestimmten Erweiterung eines besonderen Merkmals hervor. Darin liegt aber kein Widerspruch gegen die logische Theorie, sondern eine Modification derselben, die durch eine in der allgemeinen Erörterung der logischen Wissenschaft nicht einbegriffene Anwedung auf die eigenartige Natur des Zahlbegriffs erforderlich wird, dessen gleichartige Synthesis eine solche Erweiterung möglich macht." (Erdmann, *Die Axiome der Geometrie*, p. 133).

clviii. "Die ganze Ausführung der Rechnung ist eine rein logische Operation; sie kann keine Beziehung zwischen den der Rechnung unterworfenen Größen ergeben, die nicht schon in den Gleichungen, welche den Ansatz der Rechnung bilden, enthalten ist. Die erwähnten neueren Untersuchungen sind deshalb fast ausschließlich mittels der rein abstrakten Methode der analytischen Geometrie geführt worden." (Helmholtz, "Über den Ursprung und die Bedeutung der geometrischen Axiome," p. 5).

clix. "In der Tat kann sich keine Wissenschaft solcher metaphysischer Voraussetzungen in den Grundbegriffen erwehren; am wenigsten aber die Psychologie, so lange der unbezweifelbare, durchgreifenden Zusammenhang der psychischen Tätigkeiten mit den Bewegungsvorgängen nur durch jenen rätselhaften Gegensatz angedeutet werden kann, der für unser gegenwärtiges Wissen die Welt des Bewusstseins von der Welt der Bewegung scheidet." (Erdmann, *Die Axiome der Geometrie*, p. 92).

clx. Erdmann explicitly assumes psychological facts have some kind of physiological manifestation in the two fundamental assumptions Erdmann is referring to in the passage I cited in endnote clxi: "Wir setzen demnach nur voraus, dass keine, auch nicht die mechanisch vollkommenste Einsicht in die Bewegungsvorgänge der Gehirnmassen uns zur geringsten Erkenntnis der psychischen Beschaffenheit der Vorstellungsgesetze führen könnte, das wir immer nur Bewegungen aus Bewegungen, nie Bewegungen aus Vorstellungen abzuleiten vermögen. Auch der zweiten für unsere Zwecke notwendigen Voraussetzung bedürfen wir nicht in der Form, wie sie von den Physiologen gern dargestellt und von den Psychologen bereitwillig zugestanden wird. Denn so wahrscheinlich es ist, dass trotz jener absoluten

Verschiedenheit beider Gebiete jeder Vorstellungsverlauf, der abstracteste nicht ausgenommen, an ihn begleitende Bewegungsvorgänge im Centralorgan gebunden ist, so genügt uns doch hier die naturwissenschaftlich fest begründete Annahme, dass allen unseren Wahrnehmungsacten sowie den unmittelbar mit ihnen verbundenen psychischen Tätigkeiten Molecularbewegungen im Gehirn vorausgehen resp. folgen oder etwa zur Seite gehen." (Erdmann, *Die Axiome der Geometrie*, p. 93).

clxi. "Aus diesen beiden Voraussetzungen ergiebt sich, dass alle unsere Anschauungen äußerer Dinge und Verhältnisse Producte einer Wechselwirkung sind, deren Bedingungen teils in der gleichviel wie bestimmbaren Beschaffenheit der Dinge, teil in dem Wesen der psychischen Vorgänge begründet liegen. Ueber die Art und Weise dieser Wechselwirkung sind wir in völliger Unkenntnis, aus seiner Tatsächlichkeit dürfen wir jedoch folgende Gesichtspunkte herleiten. Die Beschaffenheit erstens eines jeden Elements unserer Anschauungen muss abhängig sein teils von der Natur der erregenden Processe, teils von der Art, wie diese Reize von der psychischen Tätigkeiten aufgenommen und verarbeitet werden. Hieraus ergiebt sich zunächst das auch von Seiten der Naturwissenschaften nicht mehr bezweifelte Resultat, dass das ganze Material unserer Empfindungen nur ein Zeichensystem für die Dinge ist, da die Eigenschaften, die | wir ihnen zuschreiben, nichts anderes sind als Resultate einer Wechselwirkung, deren eines Glied, die Beschaffenheit unserer seelischen Tätigkeiten, wir als selbstverständlich voraussetzen." (Erdmann, *Die Axiome der Geometrie*, pp. 93 sq.).

clxii. "Wir halten deshalb an dem oben entwickelten Begriff eines materialen und formalen Zeichensystems fest. In dem Begriff desselben liegt nicht bloß, dass es in allen seinen Beziehungen von den Verhältnissen der Dinge, die es wiedergebe, vollständig verschieden sei, sondern nicht minder auch, dass es alle jene Verhältnisse treu in seiner Weise zur Darstellung bringe. Dasselbe verlangt Correspondenz zwischen den Vorstellungen und den Dingen in derselben Weise wie es ihre Congruenz ausschliesst. Denn jene Wechselwirkung bedingt auch, dass in unseren Vorstellungen kein | Element vorhanden sei, das nicht durch entsprechende Elemente in den Dingen selbst erst hervorgerufen wäre." (Erdmann, *Die Axiome der Geometrie*, pp. 95 sq.).

clxiii. "Wir halten deshalb an dem oben entwickelten Begriff eines materialen und formalen Zeichensystems fest. In dem Begriff desselben liegt nicht bloß, dass es in allen seinen Beziehungen von den Verhältnissen der Dinge, die es wiedergeben, vollständig verschieden sei, sondern nicht minder auch, dass es alle jene Verhältnisse treu in seiner Weise zur Darstellung bringe. Dasselbe verlangt Correspondenz zwischen den Vorstellungen und den Dingen in derselben Weise wie es Congruenz ausschliesst. Denn jene Wechselwirkung bedingt auch, dass in unseren Vorstellungen kein | Element vorhanden sei, das nicht durch entsprechende Elemente in den Dingen selbst erst hervorgerufen wäre. Auch hier also tritt ein wenig beachteter Parellelismus zwischen den Empfindungen und den sie formenden Elementen hervor; denn die letzteren weisen nicht mehr und nicht weniger als jene auf entsprechende Beziehungen der Dinge zurück. Diese Uebereinstimmung braucht jedoch keine so absolute zu sein, wie jene Verschiedenheit; es ist wol möglich, und die physiologische Forschung hat dies für den speziellen Fall des Gesichtssinnes nur bestätigt, dass nicht alle Beziehungen, deren Existenz in den Dingen wir voraussetzen müssen, in unseren Vorstellungen unmittelbar zu entsprechender Bezeichnung gelangen. Dennoch dürfen wir insofern von einer vollkommenen Correspondenz zwischen beiden reden, als jene Abweichungen doch so gesetzmäßig sind, dass wir sie wissenschaftlich zu construieren vermögen." (Erdmann, *Die Axiome der Geometrie*, pp. 95 sq.).

clxiv. "Aber mehrere factische Umstände machen schon die Thatsache zweifelhaft, ob Begriffe im strengen logischen Sinne wirklich im menschlichen Denken vorkommen? und [sic] es fragt sich, ob dieselben nicht vielmehr logische *Ideal* seyen, *denen sich unser wirkliches Denken mehr und mehr annähern soll*? Diese Frage wird im zweyten Theile bejahet werden; es wird sich überdies zeigen, daß die Urtheile es sind, wodurch die Begriffe dem Ideal mehr und mehr angenähert werden, daher sie den letztern in gewissem Sinne vorangehen; es wird endlich klar werden, daß aus dieser Wirksamkeit der Urtheile sehr wichtige Folgen für die *metaphysischen* Begriffe insbesondere sich ergeben." (Johann Friedrich Herbart. *Lehrbuch zur Psychologie*. In: *Sämtliche Werke*. Vol. 4. Hermann Beyer & Söhne, 1891, pp. 295–436, p. 326).

clxv. "Es handelt sich um einen Prozeß, den wir in genau derselben Weise bei der theoretischen Behandlung irgendwelcher empirischer Daten

immerzu vollziehen und der ebendarum dem Naturforscher völlig selbstverständlich erscheinen mag. Ich werde mich in allgemeiner Fassung so ausdrücken: die Ergebnisse irgendwelcher Beobachtungen gelten immer nur innerhalb bestimmter Genauigkeitsgrenzen und unter partikulären Bedingungen; indem wir die Axiome aufstellen, setzen wir an Stelle dieser Ergebnisse Aussagen von absoluter Präzision und Allgemeinheit. In dieser 'Idealisierung' der empirischen Daten liegt meines Erachtens das eigentliche Wesen der Axiome." (R. Fricke and A. Ostrowski, eds. *Gesammelte Mathematische Abhandlungen. Liniengeometrie, Grundlegung der Geometrie, zum Erlanger Program.* Vol. Erster Band. Springer, 1921, p. 386).

clxvi. "Unser Geschäft wird ein dreifaches sein: 1) unsere Anschauung zu *analysiren*, um allgemeine Begriffe und Sätze zu gewinnen; 2) die darin gefundenen Bestimmungen zu *idealisieren*, indem wir ihnen eine absolute, unendliche Genauigkeit beilegen; 3) auf den so gewonnen Grundlagen *synthetisch* fortzubauen." (Friedrich Ueberweg. "Die Principien der Geometrie, wissenschaftlich dargestellt." In: *Archiv für Philosophie und Pädagogik* 17 [1851], pp. 20–54, p. 21).

clxvii. Erdmann cites Delboeuf's book on p. 64 in particular for "Delboeuf's remarks on the homogeneity of space". In Delbœuf's book one finds remarks like the following about homogeneity: "L'espace, le plan et la droite, au contraire, sont, en tant qu'homogènes, des déterminations tout idéales, purs produits de la pensée, et par cela même, formes immuables, typiques, mesures par excellence." (Joseph Delbœuf. *Prolégomènes philosophiques de la géométrie et solution des postulates.* J. Desoer, 1860, p. 181).

clxviii. Delbœuf says so in the preface: "Avant de terminer nous avons à dire quelquesmots de la traduction qui se trouve à la fin de l'ouvrage. La dissertation de M. Ueberweg, d'abord imprimée dans les Archives pédagogiques, nous avait été communiquée par l'auteur lui-même, et nous y avions fait quelques remarkques. Quand nous nous sommes décidé à faire imprimer notre ouvrage, nous avons obtenu de M. Ueberweg l'autorisation de traduire le sien, et il a bien voulu écrire une nouvelle introduction où il rencontre quelques-unes de nos objections. [...] | Le lecteur pourra du reste, par la comparaison des deux éditions du même ouvrage, juger de ces modifications." (Delbœuf, *Prolégomènes philosophiques de la géométrie et solution des postulates*, pp. XX sq.). I have not attempted to compare Delbœuf's translations systematically with Ueberweg's original article.

clxix. Hilbert writes: "Die Aufstellung der Axiome der Geometrie und die Erforschung ihres Zusammenhanges ist eine Aufgabe, die seit *Euklid* in zahlreichen vortrefflichen Abhandlungen der mathematischen Literatur sich erörtert findet [here a footnote refers to Klein and Veronese]. Die bezeichnete Aufgabe läuft auf die logische Analyse unserer räumlichen Anschauung hinaus." (Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*, p. 436).

clxx. "Geometrie ist eine Wissenschaft, welche die Eigenschaften des Raums synthetisch und doch a priori bestimmt. Was muß die Vorstellung des Raumes denn sein, damit eine solche Erkenntnis von ihm möglich sei? Er muß ursprünglich Anschauung sein; denn aus einem [B42] bloßen Begriffe lassen sich keine Sätze, die über den Begriff hinausgehen, ziehen, welches doch in der Geometrie geschieht (...). Aber diese Anschauung muß a priori, d.i. vor aller Wahrnehmung eines Gegenstandes, in uns angetroffen werden, mithin reine, nicht empirische Anschauung sein. Denn die geometrischen Sätze sind insgesamt apodiktisch, d.i. mit dem Bewußtsein ihrer Notwendigkeit verbunden, z.B. der Raum hat nur drei Abmessungen; dergleichen Sätze aber können nicht empirische oder Erfahrungsurteile sein, noch aus ihnen geschlossen werden (...)." (Kant, *Kritik der reinen Vernunft*, B40 sq.).

clxxi. "In dieser [i.e. the section entitled 'Transzendentale Ästhetik' in Kant's *Kritik der reinen Vernunft*] steht in der That die Anschauung als ein dem Begriffe Fremdes diesem gegenüber. Damit hat Kant eine der grössten mathematischen Thaten, eben die Analysis, in der Ästhetik nicht zur Geltung kommen lassen, obwohl seine eigene Analytik derselben den Weg zu ihr weist. [...] | Mit Recht bezeichnet darum David Hilbert den Weg, auf dem wir zu den 'Grundlagen der Geometrie' gelangen, als 'logische Analyse unserer räumlichen Anschauungen'. Kürzer und treffender kann die Unterstellung der Anschauung unter die Logik nicht bezeichnet werden. Hier wird vollkommen deutlich, was wir eben bei Kant vermissen, gefordert, dass durch die wissenschaftliche Geometrie die Anschauung der logischen Analyse zugänglich

gemacht und logisch bestimmt werde. Dieses Ziel bestimmte in gewisser Weise ja auch schon im Besonderen die analytische Geometrie in dem Augenblicke, da sie in der genialen Erfindungskraft eines Descartes geboren ward. Es ist also der Wissenschaft keineswegs erst in unserer Zeit gewiesen worden. Was aber unserer Zeit zu besonderem Verdienste gereicht, das ist gerade, um mit Cassirer zu reden, die allgemeine 'Tendenz der Wissenschaftlichen Geometrie, die anschaulichen Elemente, die sie zur ersten Anknüpfung nicht entbehren kann, im Fortgange der Untersuchung mehr und mehr zurückzudrängen, ja sie für die eigentliche Methode des Beweisses entbehrlich zu machen', und wie ich bloss explizite hinzufügen möchte, den Beweis wirklich logisch zu gestalten." (Bruno Bauch. "Erfahrung und Geometrie in ihrem erkenntnistheoretischen Verhältnis." In: *Kant-Studien* 12 [1907], pp. 213–235, pp. 214 sq.).

clxxii. "Endlich können wir unsere Aufgabe als eine *logische Analyse unseres Anschauungsvermögens* bezeichnen; die Frage, ob unsere Raumanschauung apriorischen oder empirischen Ursprung habe, bleibt dabei unerörtert." (Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*, p. 303).

clxxiii. "Zwischen ist ja logisch zunächst eine blosse Beziehung von einem Punkt zu irgend 2 anderen; sie bekommt erst Inhalt durch die Axiome. Wenn Sie wollen, dürfen wir erst dann das Wort 'zwischen' anwenden. Aber deshalb dürfen Sie nicht denken, dass unsere Untersuchungen überflüssig sind. Sie sind eben die logische Analyse unseres Anschauungsvermögens. Ueber Wesen und Bedeutung dieser Untersuchungen v[er]gl[eiche] die sehr zutreffenden Ausführungen in | Pasch, Neuere Geometrie, S. 99-100" (Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*, pp. 230 sq.); see also editorial note 35.

clxxiv. "Die Wissenschaft schöpft einen Theil ihres Stoffes unmittelbar aus der Sprache des täglichen Lebens. Aus dieser Quelle sind Ausdrucksweisen und Anschauungen, mit denen man wissenschaftliche Sätze nicht formuliren sollte, auch in die Mathematik hineingelangt und dort die Veranlassung geworden, dass gewisse Partieen unklar erscheinen, und dass sich zahlreiche Discussionen, namentlich über geometrische Dinge, erhoben haben. Welche Rolle die einzelnen Begriffe und Relationen in dem Systeme spielen, wieweit sie für das Ganze nothwendig oder entbehrlich sind, tritt nur bei absolut strenger Darstellung an den Tag. Erst wenn auf solchem Wege die wesentlichen Bestandtheile vollständig gesammelt, die überflüssigen aber ausgeschieden sind, wird man für jene Discussionen, soweit sie nicht dadurch gegenstandslos werden, die richtige Grundlage besitzen." (Moritz Pasch. *Vorlesungen über Neuer Geometrie*. 2nd ed. Teubner, 1912, p. 100).

clxxv. "Da Kant trotz aller Anerkennung, dass es notwendig sei, die Formen der Sinnlichkeit und des Verstandes auf mögliche Erfahrung zu beschränken, durch den Rationalismus seiner Kategorienlehre gezwungen ist, jeden bestimmenden Einfluss derselben auf diese Formen zu leugnen, so wird er hier und überall da, wo er sich mit diesem tatsächlichen Einfluss auseinander zu setzen hat, zu einer idealistischen Wendung seines Empirismus genötigt, welche die Berechtigung seiner Fassung des A priori aufhebt, da sie die objective Erfahrung eliminirt, gleich nichts setzt. Kant konnte jenes Argument, das einen positiv bestimmenden Einfluss der Erfahrung auf die psychischen Formen zugesteht, daher in seinen Gedankenkreis nicht aufnehmen, ohne in einen unlösbaren Widerspruch mit seinen rationalistischen Voraussetzungen zu geraden, da diese den Idealismus ausschließen, der die Begriffsbestimmung des A priori illusorischen machen würde." (Erdmann, *Die Axiome der Geometrie*, p. 166).

clxxvi. "Die Behauptung endlich, dass alle Elemente unserer Vorstellung von den Dingen nicht bloß gänzlich unab|hängig, sondern auch spezifisch verschieden sind, lässt sich als absoluter Rationalismus oder als Nativismus bezeichnen. [...] Kant's Kriticismus z.B., der allerdings weniger noch als die meisten übrigen philosophischen Systeme einen einheitlich durchgeführten erkenntnistheoretischen Grundgedanken enthält, würde sich einerseits als naiver Realismus, andrerseits als formaler Rationalismus auffassen lassen, daneben aber auch die Principien des Apriorismus enthalten, sofern er den Stoff der Sinnlichkeit als gegeben betrachtet, und die Formen des Verstandes und der Vernunft in schreiendem Widerspruch gegen ihren rationalen Charakter lediglich auf das Feld möglicher Erfahrung beschränkt." (Erdmann, *Die Axiome der Geometrie*, pp. 114 sq.).

clxxvii. "Die Axiome der Geometrie enthalten demnach die Merkmale des Raumbegriffs. Bis hierher ist der Beweisgang ohne Einfluss auf irgend eine der möglichen psychologischen Hypothesen; denn die Frage, ob der Inhalt dieser Raumvorstellung in nativistischem Sinne *a priori* oder in empiristischem *a posteriori* erworben wird, ist noch gar nicht berührt." (Erdmann, *Die Axiome der Geometrie*, p. 91).

clxxviii. "2.) Axiome der Geometrie (untersucht, welche Axiome bei den in der Geometrie der Anschauung gewonnenen Thatsachen benutzt werden und stellt systematisch die Geometrien gegenüber, bei welchen einige dieser Axiome weggelassen werden). 3.) Analytische Geometrie (ordnet den Punkten einer Geraden von vorneherein die Zahl zu und führt so die Geometrie auf die Analysis zurück). Die Bedeutung von [...] 2.) [ist] erkenntnisstheoretisch. [Von] 3.) wissenschaftlich mathematisch." (Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*, p. 22).

clxxix. "Die gemeinten Untersuchungen über Parallelentheorie haben mit ihren Weiterbildungen mathematisch nach zwei Seiten einen bestimmten Wert. Sie zeigen einmal – und dieses ihr Geschäft kann man als ein einmaliges, abgeschlossenes betrachten –, daß das Parallelenaxiom keine mathematische Folge der gewöhnlich vorangestellten Axiome ist, sondern daß ein wesentlich neues Anschauungselement, welches in den vorhergehenden Untersuchungen nicht berührt wurde, in ihm zum Ausdruck gelangt. Ähnliche Untersuchungen könnte man und sollte man mit Bezug auf jedes Axiom nicht nur der Geometrie durchführen; man würde dadurch an Einsicht in die gegenseitige Stellung der Axiome gewinnen." (Klein, *Gesammelte Mathematische Abhandlungen*, p. 493).

clxxx. "Bei *Riemann* ist die rein mathematische Betrachtung nicht überall von der Betrachtung mehr spekulativen Charakters geschieden, die sich auf die Objektivität der Raumanschauung usw. beziehen. Auf diesen Umstand ist die vielfach über die bez[üglichen] Dinge verbreitete Unklarheit wohl zum großen Teile zurückzuführen." (Felix Klein. "Über die sogenannte Nicht-Euklidische Geometrie (zweiter Aufsatz)." In: *Gesammelte Mathematische Abhandlungen. Liniengeometrie, Grundlegung der Geometrie, zum Erlanger Program.* Ed. by R. Fricke and A. Ostrowski. Vol. Erster Band. Springer, 1921, pp. 311–343, p. 312). Klein occasionally uses the word "bezüglich" (abbreviated "bez.") not in the common sense usually rendered in English by "respective", but rather in the sense of the English "relevant"; so here "die bez. Dinge" should be understood in the sense of "the relevant matters".

clxxxi. "Die nachstehenden Erörterungen beziehen sich auf die sogenannte Nicht-Euklidische Geometrie von Gauß, Lobatschewsky, Bolyai und die verwandten Betrachtungen, welche von Riemann und von Helmholtz über die Grundlagen unserer geometrischen Vorstellungen angestellt worden sind. Sie sollen indes nicht etwa die philosophischen Spekulationen weiterverfolgen, welche zu den genannten Arbeiten hingeleitet haben; vielmehr ist ihr Zweck die mathematischen Resultate dieser Arbeiten, so weit sie sich auf Parallelentheorie beziehen, in einer neuen, anschaulichen Weise darzulegen und einem allgemeinen deutlichen Verständnis zugänglich zu machen." (Felix Klein. "Über die sogenannte Nicht-Euklidische Geometrie (erster Aufsatz)." In: *Gesammelte Mathematische Abhandlungen. Liniengeometrie, Grundlegung der Geometrie, zum Erlanger Program.* Ed. by R. Fricke and A. Ostrowski. Vol. Erster Band. Springer, 1921, pp. 254–305, p. 254, the same opening remarks occur in (Felix Klein. "Über die sogenannte Nicht-Euklidische Geometrie (Vorläufige Mitteilung." In: *Gesammelte Mathematische Abhandlungen. Liniengeometrie, Grundlegung der Geometrie, zum Erlanger Program.* Ed. by R. Fricke and A. Ostrowski. Vol. Erster Band. Springer, 1921, pp. 244–253, p. 244).

clxxxii. "Man verknüpft mit dem Namen Nicht-Euklidische Geometrie eine Menge unmathematischer Vorstellungen, die auf der einen Seite mit ebenso viel Eifer gepflegt als auf der anderen perhorresziert werden, mit denen aber unsere rein mathematischen Betrachtungen gar nichts zu schaffen haben." (Felix Klein. "Vergleichende Betrachtungen über neuere geometrische Forschungen." In: *Gesammelte Mathematische Abhandlungen. Liniengeometrie, Grundlegung der Geometrie, zum Erlanger Program.* Ed. by R. Fricke and A. Ostrowski. Vol. Erster Band. Springer, 1921, pp. 460–497, p. 493).

clxxxiii. Erdmann, *Die Axiome der Geometrie*, p. 169.

clxxxiv. "Das Wesen dieser Methode besteht darin, dass man sich über die Voraussetzungen und die Methoden des Schliessens klar wird, die

man in einer Wissenschaft gebraucht. Axiomatisch zu verfahren ist also nichts anderes, als mit Bewusstsein zu denken." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 363).

clxxxv. Hilbert's lecture notes begin: "Meine Herren. Collegheft Grundlagen der Geometrie, S. 7-9 [bis] 'im Einzelnen einzugehen.'" (Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*, p. 221). By contrast, the opening remarks to the 1891 lecture course are exceedingly brief. The page-range Hilbert references includes precisely the paragraphs quoted in the following endnote, i.e. precisely the introductory remarks from the 1894 lecture.

clxxxvi. Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*, pp. 72 sq.

clxxxvii. The opening remarks for the public lecture notes of the 1898-99-course read Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*, pp. 302 sq.

clxxxviii. "*Anmerkung 2.* Zu den verschiedenen Methoden gehören ebenso verschiedene Übungen im Denken. Schädlich aber ist es, sich gewisse Fachwerke einzuprägen, in die alles passen soll; oder gar bestimmte Zahlen ein für allemal anzunehmen, nach denen alle Begriffe sich sollen spalten und wieder spalten lassen." (Johann Friedrich Herbart. *Lehrbuch zur Einleitung in die Philosophie.* Ed. by Wolfhart Henckmann. Meiner, 1993, p. 59) (This is remark is likely directed against such enterprises as Hegel's *Encyclopedia*.)

clxxxix. "Jede Wissenschaft nimmt ihren Anfang dann, wenn ein genügendes zusammenhängendes Tatsachenmaterial vorliegt. Sie entsteht aber erst durch *Ordnen* dieses Materials. Die Ordnung erfolgt durch die *axiomatische Methode*, d.h. man construiert ein *logisches Fachwerk von Begriffen* derart, dass den Beziehungen zwischen diesen Begriffen auch Beziehungen der zu ordnenden Thatsachen entsprechen. In der Aufstellung eines solchen Fachwerks von Begriffen liegt eine Willkürlichkeit [added: z.B. Stellung des Begriffes der Stetigkeit, ob zum Schluss oder gleich am Anfang des Axiomensystems – liefert ganz verschiedene Zugänge.]; wir verlangen aber von Ihnen: 1.) *Vollständigkeit*, 2.) *Unabhängigkeit*, 3.) *Widerspruchsfreiheit.*" (Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*, p. 540).

cxc. "Alle meine Geometrieen und die aus denselben folgenden Resultate laufen auf Nicht-Archimedische Gruppen hinaus." (Wissenschaftliches Tagebuch III, p. 40)

cxci. On the subject of Grelling's dissertation, Peckhaus notes: "Im Mai 1910 reichte Grelling bei der Philosophischen Fakultät in Göttingen sein Promotionsgesuch unter Vorlage der Dissertation *Die Axiome der Arithmetik* [...] ein, die auf eine Anregung von Zermelo hin entstanden war. [...]. Der mit der Begutachtung betraute David Hilbert bat den inzwischen in Zürich lehrenden Ernst Zermelo um sein Urteil und schloß sich dessen Gutachten an." (Volker Peckhaus. *Hilbertprogramm und Kritische Philosophie. Das Göttinger Modell interdisziplinärer Zusammenarbeit zwischen Mathematik und Philosophie.* Vandenhoeck & Rupprecht, 1990, p. 145).

Peckhaus does not cite any source for the claim that Hilbert deferred to Zermelo's judgement of the quality of Grelling's dissertation, but in the sentence I skipped, which summarises the dissertation's content, Peckhaus refers (in footnote 420) to the "Promotionsakte Kurt Grelling" held at the Universitätsarchiv Göttingen, so Peckhaus may – I have not, however, seen the Promotionsakte myself! – simply be reporting an explicit note to that effect by Hilbert from the evaluation Hilbert filed with the university.

cxcii. The passage in question is a reference to the "Satz von Hermite" on the page numbered "5." of the Courant-version (which is 4r of the notebook Hilbert 570/2:a). The name "Hermite" has been subsequently struck out (presumably by Courant) and instead the name "Paul Serret" appears as its replacement next to it *on the line*. The corresponding place in the Zermelo-version (which is 4v of the notebook Hilbert 570/2:b) makes no mention of Hermite, but rather (without any sign of correction) only of Paul Serret.

cxciii. On the page with number "3." (which is 3r of the notebook 570/2:a) of the Courant-version, the sentence "Abgesehen von dem

bekanntesten Falle $n = 2$." has been struck out; the sentence does not appear at all at the corresponding place in the Zermelo-version (3v of notebook 570/2:b). On the page with number "6." (which is 4v of the notebook Hilbert 570/2:a) the word "symmetrischen" in line 5 and an entire sentence further down the page have been struck out in ink (possibly by Courant); again, neither the word "symmetrisch" nor the struck-out sentence appears in the corresponding place of the Zermelo-version (on 5r of the notebook Hilbert 570/2:b). On the page with number "9." (i.e. 6v of the notebook Hilbert 570/2:a) the word "schweren" has been added (by Courant) above the line; in the corresponding place in Zermelo's version (7v of the notebook Hilbert 570/2:b) the word appears on the line, not as an insertion. Finally, on the page with number "11." (8v of the notebook Hilbert 570/2:a) Hilbert has added 3 additional presentation topics in pencil; only the first of these appears in the Zermelo-version (on 9v of the notebook Hilbert 570/2:b), but appears there written in ink and by Zermelo's hand as part of the text. More evidence of the fact that the Zermelo-version was copied after Hilbert had made some additions to Courant's version (besides the additional presentation topic) is that some, but not all, of the names of students committed to presenting on particular topics also appear in pencil in the margins of the Zermelo-version.

cxciv. When his preparatory notes for the 1914 lecture reach the section on logic (David Hilbert. "Ms. Cod. D. Hilbert 559. Ergänzungen zu Vorl[esung] S[ommer-]S[emester] 1910." SUB Göttingen, 1913-1915, p. 32v), Hilbert refers to the exposition of the "Axiome der Logik" in the 1905 lecture-notes "S. 219-[crossed out: 268]249". Then a brief connecting paragraph appears, which is, however, identical in substance to the introductory remarks Hilbert found at the start of Hilbert's discussion of logic in the official lecture-notes prepared by Hellinger for the 1905 lecture course on pp. 219ff., followed by a reference to "S. 225-", which, incidentally, marks the end of Hilbert's preparatory notes.

cxcv. See p. 223 of the official lecture-notes compiled by Hellinger.

cxcvi. See p. 226 of the official lecture-notes compiled by Hellinger.

cxcvii. See p. 225 of the official lecture-notes compiled by Hellinger.

cxcviii. "Von vorstehender Multiplikationsregel für Polynome kann man sagen, dass sie auch das Distributionsgesetz 27) als besondern Fall in sich schliesse, indem man es als zulässig erachtet und sich vorstellen kann, dass das eine der beiden zu multiplizirenden Polynome von vornherein als ein Monom gedacht werde oder auf ein solches sich reduzire [...]." (Schröder, *Vorlesungen über die Algebra der Logik*, p. 311).

cxcix. "Diese Axiome, welche jedenfalls (wie auf anderen Gebieten manche) einen empirischen Charakter durchaus nicht haben, möchte ich deshalb genauer als lediglich *formale* bezeichnen, indem ich als solche nur diejenigen Behauptungen aufführe, welche ich nicht durch formelle Rechnung nach bereits begründeten und klassificirten Methoden oder durch äusserlich dargestellte und verfolgbare Schlüsse ausdrücklich aus früheren ableite." (Ernst Schröder. *Der Operationskreis des Logikkalkuls*. Verlag von B. G. Teubner, 1877, p. 4).

cc. So Peirce says: "I use the sign $-\!\!<$ in place of $\underset{=}{<}$. My reason for not liking the latter sign are that it cannot be written rapidly enough, and that it seems to represent the relation it expresses as being compounded of two others which in reality are complications of this." (Charles Sanders Peirce. "Description of a Notation for the Logic of Relatives, resulting from an Amplification of the Conception of Boole's Calculus of Logic." In: *Writings of Charles S. Peirce: A chronologial edition. Volume 2: 1867-1871*. Ed. by Edward C. Moore. Indiana University Press, 1984, pp. 359–429, p. 360). One way of understanding Peirce's concern with "rapidity" is as meaning that he wanted to write the symbol without having to lift the pen off the paper. So, presumably, in the manuscript form Schröder may have seen in Peirce's letters the '$<$'-part would look more like '$\subset$'.

cci. "Haben wir $A \vee B \vee C \vee \cdots \vee \overline{B} \vee \ldots$, so ist dies richtig. (Ist ein Faktor null, so ist das Produkt null.)" (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 604).

ccii. Hermann Schubert. "Grundlagen der Arithmetik." In: *Encyklopädie der mathematischen Wissenschaften mit Einschluss ihrer Anwendungen. Arithmetik und Algebra*. Vol. I. Verlag von B.G. Teubner, 1898, pp. 1–27, p. 7.

cciii. David Hilbert. "Logische Prinzipien des Mathematischen Denkens. Vorlesungen im Sommer-Semester 1905, ausgearbeitet von stud. math. Ernst Hellinger." MI Göttingen, 1905, p. 216; substantially the same remark appears in the Born-notes David Hilbert. "Logische Prinzipien des Mathematischen Denkens. Vorlesungen im Sommersemester 1905, ausgearbeitet von Max Born, cand. math." SUB Göttingen, 1905, p. 141.

cciv. "Vorangestellt sei I. Die *Definition der Gleichheit* zweier oder mehrerer Klassensymbole. Die letzteren sollen einander gleich heissen, wenn die von ihnen vorgestellten Klassen identisch die nämlichen Individuen umfassen, wenn jene also nur Namen für ein und dieselbe Klasse sind. Darnach gilt II. Das *Axiom*: Jedes Klassensymbol ist sich selbst gleich, III Das *Axiom*: Wenn zwei Klassensymbole einem dritten gleich sind, so sind sie auch unter sich gleich — nebst den noch allgemeineren Sätzen, welche sich in bekannter Weise hieraus folgern lassen, cf. z.B. [Schröder's *Lehrbuch der Arithmetik*] S. 25 und 26." (Schröder, *Der Operationskreis des Logikkalkuls*, p. 5).

ccv. "*Gleiche Rechnungen, mit gleichen Zahlen vorgenommen, müssen gleiche Resultate liefern* [...]." (Ernst Schröder. *Lehrbuch der Artihmetik und Algebra*. Vol. 1 (mehr nicht erschienen). Verlag von B. G. Teubner, 1873, p. 26), which Schröder presents as somehow derived from the "principle" that "equals may be substituted for equals": "Es liegt überhaupt in dem oben formulierten Begriffe der (Zahlen-)Gleichheit, dass man die Namen von gleichen Zahlen stets für einander setzen darf — überall wenigstens, wo von den Zahlen selbst die Rede ist und nicht etwa blos von der Beschaffenheit ihres betreffenden Namens. Namentlich also in jedem Ausdruck in jeder Relation *darf gleiches stets für gleiches gesetzt werden*; der Ausdruck wird dadurch seine Bedeutung (seinen Werth) nicht ändern und die Relation wird richtig bleiben. Man pflegt dieses Princip von alters her durch den Satz auszudrücken: *Gleiches, für gleiches gesetzt, gibt gleiches.*" (Schröder, *Lehrbuch der Artihmetik und Algebra*, p. 24).

ccvi. "Aus dem obigen Grundsatze der Vertauschbarkeit von gleichen Zahlen folgt auch leicht jener berühmte Satz, der fälschlich noch hie und da als ein Axiom aufgeführt wird: (A) *Wenn zwei Zahlen einer dritten gleich sind, so sind sie auch unter sich gleich.*" (Schröder, *Lehrbuch der Artihmetik und Algebra*, p. 25).

ccvii. "Als den eigentlichen Inhalt dieser beiden Nummern [i.e. 1^0 and $1'$] sehe ich aber nicht die angeführten Definition, sondern die sich an sie anlehnenden *Postulate* an, *gegebene Klassensymbole mit einander zu multipliciren resp. zu addiren*, d.h. also mit anderen Worten die axiomatischen Sätze: *Axiom. Das Produkt/Die Summe von Klassensymbolen ist immer wieder ein Klassensymbol*, oder die logische Multiplikation resp. Addition ist eine *unbedingt ausführbare* Operation;" (Schröder, *Der Operationskreis des Logikkalkuls*, p. 7).

ccviii. "Ich unterscheide in der reinen Mathematik *dreierlei* Axiome: 1) Die *logischen Axiome*, die sie mit *allen anderen Wissenschaften gemein* hat und die in der formalen Logik, neuerdings im Logikcalcül systematisch behandelt werden." (Georg Cantor. *Briefe*. Ed. by Herbert Meschkowski and Winfried Nilson. Springer, 1991, p. 426).

ccix. "Für den Satz hat, wie ich glaube, zuerst H[err] Schröder einen direkten Beweis gefunden, allein auch ganz unabhängig davon einer meiner Zuhörer H[err] Felix Bernstein so dass der Beweis keineswegs vom Logikcalcul [sic] abhängig ist." (Cantor, *Briefe*, p. 401).

ccx. "Um das Problem [der Widerspruchsfreiheit] von dieser Seite in Angriff zu nehmen, werden wir darauf ausgehen, uns einen Überblick über die möglichen logischen Schlüsse zu verschaffen, welche aus einem Axiomensystem gezogen werden können. Als das geeignete Mittel hierfür bietet sich die Methode der *Formalisierung des logischen Schließens*, wie sie von Frege, Schröder, Peano und Russell ausgebildet worden ist." (Hilbert and Bernays, *Grundlagen der Mathematik*, p. 18).

ccxi. "Näheres darüber mögen Sie in Schröders Handbuch des Logikkalküls nachlesen." (David Hilbert. "Probleme und Prinzipienfragen der

Mathematik." Library of the Institute for Advanced Studies, Princeton, 1914-15, p. 218).

ccxii. Like most of Schröder's theorems, theorem 14 is stated in two versions, each of which Schröder as the other's dual. The notational convention Schröder follows in distinguishing the two dual versions of a theorem are less perspicuous in the 1877 than they are later in the *Vorlesungen*: In the *Vorlesungen* Schröder subscripts either "$_+$" or "$_\times$" to the number of the theorem at issue; in the 1877 article, Schröder superscripts "^{0}" to the version of the theorem concerning the logical product as main operator and to the version of the the theorem concerning the logical sum as main operator, Schröder superscripts "$'$" (the convention is introduced on Schröder, *Der Operationskreis des Logikkalkuls*, p. 3). The two dual versions of theorem 14 read: "14^0) *Theorem. Jede Klasse b kann linear und homogen durch jede andere a ausgedrückt werden in der Form*: $b = xa + ya_1$, [14') *Theorem.* Desgleichen kann stets gesetzt werden: $b = (y + a)(x + a_1)$.] wo x und y gewisse nicht vollkommen bestimmte *Klassensymbole* vorstellen, die auch gleich 0 oder 1 sein können." (Schröder, *Der Operationskreis des Logikkalkuls*, p. 14).

ccxiii. "Was die behufs Herstellung des Ausdrucks auszuführenden Operationen betrifft, so wollen wir zunächst annehmen, dass die Symbole a und a_1 nur durch die Operation der logischen Addition und Multiplikation unter sich und mit andern unabhängig von ihnen gegebenen zu verknüpfen seien – ein besonders einfacher Fall, auf welchen | aber – wie aus dem später folgenden erhellen wird – alle denkbaren Fälle sich leicht zurückführen lassen. In diesem Falle nun kann man alle etwa als Faktoren auftretenden Summenausdrücke nach 6^0 [i.e. the distributive law] ausmultipliciren, und den ganzen Ausdruck in ein Aggregat von Monomien auflösen. Jedes dieser Monome kann die Symbole a und a_1 nie gleichzeitig und jedes höchstens einmal als Faktor enthalten wegen 7^0) [i.e. the laws of excluded middle and non-contradiction] und 5^0) [i.e. the laws of idempotency]." (Schröder, *Der Operationskreis des Logikkalkuls*, pp. 14 sq.).

ccxiv. "Ein *Monom* oder Monomium (zusammengezogen aus Mononomium) soll eine für sich stehende Zahl bedeuten, überhaupt einen Ausdruck, der nicht aus einer Addition hervorgegangen ist, eigentlich also gar nicht in der Form einer Summe erscheint. Zum Beispiel die Ausdrücke $(x + y)^2$, $3b$, c^n, 7, $\frac{5(a^2+b^2)}{\sqrt{c}}$, a, etc. sind lauter Monome, und es erhalten viele Sätze über Summen in vortheilhafter Weise eben dadurch eine grössere Tragweite, dass in ihnen auch derartige Ausdrücke als *eingliedrige* Summen mit zugelassen sind, wenngleich allerdings, nach der ursprünglichen Erklärung der Summe zur Bildung derselben mehr als *ein* Glied erforderlich ist." (Schröder, *Lehrbuch der Artihmetik und Algebra*, p. 55).

ccxv. "Einen ersten Theil des Logikkalkuls bildet demgemäß die *Rechnung mit Begriffen*; [...]. Der zweite Theil umfasst das *Rechnen mit Urtheilen* [...]. Während in beiden Theilen die Rechnung nach denselben Gesetzen vor sich geht, ist nur die Interpretation der Formeln in jedem derselben eine andere. Indem wir deshalb zunächst nur dem ersten derselben unsere Aufmerksamkeit zuwenden, werden wir finden, dass hernach der andere Theil durch eine einfache Bemerkung sich miterledigt, die nämlich – um es gleich vorweg zu sagen –, dass man unter den Buchstaben, welche die Urtheile vorstellen, statt dieser lediglich die Zeiten (oder 'Klassen von Zeittheilen') zu setzen braucht, während welcher sie bezüglich wahr sind, um sofort die Untersuchung zu einer dem ersten Theile des Logikkalkuls angehörigen zu stempeln." (Schröder, *Der Operationskreis des Logikkalkuls*, p. 1).

ccxvi. "Die Rechenarbeit kann hierbei oft sehr verringert werden durch die Bemerkung von *Boole*, welcher dem Satze die Form gibt: $f(a) = f(1) \cdot a + f(0) \cdot a_1$, da offenbar $f(1)$ und $f(0)$ auch aus dem noch unreducirten Ausdruck $f(a)$ direkt abgeleitet werden können [...]." (Schröder, *Der Operationskreis des Logikkalkuls*, p. 15).

ccxvii. See pp. 239ff. of the official lecture notes compiled by Hellinger for the 1905 lecture course.

ccxviii. "Hierzu werden wir veranlaßt, indem wir bemerken, daß die konjunktive sowie die disjunktive Normalform keineswegs eindeutig ist [...] Wir werden daher versuchen, durch zusätzliche Forderungen die Normalform festzulegen. Eine solche Normierung gelingt in der Tat, allerdings nur in dem Sinne, daß im voraus angegeben wird, welche Variablen, außer den in dem betrachteten Ausdruck auftretenden, noch mit als Argumente der betreffenden Wahrheitsfunktionen angesehen werden sollen. [...] | Hierzu verfahren wir nach Boole so [...]"

(Hilbert and Bernays, *Grundlagen der Mathematik*, pp. 55 sq.).

ccxix. He says of his theorem 18 (i.e. de Morgan's laws), among other results: "Vorstehende 3 einfachen Sätze sind es hauptsächlich, welche den *Boole*'schen arithmetisch-logischen Rechenapparat durchaus entbehrlich machen helfen – ein Apparat, welcher im wesentlichen besteht in der Aufrechterhaltung [Rechtfertigung, s.u. § 4, 40'] und Anwendung der beschwerlichen Entwicklungsschemata (D), (E), (F),... sub 14^0) für alle möglichen durch Auflösung von Gleichungen noch *nach arithmetischen Regeln* gewonnen Ausdrücke." (Schröder, *Der Operationskreis des Logikkalkuls*, p. 20).

ccxx. "Die Sätze [i.e. Schröder's axioms 5^0 and $5'$, which are the laws of idempotency for multiplication and addition respectively] sind natürlich auch auf beliebig viele Operationsglieder auszudehnen: $aaa\cdots = a$, und kommen demnach im Logikkalkül keine Potenzen vor." (Schröder, *Der Operationskreis des Logikkalkuls*, p. 9).

ccxxi. "So erhalten wir endlich ebenfalls die Form: $f(a) = xa + ya_1$, worin nun x und y unabhängig von a und a_1 gegeben sind. Es enthalten diese Coefficienten hier nichts willkürliches mehr, sondern die Forderung ihrer Unabhängigkeit von a in Verbindung mit der ihrer [sic] Entstehung aus dem gegebenen Ausdruck $f(a)$ haben dieselben ihre Bestimmung gefunden." (Schröder, *Der Operationskreis des Logikkalkuls*, p. 15).

ccxxii. "Das symbolische Produkt deute man als die Verknüpfung zweier Aussagen durch 'oder' [...] $X + Y$ bezeichnet das Gegenteil einer Aussage, welche dann und nur dann zutrifft, wenn mindestens eine der beiden Aussagen X, Y falsch ist; $X + Y$ | bedeutet also, dass sowohl X wie Y wahr ist und kann demgemäss sprachlich durch 'X und Y' ausgedrückt werden." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, pp. 235 sq.).

ccxxiii. "Wegen dieser Analogie zur Algebra wollen wir $X\&Y$ auch als die *logische Summe* und $X \vee Y$ als das *logische Produkt* bezeichnen." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 815).

ccxxiv. "Ebensogut hätten wir übrigens $X\&Y$ als logisches Produkt und $X \vee Y$ als logische Summe bezeichnen können, und diese Bezeichnung ist sogar in der Logik gebräuchlicher. Es gilt nämlich, im Unterschied zur Algebra, noch ein *zweites distributives Gesetz*: $X\&(Y \vee Z)$ äq $(X\&Y) \vee (X\&Z)$." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 815).

ccxxv. See p. 243 of the official lecture notes compiled by Hellinger for the 1905 lecture course.

ccxxvi. "Bei der ersten Normalform kann man sofort Teilurteile herauslesen, daher wird sie in der Regel bevorzugt; allerdings hat das nur einen Sinn wenn etwas positives gesagt wird. Bei einer Kritik wäre die zweite Normalform zu bevorzugen." (Hilbert, "Probleme und Prinzipienfragen der Mathematik," p. 217).

ccxxvii. "Die disjunktive Normalform hat den Vorzug einer besonderen Übersichtlichkeit. Die einzelnen Faktoren geben die verschiedenen Möglichkeiten, unter denen die gegebene Aussagenverbindung zu Rechzt besteht. So lautet z.B. die zu $X \sim Y$ gehörige disjunctive Normalform $(X\&Y)(\overline{X}\&\overline{Y})$, und diese läßt erkennen, daß X und Y entweder beide bestehen oder beide nicht bestehen müssen, falls $X \sim Y$ richtig sein soll." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 822).

ccxxviii. "Man erkennt [bei der Benennung und der Bezeichnung der grundlegenden Verknüpfungen] keine Nötigung, die Verknüpfungen der Disjunktion – des nicht ausschließenden 'oder' – und der Konjunktion – des 'und' zwischen Aussagen oder Prädikaten –, die einander in der symbolischen Logik bekanntlich ganz gleichberechtigt gegenüberstehen, gerade mit den Namen und den Zeichen der Addition und der Multiplikation zu versehen. Es wäre in der Tat genau so berechtigt und nicht weniger sinnvoll gewesen, die Konjunktion – als das logische 'und'! – als Addition und die Disjunktion demgegenüber als Multiplikation zu bezeichnen, wie dies von Hilbert [footnote: Vorlesung über die Prinzipien der Mathematik] tatsächlich geschieht; vom Standpunkt der Aussagen- oder Inhaltslogik ist der zweite

Weg genau ebenso der natürlichere, wie der erste von der Klassen- oder Umfangslogik – dem 'Gebietskalkül' der Algebra der Logik – her näher liegen mußte. Ich habe jedenfalls den Eindruck, daß die obige falsche Parallele, statt das Verständnis zu erleichtern, in Wahrheit nur Verwirrung gestiftet hat." (Heinrich Behmann. "Beiträge zur Algebra der Logik, insbesondere zum Entscheidungsproblem." In: *Mathematische Annalen* 86 [1922], pp. 163–229, p. 165).

ccxxix. Hilbert, "Probleme und Prinzipienfragen der Mathematik," p. 218.

ccxxx. See p. 248 of the official lecture notes produced by HellingerHilbert, "Logische Prinzipien des Mathematischen Denkens," p. 248.

ccxxxi. "Wir fragen wiederum, ob gewisse Folgerungen aus den Axiomen 1.-5. einen Widerspruch bilden oder [...]. Zur Beantwortung dieser Frage berücksichtigen wir, daß das Axiom 5. das einzige ist, welches zu Aussagen von der Form $\overline{a}$ [...] Anlaß gibt. Aussagen die mit 5. einen Widerspruch bilden, müssen daher jedenfalls von der Form 6.$\mathfrak{f}(\mathfrak{u}x^{(0)}) = \mathfrak{u}1$ sein; eine solche Folgerung aber kann aus den Axiomen 1.-4. auf keine Weise entstehen. | Um dies einzusehen, bezeichnen wir die Gleichung, d.h. das Gedankending $a = b$, als eine *homogene Gleichung*, wenn sowohl a wie b Kombinationen von je zwei einfachen Dingen sind, ebenso wenn a und b beide irgendwelche Kombinationen von je drei oder beide irgendwelche Kombinationen von je vier oder mehr einfachen Dingen sind; [...]. Aus den Axiomen 1. und 2. allein folgen, wie wir vorhin gesehen haben, lauter homogene Gleichungen, nämlich die Gleichungen von der Form $a = a$. Ebenso liefert Axiom 3., wenn wir darin für x irgendein Gedankending nehmen, nur homogene Gleichungen. Desgleichen weist Axiom 4. in der Behauptung gewiß stets eine homogene Gleichung auf, sobald die Voraussetzung eine homogene Gleichung ist, und somit können überhaupt nur homogene Gleichungen als Folgerungen aus den Axiomen 1.-4. auftreten." (David Hilbert. "Über den Zahlbegriff." In: *Grundlagen der Geometrie*. 7th ed. Teubner, 1904, pp. 241–246, pp. 254 sq.).

ccxxxii. In the remarks at the end of the 1904 article, Hilbert makes some programmatic remarks, including: "Die Widerspruchsfreiheit der Axiome kann dann entweder so erkannt werden, daß man zeigt, wie ein etwaiger Widerspruch sich schon auf einem früheren Standpunkt in der Entwicklung der Theorien zeigen müßte, oder indem man die Annahme macht, es gäbe einen Beweis, der aus den Axiomen auf einen bestimmten Widerspruch führt, und alsdann dartut, daß ein solcher Beweis nicht möglich ist, nämlich in sich einen Widerspruch enthielte." (Hilbert, "Über den Zahlbegriff," p. 260). Then, on the following page, he goes on to say: "[...] man hat eben den Beweis selbst als ein mathematisches Gebilde, nämlich eine endliche Menge zu betrachten, deren Elemente durch Aussagen verbunden sind, die zum Ausdruck bringen, daß der Beweis aus den Axiomen 1.-4. auf die Gleichung 6. führt, und man hat dann zu zeigen, daß ein solcher Beweis einen Widerspruch enthält und also nicht in unserem definierten Sinne widerspruchsfrei existiert." (Hilbert, "Über den Zahlbegriff," p. 261).

ccxxxiii. "Wir beschränken uns auf den einfachsten Fall und betrachten ein System von endlich vielen Sätzen, die die Form haben: Wenn a ist, ist b. Unter den so verbundenen Elementen [i.e. a and b] mögen wir etwa Naturereignisse oder auch geometrische Beziehungen verstehen [...]" (Paul Hertz. "Über die Axiomensysteme kleinster Satzzahl für ein System von Sätzen und den Begriff des idealen Elements." In: *Jahresbericht der Deutschen Mathematiker-Vereinigung* 31 [1922], [154–157, p. 154).

ccxxxiv. "Entsprechend dem Entwicklungsgange, den die Wissenschaft selbst genommen hat, werde ich also den Begriff der Zahl nur *stufenweise* ausbilden, und gehe ich aus von der Betrachtung der einfachsten Art von Zahlen, deren Wesen am leichtesten zu begreifen ist. Dies sind die sogenannten *natürlichen* Zahlen, welche auch, im Gegensatz zu den übrigen, *ganze positive* Zahlen genannt werden." (Schröder, *Lehrbuch der Artihmetik und Algebra*, p. 3).

ccxxxv. "Der Beweis von weiteren Sätzen sowie die Einführung der Multiplikation b[e]reiten keine Schwierigkeiten mehr; letztere geschieht durch folgende | Gleichungen: $a \cdot 1 = a$[,] $a(b+1) = a \cdot b + a$" (Hilbert, "Probleme und Prinzipienfragen der Mathematik," pp. 205 sq.).

ccxxxvi. "Umgekehrt, wenn $a = b + u$ ist, so folgt auch $a > b$, d.h.: (A) *Die Summe ist immer grösser als eines ihrer Glieder*, das Ganze grösser

als seine Theile." (Schröder, *Lehrbuch der Artihmetik und Algebra*, p. 61).

ccxxxvii. "Wendet man nun ein, daß bei einigermaßen großen Zahlen diese unmittelbare Anschauung zur Vermittlung der Erkenntnis nicht mehr ausreicht, so wird zur Ergänzung der *Satz von der vollständigen Induktion* hinzugenommen. *Gilt ein Satz für kleine Zahlen und folgt aus seiner Gültigkeit für eine Zahl n allemal auch die für $n+1$, so ist er allgemein für jede Zahl richtig.* Dieser Satz, dessen Ursprung ich für echt intuitiv halte, hilft in der Tat gerade über die Schranke hinweg, an der die sinnliche Anschauung versagt. Das ist mehr oder minder auch der Standpunkt von *Poincaré* in seinen bekannten philosophischen Schriften." (Felix Klein. *Elementarmathematik vom höheren Standpunkte aus. Erster Band: Arithmetik, Algebra, Analysis.* Ed. by Ernst Hellinger and Friedrich Seyfarth. Vierte Auflage. Springer-Verlag, 1933, p. 12).

ccxxxviii. Schröder, *Lehrbuch der Artihmetik und Algebra*, p. 51.

ccxxxix. Schröder says: "Die sogenannte *independente* Behandlungsweise besitzt den Vorzug, eine jedermann sofort zugängliche *Motivierung* für sämmtliche Festsetzungen abzugeben, und zwar überall die einfachsten Beweggründe zu offenbaren, die sich denken lassen; sie führt alle neuen Begriffe auf leichte und naturgemäße Weise ein. Bei dieser Behandlungsweise sind überdies weniger lange Schlussreihen erforderlich, sie eignet sich mehr zu abgekürzter Behandlung und ist gewissermassen intuitiver, macht dafür aber grössere Ansprüche an das innere Anschauungsvermögen. Sie ist ausserdem diejenige Methode, durch welche die Theorie historisch nach und nach herangebildet worden ist." (Schröder, *Lehrbuch der Artihmetik und Algebra*, p. 51). Cf. Hilbert, "Logische Prinzipien des Mathematischen Denkens," pp. 9 sq.

ccxl. "Die andere Behandlungsweise, die wir die *recurrente* nennen, ist aus dem Bedürfniss grösserer Strenge in der Absicht nach möglichster Vereinfachung der zum Ausgangspunkt genommenen Voraussetzungen und Einschränkungen der alsdann benutzten Schlussmittel hervorgegangen. Man verdankt dieselbe hauptsächlich *Grassmann* [Schröder refers to Grassmann's *Lehrbuch der Arithmetik* (1861)] und kann das genannte Ziel als durch sie erreicht angesehen werden; dieselbe lässt in Hinsicht der Gründlichkeit kaum noch zu wünschen übrig." (Schröder, *Lehrbuch der Artihmetik und Algebra*, p. 51).

ccxli. "Indem wir von den bisherigen Ergebnissen jetzt absolut nichts voraussetzen, führen wir die Namen der natürlichen Zahlen ein durch folgende Reihe von Gleichungen: (5) $2 = 1 + 1$), $3 = 2 + 1$, $4 = 3 + 1$, $5 = 4 + 1$, etc., die wir uns ins unbegrenzte fortgesetzt denken, so jedoch, dass niemals der Name einer früheren Zahl wiederkehrt." (Schröder, *Lehrbuch der Artihmetik und Algebra*, p. 63).

ccxlii. "Ist nun a eine Zahl aus unserer Reihe: $1, 2, 3, 4, 5, ...$ so ist auch $a+1$ eine solche, indem: $a' = a+1$ die allgemeine Form der Gleichungen (5) [see the passage in ccxli] ist." (Schröder, *Lehrbuch der Artihmetik und Algebra*, p. 64).

ccxliii. "*Wenn $a > b$ und $b > c$, so ist um so mehr* (a fortiori) *auch $a > c$* [...]." (Schröder, *Lehrbuch der Artihmetik und Algebra*, p. 61).

ccxliv. "Ich gehe nun dazu über, das Associationsgesetz für die Addition [...] allgemein zu beweisen. Zu diesem Zwecke kann man eine Ueberlegung ausführen, welche von vielfältiger Anwendung in der ganzen Mathematik und unter dem Namen der *vollständigen Induktion* (*Schluss von n auf $n+1$*, *Bernoulli'scher Inductionsschluss*, nach andern auch *Kästner'sches Verfahren*) bekannt ist. Zuerst nämlich beweist man, dass wenn die Formel für einen bestimmten Werth c richtig ist, sie auch für den nächst höheren $c+1$ gelten muss. [...] Nun überlegt man weiter, dass die Formel jedenfalls erfüllt ist, wenn c den Werth 1 hat [...]. Gilt sie aber für $c = 1$, so muss sie nach dem eben bewiesenen Satze auch für $c = 1 + 1 = 2$ richtig sein; ist letzteres der Fall, so muss die Formel nach demselben Satze auch für $c = 2 + 1 = 3$ gültig | bleiben, woraus wieder ihre Gültigkeit für $c = 3 + 1 = 4$ folgt, und so weiter. Da durch successive Addition von 1 alle Zahlen erhalten werden, so muss also das associative Princip für jeden Werth von c, mithin allgemein gültig sein." (Schröder, *Lehrbuch der Artihmetik und Algebra*, pp. 66 sq.).

ccxlv. "Es kann also die obige Ungleichung auch in Gestalt der Gleichung ausgedrückt werden: $a = b + u$, worin u wie gesagt eine *unbestimmte* Zahl vorstellt, auf deren Werth es uns weiter nicht ankommt, und die, wie später gezeigt wird, den *Unterschied* von a und b oder den *Ueberschuss* von a und b vorstellt." (Schröder, *Lehrbuch der Artihmetik und Algebra*, p. 61).

ccxlvi. "An zweiter Stelle ist nun [...] zu nennen [...], daß man versucht, jene 11 Grundgesetze in eine *größere Zahl kleinerer Schritte* zu zerlegen, von denen | man dann nur die einfachsten direkt der Anschauung zu entnehmen braucht, während sich die andern aus ihnen logisch ohne weitere Hinzuziehung der Anschauung erschließen lassen. Während sonst die Möglichkeit rein logischen Operierens erst *nach* Aufstellung der 11 Gesetze beginnt, kann dieses hier schon früher, nach jenen einfacheren Sätzen, einsetzen: *Die Grenze zwischen Anschauung und Logik wird zugunsten der letzteren verschoben.* Bahnbrechend für dieses Vorgehen ist *Hermann Graßmann* in seinem '*Lehrbuch der Arithmetik*' *von 1861.* Als Beispiel daraus erwähne ich nur, daß das Gesetz der Kommutativität sich aus dem Assoziativitätsgesetz mit Hilfe des Prinzips der vollständigen Induktion ableiten läßt. — Neben dem Graßmannschen Buche ist wegen der Präzision seiner Darstellung insbesondere ein Buch des Italieners *Peano* zu nennen: '*Arithmetices principia nova methodo exposita*'." (Klein, *Elementarmathematik vom höheren Standpunkte aus*, pp. 12 sq.).

ccxlvii. "Durch diese Ueberlegung, welche der landläufigen Begründung der Arithmetik ganz vertraut ist — (vgl. insbesondere die Darstellung bei Helmholtz in der Abhandlung 'Zählen und Messen, erkenntnistheoretisch betrachtet'[)] — wird der Anschluss der Theorie der Zahlzeichen an den gewöhnlichen Anzahlbegriff erreicht. Und damit ist die Frage nach der Anwendbarkeit der Zahlentheorie auf die Wirklichkeit in befriedigender Weise beantwortet. Für die elementare Zahlentheorie erweist sich somit in der Tat die landläufige Auffassung von der mathematischen Erkenntnis als berechtigt; und wir würden in der Begründung der Mathematik aller Schwierigkeiten enthoben sein, wenn wir im Gebiete der Arithmetik und Analysis mit derjenigen Art der anschaulichen Ueberlegung auskämen, wie ich sie Ihnen in der Theorie der Zahlzeichen dargelegt habe." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 470).

ccxlviii. "Weiter als die übrigen Arithmetiker, deren Arbeiten ich kenne, und gleichzeitig philosophische Gesichtspunkte verfolgend, sind die Herren Hermann und Robert Grassmann in diese Untersuchung eingedrungen, und in der Ausführung der arithmetischen Schlußfolgerungen werde ich mich im folgenden ihrem Wege durchaus anzuschließen haben. [...] An die Brüder Grassmann hat sich unter den neueren Arithmetikern auch Herr E. Schröder im wesentlichen angeschlossen, ist aber in einigen wichtigen Erörterungen noch tiefer gegangen." (Hermann von Helmholtz. "Zählen und Messen, erkenntnistheoretisch betrachtet." In: *Schriften zur Erkenntnistheorie.* Ed. by Moritz Schlick and Paul Hertz. Springer Verlag, 1921, pp. 70–97, p. 71).

ccxlix. "Die *Zahl* (...) ist jedenfalls ein willkürlich von uns geschaffenes Zeichen, welches als Mittel dient, um sehr mannigfaltige Zwecke zu erreichen." (Schröder, *Lehrbuch der Artihmetik und Algebra*, p. 2). Likewise in the passage I quoted in endnote ccxli, Schröder speaks of introducing not the numbers, but rather their names by means of a recursive definition.

ccl. "*Jedes der zu zählenden Dinge wird eine Einheit genannt.* [...] Um nun ein Zeichen zu erhalten, welches fähig ist auszudrücken, *wie viele* jener Einheiten vorhanden sind, richtet man die Aufmerksamkeit der Reihe nach *ein* mal auf eine jede derselben und bildet sie mit einem Strich: 1 (eine *Eins*, ein *Einser*) ab; diese Einer setzt man in eine Zeile nebeneinander, verbindet sie jedoch unter sich durch das Zeichen + (*plus*) [...] | Wie wir eben gesehen haben, ist die Zahl gewissermassen eine *Abbildung* der zu zählenden Dinge. Während aber die Gegenstände der Körperwelt von dem Bildhauer abgebildet werden hinsichtlich ihrer räumlichen Ausdehnung und Begrenzung (Gestalt), während sie von dem Maler, Photographen, Zeichner abgebildet werden hinsichtlich ihrer Farbenerscheinung, resp. Licht- und Schattenparthieen oder Contouren, bildet der Arithmetiker die Einheiten nur noch hinsichtlich ihrer *Häufigkeit* ab. *Die natürliche Zahl ist eine Abbildung der Einheiten in Hinsicht ihrer Häufigkeit.*" (Schröder, *Lehrbuch der Artihmetik und Algebra*, pp. 5 sq.).

ccli. "Ein solches Zeichen ist mitunter sehr verschiedenartiger, mitunter gar keiner Bedeutung fähig oder doch theilhaftig. Man unterwirft

dasselbe willkürlich soviel Gesetzen als nothwendig und hinreichend sind zur Bestimmung der Eigenschaften desselben, um sodann durch Schlussfolgerungen diese übrigen Eigenschaften daraus abzuleiten." (Schröder, *Lehrbuch der Artihmetik und Algebra*, p. 3).

ccliii. The postcard is dated "7.11.1903"; it is item XV/9 in Frege's collected correspondence as catalogued by the editors of *Wissenschaftlicher Briefwechsel*. The relevant remark reads: "Ihren Kritiken kann ich im Allgemeinen zustimmen; nur dass Sie Dedekind u. vor Allem Cantor nicht voll gerecht werden." (Frege, *Wissenschaftlicher Briefwechsel*, p. 80).

ccliii. "Denn für unendlichviele Zahlen können wir nicht Zahlenzeichen hinschreiben oder Abkürzungen einführen; wir würden, sobald wir diese Schwierigkeit nicht bedenken, zu denjenigen Ungereimtheiten gelangen, die Frege in seinen kritischen Ausführungen über die hergebrachten Definitionen der Irrationalzahl mit Recht rügt." (David Hilbert. "Neubegründung der Mathematik. Erste Mitteilung." In: *Gesammelte Abhandlungen. Analysis, Grundlagen der Mathematik, Physik, Verschiedenes, nebst einer Lebensgeschichte.* Vol. 3. Springer Verlag, 1935, pp. 157–177, p. 165).

ccliv. "Auf der Suche nach dem 'Wesen' der ganzen Zahlen sind die lächerlichsten und naivsten Vorstellungen und Definitionen gegeben worden. Die Zahlen sind 'Zeichen'. Es wird Wert gelegt auf die Größe und den Abstand dieser 'Zeichen'. Überhaupt spielt das Aussehen eine große Rolle. Bei solchen Erörterungen über das Wesen der ganzen Zahlen kann man sich nicht über Gegensätze und Widersprüche wundern, die sich in der damaligen Philosophie zeigten. Die Versuche, sich von dem wahren Wesen der Zahlen Rechenschaft zu geben, mißlangen immer wieder.

(Siehe: Dedekind: 'Was sind u[nd] was sollen [die] Zahlen' und Frege: 'Grundgesetze der Arithmetik'.)" (Hilbert, "Ms. Cod. D. Hilbert 559," 3r-v).

cclv. "Für die mathematische Erkenntnis empfiehlt es sich, die Zeichen so zu wählen, dass sich an ihnen die Anzahl-Beziehungen gestaltlich darstellen. Auf diese Weise gewinnen wir die Möglichkeit, die Zahlentheorie als eine anschauliche Theorie gewisser einfacher Figuren zu entwickeln, welche wir Zahlzeichen nennen wollen. Der Zusammenhang zwischen diesen Zahlzeichen und den Anzahlen wird sich nachher auf ganz naturgemässe Art ergeben. Unsere Theorie handelt also von anschaulichen Figuren, von denen wir voraussetzen, dass sich ihre Gestalt unabhängig von Ort und Zeit und der besonderen Art ihrer Herstellung immer wiedererkennen lässt. Und zwar sollen unter den Zahlzeichen Figuren von folgender Beschaffenheit verstanden werden: Sie bestehen in einer Aufeinanderfolge der Zeichen '1' und '+'; sie fangen mit 1 an und endigen mit 1. Auf eine 1, sofern sie nicht am Ende steht, folgt +; und auf + folgt immer eine 1." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 463).

cclvi. So Hilbert says in footnote 1): "Noch einfacher wäre es, die Zahlzeichen zu erklären als Aufeinanderfolgen von Punkten. Dass wir hier die Zusammensetzung von 1 und + nehmen, geschieht nur, um uns enger an das Gewohnte anzuschliessen." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 463).

cclvii. Hilbert: "So soll 2 eine Abkürzung sein für das Zahlenzeichen 1+1+, 3 eine Abkürzung für das Zahlzeichen 1+1+1. Zu den aufgezählten Arten von Zeichen kommen nun noch die Zeichen für gewisse Zusammensetzungen von Zahlzeichen [...] Beispielsweise bedeutet $(1 + 1) + (1 + 1 + 1)$ | das Zahlzeichen, welches entsteht, wenn man hinter $1 + 1$ erst + und dann das Zahlzeichen $1 + 1 + 1$ setzt, also das Zeichen $1 + 1 + 1 + 1 + 1$;" (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, pp. 463 sq.)

Schröder: "Der gewöhnliche Name der oben als Exempel benutzten Zahl $1 + 1 + 1 + 1 + 1$ ist z.B. 'fünf' und sie pflegt durch das Zeichen 5 kürzer dargestellt zu werden." (Schröder, *Lehrbuch der Artihmetik und Algebra*, p. 22). "Sind die Glieder [einer Summe] in ihrer Urform, als Summen von Einern gegeben, wie z.B. $2 = 1 + 1$, $3 = 1 + 1 + 1$, so wird also nach dem Sinne und der Meinung der obigen Definition ihre Summe erhalten, indem man diese Reihen von Einern einfach durch das Zeichen + verknüpft aneinanderhängt; sie ist demnach: $1 + 1 + 1 + 1 + 1 = 5$." (Schröder, *Lehrbuch der Artihmetik und Algebra*, p. 53).

cclviii. "Um nun ein Zeichen zu erhalten, welches fähig ist auszudrücken, *wie viele* jener Einheiten vorhanden sind, richtet man die Aufmerk-

samkeit der Reihe nach *ein* mal auf eine jede derselben und bildet sie mit einem Strich: 1 (eine *Eins*, ein *Einer*) ab; diese Einer setzt man in eine Zeile nebeneinander, verbindet sie jedoch unter such durch das Zeichen + (*plus*), da sonst zum Beispiel 111 nach der gewöhnlichen Zahlenbezeichnung als einhundertundelf gelesen würde. Man erhält auf diese Weise ein Zeichen wie: $1+1+1+1+1$, dessen Zusammensetzung man dadurch beschreiben kann, dass man sagt: *Eine natürliche Zahl ist eine Summe von Einern.*" (Schröder, *Lehrbuch der Artihmetik und Algebra*, p. 5).

cclix. "Dass es überhaupt Dinge gebe, die in gleicher Anzahl vorhanden sind, wie z.B. die Einer der beiden Zahlen $1+1$ und $1+1$, sowie auch, dass es andere Dinge gebe, bei welchen dies nicht der Fall ist, muss allerdings als ein Axiom zugegeben werden." (Schröder, *Lehrbuch der Artihmetik und Algebra*, p. 9).

cclx. "Wie ich jetzt die Sache ansehe, so sind folgende *zwei Axiome* als Grundlage unserer endlichen Zahlentheorie *nothwendig* und *hinreichend*. I. Es giebt Dinge (d.h. Gegenstände unseres Denkens). (Ohne dieses Axiom würde der Begriff 1 nicht möglich sein. Es läßt sich ja eine dermaßen utrirte Skepsis denken, die selbst dieses Axiom verwirft.) II. Ist V eine *consistente* Vielheit von Dingen und d ein nicht in V als Theil enthaltenes Ding, so ist die Vielheit $V+d$ *auch consistent*. Diese *beiden Axiome* liefern mir die unbegrenzte Zahlenreihe $1, 2, 3, 4, \ldots$ der endlichen ganzen Cardinalzahlen und alle Gesetze unter ihnen lassen sich *beweisen*, *ohne Zuhülfenahme weiterer Axiome*." (Cantor, *Briefe*, p. 430).

cclxi. "Das gedachte Princip könnte wohl das *Axiom von der Inhärenz der Zeichen* genannt werden. Es gibt uns die Gewissheit, dass bei | allen unsern Entwicklungen und Schlussfolgerungen die Zeichen in unsrer Erinnerung – noch fest aber am Papiere – haften. Die Anzahl der Einer, die wir schreiben, ändert sich zum Beispiel nicht, während wir sie umstellen und versetzen; dieselben können weder wie Pilze aus dem Papier hervorwachsen, noch von selbst verschwinden. Es bleiben überhaupt die Buchstaben sich gleich, während wir mit ihnen operiren; ein a geht nie in ein b über; auch – bei der nöthigen Sorgfalt von unsrer Seite – kann kein Betrug mit unterlaufen, durch welchen uns ein x für ein u gemacht würde! Ohne diesen Grundsatz, den wir aus einer sehr reichhaltigen Erfahrung durch inductorische Ausdehnung oder Verallgemeinerung gewinnen, würde in der That jede Deduction illusorisch sein, denn die Deduction beginnt eben dann, wenn – nachdem die Grundeigenschaften der Dinge hinlänglich in Zeichen eingekleidet sind – das Studium dieser Dinge Platz macht dem ihrer Zeichen." (Schröder, *Lehrbuch der Artihmetik und Algebra*, pp. 16 sq.).

cclxii. The discussion of the foundations of arithmetic and geometry begins (and these are also the opening sentences of the lecture itself, as recorded in the official lecture-notes)David Hilbert. "Prinzipien der Mathematik. S[ommer-]S[emester] 1908." (16205y). MI Göttingen, 1908, p. 3. After the brief discussion Hilbert goes on on Hilbert, "Prinzipien der Mathematik," p. 6.

cclxiii. The passage I quoted in the previous endnote continues Hilbert, "Prinzipien der Mathematik," p. 6.

cclxiv. David Hilbert. "Elemente und Prinzipienfragen der Mathematik. Vorlesung gehalten im Sommer 1910, ausgearbeitet von R[ichard] Courant." (16206t). MI Göttingen, 1910, p. 2. A brief sketch of foundational issues in analysis and geometry follows. Then Hilbert returns to the programmatic level Hilbert, "Elemente und Prinzipienfragen der Mathematik," pp. 3 sq.

cclxv. After the passage referred to in the previous footnote: Hilbert, "Elemente und Prinzipienfragen der Mathematik," pp. 3 sq.

cclxvi. At the end of his exposition of the first presentation-topic (the so-called "Warring-Problem"), Hilbert says: "*Aufgabe des Vortrages ist es, die Beweise des Satzes für die behandelten Fälle so [struck out: 'zu'] darzustellen, dass ihre gemeinsamen Grundideen möglichst hervortreten.*" (David Hilbert. "Themata zu Vorträgen für das Seminar über Prinzipien der Mathematik im Wintersem[ester] 1908/09." Ms. Cod. M. Hilbert 570/2. SUB Göttingen, 1908-1909, p. 2), similarly after outlining the fourth topic (the distribution of prime numbers): "*Aufgabe des ersten Vortrags* [there are two possible presentations subordinate to topic 4] wäre es nun, *den Beweis des obigen Satzes in durchsichtiger Weise zur Darstellung zu bringen, wobei vor allem auf Herausarbeitung der wesentlichen Gedanken des*

Beweises unter möglichster Vermeidung komplizierter Rechnungen Wert zu legen ist." (Hilbert, "Themata zu Vorträgen für das Seminar über Prinzipien der Mathematik im Wintersem[ester] 1908/09," p. 7), and the 13th (to do with differential equation): "*Aufgabe des Vortrags* | soll es nun sein, *die wesentlichen Punkte der Beweismethoden mit möglichster Ausschaltung alles Beiwerkes herauszukehren, und zwar an irgend einem einfachen Falle* [...]" (Hilbert, "Themata zu Vorträgen für das Seminar über Prinzipien der Mathematik im Wintersem[ester] 1908/09," pp. 9 sq.).

cclxvii. In his preparatory remarks for the 1914/15 lecture, Hilbert wrote: "Wenn es uns nun obliegt, uns mit den Prinzipien der Math. in diesem Semester zu beschäftigen, so sind die Schwierigkeiten dabei 2erlei Art[:] 1.) sachliche, weil wir [insertion: trotz des elementaren Charakters der Fragen, die wir betrachten wollen] dabei auf manche auch heute | noch ungelöste Fragen treffen[;] 2.) pedagogische, weil das Verständniss der Prinzipien doch gewisse ausholende Kenntnisse [Hilbert initially wrote 'die [Kenntnisse]', then struck out 'die', writing 'gewisse' underneath, then struck that out writing 'gewisse tiefere' next to it, finally replaced 'tiefere' with 'ausholende'] der math. Wissenschaft voraussetzt. Es giebt einen Weg, auf welchem sich diese 2 Schwierigkeiten völlig vermeiden lassen und welchen ich einschlagen will d.i. an der Hand spezieller Probleme die prinzipiellen Gesichtspunkte hervortreten zu lassen [...]." (Hilbert, "Ms. Cod. D. Hilbert 559," 30r-v). His speaking of rendering certain "principal aspects" salient "by tracing specific problems" echoes similar formulations from the 1908 lectures (Hilbert, "Prinzipien der Mathematik," p. 9).

cclxviii. Klein characterises the aim of his lectures as follows: "Ich wende mich damit [i.e. with the lectures] keinesfalls an den Anfänger, sondern setze voraus, daß Ihnen allen der Hauptinhalt der | wichtigsten mathematischen Disziplinen bereits bekannt ist. Ich werde vielfach von Problemen der Algebra, der Zahlentheorie, der Funktionentheorie usw. zu reden haben, ohne auf Einzelheiten eingehen zu können; Sie müssen diese Dinge also schon einigermaßen kennen, wenn sie meinen Auseinandersetzungen folgen wollen. Meine Aufgabe hier wird stets sein, Ihnen den *gegenseitigen Zusammenhang der Fragen der Einzeldisziplinen* vorzuführen, der in den Spezialvorlesungen nicht genügend zur Geltung kommt, sowie insbesondere ihre *Beziehungen zu den Fragen der Schulmathematik* zu betonen. Dadurch, so hoffe ich, wird es Ihnen erleichtert werden, sich diejenige Fähigkeit anzueignen, die ich doch als eigentliches Ziel ihres akademischen Studiums bezeichnen möchte: *daß Sie dem großen Wissensstoff, der Ihnen hier zukommt, einst in reichem Maße lebendige Anregungen für Ihren eigenen Unterricht entnehmen können.*" (Klein, *Elementarmathematik vom höheren Standpunkte aus*, p. 2).

cclxix. "Ich habe zum ersten Male vor drei Jahren eine *Vorlesung mit ähnlichem Ziele* gehalten; mein damaliger Assistent, Herr *R. Schimmack*, hat den Gegenstand weiter durchgearbeitet und kürzlich ist ein erster Teil jener Vorlesung im Druck erschienen." Assuming this remark dates from the first edition of Klein's lectures in 1908, the first lecture on the subject Klein mentions here would have taken place in 1905; that the reference-point for this remark is 1908 is also suggested by a footnote referencing the work edited by Schimmack, which was published "not long ago" in 1907.

cclxx. David Hilbert. "Quadratur des Kreises. Vorlesungen im Sommer [?]-Semester 1904, ausgearbeitet von Max Born, cand. math." MI Göttingen, 1904, See the separate sheet bound into the volume on p. 105.

cclxxi. Hilbert, "Prinzipien der Mathematik," p. 8.

cclxxii. The following passage is a direct expansion of the added remark in the 1908 notes Hilbert, "Elemente und Prinzipienfragen der Mathematik," p. 1.

cclxxiii. "Den systematischen Aufbau der Grundlagen der Math. darzulegen behalte ich dann einer späteren Vorlesung vor." (Hilbert, "Ms. Cod. D. Hilbert 559," p. 30v).

cclxxiv. One finds such declarations in the sketches for the opening remarks of both the 1924/25 and the 1928/29 iteration of the lecture course on the quadrature of the circle, which, as I mentioned before (in footnote 201), have been preserved in the official lecture notes for the

1904 iteration. Hilbert, "Quadratur des Kreises," See the separate sheet bound into the volume on p. 105.

cclxxv. "Es entsteht dann die notwendige Aufgabe, die *Widerspruchsfreiheit* und *Vollständigkeit* dieser Axiome zu zeigen, d.h. es muß bewiesen werden, daß die Anwendung der aufgestellten Axiome nie zu Widersprüchen führen kann, und ferne, daß das System der Axiome zum Nachweis aller geometrischen Sätze ausreicht." (David Hilbert. *Grundlagen der Geometrie*. 7th ed. Teubner, 1930, p. 242).

cclxxvi. Hilbert, "Logische Prinzipien des Mathematischen Denkens," p. 36.

cclxxvii. This is on p. 147 of the typed lecture-notes deposited in the library of the Mathematische Institut.

cclxxviii. See p. 148f. of the typed lecture-notes deposited in the library of the Mathematische InstitutHilbert mentions there (on p. 145f.) the idea that the consistency of arithmetic does not admit of any proof.

cclxxix. "Mit Hilfe dieser Axiome kann man nun die ganze euklidische Geometrie auf|bauen. Davon werden wir aber nicht weiter reden; Sie finden das in meinem Buche über die Grundlagen der Geometrie." (Hilbert, "Probleme und Prinzipienfragen der Mathematik," pp. 131 sq.).

cclxxx. "Eine zweite Frage ist die nach der Widerspruchslosigkeit; das ist i[m] A[llgemeinen] bei allen axiomatischen Untersuchungen der schwierigste Punkt. Drittens ist aber der Punkt zu erwähnen, der am besten die Fruchtbarkeit axiomatischer Untersuchungen ins Licht stellt, und auf den ich vor allem eingehen will; der ist die Frage nach der Unabhängigkeit der Axiome." (Hilbert, "Probleme und Prinzipienfragen der Mathematik," p. 132).

cclxxxi. "Damit gewinnen wir folgenden Satz, der als das Endziel der gesamten Entwicklungen dieses Kapitels V anzusehen ist: Satz 35. *Es seien in einer ebenen Geomtrie die Axiome I 1-2, II, III erfüllt: dann ist die Gültigkeit des Desarguesschen Satzes die notwendige und hinreichende Bedingung dafür, dass diese ebene Geometrie sich auffassen lässt als ein Teil einer rämulichen Geometrie, in welcher die sämtlichen Axiome I, II, III erfüllt sind.* Der Desarguessche Satz kennzeichnet sich so gewissermassen für die ebene Geometrie als das Resultat der Elimination der räumlichen Axiome." (Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*, p. 505). A remark like this occurs already in the notes for the 1989/99 lecture course (see: Hilbert, *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*, p. 261).

cclxxxii. The section is entitled "Nachweis, dass die axiomatisch postulierten ganzen Zahlen auch die uns bekannten Zahlen sind." and begins on p. 149.

cclxxxiii. "Die Geometrie dient als Vorbild für [struck out: 'alle übrigen' die Axiomatisierung aller übrigen Disciplinen und zwar ist es immer so dass [added above the line: 'das'] Ziel der Beweis des im Mittelpunkt der Disziplin stehenden Satzes ist: wie in der Geometrie der Satz, dass die Gerade durch eine lineare Gl[eichung] geg[eben] und die Drehung durch orthogonale Transformation der Koordinaten. Die axiomatischen Untersuchungen in der Geometrie laufen stets darauf hinaus, Axiome | d.h einfache, plausible, durchsichtige, Einzel-Annahmen zu machen, auf Grund derer dann jener Satz bewiesen wird: also gewisserma[s]sen eine logische Zerlegung jenes Satzes in seine Bestandteile." (Hilbert, "Ms. Cod. D. Hilbert 559," 30v-31r).

cclxxxiv. "Wenn wir eine bestimmte Theorie näher betrachten, so erkennen wir allemal, daß die Konstruktion des Fachwerks von Begriffen einige wenige ausgezeichnete Sätze zugrunde liegen und dass diese dann allein ausreichen, um aus ihnen nach logischen Prinzipien das ganze Fachwerk aufzubauen. So genügt in der Geometrie der Satz [singular!] von der Linearität der Gleichung der Ebene und von der orthogonalen Transformation der Punktkoordinaten vollständig, um die ausgedehnte Wissenschaft der Euklidischen Raumgeometrie allein durch die Mittel der Analysis zu gewinnen." (David Hilbert. "Axiomatisches Denken." In: *Gesammelte Abhandlungen. Analysis, Grundlagen der Mathematik, Physik, Verschiedenes, nebst einer Lebensgeschichte*. Vol. 3. Springer Verlag, 1918, pp. 146–156, p. 147).

cclxxxv. "Man kann in jeder Wissenschaft verschiedene Axiomschichten unterscheiden, z.B. meine früheren Grundlagen der Geometrie, und die jetzige Rückkehr auf den Punkt als alleiniges Element. Der Physiker nimmt einfach alle Thatsachen als ein Axiomensystem hin, das ist die leidlich erste und gröbste Schicht; Jeder ist ein Arbeiter in Axiomen, nur in verschiedenen Schichten." (Wissenschaftliches Tagebuch III, p. 39)

cclxxxvi. "Jede tiefere Axiomschicht bedeutet eine abstraktere philosophischere Erkenntniss, jede höhere Axiomschicht ein Fortschritt nach der positiven Seite hin – wie die Baumringe. Die einzelnen Axiomschichten sind zugleich die wahren Ruhepunkte u. Stationen.

Die Axiomatische Methode ist die Methode der Zukunft. Meine Geometrie als Beispiel nehmen. Zähle auf, was dabei Alles zur Sprache kommt und wie es neu und übersichtlich geordnet erscheint. Meine Lebensaufgabe, die getrennt überlieferten Wissens|zweige vereinigen (innerlich durch die Axiomatische Methode, nicht blos äusserlich wie Klein)." (Wissenschaftliches Tagebuch III, pp. 113f.)

cclxxxvii. In a footnote to the text of the 1921 lectures, Hilbert remarks: "Man beachte den Unterschied zwischen dieser Forderung der Vollständigkeit und derjenigen, welche in dem Vollständigkeitsaxiom ausgesprochen wird. Das Vollständigkeitsaxiom besagt: es können ohne Widerspruch keine neuen Gegenstände hinzugefügt werden: die Forderung der Vollständigkeit des Axiomensystems aber lautet: es können ohne Widerspruch keine neuen Sätze hinzugefügt werden." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 443).

cclxxxviii. "Es sei ausdrücklich bemerkt, daß Satz XI (...) in keinem Widerspruch zum Hilbertschen formalistischen Standpunkt stehen. Denn dieser setzt nur die Existenz eines mit finiten Mitteln geführten Widerspruchsfreiheitsbeweises voraus und es wäre denkbar, daß es finite Beweise gibt, die sich in P (...) *nicht* darstellen lassen." (Kurt Gödel. "Über formal unentscheidbare Sätze der 'Principia Mathematica' und verwandter Systeme I." in: *Monatshefte für Mathematik und Physik* 38 [1931], pp. 173–198, p. 197).

cclxxxix. "Ces principes [Poincaré is duscussing Russell's fundamental propositions of logic, but, again, I think, making a general point] pourraient-ils être considérés comme des définitions déguisées? pour cela il faudrait que l'on eût le moyen de démontrer qu'ils n'impliquent pas contradiction. Il faudrait établir que, quelque loin qu'on poursuive la série des déductions, on ne sera jamais exposé à se contredire. Sans doute on verrait facilement qu'une opération nouvelle ne peut introduire de contradiction s'il ne s'en est pas produit aux étabes précédentes. Mais conclure de là qu'il n'y en aura jamais, ce serait faire de l'induction complète; et *le principle d'induction complète, rappelons-le bien, nous ne le connaisons pas encore.* Nous n'avons donc pas le droit de regarder ces axiomes comme des définition déguisées et il ne nous reste qu'une ressource, il faut pour chacun d'eux admettre un nouvel acte d'intuition." Henri Poincaré. "Les mathématiques et la logique [I]." in: *Revue de métaphysique et de morale* 13 (1905), pp. 815–835, p. 829

ccxc. "Um dieser insofern vorläufigen Darstellung nichtsdestoweniger eine gewisse innere Geschlossenheit zu geben, möchte ich ihr gewissermaßen als Leitgedanken die Aufgabe zuweisen, zu der Lösung des folgenden logischen Problems beizutragen, das ich geradezu als das *Hauptproblem der symbolischen Logik* bezeichnen möchte: *Es soll eine ganz bestimmte allgemeine Vorschrift angegeben werden, die über die Richtigkeit oder Falschheit einer beliebig vorgelegten mit rein logischen Mitteln darstellbaren Behauptung nach einer endlichen Anzahl von Schritten zu entscheiden gestattet, oder zum mindesten dieses Ziel innerhalb derjenigen – genau festzulegenden – Grenzen verwirklicht werden, innerhalb deren seine Verwirklichung tatsächlich möglich ist.* Wenn auch heute selbstverständlich noch nicht im entferntesten daran gedacht werden kann, dieses Problem, dessen Auflösung die ganze Mathematik in eine ungeheure Trivialität verwandeln würde, in seiner vollen | Allgemeinheit in Angriff zu nehmen, so verlohnt es sich, wie ich glaube, dennoch – und eben dies soll im folgenden geschehen –, es wenigstens für einen passend gewählten bescheideneren Bereich von Aussagen vollständig durchzuführen [...]." (Behmann, "Beiträge zur Algebra der Logik, insbesondere zum Entscheidungsproblem," pp. 166 sq.).

ccxci. The section is entitled "Kurze Besprechung zweier Fragen, die wesentlich axiomatischer Natur sind". Behmann writes there: "Es sind namentlich in neuerer Zeit gewisse mathematische Fragen hervorgetreten, bei denen wir nicht nur nicht wissen, welchen Weg wir zu

ihrer Entscheidung zu wählen haben, sondern nicht einmal dies, ob sie aufgrund der uns bekannten Axiome überhaupt entschieden werden können, und zu denen wir daher schwer eine befriedigenden Standpunkt gewinnen können. Und zwar handelt es sich hier namentlich um Fragen wie diese, ob, wenn irgend eine bestimmte reelle Zahl wie z.B. e^{π} vorgelegt ist, es notwendig entscheidbar ist, ob sie rational oder irrational bez. algebraisch oder transzendent ist. (Ganz von selbst gelangen wir von hier aus auf die Frage, ob das zugrunde liegende Axiomensystem die Eigenschaft der Vollständigkeit hat, d.h. ob jede vernünftig gestellte Frage einer Beantwortung in dem gemeinten Sinne fähig ist.)" (Heinrich Behmann. *Die Antinomie der transfiniten Zahl und ihre Auflösung durch die Theorie von Russell und Whitehead*. Inauguraldissertation (Tag der mündlichen Prüfung: 5. Juni 1918). 1918. SB Berlin, signature 50/1918; alternative: 03/4346, p. 324). He goes on to point to Cantor's continuum-problem as an example of such a question and remarks: "Solange aber weder dies Letzte [i.e. that the existence of a function mapping real numbers to a certain section of ordinal numbers] bewiesen noch eine passende Funktion aufgezeigt werden kann, muss die Frage natürlich als unentschieden gelten. Vielleicht ist es sogar möglich, die Unentscheidbarkeit ihrerseits zu beweisen oder gar die Unentscheidbarkeit der Entscheidbarkeitsfrage. Hinsichtlich der grundsätzlichen Möglichkeit derartiger Fälle Klarheit zu schaffen, erkennt man schon allein hieraus als eine dringende Aufgabe." (Behmann, *Die Antinomie der transfiniten Zahl und ihre Auflösung durch die Theorie von Russell und Whitehead*, p. 325); Mancosu likewise sees in these passages "a statement of what will become the general 'Entscheidungsproblem'" (Paolo Mancosu. "The Russellian Influence on Hilbert and His School." In: *The Adventure of Reason. Interplay between Philosophy of Mathematics and Mathematical Logic 1900-1940*. Oxford University Press, 2010, pp. 176–198, p. 196)

ccxcii. "Ein wichtiger Vorzug der f-Begriffe [i.e. the concept Carnap calls 'Folge' and others defined in terms of it] vor den a-Begriffen [i.e. the concept Carnap calls 'Ableitung' and others defined in terms of it] liegt z.B. darin, daß mit ihrer Hilfe die vollständige Zweiteilung der $\mathfrak{S}_{\mathfrak{l}}$ [i.e. the sentences of the language] in analytische und kontradiktorische [i.e. truths and falsehoods] möglich ist, während die entsprechende Einteilung in beweisbare und widerlegbare $\mathfrak{S}_{\mathfrak{l}}$ i[m] a[llgemeinen] unvollständig ist." (Carnap, *Logische Syntax der Sprache*, p. 126).

ccxciii. "Die Folgebeziehung hat einen weiteren Umfang als die Ableitbarkeitsbeziehung. [...] Denn diese Bestimmung [i.e. the rules of inference by which deducibility in a deductive system is usually defined] beziehen sich stets auf endlich viele Prämissen, | da ja eine Ableitung eine endliche Reihe von Sätzen sein soll; [Folgerung] aber bezieht sich unter Umständen auf unendliche Satzklassen." (Carnap, *Logische Syntax der Sprache*, pp. 35 sq.).

ccxciv. "Nicht, wie Weierstrass meint, die Ordnung einer ganzen Funktion in den (homogenen) Variablen ist etwas zu unbedeutendes, um darauf zu Theorien zu gründen. Vielmehr ist die Ordnung das einzig tiefe, nur muss man nicht wenigstens nicht in höherem als binärem Gebiete an der einen speziellen Ordnung haften bleiben sondern alle einschlägigen Systeme von Ordnungszahlen betrachten. Dann wird man nothwendig auf das Geschlecht p geführt, aber man erhält mehr und tieferes." (Wissenschaftliches Tagebuch I, p. 12).

ccxcv. The report of Hilbert's encounter with Weierstrass found on page 6^v in Cod. Ms. D. Hilbert 741:1 read (in its entirety): "*Weierstrass*

Ich erzählte, dass Kronecker nichts von 'litteraler Invariantentheorie'" halte und meine, derselben werde es wie, wie der Combinatorik, ergehen. W. sagte: Auch aus der combinatorischen Schule sei nicht alles untergegangen und untergehen werde auch vieles von der Invariantentheorie, aber nicht von ihr allein. Denn aus allem müsse sich erst allmählig das wesentliche herauskrystallisiren und es ist nicht unsere Möglichkeit auch nicht unsere Aufgabe zu entscheiden, was wichtig ist. Es darf nur nicht abhalten, nur auch mit solchen (invariantentheoretischen) Fragen zu beschäftigen und sich in dieselbenz u vertiefen. Denn solche Untersuchungen z.B. über die Endlichkeit werden stets in der Wissenschaft ihren Platz bewahren.

Er meint, dass möglicherweise auch Curven mit reellen Zügen sich werden mittelst imaginärer Transformation in solche mit mehr oder weniger reellen Zügen verwenden lassen. Der Grad d.h. Ordnung einer Curve sei etwas zu unbedeutendes, um ihn bei Realitätsfragen zu Grunde zu legen. Die Curven gleichen Geschlechtes stehen sich näher als die gleicher Ordnung.

ccxcvi. "Frege hat die Begründung der Zahlenlehre auf reine Logik, Dedekind der Mengenlehre als Kapitel der reinen Logik versucht: beide

haben ihr Ziel nicht erreicht. Frege hatte die gewohnten Begriffsbildungen der Logik in ihrer Anwendung auf Mathematik nicht vorsichtig genug gehandhabt: so hielt er den Umfang eines Begriffs für etwas ohne weitere Gegebenes, derart, daß er dann diese Umfänge uneingeschränkt wieder als Dinge selbst nehmen zu dürfen glaubte. Er verfiel so gewissermaßen einem extremen Begriffsrealismus. Ähnlich erging es Dedekind; [...] Die scharfsinnigen Untersuchungen von Frege und Dedekind haben trotzdem wertvolle Früchte gezeitigt; Frege und Dedekind haben die moderne Kritik der Analysis inauguriert, und diese, getragen von Männern wie Cantor, Zermelo und Russell 'mündet' nicht, wie Weyl behauptet, 'im Chaos und Leersinn': vielmehr verdanken wir ihr einmal tiefgehende auf axiomatischer Grundlage ruhende Theorien – insbesondere die von Zermelo und Russell – und andererseits die sachgemäße Entwicklung des sogenannten Logikkalküls, dessen Grundideen sich immer mehr und mehr als unentbehrliches Hilfsmittel bei logisch-mathematischen Untersuchungen herausstellen." (Hilbert, "Neubegründung der Mathematik," p. 162).

ccxcvii. "Dedekinds Grundgedanke und seine Endabsicht haben viel Bestechendes: es stände die Zahlentheorie als absolut voraussetzungslose Wissenschaft da! Er fand auch starke Stützen z. B. in dem Philosophen Frege, der seinerseits sogar früher als Dedekind ähnliche Bestrebungen betätigt hatte. So wurde durch die Zusammenarbeit von Frege, Dedekind, Cantor das Unendliche auf den Thron erhoben und genoss damals seinen höchsten Triumph. Es sollte der Urquell der so weitreichenden Mathematik und damit der Wissenschaft überhaupt bilden. Aber es konnte sich auf dieser Höhe, in dieser selbständigen Stellung ohne die Stütze der Anschauung ohne jede empirische Hilfe nicht halten. Es erfolgte ein jäher Sturz." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 739). Hilbert goes on to say: "Das Bekanntwerden der Paradoxien wirkte katastrophal. [...] Dedekind und Frege [...] gaben tatsächlich den Kampf für ihren Standpunkt auf und erklärten sich als des Irrtums überführt." (p. 741), where Hilbert attributes to Dedekind and Frege a common, shared "standpoint". Similar remarks are also found in the shorter article (see the re-printing of it as an appendix in Hilbert, *Grundlagen der Geometrie*, p. 273).

ccxcviii. "Und das Mißlingen des Fregeschen Unternehmens hat mehr noch als Freges Dialektik das Problematische an der Annahme der Totalität der Zahlenreihe zum Bewußtsein gebracht." (Hilbert and Bernays, *Grundlagen der Mathematik*, p. 14).

ccxcix. "Meine Meinung ist eben die, dass ein Begriff nur durch seine Beziehungen zu anderen Begriffen logisch festgelegt werden kann. Diese Beziehungen, in bestimmten Aussagen formuliert, nenne ich Axiome und komme so dazu dass die Axiome ev. nach den Namensgebungen die Definitionen der Begriffe sind. Diese Auffassung habe ich mir nicht etwa zur Kurzweil ausgedacht, sondern ich sah mich zu derselben gedrängt durch die Forderung der Strenge beim logischen Schliessen und beim logischen Aufbau einer Theorie. Ich bin zu der Ueberzeugung gekommen, dass man in der Math. u den Naturwissenschaften subtilere Dinge nur so mit Strenge behandeln kann, andernfalls sich blos im Kreise dreht." (Wissenschaftliches Tagebuch III, p. 75f.)

ccc. "Freges 'Ellaj baset mindestens 2 Technizek' beweist nur Freges Fähigkeit sinnlose Sätze zu bilden, aber nichts gegen meine Theorie. Denn meine Sätze sind stets von der Form '... ist zugeordnet ...', wo der Begriff ist zugeordnet als bekannt und vor aller Logik festgesetzt angenommen wird und auch angenommen werden muss. Denn die Fähigkeit unseres Geistes Dinge auf Dinge zu beziehen muss allerdings vorweg angenommen werden. Frege verräth dadurch, dass er Sinn und Zweck meiner Begründung ganz missverstanden hat. Natürlicher Weise kann man statt 'Punkt', 'Gerade', 'Ebene', 'zwischen' etc. andere Worte sagen, was aber nichts Neues ist. Dass 'liegen auf' 'gehen durch' 'enthalten' nichts anderes als das obige 'zugeordnet sein' bedeuten soll, habe ich zum Ueberfluss noch ausdrücklich bemerkt. Ich könnte vielleicht | später einmal bei einer Gelegenheit unter dem Text folgendes bemerken: Das Beispiel [struck out: 'Diese Buchstabengruppierung'] Freges 'Anej bazet mindestens 2 Tezhnizek' beweist nur Freges Fähigkeiten sinnlose Sätze zu bilden, nichts aber gegen meinen Aufbau der Geometrie: Denn jene Fregesche Silbengruppierung hat nicht die Form meiner Aussagen '... ist zugeordnet ...'. Meine Auffassung deckt sich genau mit derjenigen von Hertz (Einleitung seiner Mechanik). Was ich Gedankendinge nenne sind die 'Bilder' von Hertz 'die Zeichen' von Pringsheim, Thomae etc...

'Freges Distinktionen zwischen 'Axiomen' und 'Definitionen' sind leeres Stroh!'"(Wissenschaftliches Tagebuch III, p. 75f.)

ccci. "Verfolgt man genau, was wir bei dem Zählen der Menge oder Anzahl von Dingen thun, so wird man auf die Betrachtung der Fähigkeit des Geistes geführt, Dinge auf Dinge zu beziehen, einem Dinge ein Ding entsprechen zu lassen, oder ein Ding durch ein Ding abzubilden, ohne welche Fähigkeit überhaupt kein | Denken möglich ist. Auf dieser einzigen, auch sonst ganz unentbehrlichen Grundlage muß nach meiner Ansicht, wie ich auch schon bei einer Ankündigung der vorliegenden Schrift ausgesprochen habe die gesammte Wissenschaft der Zahlen errichtet werden." (Richard Dedekind. *Was sind und was sollen die Zahlen — Stetigkeit und Irrationale Zahlen.* Springer, 1965, pp. III sq.).

cccii. "Jedes mathem. Problem kann auf folgende Frage zurückgeführt werden:

Gegeben ist ein Gesetz, vermöge dessen zu jeder vorgelegten nur aus Null und 1 bestehenden nichtabbrechenden Reihe

(α) 0 0 1 1 0 0 . . .

eine bestimmte andere solche Reihe (durch Rechenoperationen: Grösste Ganze suchen, Teiler suchen, etc. nicht Würfeln…)

(β) 1 0 0 1 1 1 . . .

construirt werden kann. Man soll durch eine endliche Zahl von Operationen entscheiden ob in irgend einer Reihe (β) eine 0 vorkommt oder ob alle Reihen (β) nur Einsen bestehen [sic]?

Ich behaupte: Jede solche Entscheidung ist durch eine endliche Zahl von Operationen (Rechenoperationen) möglich. d.h. Es giebt kein Gesetz, bei welchem die Entscheidung nicht durch eine endliche Zahl von Operationen möglich wäre. d.h. Jedes math. Problem ist lösbar. Alles dem menschlichen Verstande erreichbare (durch reines Denken ohne Materie) ist auch aufzulösen. Es giebt nur ein Problem. (z.B. Quadratur des Kreises [one unreadable word] $\pi = 3,14\ldots$ 10 aufeinander folgende 7en etc.)

Von der Annahme der Möglichkeit geht man von vorneherein aus." (Wissenschaftliches Tagebuch, I p. 37).

ccciii. "Jeder Satz in der Mathematik kann positiv gefasst und als Existenzsatz von folgender Gestalt ausgesprochen werden: Zu einem System von Zahlen von der und der Eigenschaft lässt sich ein System von Zahlen von der und der Eigenschaft zuordnen. Das erste System repräsentiert das Gegebene, Constante, das letztere System repräsentiert das Gesucht, Veränderliche, Beliebige. Die Eigenschaften drücken sich wieder durch solche Zuordnungen aus, und so steigt der Ganze Bau auf." (Wissenschaftliches Tagebuch II/f, p. 14)

ccciv. "Der Übergang zu den weiteren System kann entweder auf konstruktivem Wege geschehen, indem die neuen Elemente durch mathematische Konstruktion aus den alten abgeleitet werden, oder auf axiomatischem Wege, indem das neue System durch Relationseigenschaften charakterisiert wird. In dem zweiten Falle bedarf is noch des Nachweises, daß die Annahme eines Systems von der verlangten Beschaffenheit keinen Widerspruch in sich schließt." (David Hilbert. *Natur und mathematisches Erkennen. Vorlesungen, gehalten 1919-1920 in Göttingen.* Ed. by David E. Rowe. Birkhäuser, 1992, p. 91).

cccv. "Die Math. ist vollkommen überall, wo der Indexixer nicht hinkommt mit seiner Qual." (Wissenschaftliches Tagebuch III/b, p. 119) This entry follows immediately after the extract from the postcard to Frege.

cccvi. "Antwort auf Studys Brief vom 27/6.91. Grade durch diese flache Art zu phantasiren wie sie durch die Schule von Clebsch eingeführt, wird die tiefere mathematische Erfindungskraft gelähmt." (Wissenschaftliches Tagebuch I, p. 80) Before the mention of the "Schule von Clebsch", the letters "Kleb" have been written, but struck out, apparently a misspelling of Clebsch' name, since before "Clebsch" another struck-out "k" appears. Since this entry is in Käthe Hilbert's handwriting, Hilbert may have dictated it to Käthe, who misspelled the name while hurrying to write down the dictation.

cccvii. "Ich komme nun zu dem zweiten Gesichtspunkt, unter welchem die landläufige Auffassung von der Mathematik einer Korrektur bedarf. Man sagt da, in der Mathematik werden alle Sätze beweisen. Danach würde also die Mathematik eine gänzlich voraussetzungslose

Wissenschaft sein, in der es keinerlei prinzipielle Schwierigkeiten gibt. [...] Wann man sich das näher überlegt, dann sieht man gleich, daß diese Meinung jedenfalls nicht so ohne weiteres zutreffen kann. Denn aus nichts kann auch nichts beweisen werden. Es muß also bei dem logischen Schließen ein Rest von Annahmen übrig bleiben; es müssen Voraussetzungen da sein, auf die wir uns bei den Beweisen stützen." (Hilbert, *Natur und mathematisches Erkennen*, p. 15).

cccviii. "Durch die Clebsch-Jordansche Schule mit ihrem wüsten Formalismus und ihrer öden ixerei läuft die Mathematik – die erhabenste Wissenschaft – Gefahr – sie wird in den Augen aller denkenden Menschen miscreditirt und sinkt herabzu einer gedankenlosen Spielerei [footnote: welche sie dann dem Staat nicht noch – wie etwa Zauberquadrate etc. zur Unterhaltung der Gesellschaft beiträgt – und zur Entwicklung des menschlichen Geistes nicht das mindeste beiträgt.]. Was würde Gauss dazu sagen. Auch liegt es auf der Hand, dass durch ein so äusserliches Hilfsmittel, wie es die Symbolik ist, nichts erreicht werden kann. Man muss eben Ideen hineintragen. Aus Nichts wird Nichts." (Wissenschaftliches Tagebuch I, p. 65)

cccix. "Zu Gordan sagen: Wenn Sie kommen, so weiss ich entweder, es ist Prof. Gordan, oder es | ist nicht Prof. Gordan. Sie aber würden sagen: oder drittens. Die Kommende Person ist keins von Beiden.

Von meinen Invariantentheoretischen Sätzen folgendes sagen: 1.) Die Symbolische Rechnung und $x$erei in der Invariantentheorie stets zu meinen Sätzen in demselben Verhältniss wie die Jakobische Rechnerei in der Theorie der elliptischen Funktionen zu der nachfolg. Theorie von Weierstrass oder von Riemann wo durch die Funktion durch die Nullstellen bestimmt wird.

2) Die Symbolik hat, so kann man mit einiger Übertreibung sagen der Invariantentheorie geschadet, da meine Sätze sonst vielleicht schon früher entdeckt wären, wie die Erfindung des Luftballons dem Fortschritt in der Beschreibung eine Flugmaschine zu erfinden geschadet hat." (Wissenschaftliches Tagebuch I, p. 76f.)

cccxi. "Jakobi u[nd] Kummer waren die grössten Rechner und — werden es ewig bleiben." (Wissenschaftliches Tagebuch II, p. 95)

cccxii. "Die Kroneckersche Math. ist als zu einem Theil ein öder, äusserlicher Formelkram, und zum andern Theil unbestimmte Gefühlsduselei." (Wissenschaftliches Tagebuch I, p. 78)

cccxiii. "Die Originalarbeiten Galois über die Gleichungen verhalten sich zu den neueren Darstellungen von Kronecker Netto und Kneser, wie Homer zum Homerlexikon: Die klaren, einfachen, natürlichen, dem Kinde verständlichen Gedanken Galois sind mühsam ausgezogen – verstopft und formal verschmiert." (Wissenschaftliches Tagebuch I, p. 62)

cccxiv. "Wie wenig der formale, analytische Ausdruck vorstellt, lehrt das Beispiel der Dirichletschen Reihe, die man in endlicher geschlossener Form summiren kann, und mit deren Hilfe man nicht einmal entscheiden kann ob er gleich 0 ist." (Wissenschaftliches Tagebuch III/f, p. 38).

cccxv. "Die Formel (Rechnung) ist entweder als Beispiel oder Hilfsmittel (z.B. Theorie der Potenzreihen) berechtigt, nie aber ist sie Merkmal eines Fortschrittes oder Endzweck. Wie über der Ausbildung der Mittel nie der Zweck so das eigentliche Ziel, so darf über der Formelreihung nie der dazu leitende und treibende Gedanke vergessen werden." (Wissenschaftliches Tagebuch I, p. 36)

cccxvi. "Die Formel muss unser Knecht sein, aber nicht wir die Knechte der Formeln! —" (Wissenschaftliches Tagebuch II/b, p. 97)

cccxvii. "Begriff der Gleichheit ist nur relativ gleich. Gleich ist was unter denselben Begriff fällt, z.B. $2 = 3$, wenn der Begriff in Bezug auf den die Gleichung gemeint ist der der ganzen Zahl ist. $2 = 4$, $5 = 9$, im Sinne der Congruenz nach 2 oder 4. Der Mathematiker wendet statt gleich auch verschiedene Worte congruent, equivalent, inhaltsgleich, längengleich, absolut gleich etc. an. Umgekehrt kann man sagen $2 \neq 2$. Es sind das keine Sophismen, sondern triviale Wahrheiten, welche Jeder von uns anwendet, sobald er denkt, welche nur dem neu sein werden, welcher nie denkt. Wo das $x$en anfängt, hört das Denken auf. Wir müssen immer uns erinnern, dass die Buchstaben

Begriffe bedeuten und die Zeichen die Verknüpfung der Begriffe andeuten." (Wissenschaftliches Tagebuch II/b, p. 103)

cccxviii. "Die Formel kann das Denken (den Gedanken) nur beschreiben, nicht ersetzen, oder ersparen: Maschienenfabrikat — Handarbeit." (Wissenschaftliches Tagebuch I, p. 34)

cccxix. Hilbert's report of his encounter with Thomae in his travel-journal (Cod. Ms. D. Hilbert 741:1, p. 12^v) reads (in full) "*Thomae*

Mit der von Hesse angegeben C_{14}, welchen die 28.2 Berührungspunkte der Doppeltangente einer C_4 ausschneidet kann man nichts machen. Er stellt das Princip auf: Nicht etwa die endliche Zahl der Operationen wird verlangt und betont. Sondern man muss alles hinschreiben können und dann einsetzen und damit rechnen können. Er will das Denken erspart haben. Schreibt über Kunstgriff und Princip am Beispiel der linearen Transformation. Er will nichts von [one word unreadable] (pä) wissen. Pilz schreibe ein Buch über das Dreikörperproblem. Die Maße des 2^{ten} Körper sei unendlichklein von der ersten, die des 3 Körpers unendlichklein von der zweiten Ordnung. Er sei unendlich unwahrscheinlich, dass dabei kein Körper ins Unendliche gehe (Stabilität)[.] Es gebe ∞ viele Primzahlen von der Different 2.".

cccxx. "Oftmal hört man über die Mathematik klagen von denen, die sie anwenden wollen. Man verlangt eben, dass die Mathematik das Denken ersparen oder | überflüssig machen soll; gerade im Gegenteil die Math. lehrt und leitet an, überall mehr zu denken, als für den augenblicklich anstrebten praktischen Zweck nöthig ist." (Wissenschaftliches Tagebuch II/b, p. 92f.)

cccxxi. "Es ist gut auch als Gegengewicht gegen den Betrieb der Mathematik zum Zweck der Anwendungen, dass sie sich auch an die Philosophie anlehnt und mit den Principien der Logik sich befasst. Es ist geradezu eine Lebensfrage für die Math.: entweder sie wird über beides Logik und Naturwissenschaften herrschen oder sie wird gar nicht herrschen." (Wissenschaftliches Tagebuch III/b, p. 100)

cccxxii. "Klein will die Math. zur Herrscherin in der Welt der Wirklichkeit, ich in der der Ideale machen." (Wissenschaftliches Tagebuch III/b, p. 115)

cccxxiii. "Polemisches gegen F. K.'s Richtung in der Math. und Wiss. überhaupt.

Für Minister: wir dürfen uns von Ausland d.h. Frankreich und Italien nicht überflügeln lassen in rein wissenschaftlicher Hinsicht und nicht zu dem Stande der Engländer u. Amerikaner herabsinken." (Wissenschaftliches Tagebuch III/b, 127)

cccxxiv. "F.K. soll nun die Differentialgl., die die Technik bietet d.h. selbst darin etwas Ordentliches machen; es genügt nicht der billige Hinweis auf Methoden, die nur der Idee existiren und sich erst bewähren sollen. (Vgl. Fritz Kötter gegen F.K.)" (Wissenschaftliches Tagebuch III/b, 127)

cccxxv. "Es kommt ebenso darauf an, *wie* man etwas macht, als *was*

Nachdenken ist schwieriger und unbequemer als in Gyps modelliren und mit Zeichenmitteln zeichnen und an kinematischen Modellen drehen, besonders wenn es dadurch gelingt den Zuhörer weiss zu machen, dass er selbst glaubt, er verkünde etwas. Schwieriger ist es mitunter dem Zuhörer klar zu machen, dass er nicht versteht." (Wissenschaftliches Tagebuch III/b, 127)

cccxxvi. "Das wesentlichste Merkmal der modernen Mathematik, das in Zukunft und immer deutlicher hervortreten wird ist: man will innerlich verstehen, während früher durch formale Prozesse, die uns heute wie ein glücklicher Zufall erscheinen, Funde gemacht wurden. Letzteres ist heute ganz ausgeschlossen: man watet im Sande, kommt sich selbst unglücklich vor, wenn man Rechnerei macht und auf Glückszufall wartet. Heute ist die Idee die Hauptsache und zwar die in Worten zu beschreibende, den Zusammenhang beherrschende, unsere Anschauung liefernde Idee. Anfänge bei Riemann's Funktionenth., dann Zahlentheorie, Differentialgl. (Lie etc) Gipfel ist die axiomatische Methode, durch die das Raten d.h. der glückliche Zufall gewissermassen systematisch ausgeschaltet wird, indem eben alle Combination, alle logisch verträglichen Voraussetzungssysteme Berücksichtigung finden und es darauf ankommt das ganze vorliegende Material nach

diesen Gesichtspunkten der axiomatischen Methode zu ordnen." (Wissenschaftliches Tagebuch III/b, p. 103)

cccxxvii. "Was Gordan und Klein wollen, alles auf 7 Operationen zurückführen, das ist ja gerade die Absicht meiner axiomatischen Methode, so dass man jede richtige Thatsache in der Math. durch Erbsen beweisen, controlliren und jeden Fehler ebenso aufdecken kann. Nur der Anlass | die Anwendung meiner Th. ist eine andere, eine allgemeinere geistigere. Denjenigen, die Alles ausser Rechnen leugnen, die das Rechnen für etwas besonderes und das Ueberlegen, Begriffsbilden für etwas unerlaubtes halten, den Kroneckerschen Endlichkeitsfanatikern soll freilich durch meine Methode der Boden entzogen werden." (Wissenschaftliches Tagebuch III/b, p. 102f.)

cccxxviii. "Unsere Regeln: Einsetzungen, Abkürzungen, Argument-anhängen auch an Aussagen sind definitiv, vollständig und keiner Erweiterung fähig. Denn unser Denken geht in deren Handeln auf und ist finit (keine Hexerei!)" (This entry is found on p. 4^r of the Beilage to the third notebook.)

cccxxix. "Der Satz nun: Keinem Dinge kommt ein Prädikat zu, welches ihm widerspricht; heißt der Satz des Widerspruchs, und ist ein allgemeines, obzwar negatives, Kriterium aller Wahrheit, gehört aber auch darum bloß in die Logik, weil er von Erkenntnissen, bloß als Erkenntnissen überhaupt, ungesehen ihres Inhalts gilt, und sagt: daß der Widerspruch sie gänzlich vernichte und aufhebe." (Kant, *Kritik der reinen Vernunft*, A151/B190) "Denn daß ihm [i.e. the principle of non-contradiction] gar keine Erkenntnis zuwider sein könne, ohne sich selbst zu vernichten, das macht diesen Satz wohl zur conditio [A152] sine qua non, aber nicht zum Bestimmungsgrunde der Wahrheit unserer Erkenntnis." (Kant, *Kritik der reinen Vernunft*, A151 sq./B191).

cccxxx. "Von Kroneckers allgemeiner Anschauung kann man wohl sagen, dass die Wahrheit, die ja meist in der Mitte liegen soll, hier ganz auf einer Seite nämlich auf der Gegenseite zu Kronecker liegt." (Wissenschaftliches Tagebuch III/b, p. 120)

cccxxxi. The entire entry, which reads like a collection of key-words laying out the main points to be covered in a lecture or presentation, reads: "Kronecker war ein Reaktionär. Verhältniss der Physiker zu den Existenz-Beweisen. Gegensatz ob bestimmte Schwierigkeit zu überwinden, z.B. unendliche Prozesse durch einen Gedanken ausführen, worauf jede rein mathematische | Schwierigkeit zurückläuft, oder blos Beziehungen gesucht werden." (Wissenschaftliches Tagebuch III/f, p. 38)

cccxxxii. "Von verschiedenen Quellen (von Minkowski durch Hensel und von Eberhard durch Gutzmer und andere) habe ich gehört, | es herrsche in Berlin die Ansicht, 'als sei in meinen Arbeiten nichts zu finden, was nicht schon in Kroneckers Untersuchungen stände' (Hensel) und 'meine Untersuchungen seien nur eine Uebertragung der Kroneckerschen Ideen in die Invariantentheorie' (Kronecker selbst soll dies gesagt haben, wie Gutzmer dabei gewesen ist.) Ein flüchtiger Blick in meine Arbeit zeigt das Gegenteil: Sogar der übrigens noch gar nicht einman von Kronecker herrührende Begriff eines 'Moduls' ist wesentlich modifizirt (durch die Forderung der Homogenität). Alle weiteren Begriffe und überhaupt alle Methoden und Resultate sind mein Eigenthum (insbesondere derer auch der erste Fundamentalsatz) und ich lasse mir davon auch nicht ein Tüpfelchen nehmen von niemand, wer es auch sei. Vielmehr betrachte ich es als selbstverständliche Ehrensache, dass, nachdem ich dies gesagt, jeder anständige Mensch zur Beseitigung dieses offenbaren Irrthums beiträgt." (Wissenschaftliches Tagebuch I, pp. 53f.)

cccxxxiii. "Kroncker, der als erster die Anforderungen des finiten Standpunktes geltend gemacht hat, ging darauf aus, die nichtfiniten Schlußweisen allenthalben aus der Mathematik auszuschalten. In der Theorie der algebraischen Zahlen und Zahlenkörper ist er damit zum Ziel gekommen." (Hilbert and Bernays, *Grundlagen der Mathematik*, p. 42).

cccxxxiv. "Über die Endlichkeit ausführbarer Operationen in de[r] Mathematik. Die Kronecker'sche Ansicht, dass der Nachweis dieser Endlichkeit dem Begriff erst eine sichere Grundlage gibt ist nicht logisch. Eher könnte man sagen, dass der Nachweis nothwendig ist, dass etwas unter den Begriff fällt. —" (Wissenschaftliches Tagebuch II/b, p. 96)

cccxxxv. "Die Kroneckersche Forderung alles durch eine endliche Anzahl von Processen ausführen zu müssen, ist nicht klar z.B. $F(a,b)$ sei $= 0$ oder die kleine von ax^3+b dargestellte Primzahl. Dennoch ist es nicht möglich zu entscheiden, ob ax^3+b eine Primzahl darstellt, obwohl nur $F(a,b)$ Operationen dazu nothwendig wären Dagegen wären wir völlig befriedigt, wenn etwa $F(a,b) < a!b!$ nachweisbar wäre. Vgl. meinen Beweis für die Endlichkeit der Invarianten." (Wissenschaftliches Tagebuch III/b-22)

cccxxxvi. "Die Punkte, Geraden, Ebenen in meiner Geometrie werden nie etwas anderes als Gedankendinge und haben als solche nie etwas mit wirklichen Punkten, Geraden, Ebenen zu thun. Ganz anders ist es mit meinen Dingen 'oder', 'und', 'nicht'. Es ist eine Pikanterie, die einzig in der Wissenschaft dasteht und diesen Untersuchungen über das Denken einen eigenen Reiz giebt, dass da plötzlich die 'und', 'nicht', 'oder' auch die Bedeutung wirklichen 'oder', 'nicht' 'und' erlangen, indem wir mit diesen Dingen auf den Papier gewissermassen experimentiren, während wir mit den Punkten, Geraden, Ebenen, niemals experimentiren durften. Es ist da die Region, wo die Grenze zwischen grob sinnlicher Empirie und reinstem Denken in einanderfliessen und man als Logiker zugleich das thut, was in der Geometrie der Geodät, in der Elektrizitätstheorie der Experimentator thut, nämlich mit den Dingen 'oder', 'und', 'nicht' auf dem Papier experimentiren." (Wissenschaftliches Tagebuch III/b, p. 101)

cccxxxvii. "Die ausgeführte Betrachtung der Anfangsgründe von Zahlentheorie und Algebra diente dazu, uns das direkte inhaltliche, in Gedanken-Experimenten an anschaulich vorgestellten Objekten sich vollziehende und von axiomatischen Annahmen freie Schließen in seiner Anwendung und Handhabung vorzuführen. Diese Art des Schließens wollen wir, um einen kurzen Ausdruck zu haben, als das '*finite*' Schließen und ebenso auch die diesem Schließen zugrunde liegende methodische Einstellung als die 'finite' Einstellung oder den 'finiten' Standpunkt bezeichnen. Im gleichen Sinne wollen wir von finiten Begriffsbildungen und Behauptungen sprechen, indem wir allemal mit dem Worte 'finit' zum Ausdruck bringen, daß die betreffende Überlegung, Behauptung oder Definition sich an die Grenzen der grundsätzlichen Vorstellbarkeit von Objekten sowie der grundsätzlichen Ausführbarkeit von Prozessen hält und sich somit im Rahmen konkreter Betrachtung vollzieht." (Hilbert and Bernays, *Grundlagen der Mathematik*, p. 32).

cccxxxviii. "Der Logikkalkül sollte zunächst dazu dienen, die Sprache zu präzisieren. Kann er etwa auch prinzipielle Fragen lösen? Die Frage des tertium non datur natürlich nicht; denn sie haben wir durch die Äquivalenz $\overline{(x)}Ax$ aeq. $(Ex)\overline{A}x$ in den Kalkül hinein gesteckt. Wohl aber ist das Entscheidungsproblem von dieser Art: kann man durch ein endliches Verfahren entscheiden, ob eine vorgelegte Formel (mit Relationsaussagen) richtig ist? Das ist ein Problem mathematischen Charakters." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 608).

cccxxxix. The official lecture notes read: "Wie bereits gesagt, ist für den allgemeineren Fall, dass die logischen Funktionszeichen auch zwei oder mehrere Argumente haben können, die Entscheidungsmöglichkeit bisher nicht erwiesen. Die Lösung des Entscheidungsproblems ist für die Fragen der Axiomatik von grundsätzlicher Wichtigkeit. [...] Wir wollen uns klar machen, dass mit der Lösung des Entscheidungsproblems ein Verfahren gegeben wäre, durch das jene Unbeweisbarkeit sich wenigstens grundsätzlich feststellen lassen müsste, (wenn auch vielleicht die Umständlichkeit des Verfahrens die praktische Durchführung illusorisch machen könnte.)" (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 542). In the corresponding place in Kneser's notes, one reads: "Um aber weiter zu kommen, brauchen wir mindestens eine Relation, die der Identität $\equiv (x,y)$. Auch mit dieser ist das Entscheidungsproblem zu lösen. Herr Behmann hat es auch noch etwas weiter getrieben. Die allgemeine Lösung, auch bei nur zwei16 Relationen mit zwei Argumenten, stellt ein äußerst schwieriges mathematisches Problem vor. Nehmen wir einmal an, es sei gelöst für beliebig viele Argumente. Dann kann man tiefere Probleme in Angriff nehmen." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 609).

cccxl. "Bereits im § 1 haben wir uns mit der logischen Symbolik vertraut gemacht. Sie diente uns dort als eine Formelsprache, an Hand deren wir uns die Struktur von mathematischen Axiomen deutlich vor Augen führten. Jetzt wollen wir mit Hilfe dieser Formelsprache zu einer Formalisierung der logischen Schlüsse gelangen. Das logische Schließen soll nachgebildet werden durch ein äußeres Handeln nach

bestimmten Regeln" (Hilbert and Bernays, *Grundlagen der Mathematik*, p. 45).

cccxli. "Durch die Ausführung des vorigen Paragraphen [§ 3 on the sentential calculus] ist die Formalisierung des logischen Schließens vorbereitet. Wir haben die Theorie der Wahrheitsfunktionen als eine Hilfsdisziplin entwickelt und aus ihr ein Verfahren zur Formalisierung gewisser Schlüsse entnommen. Dieses Verfahren besteht darin, daß man ausgeht von gewissen Formeln, die entweder identisch wahre Ausdrücke sind oder symbolische Darstellung von Prämissen (Axiomen), und aus diesen nach der Regel der Einsetzung und mit Hilfe des Schlußschemas weitere Formeln ableitet. Bei diesem Verfahren kommt ein wesentliches logisches Moment gar nicht zur Geltung, nämlich die Beziehung der Aussagen auf Gegenstände, d.h. das Verhältnis von *Subjekt und Prädikat*. Dieses Verhältnis und die darauf sich gründenden Schlüsse haben wir nun im Formalismus darzustellen." (Hilbert and Bernays, *Grundlagen der Mathematik*, p. 87).

cccxlii. "Boole wollte, wenn ich ihn recht verstehe, eine Technik ausbilden, durch welche logische Aufgaben in systematischer Weise gelöst werden könnten, ähnlich wie die Algebra eine Technik der Elimination und der Berechnung von Unbekannten lehrt. [...] Die logischen Gesetze nehmen dann die Form eines Algorithmus an, der jedoch nur teilweise mit dem für dieselben Zeichen gültigen der Arithmetik zusammenhält. Vom Inhalte wird dabei ganz abgesehen. Diese Mittel entsprechen im Allgemeinen ihrem Zwecke, wenigstens in Bezug auf den Kreis von Aufgaben, den Boole im Sinne hat. [...] Ich hatte dabei [when conceiving his Begriffsschrift] von vornherein den *Ausdruck eines Inhaltes* im Auge." (Frege, *Nachgelassene Schriften*, p. 13).

cccxliii. "Bei näherer Überlegung erkennen wir nämlich bald, daß die Frage der Widerspruchslosigkeit bei den ganzen Zahlen und Mengen nicht eine für sich alleinstehende ist, sondern einem großen Bereich schwierigster erkenntnistheoretischer Fragen von spezifisch mathematischer Färbung angehört: ich nenne, um diesen Bereich von Fragen kurz zu charakterisieren, das Problem der prinzipiellen *Lösbarkeit einer jeden mathematischen Frage*, das Problem der nachträglichen *Kontrollierbarkeit* des Resultates einer mathematischen Untersuchung, ferner die Frage nach einem *Kriterium für die Einfachheit* von mathematischen Beweisen, die Frage nach dem Verhältnis zwischen *Inhaltlichkeit und Formalismus* in Mathematik und Logik und endlich das Problem der *Entscheidbarkeit* einer mathematischen Frage durch eine endliche Anzahl von Operationen" (Hilbert, "Axiomatisches Denken," p. 153).

cccxliv. "10. Entscheidung der Lösbarkeit einer diophantischen Gleichung. Eine diophantische Gleichung mit irgendwelchen Unbekannten und mit ganzen rationalen Zahlkoeffizienten sei vorgelegt: *man soll ein Verfahren angeben, nach welchem sich mittels einer endlichen Anzahl von Operationen entscheiden läßt, ob die Gleichung in ganzen rationalen Zahlen lösbar ist.*" (David Hilbert. *Gesammelte Abhandlungen. Analysis, Grundlagen der Mathematik, Physik, Verschiedenes, nebst einer Lebensgeschichte.* Vol. 3. Springer Verlag, 1935, p. 310).

cccxlv. "Es ist völlig willkürlich bei den gebrochenen, rationalen Zahlen abzusch[l]iessen und die irrationalen Zahlen nicht mehr zuzulassen. Wir können auch die rationalen gebrochenen Zahlen nicht mehr als Cardinalzahlen auffassen sondern müssten dann logischer Weise bei den *ganzen positiven* Zahlen (vielleicht streng bloss von 1 bis höchstens 7) stehen bleiben. Schon die Zahl $\frac{1}{2}$ erfordert die Anschauung der Strecke, wir können die Mitte einer Strecke praktisch nie genau angeben, wohl aber genau anschauen und vorstellen. Eben so genau können wir auch den Punkt $\sqrt{2}$ anschauen und vorstellen. Es giebt ein Ueberschreiten von rein anschaulichen zu rein formal gedachten. Diesen Uebergang vermittelt ein breiter Weg mit vielen gleich bequemen Zugängen und offenstehenden Thoren, während Kronecker sich darin verschanzt hat durch ein willkürlich gelegte [?] Hemmnisse durch ein Nadelöhr durchzuzwängen." (Wissenschaftliches Tagebuch I, p. 8)

cccxlvi. "Die Zahl ist der fingirte, gedachte Träger einer Regel, oder ein wandernder Schnitt." (Wissenschaftliches Tagebuch I, p. 26)

cccxlvii. "Die irrational Zahl hat nichts unendliches (was überhaupt mathematisch ein Unsinn ist) an sich sondern die Fundamentalreihe ist nur Träger eines end|lichen Gesetzes d.h. a_n ist für ein endliches n stets durch eine endliche Zahl von Operationen zu finden." (Wissenschaftliches Tagebuch I, p. 38f.)

cccxlviii. “Existenz der Irrational-Zahlen. Der Streit darüber ist gegenstand[s]los bei der Definition: die Irrational Zahl gleich Träger eines Gesetzes d.h. wenn mir später ein Gesetz gegeben wird so kann ich für dasselbe alle meine Aussagen machen. Die Erfindung der Irrational Zahl ist in dem Sinne gleichberechtigt u. von gleichem Werthe wie die Erfindung der Differential[-]|Rechnung gleich wie bei letzterer besteht auch bei jener der Kernpunkt darin, dass die immerfort vorkommenden u. sich wiederholenden Schlüsse vornweg gewonnen u. zu Anfang in einer allgemeinen Gestalt behandelt werden. Es sind die Behauptungen über die Irrational Zahl in gleicher Linie mit allen andern allgemeinen Sätzen. Z.B. Satz von Minkowski über den Maximal-Minimal Inhalt convexer Körper sagt in der That nichts anderes aus als: wenn wir bei meiner Untersuchung später einmal ein Körper begegnet, welcher convex ist, so ist sein Inhalt etc. Ebenso der Satz von den Bedingungen über die Convergenz einer Potenzreihe etc, also: die Gesammtheit = System Aller = Jeder = wenn eins mir begegnet.” (Wissenschaftliches Tagebuch I, p. 79f.)

cccxlix. “Beim Begriff der Irrationalzahl ist es nöthig zu sagen, Wenn ein *Reihungsgesetz* gegeben ist, vermögen dessen zu jeder Zahl n eine Zahl a_n gefunden werden kann etc... Reihungsgesetz ist dabei zu definieren als ein System von Rechnungsoperationen (Addition und Subtraktion ganzer Zahlen reicht aus, da hierauf alles andere zurückzuführen ist), die es mir ermöglichen *lediglich* [footnote: d.h. nach der Vorschrift (also z.B. die Benutzung einer endlichen, wenn auch willkürlichen Zahlentabelle erlaubt] durch eine endliche Zahl von Anwendungen des Subtrahirens und Addirens allemal zu n eine Zahl a_n zu finden. Diese Definition reicht aus, denn alle|mal, wenn wir irgend ein Ding vorgelegt sehen, kann sich aus dieser Definition entscheiden ob das vorgelegte Ding ein Reihungsgesetz ist oder nicht. Das ist aber meiner Ansicht das wahrhaft einzige Merkmal einer Definition. Danach die eine Definition mit negativen Merkmalen nicht fehlerhaft.” (Wissenschaftliches Tagebuch I, p. 39f.)

cccl. “Wie Lie den Gruppenbegriff, so will ich den Idealbegriff auf alle mathem. Disciplinen anwenden, insbesondere auf die Functionentheorie und die Theorie der Differentialgleichung. Vielleicht kommt beides auf dasselbe hinaus, verg. meinen Brief an Klein vom 15. Dec. 1894.” (Wissenschaftliches Tagebuch II/f, p. 67)

cccli. The first half of the paragraph is quoted in the following endnote. “Der einzige Begriff, der a priori diesen empirischen Gehalt der Erscheinungen vorstellt, ist der Begriff des *Dinges* überhaupt, und die synthetische Erkenntnis von demselben a priori kann nichts weiter, als die bloße Regel der Synthesis desjenigen, was die Wahrnehmung a posteriori geben mag, niemals aber die Anschauung des realen Gegenstandes a priori liefern, weil diese notwendig empirisch sein muß.” (Kant, *Kritik der reinen Vernunft*, A720/B748)).

ccclii. “Nun ist von aller Anschauung keine a priori gegeben, als die bloße Form der Erscheinungen, Raum und Zeit, und ein Begriff von diesen, als Quantis, läßt sich entweder zugleich mit der Qualität derselben (ihre Gestalt), oder auch bloß ihre Quantität (die bloße Synthesis des Gleichartigmannigfaltigen) durch Zahl a priori in der Anschauung darstellen, d.i. konstruieren. Die Materie aber der Erscheinungen, wodurch uns *Dinge* im Raume und der Zeit gegeben werden, kann nur in der Wahrnehmung, mithin a posteriori vorgestellet werden.” (Kant, *Kritik der reinen Vernunft*, A720/B748).

ccccliii. “Begriff der ganzen Zahl beruht nothwendig auf Zeit und Raum. Begriff der Stetigkeit einfacher also als der Begriff der ganzen Zahl. Kant hat Recht.” (Wissenschaftliches Tagebuch I, p. 28)

ccccliv. “So gibt es denn einen doppelten Vernunftgebrauch, der, unerachtet der Allgemeinheit der Erkenntnis und ihrer Erzeugung a priori, welche sie gemein haben, dennoch im Fortgange sehr verschieden ist, und zwar darum, weil in der Erscheinung, als wodurch uns alle Gegenstände gegeben werden, zwei Stücke sind: die Form der Anschauung (Raum und Zeit), die völlig a priori erkannt und bestimmt werden kann, und die Materie (das Physische), oder der Gehalt, welcher ein Etwas bedeutet, das im Raume und der Zeit angetroffen wird, mithin ein Dasein enthält und der Empfindung korrespondiert.” (Kant, *Kritik der reinen Vernunft*, A723/B751).

ccclv. “In Ansehung des erstern können wir unsere Begriffe in der Anschauung a priori bestimmen, indem wir uns im Raume und der Zeit die Gegenstände selbst durch gleichförmige Synthesis schaffen, indem wir sie bloß als Quanta betrachten. [...] dieser ist der Vernunftgebrauch

durch Konstruktion der Be[A724/B752]griffe, indem diese, da sie schon auf eine Anschauung a priori gehen, auch eben darum a priori und ohne alle empirischen data in der reinen Anschauung bestimmt gegeben werden können." (Kant, *Kritik der reinen Vernunft*, A723 sq./B751 sq.).

ccclvi. "Aber im Raume eine Anschauung a priori zu bestimmen (Gestalt), die Zeit zu teilen (Dauer), oder bloß das Allgemeine der Synthesis von einem und demselben in der Zeit und dem Raume, und die daraus entspringende Größe einer Anschauung überhaupt (Zahl) zu erkennen, das ist ein *Vernunftgeschäfte* durch Konstruktion der Begriffe, und heißt *mathematisch*." (Kant, *Kritik der reinen Vernunft*, A724/B752).

ccclvii. In his discussion of logic in general, Kant characterises logic as the "Wissenschaft der Verstandesregeln überhaupt" (A52/B76). In his motivation of the distinction between general and what Kant calls "transcendental" logic, he describes the subject matter of transcendental logic, which he goes on to identify (A79f/B105) with the categories, as "die Regeln des reinen Denkens eines Gegenstandes" (A55/B80).

ccclviii. Kant first introduces and defined continuity in the same section of the *Kritik der reinen Vernunft*: "Die Eigenschaft der Größe, nach welcher an ihnen kein Teil der kleinstmögliche (kein Teil einfach) ist, heißt die Kontinuität derselben. Raum und Zeit sind quanta continua, weil kein Teil derselben gegeben werden kann, ohne ihn zwischen Grenzen (Punkten und Augenblicken) einzuschließen, mithin nur so, daß dieser Teil selbst wiederum ein Raum, oder eine Zeit ist. [...] [A170] Dergleichen Größen kann man auch *fleißende* nennen, weil die Synthesis (der produktiven Einbildungskraft) in ihrer Erzeugung ein Fortgang in der Zeit ist, deren Kon[B212]tinuität man besonders durch den Ausdruck des Fließens (Verfließens) zu bezeichnen pflegt." (Kant, *Kritik der reinen Vernunft*, A169 sq./B211 sq.).

ccclix. This passage begins immediately after the one quoted in the previous endnote: "Alle Erscheinungen überhaupt sind demnach kontinuierliche Größen, sowohl ihrer Anschauung nach, als extensive, oder der bloßen Wahrnehmung (Empfindung und mithin Realität) nach, als intensive Größen. Wenn die Synthesis des Mannigfaltigen der Erscheinung unterbrochen ist, so ist dieses ein Aggregat von vielen Erscheinungen, und nicht eigentlich Erscheinung als ein Quantum, welches nicht durch die bloße Fortsetzung der produktiven Synthesis einer gewissen Art, sondern durch Wiederholung einer immer aufhörenden Synthesis erzeugt wird. Wenn ich 13 Taler ein Geldquantum nenne, so benenne ich es so fern richtig, als ich darunter den Gehalt von einer Mark fein Silber verstehe; welche aber allerdings eine kontinuierliche Größe ist, in welcher kein Teil der kleinste ist, sondern jeder Teil ein Geldstück ausmachen könnte, welches immer Materie zu noch kleineren enthielte. Wenn ich aber unter jener Benennung 13 runde Taler verstehe, als so viele Münzen (ihr Silbergehalt mag sein, welcher er wollte,) so benenne ich es unschicklich durch ein Quantum von Talern, sondern muß es ein Aggregat, [A171] d.i. eine Zahl Geldstücke, nennen. Da nun bei aller Zahl doch Einheit zum Grunde liegen muß, so ist die Erscheinung als Einheit ein Quantum, und als ein solches jederzeit ein Kontinuum." (Kant, *Kritik der reinen Vernunft*, A171 sq./B212).

ccclx. "Die Mathematische Grenze ist, was ein Leben und in der Philosophie die Ideen sind (nach Kant). Math. Grenze ist das genaue physikalische Gesetz, welches nur in uns existirt. Wir thun, als ob dieses genaue Gesetz gelte. Das ist der Einfachheit wegen nöthig." (Wissenschaftliches Tagebuch II/b, p. 94)

ccclxi. "Das Ideal wird dem Realen gegenübergestellt, und zwar einerseits als das Unwirkliche (bloß Gedachte) dem Wirklichen, andererseits als das Vollkommene dem existierenden Unvollkommenen. Ich will zuerst nur von der ersten Bedeutung des Idealen sprechen. Diese spielt besonders in der Mathematik eine große Rolle." (Hilbert, *Natur und mathematisches Erkennen*, p. 90) When Hilbert returns to the second sense of idealisation towards the end of the chapter he says: "Nun gibt es aber noch eine ganz *andere Art von Idealisierung*, die für die reine Mathematik nicht in Betracht kommt, in der Physik aber reiche Anwendung findet. Bei der mathematischen Idealisierung ist das Wesentliche der Übergang zu einem größeren System, bei welchem die Beziehungen übersichtlicher werden. Die andere Methode des Idealisierens dient ebenfalls dem Zweck der Vereinfachung, und zwar wird diese hier dadurch bewirkt, daß man von gewissen

Komplikationen absieht. Man betrachtet einen gegebene Gegenstand als *Annäherung an ein Ideal*, das durch größere Einfachheit ausgezeichnet ist." (Hilbert, *Natur und mathematisches Erkennen*, p. 98).

ccclxii. "Wenn man nun die immer weiter gehende Anpassung der physikalischen Begriffsbildung an die Wirklichkeit betrachtet, so wird der Gedanke an eine *Grenze* dieses Prozesses nahegelegt. Das hierbei vorschwebende Endziel besteht in einer vollständigen mathematischen Beschreibung der gesamten Wirklichkeit, wie ich sie zu Beginn unserer Erörterung über die physikalische Gesetzlichkeit in Betracht gezogen habe. Wir wollen uns nun klar machen, wie dieses *Limesideal* beschaffen sein müßte, indem wir uns dabei an die moderne Feldtheorie halten, durch welche das physikalische Limesideal eine bestimmtere Form gewinnt." (Hilbert, *Natur und mathematisches Erkennen*, p. 99).

ccclxiii. "Bedeutung der Wissenschaften für alle Wissenschaften, Fertigkeiten und Künste, für den pract. Iurist ist eine Entscheidung gleich einer math. Aufgabe. Kant nahm die Mathematik zum Ausgangspunkt. Der Vorwurf der Trockenheit kommt in dieser Hinsicht jeder Wissenschaft zu." (Wissenschaftliches Tagebuch II/b, p. 106)

ccclxiv. "Die Kantischen Antinomien erscheinen im Logikalkül als einfache Rechenfehler. Die Frage ist also nur die, wo steckt dieser Rechenfehler? So ist es mit allen Antinomien, von denen es wimmelt, auch im täglichen Leben, wo man dann trivial sagt, jede Sache habe ihre 2 Seiten!" (Wissenschaftliches Tagebuch III/b, p. 93)

ccclxv. "Kant hat zuerst Unmöglichkeitsbehauptungen gemacht: 'Es ist unmöglich zu beweisen, dass Gott existirt' und 'Es ist unmöglich zu beweisen, dass Gott nicht existirt', sind 2 unabhängige Behauptungen." (Wissenschaftliches Tagebuch Beilage, p. 3^v)

ccclxvi. "Die Kantsche Frage, wie ist reine Mathematik möglich? ist das erste Glied einer Kette von Fragen, an der wir auf den hohen Standpunkt gelangen werden, von dem aus Philosophie genaue Wissenschaft werden wird." (Wissenschaftliches Tagebuch Beilage, p. 3^r)

ccclxvii. "Titel meines künftigen Buches: Philosophie auf naturwissenschaftlicher Grundlage." (Wissenschaftliches Tagebuch Beilage, p. 2^r)

ccclxviii. "In allen anderen Wissenschaften Material, von dem man nicht weiss, wo es herkommt, von dem man nur weiss, dass es da ist. Hier reines Denken, reine Philosophie. Kant hat das als erste[r] gefühlt, aber nicht bewiesen, er hat angefangen, aber nicht zu Ende geführt." (Wissenschaftliches Tagebuch I, p. 28)

ccclxix. "Reine Geometrie lesen: (1) Axiome der Geometrie (Helmholtz, Grassmann, Riemann, Kant etc. Welches sind die nothwendigen Anschauung entnommenen Postulate, auf Grund derer später alles aus Logik (Schematismus) wird. Was entspricht also den ganzen Zahlen in der Analysis, die nicht zum ganzen Gebäude ausreichen, sobald man ihre Reihungsgesetze der Anschauung entommen hat? 2.) Euklidische (Schul)geometrie (Eigenschaft des Dreieckes, Kreises, des Tetraeders, Kugel etc.) 3.) Analysis situs. (Kurven mit Wendepunkte, Knoten im Raume, Riemannsche Fläche etc. 4.) Geometrie der Lage. Hauptsatz mit Deutung der dritten Dimension also ohne jede Benutzung ohne Doppelverhältnis abzuleiten." (Wissenschaftliches Tagebuch I, p. 38)

ccclxx. "Weierstrass ist der Euklid der Analysis, indem er strenge die Grundlagen u. Voraussetzungen auseinandergelegt hat u. auf diesen consequent wieder aufgebaut den ganzen stolzen Bau der Wissenschaft. Die Voraussetzungen | sind nichts anders als das Rechnen mit ganzen u. positiven Zahlen, welches die Anschauung lehrt. Vergl. Kant. der diese Thatsachen als a priori hinstellt. Weiter kann man nicht zurückführen. Also die Behauptungen der Mathematik sind in gleicher Weise gewiss, wie das Rechnen mit ganzen Zahlen." (Wissenschaftliches Tagebuch I, p. 75f.)

ccclxxi. "Vorausgesetzt, dass es keine praktischen Schwierigkeiten gäbe, ist der menschliche Verstand vermögend, alle Fragen, die er sich selbst stellt, zu lösen? Muss es möglich sein zu entscheiden über die Quadratur des Kreises, gibt es eine wohl definierte, auf die sinnlichen Dinge bezügliche Frage, welche eine Antwort haben muss und die gleichwohl sich nicht beantworten lässt. Kann man einen allgemeinen

zwingenden Beweis führen, dass jede Erkenntniss möglich sein muss?" (Wissenschaftliches Tagebuch I, p. 32)

ccclxxii. "Math. ist Denken, reines Denken, nichts weiter d.h. bestimmtes, richtiges Denken, Denken schlechtweg." (Wissenschaftliches Tagebuch III/b, p. 94)

ccclxxiii. "Die Mathematik gibt das glänzendste Beispiel, einer sich, ohne Beihülfe der Erfahrung, von selbst glücklich erweiternden reinen Vernunft." (A712/B741).

ccclxxiv. "Außer der Transzendentalphilosophie gibt es noch zwei reine Vernunftwissenschaften, eine bloß spekulativen, die andere praktischen Inhalts: *reine Mathematik*, und *reine Moral*. Hat man wohl jemals gehört: daß, gleichsam wegen einer notwendigen Unwissenheit der Bedingungen, es für ungewiß sei ausgegeben worden, welches Verhältnis der Durchmesser zum Kreise ganz genau in Rational- oder Irrationalzahlen habe? Da es durch erstere gar nicht kongruent gegeben werden kann, durch die zweite aber noch nicht gefunden ist, so urteilte man, daß wenigstens die Unmöglichkeit solcher Auflösung mit Gewißheit erkannt werden könne, und Lambert gab einen Beweis davon." (Kant, *Kritik der reinen Vernunft*, A480/B508).

ccclxxvi. "Alle Aufgaben auflösen und alle Fragen beantworten zu wollen, würde eine unverschämte Großsprecherei und ein so ausschweigender Eigendünkel sein, daß man dadurch sich so fort um alles Zutrauen bringen müßte. Gleichwohl gibt es Wissenschaften, deren Natur es so mit sich bringt, daß eine jede darin vorkommende Frage, aus dem, was man weiß, schlechthin beantwortlich sein muß, weil die Antwort aus denselben Quellen entspringen muß, daraus die Frage entspringt, und wo es keinesweges erlaubt ist, unvermeidliche Unwissenheit vorzuschützen, sondern die Auflösung gefodert werden kann." (Kant, *Kritik der reinen Vernunft*, A476/B504).

ccclxxvii. "Diese Überzeugung von der Lösbarkeit eines jeden mathematischen Problems ist uns ein kräftiger Ansporn während der Arbeit; wir hören in uns den steten Zuruf: *Da ist das Problem, suche die Lösung. Du kannst sie durch reines Denken finden; denn in der Mathematik gibt es kein Ignorabimus!*" (Hilbert, *Gesammelte Abhandlungen*, p. 298).

ccclxxviii. "Die Mathematik ist die einzige Wissenschaft, bei welcher wir nicht ignorabimus sondern im Gegenteil in weitesten Umfang im Bezug auf alle und noch so schwierige Probleme sagen müssen: *noszemus*." (Wissenschaftliches Tagebuch I, p. 72)

ccclxxix. "Das es kein Ignorabimus in der Mathematik giebt, ist wahrscheinlich durch meine Theorie der Logik-Arithmetik beweisbar." (Wissenschaftliches Tagebuch III/b, p.104)

ccclxxx. "Beweis aller Beweise: dass man den Beweis immer muss finden können!" (Wissenschaftliches Tagebuch III/b, 96)

ccclxxxi. "Was die Theorie von Problemstellungen betrifft, so kann man in der Zahlentheorie leicht beliebig schwierige Probleme stellen, die gar keine Angriffspunkte bieten. Da muss man dann zunächst die nächst leichteren Probleme suchen." (Wissenschaftliches Tagebuch III/b, p. 133)

ccclxxxii. "Die Mathematik hat anderen Wissenschaften gegen|über den Vorzug, dass man bestimmt sagen kann, ob eine Frage erledigt ist oder nicht. – Schachspiel hat vor der Wissenschaft den bestimmten – freilich willkürlichen – aber ein für alle Mal gesetzten Zweck – das Matt-Setzen voraus, nach dessen Erreichung oder Verfehlung die Güte des Gedankens (der Methode) gemessen werden kann. In der Physik stellt die Natur die Aufgabe. Bei der Mathem. liegt die Gefahr des Fortrennens in der Willkür – der Thatsache entsprechend, dass der nämliche Verstand selbstständig aus sich heraus die Aufgabe stellt, sie löst und kritisch nach eignen Ermessen ohne äusseren Anhalt sie abschätzt und würdigt: Streben nach grossen leitenden Gesichtspunkten und die Vorschrift, die man sich selbst als Pflicht auferlegt, jedes einzelne Resultat in einer Beziehung zur Gesamtwissenschaft zu betrachten und zu vergleichen. Daraus ergiebt sich im Durcharbeiten des vorhandenen Stoffes besser als, wenn jeder seinen eigenen Mist weitergräbt, um den sich doch niemand sonst kümmert. Wer Referate macht, sieht, dass viele in dieser Weise die Wissenschaft treiben: Niemand zur Freude, sich selbst zur Qual.

Nicht auf die Erledigung | bestimmter Fragen (diese nun als Leitmotiv, als vorschwe[be]ndes Ziel, als anreizendes Lockmittel) sondern darauf kommt es an, dass sich vor unserem geistigen Augen das stolze Gebäude der Wissenschaft harmonisch und einheitlich erhebt, wie aus einem Guss. Eine Frage F, für welche die nächste Oberfrage F^2 mit ja beantwortet wird, ist minder hoch zu schätzen – vielleicht Criterium für die Wichtigkeit einer Frage. Uebrigens ist es selbst wenn die nächste Oberfrage F^2 beantwortet ist, noch ein grosser Unterschied, ob ich bloss zeige dass etwas durch eine endliche Zahl von Processen möglich (wie bei meinem Beweis für die Endlichkeit der Invarianten ternärer Formen) oder ob ich wirklich eine obere Grenze, für die Zahl der Processe angebe, so dass es nur auf die Zeit und die Anwendung von Arbeit ankommt. Ferner ist auch noch die Frage, ob es Probleme in der Mathematik giebt, deren Erledigung nicht in einer vorgeschriebenen kleinen Zeit möglich ist? etc." (Wissenschaftliches Tagebuch I, p. 51ff.)

ccclxxxiii. "Viele Mathematiker gleichen den Bergleuten, welche sich tiefer den Schachten graben, obwohl der Weg zu ihnen schon ver|schüttet ist, so dass keine freie Luft und keine neue Lebenskraft an die Arbeitsstelle geschafft werden kann: sie leisten für die Gesamtheit der Wissenschaft nichts und verkümmern schliesslich elend." (Wissenschaftliches Tagebuch I, p. 40f.)

ccclxxxiv. "Wer in der Wissenschaft das Bergsteigen liebt und die frische und gesunde Luft, der suche die Lichten Höhen der Arithmetik auf" (Wissenschaftliches Tagebuch II/f, p. 77)

ccclxxxv. "Bei der Auswahl von Problemen darf niemals die Rücksicht auf die Anwendung den Ausschlag geben. Wer würde wohl um das Linsengericht einer wohlgelungenen physikalischen Anwendung oder Anregung das Erstgeburtsrecht der wissenschaftlichen Selbstständigkeit verkaufen?" (Wissenschaftliches Tagebuch III/b, p. 123)

ccclxxxvi. "Wenn ein math. Problem, ich will sagen eine Frage A gestellt ist, so kann man zuvor die Frage erledigen, ob diese Frage A durch eine endliche Zahl von Operationen erledigt werden kann oder nicht. Dies Frage sei B genannt. Hieraus ebenso Frage C, D,... Der jedemalige Stand der Wissenschaft entscheidet wo diese Kette von Fragen abbricht. z.B. Frage A: Giebt es C_6 mit 11 getrennten in Ovalen. kann ich bisher nicht beantworten. Dagegen sehr wohl die hieraus stammende Frage B. Diese ist mit ja zu beantworten und zwar so: Man konstruiert für die Diskriminantenfläche die Gl. der so viel als möglich berührenden Kugeln von so viel Dimensionen. Dann teilt man den ganzen Raum in Cuben C $\frac{1}{2^n}$ Volumen jener Kugel d.h. endliche Anzahl Cuben, wo in jedem Cubus nur ein Punkt zu nehmen und auszuprobieren ist." (Wissenschaftliches Tagebuch I, p. 50.)

ccclxxxvii. The full report, which, by the standards of these reports, is rather expansive reads: "*Sitzung vom 21. Dezember*: D. Hilbert teilt einige allgemeine Betrachtungen über mathematische Probleme mit. Er ordnet dabei jedem Problem A ein 'Oberproblem' B zu, das in der Frage besteht, ob das Problem A sich durch endlich viele Schritte lösen läßt, und zeigt an einigen Beispielen, daß die Lösung des Oberproblems B, obwohl leichter als die des Problems A, keineswegs trivial zu sein braucht, ja sogar – wie beim Fermatschen Satz – sehr schwierig sein kann. – Im Anschluß hieran erwähnt er die nun im Druck vorliegenden Vorträge, die Hr. Poincaré im April in Göttingen gehalten [sic], und erörtert insbesondere die dort gegebenen Ausführungen zur Mengenlehre." (August Gutzmer, ed. *Jahresbericht der Deutschen Mathematiker-Vereinigung*. Vol. 19. 1910, p. 28 of the *Mitteilungen und Nachrichten*).

ccclxxxviii. "Es giebt Probleme, bei denen die Oberfrage wahrscheinlich dadurch am leichtesten beantwortet wird, dass man das Problem selbst löst z.B. $p - q = 2$ auf ∞ viele Arten zu lösen, wo p, q Primzahlen sind; aber auch umgekehrt z.B die $10^{(10^{10})}$te Ziffer in der Dezimalbruchentwicklung für π angeben" (Wissenschaftliches Tagebuch III/b, p. 51ff.) – This entry is referred to in the retrospective overview of highlights inlaid in the first notebook.

ccclxxxix. "Probleme, wie die Frage, ob es eine Fläche 4^{ter} Ordnung mit 11 getrennten Ovalen giebt, oder welches die Maximalzahl der sich einander ausschliessenden Züge einer ebenen | Curve 8ter Ordnung ist, sind von der Art, dass die zugehörige Oberfrage d.h. die Frage, ob jenes Problem durch endliche Anzahl von Schritten entscheidbar ist, zu bejahen ist, aber diese Probleme sind von derselben Art, wie das Problem die $10^{(10^{10})}$ Ziffer in der Dezimalbruchentwicklung von π^π zu finden." (Wissenschaftliches Tagebuch III/b, p. 87f.)

cccxc. “Unter 100 Gedanken, die zunächst an sich gleich gut sind, hat vielleicht einer durchschlagenden Erfolg. Man hat letzteren aber nicht von vorne herein in der Gewalt. – Man muss eine Sache so lange durchdenken, bis man sich genau Rechenschaft ablegen kann, warum man nicht weiter kommt, d.h. welche vorläufig zu complicirten aber durchführbaren Vorarbeiten geleistet werden müssen, um die gestellte Aufgabe zu lösen d.h. man muss wenigstens die nächste Oberfrage $B = F^2$ lösen.” (Wissenschaftliches Tagebuch I, p. 51)

cccxci. “Das ganze Denken besteht darin, einfachere Probleme als das vorgelegte zu finden als gewissermassen ist jedes Problem ein Problem der Variationsrechnung, indem es darauf ankommt dasjenige Problem zu finden, welches unter den ungelösten das einfachste ist mit der Nebenbedingung, dass es zur Lösung des vorgelegten Problems nothwendig ist. z.B. Problem den Pascalschen Satz in der Ebene ohne Parallelenaxiom zu beweisen, oder seine Unbeweisbarkeit zu zeigen, so erkennt man, dass das dasselbe ist, wie das Problem alle 3 gliedrigen Gruppen von continuirlichen Transformationen aufzustellen (ohne aber die Differenzirbarkeit der die Transformation vermittelnden Funktionen vorauszusetzen) und da sieht man, dass man zuvor die 1- und die 2-gliedrigen alle aufstellen muss.” (Wissenschaftliches Tagebuch III/b, p. 126)

cccxcii. “Häufig, wenn man durchaus ein Problem nicht lösen kann, liegt das daran, dass man ein anderes näher liegendes noch nicht gelöst hat: Wenn man sich die Strümpfe und dann die Stiefel ausziehen will wird das unmöglich, umgekehrt dagegen geht es ganz leicht.” (Wissenschaftliches Tagebuch III/b, p. 117)

cccxciii. “Beweisen, dass man durch eine endliche Anzahl von Operationen entscheiden kann, ob und wie viele ganzzahlige Lösungen $\phi(x', y, \dots) = 0$ mit ganzzahligen Coefficien[ten] besitzt.” (Wissenschaftliches Tagebuch I, p. 7)

cccxciv. “Stelle auf eine Theorie des Rechnens d.h. ergründe worin die Vortheile bekann|ter arithmetischer Formeln und Sätze (Summation arithmetischer Reihen, Congruenzformeln, Stirlungsche Formel, vergl. Schlömilch Compendium Bd 2 S. 264.) zum Ausrechnen bestimmter Aufgaben beruhen und wie man diese Vortheile rechnerisch abschätzen kann. (Vielleicht indem man gewisse elementare Rechnungsoperationen (addiren, multipliciren, potenziren) als Einheiten einführt und die Zahl der anzuwendenden Einheiten auf verschiedenen Wegen, die man beim Rechnen einschlägt, bestimmt. Wie viel Rechnungseinheiten z.B. sind nöthig, um so und so vielte Stelle der Decimalbruch-Entwiclung von π zu bestimmen? Wie berechnet man $n!$ am schnellsten? Vielleicht die ersten Ziffern aus Stirlings Formel, und die letzten durch Congruenz-Rechnung.” (Wissenschaftliches Tagebuch II/f, p. 0f.)

cccxcv. “Als 24stes Problem in meinem Pariser Vortrag wollte ich die Frage stellen: Kriterien für die Einfachheit bez. Beweis der grössten Einfachheit von gewissen Beweisen führen. Ueberhaupt eine Theorie der Beweismethoden in der Mathematik entwickeln. Es kann doch bei gegebenen Voraussetzungen nur einen einfachsten Beweis geben. Ueberhaupt, wenn man für einen Satz 2 Beweise hat, so muss man nicht eher ruhen, als bis man sie beide aufeinander zurückgeführt hat oder genau erkannt hat, welche verschiedenen Voraussetungen (und Hilfsmittel) bei den Beweisen benutzt werden: Wenn man 2 Wege hat, so muss man nicht bloss diese Wege gehen oder neue suchen, sondern dass ganze zwischen den beiden Wegen liegende Gebiet erforschen. Ansätze diese Einfachheit der Beweise zu beurteilen, bieten meine Untersuchungen über Sysygien und Sysysien zwischen Sysygien. Die Benutzung oder Kenntnisse einer Sysygie vereinfacht den Beweis, dass eine gewisse | Identität richtig ist, erheblich. Da jeder Process des Addirens Anwendung des commutativen Gesetzes der Addition ist. – dies immer geometrischen Sätzen oder logischen Schlüssen entspricht, so kann man diese zählen und z.B. beim Beweis bestimmter Sätze in der Elementargeometrie (Pythagoras, oder über merkwürdige Punkte im Dreieck) sehr wohl entscheiden, welches der einfachste Beweis ist.” (Wissenschaftliches Tagebuch III/f, p. 25f.)

cccxcvi. “Apriori bei Kant heisst 1.) apodiktisch gültig 2.) den Gegenstandsbegriff konstituirend. Die 2^{te} Bedeutung stimmt mit obigen Ausführungen überein sie giebt dem Apriori die zentrale Stellung, die es seit | Kant hat: die grosse Entdeckung Kants, dass der Gegenstand d. Erkenntnis nicht schlechthin geg. sondern konstruirt ist, dass er begriffliche Elemente enthält, die in der reinen Wahrnehumung nicht enthalten sind. Die eindeutige Zuordnung der Erkenntnisth. ist eine eigentümliche Relation zwischen dem wirklichen Ding u dem Begriff,

indem erst durch die Zuordnung definirt wird, was in dem Kontinuum der Wirklichkeit ein Einzelding ist und weil erst der begriffliche Zusammenhang auf Grund von Wahrnehmung entscheidet, ob ein gedachtes Einzelding in Wirklichkeit da ist.

Die Relativitätsth. zeigt, dass gerade dasjenige Zuordnungssystem, welches (scheinbare) Evidenz hat, einen Widerspruch ergiebt. Wie soll es nun aber logisch überhaupt möglich sein, mit Erfahrungen, die mit dem alten, (scheinbar) evidenten Zuordnungsprinzipien gewonnen sind und die wir daher unmöglich alle aufgeben können, ein neues Zuordnungsprinzip zu beweisen? Dies geht, wenn das alte Prinzip als eine Näherung für gewisse einfache Fälle angesehen werden kann. Da die Erfahrungen doch nur Näherungen liefern, so ist es logisch Zulässig auf induktivem Wege zu neuen Zuordnungsprinzipien zu gelangen, die eine stetige Erweiterung der alten sind. Damit ist Kant widerlegt, der eine Aenderung der Zuordnungsprinzipien für allemal aussichtslos — während er andererseits an sich es nicht für ausgeschlossen hielt, dass Erkenntnis unmöglich wäre und sah es für eine besonderen Zufall an, dass die Natur eine solche Einfachheit und Regelmässigkeit besitzt, dass sie nach den Grundsätzen der menschlichen Vernunft geordnet werden kann. Nach Reichenbach hat Kant die Vernunft analysirt, anstatt die Erkenntnis und Kant hätte nach Axiomen suchen müssen anstatt nach Kategorieen." (Cod. Ms. D. Hilbert 601, sheet 1).

cccxcvii. "Das Reden über allgemeine Themata, die ich nicht ganz beherrsche, ist für mich ein nicht angenehmer Zustand und ich versetze mich in diesen Zustand daher nur im äussersten Notfall." (Wissenschaftliches Tagebuch II/b, p. 94).

cccxcviii. "Auch die geometrischen Sätze gehören meiner Ansicht nach hierher, als die Eigenschaften des [The manuscript originally had 'nach Riemann'] unsrigen Raums im Unterschied zu andern denkbaren Mannigfaltigkeiten nur aus der Erfahrung entnommen werden können vgl. Riemanns gesammelte Werke über die Hypothesen, welche der Geometrie zu Grunde liegen. Hiernach dürfte die Kantsche Behauptung der apriorischen Natur der mathem. Urteile auf die arithm. einzuschränken sein." (Cod. Ms. D. Hilbert 606, p. 3)

cccxcix. "Dadurch, dass mit den natürlichen Zahlen, denen das Wie-oft wesentlich ist, immer zusammengesetztere Operationen vorgenommen werden, entstehen so allerdings neue Formen von quantitativen Werthen, die in die natürlichen Zahlen *durch das einfache Wie-oft* sich nicht mehr einreihen lassen; deshalb aber bleiben die natürlichen Zahlen doch immer die *Elemente, an denen schliesslich* alle jene Operationen vorgenommen werden und die zu jenen höheren Formen von quantitativen Werthen die Grundlage bilden, […]. In rein *logischer* Hinsicht ist die natürliche Zahl die Grundlage; neue Kategorien treten durch die höheren Operationensformen nicht auf." (Günther Thiele. *Grundriss der Logik und Metaphysik*. Max Niemeyer, 1878, p. 113n7)).

cd. "Wie bereits das des öfteren begründet schließen wir uns in diesem Punkte vollkommen der Auffassung Kants an, welcher den Satz 7+5 = 12 für synthetisch erklärt. Auch den höheren arithm. u. algebraischen Operationen [The manuscript originally had 'Rechnungsarten', which has been struck out and replaced above the line with 'arithm. u. algebraischen Operationen'] beruhen im innersten Wesen auf dem gleichen Konstruiren und Denken in der sinnlichen Anschauung, wie die niedrigen Rechnungsarten." (Cod. Ms. D. Hilbert 606)

cdi. "Zu bedeuten freilich hat dies Anschauen im abstrakten Denken Nichts. Die *Ueberzeugung von der Wahrheit* der Urtheile stützt sich durchweg auf das Wesen der Begriffe, und zwar nicht etwa nur in der Metaphysik, sondern sogar in der Mathematik. Wer von der das Denken begleitenden Anschauung nicht loskommen kann, der studire absolute Geometrie, oder die Untersuchungen von Riemann und Helmholtz über die Thatsachen, die der Geometrie zu Grunde liegen. Diese Untersuchungen sind, wie wir uns ausdrücken können, das Unternehmen, die Grundanschauungen der Geometrie in *Begriffe* zu zerlegen, ihre sonst als selbstverständlich geltenden Grundlehren im Gewande des abstrakt mathematischen (analytischen) Denkens als nicht weiter bewiesene Annahmen erscheinen zu lassen; sie sind in Wahrheit der Anfang zu dem Versuche, das der Raumanschauung zu Grunde liegende Kategoriengerüst aufzuzeigen." (Thiele, *Grundriss der Logik und Metaphysik*, p. 47).

cdii. "Die Vielheit und Festigkeit dieser gegen|ständlichen Welt muss als das *erkannt* werden, was sie ist, als durch Kategorienthätigkeit verselbständigte und systematisierte Empfindung, und das dynamische Ganze muss durch Verstärkung seiner Einheit und damit ver-

trägliche Ermöglichung seiner inneren Vielheit und Lebendigkeit in sich selbst Halt bekommen; S muss sich erheben zur *Welt des Bewusstseins*." (Thiele, *Grundriss der Logik und Metaphysik*, pp. 167 sq.).

cdiii. "In diesem Begriffe wird nicht nur gedacht, dass A und B durch die in 'Wechselwirkung' erkannte Eine Kraft (K) mit einander *verbunden* sind, sondern, *dass A, B, und K nur Isolirungen, Theile in dem über sie übergreifenden Einen dynamischen Ganzen (G) sind, dass G Ein Wesen ist, in dem und durch das | K seine Begründung und Bestimmtheit hat.*" (Thiele, *Grundriss der Logik und Metaphysik*, pp. 162 sq.) The concept of the "dynamic whole" first appears at the end of the previous section, where Thiele writes: "Durch den Begriff der Wechselwirkung ist nun Alles vollkommen begreiflich [...]. Zugleich aber ist das System der Substanzen dadurch, dass die Begriffe 'Wirkung von B auf A', oder 'Wirkung von A auf B' etc. *als Einseitigkeiten erkannt* worden sind, aus einer blossen Summe zu einem einheitlichen und zwar *dynamischen Ganzen* geworden." (Thiele, *Grundriss der Logik und Metaphysik*, p. 161).

cdiv. "Die Einheit tragen wir in die Natur hinein, indem wir ein gewisses Thatsachenmaterial durch *ein* System von Axiomen zu beherrschen suchen z.B die geometrischen Thatsachen; aber abstrahiren wir von gewissen anderen Eigenschaften der Körper z.B von den physikalischen, was gewissermassen willkürlich ist und daher ist die Ablösung eines bestimmten Wissenschaft z.B der Geometrie nichts notwendiges; im Gegenteil in Wahrheit giebt es nur eine Wissenschaft, alles hängt zusammen." (Wissenschaftliches Tagebuch III/b, p. 112)

cdv. "[I]ndes wir vermuten, dass Kant auch in den Sätzen ['dass [die Gegenstände] *Eindrücke* auf die Sinne mache, dadurch Empfindungen *erregen*, und auf diese Weise Vorstellungen *zuwege* bringen'] noch in menschlicher Sprache zu uns reden will, dass die Sätze *nur unsere Auffassungsweise* des Ansichseins zum Ausdruck bringen sollen und – das ist ja eben Kriticismus. Kant hat also 'den Geist seines Systems' *nicht verlassen*, – vielmehr ist er in denselben, den Geist des Kriticismus, nicht tief genug eingedrungen [...]." (Günther Thiele. *Kant's Intellektuelle Anschaung als Grundbegriff seines Kriticismus.* Max Niemeyer, 1876, p. 264).

cdvi. "Die irrationale Zahl hat *einen ganz bestimmten Werth* (cf. Heine, [Die Elemente der] F[unctionenlehre] S. 2), aber er ist *nicht als Summe und Quotient* zu bestimmen, sondern eben *durch die Operation*, durch welche die betreffende Irrationalzahl definirt ist: [...]; der ebenfalls ganz bestimmte Werth (der Grenzwerth) einer irrationalen unendlichen Reihe, – vorausgesetzt natürlich, dass sie einen solchen überhaupt hat –, wird *erschöpfend* nur definirt entweder durch das Bildungsgesetz der Reihe, oder durch den Ausdruck, der in die unendliche Reihe entwickelt wird und zu dessen näherungsweiser Bestimmung die unendliche Reihe eben blosses Hülfsmittel ist." (Thiele, *Grundriss der Logik und Metaphysik*, p. 113n7)).

cdvii. "Dieses Formelspiel vollzieht sich nämlich nach gewissen bestimmten | Regeln, in denen die *Technik unseres Denkens* zum Ausdruck kommt. Diese Regeln bilden ein abgeschlossenes System, das sich auffinden und endgültig angeben läßt. Die Grundidee meiner Beweistheorie ist nichts anderes als die Tätigkeit unseres Verstandes zu beschreiben, ein Protokoll über die Regeln aufzunehmen, nach denen unser Denken tatsächlich verfährt." (David Hilbert. "Die Grundlagen der Mathematik." In: *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933.* Ed. by William Ewald and Wilfried Sieg. Vol. 3. David Hilbert's Lectures on the Foundations of Mathematics and Physics, 1891-1933. Springer, 2013, pp. 922–941, pp. 935 sq.).

cdviii. E.g. "Wir können aber einen entsprechenden Standpunkt gewinnen, indem wir uns auf eine höhere Stufe der Betrachtung begeben, von der aus die Axiome, Formeln und Beweise der mathematischen Theorien selbst Gegenstand einer inhaltlichen Überlegung sind. Dazu müssen aber zunächst die üblichen inhaltlichen Überlegungen der mathematischen Theorie durch Formeln und Regeln ersetzt bzw. durch Formalismen nachgebildet werden, d.h. es muß eine strenge Formalisierung der ganzen mathematischen Theorie einschließlich ihrer Beweise durchgeführt werden, so daß die mathematischen Schlüsse und Begriffsbildungen – nach dem Muster des Logikkalküls – in das Gebäude der Mathematik als formale Bestandteile einbezogen sind." (Hilbert, "Neubegründung der Mathematik," p. 165).

cdix. "Da uns das Denken nicht erspart bleiben darf, wir das logische inhaltliche Schliessen niemals entbehren können, so haben wir offenbar

das inhaltliche Schliessen falsch ausgeführt, wir haben notwendige Vorbedingungen für die Anwendung logischen Schliessens nicht innegehalten. Als Vorbedingung für die Anwendung logischer Schlüsse und die Betätigung logischer Operationen | muss sicherlich schon etwas in der Vorstellung gegeben sein: gewisse ausserlogische konkrete Objekte, die anschaulich als unmittelbares Erlebnis vor allem Denken da sind." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 744).

cdx. David Hilbert. "Über das Unendliche." In: *Grundlagen der Geometrie*. 7th ed. Teubner, 1930, pp. 162–188, p. 275.

cdxi. "Die Mathematik wie jede andere Wissenschaft kann nie durch Logik allein begründet werden; vielmehr ist als Vorbedingung für die Anwendung logischer Schlüsse und für die Betätigung logischer Operationen uns schon etwas in der Vorstellung gegeben: gewisse außerlogische konkrete Objekte, die anschaulich als unmittelbares Erlebnis vor allem Denken da sind." (Hilbert, "Die Grundlagen der Mathematik," p. 922).

cdxii. "Nur dann ferner ist eine Anschauung wahrhaft eine solche, '*durch die selbst* das Dasein des Objekts der Anschauung' gegeben wird, wenn zur Anschauung nichts hinzukommen, *keine schöpferische Kraft ausser oder in ihr* zu wirken braucht, damit sie ein für sich bestehender Gegenstand sei, wenn die Anschauung *unmittelbar als solche* reell seiender Gegenstand ist, wenn sie unmittelbar an sich selbst ihr Objekt ist, wenn Sein und Wissen identisch sind." (Thiele, *Kant's Intellektuelle Anschaung als Grundbegriff seines Kriticismus*, p. 94)

cdxiii. "Gegenstand und Inhalt der Philosophie ist allerdings nicht nur die Logik, sie hat vielmehr das *Wirkliche überhaupt* zu untersuchen, und dabei hat sie mit den einzelnen Wissen|schaften von dem *Thatsächlichen* auszugehen, sie hat zu versuchen, das *erfahrungsmässig Gegebene* in einem einheitlichen Ganzen zu begreifen. Aber die Verarbeitung des Thatsächlichen zur Theorie ist ohne die Kategorien, d.i. ohne die logischen Operationsformen des Verstandes eben so wenig möglich, als die Ausbildung unserer gegenständlichen Welt auf Grund und vermittelst der ursprünglich allein gegebenen Empfindungswelt. Diese Operationsformen aber haben *nicht nur äusserlich-formelle* Bedeutung, sie sind vielmehr von principiellem und damit von durchweg bestimmendem Werth, wie für die Natur der Dinge so auch für den *Inhalt* der wissenschaftlichen Lehren: so sind vor Allem die Voraussetzungen, deren keine Wissenschaft entbehrt, deren selbst die Mathematik bedarf [footnote: Vgl. *Riemann*, Ueber die Hypothesen, welche der Geom. zu Grunde liegen (W W. p. 254) *Helmholtz*, Ueber die Thatsachen, die der Geom. zum Grunde liegen (Gött. Nachrichten 1868; vgl. Populäre wissensch Vortr. III; Die Thatsachen in der Wahrnehmung 1879, p. 53). *G. Cantor*, Ein Beitrag zur Mannichfaltigkeitslehre (Crelle's Journal, 84. Bd.); Ueber unendl. lineare Punktmannichfaltigkeiten (Mathem. Annalen, XV. XVII. XX.) u. A.] und die nicht nur am Anfange, sondern durch das Ganze eines wissenschaftlichen Gebäudes sich geltend machen, aufzufassen als Folge aus dem Wesen des menschlichen Verstandes, als Aeusserungen der Kategorienthätigkeit. [footnote: S. m. Grundriss der Logik und Metaphysik, dargestellt als Entwicklung des endlichen Geistes 1878, § 2ff. 17. 30. 40. 53. 57. 64.]" (Günther Thiele. *Die Philosophie Immanuel Kants nach ihrem systematischen Zusammenhange und ihrer logisch-historischen Entwicklung dargestellt und gewürdigt. Kant's Vorkritische Naturphilosophie.* Vol. 1.1. Max Niemeyer, 1882, pp. 2 sq.; though this book is designated part 1 of volume 1, only the second part of volume 1 appeared ("Kants vorkritische Erkenntnistheorie", 1887). No further *volumes* were published. All in all Thiele thus covers only Kant's so called "pre-critical period").

cdxiv. "Er [Hilbert] versuchte damit [with his 'Neubegründung der Mathematik'], seiner Methode eine höhere Legitimation zu erteilen, eine Legitimation, die im Einklang mit bewährten historischen Prinzipien stünde, nach denen sich seiner Ansicht nach die Mathematik zwangsläufig entwickeln müßte. [...]. Wir wissen, daß Hilbert zu dieser Zeit unter perniciöser Anamie litt, einer damals sehr lebensbedrohlichen Krankheit, deren psychopathische Wirkung beim ihm schon bemerkbar war. Andererseits war Hilbert zum großen Teil seiner eigenen Legende verhaftet. Den bekannten Ausspruch aus seiner Pariser Rede von 1900 – [...] wiederholte er wörtlich in diesem Münsteraner Vortrag. Hilbert wollte immer an diese heile Welt glauben, und seine Bestrebungen, sie aufrecht zu erhalten, führten ihn zu beinahe wahnsinnigen Vorstellungen. Er meinte, seine Beweistheorie bilde 'den Schlußstein in dem Lehrgebäude der Axiomatik'. [...]." (David E. Rowe. "David Hilbert (1862-1943)." In: *Die Albertus-Universität zu Königsberg und ihre Professoren. Aus Anlaß*

der Gründung der Albertus-Universität vor 450 Jahren. Ed. by Dietrich Raschning and Donata v. Nerée. Vol. XXIX. Jahrbuch der Albertus Universität. Duncker & Humblot, 1995, pp. 544–552, pp. 549 sq.).

cdxv. "Somit geht jede richtige identische Gleichung wieder in eine richtige Gleichung über, wenn man jede Summe durch ein Produkt, jedes Produkt durch eine Summe ersetzt und 0 mit 1 vertauscht. Insbesondere gilt dies also von allen unter den *Axiomen* vorkommenden Gleichungen. So besteht z.B. neben dem distributiven Gesetz VIII $X(Y+Z) = XY + XZ$ ein zweites distributives Gesetz $X + YZ = (X+Y) \times (X+Z)$, welches aus dem vorigen durch dualistische Umformung hervorgeht. [Footnote:] In diesem zweiten distributiven Gesetz tritt die Abweichung des logischen Kalküls von dem Formalismus der Arithmetik besonders deutlich zutage." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 113).

cdxvi. "6') *Theorem*. Ebenso gilt das duale Gegenstück der Distributionsgesetze [Schröder gives two forms of each distributive law, which differ from one another by having commuted the arguments of the operation being distributed]: $a+bc = (a+b)(a+c)$ oder $bc+a = (b+a)(c+a)$. | Der Beweis dieses fernerhin von uns nicht wesentlich verwendeten Satzes ist erst weiter unten sub 10) [p. 12] zu führen." (Schröder, *Der Operationskreis des Logikkalkuls*, pp. 9 sq.).

cdxvii. "*Zustäze*. Wieder gestattet uns das Kommutationsgesetz, in den bewiesenen Formeln sowohl Faktoren als Glieder beliebig umzustellen, und dadurch denselben noch andere Gestalten zu geben. Namentlich sei hervorgehoben, dass auch: $ba + ca \eqslantless (b+c)a$ [und dual] $bc + a \eqslantless (b+a)(c+a)$ fortan gelten muss." (Schröder, *Vorlesungen über die Algebra der Logik*, pp. 280 sq.).

cdxviii. "Die Formel 27_+) werden wir '*das duale Gegenstück des Distributionsgesetzes*' nennen. [Footnote:] In ['Der Operationskreis des Logikkalküls'] glaubt ich, dieselbe [Formel] entdeckt zu haben; jedoch war mir Herr *Peirce* in ['On an improvement in Booole's calculus of logic'] zuvor bekommen." (Schröder, *Vorlesungen über die Algebra der Logik*, p. 285). I believe there is a typo in the printed text here, which actually read "Die Formel $27_\times$) […]", but from what Schröder goes on to say I believe he must mean to call "27_+)" the "dual of the distributive law".

cdxix. "Von Seiten ihres Verfassers Herrn Charles S. Peirce sind mir inzwischen die Seperatabzüge zweier Abhandlungen zugestellt und dadurch bekannt geworden [...]. Aus ['On an improvement in Boole's calculus of logic'] ersehe ich, dass bezüglich einiger wesentlichen Punkte in meine[m *Operationskreis*], nämlich in Hinsicht auf die Wahrnehmung der vorstehend angeführten doppelten Distributivität sowohl, als auch bezüglich der für logische Differenzen und Quotienten auf S. 29–31 von mir aufgestellten Ausdrücke, die Priorität Herrn Peirce zukommt." (Ernst Schröder. "Nachschrift [zu Seite 87]." In: *Repertorium der literarischen Arbeiten aus dem Gebiete der reinen und angewandten Mathematik*. Vol. 2. Teubner, 1879, p. 162, p. 162).

cdxx. "Von diesen beiden Sätzen [i.e. $27_\times$) $a \cdot (b+c) = (a \cdot b) + (a \cdot c)$ and 27_+) $a + (b \cdot c) = (a+b) \cdot (a+c)$] ist wol der letztere für die Technik des identischen Kalküls noch von einigem Werte. Wie sich zeigen wird, lässt aber die Anwendung des Th. 27_+) sich überhaupt umgehen, und kann man schon mit dem Distributionsgesetze $27_\times$) auskommen.

In der Arithmetik gilt die Formel 27_+) *nicht*; hier stehen Multiplikation und Addition nur in *einseitig distributivem Zusammenhange*: die | Multiplikation verhält sich distributiv zur Addition, aber nicht umgekehrt. *Im identischen Kalkul* dagegen *stehen Addition und Multiplikation in gegenseitig distributivem Zusammenhange*." (Schröder, *Vorlesungen über die Algebra der Logik*, pp. 285 sq.).

cdxxi. "Dieser Gedanke führt zur Aufstellung eines kombinierten Kalküls, dessen Beziehungen Aussagen-Gleichungen sind, in welchen als Glieder der symbolischen Ausdrücke teils die speziellen Aussagen 0, 1, teils Prädikaten-Relationen auftreten (die zur Deutlichkeit der Schreibweise zwischen Klammern zu setzen sind). Die Deutung der Formeln ergibt sich dabei ohne weiteres aus der Bedeutung der Prädikaten-Gleichungen und der Aussagen-Gleichungen. Beispielsweise besagt die Formel $(X + Y = Z) = 0$, dass die Prädikaten-Beziehung X + Y = Z zutrifft; $(X = 1) + (Y \to Z) = 1$ bedeutet, dass die Beziehungen $X = 1$ und $Y \to Z$ nicht beide zusammen erfüllt sind; $(X = 0) + (Y \to Z) \to (U \to V)$ besagt: 'aus dem Bestehen von $X = 0$ und dem Nicht-Bestehen von $Y \to Z$, folgt

das Nicht-Bestehen von $U \to V$'. Die identisch geltenden Beziehungen dieses zusammengesetzten Kalküls haben die Bedeutung von Sätzen des Prädikaten-Kalküls, und wir erhalten daher durch solche Identitäten formale Darstellungen von Definitionen, Axiomen und Lehrsätzen, die wir bisher nur sprachlich zum Ausdruck bringen konnten." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 132).

cdxxii. "So oft wir eine Aussage *in Rechnung setzen*, und zwar einerlei, ob sie dabei durch einen Buchstaben vertreten, oder ob sie vollinhaltlich, detaillirt (in einer Klammer) angegeben wird, soll sie als ihre *Gültigkeitsdauer* verstanden, ausgelegt werden. | Für die obigen Beispiele dürfen wir demnach sagen, dass: $a = \dot{1}$ und $b = 0$ ist. Anstatt – wie hiernach berechtigt – zu schreiben: $(2 \times 2 = 4) = \dot{1}$, wird man aber kürzer die Behauptung $2 \times 2 = 4$, oder a selbst, einfach hinstellen. Wogegen die Falschheit der Behauptung, dass 2×2 gleich 5 sei, vorerst nicht einfacher darzustellen ist als mittelst des Ansatzes: $(2 \times 2 = 5) = 0$." (Ernst Schröder. *Vorlesungen über die Algebra der Logik. Exakte Logik.* Vol. 2.1. Teubner, 1891, pp. 9 sq.).

cdxxiii. "Somit stellt sich unser Kalkül als ungenügend heraus, und wir sind genötigt, nach einer neuen Art der logischen Symbolik zu suchen." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 134).

cdxxiv. "Wir schreiben also, wenn $P(3)$ den Satz '3 ist eine Primzahl' darstellt, das Urteil '6 ist keine Primzahl' in der Form $\overline{P(6)}$. Zu beachten ist dabei, dass die Zeichen '$= 0$' und '$= 1$' des Aussagen-Kalküls hier wegfallen, indem jede gesondert aufgeschriebene Aussage bereits als Behauptung gilt." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 135).

cdxxv. Hilbert, "Elemente und Prinzipienfragen der Mathematik," p. 158.

cdxxvi. "Ist $a_1, a_2, \ldots, a_k, \ldots$ eine abzählbar unendliceh Reihe positiver ganzer Zahlen (vom Typus ω), so sollen die Dinge $(a_1, a_2, a_3, \ldots, a_k, \ldots)$ die als 'Kontinuum' bezeichnete Menge definieren. [...] Ein Element des Kontinuums soll 'endlich definiert' heißen, wenn wir mit Hilfe einer zur Fixierung unseres wissenschaftlichen Denkens geeigneten Sprache in endlicher Zeit ein Verfahren (Gesetz) angeben können, das jenes Element des Kontinuums von jedem anderen begrifflich sondern, oder – anders ausgedrückt – für ein beliebig gewähltes k die Existenz einer und nur einer zugehörigen Zahl a_k ergibt." (Julius König. "Über die Grundalgen der Mengenlehre und das Kontinuumsproblem." In: *Mathematische Annalen* 61 [1905], pp. 156–160, p. 156).

cdxxvii. "Man zeigt sehr leicht, daß *die endlich definierten Elemente des Kontinuums eine Teilmenge des Kontinuums von der Mächtigkeit* $\aleph_0$ *bestimmen*, die wir weiterhin mit E bezeichnen wollen. Eine solche endliche Definition muß nämlich durch eine endliche Zahl von Buchstaben und Interpunktionszeichen, die selbst nur in bestimmter, endlicher Zahl vorhanden sind, vollständig gegeben sein. Man kann weiter jene verschiedenen endlichen Definitionen wieder so anordnen, daß jeder beliebigen Definition eine und nur eine bestimmte positive ganze Zahl als (endliche) Ordnungszahl entspricht." (König, "Über die Grundalgen der Mengenlehre und das Kontinuumsproblem," p. 157)

cdxxviii. "Da aber das Kontinuum seiner Definition nach nicht abzählbar ist, muß es Elemente des Kontinuums geben, die nicht endlich definiert sein können." (König, "Über die Grundalgen der Mengenlehre und das Kontinuumsproblem," p. 157).

cdxxix. "Denken wir die Elemente des Kontinuums als wohlgeordnete Menge, so bilden jene Elemente, die nicht endlich definiert werden können, eine Teilmenge jener wohlgeordneten Menge, die gewiß Elemente des Kontinuums enthält. Diese Teilmenge ist aber dann auch wohlgeordnet, und enthält ein und nur ein erstes Element. Man beachte ferner, daß nach den jetzt gültigen Annahmen das Kontinuum, wie jede wohlgeordnete Menge, eine lückenlose Folge bestimmter Ordnungszahlen definiert; und zwar in der Weise, daß jedem Element des Kontinuums eine und nur eine solche Ordnungszahl entspricht, wie auch umgekehrt. Es ist demnach 'die einem endlich definierten Elemente des Kontinuums entsprechende Ordnungszahl', sowie auch 'das einer endlich definierten solchen Ordnungszahl entsprechende Element des Kontinuums' endlich definiert. Es müßte | demnach in jener Folge eine erste nicht endlich definierbare Ordnungszahl

vorhanden sein. Dies ist aber unmöglich. Es gibt nämlich eine bestimmte (wohlgeordnete) Menge endlich definierter Ordnungszahlen, die von der ersten ab lückenlos aufeinander folgen. 'Die der Größe nach auf alle diese zunächst folgende Ordnungszahl' wäre aber durch das Gesagt eben endlich definiert, während sie doch – der Annahme nach – nicht endlich definiert werden kann." (König, "Über die Grundalgen der Mengenlehre und das Kontinuumsproblem," pp. 158 sq.).

cdxxx. "Der Irrthum (welcher sich auch in der Note eines Herrn Richard im letzten Hefte der Acta mathematica findet, welche Note Herr Poincaré in den letzten Heften der Revue de Métaphysique et de Morale mit Emphase herausstreicht) ist, wie mir scheint, dieser: Es wird vorausgesetzt, dass das System (B) der Begriffe B, welche eventuell zur Definition von reellen Zahlenindividuen herangezogen werden müssen, ein endliches oder höchsten abzählbar unendliches sei. Diese Voraussetzung *muß ein Irrthum* sein, da sich sonst der *falsche Satz* ergeben würde: 'das Zahlencontinuum hat die Mächtigkeit $\aleph_0$'." (Cantor, *Briefe*, p. 446).

cdxxxi. "Ein gutes Beispiel für eine nicht prädikative Definition hat Russell gegeben: A sei die kleinste ganze Zahl, deren Definition mehr als hundert deutsche Worte erfordert. A muß existieren, da man mit hundert Worten jedenfalls nur eine endliche Anzahl von Zahlen definieren kann. Die Definition, die wir eben von dieser Zahl gegeben haben, enthält aber weniger als hundert Worte. Und die Zahl A ist also *definiert* asl *undefinierbar*." (Henri Poincaré. *Sechs Vorträge über ausgewählte Gegenstände aus der reinen Mathematik und mathematischen Physik*. Auf EInladung der Wolfskehl-Kommission der Königlichen Gesellschaft der Wissenschaften gehalten zu Göttingen vom 22.–28. April 1909. Teubner, 1910, p. 47).

cdxxxii. Hilbert, "Elemente und Prinzipienfragen der Mathematik," p. 158.

cdxxxiii. "Montrons que cette contradiction n'est qu'apparente. Revenons à nos arrangements. Le groupe de lettres G est un de ces arrangements; il existera dans mon tableau. Mais, à la place qu'il occupe, il n'a pas de sens. Il y est question de l'ensemble E, et celui-ci n'est pas encore défini. Je devrai donc le biffer. Le groupe G n'a de sense que si l'ensemble E est totalement défini, et celui-ci ne l'est que par un nombre infini de mots. *Il n'y a donc pas contradiction.*" (Jules Richard. "Les principes des Mathématiques et le problème des ensembles." In: *Revue générale des Sciences pures et appliquées* 16.12 [1905]. Ed. by Louis Olivier, pp. 541–543, p. 541).

cdxxxiv. In total, the contents of the sheet numbered 31/32 in the convolute Cod. Ms. D. Hilbert 601 are: "[31/32r] Zur Aufklärung des Paradoxons von der endlichen Wortzahl: 'Sinn haben' ist [31/32v] nicht definirt. Richtig ist dass jede vorgelegte Irrationalzahl durch eine endliche Anzahl deutscher Worte definirt werden kann, die eindeutig jene Irrationazahl beschreibt. Aber nicht umgekehrt giebt es eine Regel, vermöge der eindeutig entschieden werden kann, ob ein vorgelegter Wortsatz eine Irrationalz definirt.

Sprache allein ohne Experiment ist unverständlich. Also Kroneckersche Entscheidbarkeit muss hinzukommen.

[31/32'v] Der Widerspruch mit der endlichen Wortanzahl muss sich so klären, dass das Gesetz, das die Irrationalzahl definirt, erst eine bestimmte Bedeutung erhält d.h. eindeutig interpretierbar [struck out: wird wenn man (also die Bedeutung der n^{ten} Ziffer erst eindeutig geschehen kann] werden muss, so dass dann in Wahrheit doch so viele neue Operationen, Processe, oder Begriffe herauskommen, als es Zahlen der 2^{ten} Cantorschen Zahlenklasse giebt, weil es eben eindeutigen Interpretation d.h. Denkvorgängen immer auf die Reihenfolge d.h. auf die Anordnung ankommt.".

cdxxxv. "*Erstes Paradoxon*: S. 199-203 [of Hellinger's notes for the 1905 lecture course] Aufklärung allenfalls, indem man den Begriff der Worte oder des Wortlautes anzweifelt, der doch nicht einen von der Interpretation unabhängigen Sinn zu haben braucht." (Hilbert, "Ms. Cod. D. Hilbert 559," 26^r).

cdxxxvi. Hilbert, "Ms. Cod. D. Hilbert 559," 30^v-31^r.

cdxxxvii. At the start of the 1922/23 lectures Hilbert presents two paradoxes, of which this is the second. Later on in his notes, Kneser records the following remark: "Die Paradoxien rufen uns Halt zu. Die zweite (Richard) soll uns einen Fingerzeig geben, wie wir weiterzukommen

versuchen werden. Wir müssen die Sprache formalisieren und präzisieren, mit Hilfe des von Mathematikern und der Mathematik fernstehenden Philosophen vielfach mißachteten *Logikkalküls*." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 602). The corresponding passage in the official lecture notes reads: "So weisen die beiden Paradoxien *zwei Richtungen*, nach denen wir das Problem der strengen Begründung der Mathematik in Angriff zu nehmen haben: *Einhaltung des Standpunktes der finiten Logik und Präzisierung der Sprache*. [...] | Dieser zweite Gesichtspunkt bestand in dem Hinweis auf das Problem der Präzisierung der Sprache. Diese Aufgabe ist seit langem und mit Erfolg in Angriff genommen durch den *Logikkalkül*, in welchem diejenigen Bestandteile der Sprache, welche für die Mitteilung mathematischer Gedanken in Betracht kommen, einer strengen Formalisierung von der Art des mathematischen Formalismus unterzogen werden. Der Logikkalkül ist von den Mathematikern viel missachtet worden,und er erscheint auch zunächst als eine unfruchtbare Spielerei." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, pp. 531 sq.).

cdxxxviii. Kneser records Bernays (!) saying at the end of the 1923/24 lectures: "So [by admitting more involved recursion-schemata than primitive recursion on a single variable] kann man noch mehr einführen; in jedem Falle wäre dann die Reduktion der Funktionale durchzuführen. Hier bekommen wir mehr als abzählbar [viele Funktionale?]; das klärt uns zugleich das Richardsche Paradoxon auf. Wir werden nämlich die *Zahlen der 2. Zahlklasse* als *Funktionale* durch *Rekursion* einführen." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 630). At the end of Kneser's notes for the 1923/24 notes, it is similarly asserted (in a lecture held by Hilbert himself this time): "Auf der sechsten Stufe hatten wir die Axiome 18 der Rekursion [i.e. the scheme for the definition by primitive recursion on p. 639]. Man kann damit vieles machen; weiß aber nicht, ob man nicht durch die Entwicklung gezwungen wird, höhere Funktionstypen, Rekursionstypen heranzuziehen. Diese Unsicherheit soll die Siebente Stufe beseitigen (jedenfalls zum Teil). [Hilbert introduces the symbol 'Φ' for 'zahlentheoretische Funktion sein'] | Dann (nämlich wenn man diesen Gedanken ausführt) braucht man nicht mehr verschiedene Sorten von Variablem. Man darf 'alles' einsetzen. Dadurch soll das Richardsche Paradoxon aufgeklärt werden." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, pp. 642 sq.).

cdxxxix. "Das Auswahlprinzip z.B. kann bei richtigem Schreiben (formale Anwendung der Schreibungsregeln) mittels Experimentierens nie zum Widerspruch führen. Math. Experiment = Exp., welches nie misslingen kann immer sicher und zutreffend, wenn man nur aufpasst. Was heisst aufpassen?, aufmerksam sein? richtig die Vorschriften beim Experiment anwenden. Ist da zur Kontrolle ein Uebergeist nöthig?" (Wissenschaftliches Tagebuch III, p. 100)

cdxl. "Beweise, dass, wenn man durch einen solchen allgemeinen [added above the line: '(transfiniten)'] Satz (z.B. durch das Auswahlprinzip) einen gewöhnlichen Satz (z.B. der Zahlentheorie oder Funktionentheorie) findet, dieser auch direkt beweisbar sein muss (z.B. ohne das Auswahlprinzip) d.h. Elimination, indirekter, direkter Beweis. Darin allein würde schon ein hoher Wert solcher transfiniten Prinzipien liegen." (Wissenschaftliches Tagebuch III, p. 100)

cdxli. "Zermelos Auswahlaxiom möchte ich so formuliren: Wenn M eine Menge von Mengen ist, die [originally, but struck out: 'deren Axiome'] nicht einen Widerspruch einschliesst, so thut sie es auch nicht, wenn wir das Auswahlaxiom hinzufügen." (Wissenschaftliches Tagebuch III, p. 99)

cdxlii. "Ich stelle mich damit in bewußten Gegensatz zu der Annahme, daß es *nicht* gestattet ist, über 'endliche Gesetze' hinauszugehen. Mit dieser Annahme wird meiner Ansicht nach die Existenz des Kontinuums und des Kontinuumsproblems geleugnet. Die hier genützte Annahme ist im Gegenteil die, daß es Elemente des Kontinuums gibt, die wir nicht 'zu Ende denken' können, und die 'trotzdem' widerspruchsfrei, also, wenn der Ausdruck in allerdings ganz neuem Sinne gestattet ist, 'ideale' Elemente sind." (König, "Über die Grundalgen der Mengenlehre und das Kontinuumsproblem," p. 158).

cdxliii. "Wenn wir einen Widerspruch haben, so muß, da unser Denken ein finiter Proceß ist, ein (endlicher) Beweis dafür vorliegen. Wird in

diesem Beweise die Auswahl benutzt, so muß uns *der Beweis selbst* die Auswahl liefern. Es wird nicht behauptet, daß man stets die Auswahl treffen kann; wohl aber kann man ohne einen Widerspruch zu riskieren, *tun* als sei sie getroffen. Die ausgewählten Elemente spielen dieselbe Rolle wie z.B. die idealen Elemente (∞ fern, imaginär)." (Hilbert, *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*, p. 628).

A somewhat expanded and revised «version» of this paragraph is found in David Hilbert. "Die logischen Grundlagen der Mathematik." In: *Gesammelte Abhandlungen. Analysis, Grundlagen der Mathematik, Physik, Verschiedenes, nebst einer Lebensgeschichte*. Vol. 3. Springer Verlag, 1935, pp. 178–191, p. 187: "Man kann die Lösung der Schwierigkeit, wie sie meine Beweistheorie gibt, sich so begreiflich machen: Unser Denken ist finit; indem wir denken, geschieht ein finiter Proceß. Diese sich von selbst bestätigende Wahrheit wird in meiner Beweistheorie gewissermaßen mit benutzt in der Weise, daß, wenn irgendwo sich ein Widerspruch herausstellen würde, mit der Erkenntnis dieses Widerspruchs auch zugleich die betreffende Auswahl aus unendlich vielen Dingen verwirklicht sein müßte. In meiner Beweistheorie wird demnach nicht behauptet, daß die Auffindung eines Gegenstandes unter den unendlich vielen Gegenständen stets bewirkt werden kann, wohl aber, daß man ohne Risiko eines Irrtums stets so tun kann, als wäre die Auswahl getroffen."

Bibliography

Abrusci, Vito Michele. "David Hilbert's *Vorlesungen* on Logic and Foundations of Mathematics." In: *Atti del Convegno Internazionale di Storia della Logica. Le teorie delle modalità*. Bologna: CLUEB, pp. 333–338.

— "Hilbert e i fondamenti della mathematica. Il methodo assimatico." In: *Le matematiche* 55 (Supplement 1), pp. 127–142.

— "Per una caratterizzazione del programma hilbertiano." In: *Rivista di filosofia* 66.3 (1975), pp. 315–338.

Bauch, Bruno. "Erfahrung und Geometrie in ihrem erkenntnistheoretischen Verhältnis." In: *Kant-Studien* 12 (1907), pp. 213–235.

Behmann, Heinrich. "Beiträge zur Algebra der Logik, insbesondere zum Entscheidungsproblem." In: *Mathematische Annalen* 86 (1922), pp. 163–229.

— *Die Antinomie der transfiniten Zahl und ihre Auflösung durch die Theorie von Russell und Whitehead*. Inauguraldissertation (Tag der mündlichen Prüfung: 5. Juni 1918). 1918. SB Berlin, signature 50/1918; alternative: 03/4346.

Benjamin, Walter. *Ursprung des deutschen Trauerspiels*. In: *Gesammelte Schriften I.1*. Suhrkamp, 1974, pp. 203–430.

Blanchette, Patricia. "Frege and Hilbert on Consistency." In: *The Journal of Philosophy* XCIII.7 (1996), pp. 317–336.

Bloor, David. *Knowledge and Social Imagery*. 2nd ed. University of Chicago Press, 1991.

Blumenthal, Otto. "Lebensgeschichte [David Hilberts]." In: Hilbert, David. *Gesammelte Abhandlungen. Analysis, Grundlagen der Mathematik, Physik, Verschiedenes, nebst einer Lebensgeschichte*. Vol. 3. Springer Verlag, 1935, pp. 388–429.

Cantor, Georg. *Briefe*. Ed. by Herbert Meschkowski and Winfried Nilson. Springer, 1991.

Carnap, Rudolf. *Logische Syntax der Sprache*. Springer-Verlag, 1968.

Cassirer, Ernst. *Substanzbegriff und Funktionsbegriff. Untersuchungen über die Grundlagen der Erkenntniskritik*. Ed. by Reinhold Schmücker. Vol. 6. Hamburger Ausgabe der gesammelten Werke Ernst Cassirers. Meiner Verlag, 2000.

Davidson, Donald. "Theories of Meaning and Learnable Languages." In: *Inquiries into Truth and Interpretation*. Second Edition. Oxford University Press, 2001, pp. 3–15.

— *Truth and Predication*. Harvard University Press, 2005.

Dedekind, Richard. *Was sind und was sollen die Zahlen — Stetigkeit und Irrationale Zahlen*. Springer, 1965.

Delbœuf, Joseph. *Prolégomènes philosophiques de la géométrie et solution des postulates*. J. Desoer, 1860.

Detlefsen, Michael. "Completeness and the Ends of Axiomatization." In: *Interpreting Gödel*. Ed. by Juliette Cara Kennedy. Cambridge University Press, 2014, pp. 59–77.

— "Constructive Existence Claims." In: *The Philosophy of Mathematics Today*. Ed. by Matthias Schrin. Oxford University Press, 2005, pp. 307–335.

— "Hilbert's Formalism." In: *Revue Internationale de Philosophie* 47.186 (4 1993), pp. 285–304.

Dummett, Michael. *Frege: Philosophy of Language*. Second Edition. Duckworth, 1981.

Ebbinghaus, Heinz-Dieter. "Zermelo and the Heidelberg Congress 1904." In: *Historia Mathematica* 34 (2007), pp. 428–432.

Edwards, Harold M. "Kronecker's Place in History." In: *History and Philosophy of Modern Mathematics*. Ed. by William Aspray and Philip Kitcher. University of Minnesota Press, 1988, pp. 139–144.

Erdmann, Benno. *Die Axiome der Geometrie. Eine philosophische Untersuchung der Riemann-Helmholtz'schen Raumtheorie*. Verlag von Leopold Voss, 1877.

Ewald, William. "Hilbert's Wide Program." In: *Logic Colloquium 2000*. Ed. by R Cori et al. 2005, pp. 228–251.

Feferman, Solomon. In: *In the Light of Logic*. Oxford University Press, 1998, pp. 3–27.

Frege, Gottlob. *Begriffsschrift und andere Aufsätze*. Ed. by Ignacio Angelelli. 2nd ed. Verlag Georg Olms, 2007.

— *Grundgesetze der Arithmetik*. Vol. 1. Verlag von Hermann Pohle, 1893.

— *Grundgesetze der Arithmetik*. Vol. 2. Verlag von Hermann Pohle, 1903.

— *Kleine Schriften*. Ed. by Ignacio Angelelli. Georg Olms Verlagsbuchhandlung, 1967.

— *Nachgelassene Schriften*. Ed. by Hans Hermes, Friedrich Kambartel, and Friedrich Kaulbach. Zweite, revidierte Auflage. Meiner Verlag, 1983.

— "Über die Grundlagen der Geometrie." In: ed. by Ignacio Angelelli. Georg Olms Verlagsbuchhandlung, 1967, pp. 263–266.

— "Über Sinn und Bedeutung." In: ed. by Ignacio Angelelli. Georg Olms Verlagsbuchhandlung, 1967, pp. 143–162.

— *Wissenschaftlicher Briefwechsel*. Ed. by Gottfried Gabriel et al. Felix Meiner Verlag, 1976.

Friedman, Michael. *Kant and the Exact Sciences*. Harvard University Press, 1992.

— "Kant's Theory of Geometry." In: *The Philosophical Review* 94.4 (1985), pp. 455–506.

Genocchi, Angelo. *Differentialrechnung und Grundzüge der Integralrechnung*. Autorisierte Deutsche Übersetzung. Ed. by Giuseppe Peano. Trans. by G. Bohlmann and A. Schepp. With a forew. by A Mayer. Verlag von B.G. Teubner, 1899.

Gödel, Kurt. "Über formal unentscheidbare Sätze der 'Principia Mathematica' und verwandter Systeme I." In: *Monatshefte für Mathematik und Physik* 38 (1931), pp. 173–198.

Gray, Jeremy. *Plato's Ghost. The Modernist Transformation of Mathematics*. Princeton University Press, 2009.

Gutzmer, August, ed. *Jahresbericht der Deutschen Mathematiker-Vereinigung*. Vol. 19. 1910.

Hallett, Michael. "Frege and Hilbert." In: *The Cambridge Companion to Frege*. Ed. by Thomas Ricketts and Michael Potter. Cambridge University Press, 2010, pp. 413–464.

— "Frege on Creation." In: *Essays on Frege's Basic Laws of Arithmetic*. Ed. by Marcus Rossberg and Philip A. Ebert. Oxford University Press, 2019, pp. 285–324.

— "Hilbert and Logic." In: *Québec Studies in the Philosophy of Science. Part I: Logic, Mathematics, Physics and the History of Science*. Ed. by Mathieu Marion and Robert S. Cohen. Essays in Honor of Hugues Leblanc. Kluwer, 1995, pp. 135–188.

— "Hilbert's Axiomatic Method and the Laws of Thought." In: *Mathematics and Mind*. Ed. by Alexander George. Oxford University Press, 1994, pp. 158–200.

— "More on Frege and Hilbert." In: *Analysis and Interpretation in the Exact Sciences. Essays in Honour of William Demopoulos*. Ed. by Melanie Frappier, Derek Brown, and Robert DiSalle. 2012, pp. 135–162.

Hallett, Michael and Oran Magal. *The Ideal in Hilbert's Grundlagen der Geometrie*. Draft. 2015.

Hankel, Hermann. *Vorlesungen über die Complexen Zahlen und Ihre Funktionen. In Zwei Theilen: I. Theil: Theorie der Complexen Zahlensysteme*. Leopold Voss, 1867.

Heck, Richard Kimberly. *Frege's Theorem*. Oxford University Press, 2011. (Originally published under the name "Richard G. Heck, Jr".)

Heijenoort, Jean van. "Logic as calculus and logic as language." In: *Selected Essays*. Bibliopolis, 1985, pp. 11–16.

Schlick, Moritz and Paul Hertz, eds. *Schriften zur Erkenntnistheorie*. Springer Verlag, 1921.

— "Über den Ursprung und die Bedeutung der geometrischen Axiome." In: *Schriften zur Erkenntnistheorie*. Ed. by Moritz Schlick and Paul Hertz. Springer Verlag, 1921, pp. 1–24.

— "Zählen und Messen, erkenntnistheoretisch betrachtet." In: *Schriften zur Erkenntnistheorie*. Ed. by Moritz Schlick and Paul Hertz. Springer Verlag, 1921, pp. 70–97.

Herbart, Johann Friedrich. *Lehrbuch zur Einleitung in die Philosophie*. Ed. by Wolfhart Henckmann. Meiner, 1993.

— *Lehrbuch zur Psychologie*. In: *Sämtliche Werke*. Vol. 4. Hermann Beyer & Söhne, 1891, pp. 295–436.

Hertz, Paul. "Axiomensystem für beliebige Satzsysteme I." In: *Mathematische Annalen* 87 (1922), pp. 246–269.

Hertz, Paul. "Über die Axiomensysteme kleinster Satzzahl für ein System von Sätzen und den Begriff des idealen Elements." In: *Jahresbericht der Deutschen Mathematiker-Vereinigung* 31 (1922), [154–157.

Hilbert, David. "Axiomatisches Denken." In: *Gesammelte Abhandlungen. Analysis, Grundlagen der Mathematik, Physik, Verschiedenes, nebst einer Lebensgeschichte*. Vol. 3. Springer Verlag, 1918, pp. 146–156.

— *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*. Ed. by William Ewald and Wilfried Sieg. Vol. 3. David Hilbert's Lectures on the Foundations of Mathematics and Physics, 1891-1933. Springer, 2013.

— *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*. Ed. by Michael Hallett and Ulrich Majer. Vol. 1. David Hilbert's Lectures on the Foundations of Mathematics and Physics, 1891-1933. Springer, 2003.

— *David Hilbert's Lectures on the Foundations of Physics 1915-1927. Relativity, Quantum Theory and Epistemology*. Ed. by Tilman Sauer and Ulrich Majer. Vol. 5. David Hilbert's Lectures on the Foundations of Mathematics and Physics, 1891-1933. Springer, 2009.

— *Die Grundlagen der Geometrie. The* Festschrift *of 1899*. In: *David Hilbert's Lectures on the Foundations of Geometry 1891-1902*. Ed. by Michael Hallett and Ulrich Majer. Vol. 1. David Hilbert's Lectures on the Foundations of Mathematics and Physics, 1891-1933. Springer, 2003, pp. 407–529.

— "Die Grundlagen der Mathematik." In: *David Hilbert's Lectures on the Foundations of Arithmetic and Logic 1917-1933*. Ed. by William Ewald and Wilfried Sieg. Vol. 3. David Hilbert's Lectures on the Foundations of Mathematics and Physics, 1891-1933. Springer, 2013, pp. 922–941.

— "Die logischen Grundlagen der Mathematik." In: *Gesammelte Abhandlungen. Analysis, Grundlagen der Mathematik, Physik, Verschiedenes, nebst einer Lebensgeschichte*. Vol. 3. Springer Verlag, 1935, pp. 178–191.

— *Gesammelte Abhandlungen. Analysis, Grundlagen der Mathematik, Physik, Verschiedenes, nebst einer Lebensgeschichte*. Vol. 3. Springer Verlag, 1935.

— *Grundlagen der Geometrie*. 7th ed. Teubner, 1930.

— *Natur und mathematisches Erkennen. Vorlesungen, gehalten 1919-1920 in Göttingen*. Ed. by David E. Rowe. Birkhäuser, 1992.

— "Neubegründung der Mathematik. Erste Mitteilung." In: *Gesammelte Abhandlungen. Analysis, Grundlagen der Mathematik, Physik, Verschiedenes, nebst einer Lebensgeschichte*. Vol. 3. Springer Verlag, 1935, pp. 157–177.

— "Über das Unendliche." In: *Grundlagen der Geometrie*. 7th ed. Teubner, 1930, pp. 162–188.

— "Über den Zahlbegriff." In: *Grundlagen der Geometrie*. 7th ed. Teubner, 1904, pp. 241–246.

Hilbert, David and Paul Bernays. *Grundlagen der Mathematik*. Vol. 1. Springer, 1934.

Hintikka, Jaakko. "Kant's 'New Method of Thought' and his Theory of Mathematics." In: *Knowledge and the Known. Historical Perspectives in Epistemology*. Kluwer Academic Publishers, 1991, pp. 126–134.

Hodges, Wilfrid. "Truth in a Structure." In: *Proceedings of the Aristotelian Society* 86.1 (1986), pp. 135–152.

Houser, Nathan. "The Schröder-Peirce Correspondence." In: *Modern Logic* 1.2-3 (1990-1991), pp. 206–236.

Huntington, Edward Vermilye. "Complete sets of postulates for the theories of positive integral and positive rational numbers." In: *Transactions of the American Mathematical Association* 3.2 (1902), pp. 280–284.

— "Complete sets of postulates for the theory of real quantities." In: *Transactions of the American Mathematical Association* 4.3 (1903), pp. 358–270.

Kahle, Richard. "David Hilbert and *Principia Mathematica*." In: *The Palgrave Centenary Companion to* Principia Mathematica. Ed. by Nicolas Griffin and Bernard Linsky. Palgrave Macmillan, 2013, pp. 21–34.

— "Poincaré in Göttingen." In: *Poincaré, Philosopher of Science. Problems and Perspectives*. Springer, 2014, pp. 83–99.

Kambartel, Friedrich. "Frege und die axiomatische Methode. Zur Kritik mathematikhistorischer Legitimationsversuche der formalistischen Ideologie." In: *Studien zu Frege. Band I: Logik und Philosophie der Mathematik*. Ed. by Matthias Schirn. Problemata 42. Frommann-Holzboog, 1976, pp. 215–228.

Kant, Immanuel. *Kritik der reinen Vernunft*. Ed. by Jens Timmermann. Philosophische Bibliothek 505. Meiner Verlag, 1998.

Kitcher, Philip. "Hilbert's Epistemology." In: *Philosophy of Science* 43 (1976), pp. 99–115.

— *The Nature of Mathematical Knowledge*. Oxford University Press, 1983.

Klein, Felix. *Elementarmathematik vom höheren Standpunkte aus. Erster Band: Arithmetik, Algebra, Analysis*. Ed. by Ernst Hellinger and Friedrich Seyfarth. Vierte Auflage. Springer-Verlag, 1933.

— eds. *Gesammelte Mathematische Abhandlungen. Liniengeometrie, Grundlegung der Geometrie, zum Erlanger Program*. Vol. Erster Band. Springer, 1921.

— "Über die sogenannte Nicht-Euklidische Geometrie (erster Aufsatz)." In: *Gesammelte Mathematische Abhandlungen. Liniengeometrie, Grundlegung der Geometrie, zum Erlanger Program*. Ed. by R. Fricke and A. Ostrowski. Vol. Erster Band. Springer, 1921, pp. 254–305.

— "Über die sogenannte Nicht-Euklidische Geometrie (Vorläufige Mitteilung." In: *Gesammelte Mathematische Abhandlungen. Liniengeometrie, Grundlegung der Geometrie, zum Erlanger Program*. Ed. by R. Fricke and A. Ostrowski. Vol. Erster Band. Springer, 1921, pp. 244–253.

— "Über die sogenannte Nicht-Euklidische Geometrie (zweiter Aufsatz)." In: *Gesammelte Mathematische Abhandlungen. Liniengeometrie, Grundlegung der Geometrie, zum Erlanger Program*. Ed. by R. Fricke and A. Ostrowski. Vol. Erster Band. Springer, 1921, pp. 311–343.

— "Vergleichende Betrachtungen über neuere geometrische Forschungen." In: *Gesammelte Mathematische Abhandlungen. Liniengeometrie, Grundlegung der Geometrie, zum Erlanger Program*. Ed. by R. Fricke and A. Ostrowski. Vol. Erster Band. Springer, 1921, pp. 460–497.

König, Julius. "Über die Grundalgen der Mengenlehre und das Kontinuumsproblem." In: *Mathematische Annalen* 61 (1905), pp. 156–160.

Korselt, Alwin. "Über die Grundlagen der Geometrie." In: *Jahresberichte der Deutschen Mathematiker-Vereinigung* 12 (1903), pp. 402–407.

— "Über die Grundlagen der Mathematik." In: *Jahresberichte der Deutschen Mathematiker-Vereinigung* 14 (1905), pp. 465–388.

— "Über die Logik der Geometrie." In: *Jahresberichte der Deutschen Mathematiker-Vereinigung* 17 (1908), pp. 98–124.

— "Über Mathematische Erkenntnis." In: *Jahresberichte der Deutschen Mathematiker-Vereinigung* 20 (1911), pp. 464–379.

Kreisel, Georg. "Hilbert's Programme." In: *Dialectica* 12 (3-4 1958), pp. 346–372.

— "On the concepts of completeness and interpretation of formal systems." In: *Fundamenta mathematicae* 39 (1953), pp. 103–127.

Kreiser, Lothar. "Die Hörer Freges und sein Briefpartner Alwin Korselt." In: *Wittgenstein-Studien* 2.1 (1995), pp. 1–106.

Kripke, Saul A. *Naming and Necessity*. Harvard University Press, 1972.

Künne, Wolfgang. "'Die Ernte wird erscheinen...' Die Geschichte der Bolzano-Rezeption (1848-1939)." In: *Versuche über Bolzano*. Beiträge zur Bolzano-Forschung 21. Academia Verlag, 2008, pp. 305–404.

Lakatos, Imre. "What does a mathematical proof prove?" In: *Mathematics, Science and Epistemology. Philosophical Papers Volume 2*. 1978, pp. 61–69.

Majer, Ulrich. "Hilbert, Reichenbach und der Neu-Kantianismus." In: *Hans Reichenbach und die Berliner Gruppe*. Ed. by Lutz Danneberg, Andreas Kamlah, and Lothar Schäfer. Vieweg, 1994, pp. 253–273.

— "Hilbert's Methode der Idealen Elemente und Kants regulativer Gebrauch der Ideen." In: *Kant-Studien* 84.1 (1993).

— "Husserl and Hilbert on Completeness. A Neglected Chapter in Early Twentieth Century Foundations of Mathematics." In: *Synthese* 110 (1997), pp. 37–56.

Mancosu, Paolo. "Between Russell and Hilbert. Behmann on the Foundation of Mathematics." In: *The Adventure of Reason. Interplay between Philosophy of Mathematics and Mathematical Logic 1900-1940*. Oxford University Press, 2010, pp. 159–175.

— "Hilbert and Bernays on Metamathematics." In: *The Adventure of Reason. Interplay between Philosophy of Mathematics and Mathematical Logic 1900-1940*. Oxford University Press, 2010, pp. 125–158.

Mancosu, Paolo. "On the Constructivity of Proofs. A Debate among Behmann, Bernays, Gödel, and Kaufmann." In: *The Adventure of Reason. Interplay between Philosophy of Mathematics and Mathematical Logic 1900-1940*. Oxford University Press, 2010, pp. 199–216.

The Adventure of Reason. Interplay between Philosophy of Mathematics and Mathematical Logic 1900-1940. Oxford University Press, 2010.

Mancosu, Paolo. "The Russellian Influence on Hilbert and His School." In: *The Adventure of Reason. Interplay between Philosophy of Mathematics and Mathematical Logic 1900-1940*. Oxford University Press, 2010, pp. 176–198.

Mancosu, Paolo and Richard Zach. "Heinrich Behmann's 1921 lecture on the decision problem and the algebra of logic." In: *Bulletin of Symbolic Logic* 21.2 (2015), pp. 164–187.

Mancosu, Paolo, Richard Zach, and Calixto Badesa. "The Development of Mathematical Logic from Russell to Tarski, 1900-1935." In: *The Adventure of Reason. Interplay between Philosophy of Mathematics and Mathematical Logic 1900-1940*. Oxford University Press, 2010, pp. 5–119.

Marion, Mathieu. "Kronecker's 'Safe Haven of Real Mathematics'." In: *Québec Studies in the Philosophy of Science. Part I: Logic, Mathematics, Physics and the History of Science*. Ed. by Mathieu Marion and Robert S. Cohen. Essays in Honor of Hugues Leblanc. Kluwer, 1995, pp. 189–215.

Marion, Mathieu and Robert S. Cohen, eds. Essays in Honor of Hugues Leblanc. Kluwer, 1995.

Moore, Gregory H. "The Emergence of First-Order Logic." In: *History and Philosophy of Modern Mathematics*. 1988, pp. 95–135.

Parsons, Charles D. "Kant's Philosophy of Arithmetic." In: *Mathematics in Philosophy. Selected Essays*. Cornell University Press, 2005, pp. 110–149.

Pasch, Moritz. *Vorlesungen über Neuer Geometrie*. 2nd ed. Teubner, 1912.

Peckhaus, Volker. "Alwin Reinhold Korselt." In: *Arbeitsberichte aus dem DFG-Projekt Fallstudien zur Begründung einer Sozialgeschichte der Logik* 6 (1986). Ed. by Volker Peckhaus, pp. 10–24.

— "Ernst Schröder und die 'pasigraphischen Systeme' von Peano und Peirce." In: *Modern Logic* 1.2-3 (1991), pp. 174–205.

— *Hilbertprogramm und Kritische Philosophie. Das Göttinger Modell interdisziplinärer Zusammenarbeit zwischen Mathematik und Philosophie*. Vandenhoeck & Rupprecht, 1990.

— "Hilberts Logik. Von der Axiomatik zur Beweistheorie." In: *Internationale Zeitschrift für Geschichte und Ethik der Naturwissenschaften, Technik und Medizin* 3 (1995), pp. 65–86.

— "Karl Eugen Müller (1865–1932) und seine Rolle in der Entwicklung der Algebra der Logik." In: *History and Philosophy of Logic* 9.1 (1988), pp. 43–56.

Peckhaus, Volker. "Logic in Transition. The Logical Calculi of Hilbert (1905) and Zermelo (1908)." In: *Logic and Philosophy of Science in Uppsala*. Ed. by Dag Prawitz and Dag Westerståhl. 1994.

— "Schröder's Logic." In: *Handbook of the History of Logic. The Rise of Modern Logic: from Leibniz to Frege*. Ed. by Dov M. Gabbay and John Woods. Vol. 3. Elsevier, pp. 557–609.

— "The axiomatic method and Ernst Schröder's algebraic approach to logic." In: *Philosophia Scientiae* 1.3 (1996), pp. 1–15.

Peirce, Charles Sanders. "Description of a Notation for the Logic of Relatives, resulting from an Amplification of the Conception of Boole's Calculus of Logic." In: *Writings of Charles S. Peirce: A chronologial edition. Volume 2: 1867-1871*. Ed. by Edward C. Moore. Indiana University Press, 1984, pp. 359–429.

— "On an Improvement in Boole's Calculus of Logic." In: *Writings of Charles S. Peirce: A chronologial edition. Volume 2: 1867-1871*. Ed. by Edward C. Moore. Indiana University Press, 1984, pp. 12–22.

— "On the Algebra of Logic." In: *Writings of Charles S. Peirce: A chronologial edition. Volume 4: 1879-1884*. Ed. by Christian J. W. Kloesel. Indiana University Press, 1986, pp. 163–209.

— "Questions Concerning Certain Faculties Claimed for Man." In: *Writings of Charles S. Peirce: A chronologial edition. Volume 2: 1867-1871*. Ed. by Edward C. Moore. Indiana University Press, 1984, pp. 193–211.

— "The Probability of Induction." In: *Writings of Charles S. Peirce: A chronologial edition. Volume 3: 1872-1878*. Ed. by Christian J. W. Kloesel. Indiana University Press, 1986, pp. 290–305.

— "Upon the Logic of Mathematics." In: *Writings of Charles S. Peirce: A chronologial edition. Volume 2: 1867-1871*. Ed. by Edward C. Moore. Indiana University Press, 1984, pp. 59–69.

— *Writings of Charles S. Peirce: A chronologial edition. Volume 2: 1867-1871*. Ed. by Edward C. Moore. Indiana University Press, 1984.

Poincaré, Henri. "Les Fondaments de la Géometrie." In: *Bulletin des sciences mathématiques* 26 (1902), pp. 249–272.

— "Les mathématiques et la logique [I]." In: *Revue de métaphysique et de morale* 13 (1905), pp. 815–835.

— "Réflexions sur les deux notes précédentes." In: *Acta mathematica* 32 (1909), pp. 195–200.

— *Science et méthode*. Flammarion, 1908.

— *Sechs Vorträge über ausgewählte Gegenstände aus der reinen Mathematik und mathematischen Physik*. Auf EInladung der Wolfskehl-Kommission der Königlichen Gesellschaft der Wissenschaften gehalten zu Göttingen vom 22.–28. April 1909. Teubner, 1910.

Purkert, Walter. "On Cantor's Continuum Problem and Well Ordering. What really happened at the 1904 International Congress of Mathematicians in Heidelberg." In: *A Delicate Balance. Global Perspectives on Innovation and Tradition in the History of Mathematics*. Springer, 2015, pp. 3–24.

Quine, Willard van Orman. *Mathematical Logic*. Revised Edition. 1981.

— *Methods of Logic*. Revised Edition. 1959.

Reck, Erich H. and Steve Awodey. "Completeness and Categoricity. Part I: Nineteenth-century Axiomatics to Twentieth-century Metalogic." In: *History and Philosophy of Logic* 23 (2002), pp. 1–30.

Reid, Constance. *Hilbert*. Springer, 1996.

Resnik, Michael. "The Frege-Hilbert Controversy." In: *Philosophy and Phenomoenological Research* 34.3 (1974), pp. 386–403.

Richard, Jules. "Les principes des Mathématiques et le problème des ensembles." In: *Revue générale des Sciences pures et appliquées* 16.12 (1905). Ed. by Louis Olivier, pp. 541–543.

— "Lettre à Monsieur le rédacteur de la Revue Générale des Sciences." In: *Acta mathematica* 30 (1906), pp. 295–296.

Riemann, Bernhard. *Gesammelte Mathematische Werke, Wissenschaftlicher Nachlass und Nachträge*. Ed. by Henrich Weber and Richard Dedekind. Ed. by Raghavan Narashimhan. 1990.

— "Ueber die Hypothesen, welche der Geometrie zu Grunde liegen." In: ed. by Henrich Weber and Richard Dedekind. Ed. by Raghavan Narashimhan. 1990, pp. 304–319.

Rowe, David E. "David Hilbert (1862-1943)." In: *Die Albertus-Universität zu Königsberg und ihre Professoren. Aus Anlaß der Gründung der Albertus-Universität vor 450 Jahren*. Ed. by Dietrich Raschning and Donata v. Nerée. Vol. XXIX. Jahrbuch der Albertus Universität. Duncker & Humblot, 1995, pp. 544–552.

— "Klein, Hilbert, and the Göttingen Mathematical Tradition." In: *Osiris* 5 (1989), pp. 186–213.

— "The Calm before the Storm. Hilbert's Early Views on Foundations." In: *Proof Theory. History and Philosophical Significance*. Ed. by Vincent F. Hendricks, Stig Andur Pedersen, and Klaus Frovin Jørgensen. Kluwer, 2000, pp. 55–93.

— "The Philosophical Views of Klein and Hilbert." In: (1994). Ed. by Sasaki Chikara, Sugiura Mitsuo, and Joseph W. Dauben. Proceedings of the 1990 Tokyo Symposium on the History of Mathematics, pp. 187–202.

Russell, Bertrand. "Les paradoxes de la logique." In: *Revue de métaphysique et de morale* 14 (1906), pp. 627–650.

Schlimm, Dirk. "Metaphors for Mathematics from Pasch to Hilbert." In: *Philosophia Mathematica* 24.3 (2016), pp. 308–329.

Schoenflies, Arthur. "Über die Stellung der Definition in der Axiomatik." In: *Jahresberichte der Deutschen Mathematiker-Vereinigung* 20 (1911), pp. 222–254.

Scholz, Heinrich and Friedrich Bachmann. "Der Wissenschaftliche Nachlass von Gottlob Frege." In: *Actes du Congrès international de philosophie scientifique 1935, Paris, Sorbonne*. Vol. 7. 1936, pp. 24–30.

Schröder, Ernst. *Der Operationskreis des Logikkalkuls*. Verlag von B. G. Teubner, 1877.

— *Lehrbuch der Artihmetik und Algebra*. Vol. 1 (mehr nicht erschienen). Verlag von B. G. Teubner, 1873.

— "Nachschrift [zu Seite 87]." In: *Repertorium der literarischen Arbeiten aus dem Gebiete der reinen und angewandten Mathematik*. Vol. 2. Teubner, 1879, p. 162.

Schröder, Ernst. "Note über die Algebra der binären Relative." In: *Mathematische Annalen* 46 (1895), pp. 144–158.

— *Vorlesungen über die Algebra der Logik. Exakte Logik*. Vol. 1. Teubner, 1890.

— *Vorlesungen über die Algebra der Logik. Exakte Logik*. Vol. 2.1. Teubner, 1891.

Schubert, Hermann. "Grundlagen der Arithmetik." In: *Encyklopädie der mathematischen Wissenschaften mit Einschluss ihrer Anwendungen. Arithmetik und Algebra*. Vol. I. Verlag von B.G. Teubner, 1898, pp. 1–27.

Sieg, Wilfried. Oxford University Press, 2013.

— "Hilbert's Programs: 1917-1922." In: *Hilbert's Programs and Beyond*. Oxford University Press, 2013, pp. 91–127.

— "Methods for real arithmetic." In: *Hilbert's Programs and Beyond*. Oxford University Press, 2013, pp. 73–90.

— "Relative consistency and accessible domains." In: *Hilbert's Programs and Beyond*. Oxford University Press, 2013, pp. 299–326.

Sieg, Wilfried and Dirk Schlimm. "Dedekind's analysis of number. Systems and axioms." In: *Hilbert's Programs and Beyond*. Oxford University Press, 2013, pp. 35–72.

Benis-Sinaceur, Hourya. "Du formalisme à la constructivité: le finitisme." In: *Revue Internationale de Philosophie* 47.186 (4 1993), pp. 251–283.

Sterrett, Susan. *Frege and Hilbert on the Foundations of Geometry*. Unpublished Talk. 1994. URL: http://philsci-archive.pitt.edu/723/1/SterrettFregeHilbert1994.pdf.

Sutherland, Daniel. "Kant on Arithmetic, Algebra, and the Theory of Proportions." In: *Journal of the History of Philosophy* 44.4 (2006), pp. 533–558.

— "The Role of Magnitude in Kant's Critical Philosophy." In: *Canadian Journal of Philosophy* 34.3 (2004), pp. 411–442.

Tappenden, Jamie. "Extending Knowledge and 'Fruitful Concepts'. Fregean Themes in the Foundations of Mathematics." In: *Nous* 29.4 (1995), pp. 427–467.

Thiele, Günther. *Die Philosophie Immanuel Kants nach ihrem systematischen Zusammenhange und ihrer logisch-historischen Entwicklung dargestellt und gewürdigt. Kant's Vorkritische Naturphilosophie*. Vol. 1.1. Max Niemeyer, 1882.

— *Grundriss der Logik und Metaphysik*. Max Niemeyer, 1878.

— *Kant's Intellektuelle Anschauung als Grundbegriff seines Kriticismus*. Max Niemeyer, 1876.

Thomae, Johannes. *Elementare Theorie der analytischen Functionen einer complexen Veränderlichen*. Verlag von L. Nebert, 1880.

Tilitzki, Christian. *Die Albertus-Universität Königsberg. Die Geschichte von der Reichsgründung bis zum Untergang der Provinz Ostpreußen*. Vol. 1, 1871-1918 [volume 2 remains unpublished]. Akademie Verlag, 2012.

Toepell, Michael-Markus. *Über die Entstehung von David Hilberts "Grundlagen der Geometrie"*. Vol. 3. Studien zur Wissenschafts-, Sozial- und Bildungsgeschichte der Mathematik. Vandenhoeck & Ruprecht, 1986.

Ueberweg, Friedrich. "Die Principien der Geometrie, wissenschaftlich dargestellt." In: *Archiv für Philosophie und Pädagogik* 17 (1851), pp. 20–54.

Veraart, Albert. "Geschichte des wissenschaftlichen Nachlasses Gottlob Freges und seiner Edition. Mit einem Katalog des ursprünglichen Bestands der nachgelassenen Schriften Freges." In: *Studien zu Frege. Band I: Logik und Philosophie der Mathematik*. Ed. by Matthias Schirn. Problemata 42. Frommann-Holzboog, 1976, pp. 49–106.

Volkert, Klaus, ed. *David Hilbert: Grundlagen der Geometrie (Festschrift 1899). Kommentiert von Klaus Volkert*. Springer, 2015.

Wehmeier, Kai and H.-C. Schmidt. "The Quest for Frege's Nachlass." In: *Critical Assessments of Leading Philosophers: Gottlob Frege*. Ed. by Michael Beaney and Erich Reck. Vol. 1. Routledge, 2005, pp. 54–67.

Wilson, Mark. "Frege's Mathematical Setting." In: *The Cambridge Companion to Frege*. Ed. by Thomas Ricketts and Michael Potter. Cambridge University Press, 2010, pp. 379–412.

Zach, Richard. "Completeness before Post. Bernays, Hilbert, and the Development of Propositional Logic." In: *Bulletin of Symbolic Logic* 5.3 (1999), pp. 331–364.

Appendix A

Material on Korselt's Later Work

Editorial Introduction

Alwin Korselt is, if at all, remembered for his contributions to the controversy with Hilbert Frege attempted to instigate by the publication of his article entitled "Die Grundlagen der Geometrie" (see § 2). Indeed, the two articles Korselt published in order to mount a defence of Hilbert's position in response to Frege's attack are two of the longest he published (as far as I know), and also among the last of Korselt's publications, even though they appeared almost 30 years before Korselt's death in 1947. In fact, Korselt kept working and writing also in his later years and on at least two occasions in the 1930s attempted unsuccessfully to publish more of his work.

A few letters exchanged between Heinrich Behmann, Paul Bernays and Otto Blumenthal in 1937 testify to these efforts: Early in 1937 Korselt submitted a lengthy manuscript entitled "Eine Fortbildung des Schröderschen Logikkalküls" for publication in the *Mathematische Annalen*. Bernays forwarded the manuscript to Behmann with the request that Behmann, on the basis of his understanding of Schröder's work, review it and make a recommendation whether to publish the article – and whether to do so in its present or a revised form – or not.

In response, Behmann sent a lengthy report of his impressions to Blumenthal, then the editor-in-chief of the *Mathematische Annalen* (and a copy to Bernays), and a little later another letter containing some further observations about technical details. Behmann advised to reject the manuscript in its present form, but encourage Korselt to re-submit a «condensed» revision of the text. Behmann implies that he no longer thinks Korselt himself capable of carrying out the requisite revisions – perhaps due to Korselt's age at the time, although Behmann does not lay out his reasons explicitly –, which, indeed, Korselt seems not to have carried out.

It is reproduced here in full, along with Behmann's correspondence with Bernays and Blumenthal surrounding it; while the letters to and from Blumenthal reproduced here are the *entirety* of the correspondence between Behmann and Blumenthal preserved among Behmann's papers, Behmann's correspondence with Bernays is more extensive

and only a small portion of it is reproduced here[342].

Since Korselt's manuscript is not known to have survived (nor is any other unpublished material from Korselt's hand[343]), Behmann's report is the most extensive source of information about Korselt's later, logical work.

To characterise spirit very broadly I want to quote a statement Korselt himself had come up with a few years earlier in a postcard (dated March 6 1929) he sent to Behmann. There, Korselt writes:

> Übrigens habe ich dabei mühsam wichtige rein log. Eliminationsformeln entdeckt, über *Schröder* hinaus, welche auch *Ihre* Probleme allgemein und ohne Kunstgriffe erledigen, die ich Ihnen auf Nachfrage mitteile. Denn *Schröders* A[lgebra der Logik] III enthält leider viel Unrichtiges, seine Methoden sind unfruchtbarbar und zu beschränkt, obwohl er das Richtige zuweilen ausspricht, aber nicht, aber nicht festhält.

While Korselt made this remark in a postcard concerned with the formulation of number-theory leading up to Fermat's theorem within Schröder's symbolism, it seems plausible to me that Korselt would later have compiled the "important" fruits of his years of "arduous" labour at logic under the title of the manuscript Behmann reviewed, "further developments of Schröder's logical calculus".

I want to concludes this brief introduction by contextualising Behmann's apparently harsh verdict to some extent[344]: When reading Behmann's report one should also bear in mind, it seems to me, that Behmann was not a neutral arbitrator in the matter. Behmann had a stake of his own in the "further development of Schröder's logical calculus", which he saw already largely realised and whose direction, anyway, he believed to have firmly defined himself in his 1922 article on the decision problem. Behmann claims there to have isolated from Schröder's work those aspects that have use and meaning beyond being mere technical curiosities. In light of Behmann's own agenda one should, it seems to me, take some of Behmann's criticisms of Korselt, e.g. that Korselt did not duly take into account Behmann's work on the decision problem, with a proverbial grain of salt.

In fact, when Behmann wrote this report his ego had recently taken several beatings: Behmann's efforts, to which he seems to have devoted much of the 1920s, after giving up on resolving the decision problem soon after 1922, of resolving the paradoxes of set-theory in a satisfactory fashion failed: Behmann sent a lengthy letter to Hilbert (dated March 18 1928) proudly announcing his resolution of the paradoxes. As far as I know, no response of Hilbert's to Behmann's letter is extant. In fact, it seems to me not improbable that Hilbert never did respond to Behmann's letter. Rather, Hilbert likely regarded the matter as settled upon conferring with Bernays, who almost

[342]The post-war correspondence between Bernays and Behmann is full of small kindnesses Bernays imparted by Bernays to the (after the Second World War) rather destitute Behmann, such as Swiss cheeses, honey, or (on one occasion) a suit.

[343]Kreiser, "Die Hörer Freges und sein Briefpartner Alwin Korselt" mentions a handful of what he calls "splinters" of Korselt's papers, which I have, however, been unable (so far) to locate. The main difficulty in locating these manuscripts is that Kreiser lists only their titles and contents, but provides no information to trace the pieces in library systems. Another problem is that there no longer is a Staatsarchiv Plauen. The present-day *Stadt*archiv Plauen informs me (in response to a request of mine submitted Jan. 17 2022) that in their records contain no trace of the manuscripts mentioned by Kreiser.

[344]Peckhaus, in his study of Korselt, describes Behmann's verdict as "devastating" (Peckhaus, "Alwin Reinhold Korselt," p. 20). I think this is an exaggerated description of the tone and content of Behmann's report, even if the context of Behmann's verdict is not taken into account: Behmann does acknowledge that the manuscript constitutes a genuine improvement on Schröder's own work (line 39 of his report A.2.2) and that it contains valuable insights which should not be lost and therefore suggests providing both encouragement and perhaps even practical help to Korselt in suitably revising the manuscript (line 131ff. of Behmann's report A.2.2) for eventual publication; indeed, Behmann expresses some hesitation about sending his critical report to Korselt himself so as to not discourage him.

immediately raised counter-instances defeating Behmann's attempted resolution[345].

Behmann had also failed in his efforts to propagate and install as standard the system of notation for logic he had designed during the 1920s, and his efforts to develop a system of modal logic had stalled (likely also as a result of being overburdened with teaching duties, as Behmann mentions in his letter to Bernays (see 33 in A.3.1), but presumably not only for this reason, but also because Behmann found himself unable to overcome substantive difficulties).

So, when Behmann wrote his report on Korselt's manuscript, the last – and, indeed, the only – major contribution he had made to the development of mathematical logic was the work presented in his 1922 article on the decision problem[346], which, one might speculate, he therefore wanted to see duly acknowledged. Korselt's failure to do so may have spurned Behmann to be more severe in his evaluation than he might otherwise have been (and, as is suggested by the surprise Bernays voices about how decisively Behmann opted to reject Korselt's manuscript, perhaps more severe than may have, in light of the facts of the matter, been justified.

Indeed, as Korselt's remarks about "[Behmann's] problems" in the postcard to Behmann, from which I quoted above, document: Behmann had good reason to think that Korselt was, in fact, well aware of Behmann's work; on the other hand, Behmann thus had reason to see Korselt's choice not to explicitly engage with his work as slighting him. One may further speculate that the somewhat arrogant tone of Korselt's postcard, which was asking Behmann to explicitly request from Korselt a communication of Korselt's "important purely logical elimination-formulas" that would "in full generality and without any artificial devices solve" Behmann's "[decision?] problems", may have played its part in producing in Behmann an unfavourable opinion of Korselt long before Behmann found himself asked to report his opinion on Korselt's manuscript.

I do not mean to suggest that Behmann is grossly misjudging the quality of Korselt's manuscript on account of personal animosity he harboured against Korselt. If Korselt's earlier publications are representative also of his later style of exposition, the manuscript is likely to have been, as both Behmann and Bernays seem to have found it: diffuse and, at times, trifling. I mean only to caution qualify Behmann's judgement some, and to suggest that Bernays' initial, kinder estimation (in his initial letter, A.1, line 19) may be truer to the facts, as they were.

[345] Behmann's resolution of the paradoxes is also the subject of Behmann's correspondence with Ramsey, who, like Bernays, described in his friendly and indulgent response to Behmann's letter dated Oct 4 1928 versions of the set-theoretic paradoxes not – as Behmann conceded in his response to Ramsey – resolvable in the fashion Behmann's described.

[346] Peckhaus, "Alwin Reinhold Korselt," p. 21 observes that Behmann himself later in his life became what he reproached Korselt for in his report, an ageing researcher who had lost touch with the forefront of ongoing research.

Texts

All the letters edited here are held by the Staatsbibliothek zu Berlin – Preußischer Kulturbesitz. They are found in box 1 of Nachlass 335 (Heinrich Behmann) in the folders containing Behmann's correspondence with Bernays and Blumenthal. The letters appear here with kind permission of the Handschriftenabteilung, Referat Nachlässe und Autographen of the Staatsbibliothek zu Berlin – Preußischer Kulturbesitz.

A.1 Bernay's initial letter to Behmann (dated Nov. 16 1937)

Lieber Herr Behmann!

Wir haben lange nichts von einander gehört[I]. Wie Sie vielleicht wissen, lebe ich jetzt (seit annährend drei Jahren) in Zürich. – Der Anlass meines heutigen Schreibens ist, dass ich Sie – zugleich im Namen der Redaktion der Math. Annalen – um Ihre Stellungnahme zu einer logischen Arbeit bitten wollte, die Prof. Korselt für die Annalen eingereicht hat. Da die Ausführungen dieser Art sich im Rahmen des Schröder'schen Formalismus bewegen, mit dem Sie ja wohlvertraut sind, so werden Sie gewiss sehr viel schneller ein massgebliches Urteil über das Geleistete gewinnen als ich, der ich mir die Formeln immer erst in den gewohnteren Kalkul übersetzen muss. Es handelt sich um Methoden zur Behandlung des Entscheidungsproblems unter scharfer Ausnutzung des Auswahlprinzips. Was aber nicht so ohne weiteres ersichtlich ist, das ist, in wie weit diese Arbeit gegenüber den bisher bekannten Methoden Neues liefert, und ob nicht darin mit Vorteil gekürzt werden kann. Eine weitere Frage, über die Ihre Meinungsäusserung erwünscht wäre, ist, ob es Ihnen tunlich erscheint, die Arbeit in der vorliegenden Form der Darstellung zu publizieren, bezw. ob es nicht einerseits möglich, andrerseits auch vorteilhaft wäre, die allgemeinen philosophischen Erörterungen von den formalen Ausführungen zu trennen, also etwa die Arbeit in zwei Abschnitte zu teilen.

Die vorliegende Arbeit bildet gewissermassen eine Umarbeitung eines vor ein paar Jahren von Korselt eingereichten Konzeptes, welches zur Publikation nicht geeignet erschien, da es ausser allgemeinen Betrachtungen an Positivem nur die Behandlung von Beispielen enthielt, wobei einerseits die allgemeine Methode nicht ersichtlich war, andrerseits die Beispiele teilweise auf direktem Wege einfacher behandelt werden | konnten. – Von dem gegenwärtigen Konzept glaube ich hingegen, dass es sehr wohl zur Publiation lohnend ist.

Wenn es Ihnen im Interesse des rascheren Fortganges der Angelegenheit vorteilhaft erscheint, so können Sie ja Ihr Gutachten und Ihre Vorschläge direkt an Herrn Prof. Blumenthal – (seine Adresse: Aachen, Beseler Str. 22 ist Ihnen gewiss bekannt) – senden, dem ich von meinem Vorhaben, Sie in dieser Sache zu interpellieren, bereits geschrieben habe.

Das Konzept der Arbeit von Korselt sende ich Ihnen mit gleicher Post.

[I] The last previous letter is dated "4.XI.30".

Ich hoffe, dass es Ihnen gut geht und dass Ihre berufliche Situation in Halle eine befriedigende ist. – Die neueren amerikanischen Publikationen auf dem Gebiet der Logik, insbesondere die von Curry, Church, Kleene und Rosser haben Sie wohl verfolgt; da diese ja in der Richtung des auch von Ihnen, freilich auf einem anderen Wege, angestrebten stufenfreien Kalkuls liegen. Jedenfalls ist ja bei diesen Untersuchungen vieles Interessante herausgekommen.

Mit freundlichen Grüssen

Ihr

P. Bernays

[P.S.] Entschuldigen Sie bitte die Bemühung![A] Ich nehme wie gesagt an, dass es für Sie ein Leichtes sein wird, über die Korseltsche Arbeit Ihr Urteil zu gewinnen. Vielleicht findet diese auch Ihr näheres Interesse.

A.2 Behmann's letters to Blumenthal

Behmann sent his report on Korselt's manuscript to Blumenthal along with a letter dated February 24 1937.

A.2.1 Behmann's Report

Sehr geehrter Herr Professor, -1- [recto]

vor kurzem wurde mir die Arbeit von Korselt "Eine Fortbildung des Schröderschen Logikkalküls" zur Prüfung übersandt. Ich habe sie inzwischen durchgesehen und möchte Ihnen nun darüber berichten.

Als entscheidend für die Beurteilung betrachte ich die Feststellung, inwieweit die Arbeit sachlich Neues bringt oder, sofern es sich um bereits Bekanntes handelt, wenigstens durch eine neue in einigen Hinsichten belangreiche Art des Aufbaues von Wert ist, und, ob und inwieweit dieser sachliche Gehalt dem Leser in angemessener und zweckmäßiger Form dargeboten wird.

Zunächst zur Sache selbst. Bekanntlich lassen sich in der Logik Quantitätsoperatoren nicht allgemein vertauschen, sondern wesentlich nur dann, wenn sie (unmittelbar benachbart und) *gleichartig* sind. So ist (in der Hilbertschen Symbolik, die für Sie wohl die geläufigste ist) zwar $(x)(y)f(x,y)$ äquivalent $(y)(x)f(x,y)$, dagegen $(x)(Ey)f(x,y)$ nicht allgemein äquivalent $(Ey)(x)f(x,y)$. Nun hat Schröder (Algebra der Logik, Bd. 3) entdeckt, daß auch im zweiten Fall die Vertauschung sich in gewisser Weise bewerkstelligen läßt; es ist nämlich

$$(x)(Ey)f(x,y) \sim (Ey)(x)(x,y(x)), \tag{a}$$

wo y nun aber nicht mehr seinen früheren Bereich (etwa der Dinge oder der Aussagen) durchläuft, sondern als $y(x)$ den der Belegungen des x-Bereiches mit dem y-Bereich, also den der Funktionen mit den x-Werten als Argumen-

[A] This postscript, as well as Bernays' signature, are hand-written.

10 $(x)(y)f(x,y)$] In this formula, but in none of the following, a capital "F" appears, presumably by mistake.

twerten und y-Werten als Funktionswerten. Übrigens gilt genau so für gleichartige Operatoren z.B.

$$(x)(y)f(x,y) \sim (y)(x)f(x,y(x)), \tag{b}$$

was weder von Schröder noch von K. erwähnt wird. K. geht nun darauf aus, die Möglichkeit (a) noch systematischer, als es durch S. geschehen ist, für die Behandlung logischer Probleme (u[nd] z[war] handelt es sich wesentlich um das Eliminationsproblem im Sinne meiner Arbiet Math. Ann. 86 als wichtige Vorstufe des Entscheidungsproblems) auszunutzen und insbesondere die S.sche Symbolik gerade in diesem Sinne weiter auszubauen.

Nun hat S.s "Logik der Relative" vom heutigen Standpunkt zwei ganz entscheidende methodische Fehler: 1. zwängt er künstlich die Aussagen- und die Klassenlogk in die Logik der zweistelligen Relative, indem er, mathematisch gesprochen, die Konstanten als Funktionen zweier "gleichgültiger" Variabler und die Funktionen einer Variablen als Funktionen einer wesentlichen und einer gleichgültigen Variablen behandelt, um es dann einzig mit Funktionen zweier Variablen zu tun zu | haben. Daß damit der natürliche Aufbau völlig auf den Kopf gestellt -2- [verso] und verwischt wird und die Erkenntnis der natürlichen Zusammenhänge und Gesetzmäßigkeiten unmöglich werden muß, ist klar. 2. erstrebt er durchgängig die "Kondensation" der logischen Ausdrücke, d.h. die Zurückführung auf die Knüpfungen (in seiner Schreiung) $a + b$, $a \cdot b$, $a; b$ und $a \dagger b$, was nach meiner – und wohl überhaupt der heutigen maßgeblichen – Ansicht nichts weiter als einen rein sportlichen Ehrgeiz ohne den geringsten sachlichen Wert bedeutet und dabei eine außerordentliche methodische Belastung und Erschwerung darstellt.

K. mildert zwar den ersten Fehler insoweit, als er auch u.z. vorzugsweise von "Gebieten" spricht, also immerhin den Funktionen einer Variablen zum mindesten nach der Seiten der Funktionen zweier Variablen ihr Eigenrecht zubilligt; doch erscheinen die Aussagewerte Wahrheit und Falschheit als ausgeartete Gebiete, indem sie künstlich dem Allgebiet und dem leeren Gebiet gleichgesetzt werden. Zum anderen versteift er sich seinerseits nicht allzusehr auf die Kondensationsforderung. Ich glaube freilich nicht, daß dies gerade Verdienste K.s sind, sondern daß es überhaupt in der Entwicklung seit S. (Löwenheim usw.) liegt.

Indessen ist klar, daß die K.sche Arbeit nicht von dem Standpunkt dessen gewertet werden muß, was bisher in S.scher Symbolik geschrieben worden ist, wobei sie immerhin einen belangreichen Fortschritt darstellen mag, sondern natürlich auf Grund dessen, was überhaupt auf dem Gebiet der symbolischen Logik heute bekannt und veröffentlicht ist. Und da kann nun nicht geleugnet werden, daß sie hinter diesem Standpunkt ernstlich zurückbleibt. Ich greife einige Punkte heraus, die man dem mit symbolischer Logik vertrauten Leser heute wohl nicht mehr vorsetzen kann.

1. Es ist K. offenbar unbekannt, daß (a) – wie natürlich auch (b) – wesentlich auf dem *Auswahlprinzip* beruht. Er erklärt, daß er (a) nicht beweisen könne (S. 18), was hiernach kein Wunder ist. Er braucht fünf Seiten, um mathematisch-philosophisch-psychologisch zu begründen, daß er (a) als Axiom annehmen will.

2. Er führt (S, 69) den "Müllerschen Bestätigungssatz" an, wobei ihm die Beweisfrage wiederum – begreiflicher-

43 daß] The typescript has "das", which has been struck out and correct (in handwriting) by Behmann above the line.

44 (a)] The manuscript originally had "sie", which has been struck out and replaced by "(a)" in Behmann's handwriting above the line.

weise – einiges Unbehagen macht. Es entgeht ihm nämlich zwangsläufig, daß der genannte Satz nichts weiter als eine unmittelbare Anwendung des von ihm außer acht gelassenen Prinzips (b) ist. (Ich kann freilich nicht sagen, ob (b) explizit in der Literatur auftritt, also auch anderen bekannt ist.) Allerdings kommt hier die künstliche Einordnung der Aussagenlogik in die Begriffslogik bzw. den Gebiete- und Relativkalkül (statt der natürlichen Voranstellung) als die Sachlage verdunkelndes Moment hinzu.

3. Das doch wirklich recht nahe liegende Prinzip, daß $f(x, y)$ bei festem y eine Funktion einer Variablen, bei festem x und y eine Funktion von null Variablen, also eine Konstante (im Fall der Logik eine Aussage bzw. einen Aussagewert) darstellt, das ich in meiner Arbeit Math. Ann. 86 u.a. (§ 19) auf die formalen | Probleme des -3- [recto] Relativkalküls angewandt habe, wird von K. weder zu Vereinfachung der Darstellung der Gesetzmäßigkeiten noch zur Vereinfachung der Problembehandlung herangezogen, so als ob meine Arbeit überhaupt nicht existierte, wobei er keine andere Entschuldigung hat als sein gewohnheitsmäßiges Festhalten an der S.schen Verkünstelung des Systems, die schon die Anwendung dieses einfachen Prinzips erheblich zu hemmein scheint. K.s Hauptprinzip wird also nur sehr unvollkommen, ein sehr viel primitiveres und an sich näher liegendes dagegen gar nicht ausgenutzt.

4. In einer für den Kenner unverständlichen Weise stockt K. bereits, wo es sich (S. 50) um den uns völlig geläufigen Übergang zu Begriffen höherer Stufe, zu Eigenschaften und Beziehungen von Gebieten oder Relativen handelt. Da diese sich in keiner Weise anders verhalten als Eigenschaften und Beziehungen von anderen Dingen auch, erübrigen sich durchaus die langatmigen und trivialen Betrachtungen S. 50 usf. Bezeichnend sind die Äußerungen (S. 54) "Da man also mit dem Zeichen $\mathcal{L}$ rechnen kann, ist seine Einführung gerechtfertigt" (dies tut man nämlich schon sehr lange!) und (S. 80) "Eine einfache Ordnung oder Wohlordnung ist ... Beschaffenheit eines Inbegriffes ... Oder darf man sie ohne Widerspruch als Arten von Inbegriffen erklären?" (auch hier liegt seit Frege die Entscheidung völlig fest).

5. Es bedeutet bestimt keine Vervollkommnung eines symbolischen Verfahrens, wenn man seiner Aufstellung eine riesenlange Formelsammlung beifügt; vielmehr wird man vernünftigerweise dahin streben, alles auf einige wenige und dabei möglichst weittragende Rechenvorschriften zurückzuführen; dies ist anderweitig bereits in viel höherem Grade erreicht, als es aus der Arbeit K.s hervorgeht. Überdies kann man die Mehrzahl von ihnen bequemer und in zweckmäßigerer Darstellung den "Principia Mathematica" entnehmen, auf die von K. nicht einmal verwiesen wird.

Eine gewisse formal zwekchmäßige, aber sachlich kaum weiter führende Fortbildung der S.schen Symbolik (die S. 44-47 erklärt wird) liegt allerdings vor; sie sieht hier gekünstelter aus, als sie in Wirklichkeit ist. Sie geht zum mindesten teilweise auf Löwenheim zurück; wieweit K. selbständigen Anteil an ihr hat, kann ich nicht genau sagen. Einige der in der Arbeit aufgestellten Formeln (bestimmt nur ein kleiner Teil) mögen immerhin neu sein; auch darüber kann ich nicht mit Sicherheit entscheiden. Allerdings macht sich auch hier, wie es sich mir immer wieder im einzelnen bestätigt, die grundsätzliche Unzulänglichkeit des Systems durchweg geltend. Natürlich rechne ich

64 $\mathcal{L}$] This symbol has been added by hand in the typescript.

durchaus damit, daß ich die Absichten des Verfassers und seine Überlegungen nicht überall richtig aufgefaßt habe; ein paar Punkte von nicht erheblicher Natur sind mir tatsächlich dunkel geblieben. Zudem würde die eigentliche Bewährung ja erst in dem beabsichtigten zweiten Teil zu erwarten sein.

Dies als Kritik in sachlicher Hinsicht. Zu der S.-schen Auffassung, Symbolik und Methode habe ich in meiner vorhin erwähnten Arbeit eingehend Stellung genom|men, insbes. S. 164-167, 207 u. 218-219. Wenn K. (S. 1) erklärt: -4-[verso] "Der Logikkalkül und insbesondere der Relativkalkül ist bei Schröder stecken geblieben" (wobei S. immerhin über den Standpunkt seiner Zeit weit hinauskam), muß nach dem von mir Ausgeführten leider gesagt werden, daß K. seinerseits in dem heute grundsätzlich überwundenen S.schen Gebiete- und Relativkalkül stecken und damit hinter dem gegenwärtigen Standpunkt zurückgeblieben ist.

Ist so der sachliche Inhalt wesentlich bescheidener, als der erste Anschein vermuten läßt (womit ich noch nicht gesagt haben will, daß er für eine Veröffentlichung in wirklich angemessener Form zu unbedeutend sein würde), so ist zugleich außer allem Zweifel, daß der tatsächliche Umfang der Arbeit zu diesem Gehalt in keinem vernünftigen und irgendwie zu rechtfertigendem Verhältnis steht. Da es sich ja ausdrücklich um die Fortbildung, nicht um die Begründung eines Kalküls handelt, sind in diesem Zusammenhang ausführliche philosophische und mathematische Erörterungen schon an sich nicht am Platze. Denen, die sie allenfalls gebrauchen könnten, käme sie nicht zugute, weil diese zu einem Studium der Arbeit im ganzen noch nicht im stande sein würden, und für solche, welche die sachlichen Voraussetzungen mitbringen, wäre sie überwiegend langweilig und ermüdend. Es ist wirklich keine Freude, nach einer Einleitung über das Wesen des Kalküls ein buntes Durcheinander der allerverschiedensten philosophischen und mathematischen Betrachtungen, wie die Philosophie der Hypothesenbildung und der Relativität des Raumes, das Wesen der Funktion, der Variablen und des Parameters (dieser letzte Punkt ist besonders ausführlich behandelt), Matrizen und Determinanten, zahlentheoretische Fragen, totale Differentialgleichungen, analytische Geometrie der Kurven, algebraische Gebilde usf., über sich ergehen lassen zu müssen, bis man endlich die beruhigende Erklärung erhält: "Nach diesem notwendigen (für wen und wozu eigentlich?) Aufklärungen über mathematische Begriffe komme ich wieder zum Logikkalkül". Sofern alles das überhaupt einen ernst zu nehmenden sachlichen Wert hat (der mir selbst freilich noch nicht aufgegangen ist), gehörte dies in einen Aufsatz für sich und bedürfte vor allem einer klaren und einsichtigen Ordnung.

Es kommt als wesentlicher Umstand hinzu, daß wir gegenwärtig in einem internationalen Arbeitsausschuß mit der Festlegung einer logischen Symbolik beschäftigt sind; die Sache wird im Sommer auf dem nächsten Kongreß zur Einheit der Wissenschaft voraussichtlich entschieden werden. Es sind hierbei Sonderbestimungen vorgesehen, die gerade den Bedürfnissen des logischen Rechnens besonders entsprechen und damit für alle künftigen Bearbeitungen von Problemen des logschen Kalküls die maßgebende Darstellungsform bedeuten werden. Sobald man sich nun aber in diesem Sinne auf eine dem heutigen und nach Möglichkeit auch künftigen Bedürfnissen entsprechende Form geeinigt haben wird, kann es natürlich nicht ausbleibe, daß ältere und im Prinzip überwundene Darstellungsformen

wie die S.sche dann besonders rasch veralten und damit natürlich auch die darin geschriebenen Ver|öffentlichungen. -5-[recto]
Denn nur die wenigne werden sie noch lernen, die beabsichtigen, das Werk S.s zu studieren, aber natürlich niemand mehr, um selbst damit zu arbeiten. Noch weniger lohnt es sich dann natürlich, eine solche im Prinzip unzulängliche und überwundene Symbolik noch irgendwie weiter auszugestalten; vielmehr wird man dann natürlich gleich die neue Symbolik entsprechend ausbauen, was dann keine Schwierigkeiten machen wird.

Ich komme damit zu dem Ergebnis, daß eine Veröffentlichung der K.schen Arbeit in der vorliegenden Form nach meiner Beurteilung weder wissenschaftlich noch mit Rücksicht auf die in Betracht kommenden Leser zu verantworten sein würde und daß zum mindesten eine auch in sachlicher Hinsicht erheblich verbesserte Neubearbeitung mit strenger Beschränkung auf den sachlichen Gehalt zur Voraussetzung gemacht werden müßte, wobei maßgebene Werke wie insbesondere die Principia Mathematica, aber auch meine einschlägige Arbeit als gegebene, nicht als unbekannte Größen zu fungieren haben. (Selbstverständlich würde eine genaue Abgrenzung des tatsächlich wertvollen Gehaltes erst bei Kenntnis des zweiten Teiles möglich sein, sobald man sieht, was sich in der Anwendung bewährt – also für diese nicht nur brauchbar, sondern in gewissem Sinne belangreich ist – und was nur darum ausgeführt worden ist, weil der Zusammenhang als eine geeignete Gelegenheit erschien, es mit anzubringen. Ich denke hier besonders an die vielen verwickelten Begriffszerspaltungen, bei denen sich nicht recht vermuten läßt, in welcher Weise sie etwa für die später zu entwickelnden Rechenverfahrenvon Belang werden sollten.) Doch sprechen darüber hinaus begründete taktische Erwägungen dafür, von einer Verwendung oder gar Weiterbildung der S.schen Symbolik überhaupt abzusehen und nach Möglichkeit die Sahe bis zur Festlegung einer allgemein anerkannten und sachlich angemessenen Symbolik zurückzustellen.

Da ich nicht glaube, daß K. noch imsande sein wird, die vorgeschlagene Umareitung selbst zu übernehmen, würde ich es für das beste halten, wenn ein jüngerer Mathematiker sich bereit finden würde, sich mit K. wegen des gesamten Stoffes in Verbindung zu setzen und gemgemäß die Bearbeitung durchzuführen, am besten, sobald die Symbolikfrage geklärt ist. Ich selbst komme dafür nicht in Frage, weil ich schon auf ängere Zeit hinaus mit anderen Aufgaben beschäftigt bin; auch würde es dann wohl zu sehr meine eigene Arbeit werden statt eine gemeinsame. Auf alle Fälle würde ich es sehr begrüßen, wenn man K. die Zusicherung geben könnte, daß seine recht dankenswerten Leistungen auf diesem Gebiet davor bewahrt bleiben, nicht beachtet oder vergessen zu werden, sondern die gerechte Würdigung erhalten werden.

Falls ich die K.sche Urschrift Ihnen zuschicken soll, bitte ich um Benachrichtigung; andernfalls würde ich mich mit Herrn Bernays darüber verständigen, ob ich sie an ihn oder an K. zurücksenden soll.

Mit verbindlichem Groß

Ihr

Heinrich Behmann

A.2.2 Blumenthal's Response to Behmann (dated March 4 1937)

Lieber Herr Kollege!

Ich danke Ihnen, selbst und im Namen der Annalenredaktion, herzlich für die grosse und erfolgreiche Mühe, die Sie sich mit der Arbeit Korselt gegeben haben. Es besteht kein Zweifel, dass die Arbeit zurückgeschickt werden muss. In anderen Arbeiten Korselts fanden wir unter dem Wust der Darstellung so vielen guten Kern, dass eine Veröffentlichung in gekürzter Form (wobei sich die Kürzungen leicht angeben liessen) sich empfahl. Aus Ihrem Briefe schliesse ich, dass es mit dieser Arbeit anders liegt, dass nämlich die Umarbeitung so gründlich sein muss, dass Korselt sich nicht machen kann und ein anderer Mann seine ganze Zeit daran wenden müsste. Andererseits ist der Inhalt zwar nicht wertlos, aber jedenfalls nicht so, dass man ein Unrecht tut, wenn man ihn dem Publikum vorenthält. Ich möchte die Arbeit mit einem sehr liebenswürdigen Brief zurückschicken. Ich bitte Sie dazu um die Erlabunis, einen Teil Ihres Briefes an mich Herrn Korselt vorlegen zu dürfen. Ich lege den Auszug, in dem ich einige Stellen gebildert habe, Ihnen zur Einsicht bei. Ich wäre Ihnen dankbar, wenn Sie mir die Erlaubnis geben wollten. Natürlich dürfen Sie im einzelnen nach Belieben ändern. Ich bitte Sie um recht baldige Antwort und um Rücksendung des Manuskripts Korselt. Ersatz Ihrer Portokosten füge ich bei.

Nochmals sehr herzlichen Dank und beste Grüsse!

Ihr

O. Blumenhal.

A.2.3 Behmann's Response to Blumenthal (dated March 8 1937)

Verehrter Herr Professor,

natürlich bin ich mit der Übersendung des Briefauszuges in der von Ihnen beabsichtigten Form an K. gern einverstanden[I]. Anfänglich hatte ich selbst daran gedacht, einen Durchschlag an K. zu schicken[II], hatte dann aber doch ernstliche Bedenken. Herrn Bernays habe ich inzwischen einen Durchschlag geschickt.

Ich habe Herrn B. bei dieser Gelegenheit von einer sachlichen Schwierigkeit für mein Verständnis der Arbeit geschrieben, die sich m. E. wohl beheben ließe, die mir aber immerhin bezeichnend erscheint.

Ich schicke voraus, daß der von mir erwähnte von K. benutzte "Müllersche Bestätigungssatz" in vernünftiger Formulierung (*nicht* in der seines Urhebers) so lautet:

$$(f)(g)..(x)(y)..F(f(x,y,..),g(x,y,..),..) \sim (U)(V)..F(U,V,..)$$

bzw. für den einfachen Fall je einer Variablen der beiden Reihen:

$$(f)(x)F(f(x)) \sim (U)F(U).$$

[I] The excerpt seems not to have been preserved.

[II] See the letter to Bernays.

10 Herrn] This word has been added by Blumenthal in handwriting above the line with an insertion mark.

U, V,... sind variable Ausagen bzw. Aussagewerte, also Variable, die nur der Werte Wahr und Falsch fähig sind. Wie die Formel zeigt, ist es wesentlich, daß F eine Form (Aussagefunktion) ist, die $f, g, \ldots, x, y, \ldots$ als freie Variable aufweist, u.z. derart, daß sie lediglich in den Verbindungen $f(x,..)$, $g(x,..)$ usw. auftreten. Der durch die Formel ausgedrückte Übergang ergibt sich nun folgendermaßen, wobei ich lediglich um das Wesentliche schärfer hervortreten zu lassen, mich auf den engeren Fall beziehe: Von $(f)(x)F(f(x))$ gestattet das Prinzip (b) meines vorigen Briefes, rückwärts angewandt, den äquivalent Übergang zu $(x)(f)F(f)$, wo f nun von x unabhängig, also nicht mehr eine variable Aussagefunktion, sondern eine Aussagenvariable ist, die U genannt werden mag, während der Operator (x) nunmehr natürlich als belanglos wegfällt. Beispiel: $(f)(x)(f(x) \vee \overline{f(x)})$ $(U)(U \vee \overline{U})$. Gewisse Formeln über Eigenschaften (Klassen) – oder im allgemeineren Fall auch über Beziehungen (Zuordnungen, Relative) – "verwandeln" sich hiernach in Formeln der Aussagenlogik.

Bei K. lautet der Müllersche Satz so: "Sind f und g endlich Gebietsunktionen mit denselben Argumenten, die auch Gebiete als Werte haben, so gilt: Die Subsumtion $f(x,y,..) \Subset g(x,y,..)$ wie auch die Gleichung $f(x,y,..) = g(x,y,..)$ ist genau dann eine (allgemeingültige) Formel, wenn die betreffende Beziehung immer erfüllt ist, falls man für die Argumente irgendwelche Satzwerte (0 oder 1) einsetzt. In Zeichen des Kalküls:

$$\prod_{xy..}(f(x,y,..)) \underset{=}{\Subset} g(x,y,..)) = \prod_{\alpha\beta..}(f(\alpha,\beta,..) \underset{=}{\Subset} g(\alpha,\beta,..)).$$

"[1]

Auf den ersten Blick sieht man, da diese Formulierung enger ist als meine, weil es ja einer Einschränkung auf Subsumtionen (Implikationen) und Gleichungen (Äquivalenzen) an sich nicht bedarf. Was besagt nun aber die bei K. in den Worten formulierte Voraussetzung? (Wenn die Formel venrünftig geschrieben wird, bedarf es ja gar keine Voraussetzung in Worten, weil dann die Formel schon alles sagt.) Was ist ein "Gebiet" und was ist eine "endliche Gebietsfunktion"? Die erste Antwort lautet (S. 6): "Schrödersches Gebiet = Gegenstand, der den S.schen Axiomen in Band I und II seiner 'Algebra der Logik' (AI und AII) gehorcht". Sieht man von der vom mathematischne Standpunkt unmöglichen Formulierung ab, so scheint daraus hervorzugehen, daß das "Gebiet" der "Klasse" in der Loik entsprechen soll und nicht etwa dem "Relativ", von dem bie S. ja auch erst im dritten Band zum ersten Mal die Rede ist. Nun werden aber von K. (S. 13 u. 14) $0'$ (Verschiedenheit) und $1'$ (Identität) ausdrücklich Gebiete genannt; es müßten demanch bei K. auch die Relative (Umfänge von Beziehugnen, nicht nur von Eigenschaften) "Gebiete" sein. Andererseits (S. 12-13): "Eine Funktion heißt *logische endliche* Funktion, wenn sie keine Erstrecker (=Operatoren) enthält und nur duch endliche-oftmalige Anwendung von einigen der 6 logischen Grundrechenarten ($a+b$, $a \cdot b$, $\overline{a}$ $\breve{a}$, $a \dagger b$, $a;b$ auf endlich viele Gebiete oder Veränderliche) entsteht." Dies würde nun aber (abgesehen davon, daß man nicht recht weiß, was nun mit den in $a \dagger b$ und $a;b$ doch tatsächlich steckenden

[1] α, β,... sind Variable, die nur die Werte 0 und 1 (leere Klasse und Allklasse bzw. ev. leeres Relativ und Allrelativ) annehmen.

40 (] The closing parenthesis later in this sentence has no opening mate in the typescript. It makes the most sense to supply one here.

gebundenen Veränderlichen mitzählen soll oder nicht) die logischen Formulierung über den Haufen werfen. Denn z.B. $x \in\!\!\!/\, x$ bzw. $x; x \in\!\!\!/\, x$ ist sowohl für $x = 0$ als auch für $x = 1$ erfüllt, gilt aber dennoch nicht allgemein, denn nicht jede Zuordnung ist symmetrisch bzw. transitiv. – Demgegenüber schließt die von mir angegebene, nach meiner Ansicht naturgemäße Formulierung des M.schen Satzes die unzulässigen Anwendungen in den Fällen $\breve{a}$, $a \nmid b$ und | [verso] $a; b$ automatisch aus und gilt dabei für Klassen wie für Zuordnungen (Relative) von beliebiger Stellenzahl.

Hieraus ergibt sich zweierlei 1. daß in der K.schen Arbeit nicht unerhebliche Unklarheiten und Verwirrungen enthalten sind, die insbesondere einen grundlegenden Begriff betreffen und die unmöglich stehen bleiben können, und 2. daß in der Tat durch das nicht mehr gerechtfertigte Festhalten an alten starren Formen der Zugang zu der natürlichsten und zugleich einsichtigsten Auffassung und Darstellung geradezu verbaut wird, daß es also nicht einer bloß sachlichen Klärung und Berichtigung, sondern (zugleich als Voraussetzung hierfür) eines grundsätzlich anderen und angemesseneren Darstellungsschemas bedarf.

Wegen der in meinem ersten Brief erwähnten K.schen Fortbildung der S.schen Symbolik (S. 44-47) habe ich inzwischen die Arbeiten Löwenheims nachgesehen. Tatsächlich erwähnt L. (Math. Ann. 68, S. 188, also bereits 1910), daß diese (dort erläuterte) Symbolik ihm von K. mitgeteilt worden sei. Demnach lohnt sich eine nochmalige Veröffentlichung jedenfalls an und für sich nicht, sondern allenfalls nur mit Rücksicht auf solche für den zweiten Teil beabsichtigte Beispiele, für die diese Erweiterung tatsächlich von Belang sein sollte.

Den tieferen Grund der festgestellten Unzulänglichkeiten sehe ich übrigens in einer gewissen partiellen Blindheit der hergebrachten Algebra der Logik (zum Unterschied von der heutigen symbolischen = exakten Logik), die zu ihrer Zeit verzeihlich sein mochte, es aber heute nicht mehr ist. Sie scheint nämlich zu übersehen, daß ihre Lehrsätze ja nicht lediglich die Logik *betreffen*, sondern zugleich selber Sätze der Logik, also logische Gesetze *sind*. Man stellt sich zu sehr auf den Standpunkt, die Logik *zu behandeln*, statt daß man Logik *betreibt*, wodurch eine künstliche Trennung nach Gegenstand und Gehalt hereinkommt, durch die die naturgemäße Auffassung und Darstellung gewaltsam verhindert wird. Ausführlicher habe ich mich über diese Dinge ja, wie gesagt, in meiner Arbeit Math. Ann. 86, besonders in § 1, geäußert. Aber ein ernstliches Umlernen ist nun einmal in einem gewissen Alter leider nicht mehr möglich.

Selbstverständlich steht es Ihnen frei, auch diesen Brief, soweit Sie es für erwünscht halten, und mit den Ihnen angebracht erscheinenden Änderungen im Ausdruck K. mitzuteilen. Im Hinblick hierauf bin ich auf einiges eingegangen, was Ihnen nach meinen kurzen Andeutungen kaum ganz verständlich geworden sein kann, was es aber für K. ist oder doch sein sollte. Mein Zweck war hierbei nicht mehr die Beurteilung der Publikationsreife an sich, sondern in erster Linie der, K. die Gewißheit zu geben, daß seine Arbeit unbedingt mit der Gewissenhaftigkeit die ja im Sinne Ihrer Mitteilung bereits erledigt ist, geprüft worden ist, die sie als ein Werk von ihm verdient, und

42 logischen] This word has been added below the line (typewritten) with an insertion mark and two slashes separating it from the text of the following line proper.

zugleich die positive Verwertung seiner Leistungen vorbereiten zu helfen.

Leider ist Ihre Bite um Rücksendung nicht eindeutig; Sie schreiben nämlich: "Ich bitte Sie um Rücksendung des Manuskripts Korselt[s]". Ich ersehe daraus noch nicht mit Bestimmtheit, ob ich es an Sie oder an den Verfasser senden soll. Ich nehme das zweite als das Wahrscheinliche an, möchte Sie aber vorsichtshalber bitten, durch Postkarte mitzuteilen, ob meine Annahme richtig ist oder Sie es selbst noch einsehen möchten, damit die Handschrift nicht mehr als nötig umhergeschickt wird. – Leider konnte ich wegen meines augenblicklichen gesundheitlichen Bedingens die Angelegenheit nicht so rasch erledigen, wie ich es gern getan hätte.

Mit den besten Grüßen

Ihr

Heinrich Behmann

A.3 Behmann's Letters to Bernays

In parallel to his correspondence with Blumenthal about the manuscript, Behmann also responded personally to Bernays in a letter dated March 1 1937:

A.3.1 Behmann's Response to Bernays

Lieber Herr Bernays,

die übersandte Arbeit von Korselt habe ich inzwischen durchgesehen und Ihrem Vorschlag gemäß meine Beurteilung an Blumenthal geschickt. Aus dem beiligenden Durchschlag werden Sie ersehen, dass ich bezüglich einer Veröffentlichung in dieser breiten, durch vielerlei kaum oder überhaupt nicht zur Sache gehöriges Beiwerk überladenen und dabei vielfach unklaren und wenig übersichtlichen Darstellung leider doch ernstliche Bedenken habe. Ich glaube, daß so der tatsächliche Gewinn den Aufwand – und auch die Mühe des Lesers – nicht lohnen würde. Nach meiner Ansicht käme nur eine dem heutigen Standpunkt sowohl sachlich als formal entsprechende und zugleich den üblichen und berechtigten Anforderungen an eine mathematische Abhandlung gemäße Umarbeitung in Frage, für die irgendwie gesorgt werden könnte und sollte.

Bezüglich der sachlichen Einzelheiten kann auch ich freilich insofern nur mit einem gewissen Vorbehalt urteilen, als mir hier und da einiges – was nicht gerade zugunsten der Arbeit spricht – durchaus dunkel geblieben ist. Wenn das Gebiet, wie man doch wohl annehmen muß, der Klasse bzw. der Aussagefunktion einer Variablen entspricht, so ist mir rätselhaft, was unter dem konversen Gebiet $\breve{a}$ (zum Unterschied vom Negat $\overline{a}$) verstanden werden soll. Eben sehe ich allerdings, daß im folgenden Satz (S. 14), wie auch schon auf der Seite vorher $0'$ und $1'$ als Gebiete bezeichnet werden. Danach würde also die Relative zu den Gebieten gerechnet. Dann stimmt aber die Formulierung des "Müllerschen Bestätigungssatzes" (S. 69-70) nicht. Diese lautet: "Sind f und g endliche Gebietsfunktionen mit

denselben Argumenten, die auch Gebiete als Werte haben, so gilt: Die Subsumption $f(x,y,...)\Subset g(x,y,...)$ wie auch die Gleichung $f(\ ) = g(\ )$ ist genau dann eine Formel, wenn die betreffende Beziehung immer erfüllt ist, falls man für die Argumente irgendwelche *Satz*werte (0 bzw. 1) einsetzt". Dazu gehört die Erklärung (S. 12): "Eine Funktion heiße logisch endliche Funktion, wenn sie keine Erstrecker enthält und durch endlich-oftmalige Anwendung der 6 (!) [^{v}]logischen Grundrechenarten entsteht". (Dabei weiß man nicht, ob man die in $a\natural b$ und $a;b$ steckenden gebundenen Veränderlichen mitzählen soll oder nicht.) Nun sind aber z.B. die Formen $\breve{a}\Subset a$ oder $a;a\Subset a$ nicht allgemeingültig, während sie doch für die "Satzwerte" 0 und 1 erfüllt sind. – Ich meine, eine solche Darstelung sollte der Mindestanforderung genügen, daß sie für jeden, der den ersten Band der Principia Mathematica kennt, ohne Schwierigkeit verständlich sein wollte; alles weitere sollte in strenger und zugleich verständlicher Weise erklärt werden.

Vor kurzem habe ich meinen Vorschlag zur Vereinheitlichung der logischen Symbolik und Terminologie im wesentlichen fertiggestellt und zunächst an Scholz geschickt; er soll dann vervielfältigt und an die Mitglieder des Arbeitsausschusses verschickt werden. Sch. ist gerade damit zur Erholung in St. Moritz.

An der Universität ist seit einiger Zeit recht wenig los. Abgesehen von dem wegen des vernünftigen Berufseinsatzes notwendigen Rückgang der Zahl der Studierenden kommen jetzt noch die vorübergehenden, aber doch eine Reihe von Jahren dauernden Ausfälle durch Heeres- und Arbeitsdienst und durch den Geburtenausfall der Kriegsjahre hinzu. So haben wir im letzten Winter keine Anfängervorlesungen gehalten. Für mich ist immerhin angenehm, daß ich dadurch mehr zum eigenen Arbeiten komme.

Vielleicht haben Sie eine Diskussion im "Mind" verfolgt. Perelman gab vor einiger Zeit eine angebliche neue Auflösung der Paradoxien. Zunächst hat Grelling darauf erwidert; doch schien mir dies recht unbefriedigend. Ich habe nun inzwischen eine Entgegnung eingeschickt, die wohl im nächsten Heft erscheint.

Vielleicht schreiben Sie mir noch, was mit dem Manuskript geschehen soll. Ich habe noch einen weiteren Durchschlag des beiliegenden Briefes; doch bin ich etwas im Zweifel, ob ich ihn an K. schicken soll, da ich ihn nicht entmutigen möchte. In der Hoffnung, daß es auch Ihnen gut geht, verbleibe ich mit freunglichem Gruß

Ihr

Heinrich Behmann[A]

A.3.2 Bernays' Response (dated April 13 1937)

Lieber Herr Behmann!

Schon vor etwa drei Wochen hatte ich einen Brief an Sie begonnen[I], um Ihnen auf Ihren freundlichen Brief

[A] The signature is hand-written.

[I] A draft of this letter is extant in a photocopy from the Bernays-archive in Zürich – its library signature there is "Hs 975:278" – in the same folder of the Behmann-Nachlass held by the SBB. It is dated "24. März 37".

von Anfang März zu antworten und Ihnen für Ihre Mühewaltung bei der Begutachtung der Arbeit von Korselt zu danken. Beim nochmaligen Durchgehen Ihres Briefes wurde ich jedoch daran erinnert, dass Sie einen Bescheid darüber wünschten, was mit dem Korselt'schen Manuskript geschehen solle. Da ich von Prof. Blumenthal in der Angelegenheit noch nichts gehört hatte, so fand ich es gut, ihn zunächst deswegen anzufragen. Nun habe ich bisher auf diese Anfrage noch keinen Bescheid erhalten, (es muss wohl irgend etwas besonderes vorliegen, da sonst Prof. Blumenthal immer sehr umgehend antwortet). Auf diese Weise kommt nun mein Schreiben recht spät, und ich bitte Sie, aus den genannten Gründen zu entschuldigen, dass ich Ihnen nicht früher geantwortet habe.

Ich bin Ihnen sehr dankbar dafür, dass Sie die Durchsicht und Begutachtung der Korselt'schen Abhandlung ausgeführt haben. Ihre resultierende Beurteilung hat mich nicht sehr überrascht, immerhin ist sie noch etwas ungünstiger ausgefallen, als ich erwartet hatte. Dass die Arbeit in der vorliegenden Fassung nicht zur Publikation geeignet ist, schien mir schon auf Grund meiner provisorischen Einsichtnahme als wahrscheinlich. Jedenfalls stimme ich Ihnen darin zu, dass eine Arbeit über Logistik (sofern sie nicht ausdrücklich auf kurz vorher erschienene Bezug nimmt) so abgefasst sein soll, dass ein Kenner der Principia Mathematica sie ohne Schwierigkeit versteht. Hierzu dürfte es aber wohl im vorliegenden Falle nicht nötig sein, dass Herr Korselt von seiner Symbolik (die ja auch an eine Tradition anknüpft) abgeht, | und es würde in betreff der Symbolik und der Bezeichnungen wohl genügen, wenn am Anfang eine übersichtliche Zusammenstellung der gebrauchten Begriffe, Termini und Symbole nebst Uebersetzung in die Russell'sche Symbolik gegeben würde. Natürlich bestehen aber vor allem die von Ihnen geltend gemachten Erfordernisse einer redaktionellen Umarbeitung.

Für die Bemühungen um die Vereinheitlichung der logischen Terminologie und Symbolik habe ich übrigens entschiedene Sympathie, und es interessierte mich sehr, von Ihnen zu hören, dass Sie sich von neuem dieser Aufgabe gewidmet haben.

Mit den besten Wünschen für ein gutes Sommer-Semester und mit freundlichen Grüssen

Ihr

Paul Bernays

Nach Ihrer Erwiderung auf Grelling in der Paradoxien-Diskussion werde ich bei nächster Gelegenheit Ausschau halten.[B]

A.3.3 Bernays on Unifying Notation

Following this letter there is an undated sheet (a carbon copy, judging by the paper) concerned with the unification of symbolism. On it has been written "Kopie Brief Bernays, Besenrain str 30 Zürich.", which, unfortunately, is

[B] This postscript, as well as the signature, are hand-written.

8)] The closing parenthesis is missing in the typescript but should presumably be inserted here.

17 und der Bezeichnungen] These words have been added (typewritten) above the line with a (hand-drawn) insertion mark.

ambiguous and could mean, either, that the sheet is a copy of a letter *to* or a letter *from* Bernays. Since the views expressed are not those Behmann expresses in his letters and manuscripts on the topic, I assume the following reflections on the unification of symbolism are *Bernays*' views on the topic.

Was Zunächst die Gesichtspunkte der Zielsetzung betrifft, so scheinen mir Verabredungen für die Symbolik beim einführenden Unterricht nur mittelbar relevant, insofern sie durch die bei solchem Unterricht sich bildenden Gewohnheiten Einfluss auf die Symbolik der Publikationen ausüben. Es kann wohl darüber Einigkeit bestehen, dass es bei den vorliegenden Bemühungen um die Vereinheitlichung der Symbolik nicht darauf ankommen kann, wie der einzelne zu seinem privaten Vergnügen, oder ein Kreis von privatim Arbeitenden die logische Formeln schreibt. Worauf es meiner Meinung ankommt, ist, dass einer Ueberproduktion an logischer Symbolik vorgebeugt wird, welche die Lektüre der Arbeiten über die Logistik in unnötiger und unfruchtbarer Weise erschweren könnte. Der geeignete Weg zu diesem Ziele scheint mir nicht der zu sein, dass man eine einzige Symbolik als die allgemein zu bevorzugende hinstellt, welche im wesentlichen nichts anderes ist als die Symbolik der Principia Mathematica, vielmehr dürfte es aussichtsreicher sein, wenn durch Nebeneinanderstellung von einigen wenigen bereits im Gebrauch befindlichen, bezw. aus solchen kombinierten Arten der Symbolik deutlich gemacht würde, was für Möglichkeiten innerhalb des bereits vorliegenden Vorrates an Symbolik schon gegeben sind; es könnte dann auch für solche Arten der Symbolik (sofern sie nicht nach Autoren oder Schulen charakterisiert sind) kurze Bezeichnungen eingeführt werden.

Gewiss würde sich eine solche Mehrheit erübrigen, wenn die Russellsche Symbolik in jeder Beziehung offensichtlich das Ideale wäre. Das kann aber wohl nicht behauptet werden. Z.B. haben andere Arten der Symbolik gegenüber dieser den Vorzug grösserer dualistischer Symmetrie. Für mengentheoeretische Zwecke ist ferner die Schröderische Summen- und Produktsymbolik von Vorteil, (wie dies insbesondere von Kuratowski hervorgehoben worden ist). Ein äusserlicher Nachteil der Russellschen Symbolik ist, dass auf Grund der naturgemässen Verabredung für die Klammerersparung (Nr. 13 des Entwurfes) bei einer Verbindung der Form $p \vee \sim q$ ein Hiatus entsteht. Das schon sehr alte Ueberstreichungssymbol für die Negation erscheint mir als recht übersichtlich und hinsichtlich der Klammerersparung vorteilhaft. Der Nachteil, dass bei diesem die lineare Anordnung durchrochen würde, ist für die meisten Zwecke nicht erheblich.

Als Beispiele von Arten der Symbolik, welche *neben* der Russellschen erwähnenswert scheinen, möchte ich die folgenden beiden anführen:

---

8 sein] The typscript erroneously has "sien".

16 kann] In the typescript "kann" is erroneously capitalised.

20 $p \vee \sim q$] The formula is inserted by hand in the typescript.

	Mengentheoretische Symbolik	Dualistische Symbolik
1. Negation	$-$	$-$, bzw. $\neg$, $\sim$
2. Disjunktion	$+$	$\vee$
3. Konjunktion	$\cdot$	$\wedge$ (Heyting)
4. "es gibt"	$\sum$	$\bigcup_x$ nach Peano
5. "alle"	$\prod$	$\bigcap_x$ nach Peano
6. Implikation	$\supset$	$\supset$ bzw. $\rightarrow$
7. Aequivalenz	$\supset\subset$	$\supset\subset$ " $\equiv$

Als Variante der Russellschen Symbolik erscheint mir jedenfalls die Ersetzung der Symbole für Konjunktion und Implikation durch & und $\rightarrow$ erwähnenswert, für manche Zwecke der Aussagenlogik ferner die klammernfreie Schreibweise von Lukasiewicz. Wie gesagt sind diese Angaben nur beispielsweise gemeint.

Gegenüber dem Einwand, dass auf diese Weise eine gemeinsame Symbolik nicht erreicht werden könne, möchte ich sagen, dass mir die Erreichung dieses Zieles überhaupt als recht unwahrscheinlich und auch als nicht so erheblich erscheint. Der wesentliche Gesichtspunkt scheint mir zu sein, der Gefahr vorzubeugen, dass bei fast jeder neuen logischen Publikation eine neue Symbolik vorgesetzt wird. Hierfür kann es wohl das geeignete Mittel sein, dass den Autoren anhand einer fachlich vereinbarten Zusammenstellung zum Bewusstsein gebracht wird, dass für verschiedene Zwecke bereits angepasste Ausdrucksformen vorgesehen sind.

Um noch auf einen speziellen Punkt hinzuweisen, so möchte ich vorschlagen, dass man sich betreffs der Buchstaben (Punkt B) auf Vorschläge über Buchstaben*sorten* beschränkt. Zeichen für Konstanten (Symbole) aus demselben Alphabet zu wählen wie Variablen, erscheint mir als unvorteilhaft. Es wird wohl zweckmässig sein, das kleine und das grosse lateinische Alphabet vollständig für Variablen zu reservieren (sofern nicht zwischen Kursiv- und Druckbuchstaben unterschieden wird). Es ist hier auch zu beachten, dass es vielfach angezeigt erscheint, für die gebundenen Variablen andere Buchstaben zu wählen als für die freien Variablen. Wenn für die Individuen-Variablen kleine lateinische Buchstaben genommen werden, so dürfte es angemessen sein, für die Satzvariablen auch grosse Buchstaben zu nehmen.

---

34 Zusammenstellung] The manuscript erroneously has "Zusammendtellung".

35 angepasste] The manuscript erroneously has "angepaste".

39 reservieren] The typescript erroneously has "reservierne".

40 hier] The typescript erroneously has "heir".

Appendix B

The Convolute Cod. Ms. D. Hilbert 559

Editorial Introduction

The convolute assigned the signature "Cod. Ms. D. Hilbert 559" by the SUB Göttingen comprises three items: (1) an exercise book with a dark purple cover of 48 originally blank pages, into which (2 & 3) two packets of separate sheets have been laid (at the end). The exercise book (1) itself has been filled with a (partial) set of lecture notes of Hilbert's lecture-course from the summer semester of 1913 written by Bernhard Baule (with the exception of three sheets in Hilbert's handwriting glued into the exercise book (on 1^v, 20^r, 24^v)).

The text on the separate sheets inlaid in back is in Hilbert's own handwriting.

The first packet (2) of sheets is smaller than the pages of the exercise book; the second (3) is the same size, but (since the sheets show no sign of having ever been stapled, as the pages of the exercise book have) not torn from the exercise book itself, or any other exercise book of the same kind.

There is, in addition, a noticeable imprint of another item on many pages of the exercise book, which suggests that a smaller notebook with rounded corners was also at some point and for a long time part of this convolute, but has since disappeared. Indeed, Hilbert's corrections refer on at least two occasions to page numbers, which do not seem to correspond to any of the material presently contained in the convolute. These references may have been to the missing item whose imprint lingers, and that missing item may have been Hilbert's Kollegheft for the 1905 lecture (which, as far as I am aware, is considered lost).

Only 8 pages (21^r-24^v of the notebook) were originally paginated (with page-numbers "-1-" to "-8-"), but all sheets of the convolute have been continuously paginated subsequently by an archivalist.

Texts

B.1 Baule-Ausarbeitung.

Einige Abschnitte aus der Vorlesung über die Grundalgen der Mathematik und Physik (S.S. 1913) [1r]

(Axiomatische Begründung der ganzen Zahlen, Begründung der ganzen Zahlen mit Hilfe der Mengenlehre, Unmöglichkeitsbeweise für geometrische Probleme, Grundzüge der neuen Mechanik.)

(Ausgearbeitet von B. Baule)

Der Zahlbegriff und die Axiomenlehre.

In diesem Kapitel werden wir Fragen zu untersuchen haben, in denen Mathematik, Logik und Philosophie aufs engste ineinandergreifen.

Im ersten Abschnitt[1] hatten wir die Axiome der Algebra formuliert, ohne jedoch ihre Widerspruchslosigkeit nachzuweisen. Die Widerspruchslosigkeit der *geometrischen* Axiome gelang uns auf die Widerspruchslosigkeit der *algebraischen* Axiome zurückzuführen. Wie steht es nun mit der Widerspruchslosigkeit der algebraischen Axiome? Diese Frage soll uns jetzt beschäftigen. Derartige Beweise sind durchaus nicht einfach. Der Beweis der Widerspruchslosigkeit ist vollkommen aequivalent dem Beweis für die mathematische Existenz. Wollte |man auf diesen Beweis [2v] verzichten, so könnte man sich die Sache sehr einfach machen, indem man alle Sätze als Axiome hinstellte. Das geht natürlich nicht. – Unter Umständen kann es jedoch vorteilhaft sein, gewisse Sätze den Axiomen hinzuzufügen. Weist man dann nach, daß das neue System in sich widerspruchslos ist, so ist damit gleichzeitig die Richtigkeit des betreffenden Satzes dargetan. Beispielsweise könnten wir dem System der 16 algebraischen Axiome[2] den Fundamentalsatz der Algebra hinzufügen, ohne fürchten zu brauchen, jemals auf Widersprüche zu geraten. –

Auch vom pädagogischem Standpunkte ist es vielfach ratsam, schwierigere Sätze zunächst als Axiome hinzustellen und den Beweis auf später zurückzustellen. Man muß sich nur bewusst sein, *was* man voraussetzt; solange bleibt man auf dem Boden der strengen Mathematik. | [3r]

Für die Widerspruchslosigkeit der 16 algebraischen Axiome lieferten Dedekind und Weierstraß die ersten Beweise. Sie gaben durch Schnitte bzw. unendliche Reihen die "Zahlen" an, die den 16 Axiomen Genüge leisten.

Sowohl Dedekind wie Weierstraß geht dabei von der bis dahin stets vertretenen Ansicht aus, daß die Gesetze für die ganzen Zahlen "von Gott geschaffen"[3] und unantastbar seien. Es bestand demgemäß bei Weierstraß jeder Beweis

[1] No such section is recorded in Baule's Ausarbeitung. Presumably Hilbert took it from the 1905 lecture. Cod. Ms. Hecke Df 10, which appears to record (a different portion) of the same lecture-course, does begin with a heading "Axiome der Algebra."

[2] Cod. Hecke Df 10 lists 18 axioms of algebra...

[3] See the remarks on the fly-leaf in front.

eines Satzes darin, den betreffenden Satz auf die Gesetze der ganzen natürlichen, positiven Zahlen zurückzuführen.

Auf der Suche nach dem "Wesen" der ganzen Zahlen sind die lächerlichsten und naivsten Vorstellungen und Definitionen gegeben worden. Die Zahlen sind "Zeichen". Es wird Wert gelegt auf die Größe und den Abstand dieser "Zeichen". Überhaupt spielt das Aussehen eine große Rolle. Bei solchen |Erörterungen über das Wesen der ganzen [3v] Zahlen kann man sich nicht über Gegensätze und Widersprüche wundern, die sich in der damaligen Philosophie zeigten. Die Versuche, sich von dem wahren Wesen der Zahlen Rechenschaft zu geben, mißlangen immer wieder.

(Siehe: Dedekind: "Was sind und was sollen die Zahlen" und Frege: "Grundgesetze der Arithmetik".)

Axiome der ganzen positiven Zahlen:

I. 1 ist eine Zahl.

(Es existiert ein Anfang)

II. Wenn n eine Zahl ist, so ist auch $n+1$ eine Zahl.

(Es gibt einen Process des Zählens)

III. $n+1 > n$; wenn $b > a$ und $c > b$, so $c > a$

(Anordnungsaxiom)

IV. Wenn $b > a$, so $b \neq a$; (Axiom der Verschiedenheit)

V. Wenn eine Aussage X_n richtig ist für $n = 1$ und für $n+1$, falls sie für n richtig ist, so ist sie allgemein richtig [4r] (Induktionsaxiom) |

Unabhängigkeit der Axiome:

Die Unabhängigkeit eines Axiomes von den übrigen zeigt man am einfachsten, indem man ein System von Dingen vorzeigt, das alle anderen Axiome befriedigt, nur das eine nicht.

---

25 natürlichen, positiven] Added (by Baule) in ink above the line: "nat. pos.".

35 Wenn n eine Zahl ist,] "II." has been crossed out (by pencil), indicating that in the revision of the list the first two axioms are to be taken together as one.

37 $n+1 > n$;] Concurrent with the crossing out of "II." above, the third "I" of "III." has been crossed out here, making "$n+1 > n$" the second axiom in the revised list.

37 wenn $b > a$ und $c > b$,] A new "III" has been inserted before "wenn" in pencil, making the following the third axiom of the revised list. From here on, the original numbering is left unchanged.

38 (Anordnungsaxiom)] In pencil Hilbert has written "Transitivität" next to "(Anordnungsaxiom)".

39 Wenn $b > a$, so $b \neq a$;] In pencil, Hilbert has written $a \not> a$ in the left margin next to this line.

39 (Axiom der Verschiedenheit)] In pencil, Hilbert has written "nicht reflexiv" underneath "(Axiom der Verschiedenheit)".

40 allgemein] Hilbert has crossed out "allgemein" and written "für jede ganze Zahl n" underneath the line.

So erkennt man[4] die *Unabhängigkeit von IV* indem man etwa ein System gibt, das nur aus der Zahl 1 besteht, und für das die Beziehungen gelten sollen

$1+1=1; \qquad 1>1$

Dieses System befriedigt sämtliche Axiome außer IV. Also ist IV unabhängig von den übrigen.

Die Unabhängigkeit von III leuchtet ein, wenn wir das System angeben:

$1; \qquad 1+1=2; \quad 2+1=2$

$1>2; \qquad 2>1$

Nur Axiom III wird durch dieses System nicht befriedigt

Unabhängigkeit von V wird bewiesen durch das System:

$$\left.\begin{array}{llll} 1, & 2, & 3 & \dots \\ 1', & 2', & 3' & \dots \end{array}\right\} \begin{array}{l} 1'>1,\dots \\ 2'>2 \end{array}$$

| [4v]

Das Problem ist nun , zu zeigen, daß aus diesem System von 5 Axiomen alle Sätze bewiesen werden können.

Satz 1: Wenn $a \neq 1$; so $a > 1$

Denn: 1) Der Satz ist richtig für $a = 1$, dann da sagt er nichts aus

2) wenn er für a richtig ist, so ist er für $a+1$ richtig

für $a = 1$ ist aber $a+1 = 1+1$ auch > 1, d.h. der Satz ist allgemein richtig.

Satz 2: Wenn $b > a$, so $a < b$

(Definition $a < b$ heißt: "$a \neq b$ und a nicht $> b$")

Wäre a nicht $< b$, so müßte entweder

[4]Hilbert rearranged the order of these independence proofs later (by numbering them according to the new intended order with blue crayon such that the independence proof for axiom IV comes last (it is numbered "4"), the one for axiom III comes second-to-last (it is numbered "3."), as well as adding two further brief remarks (numbered "1." and "2."), which read: "1. Axiome I u. II [Hilbert crossed out 'u. II' again, but did not change the plural 'Axiome'] sind offenbar nothwendig. 2. Axiom II ist von den übrigen unabh. Beispiel $1, 1+1=1$". This new order has been followed in the second Ausarbeitung.

45 ein] Baule originally wrote "folgendes", then put it in brackets as well as crossed it out and wrote "ein" above the line to replace it.
52–54 $1, 2, 3 \dots 2' > 2$] Hilbert has crossed out the formulas and written to their right (in pencil) "$n(+1) = n+1$ / $n'+1 = (n+1)'$" and underneath: "Definition X_n also $1 = 1$ und X'_n also $1 = 2$".
56 *Satz 1*:] Hilbert has written another "1" (in pencil) next to the number "1" written by Baule (in ink), indicating that he intends this proposition to appear as proposition 11 in a revised arrangement of the propositions.
57 Denn:] Hilbert has crossed out the entire original proof in pencil and written "aus Satz 2 u. [crossed out: 9] 7" in the left margin.
59 auch > 1] Hilbert has added in pencil "nach Axiom III" (of the revised numbering).
60 *Satz 2*:] Hilbert has crossed out "2" and written "1" instead. He also added the remark "(Statt dieses Satzes" (in pencil) followed by "Satz 1 (Meine S. 2!)" (in ink (but still Hilbert's handwriting). What "Meine S. 2" refer to is unclear.
62 Wäre a nicht $< b$, so müßte entweder] Hilbert has crossed this phrase out in pencil and has written "Es ist also zu beweisen" to the left with a pencil-line indicating that these words are to be read first in the following line, where the proof starts.

$a = b$ sein, das widerspricht der Voraussetzung

oder $a > b$, dann folgt aus Axiom III: $a > a$ und das widerspricht dem Axiom IV. |[5] [5r]

Satz 3 $a + b = b + a$

Wir bezeichnen:

$$(a, b) = a + b$$

$$(a, 1) = a + 1$$

$$(a, b + 1) = (a, b) + 1$$

Zu beweisen ist: $(a, b) = (b, a)$;

a) für $b = 1$ $(a, 1) = (1, a)$

1) Der Satz ist richtig für $a = 1$

2) er ist richtig für $a + 1$, falls er für a richtig denn:

$$(1, a + 1) = (1, a) + 1$$

$$= (a, 1) + 1 = a + 1 + 1$$

$$= (a + 1, 1)$$

b) wenn der Satz für b richtig ist, so ist er auch für $b + 1$ richtig.

Denn

$$(a, b + 1) = (a, b) + 1 = (b, a) + 1$$

$$= (b, a + 1)$$

Nun ist:

[5] At the top of the page, Hilbert has written in blue crayon "Erst Satz 9: assoziative Gesetz!".

63 $a = b$ sein] Hilbert has written "1. $a \neq b$ in dem Fall wäre" in front of this line. Though the "sein" has no place in the revised sentence, Hilbert neglected to cross it out.

63 widerspricht der Voraussetzung] Hilbert has written "u. Axiom IV" to the right of this phrase.

64 oder $a > b$] Hilbert has written "2. a nicht $> b$ in dem Fall wäre" in front of this line and crossed out "oder".

64 aus Axiom III] Hilbert has inserted "Voraussetzung u" between "aus" and "Axiom III".

64 widerspricht dem Axiom IV] Hilbert has inserted "wiederum" between "dem" and "Axiom IV".

65 *Satz 3*] Hilbert has crossed out "3", correcting it, first, to "6", then (crossing out "6") to "4" and finally (crossing out "4") to "10".

66–68 Wir bezeichnen: … $(a, b) = (b, a)$] Hilbert has crossed out this part of the original proof and written to its left: "Def. der Addition: $a + (b + 1) = (a + b) + 1$ nach Axiom V ist damit $a + b$ definiert.".

69 a) für $b = 1$ $(a, 1) = (1, a)$] Hilbert has written "(a als Parameter)" above "$(a, 1) = (1, a)$", but (subsequently) crossed out that addition, as well as "$(a, 1) = (1, a)$" and also the "a)" along with the part of the proof originally given "für $b = 1$" and written in red crayon "nach □". A red □ appears on the following page, where the following replacement proof is found: "$1 + a = a + 1$ Denn für $a = 1$ richtig. Wenn für a richtig, so für $a + 1$. Denn $1 + (a + 1) = (1 + a) + 1 = (a + 1) + 1$.".

73 b)] Hilbert has crossed out "b)".

74–78 Denn $(a, b + 1) =$…$= (b + 1, a)$] Hilbert crossed out this portion of the proof as well, writing the following replacement in the right margin: "$a + (b + 1) \overset{\text{nach Def.}}{=} (a + b) + 1 \overset{\text{nach Def.}}{=} (b + a) + 1 = b + (a + 1) = b + (1 + a)$ nach □ $= (b + 1) + a$ nach Satz 3".

74–76 $(a, b)+1 =$…$= (b, a+1)$] Hilbert has added "=(b+1)+a nach Satz 2 u. 3" after the final formula in this sequence, then subsequently crossed out "2 u." before crossing out the entire proof.

$$\begin{array}{rcll}(b, a+1) & = & (b, 1+a) & \text{(nach a))}\\ & = & (b,1)+a) & = \quad (b+1, a)\end{array}$$

Also $(a, b+1) = (b+1, a)$ |[A] [5v]

Satz 4: $(a+1)+b = a+b+1$.

Denn:

$$\begin{array}{rcll}(a+1, b) & = & (b, a+1) & \text{(Satz 3)}\\ & = & (b,a)+1 & \text{(Def.)}\\ & = & (b+1, a) & \\ & = & (a, b+1) & \text{} \} \text{ (Beweis zu Satz 3)}\\ & = & a+b+1 & \end{array}$$

Satz 5: Wenn $a \neq 1$, so $a = b+1$

(Wenn $a \neq 1$, so gibt es stets eine vorangehende)

1) Der Satz ist richtig für $a = 1$; (sagt nichts aus)

2) Wenn er für a richtig ist, so auch für $a+1$[;] für $a+1$ is er per se richtig

Also ist der Satz nach Axiom V allgemein richtig. | [6r]

Satz 5: Wenn $a < b$, so $b = a+c$

1) Der Satz ist richtig für $a = 1$ (nach Satz 5)

Wenn nur $b \neq 1$, $b = 1$ ist durch d. Voraussetzung ausgeschlossen.

2) Wenn der Satz für a richtig ist, so auch für $a+1$, denn wenn $a+1 < b$, so auch $a < b$ (denn wäre $a > b$, so

[A] Hilbert has inserted at the top of this page in pencil: "*Satz 9* (a+b)+c=a+(b+c)". The proposition was first numbered "4", which was crossed out and replaced by "3", which was crossed out and replaced by "4" (in blue crayon), which was also crossed out, leaving "9" and the remark (written above the line): "Vor 3 rücken [unreadable] von Hülfssatz und Beweis der Rekursion.". A sketch of the proof is written in the left and top margin: "Satz 4" the following remark is written in the margin: "ist richtig für $c = 1$ nach Def. der Addition[.] Wenn für c richtig so auch für $c+1$ denn [the remark continues at the top of the page] $(a+b)+(c+1) = ((a+b)+c)+1 = (a+(b+c))+1 = a+((b+c)+1) = a+(b+(c+1))$"

79 *Satz 4*: $(a+1)+b = a+b+1$.] Proposition 4 and its proof has been crossed out entirely, apparently to be replaced by the proposition 9 he wrote above it. In the statement of the proposition Hilbert has (presumably before striking out the proposition entirely) replaced "1" with "c" and added parentheses around "$b+1$" on the right-hand side of the equality-symbol.

82 *Satz 5*:] Hilbert has crossed out the "5" and changed it first to "3" and then to "2". He has also written above the line (half in pencil, half in ink): "[in pencil] Statt dieses Satzes [in ink] Satz 2a, 2b Meine Seite 2!".

87 *Satz 5*:] Hilbert has crossed out the "6" and changed it first to "7" and then to "5". He has also written above the line (in pencil): "(weglassen!)".

88 (nach Satz 5)] Hilbert has changed the number of the proposition referred to from "5" to "2" and added (in pencil) "und Satz [crossed out: 5] 4".

89 Wenn nur $b \neq 1$, $b = 1$ ist durch d. Voraussetzung ausgeschlossen.] This line of the proof has been stricken out by Hilbert.

90–91 (denn wäre $a > b$, so wäre auch $a+1 > b$ nach III)] Hilbert has added underneath this line "und wenn $a = b$, so $a+1 > b$".

wäre auch $a+1>b$ nach III)[;] also: $b=a+c$; nun ist $c\neq 1$ dann $b\neq a+c$ (Vor.), d.h. $b=a+1+c'$ (nach Satz 5 und 4)

Also ist der Satz nach Ax. V allgemein richtig.

Satz 7: Wenn $b>a$, so $b=a+c$

Denn: Wenn $b>a$, so $a<b$ (Satz 2) und somit $b=a+c$ (Satz 6)

Satz 8 Wenn $b=a+c$, so $b>a$

1) Der Satz ist richtig für $c=1$ (Ax. III) für alle a

2) Wenn er richtig ist für c, so auch für $c+1$

Denn:
$$\begin{aligned} b'=a+c+1 &> a+c && \text{(Ax. III)}\\ &> a && \text{(Voraussetzung)}\end{aligned}$$

Somit gilt der Satz nach Ax. V allgemein. | [6v]

Satz 9: Wenn $a<b$, so $b>a$

Denn[6] nach Satz 6 ist $b=a+c$ d.h. $b>a$ (Satz 8).

Satz 10: Wenn $a>b$ und $a\neq b+1$ so $a>b+1$

[6] Next to the proof, Hilbert has written in pencil "Satz 5,7"

91 (Vor.)] Hilbert has crossed out "Vor[aussetzung]" and replaced it with "weil $a+1<b$".

91 d.h.] Hilbert has stricken out "d.h." and replaced it with "also nach Satz 2 $c=d+1$".

91–92 $b=a+1+c'$ (nach Satz 5 und 4)] Hilbert has crossed out everything after "$b=$" and written underneath the line "$a+d+1=(a+1)+d$ q.e.d.".

94 *Satz 7*:] Hilbert has changed the number first to "8", then to "6" and finally to "11".

94 Wenn $b>a$, so $b=a+c$] Hilbert has switched the variables, so the proposition reads: "Wenn $a>b$, so $a=b+c$".

95 Denn: Wenn $b>a$, so $a<b$ (Satz 2) und somit $b=a+c$ (Satz 6)] The entire proof has been crossed out. In the right margin, Hilbert has written: "[crossed out: 'Indu'] a [crossed out: 'b'] als Parameter Induktion nach [crossed out: 'a'] b Anwendung von Satz 4".

95 (Satz 2)] Hilbert has corrected "2" to "1".

95 (Satz 6)] Hilbert has corrected "6" to "5", then stricken out the reference and added "nach Satz 5" earlier in the line after "und somit".

96 *Satz 8*] Hilbert has changed the number first to "9", then to "7" and finally to "12".

96 Wenn $b=a+c$, so $b>a$] Hilbert initially switched the variables so the proposition would read "Wenn $a=b+c$, so $a>b$", but then crossed out the correction along with the original statement of the proposition and written above it "$a+b>a$". And underneath that: "Induktion nach b!". The original proof has been crossed out along with the original statement of the proposition.

97 für alle a] Hilbert has crossed out this addition.

99 $\begin{aligned} b'=a+c+1 &> a+c && \text{(Ax. III)}\\ &> a && \text{(Voraussetzung)}\end{aligned}$] Hilbert has crossed out the prime following the "b". Hilbert also crossed out "Voraussetzung" and written underneath: "weil der Satz für c gilt.".

101 *Satz 9*:] Hilbert has corrected the number "9" first to "10", then to "8". Subsequently (but without crossing out either the number "8" or the proposition, he has written above the proposition: "Statt 8: Es muss mindest[ens] einer der 3 Fälle $a \overset{<}{=}{>} b$ eintreten.".

103 *Satz 10*:] Hilbert has changed the number of the proposition to "12".

$a = b + c$ (Satz 7)

$c \neq 1$; denn $a \neq b + 1$ (Voraussetzung)

Also: $a = b + c' + 1 = b + 1 * c'$ (Satz 4) d.h. $a > b + 1$; (Satz 8)[B]

Satz 11: Wenn $a + 1 = b + 1$, so $a = b$

Denn wäre $a > b$[C], so $a = b + c$ d.h. $a + 1 = b + 1 + c > b + 1$ und das widerspricht der Voraussetzung

derselbe Widerspruch folgt für $a < b$[D]

Auf gleiche Weise ergibt sich:

Wenn $a + c = b + c$

so $a = b$

[E] | [7r]

Begründung der ganzen Zahlen mit Hilfe der Mengenlehre.

Mengen haben gleiche Mächtigkeit, wenn sie umkehrbar eindeutig aufeinander abgebildet werden können.

Mengen gleicher Mächtigkeit heißen äquivalent.

Die "Zahl" definiren wir als Mächtigkeit einer Menge.

Die "Zahl" ist also ein Attribut der Mengen; durch die "Zahl" a ist die "Menge" "a" charakterisiert.

Fügen wir zu einer Menge "a" ein Element etwa "1" hinzu, so bezeichnen wir die neue Menge also Menge von der Zahl "$a + 1$".

Dadurch haben wir den Process des Addirens definiert.[F] | [7v]

Sätze, die aus den Axiomen der ganzen Zahlen erst nach Ableitung einer ganzen Reihe von Hülfssätzen folgten, lassen sich bei dieser Begründung der ganzen Zahlen sofort beweisen.

Z.B.

[B] To the left of this proof, Hilbert has written: "Satz 14. Wenn $a + c = b + c$ so $a = b$ [crossed out: 'Bew[eis]'?] Induktion nach c", presumably meant to be inserted into the sequence of propositions here.

[C] Hilbert has added to the left: "oder a nicht grösser als b, dann ist aber nach der Def. [crossed out 'a'] von kleiner $a < b$ [crossed out: 'dah[er]'] etc.…".

[D] Hilbert has added here: "da dann ja $b > a$ ist und ebenso $b + c > a + c$ folgen würde".

[E] Hilbert has written to the left of the proof: "Satz [crossed out: '12'] 9 Wenn a [crossed out: '<'] $> b$, so $a + c > b + c$ / Wenn $a < b'$ so $a + c < b + c$ / $a = b + d$ [so] $a + c = b + c + d$.", presumably to be inserted here.

[F] In the bottom right of the page Baule has written "*Sätze*".

---

104 (Satz 7)] Hilbert has crossed out "7", but without indicating a replacement number.

105 denn $a \neq b + 1$ (Voraussetzung)] Hilbert has crossed out this remark. Underneath this line he has inserted: "fo[l]gli[ch] $c = c' + 1$ [crossed out: 'als[o]'].".

107 *Satz 11*:] Hilbert has crossed out the number "11" and replaced it with "12", then crossed out "12" and replaced it with "13", then crossed out "13" and wrote "10" the the right of "12" and finally crossed out "10" and replaced it with "13" again.

110–112 Auf gleiche Weise … $a = b$] Hilbert has crossed out this remark.

Wenn $a+1=b+1$; so $a=b$

Lässt sich die 1 der linken Seite auf die 1 der rechten abbilden[G], so ist der Satz ohne weiteres klar, dann muß sich auch nach der Definition von Gleichheit a unmittelbar eindeutig auf b abbilden lassen. Bildet sich die 1 der rechten Seite *nicht* auf die 1 der linken ab, sondern auf ein Element α in a, die 1 der linken Seite aber auf ein Element β in b, so haben wir nur unter Beibehaltung aller übrigen Associationen α mit β und 1 mit 1 zu verbinden und es folgt der Satz in ebenderselben Weise wie früher.

[Diagram] | [8r]

Definition: Wenn $e \neq e+1$, so heißt e "endlich".

(Die Endlichkeit wird also durch eine negative Eigenschaft definiert)

Es *gibt* endliche Mengen, denn die Menge 1 ist endlich.

Wir wollen jetzt alle Mengen $\mathcal{Z}$ betrachten, die die Eigenschaften haben:

$$\mathcal{Z}: \begin{cases} \text{I. 1 ist ein Element} \\ \text{II. Wenn } z \text{ enthalten ist, so auch } z+1 \end{cases}$$

Es gibt solche Zahlenmenge $\mathcal{Z}$, denn die Menge aller ganzen endlichen Zahlen hat diese Eigenschaften.

Wir definieren die "natürliche" Zahlenreihe als den "Durchschnitt von $\mathcal{Z}$"$=\mathcal{N}$

"Durchschnitt" von $\mathcal{Z} \equiv$ Gesamtheit der gemeinsamen Elemente aller $\mathcal{Z} \equiv$ kleinster gemeinsamer Teil aller $\mathcal{Z}$. | [8v]

$\mathcal{N}$ hat dieselben Eigenschaften wie $\mathcal{Z}$. Denn 1 ist in $\mathcal{N}$ enthalten, und wenn z enthalten ist, so ist auch $z+1$ enthalten.

Also: $\mathcal{N}$ ist ein $\mathcal{Z}$.

Jetzt können wir bereits den Satz von der vollständigen Induktion, den *Induktionssatz*, beweisen.

X_n bedeute eine Aussage, in der eine natürliche Zahl vorkommt. Wenn dann X_n richtig ist für $n=1$ und für $n+1$, falls es für n richtig, so ist X_n richtig für *alle* natürlichen Zahlen.

Beweis: Die Menge aller natürlichen Zahlen für die die Aussage X_n richtig ist, sei $\mathcal{R}$[.]

$\mathcal{R}$ ist in $\mathcal{Z}$ enthalten nach Voraussetzung, und da $\mathcal{R}$ nur natürliche Zahlen enthält, so ist $\mathcal{R}$ auch in $\mathcal{N}$ enthalten. Also ist $\mathcal{R} \equiv \mathcal{N}$; denn sonst müßte es Elemente in $\mathcal{N}$ geben, die nicht in allen $\mathcal{Z}$ enthalten sind, und das widerspricht der Definition von $\mathcal{N}$. | [9r]

Es sei n eine natürliche Zahl und $\mathcal{Z}_n$ eine Zahlenmenge, die die beiden Eigenschaften besitzt:

$$\mathcal{Z}_n: \begin{cases} \text{I}'.\ n+1 \text{ ist ein Element} \\ \text{II}'.\ \text{Wenn } z \text{ ein Element ist, so auch } z+1 \end{cases}$$

Entsprechend:

$$\mathcal{Z}_{n+1}: \begin{cases} \text{I}''.\ (n+1)+1 \text{ ein Element} \\ \text{II}''.\ \text{wenn } z, \text{ so auch } z+1 \end{cases}$$

[G] Baule has drawn a line in the statement of the proposition connecting the two "1"s.

$\mathcal{N}_n$ = Durchschnitt aller $\mathcal{Z}_n$

Jedes $\mathcal{Z}_n$ ist ein $\mathcal{Z}_{n+1}$; und wenn ein $\mathcal{Z}_{n+1}$ auch das Element $n+1$ enthält, so ist es auch ein $\mathcal{Z}_n$.

$$\mathcal{Z}_n = \{\mathcal{Z}_{n+1}, n+1\}$$

Um zu einer Definition von "größer als" zu kommen, beweisen wir zunächst den Satz: n is in $\mathcal{N}_n$ *nicht* enthalten.

| [9v]

1) Die Zahl 1 ist in $\mathcal{N}_1$ nicht enthalten.

Beweis: $\mathcal{N}'$ ist die Zahlenmenge, die aus $\mathcal{N}$ durch Entfernung der Zahl 1 entsteht. Es sei also $\mathcal{N} = \{\mathcal{N}', 1\}$

$\mathcal{N}'$ genügt den Bedingungen $\begin{cases} \text{I}' \\ \\ \text{II}' \end{cases}$ d.h. $\mathcal{N}'$ ist ein $\mathcal{Z}_1$

andererseits: $\mathcal{N}'$ ist in jedem $\mathcal{Z}_1$ enthalten; denn aus jedem $\mathcal{Z}_1$ entsteht durch Hinzufügung von 1 ein $\mathcal{Z}$. Also ist $\mathcal{N}'$ ein $\mathcal{N}_1$[.]

Daraus folgt: $\mathcal{N}' = \mathcal{N}_1$

Denn andernfalls gäbe es Elemente im $\mathcal{N}_1$, die nicht in allen $\mathcal{Z}_1$ (nämlich nicht in $\mathcal{N}'$) enthalten wären, was der Definition von $\mathcal{N}_1$ widerspricht.

2) Wenn n nicht in $\mathcal{N}_n$, so ist auch $n+1$ nicht in $\mathcal{N}_{n+1}$ enthalten.

Beweis: Entsprechend dem obigen $\mathcal{N}'$ bilden wir uns jetzt eine Zahlenmenge $\mathcal{N}'_n$, die aus der Zahlenmenge $\mathcal{N}_n$ durch Entfernung von $n+1$ |entsteht. [10r]

$$\mathcal{N}_n = \{\mathcal{N}'_n, n+1\}$$

$\mathcal{N}'_n$ ist ein $\mathcal{Z}_{n+1}$

andererseits ist $\mathcal{N}'_n$ in jedem $\mathcal{Z}_{n+1}$ enthalten (wie oben), d.h. $\mathcal{N}'_n$ ist ein $\mathcal{N}_{n+1}$.

folglich: $\mathcal{N}'_n = \mathcal{N}_{n+1}$ (wie oben)

d.h. $n+1$ ist in $\mathcal{N}_{n+1}$ nicht enthalten.

Somit folgt aus dem Induktionssatz allgemein:

n ist in $\mathcal{N}_n$ *nicht* enthalten.

Definition: Wenn m in $\mathcal{N}_n$ enthalten ist, so schreiben wir: $m > n$

Somit lautet der letzte Satz auch: n nicht $> n$. | [10v]

Auf Grund des Vorangehenden ist es nunmehr ein Leichtes, alle Sätze, die wir früher aus den 5 Axiomen ableiteten, zu beweisen.

So ergeben sich z.B. ohne Schwierigkeiten die Sätze:

Wenn $m > n$ und $p > m$, so $p > n$

---

180 5 Axiomen] The number of axioms seems to have been corrected (by Baule (?), in ink) from "4" (?) to "5".

Wenn $m > n$, so n nicht $> m$

Jede natürliche Zahl $n = m + 1$ außer $n = 1$

u.s.w.

Es sei noch bemerkt:

$\mathcal{N}$ und $\mathcal{N}_1$ haben die gleiche Mächtigkeit (ordne dem n in $\mathcal{N}$ $n + 1$ in $\mathcal{N}_1$ zu)

Ebenso $\mathcal{N}_n \sim \mathcal{N}_{n+1}$.

Es sing also alle Mengen $\mathcal{N}_n$ von gleicher Mächtigkeit und zwar haben sie alle laut Definition des unendlichen die Mächtigkeit "unendlich" | [11r]

Literatur:

1) Hilbert: Grundlagen der Geometrie 4. Aufl. 1913

2) Hessenberg: Grundbegriffe der Mengenlehre Abh. d. Fries'schen Schule 1906
3) Schröder: Algebra der Logik Bd. I 1890

Unmöglichkeitsbeweise.

1) Welche Aufgabe kann man *mit Zirkel und Lineal* allein lösen?

   Alle Aufgaben, die sich algebraisch durch die 5 Operationen: Addition, Subtraktion, Multiplikation, Division und Quadratwurzelziehen lösen kann[7], lassen sich geometrisch mit Leineal und Cirkel allein lösen und umgekehrt.

   [Diagram]

   Beispiele:

   a) lösbare Probleme

      1) Das reguläre 5 Eck

         Algebraisch besteht die Lösung des Problems in der Lösung der Gleichung 5. Grades $x^5 - 1 = 0$

         Diese lässt sich sofort auf Quadraturen zurückführen. $\frac{x^5-1}{x-1} \equiv x^4 + x^3 + x^2 + x + 1 \equiv y^2 + y - 1$; worin $y = x + \frac{1}{x}$ gesetzt ist)

         Ebenso ist das reguläre 17 Eck konstruierbar

         Allgemein ist jedes p-Eck konstruierbar, wenn sich das algebraische Problem auf quadratische Gle-

[7] The main verb of this sentence, "kann" is not cogruent with the subject "die". But that mistake seems not to have been noticed either by Baule or Hilbert. It would be best to read "lassen" instead.

199–200 und umgekehrt] The words "und umgekehrt" were added subsequently (by Hilbert) in pencil.

209–210 quadratische Gleichungen] Baule manuscript has "eine quadratische Gleichung" instead, but has been corrected, that is to say, the "eine" has been struck out and the plural ending "-en" has been added, in pencil.

ichungen zurückführen lässt. | [11v]

2) nicht lösbare Probleme:

e und π sind nicht durch Zirkel und Lineal allein darstellbar
ebenso nicht $\sqrt[3]{2}$ (Problem der Kubusverdoppelung)

b) Alle Aufgaben, die sich mit Zirkel und Lineal lösen lassen, lassen sich auch mit *dem Zirkel allein* lösen.

c) Welche Aufgaben lassen sich mit dem *Lineal allein* lösen?

Wenn man einen Kreis mit Mittelpunkt hat kann man alle mit Zirkel und Lineal lösbaren Aufgaben auch mit dem Lineal allein lösen. Wenn man aber vergessen hat, den Mittelpunkt zu markieren, so ist es mit Lineal allein nicht möglich, denselben zu finden. Hat man außer dem einen Kreis ohne Mittelpunkt noch einen weiteren, so ist es nicht unbedingt möglich, die Mittelpunkte der Kreise zu finden, sondern nur dann, |wenn die beiden Kreise einander schneiden. Hat man 3 Kreise, so kann man die Mittelpunkte mit [12r]
Lineal allein finden und alle Aufgaben 1) lösen, falls nur die 3 Kreise nicht einem Büschel angehören.
(Sie gehören einem Büschel an, wenn $a_1k_1 + a_2k_2 + a_3k_3 = 0$ ist, wo $\left.\begin{matrix} k_1 = 0 \\ k_2 = 0 \\ k_3 = 0 \end{matrix}\right\}$ die Kreisgleichungen sind und a_1, a_2, a_3 konstante sind)

Konstruktion des Mittelpunktes zweier sich schneidender Kreise mit dem Lineal allein:[8]

[Diagram]

Auf einem der beiden Kreise nehme ich zwei beliebige Punkte A und B an und lege durch sie und die Schnittpunkte beider Kreise je zwei Strahlen, die mir, wie die nebenstehende Figur zeigt, zwei parallele Sehnen im anderen Kreise liefern.

[(1)||(2); denn $\measuredangle 1 = \measuredangle 2 = \measuredangle 3 = \measuredangle 4 = \measuredangle 5 = \measuredangle 6$]

Wenn man zwei parallele Sehnen hat so kann man wie nebenstehende Figur lehrt, sofort ihre Mittelsenkrechte mit dem Lineal allein finden. Indem ich ein zweites Punktepaar |annehme und genau so [12v]
verfahre, finde ich einen zweiten geometrischen Ort für den Mittelpunkt des Kreises.

Wenn die beiden Kreise einander nicht schneiden, ist es auf keine Weise möglich, mit dem Lineal allein die Mittelpunkte zu finden.

Der *Unmöglichkeitsbeweis* hierfür sei hier als Charakteristikum für Unmöglichkeitsbeweise angegeben:

[8] Next to this paragraph Hilbert has made a "T"-shaped mark in blue crayon. Marks of this shape are occasionally used by Hilbert to demarcate portions of text means to be inserted elsewhere. At the top of this manuscript page also in blue crayon "1." has been written, and at the top of the following page in blue crayon "2" and "S. 3.-4" as well as (in pencil) "weiter separat". Whether Hilbert meant to insert something here from elsewhere, or insert this elsewhere, remains, unfortunately, unclear.

220 kann man die Mittelpunkte] Between "man" and "die" the word "unbedin[gt]" has been crossed out (in ink, so presumably by Baule). The reason for this delition may be that, later in the sentence, a condition is introduced (with "falls").

Ich kann eine Transformation angeben, die die Eigenschaft hat, alle Punkte einer Gerade wieder in Punkte einer Gerade überzuführen und die Punkte zweier Kreise in Punkte eben derselbnen Kreise. Angenommen nun, es gäbe eine Konstruktion für die Mittelpunkte der Kreise, so müssten die Mittelpunkte für die Transformation invariant sein; denn die transformierte Konstruktion hätte ja dieselbe Berechtigung wie die ursprüngliche. Nun sind aber in Wirklichkeit die Mittelpunkte |der Kreise gegenüber jener Transformation nicht invariant. [13r] Also ist unsere Annahme, daß esd eine Konstruktion für die Mittelpunkte der Kreise mit Lineal allein gibt, falsch.

Die Gleichungen der gegebenen Kreise seien:

$$\left.\begin{array}{ll}(x-a)^2+y^2+1-a^2=0 & a>1\\ (x-b)^2+y^2+1-b^2=0 & b>1\end{array}\right\} \; a\neq b$$

Mittelpunkte: $(a,0)$ und $(b,0)$

Die Transformation, die die oben dargelegte Eigenschaft hat, ist:

$$x=\frac{1}{x'};\; y=\frac{y'}{x'}$$

|Mit dem *Lineal allein* lassen sich nur die Aufgaben lösen, die die *projektive Geometrie* behandelt. [13v]

Die Parallelität und Orthogonalität ist in der Projektiven Geometrie keine Rede; es ist mit dem Lineal allein nicht möglich, Parallele zu ziehen und Rechte Winkel anzutragen.

Was ist nun in der Projektiven Geometrie möglich?

Da, wie gesagt, von Parallelität keine Rede sein kann, darf auch der unendlich ferne Punkte einer Geraden nicht ausgezeichnet sein. Wir legen demgemäß in der Projektiven Geometrie *homogene* Dreieckskoordinaten zu Grunde, d.h. es wird jeder Punkt der Ebene nicht durch ein Werte-*paar* (x,y) sondern durch ein Werte-tripel $(x:y:z)$ charakterisiert.

Dadurch fällt die Auszeichnung der unendlich fernen Geraden fort. Wie die X Achse die Gleichung $y=0$ und die Y-Achse die Gleichung $x=0$ hat, so lautet die Gleichung der unendlich fernen Geraden im homogenen Koordinatensystem $z=0$.

[Diagram]

Indem ich 3 Gerade als Achsen und außerdem einen Einheitspunkt annehme, ordne ich jedem Punkt der Ebene ein Wertetripel $x:y:z$ zu. Die Gleichungen der von den Dreieckseckpunkten ausgehenden Strahlen kann ich skalenmäßig auf den gegenüberliegenden Dreiecksseiten durch Vermittlung des Einheitspunktes festlegen. Den Punkt -1 finde ich auf jeder Seite als 4. harmonischen Punkt zu $\infty, 1, 0$ (-1); denn den Punkt $\frac{1}{2}$ [Diagram]; 2,3 u.s.w.

So kann ich die Einteilung beliebig weit treiben.

Wie aus der obigen Figur 2 ersichtlich, lassen sich außer der Addition und Subtraktion auch die Multiplikation und Division |sofort ausführen; und zwar mit dem Lineal allein. [14r]

$x:y:z$ sind Punktkoordinaten

$ux + vy + wz = 0$ stellt bei festgehaltenem u, v, w die Gesamtheit der Punkte auf einer Geraden dar, bei festgehaltenem x, y, z die Gesamtheit der Geraden durch einen Punkt, eben den Punkt mit den Koordinaten x, y, z, dar. Entsprechend (x, y, z) heißen u, v, w die "Koordinaten der Geraden" und es heißt: $ua + vb + wc = 0$ die "Gleichung des Punktes (a, b, c)" in "Linienkoordinaten"

Es soll nunmehr gezeigt werden, daß der *Mittelpunkt dreier gegebener Kreise, die einander nicht schneiden, mit dem Lineal allein konstruierbar* ist.

Wir legen homogene Dreieckskoordinaten zugrunde, indem wir irgend ein Dreieck in der Ebene und einen Einheitspunkt in ihm als gegeben ansehen. | [14v]

Die Gleichungen der Kreise seien:

$$K_1(xyz) = 0;$$
$$K_2(xyz) = 0;$$
$$K_3(xyz) = 0;$$

Aus den Gleichungen

$$\left.\begin{aligned} K_1(xyz) &= 0; \\ K_2(xyz) &= 0; \end{aligned}\right\} \text{ und } ux + vy + wz = 0;$$

eliminiere ich $\frac{x}{z}$ und $\frac{y}{z}$ und erhalte in $\phi(u, v, w) = 0$; (Gleichung 4. Grades) eine Gleichung für die 4 Schnittpunkte der Kreise K_1 und K_2 $\phi(u, v, w)$ zerfällt in 4 lineare Faktoren.

Entsprechend erhalte ich aus:

$$\left.\begin{aligned} K_1 &= 0 \\ K_3 &= 0 \end{aligned}\right\} \text{ und } ux + vy + wz = 0$$

$\psi(u, v, w) = 0$; wodurch die Schnittpunkte der Kreise K_2 und K_3 in Linienkoordinaten dargestellt werden. | [15r]

ϕ und ψ haben zwei gemeinsame lineare Faktoren; denn alle Kreise haben zwei Schnittpunkte, die sogenannten imaginären Kreispunkte miteinander gemein. Da nach Voraussetzung die 3 Kreise im reellen einander nicht schneiden sollen, so haben ϕ und ψ gerade einen Teiler zweiten Grades $\gamma(u, v, w)$ gemein. Dieser Teile γ stellt gleichzeitig also die beiden imaginären Kreispunkte in Linienkoordinaten dar.

γ kann ich algebraisch sofort finden und mit dem Lineal allein geometrisch konstruieren.

$\gamma + \kappa l^2 = 0$; wo l = lineare Funktion(u, v, w) ist

ist ein Kegelschnitt; ich behaupte ein Kreis, wie ich auch l wählen mag.

Denn: es sei $\alpha u + \beta v + \gamma w = 0$ die Gleichung eines der imaginären Kreispunkte (α, β, γ); also $\gamma = (\alpha\mu + \beta v + \gamma w) \cdot m$. Ich ziehe nun eine Gerade durch (α, β, γ), die den Kegel|schnitt: $\gamma + \kappa^\lambda l^2 = v$ berührt; d.h. ich bestimme u, v, w so, daß erstens $\alpha u + \beta v + \gamma w = 0$ und zweitens $\gamma + kl^2 = 0$ d.h. $l = 0$ wird. [15v]

Ich habe also zwei lineare homogene Gleichungen für $u : v : w$; d.h. es gibt nur *eine* solche Gerade, nur *eine* Tangente durch (α, β, γ) d.h. der imaginäre Kreispunkt (α, β, γ) liegt auf dem Kegelschnitt, der Kegelschnitt ist also ein Kreis. Daraus folgt weiter, da die Tangente in den unendlich fernen, imaginären Kreispunkten durch den Mittelpunkt des Kreises gehen: $l = 0$ ist die Gleichung des Mittelpunktes. Es lässt sich somit der Mittelpunkt mit dem Lineal allein konstruieren.

Grundzüge der neuen Mechanik.

Wir hatten früher gesehen, die alte Auffassung, in der die Zeit als unabhängig von den Raumkoordinaten angesehen wird und dadurch eine Sonderstellung einnimmt, läßt sich nicht aufrechterhalten. Wir kamen zu den Lorentztransformationen:

$$\left.\begin{array}{ll} \xi = \frac{1}{\sqrt{1-v^2}}(x - vt); & x = \frac{1}{\sqrt{}}(\xi + v\tau); \\ \tau = \frac{1}{\sqrt{1-v^2}}(-vc + t); & t = \frac{1}{\sqrt{}}(v\xi + \tau); \end{array}\right\} x^2 - t^2 = \text{Invariante}$$

wo x, t die Koordinaten in ruhendem, ξ, τ die des mitbewegten System sind. τ nannten wir die "Eigenzeit".

Kinematik:

Während in der alten Auffassung[9] die Bewegung eines Punktes in der X Achse gegeben wurde durch $x = x(t)$, haben wir in der neuen Auffassung entsprechend der Gleichstellung von x und t eine Parameterdarstellung:

$$\left.\begin{array}{l} x = x(\tau) \\ t = t(\tau) \end{array}\right\} \text{ indem der Parameter } \tau \text{ definiert ist durch:}$$

$$\left(\frac{dx}{d\tau}\right)^2 - \left(\frac{dt}{d\tau}\right)^2 = -1$$

|Wir stellt sich nun die *Bewegung eines starren Körpers* in der X-Richtung dar? [16r]

In alter Auffassung: [Diagram]

$$x = x_0(t) + \xi$$

Die Bewegung des Punktes x_0 ist gegeben.

In der neuen Mechanik lauten die Bewegungsgleichungen, behaupte ich,

$$\left.\begin{array}{l} x = x_0 + t_0' \cdot \xi \\ t = t_0 + x_0'\xi \end{array}\right\}$$

[9] Next to this paragraph in the margin "*Kinematik*" has been written. This may indicate that the sub-section heading has been added at a later time.

316 Lorentztransformationen] The word "Lorentztransformationen" has been written (in ink, in Baule's hand) *over* the (somehow erased) word "Maxwellgleichungen".

323 ruhendem] The word "ruhenden" has been written (in ink, in Baule's hand) over the (somehow erased) word "bewegenden".

Beweis:

$$\frac{dx}{dt} = \frac{\frac{dx}{d\tau}}{\frac{dt}{d\tau}} = \frac{x_0' + t_0''\xi}{t_0' + x_0''\xi} = \frac{x_0' + \frac{x_0'}{t_0'}x_0''\xi}{t_0' + x_0''\xi} \quad \text{(nach der Definitionsgleichung von } \tau$$
$$= \frac{x_0'(1+\frac{x_0''}{t_0'}\xi)}{t_0'(1+\frac{x_0''}{t_0'}\xi)} = \frac{x_0'}{t_0'} = \frac{dx_0}{dt_0}$$

Wenn sich der Punkt 0 mit konstanter Geschwindigkeit bewegt, so ist nach den Lorentzgleichungen:

$$x_0 = \frac{v\tau}{\sqrt{1-v^2}};\ t_0 = \frac{1}{\sqrt{1-v^2}}\tau$$

Somit ergibt sich für die |gleichförmige Bewegung des starren Körpers: [16v]

$$x = \frac{v\tau}{\sqrt{1-v^2}} + \frac{1}{\sqrt{1-v^2}} \cdot \xi = \frac{1}{\sqrt{1-v^2}} \cdot (v\tau + \xi)$$
$$t = \frac{\tau}{\sqrt{}} + \frac{v}{\sqrt{}}\xi = \frac{1}{\sqrt{1-v^2}}(\tau + v\xi)$$

Transformation auf Ruhe:

In welchenm Bezugssystem ruht der Körper?

$$X = \frac{1}{\sqrt{1-v^2}}(x_0 + t_0'\xi) - v \cdot (t_0 + x_0'\xi);$$
$$T = \frac{1}{\sqrt{1-v^2}}(-v(x_0 + t_0'\xi) + t_0 + x_0'\xi);$$

Bedingung für Ruhe ist: $\begin{cases} X' = 0 \\ T' = 1 \end{cases}$

$$X' = \frac{1}{\sqrt{1-v^2}}(x_0' + t_0''\xi - vt_0' - vx_0''\xi) = 0$$
$$T' = \frac{1}{\sqrt{1-v^2}}(-vx_0' - vt_0''\xi + t_0' + x_0''\xi) = 1$$

Daraus folgt: *Ruhe herrscht* in dem System, das sich mit einer Geschwindigkeit $v = \frac{x_0'}{t_0'}$ bewegt, d.h. im *mitbewegten* System. | [17r]

Dynamik:

Die Kraft wir als Wektor definiert durch:

$$\begin{array}{l|l} mx'' = k_x & x' \\ mt'' = k_t & t' \end{array} \quad \text{Betrag } K = \sqrt{k_x^2 - k_t^2}$$

$$\frac{d}{d\tau}\left(\frac{m}{2}(x'^2 - t'^2)\right\} = x'k_x - t'k_t = v_x k_x - v_t \cdot k_t = 0; \tag{B.1}$$

$\int k_x dx = \int k_t dt =$"Arbeit" Einfachstes Beispiel: Die Newton'sche Gravitationskraft, die nach der alten Auffassung gleich $\frac{1}{r^2}$ ist, muß neu definiert werden, da r ja gar nicht mehr invariant ist. Die Anziehungskraft wirke mit der Geschwindigkeit 1.

$\frac{1}{(\overline{x}_2 - x_1)^2}$ ist die Kraft von 2 auf 1. Hier bedeutet $\overline{x}_2 = x_2(\bar{t})$ denjenigen Ort von 2, dessen Wirkung in x_1 zur

371 Lorentzgleichungen] The manuscript initially had "Maxwellgleichungen", but "Maxwell" has subsequently been struck out (in ink) and "Lorentz" has been written (in Baule's hand) above it as a replacement.

Zeit t_1 ankommt.

$$-|x_2(\bar{t}) - x_1(t)| = \bar{t} - t \text{ (also } \bar{t} < t)$$

|In neuer Auffassung stellt sich nun die Newton'sche Kraft folgendermassen dar: [17v]

$$\left.\begin{aligned} k_x &= v_{t1} \cdot K_{21} \\ k_t &= v_{x1} \cdot K_{21} \end{aligned}\right\} \text{durch diese Darstellung wird Relation (1) erfüllt und außerdem: } K_{21} = \sqrt{k_x^2 - k_t^2}$$

Wenn: $K_{21} = \frac{1}{\left\{(\overline{x}_2 - x_1)\overline{v}_{x2} - (\bar{t} - t)\cdot\overline{v}_{t2}\right\}^2}$ ist.

$= \frac{1}{(\overline{x}_2 - x_1)^2} \cdot \frac{1-\overline{v}_2}{1+\overline{v}_2}$

(überstreichen bedeutet: zur Zeit, da der Punkt wirkte)

$(v = \frac{dx}{dt} = \frac{v_x}{v_t})$

K_{21} wird $= \frac{1}{(\overline{x}_2 - x_1)^2}$, wenn 2 zur Zeit $\bar{t}$ ruhte; und es ergibt sich die Gravitationskraft in alter Auffassung, wenn 1 und 2 zu allen Zeiten ruhen. | [18r]

Dann lauten die Bewegungsgleichungen von 2, wie wir oben gefunden hatten:

$$\left.\begin{aligned} x_2(\tau) &= x_1(\tau) + t_1'(\tau)\cdot\alpha \\ t_2(\tau) &= t_1(\tau) + x_1'(\tau)\cdot\alpha \end{aligned}\right\} \text{ wo } \alpha \text{ die Ruhelänge } \overline{12} \text{ bedeutet.}$$

Es war[A]: $K_{21} = \frac{1}{\{(x_2(\overline{\tau}) - x_1(\tau))v_{x2}(\overline{\tau}) - (t_2(\overline{\tau}) - t_1(\tau))\cdot v_{t2}(\overline{\tau})\}^2}$

$-|x_2(\overline{\tau}) - x_1(\tau)| = t_2(\overline{\tau}) - t_1(\tau)$

Wenn man nach α entwickelt bekommt man:

$$\overline{\tau} - \tau = -\alpha + \frac{1}{2}b\cdot\alpha^2 + \dots \text{ wo } b: \left\{\begin{aligned} x_1'' &= bt_1' \\ t_1'' &= bx_1' \end{aligned}\right. \tag{B.2}$$

(2) eingesetzt in K_{21} liefert:

$$K_{21} = \frac{1}{\alpha^2}\{1 + b\alpha + \dots\}$$

Andererseits ergibt sich.

$$K_{12} = \frac{1}{\alpha^2}\{1 - 3ba + \dots\}$$

Somit: $K_{21} - K_{12} = \frac{4b}{\alpha} + \dots;$

$$\begin{aligned} k_x &= t_1'\cdot\left(\tfrac{4b}{\alpha} + \dots\right) = \tfrac{4}{\alpha}\cdot x_1'' + \dots \\ k_t &= x_1'\left(\tfrac{4b}{\alpha} + \dots\right) = \tfrac{4}{\alpha}t_1'' + \dots \end{aligned}$$

$\frac{4}{\alpha}$ ="träge Masse" |[B] [18v]

$x_2(\tau), t_2(\tau)$ Parameterdarstellung für die Bewegung des Punktes 2, wo τ nicht Eigenzeit ist.

[A] Partly in the margin next to the following lines, partly between the lines, the following equations have been inserted in pencil (so presumably by Hilbert): "$x_2(\overline{\tau}) - t_1(\tau) = t_1(\tau) - t_2(\overline{\tau})$", "$\overline{\tau} = \tau + a\alpha + b\alpha^2 + \dots$", "$x_2'(\tau) + x_2'(\tau)\cdot(a\alpha + b^2\alpha^2) + x_2''(\tau)\frac{(a\alpha)^2}{2}$", "$t_1'(\tau)\cdot\alpha + \left(x_1'(\tau) + \alpha t_1''(\tau)\right)(a\alpha + b\alpha^2) + x_1''(\tau)\frac{(a\alpha)^2}{2} = t_1 + x_1'\alpha$".

[B] The following page is a sheet glued into the notebook over something else. It is written in Hilbert's hand and filled almost entirely with calculations. The page it is glues onto, which is still readable against backlight, was also filled entirely with calculations.

1.) Kraft von 1 auf 2: K_{12} $x_2(\tau) - x_1(\overline{\tau}) = t_2(\tau) - t_1(\overline{\tau})$
2.) Kraft von 2 auf 1: K_{21} $x_2(\overline{\tau}) - x_1(\tau) = t_1(\tau) - t_2(\overline{\tau})$

375 1.)

$$x_1 + t_1'\alpha = t_1 + x_1'\alpha - \bar{t}_1$$
$$x_1 - t_1 = u$$

$$u(\tau) - \alpha u'(\tau) = u(\overline{\tau})$$

B.1.1 Baule's Second Version of the Axioms of Whole Numbers

Axiome der ganzen Zahlen

[A] - 1 - [19r]

I. Axiom vom Process 1 ist eine Zahl.

Wenn n eine Zahl ist, so ist auch $n+1$ eine Zahl

5 II. $n + 1 > n$

III. Wenn $b > a$ und $c > b$, so ist $c > a$

IV. Wenn $b > a$, so $b \neq a$

V. Wenn eine Aussage X_n richtig ist für $n = 1$, und wenn sie richtig ist für $n + 1$, falls sie für n richtig ist, so ist sie für alle g. Zahlen richtig.[B]

10 [C] | - 2 - [19v]

Unabhängigkeit der Axiome.

[A] At the top of the page, Hilbert has added (in blue pencil) "Zu meinem Kollegheft: Logische Prinzipien 1905.", underneath the words "(Im Nachlaß nicht vorh.)" have been added in pencil, presumably by an archivist.

[B] Hilbert has added (in ink) under the induction axiom: "'Aussage' ersetze besser durch 'Funktion', so dass V lautet: Wenn $f(1) = 1$ und $f(n+1) = 1$ aus $f(n) = 1$ folgt, so ist $f(n) = 1$ für jede ganze Zahl n." To this remark he has furthermore added a footnote, which reads: "Zusammenhang mit Dedekind, der den Mengenbegriff benutzt. Tun wir das, so müssen wir sagen: Menge ist ein Ding, durch dessen Zusammensetzung mit irgend anderen Dingen die Elemente der Menge zu Stande kommen, wie ich in meinem Heidelberger Vortrage ausgeführt habe. Und nun an Stelle von Axiome V: Zahl ist eine Menge, in der jede Teilmenge ein kleinstes u. ein größtes Element hat. Dann kann man, wie Dedekind, das Induktionsaxiom V beweisen; aber dieser Weg ist umständlicher.".

[C] Below the axioms, Hilbert has added (in pencil) "Satz endlich = nicht unendlich" and Baule has written (in ink) "*Unabhängigkeit*".

3 Axiom vom Process] These words have been added in pencil above the line with an insertion mark.

Die Unabhängigkeit eines Axioms von den übrigen zeigt man indem man ein System von Dingen angibt, daß *alle* Axiome *außer* jenem einen befriedigt.

Die *Axiome I* sind offenbar notwendig

Axiom II ist von den übrigen unabhängig, denn das System: $1,\ 1+1=1$ genügt allen außer II.

Axiome III ist unabhängig, denn das System:

$1;\ 1+1=2;\ 2+1=1$

$1>2;\ 2>1$

genügt allen Axiomen außer III

Axiom IV: unabhängig, denn

$1+1=1; 1>1$

befriedigt nur IV nicht. | - 3 - [20r]

Axiom V schließlich ist unabhängig, weil das System:

$$\left.\begin{array}{l} 1,2,3,\ldots \\ 1',2',3',\ldots \end{array}\right\} \begin{array}{l} n(+1)=n+1 \\ n'+1=(n+1)' \end{array}$$

allen Axiomen außer V genügt.

Es soll nun gezeigt werden, dass aus dem obigen System von 5 Axiomen alle bekannten Sätze hergeleitet werden können.

Satz 1. Wenn $b>a$, so $a<b$.

(Definition: $a<b$ heißt: $a\neq b$ und a nicht $>b$)

Beweis:

1) $a\neq b$ nach Axiom IV

2) a nicht $>b$, dann wäre $a>b$, so wäre nach Axiom III und der Voraussetzung $a>a$ und das widerspricht d. Axiom IV. | - 4 - [20v]

Satz 2: *Wenn $a\neq 1$, so ist $a=b+q$*

Beweis:

1) Der Satz ist richtig für $a=1$, denn er sagt nichts aus

2) Wenn er für a richtig ist, so ist er auch für $a+1$ richtig. Für $a+1$ ist er per se richtig.

Also ist er nach Axiom V für alle Zahlen richtig.

Definition der Addition:

---

28 *Satz 1.*] The number "1" has been struck out and replaced above by "2".

34 *Satz 2*:] The number "2" has been struck out and replaced above by "1".

39 *Definition der Addition*] Underneath the word "Definition" the word "Erklärung" has been written in pencil by Hilbert.

39–41

$$a+(b+1)=(a+b)+1$$

$a + (b + 1) = (a + b) + 1$

Dadurch ist nach Axiom V $a + b$ vollständig definiert.

Satz 3. $(a + b) + c = a + (b + c)$

Beweis:

1) Der Satz ist richtig für $c = 1$ nach Definition der Addition.

2) Wenn er für c richtig ist, so ist er auch für $c + 1$ richtig. Denn:

$(a + b) + (c + 1) = ((a + b) + c) + 1$ nach Definition

$= (a + (b + c)) + 1$ nach Voraussetzung

$= a + ((b + c) + 1)$ nach Definition

$= a + (b + (c + 1))$ nach ”

Also ist der Satz nach Axiom V allgemein gültig | - 5 - [21r]

Satz 4: $a + b = b + a$

Beweis:

1) Der Satz ist richtig für $b = 1$, denn es gilt $a + 1 = 1 + a$

α) für $a = 1$ und wenn es
β) für a gilt, so gilt es auch für $a + 1$, denn nach Definition is $1 + (a + 1) = (1 + a) + 1) = (a + 1) + 1$

2) wenn der Satz für b richtig ist, so ist er auch für $b + 1$ richtig; denn es ist nach Definition

$$\begin{aligned} a + (b + 1) &= (a + b) + 1 = (b + a) + 1 && \text{nach Voraussetzung} \\ &= b + (a + 1) && \text{nach Definition} \\ &= b + (1 + a) && \text{nach 1)} \\ &= (b + 1) + a && \text{nach Satz 3} \end{aligned}$$

Satz 5. Wenn $a < b$, *so* $b = a + c$

Beweis:

1) Der Satz ist richtig für $a = 1$ nach Satz 1 und Satz 4

2) Wenn er für a richtig, so auch für $a + 1$, denn wenn $a + 1 < b$, so auch $a < b$ (denn wäre a nicht $< b$, so wäre es $> b$ oder $= b$ und aus $a > b$ folgt nach Axiom II und III $a + 1 > b$ und ebenfalls aus Axiom II folgt $a + 1 > b$, wenn $a > b$ Beides widerspricht aber der Voraussetzung.) | - 6 - [21v]

Nun ist, da $a < b$ ist, nach Voraussetzung $b = a + c$; $c \neq 1$ denn $b \neq a + 1$ sondern $a + 1 < b$ (Vorauss.) also $c = d + 1$ nach Satz 1; somit $b = a + (d + 1) = (a + 1) + d$ nach Satz 3 und 4

Also ist nach Axiom V der Satz allgemein richtig.

] In the margin next to the equation Hilbert has added (in pencil) “$a + 1$ ist nach Axiom 1 definit” and underneath “z.B. $b = 1$”.

60 Satz 1] The manuscript originally had “Satz 2”, but “2” has been struck out (in pencil) and replaced above by “1”.

65 Satz 1] The manuscript originally had “Satz 2”, but the “2” has been struck out (in pencil) and replaced to the right with “1”.

66 Axiom V] The manuscript originally had “Axiom 5”, but the numeral “5” has been struck out (in pencil) and replaced to the left

Satz 6. Wenn $b > a$, so $b = a + c$

Beweis. Wenn $b > a$ so ist nach Satz 2 $a < b$ und somit nach Satz 5 $b = a + c$

Satz 7 Wenn $b = a + c$, so $b > a$

Beweis:

1) Der Satz ist richtig für $c = 1$ nach Axiom II

2) wenn er für c richtig ist, so auch für $c + 1$

Denn: $b = a + c + 1 \quad > \quad a + c$ nach Axiom II

$a + c \quad > \quad a$ weil der Satz für c gilt

Also ist nach Axiom III auch $b > a$ und der Satz nach Axiom V allgemein richtig.[A]

Satz 8. Wenn $a < b$, so $b > a$

Denn nach Satz 5 ist $b = a + c$ d.h. nach Satz 7 $b > a$ | - 7 - [22r]

Satz 9. Wenn $a > b$, so $a + c > b + c$

Wenn $a > b$, so $a + c < b + c$

Beweis: Wenn $a > b$, so $a = b + d$; $a + c = b + c + d > b + c$

Wenn $a < b$, so $b = a + d$; $b + c = a + c + d > a + c$

Satz 10. Wenn $a + c = b + c$, so $a = b$

Beweis: Wäre $a \neq b$, so wäre es $> b$ oder $< b$

wenn $a > b$, so $a = b + d$ und $a + c = b + c + d > b + c$

wenn $a < b$, so $b = a + d$; und $b + c = a + c + d > a + c$

Beides widerspricht der Voraussetzung; also $a = b$.

Satz 11. Wenn $a \neq 1$, so $a > 1$

nach Satz 1 und Satz 7[B]

Satz 12. Wenn $a > b$ und $a \neq b + 1$, so $a > b + 1$[C]

[A] After the end of the proof, Hilbert has added the following remark (in pencil): "Speziell $a + 1 > 1$ also $a + 1 \neq 1$ d.h. *1 ist die erste Zahl*!".

[B] After the proof Hilbert has added the following remark (in pencil): "d.h. 1 ist die kleinste Zahl.".

[C] Below the statement of the proposition, Hilbert has added (in pencil): "also es giebt keine Zahl zwischen [struck out: 2 folgenden] d.h. with the Roman numeral "V".

68 Satz 2] The manuscript originally had "Satz 1", but the "1" has been struck out (in pencil) and replaced above with "2".

71 $c = 1$] The formula was originally "$a = 1$", but the "a" has been struck out (in ink) and replaced above (in Baule's handwriting) by "c".

71 Axiom II] The manuscript originally had "Axiom III", but the "III" has been struck out (in ink) and replaced to the right (in Baule's handwriting) with "II".

74 nach Axiom III auch $b > a$ und] These words have been added (by Baule) in the left margin with an insertion mark. Above the insertion mark, the word "auch" has been written, but struck out.

87 Satz 1] The manuscript originally had "Satz 2", but "2" has been struck out (in pencil) and replaced above by "1".

Beweis: Wenn $a > b$, so $a = b + c$;

90 $c \neq 1$ nach Voraussetzung also $c = c' + 1$ (Satz 2), also $a = b + c' + 1 = b + 1 + c' > b + 1$

q.e.d[D] | - 8 - [22v]

A Conjectural Reconstruction of Intermediary Revision-Stages

The portion of manuscript 559 containing this exposition of arithmetic shows substantial revision in pencil presumably by Hilbert (who habitually revised lecture-notes produced by his assistants in pencil). The changes are of three kinds: (A) rearrangements, i.e. the number of a proposition is struck out and replaced by another indicating a change in the sequence of theorems; (B) revisions, i.e. the formulation or the proof of a proposition is altered in detail; and (C) (re-)placements, i.e. a proposition or a proof is struck out entirely and replaced either explicitly or implicitly (by naming the propositions to be used in the replacement proof, or indicating the number of a proposition to be imported from some other source, which is not, as far as I can see, available to us). In several cases, different kinds of changes overlap, e.g. certain proofs and propositions were first revised only to subsequently be striken entirely; and all propositions had their place in the sequence changed at least once, some as often as 5 times (proposition 11 was first moved to 12, then presumably 13, then 10, then 13 again; propositions 8 and 3 had their place in the sequence changed 4 times).

Most heavily revised were the proofs of propositions 3 and 4, and propositions 7 and 8).

There is an obvious reason for the revision of the proof of propositions 3 and 4, namely, the proofs initially given are, as I stressed above, invalid. Both depend (3 explicitly and 4 implicitly, by way of its dependence on proposition 3) on the associative law of addition, which, however, has not been established when it is relied on in these proofs. Hilbert seems to have recognised this problem and the revisions to these propositions address this defect by inserting the associative law of addition in the sequence of theorems before proposition 3 and reworking the proofs in light of it.

Still, charting the order of revisions is difficult.

Some observations:

- Proposition 1 seems to have revised two or four times:
  - It seems to have been moved to position 11.
  - Its proof has been corrected to cite what at that revision stage was proposition 1, and to cite axiom III.

---

die grösser als a und kleiner als $a + 1$ ist".

[D] Below the proof the the following formulas have been written in pencil: "$a = b + c$" and below this: "1.) $c = 1$" and "2.) c"; then a little to the right of "$a = b + c$" the formula "$a = b + c$" has been written again and below it "$c = 1 + d$". In the bottom right corner of the page Baule has written "verte.".

  - (Subsequently) its proof has been crossed out and replaced with the (subsequently once revised) indication "from propositions 2 and [crossed out 9] 7". Considering which two propositions could be so used, this revision seems to have been added at a late revision stage, since it most likely cited the propositions assigned position 2 in the final arrangement, and the proposition assigned to position 9 and subsequently to 7 (and subsequently to 2, though it appears as proposition 7 in the revised presentation), which are (respectively) propositions 5 and 8 in the original arrangement.

- Proposition 2 has been revised probably only once, to move it to position 1, and to clarify the proof a little here and there. (In particular, the revised proof is direct, rather than indirect.)

- Proposition 3 has been revised at least three times.

  - Its position in the sequence has been changed from 3 to 6 and subsequently to 10, although this last change seems to have remained idle: It appears in the revised presentation, as well as in the 1914-15 lectures, as proposition 4.

  - Its proof has been revised once in a minor way, which cites the propositions numbered "2a" at that revision stage, later changed to 3, likely the associative law.

  - Subsequently the proof has been stricken out altogether and replaced by a new proof citing only the proposition numbered 3 at that stage in the revision, presumably the associate law.

- Proposition 4 seems to have been revised only once, with the effect of deleting it. Indeed, it does not appear in the revised presentation of the axiom system.

- The position of presentation 5 has been revised twice, once in favour of 3 and then in favour of 2, where it indeed appears in the revised presentation. The proof has not been modified.

- Proposition 6 has been revised at least twice.

  - Its position has been changed first to 7 and then to 5, and even though a note above says "leave out!", it does appear in position 5 in the revised presentation.

  - The proof has been modified in part to take account of the changing numbering of the proposition referred to (the original proposition 5), which is revised to 2 (which suggests that by the time Hilbert revised this proof, proposition 5 has been moved to position 2), and to add a further reference to the proposition numbered first 5, and later 4, which (for substantive reasons) likely is the original proposition 3; and in part to sort out the mistake I flagged above, which is rectified by citing the original proposition 5, which has at that point been moved to position 2 (the "2" there seems to be written over another number, perhaps "6", but proposition 5 was never explicitly moved to position 6...).

- Proposition 7 has been revised at least three times:
  - Its position in the sequence has been changed thrice, first to 8, then to 6 and finally to 11; again, the last change seems to have remained idle: In the revised presentation it appears as proposition 6.
  - The formulation of the proposition has been changed (in a minor way: the variables have been interchanged, i.e. what was originally "a" has been revised to "b" and conversely).
  - The references to propositions in the proof have been adjusted to reflect revised numbering.
  - Finally, the proof has been crossed out altogether and replaced by a sketch appealing to the proposition numbered 4 at that revision stage.

On the basis of these notes, it seems likely to me that the rearrangements were carried out in four stages:

I First of all, Hilbert seems to have replaced the original proposition 4 with the general formulation of the associative law of addition, as the corrections of the formulation of proposition 4 show.

At that stage, Hilbert seems to have changed little else, since the proposition inserted is initially numbered "4" and the first change of position for proposition 3 is to position 6, but Hilbert seems to have only later recognised that the associative law must be established before the original proposition 3 – as I take the explicit notes to that effect to attest –, so the first change of position for proposition 3 must have come after the replacement of the original proposition 4.

Consequently no changes can be assumed to have been made to the numbering of propositions 1 and 2, since their position has been changed only once, to move 1 to 11 and 2 to 1, which would have resulted in moving 3 to 2, but that seems never to have happened.

Hilbert may have added the new proposition ultimately numbered 9 at this stage as proposition 12.

II Next, Hilbert seems to have noticed that the associative law must precede the commutative law, given the proof he gave for it. Hilbert seems to first intend to rectify the situation by moving the original proposition 3 to position 6 and subdividing revised proposition 4 into two propositions referred to as "2a" and "2b" of some other set of notes, but never exhibited; one may conjecture (based on the reference to 2a in the revision of the proof of the original proposition 3) that 2a was the original proposition 4 and 2b the general formulation. To make space for the second of these, Hilbert seems to have moved the original proposition 5 to position 3.

Moving these two propositions "ahead of" proposition 3 caused a «build up» of propositions there which presumably caused Hilbert to move the original proposition 3 to position 6. This causes a cascade of movements: Moving the original proposition 3 to position 6 has the effect of «bumping back» everything, starting with the original proposition 6, by one place. In the course of making those changes, Hilbert seems to have noticed, as a marginal comment attests, that the original proposition 1 can be deduced from other theorems.

He thus moved the original proposition 1 to position 11 (moving the original proposition 11 to position 12, the newly added proposition 12 to position 13), thus «making space» for the associative law as proposition 2 (or 2a and 2b).

It may also have been at this point that Hilbert added the new propositions 14 at the end.

At this stage the proof for the original proposition 1 is revised to rely on propositions 3 (originally 5) and 9 (originally 8).

At this point the proof for the original proposition 3 seems to have been revised.

III Next, Hilbert seems to have moved the original proposition 5 to position 2, the associative law (in its general formulation) to position 3 (the special case, the original proposition 4, presumably intermittently numbered "2a" seems to now be dropped altogether) and the commutative law to position 4.

The propositions originally numbered 6 through 9 are then all «moved up» one position.

The new proposition 12, subsequently 13, is moved to position 9, which is now empty, and the original proposition 11 to position 10. The original proposition 1 remains in position 11, and the original proposition 10 in position 12, which is indeed where these propositions appear in Baule revised presentation.

At this stage the proof for the original proposition 6 is revised.

At this stage the remark "Satz 1 folgt aus Satz 3 und 9" is added in the margin next to the original proposition 8, which is proposition 9 at this stage; the same remark is then copied in the margin next to the original proposition 1.

At this stage the proof for the original proposition 3 is crossed out an replaced with a new one.

At this stage the revision of the proof for the original proposition 6 is updated.

At this stage the proof of the original proposition 9 is revised.

At this stage the the proofs for the original propositions 7 and 8 are replaced (presumably).

Indeed, this is the revision-stage reflected in the revised presentation prepared by Baule. (Except instead of the original formulation of proposition 11, the more general formulation initially included as a part of it, then crossed out there and written in the margin as proposition 14, is included as proposition 10.)

IV There seems to have been a subsequent rearrangement of the propositions: Hilbert seems to have intended to move the associative and commutative laws to positions 9 and 10 respectively, and to move the propositions initially numbered 7 and 8 to positions 11 and 12. Which propositions are to take their place remains unclear: No propositions are assigned to positions 3, 4 or 6, 7 in this final revision. It is worth noting that this revision is, as I said, not reflected by the revised presentation of the axiom system prepared by Baule, but it is also not

reflected by the presentation of the axiom system recorded in the Princeton manuscript recording the 1914-15 lectures, which follows the revised presentation prepared by Baule.

B.1.2 Hilbert's presentation of Peano's axioms

Peano's Axiome:

Axiom 1 wie I

Axiom 2 Aus $a+1=b+1$ folgt $a=b$

Axiom 3 Es giebt keine Zahl a, so dass $a+1=1$ $a+1\neq 1$

Axiom 4 wie Axiom V

Unabhängigkeit:

Axiom 2 ist von den übrigen unabhängig denn $1+1=2$, $2+1=2$

Axiom 3 ist von den übrigen unabhängig: denn $1+1=1$.

Axiom 4 ist unabh. wie früher

Beweis.

Satz 1 wie oben[A]

Satz 3 wie oben

Satz 4 wie oben

Satz 10 Beweis der Satz ist richtig für $c=1$ nach Axiom 2. Wenn er für c richtig ist, so ist er auch für $c+1$ richtig; denn aus $a+c+1=b+c+1$ folgt nach Axiom 2 $a+c=b+c$ und hieraus nach Voraussetzung $a=b$.

Definition Wenn $a=b+c$, so heisse $a>b$ oder $b<a$.[B]

Dann folgt Axiom II.

Beweis von Axiom III $b=a+d$, $c=b+e$, so folgt $c=a+d+e$ also $c>a$ (Benutzung von Satz 3,4)

Beweis von Axiom IV $b=a+c$. Sei ein Gegensatz zur Behauptung $a=b$, so $a=a+c$. Dies ist für $a=1$ unmöglich nach Axiom 3. Ist aber $a\neq 1$, so nach Satz 1 $a=1+d$, also wäre $1+d=1+d+c$ also nach Satz 10 $1=1+c$ was wiederum dem Axiom 3 widerspricht.

Damit haben wir alle alten Axiome bewiesen und wir sind fertig.[C]

[A] Above this line "S. 4".

[B] Below this line, two lines have been written but struck out. They read: "Def. von $<$ wie oben. / Satz 2. Wenn $b=a+c$".

[C] From here on, the remained of what has been written on this page is written in pencil and much of it has been struck out (also in

---

14 so] The manuscript originally had "d.h.", which has been struck out.

14 er] After this word the word "er" has been written once again, but struck out.

16 oder $b<a$] This alternative has been added in pencil.

18 $c=b+e$] The formula originally was "$c=b+d$", but "d" has been struck out and replaced by "e" behind it on the line.

18 Satz 3,4] The manuscript originally read "Satz 1, 3", but the numbers have been struck out and replaced above the line.

19 Dies ist] Before "dies" the word "Ist" has been written, but struck out; "dies" has not been capitalised in the manuscript.

20 nach Axiom 3] This reference has been added above the line with an insertion mark.

21 wiederum] Before this word, the words "nach A" and "dem" have been written, but struck out.

22 Damit haben wir alle alten Axiome bewiesen und wir sind fertig.] This sentence has been struck out.

Wenn $a > b$, so ist nicht $b > a$, denn sonst $a > a$, was nach Axiom IV unmöglich.[D]

Satz 12 beweisen.

Satz 13 Wenn $a \neq b$, so ist $a > b$ oder $b > a$.[E]

a) wenn $a + 1 \neq b$, so entweder 1.) $a = b$ oder 2. $a \neq b$

$a > b$ oder $b > a$ und $b \neq a + 1$, so $b > a + 1$

Satz 13[F] Wenn a mit $> b$, so $b = a$ oder $b > a$

Der Satz ist richtig für $a = 1$ nach Satz 1

Wenn der Satz 13 richtig ist für a, so auch für $a + 1$:

Denn wenn $a + 1$ nicht $= b + c$, so ist auch a nicht $= b + c$ also ebenfalls $b = a$ oder $b > a$

Ist $b = a$ so ist $b > a + 1$, also der Satz 13 für $a + 1$ richt.

Ist aber $b > a$, so $b = a + c$ und hier entweder $c = 1$, in welchem Falle der Satz auch richtig ist, wo $c = 1 + d$, d.h. $b > a + 1$, also auch richtig.

B.2 Hilbert's Preparatory Notes for the 1913 Lecture-Course

Prinzipien der Math. Sommer 1913.

M. H. Ich will in dieser Vorl. (die Grundlagen der verschied. math. Disz. behandeln, Ihnen) die charakt. Art math. Wahrheiten in math. Schlussweisen. (und math. Errungenschaften) anwickeln und erklären und endlich die

---

pencil).

[D] Above this remark, the following has been written but struck out: "$a > a$ ist nicht möglich. Denn $a = a + b$ wegen Axiom IV". Below this remark, the following formulas have been written, but struck out: "$a = b + c = a + c + d$" and "$b = a + d$".

[E] Below the formulation of the theorem "dann wäre $a \neq b + c$" (but nothing more) has been written, but struck out.

[F] This proposition and the following proof has been struck out entirely.

---

25 Satz 13] The manuscript originally had "Satz 14", but the number has been struck out and replaced.

25 $b > a$] The manuscript originally had "$b < b$", which has been struck out and replaced by "$a < b$".

26 1.)] This number has been added above the line.

26 2.] This number has been added above the line.

28 $b > a$] The manuscript originally had "$b = a + 1$", but "+1" has been struck out, and "=" has been overwritten with a ">".

31 so ist auch] Before these words, the words "sonst $b + a + 1$" have been written, but struck out. The words "so ist" have been written once again at the start of the following line.

33 und hier entweder] Before these words, the words "also entw" have been written, but struck out.

2 (die Grundlagen der verschied. math. Disz. behandeln, Ihnen)] The parentheses around this part of the sentence seem to have been added subsequently.

2 behandeln] Before this word, "ferner di" has been written, but struck out.

3 Wahrheiten in math.] These words have been added in pencil above the line with an insertion mark.

3 (und math. Errungenschaften)] The parentheses around this phrase seem to have been added subsequently.

3 anwickeln] The manuscript originally had "vorführen", which has been struck out and replaced above the line in pencil.

allge. wissenschaftl. und erkenntnisstheor. logischen Deduk. der Math. erörtern. (Diese Sätze geben zugleich eine ungefähren) Ueberblick über Inhalt u. Disp. meiner Vorl. in 2 grössere Abschnitten

A. Axiomatische Methode
B. Das math. Denken u. die Logik.[G]

Axiomatische Methode

Zur Untersuchung der Grundlagen einer math. Diszipl. hat sich heute aus dem Musterbeispiel der Geometrie heraus eine Methode entwickelt: Die axiomatische[.] Ihre Charakterisierung vgl. Vorl. S.S. 1908 S. 5

Kap I Axiome der Algebra Vorl. 1905 S. 11-35[H]

Kap II [Axiome der] Geometrie Vorl.1905 (S. 36-69) (Widerspruchsfreiheit, Unabhängigkeit der Axiome III bez. IV bez. V von allen übrigen)[I]

Nun ermöglicht die axiomatische Methode auch die Behandlung von methodischen Fragen d.h. ob ein Beweis gewisse Axiome nothwendig hat oder nicht? Ob gewisse Sätze aus anderen ohne Hinzuziehung bez. ohne nochmalige Hinzuziehung gewisser Axiome folgen? ob ein Beweis kürzer ist als ein anderer, der kürzeste Beweis? etc. Als Beispiel behandele ich hier gewisse Fragen der Elementargeometrie Vorl 1905 S. 75S. 86.

Meinen Beweis des Euklidischen Satzes Satz[J] 29 § 18 in meinen "Grundla|gen der Geometrie" gieb ausführlich. Dabei wird Satz 30 besser wie folgt bewiesen:

[Diagram]

Zuerst beweise: Man [one word unreadable] das Dreieck ABC im positiven Sinne, so definire

[G] There was originally a second main section, for which an entry appears in this list as "B. Problem der Quadratur des Kreises", which has been struck out and the "C." of the third item on the list has been struck out and replaced by "B.".

[H] After the reference to the 1905 lecture-course, Hilbert has added in pencil "Vorles. 1917 S. 43-47".

[I] With the same pencil as the reference to the "Vorles. 1917", the reference "S. 38" has been added here.

[J] Below this line (but still on this page), the following remark has been added in different ink: "(Umgekehrt aus diesem Euklidischen Satze von der Gleichheit der Höhen in gleichen Dreiecken mit gl. Grundlinie folgt der Pascalsche Satz unmittelbar vgl. mein Buch § 21 (S. 67))".

4–5 (Diese Sätze geben zugleich eine ungefähren)] The parentheses around this part of the sentence seem to have been added subsequently.

5 2] The manuscript initially had "3", which has been overwritten with "2".

5 grössere Abschnitten] The manuscript originally had "grosse Abschnitte", the trema-dots over the "o" and the comparative "er" and the "n"-ending of "Abschnitte" has been added in pencil subsequently.

6 Methode] The manuscript originally had "Methoden", but the plural "n" has been struck out.

11 Algebra] The manuscript originally had "Arithmetik", which has been struck out and repalced above the line in pencil.

11 11-35] The manuscript originally gave the page-range "7-35", but "7" has been struck out and replaced by "11".

15 Hinzuziehung] Before this word, the letters "noch" have been added below the line, presumably as the first letters of "nochmalige", but struck out again.

18 gieb] The manuscript originally had "beweise", which has been struck out and replaced above the line.

$$\left.\begin{aligned}(OAB) &= -I(\Delta 0AB)\\ (0BC) &= +I(\Delta 0BC)\\ (0CA) &= +I(\Delta 0CA)\end{aligned}\right\}\text{Dann folgt durch Fällen der punktirten Höhen}$$
$$(0AB)+(0BC)+(0BA) = I(\Delta ABC).$$

Aus dieser neuen Darstellung des Inhaltsmasses I eines Dreiecks entnimmt man leicht S. 30, weil sich ja die Zwischenstücke wegen der entgegengesetzten Richtungen immer fortheben

[Diagram]

Meine Untersuchung über das gleichschenkligen Dreieck vgl. mein Buch Anhang II insbesondere mein Axiom der Einlagerung.

So sehen wir also, dass ohne Archimedische Axiome sowohl die Lehre von den Proportionen, als die Lehre von den Flächeninhalten begründet werden kann – letztere sogar, wenn man das Dreieckcongruenzaxiom nur im anderen Sinne nimmt, aber dafür das Axiom der Einlagerung hinzufügt. |

Kap III Axiome der Physik. Vorl. S. 1905 S. 121-190[I]

Parallelogramm der Kräfte etc. Nur die Axiome und Sätze als Tatsachen *ohne Beweis* angeben. Sie werden sich dann M. Herren, wenn Sie die betreffenden Disziplinen studiren meiner Ausführungen erinnern; diese werden Ihnen dann ein Leitfaden sein können, durch den Ihnen die Gemeinsamkeit der Grundprinzipien und die Aehnlichkeiten der Formulierungen zum Bewusstsein gelangt. Erschrecken Sie daher nicht, wenn Ihnen manches nicht hier gleich völlig verständlich wird; lassen Sie aber den Eindruck auf sich wirken über die Mannigfaltigkeit der axiomatischen Methode.

Relativitätstheorie Vorl. Mechanik des Kontinuum 1911 S. 80-93

Zusatz zu S. 89: In der alten Newtonschen Auffassung wollen wir auch zuerst untersuchen 1.) derselbe Ort verschiedene Zeit $t_2 - t_1 = \tau_2 - \tau_1$ 2.) Verschiedener Ort, dieselbe Zeit $x_2 - x_1 = \xi_2 - \xi_1$

B Das math. Denken u. die Logik.

Kap. IV Die Paradoxa der Mengenlehre Vorl. S. 191-196

Erstes Paradoxon: S. 199-203 Aufklärung allenfalls, indem man den Begriff der Worte oder des Wortlautes

[I] The first "S" is presumably short for "Sommer", the second for "Seiten".

34 diese werden Ihnen] The manuscript originally had "Sie werden", which has been struck out.

36 daher] This word has been added below the line.

42 B] The section was originally labelled "C", which has been struck out and replaced to the left.

43 Paradoxa der Mengenlehre] Before these words, the word "Mengenlehre" has been written, but struck out.

44 199-203] The page range was originally "197-", which was subsequently corrected to "196-", which has, once more, been corrected to "199-".

anzweifelt, der doch nicht einen von der Interpretation unabhängigen Sinn zu haben braucht.[A]

Zweites Paradoxon: S. 210-211 (Zermelo) Aufklärung, indem man die Unbestimmtheit "aller" Dinge der "Welt" anzweifelt, wo also auch alle Erfahrungsdinge etc. gemeint sein müssen, was naturgemäss vage ist.

Drittes Paradoxon S. 203[B]-210, 211-213. Sehr schwerwiegend.[C] | Fortsetzung der Mengenth. A = Abzählbare Menge. $A^A = B > A$ = Menge aller zahlentheor. Funktionen = Continuum. = ein-ein-, drei-etc. ∞ viel dimensionalen Continuum in Menge aller stetigen Funktionen etc. $C = B^B > B$ = Menge aller Funktionen. $C^C > B$ = Menge aller Funktionen-Funktionen etc. Grösstes Problem, ob zwischen A und B§ noch Menge anderer Mächtigkeit enthalten sind. Cantor hat die nächste höher A aufgestellt u. studirt.

Theorie dieser Menge der Ordnungstypen nach meiner Vorlesung Zahlbegriff u. Quadratur des Kreises 1897/98 die 3 vorletzten Seiten 52-54.

Zum Schlusse dieses Abschnittes über Paradoxien der Mengenlehre möchte ich Ihnen noch einige lehrreiche u. charakteristische Fehlschlüsse vorführen:

1.) Geometrische Vorl. Grundlagen der Geom. 1902 S. 3-4

Täuschung durch falsche Zeichnung der Figur.

2.) Analytische $tang\ x = i$; hieraus bestimme x sodann $tg(x+y) = \frac{tg\ x + tg\ y}{1 - tg\ x\ ty\ y} = \frac{1+tg\ y}{1-i\ tg\ y} = i$ d.h. von y unabhä[n]g[ig][D].

(Es giebt kein x weil $tg\ x = \frac{e^{ix}-e^{-ix}}{i(e^{ix}+e^{-ix})}$

$e^{ix} - e^{-ix} = -e^{ix} - e^{-ix}$, $2e^{ix} = 0$) |

3.) Variationsrechnung. Steiners Beweis für die Eigenschaft des Kreises Max. des Inhaltes bei geg. Umfang

Vorl. Variationsrechnung 1904/05 S. 70-71. Sie werden Fehlschluss erkennen, wenn ich folgenden Satz beweise: Nach Steiner können wir ebenso beweisen, dass 1 die grösste ganze Zahl ist. Es lässt sich nämlich ein Verfahren finden, dass zu jeder ganzen Zahl n eine grössere zu finden gestattet nämlich das Quadriren n^2, das bei 1 versagt. Es fehlt aber der Beweis für die Existenz einer grössten ganzen Zahl. Ebenso ist es bei der

[A] With some distance to the last word but on the same line Hilbert has added "Frege zugestanden.".

[B] Below this page with a vertical line next to both "S. 206." has been written.

[C] Below this line, Hilbert has written "Bemerkung über Dedekind S. 211-212" and below "Dedekind", Hilbert has further added "zugestanden.".

[D] Below the word "unabhäg." Hilbert has written "nicht $\frac{a}{a}$!" and below that "$e^{ix} = i$".

45 doch nicht] Before these words, the words "es ein" have been written, but struck out.

47 wo also auch alle Erfahrungsdinge etc.] Before these words, the words "und wodurch nur mathematische Dinge als geg. an" have been written, but struck out.

49 ∞ viel] This remark has been added above the line.

51–52 Menge anderer Mächtigkeit] Before "Menge" the word "and[ere]" has been written, but struck out.

64 Sie werden Fehlschluss erkennen, wenn ich folgenden Satz beweise:] These words have been added between the page reference and the following sentence, partly above the line.

Riemannschen Anwendung der berühmten Dirichletschen Prinzips.

[E] Die Paradox der Alten Achilles u. Schildkröte (Unendliche Summen wie $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\frac{1}{16}+\ldots$ (brauchen nicht ∞ zu sein können endliche Werte haben; alle diese ∞ vielen Zeitintervallen geben zusam[men] das endliceh Zeitintervall 1) und der fliegende Pfeil (Unendlich viele verschwindende Grössen brauchen zusammen nicht 0 zu geben: $\frac{1}{n}+\frac{1}{n}+\ldots\frac{1}{n}=1$) sind wie gesagt Paradoxa der Differential u. Integralrechn[ung] u. längst aufgeklärt. Manche moderne Philosophen Ignoranten u. rückständig (Cohen)

Kap. V Logikkalkül Vorl. 1905 S. 215 (Vorhandensein von Paradoxien)

Aber Widersprüche wissen unbedingt ausgestaltet werden: S. 217-218. Dem Zwecke um solche zu vermeiden, hat man statt der gewöhnlichen Sprache eine der math. [one word unreadable] feste Formelsprache, die feste Kontrolle ermöglichen soll. Zuvor das Axiom vom a priori S. 219. Also Logikkalkül = Formelsprache, die ermöglichen soll von den Aussagen, wie sie in den Axiomen niederlegt, sind zu anderen überzugehen. Daher wichtigster Begriff die *Identität* von Aussagen etc. S. 224-249.

Kap VI[F] Der Zahlbegriff und die Axiomenlehre. ist extra ausgearbeitet und hier neben in Vorl. 1805 angeheftet worden.

Kap VII Einige Unmöglichkeitsbeweise.

Wenn ein fester Kreis mit Mittelp. geg. so kann man jede quadr. Gl. lösen. Wenn man 3 feste Kreise ohne Mittelpunkt hat, so kann man die Mittelpunkte finden. Aus 2 festen Kreise aber ist dies unmöglich.[G] |[H]

B.3 Hilbert's Preparatory Notes for the 1914/15 Lecture-Course

[E] The following paragraph has been written on the previous page, an arrow leads from its beginning to this place in the text.

[F] This section was originally written below what is now chapter VII, but has been moved to this place by an arrow.

[G] Towards the bottom of the page "da Kubusverdopp. des Ca[sus irreducibilis]" as well as "(Un[möglichkeit] der Quadr[a]tur des Kreises, der Cubusver[dopplung]", but both have been struck out.

[H] 3 pages later, on the bottom of the page before the notes for the 1914/15 notes begin, the following references have been written: "Litteratur: meine Grundlagen, Hessenb[er]g, Me[n]g[e]nt[h]e." Also on this page one finds the following list of topics: "Unm. der Quad[ra]t[ur] des Kresies, der Kubusverdoppel, des Casus irreducibilis der Auflö[su]ng der Gl. 5^{ten} Grades.", which probably still belong to the notes for 1913, since Hilbert had finished them by mentioning the same impossibility proofs as are mentioned here.

68 berühmten] This word has been added below the line with an insertion mark.

69–70 brauchen nicht ∞ zu sein] these words have been added above the line with an insertion mark.

74 1905] The year has been added above the line with an insertion mark.

80 Zahlbegriff und die] These words have been added above the line with an insertion mark.

82 Einige] This word has been added above the line with an insertion mark.

82 Unmöglichkeitsbeweise] The manuscript originally had "Unmöglichkeitsprobleme", but "probleme" has been struck out and replaced behind it by "beweise".

Probleme u. Prinzipien der Math. Winter 1914/15.[I]

Wir beginnen unsere Semesterarbeit in einer *ernsten* u. *schweren* Zeit. Die ganze Welt fast ist mit *Waffengewalt* über uns hergefallen u. will uns *vernichten*[J]. Aber nicht bloss die physische Vernichtung ist das Ziel unserer Feinde; nein das *gesammte* Ausland fast hat auch einen *Lügenfeldzug* gegen uns eröfnet: nicht nur die ausländische Presse sondern auch die ausländischen Minister wetteifern unter einander, wer am meisten lügen kann, um uns zu verläumden u zu schaden. Wenn auch für uns dieser Lügenfeldzug auch nicht schaden, wird, da die Wahrheit doch an den Tag kommt so ist er für uns die wir Wissenschaft hochhalten um so *verabscheuungswürdiger*. Denn die Kraft, die in der Wahrheit liegt, ist es, die uns stets treibt und zwingt, mit Wissenschaft uns zu beschäftigen. Wo kein Streben nach Wahrheit, kein Sinn für Wahrheit ist, da ist auch keine Wissenschaft und da hat *Wissenschaft zu treiben keinen Wert*. Ganz besonders gilt das für unsere math. Wiss. die die verkörperte Wahrheit an sich ist und auch unsere Wissenschaft würde zur Spielerei herabsinken, wenn sie nicht im Dienste der Wahrheit und auf dem Boden der Wahrhaftigkeit steht. Wer mit Lügen gegen uns steht, hat sich auch von aller Wissenschaft selbst ausgeschlossen. Wenn es uns nun obliegt, uns mit den Prinzipien der Math. in diesem Semester zu beschäftigen, so sind die Schwierigkeiten dabei 2erlei Art

1.) sachliche, weil wir trotz des elementaren Charakters der Fragen, die wir behandeln wollen dabei auf manche

auch heute | noch ungelöste Fragen treffen

2.) pedagogische, weil das Verständnis der Principien doch gewisse ausholende Kentnisse der math. Wissenschaft voraussetzt.

[I] At the top of the page, Hilbert has written (in pencil) "Vgl. Hessenberg, Transzendenz von e u. π 1912.".

[J] Following this sentence, the following additional remark has been written but struck out: "mit uns würden[struck out: sie] unsere Freinde auch unsere Wissenschaften vernichten.".

6–7 auch nicht schaden, wird, da die Wahrheit doch an den Tag kommt] The manuscript originally had "weniger gefährlich ist", which has been struck out and the new formulation has been written above the line and continues below.

7 die wir Wissenschaft hochhalten] These words have been added above the line with an insertion mark.

11–13 und auch unsere Wissenschaft würde zur Spielerei herabsinken, wenn sie nicht im Dienste der Wahrheit und auf dem Boden der Wahrhaftigkeit steht. Wer mit Lügen gegen uns steht, hat sich auch von aller Wissenschaft selbst ausgeschlossen] These words have been added above the line until "Wer mit Lügen gegen uns", after that the addition continues in a footnote at the bottom of the page, where the words "gegen uns" have been written once again. The words "Wer mit Lügen gegen uns steht, hat" replaced a prior formulation, which read "Die Feinde, die im Lügenfeldzug gegen uns stehen, haben", which has been partly struck out, and the inflection endings of the verbs "stehen" and "haben" have been changed to "steht" and "hat" to suit the new formulation. At the end of the footnote the following additional remark has been written, but struck out: "Wir werden künftig allein Math. treiben.".

15 trotz des elementaren Charakters der Fragen, die wir behandeln wollen] This remark has been added below the line with an insertion mark.

17 gewisse ausholende Kentnisse] The manuscript originally had just "die Kentnisse", which was struck out and replaced by "gewisse", which was struck out and replaced by "gewisse tiefere", of which "tiefere" was struck out and replaced by "ausholende".

Es giebt einen Weg, auf welchem sich diese 2 Schwierigkeiten völlig vermeiden lassen und welchen ich einschlagen will d.i. an der Hand spezieller Probleme die prinzipiellen Gesichtspunkte hervortreten zu lassen: Ich wähle die historischen u. klassischen Probleme 1.) Quadratur des Kreises 2.) Parallelenaxiom.

Den systematishcen Aufbau der Grundlagen der Math. darzulegen behalte ich dann einer späteren Vorlesung vor.

Kolleg S.S. 1910 S. 4 - S. 156

Die Geometrie dient als Vorbild für die Axiomatisierung aller übrigen Disziplinen und zwar ist es immer so dass das Ziel der Beweis der im Mittelpunkt der Diciplin stehenden Satzes ist: wie in der Geometrie der Satz, das die Gerade durch eine lineare Gl. geg und die Bewegung durch orthogonale Transformation der Koordinaten. Die axiomatischen Untersuchungen in der Geometrie laufen stets darauf hinaus, Axiome | d.h. einfache, plausible, durchsichtige, Einzel-Annahmen zu machen, auf Grund deren dann jener Satz bewiesen wird. Also gewissermassen eine logische Zerlegung jenes Satzes in seine Bestandteile.

So nun weiter ganz analogi in der Statik, in deren Mittelpunkt der Satz vom Parallelogramm der Kräfte steht (Kollegheft 1905 S. 123) ferner in der Theorie des Gleichgewicht der Satz: Summe aller Kräfte = 0 wo als Axiom z.B. das Axiom von der Unmöglichkeit des Perpetuum mobile dienen kann Kolleg 1905 S. 137.

Ferner in der Dynamik Ableitung der Lagrangeschen Gl. wo als Axiom etwa das Gausssche oder das Hamiltonsche Prinzip dienen kann. Endlich Termodynamik Ableitung des Satzes

$$H = \int \frac{dQ}{\Theta}$$

aus den 6 Axiomen Kollegheft 1905 S. 155-167

Axiome der Wahrscheinlichkeit Kolleg 1905 S. 169.

M. H. Die vor Weihnachten behandelten Axiome der Geometrie sind nichts anderes als eine systematische Untersuchung des Raumbegriffes. Neben dem Raum tritt aber die Zeit und seit Alters her sind Raum u. Zeit zusammengehörige Untersuchungsgegenstände. Vorles. Kollegheft. 1910/11 Mechanik S. 273-287.

Das letzte Problem, das ich behandeln will, sind die logischen und mathematischen Paradoxien. Die ältesten

---

22 Den] Before the definite article, the letters "Ei" have been written, presumably as the first letters of the indefinite article "Einen", but struck out.

24 S.S. 1910 S. 4 - S. 156] The manuscript originally had "S.S. 1908 S. 9 -", which has been struck out.

25 die Axiomatisierung aller übrigen Disziplinen] Before these words, the words "alle übrigen" have been written but struck out.

26 das] The definite article has been added above the line with an insertion mark.

28 hinaus] This word has been retraced in ink, overriting an initial "heraus".

31 in deren Mittelpunkt] The manuscript originally had "wo der Mittelpunkt", but "wo" has been struck out and replaced above the line by "in" and "der" extended to "deren".

41 S. 273-287] An "S." has been written behind "Mechanik", but no page-reference has been added. The page reference follows (with a new "S." in the following line.

sind die Paradoxien der Sophisten (fliegende Pfeil[K] beruhen auf dem Begriff der Stetigkeit). Dann die Antinomieen | von Kant (Beruhen auf Anwendung des math. Unendlicen im Raum u. Zeit auf die Wirklichkeit). Ich will nur die logisch-mathematischen Paradoxien behandeln, die am furchtbarsten sind vom folgenden typischen Beispiele:

1. Dieser Satz, den ich eben ausspreche, ist unwahr.

   Lügende Kreter: Wenn er wahr ist, ist er unwahr und wenn er unwahr ist, ist er wahr.

2. Die kleinste ganze Zahl, die nicht mit weniger als sechs und zwanzig Silben definirt werden kann.[L]

   - - - kann doch, wie die Auszählung zeigt, mit 25 Silben ausgedrückt werden.

   Kollegheft Logische Prinzipien 1905 S. 191-214.

Sie bekommen gewöhnlich die Wissenschaft von Grund aus konsequent aufgebaut erst Fundamente dann der Hochbau. Die historische Entwicklung aber umgekehrt und das ist zum Fortschreiten, wenn man weiter arbeiten will nötig zu wissen S. 192 etc

Zeige dass 1 u. 2. intim zusammenhängen und so verschiedenartig diese beiden Paradoxien scheinen, doch denselben Kern.

Wir machen uns zunächst das Problematische des Begriffes "durch gewisse Silbenzahl Definierbaren" klar. Vergegenwärtigen wir uns überhaupt den Begriff des Definirtsein als Allgemeinbegriff. Ist a definirt, wenn ich sage

[K] Below the line, Hilbert has added "Archilles u. Schildkröte".

[L] Hilbert has written a number above each syllable of this sentence, counting them from "1." through "25.". Only the syllables of the final formulation are so numbered, which suggests that the numbers were added last, after Hilbert was satisfied with the formulation.

43 der Sophisten] Before these words, the word "von" has been written but struck out.

44–45 nur die] Before "nur" the word "die" has been written once already.

45 logisch-mathematischen] The letters "matn", presumably short for "mathematischen" have been added subsequently, starting on but then reaching below the line.

46 Dieser Satz] Above the line the words "Diese Behauptung" have been added in pencil. Though an insertion mark places them after "Dieser Satz", the fact that "Diese" is capitalised suggests it may be intended as a replacement.

48 nicht mit] The manuscript has "mit nicht", but the order of these words has been switched by means of an arrow.

48 sechs und zwanzig] These words have been added above the line; the manuscript seems to have initially had "fünf und zwanzig", but "fünf" has been erased and replaced by "sechs". After these words the numbers "24", "23" and "19" had been written, but have been struck out.

48 definirt] The manuscript originally had "ausgedrückt", which was struck out and replaced above the line with "definirbar ist", which has also been struck out and "definirt" has been added as a replacement above the line with an insertion mark.

49 25] The manuscript initially had "18" and then "22", both of which have been struck out and replaced below the line with "25".

52 Die historische Entwicklung] The manuscript originally had "In der" instead of "Die".

54 zusammenhängen] Hilbert seems to have misspelled this word in anticipation of the following formulation "denselben Kern" as "zu selben hängen".

$a = 1$, wenn $2^{\sqrt{2}}$ irrational
$= 0$ *rational* [M] oder wenn ich sage a $= 1$, wenn $2^{\sqrt{2}}$ irrational
$=$ lem (sinnloses Wort), wenn $2^{\sqrt{2}}$ rational
vgl S. 202 unten

Definition einer Irrationalzahl durch Gesetz, aber auch durch Würfeln (nein!)

Wie kann ich mich retten?

Stand des Wissens Stufentheorie Poincare S. 199 Rüssel [sic!].[N] Empfindung, dass an der Stufentheorie etwas wahres ist; aber sie muss noch sehr vertieft werden. Und da ist es vor Allem nötig, zu sehen, ob nicht, wenn man den Begriff der Silbenzahl verbietet man alle Paradoxien abschneidet[1]. Das ist leider nicht der Fall. Zermelo's Paradoxon S.210.[2] Standpunkt von Dedekind S. 11 Frege S. 213. Cantor bestes rein mathematisches Paradoxon S. 204-209. Das führt dann zu der Einsicht, dass es der Begriff alle[3] ist, bei dem wir schon stutzen müssen. Dieser kommt schon bei den ganzen rationalen Zahlen und deren definition vor. Daher wollen wir diese genauer einführen. Zunächst reelle Zahlen. Arithmetik axiomatisch begründen nach dem Vorbilde der Geometrie. Wir besinnen uns dass wir bei der Begründung der Geometrie die Arithmetik skrupellos angewandt haben u. natürlich auch die Logik. Jetzt werden wir die Logik und den Anzahlbegriff skrupellos anwenden Kollegheft Logische Prinzipien S. 13-35. |

So sehen wir zwar, wie sehr sich wieder die axiomatische Methode bewährt; aber sind wir unserem Problem, der Aufklärung des Paradoxons näher gekommen? Wir sahen im Begriff "alle" die Schwierigkeit. Wir werden auf den einfachsten Fall der Anwendung des "alle" zurückgehen. Dieser erste u. einfachste Fall ist genau das System "aller" ganzen Zahlen. Daher wenden wir uns der Axiomatisirung dieser zu. Vgl. die beliegenden Blötter 1 bis 8.

Elemente der Theorie der Ordnungstypen:Kollegheft Zahlbegriff u. Quadratur des Kreises 1897/98 S. 52-54.

Axiome der Logik: Kollegheft Logische Prinzipien 1905 S. 219 - S. 249

[1] also Versuch durch Abschneiden der Philologie die Paradoxe zu beseitigen dies gelingt nicht, wie Zermelos Paradoxon zeigt.

[2] Nun könnte man sagen, [struck out: dass] die benutzten Begriffe seien philosophischer Natur "Dinge", wie Menschen etc. also Versuch durch Abschneiden der Philosophie die Paradoxe zu beseitigen, gelingt auch nicht, wie das math. Paradoxon zeigt.

[3] Den Begriff "alle" könnten wir aber nicht ohne weiteres aufgeben, ohne Math. überhaupt abzuschneiden.

[M] To the right of this example, Hilbert has added another with the note "vorher noch:", the example is: $a = 1$ wenn die $10^{10^{10}}$te Dezimalziffer von π $a = 0$ ...

[N] Behind Russell's (misspelled) name, the word "unbefriedigend" has been writte, but struck out.

63 nicht, wenn] Before these words, the word "nicht" has been written once already followed by the letter "a", but both have been struck out.

65 Standpunkt von Dedekind S. 11] Before these words, the word "Begriff" has been written, but struck out.

65 Cantor] Before Cantor's name, the word "Das" has been written but struck out.

65 rein mathematisches] These words have been added above the line.

70 und den Anzahlbegriff] These words have been added in pencil at the bottom of the page and inserted here with an arrow.

70 Kollegheft Logische Prinzipien S. 13-35.] The manuscript originally had "Vorl. S. 13-", which has been struck out.

73 Fall der] Before these words, the words "Fall der" have been written once already.

76 S. 219 - S. 249] The reference was originally "S. 219-268.", but "268" has been struck out and "S. 249" written behind it on the line.

Früher hatten wir "Dinge" zu grunde gelegt und fingen an mit "Es giebt ein System von Dingen, die nennen wir Punkte, Zahlen etc." Jetzt stellen wir dem analog ein Axiom des Denkens oder der Existenz einer Intelligenz voraus: Wir haben die Fähigkeit Urteile zu bilden: $X, Y, \ldots$ [O] Das spezifisch Neue ist, dass wir die Urteile (statt den früheren Dingen, nicht etwa wie in der Geometrie in Punkte, Geraden einteilen, sondern) von vorneherein die Dualistische Einteilung in falsche und richtige also in 2 Klassen. Diese Verteilung ist nicht willkürlich, sondern folgt gewissen Gesetzen. Zu jedem X giebt es in $\overline{X}$ in der anderen Klasse "-" heisst Negation. Da es in der Logik nur auf richtig oder falsch ankommt, so werden wir ein Zeichen dafür brauchen, dass 2 Urteile oder Aussagen zur selben Klasse gehören wir sagen einfach = "gleich". Dann brauchen wir nur noch die Operation und: + und oder: · Axiome Kolleg 1905 S. 225- |

[O] Behind the list of variables, Hilbert has written "Zwei Urteile sind identisch oder nicht.", but struck out the entire sentence.

82 Da] Before this word, the word "Wenn" has been written, but struck out.

83 richtig oder falsch] Before these words, the word "die" has been written but struck out.

Appendix C

Cod. Ms. Hilbert 606

Editorial Introduction

The manuscript edited here is the SUB's Cod. Ms. D. Hilbert 606.

The manuscript consists of 9 sheets of paper and was in all likelihood originally written on 4 double sheets of paper (numbered (presumably by Hilbert) "1" through "4" in blue crayon), which have since been torn apart, and a single extra sheet (which may, in fact, not belong to the manuscript at all). The sheets have been numbered "1" through "9" by an archivalist, although it should be noted that when the sheets were so numbered, they were out of order and as a result the first four sheets of the manuscript have been numbered "6" through "9"; by contrast, the pagination in blue crayon correctly identified the original double sheets as "1" and "2".

The manuscript is written in Sütterlin longhand.

There can, in my opinion, be little doubt of the fact that the manuscript was at least *written* (for the most part[347]) by Hilbert: Comparing the manuscript with other manuscripts written in Sütterlin, of which Hilbert's authorship is certain (such as Codd. Ms. D. Hilbert 503 and 504[348]), confirms that the handwriting is Hilbert's.

The comparison with Cod. Ms. D. Hilbert 503 is further enlightening because it shows that Hilbert was in the habit of leaving large outer margins, as he does in Cod. Ms. D. Hilbert 606, in particular when taking down notes of lectures he heard. This, together with the fact that Hilbert stopped writing in Sütterlin longhand soon after[349], or during his days as a student, suggest that Cod. Ms. D. Hilbert may constitute a set of notes taken at a lecture

[347] In two places (on sheet 2 (recto-side) and 5 (recto-side)) pieces of paper have been glued onto the sheets, containing text written in markedly different (and clearer) handwriting, which is certainly not Hilbert's own; (there is also one piece of paper glued onto sheet 1 (verso-side) where the text is in Hilbert's handwriting).

[348] Digitisations of these manuscripts are available on the website of the Göttingen Center for Digitisation:
Cod. Ms. D. Hilbert 503: http://resolver.sub.uni-goettingen.de/purl?DE-611-HS-3211272 (503) and
Cod. Ms. D. Hilbert 504: http://resolver.sub.uni-goettingen.de/purl?DE-611-HS-3211271 (504) (last checked 2/3/2022).

[349] This claim is based on Hilbert's scientific diary. Hilbert started writing in it some time after 1885. The first 12 entries are written in Sütterlin, but after the 12th entry, all are written in more or less the modern form of German longhand. While the entries are impossible to date precisely, the entries in the first volume very likely were made between 1885-1891. Since Hilbert stopped using Sütterlin on p. 2 of the first volume, this suggests a date-by-which for this manuscript of (estimating generously) 1887±1 year.

or presentation some time during Hilbert's time as a student at the university of Königsberg.

Finally, there is one clear indication of the date of composition: On p. 4′r, Frege's "Grundlagen der Arithmetik" is referred to as "neuerdings erschienen". Frege's book appeared in 1884, which can therefore be regarded as a date-after-which for this manuscript. Of course, "recently published" is not an exact indication as to how long the book had, by that point, been available, but between the date-by-which (based on the use of Sütterlin-longhand) and this date-after-which, the manuscript can relatively exactly be dated to the years 1884-1888; indeed, if these are notes Hilbert took on some occasion, they were likely written before 1886, when Hilbert ceased to be a student.

The SUB Göttingen assigns to it the title "Logik und Urteilslehre" and the further description "analytische und synthetische Urteile in der Mathematik". While the title "Logik und Urteilslehre" describes the content of the manuscript not altogether inadequately, it is both too broad and in other respects too narrow: On the one hand, neither the title nor the description given by the SUB mentions Kant even though the manuscript is, in fact, a discussion of *Kant's* views on arithmetical judgements, and of criticisms of Kant's views by subsequent writers. On the other hand, the manuscript does not contain a discussion of logic or judgements *in general*, but rather, as the further description given by the SUB indicates, specifically of mathematical judgements – and, in fact, even more specifically of the judgements belonging to elementary arithmetic, especially of Kant's example "$7 + 5 = 12$".

A more accurate description of the manuscript would, therefore, be something along the lines of "Kants Lehre der synthetisch-apriorischen Natur arithmetischer Urteile". Indeed, on p. 1′v the question whether arithmetical judgements are apriori or aposteriori and, if the former, whether they are analytic or synthetic, is stated explicitly and its investigation is identified as the "main task" of the discussion.

The manuscript is in general not easy to read because the handwriting is irregular and often appears rushed: the shapes of letters vary, sometimes letters or even whole syllables appear to simply be missing. The difficulties facing the reader are compounded by the fact that in many places the text has been heavily redacted and revised, leaving the original formulation even harder to make out (because it has been struck out in some manner) and the intended replacement often crammed into far too small a space between two lines.

Although I feel confident saying that I have succeeded in accurately transcribing most of the manuscript, there are a few words here and there I simply was not able to decipher; in those places, I have either inserted a conjecture between cruces, in case it was somehow clear from context what the text in all likelihood ought to say, or merely noted how many words or letters I found impossible to make sense of. (In some places, always close to the outer edges of the pages, the manuscript simply *is* impossible to read because parts of the paper have been torn away.)

Although I have made an effort to decipher as much as possible of the text, there are places where I have not succeeded. These are marked in the text by enclosing a conjectured reading in cruces († †); "†…†" indicates that I found the manuscript entirely unreadable (for some reason or other).

Text

Kant bezeichnet die Untersuchung der Möglichkeit des Erkennens als die erste und wichtigste Aufgabe der 1r
Philosophie. Während alle übrigen Wissenschaften sich die Erforschung des Erkennen besonderer Gegenstände zum Zweck setzen, †erscheint† es ihm als das eigentlich Problem der Philosophie, die Möglichkeit jenes Wissens u Erkennens selber zu erklären. Diese neue Problemstellung ist für den Kantschen Kriticismus von fundamentaler
5 Wichtigkeit und erhält durch Kant ihren präzisen Ausdruck in Form der Frage: "Wie sind synthetische Urteile a priori möglich?" Offenbar liegt dieser Fragestellung die Voraussetzung der thatsächlichen Existenz synthetisch apriorischen Urteils zu Grunde. Im Hinblick auf ihre wirkliche Existenz wird es verlangt, die Art u Natur ihres Zustandekommens und damit ihre Möglichkeit darzuthun. In der That gilt nun für Kant die Existenz s. a. Urteile[I] wenigstens in der Mathematik u den Naturwissenschaften als feststehende Thatsache, während von andrer Seite
10 diese Voraussetzung u Grundlage der obigen Kantsche Fragestellung vielfach Widerspruch erfahren hat. | 1v

Vor allem was die Sätze der reinen Mathematik und speziell der Arithmetik anbetrifft so ist ihre synt. apr. Natur von einigen Philosophen nur mit Bedenken zugegeben von andren sogar auf das entschiedenste bestritten u geläugnet worden. Es dürfte daher schon wegen der Bedeutung der Frage für diese Festigkeit der Grundlage des philosophischen Systems interessant sein, die bezügliche Standpunkte einer näheren Erörterung zu unterziehen,
15 zumal Kant selber gerade auf die synth. ap. Natur der rein mathematischen u speziell der arithmetischen Urteile besondren Wert zu legen scheint.

[I] Throughout the manuscript "synthetisch apriorisch" is frequently abbreviated, sometimes as simply as here, by merely writing the first letters of each word, "s. a.".

---

1 bezeichnet] Before this word, the letters "kennzeichn" have been written, but struck out.

4 neue] Before these word, the words "neue u fundamenen" have been written, but struck out.

5 ihren] Before this word, the word "selber" has been written, but struck out.

5 Form] This word has been added above the line with an insertion mark.

5 Frage] The manuscript originally had "Fragestellungt", but "stellung" has been struck out.

8 und] Before this word, the words "dahe ihre" have been written, but struck out.

8 gilt nun] The manuscript originally had "ist noch", which has been struck out and replaced above the line by "gilt nun".

9 als] The manuscript originally had "für eine", which has been struck out and replaced above the line with "als".

9 feststehende Thatsache] Before these words, the words "festeste" and "ausgemachte" have been written, but struck out.

12 von einigen Philosophen] The manuscript originally had "teils", which has been struck out and replaced above the line with an insertion mark "von einigen Philosophen".

12 von andren] The manuscript originally had "teils", which has been struck out and replaced above the line with "von andren".

13 schon] Before this word, the words "nicht blos an sich sond", "so", "son", "auch" have been written, but struck out.

13 wegen] This word is exceedingly hard to read due to physical damage to the manuscript, so it should be regarded as half a conjecture.

13 Festigkeit] This word is exceedingly hard to read due to physical damage to the manuscript, so it should be regarded as half a conjecture; only the ending "keit" can be definitively discerned.

14 philosophischen Systems] Before these words, the words "Kantschen" and "Syst" have been written, but struck out.

Um für die vorliegende Frage nach der Natur der analytischen Urteile, das volle Verständniß zu gewinnen sowie die bezüglichen Behauptungen Kants von vorneherein von etwaigen Mißdeutungen zu bewahren, ist es nothwendig diejenigen Stellen aus d. K. d. r. V. und d. P.[II] heranzuziehen, in denen die Definitionen der Begriffe apriori und aposteriori, analytisch und synthetisch enthalten sind.

Zu Beginn der Einleitung der K. d. r. V. sagt Kant:

"Solche allg. pag 35 erkannt wird."[A,III]

In dem darauf folgende Abschnitte findet sich der Unterschied der analytischen und | synthetischen Urteile 1′r klargelegt mit den Worten:

"In allen Urteilen pag 39 herausgezogen werden"[IV]

Dazu kommt eine weniger ausführliche Stelle von Anfang des § 2 der "Prolegomena"

"— — Es giebt einen Unterschied der Urteile dem Inahlte nach, nvermöge dessen sie entweder bloß erläuternd sind und zum Inhalte der Erkenntnis nichts hinzuthun oder erweiternd, und die gegebene Erkenntniß vergrössern, die ersten werden analytisch, die zweiten synthetische Urteile genannt werden können!"[V]

Handelt es sich nun darum, ein bestimmtes Urteil oder eine gegebene Gruppe von Urteilen nach den eben gekennzeichneten Gesichtspunkten zu charakterisiren, so bieten sich zunächst durch Kombination vier Urteilsklassen

[A] To the right of this reference, the following remark has been written and underlined in blue crayon: "Vor oder mit dem Zitat beginnt kein Absatz!".

[II] The reference presumably is to the Prolegomena.

[III] This reference is to the very first paragraph of the introduction to the *first* (A) edition of the *Kritik der reinen Vernunft*. The passage in question is indeed, as the marginal comment notes, not the beginning of a paragraph. The first words of the quotation occur in the middle of the paragraph, which is then quoted through to the end. It reads in full: "Solche allgemeine Erkenntnisse nun, die zugleich den Charakter der innern Notwendigkeit haben, müssen von der Erfahrung unabhängig, vor sich selbst klar und gewiß sein; man nennt sie daher Erkenntnisse a priori: da im Gegenteil das, was lediglich von der Erfahrung erborgt ist, wie man sich ausdrückt, nur a posteriori, oder empirisch erkannt wird." (A2). Judging by the page numbers, the edition Hilbert is using is that prepared by Karl Kehrbach (1878), where this passage appears on p. 35, and the following passage on p. 39.

[IV] A6f.=B10f.

[V] This is indeed (except for the first three words) a quotation from the first paragraph of § 2 of the Prolegomena, which, in the Akademieausgabe, reads: "Metaphysische Erkenntniß muß lauter Urtheile a priori enthalten, das erfordert das Eigenthümliche ihrer Quellen. Allein Urtheile mögen nun einen Ursprung haben, welchen sie wollen, oder auch ihrer logischen Form nach beschaffen sein, wie sie wollen, so giebt es doch einen Unterschied derselben dem Inhalte nach, vermöge dessen sie entweder blos erläuternd sind und zum Inhalte der Erkenntniß nichts hinzuthun, oder erweiternd und die gegebene Erkenntniß vergrößern; die erstern werden analytische, die zweiten synthetische Urtheile genannt werden können." (AA IV, p. 266).

17 das] Before this word, the letters "so" have been written, but struck out.

19 apriori] Before this word, the letters "syn" have been written, presumably as the first letters of "synthetisch", but struck out.

20 analytisch] Before this word, the word "synthetische" has been writte, but struck out.

30 oder eine gegebene Gruppe von Urteilen] Before these words, the words "nach dem eben gekennzeichneten Gesichtspunkten" have been written, but struck out.

zur Unterscheidung; nämlich I synth. apriori II synthetisch aposteriori III analytisch apriori IV analytisch aposteriori von diesen kommt jedoch die letzte Kombination[B] (IV) als in sich widerspruchsvoll in Wegfall, weil der obigen Definition gemäß in einem analytischen Urteil der Prädikatsbegriff schon vorher mit dem Subjektsbegriff verknüpft gedacht wird, der Gehalt eines solchen Urteils daher nicht von der Erfahrung abhängen kann, sondern vielmehr a priori gewiß ist. Eine weitere Reducktion der Anzahl obiger Urteilsklassen erweist | als nicht statthaft. 1′v

Nach diesen Vorbemerkungen[C] nehmen wir unsere Hauptaufgabe in Angriff, nämlich die Behandlung der Frage, in welche von den 3 obigen Urteilsklassen die arithm. Sätze hingehören. Kant selber entscheidet sich unbedenklich für die sy. ap. Natur der †arithm† Sätze und er seine Meinu[ng?] in § 2 der Prolegomena durch folgende Worte erhärtet: "Zuvorderst muß bemerkt werden, daß eigentliche mathematische Sätze jederzeit Urteile a priori und nicht empirisch sind, weil sie Nothwendigkeit bei sich führen, welche aus der Erfahrung nicht abgenommen werden kann."[VI] — — Man sollte anfänglich wohl denken (pag 19)[VII]

und ferner Kritik d. r. Vernunft in dem Abschnitte von den Axiomen der Anschauung

[B] In the right margin here something that may be a date has been written: "5 †Stunde [?] Monat [?]† $\frac{1}{4}$".

[C] The rest if this paragraph, down to the word "finden könnten", has been written on a separate piece of paper and glued onto the sheet; it appears that at least one line of text written on this separate paper has been cut off at the top, of which only some strokes reaching far below that line are still visible. It may be glued over something of which the word "finden könnten", which appear immediately after the paper, but do not connect with what appears on the paper, are a remainder.

[VI] This is indeed a quote from Kant's *Prolegomena*, § 2. AA IV, p. 268.

[VII] Although there are no quotation marks, these words are, in fact, the first words of the next paragraph in § 2 of the *Prolegomena* (AA IV, pp. 268f.): "Man sollte anfänglich wohl denken, daß der Satz $7 + 5 = 12$ ein blos analytischer Satz sei, der aus dem Begriffe einer Summe von Sieben und Fünf nach dem Satze des Widerspruches erfolge. Allein wenn man es näher betrachtet, so findet man, daß der Begriff der Summe von 7 und 5 nichts weiter enthalte, als die Vereinigung beider Zahlen in eine einzige, wodurch ganz und gar nicht gedacht wird, welches diese einzige Zahl sei, die beide zusammenfaßt. Der Begriff von Zwölf ist keinesweges dadurch schon gedacht, daß ich mir blos jene Vereinigung von Sieben und Fünf denke; und ich mag meinen Begriff von einer solchen möglichen Summe noch so | lange zergliedern, so werde ich doch darin die Zwölf nicht antreffen. Man muß über diese Begriffe hinausgehen, indem man die Anschauung zu Hülfe nimmt, die einem von beiden correspondirt, etwa seine fünf Finger oder (wie Segner in seiner Arithmetik) fünf Punkte, und so nach und nach die Einheiten der in der Anschauung gegebenen Fünf zu dem Begriffe der Sieben hinzuthut. Man erweitert also wirklich seinen Begriff durch diesen Satz $7 + 5 = 12$ und thut zu dem ersteren Begriff einen neuen hinzu, der in jenem gar nicht gedacht war, d. i. der arithmetische Satz ist jederzeit synthetisch, welches man desto deutlicher inne wird, wenn man etwas

32 nämlich] Before this word a Roman number "I" has been written, but struck out.

32 II synthetisch aposteriori] Before these words, the words "II analytisch apriori III" have been written, but struck out.

33 kommt] Initially, the manuscript had "füllt", which has been struck out and replaced above the line with "kommt".

37 nehmen wir] Before these words, the words "den Ste" have been written, but struck out.

38 in welche] The manuscript originally had "welche", which has been struck out and replaced above the line with "in welche".

38 hingehören] The mansuscript originally had "zugehörig sind", which has been struck out and replaced above the line by "hingehören".

39 in § 2 der Prolegomena] Before the word "in" the words "durch die folge Worte" have been written, but struck out. Before "§ 2" the words "den Wor" have been written, but struck out.

39 durch folgende Worte] These words have been added above the line with an insertion mark.

"Daß $7 + 5 = 12$ sei, ist kein analytischer Satz, denn ich denke weder in der Vorstellung von 7, noch von 5 noch in der Vorstellung von der Zusammensetzung beider die Zahl 12"[D]

"— — Mathematik muß alle Begriffe zuerst in der Anschauung darstellen, ohne welche es ihr unmöglich ist, einen Schritt zu thun, solange ihr nämlich reine Anschauung fehlt, in welcher der Stoff zu synthetischen Urteilen a priori gegeben werden kann — — Arithmetik bringt selbst ihre Zahlbegriffe durch successive Hinzusetzung der Einheiten in der Zeit zu Stande — —"[VIII]

Die bisher citirten Worte bringen die Lehre von der Synthesis a priori der arithmetischen Urteile und ihre Begründung durch Kant im Wesentlichen vollständig zum Ausdruck. Wir wollen unter den Einwendungen gegen diese Kantsche Auffassung zunächst diejenigen Argumente ins Auge fassen, welche sich gegen die behauptete Apriorität der arithmetischen Urteile wenden.

Nach Stuart Mill stammt alles Wissen ausschließlich aus der Erfahrung. Diesem fundamentalen Vorurteile entsprechend lehrt derselbe in seiner induktiven Logik auch von den Definitionen der Zahlbegriffe sowie von den Rechnungsarten, daß sie allein aus der "beobachteten Thatsache" ihre Berechtigung u Anwendbarkeit entlehnen[IX].[E]

[D] Here "(pag. 161)" has been written, but struck out. The reason for this deletion is unclear, especially since the passage quoted here is indeed found on p. 161 of Kehrbach's edition; according to the now standard citation-conventions it is found on A164/B205.

[E] At the end of the page, the words "Um Kants" have been written, but struck out. This is presumably an alternative start of the

größere Zahlen nimmt; da es denn klar einleuchtet, daß, wir möchten unsern Begriff drehen und wenden, wie wir wollen, wir, ohne die Anschauung zu Hülfe zu nehmen, vermittelst der bloßen Zergliederung unserer Begriffe, die Summe niemals finden könnten.". Judging by this page reference, the edition of the *Prolegomena* Hilbert used was that prepared by Rosenkranz and Schubert as volume 3 of their edition of Kant's complete works (1838), where this paragraph appears on p. 19.

[VIII] This quotation is taken from § 10 of the *Prolegomena*. It reads in full: "Nun sind Raum und Zeit diejenigen Anschauungen, welche die reine Mathematik allen ihren Erkenntnissen und Urtheilen, die zugleich als apodiktisch und nothwendig auftreten, zum Grunde legt; denn Mathematik muß alle ihre Begriffe zuerst in der Anschauung und reine Mathematik in der reinen Anschauung darstellen, d. i. sie construiren, ohne welche (weil sie nicht analytisch, nämlich durch Zergliederung der Begriffe, sondern synthetisch verfahren kann) es ihr unmöglich ist, einen Schritt zu thun, so lange ihr nämlich reine Anschauung fehlt, in der allein der Stoff zu synthetischen Urtheilen a priori gegeben werden kann. Geometrie legt die reine Anschauung des Raums zum Grunde. Arithmetik bringt selbst ihre Zahlbegriffe durch successive Hinzusetzung der Einheiten in der Zeit zu Stande, vornehmlich aber reine Mechanik kann ihre Begriffe von Bewegung nur vermittelst der Vorstellung der Zeit zu Stande bringen." (AA IV, p. 283; p. 38 in the edition of Rosenkranz and Schubert).

[IX] The views ascribed to Mill in this paragraph are likely extrapolated from § 5 of chapter 24 in book III ("On Induction", which presumably underlies the reference to Mill's "induktive Logik") of Mill's *System of Logic*, which was translated into German by Jacob Schiehl (the relevant, second volume (containing the second half of book III ("Von der Induktion (Fortsetzung)", starting with chapter 14, through book VI) of the translation appeared in 1863 in a second edition). Mill says, among other things (in Schiehl's translation, p. 155) "Die in der Definition einer Zahl behauptete Thatsache ist eine physikalische Thatsache. Eine jede der Zahlen eins,

45 von der Zusammensetzung] Before these words, the letters "be", presumably as the first letters of "beide", have been written, but struck out.

54 stammt] Before this word, the word "ist" has been written, but struck out.

54 Diesem] Before these words, the words "Dann entsprich" have been written, but struck out.

56 aus] This word has been added above the line with an insertion mark.

| 2r

Nach Kantscher Ausdrucksweise würde damit nichts andres als die Aposteriorität der arithmetischen Sätze behauptet sein. Um diese Ansicht Stuart Mills als unhaltbar zu erkennen bedarf es nur einer Ausführung und scharfen Betonung der Kantschen Argumente. Es sind nämlich die arit. Urteile allerdings von derart, daß sie durch die Erfahrung bestätigt werden können. Aber niemand wird darauf bestehen, daß sie diesen Bestätigung zu ihren Beweise notwendig bedürfen. In letztren Falle müßte auf eine arithmetische Formel mit größeren Zahlwerten etwa $111 + 2222 = 3333$ sich nicht an das als richtig erkennen lassen, als dadurch, daß wir gleichsam wie in einem physikalischem Experiment soviel Dinge wirklich mit einander vereinigen und abzählen. Ferner würde aus der Erfahrung allein nur folgen, daß die gefundenen Zahlenurteile ausschließlich für die gerade benutzten Gegenstände

paragraph which starts on the following page with "Nach Kantscher Ausdrucksweise", meant to be: "Um Kants Ausdrucksweise zu gebrauchen" or something like this.

zwei, drei, vier u.s.w. bezeichnet physikalische Phänomene und mitbezeichnet eine physikalische Eigenschaft dieses Phänomens. Zwei z.B. bezeichnet alle Paare von Dingen, und zwölf alle Dutzende von Dingen, indem es mitbezeichnet, was sie zu Paaren, Dutzenden macht; und das, was sie dazu macht, ist etwas Physikalisches, da es nicht zu leugnen ist, dass zwei Aepfel von drei Aepfeln, zwei Pferde von einem Pferd physikalisch verschieden sind, dass sie ein davon verschiedenes sichtbares und fühlbares Phänomen sind." and furthermore (p. 158): "Was die Arithmetik zum Typus einer deductiven Wissenschaft macht, ist die glückliche Anwendbarkeit auf dieselbe von einem so umfassenden Gesetze, wie 'die Summen von Gleichem sind gleich,' oder (um dasselbe Princip in einer weniger familiären aber charakteristischeren Sprache auszudrücken) 'was aus Theilen zusammengesetzt ist, ist aus Theilen von diesen Theilen zusammengesetzt'. Diese Wahrheit, welche in allen Fällen, wo die Entscheidung den Sinnen unterworfen werden kann, so einleuchtend ist, und die so allgemein ist, dass sie sich so weit erstreckt, als die Natur selbst, diese Wahrheit, da sie von allen Naturerscheinungen gültig ist (denn alle können gezählt werden), muss als eine inductive Wahrheit oder als ein Naturgesetz von der höchsten Ordnung betrachtet werden. Eine jede arithmetische Operation ist eine Anwendung dieses Gesetzes oder von anderen Gesetzen, die daraus abgeleitet werden können. Dies ist bei allen Rechnungen unsere Gewähr."; in the latest, critical edition of Mill's logic (*A System of Logic Ratiocinative and Inductive, Being a Connected View of the Principles of Evidence and the Methods of Scientific Investigation* (Books I-III). Ed. John M. Robson, Introduction by R.F. McRae (*The Collected Works of John Stuart Mill*, Volume VII). University of Toronto Press, 1974.), these passages are found on p. 610 and p. 613 respectively.

58 würde] Before this word, the words "bei" and "anzuwenden so" have been written, but struck out.

59 Ausführung und] These words have been added above the line with an insertion mark.

60 der Kantschen Argumente] Before these words, the words "und Ausführung" have been written, but struck out.

61 zu] The manuscript originally had "zum", but the "m" has been struck out.

62 ihren] This word has been added above the line with an insertion mark.

62 notwendig] Before this word, the words "ihrer Rich" have been written, but struck out. The original phrasing was probably "zum Beweise ihrer Richtigkeit".

64–68 der Erfahrung allein nur folgen, daß die gefundenen Zahlenurteile ausschließlich für die gerade benutzten Gegenstände ferner ausschließlich unter der gewählten Anordnung und zu der bestimmten Zeit das Versuchs, nicht aber allgemein und notwendig gültig seien. Strenge Allgemeinheit und innere Notwendigkeit sind aber sichere Kennzeichen für Urteile a priori und diese Kennzeichen kommen unseren arithmetischen Urteilen in vollsten Maße zu] This portion of the text is written on different paper, which has been glued onto the sheet, and in much more mannered handwriting, so it may not have been written by Hilbert.

ferner ausschließlich unter der gewählten Anordnung und zu der bestimmten Zeit das Versuchs, nicht aber allgemein und notwendig gültig seien. Strenge Allgemeinheit und innere Notwendigkeit sind aber sichere Kennzeichen für Urteile a priori und diese Kennzeichen kommen unseren arithmetischen Urteilen in vollsten Maße zu

Dies ist zugleich die Gelegenheit, die Kantsche Lehre gegen die völlig unzutreffenden Einwendungen in Schutz zu nehmen, welche von Frege erhoben sind. Derselbe sagt in den "Grundlagen der Arithmetik" pag 6 | Kant will die 2v Anschauung von Fingern zulassen"— — Haben wir denn überhaupt Anschauung von 135664 Fingern einleuchten — — aber das ist nicht der Fall. Kant hat offenbar nur kleine Zahlen †gemeistert†"[X]

Zunächst sei hervorzuheben, daß die Zuhülfenahme der Anschauung keineswegs die apriorische Eigenschaft der arithmetischen Urteile in Frage stellt, vielmehr im Gegentheil einige der von Kant erwiesenen Apriorität der Raum- u Zeitanschauung gerade für die Apriorität jener Urteile eine natürliche Erklärung bieten. Noch weniger wird die Kantsche Lehre durch die Einwendungen getroffen, welche sich gegen den angeblich von Kant beabsichtigte prinzipielle Unterscheidung zwischen Zahlformeln für große u solche für kleinen Zahlen richten. Die Frage "Haben wir denn überhaupt eine Anschauung von 135664 Fingern oder Punkten?" ist im Grunde vollkommen gegenstandslos. Es kommt für den Kernpunkt der Frage nicht darauf an, wie viele Gegenstände wir gleichzeitig im Raum deutlich von einander getrennt auffassen können sondern darauf, daß wir überhaupt zum Verständniß arithmetischer Begriffen und Operationen der Raume u Zeitanschauung nothwendig bedürfen, und in der That gleichviel ob es sich um einfache Zahlformeln oder um schwierige algebraische oder für mathematisch Probleme handelt, in jedem Falle ist

[X] This is a indeed a quotation from p. 6 of Frege's book. In full, the passages read: "Kant will die Anschauung von Fingern oder Punkten zu Hilfe nehmen, wodurch er in Gefahr geräth diese Sätze gegen seine Meinung als empirische erscheinen zu lassen; denn die Anschauung von 37863 Fingern ist doch jedenfalls keine reine. Der Ausdruck 'Anschauung' scheint auch nicht recht zu passen, da schon 10 Finger durch ihre Stellungen zu einander die verschiedensten Anschauungen hervorrufen können. Haben wir denn überhaupt eine Anschauung von 135664 Fingern oder Punkten? Hätten wir sie und hätten wir eine von 37863 Fingern und eine von 173527 Fingern, so müsste die Richtigkeit unserer Gleichung [$135664 + 37863 = 173527$] sofort einleuchten, wenigstens für Finger, wenn sie unbeweisbar wäre; aber dies ist nicht der Fall. Kant hat offenbar nur kleine Zahlen im Sinne gehabt.".

68 a priori] For some reason the word has been struck out and replaced above the line in different handrwriting (neither of which resembling Hilbert's) once again by "a priori".

68 in vollsten Maße zu] Before these words, which are written in a different handwriting than the one they replace, the word "zu" has been written, but struck out.

78 im Grunde] Before these words the worde "für die" have been written, but struck out.

79 den Kernpunkt der Frage] These words have been added above the line with an insertion mark; before "Frage" the word "Sache" has been written, but struck out. On the line where they are meant to be inserted, the word "uns" has been written, but struck out.

79 deutlich] This word has been added above the line with an insertion mark.

81 der Raume u Zeitanschauung] These words have been added above the line with an insertion mark.

82 Probleme] Before this word, the word "Entwicklungen" has been written, but struck out.

82–83 in jedem Falle ist es] These words have been added above the line with an insertion mark.

es die sinnliche Raum u Zeitanschauung, welche allein das rechende Zählen d. i. das Denken an der Hand u auf Grund von Vorstellungen erst ermöglicht. | 2'r

Tiefer im Wesen der Sache begründet erscheinen die von Ueberweg in seiner "Geschichte der Philosophie" bei Gelegenehit der Besprechung der Kantschen Philosophie erhobenen Bedenken, welche sich gegen den schon citirten u benutzten Kantschen Satz von der Nothwendigkeit u strengen Allgemeinheit als sichere Kriterium einer apriorischen Erkenntnis richten. In ebenjenem Satze erblickt nämlich Ueberweg das [proton pseudos] in dem gesammten Lehrgebäude des Kritizismus und glaubt schon an dem allgemeingültigen und doch aus der Erfahrung geschöpften Newtonschen Gravitationsionsprinzipe ein zureichendes Zeugnis gegen Kant zu besitzen. Gerade das von Ueberweg selber angeführte Beispiel soll uns dazu dienen, nun sein Einwurf als voreilig zurückzuweisen. Einerseits ist es nämlich nicht ersichtlich, wie die Gültigkeit des Newtonschen Gesetzes einer inneren Nothwendigkeit gleichkommen soll, da doch jenes Gesetz ausschließlich als das Resultat von Versuchen u Beobachtungen anzusehen u daher bereits seine †…†barkeit an die Voraussetzungen des Versuchs u die Bedingung der Erfahrung geknüpft ist. Als innere Nothwendigkeit erscheint nicht einmal die Thatsache daß sich Körper überhaupt anziehen, nach jenen Gesetzen anziehen. Nur eine falsch verstandene Philosophie stellt sich die Aufgabe, die von dem Denken der Physiker entdekten Naturge-

83 welche allein] Before these words, the words "allein ist es" have been written, but struck out.

83 das] Before this word, the word "und" has been written, but struck out.

83 d. i.] The manuscript originally had (perhaps) "wobei", which has been struck out and replaced above the line with "d.i.".

85 begründet erscheinen] The manuscript originally had "liegend ist", which has been struck out and replaced above the line with an insertion mark with "begründet erscheint".

86 erhobenen Bedenken] Before these words, the words "besprochenen Bednken" have been written, but struck out.

86 welche sich gegen den] The manuscript originally had "Daß wird der", which has been struck out and replaced above the line by "erblickte Ueberweg in dem", which has also been struck out and replaced below the line with "welche sich gegen den".

87 von der] The manuscript originally had a comma after "Satz" and, following the comma, the words "nach welchem", which have been struck out and replaced by the words "von der" inserted above the line at the start of the following line.

87 als] This word has been struck out and written again above the line.

87–88 apriorischen] The manuscript originally had "nicht empirischen", which has been struck out and replaced above the line by "apriorischer".

88 richten] Before this word, the words "gelten des [proton pseudos] in der gesammten Lehrgebäude des Kritizismus" have been written, but struck out.

88 ebenjenem] The manuscript originally had "diesem", which has been struck out and replaced above the line by "ebenjenem".

88 nämlich] This word has been added above the line with an insertion mark.

89 allgemeingültigen] Before this word, the word "streng" has been written, but struck out.

91 uns] Before this word, the word "dazu" has been written, but struck out.

91 als] Before this word, the word "zu" has been written, but struck out.

93–94 bereits seine †…†barkeit an die Voraussetzungen des Versuchs u die Bedingung der Erfahrung geknüpft ist.] The manuscript originally had "vollkommen empirischen Unterfangens ist.", which has been struck out and replaced above the line (running into the right margin).

setze nachträglich als Ausfluß der sogenannten inneren Nothwendigkeit hinzustellen. Derartige Bestrebungen haben stets, wie die Geschichte lehrt zu den haltlosesten, u einfältigsten Konsequenzen geführt. Als weiteres Moment kommt noch der begründete Zweifel an der strengen Richtigkeit des Newtonschen Gesetzes hinzu, indem desselben vielleicht nur eine erste Annährung an ein strengeres und allgemeineres Gravitationsgesetz darstellt, dessen Kenntnis uns bisher wegen ungenügender experimenteller Hülfsmittel verschlossen ist. Wo bleiben dann die innere Nothwendigkeit des Newtonschen Prinzipes? Ebenso be|denklich ist die strenge Allgemeinheit des Gesetzes. Die 2'v
elektrischer Massen (Fluide) gehorchen z. B. einem anderen Gesetze, welches allerdings als Erweiterung des Newtonschen Gravitationsgesetzes gelten könnte. Dazu sogar an einigen Himmelskörper, nämlich den Kometschweifen sind Abweichungen vom Newtonschen Gesetzes [sic] beobachtet worden. Zudem bleibt es noch abzuwarten ob das Gravitationsgesetz nicht einem noch höheren und noch fundamentaleren Prinzip wie etwa dem Prinzip der Undurchdringlichkeit der Materie untergeordnet ist, welch letzteres dann nur durch einseitige Spezialisierung für eine gewisse Gruppe von Erfahrungen zu jenen Newtonschen Gesetze hinleitet, während dasselbe vielleicht in seiner Anwendung auf eine z.B. †…†und chemische Erscheinung vollkommen von jenem verschieden Anziehungsgesetze bedingt.

Die angestellten Ueberlegungen zeigen, wie wenig Ueberweg berechtigt war, die empirisch erkannten u bedingte Gültigkeit des Newtonschen Gesetzes mit der innern Notwendigkeit u strengen Allgemeinheit der arithmetischen [?] Wahrheiten auf gleiche Stufe zu erheben. Solche Sätze[1] wie das Newtonsche Gravitationsgesetz sind vielmehr aposteriorisch. Die Kantschen Kriterien apriorische Urteile nämlich innere Notw u strenge Allgemeinheit treffen für dieselben keineswegs zu und das von Ueberweg ausgesprochene Bedenken wird damit hinfällig. | 3r

[1] Auch die geometrischen Sätze gehören meiner Ansicht nach hierher, als die Eigenschaften des [The manuscript originally had "nach Riemann"] unsrigen Raums im Unterschied zu [The manuscript originally had "von", which has been struck out and replaced above the line with "im Unterschied zu".] andern denkbaren Mannigfaltigkeiten nur aus der Erfahrung entnommen werden können vgl. Riemanns gesammelte Werke über die Hypothesen, welche der Geometrie zu Grunde liegen. Hiernach dürfte die Kantsche Behauptung der apriorischen Natur der mathem. Urteile auf die arithm. einzuschränken sein.

97–98 haben stets] Before these words, the words "sind in der unnatürlichsten Weise" have been written, but struck out.

99 der] The manuscript originally had "das", which has been struck out and replaced above the line by "der".

99 Zweifel] Before this word, the word "Bedenken" has been written, but struck out.

100 strengeres] The manuscript originally had "strenges un", but the letters "es un" have been struck out, and the new ending "eres" has been written behind them.

101–102 innere Nothwendigkeit] Before these words, the word "sogenannte" has been written, but struck out.

105 abzuwarten] The manuscript origianlly had "unentschieden", which has been struck out and replaced above the line.

109 von jenem] These words have been added under the line with an insertion mark.

110 wie wenig Ueberweg berechtigt war] Before these words, the words "das Ueberweig nicht" have been written, but struck out; before the word "war" the word "ist" has been written, but struck out.

113 nämlich innere Notw u strenge Allgemeinheit] The manuscript originally had just "nämlich", which has been struck out and replaced above the line by the words "nämlich innere Notw u strenge Allgemeinheit".

115 Weit entschiedener u. mit scheinbar großer Berechtigung als die Apriorität ist die synthetische Natur der math. Urteile bestritten worden. Die Vertreter erkennen die Apriorität der mathe. Urteilen an aber nur auf Grund ihrer angeblich analytischen Natur. [etwas gestrichen] So erklärt unter anderen Zimmermann in seiner Schrift "Ueber Kants mathematisches Vorurteil und dessen Folgen" das nach Kant synthetische Urteil $7+5=12$ für einen vollkommen identischen und daher analytischen Satz. Er giebt zu, daß man, um 7 und 5 zu vereinigen, sowohl
120 über den Begriff von 7 als auch über den von 5 Lange pag 25 schlechthin identisch[XI]. Kant selber hat bereits einer derartig mißverständlichen Auffassung seiner Lehre vorzubeugen gesucht, indem er in d[er] K[ritik] d[er] r[einen] V[ernunft] an der oben citirten Stelle in Klammern die Worte beifügt:

"__"[XII]

Zimmermann verlegt gewissermassen die synthetischen Bestandteile unseres Satzes in den Subjektbegriff $5+$
125 7[2], in dem er zum Verständnis derselben das Hinausgehen über den Begriff 7 und 5 verlangt, dann aber den mathematischen Satz als eine bloße Modifikation des identischen Satzes $12=12$ mithin als ein analytisches Urteil aufführt. Die Deutung, welche er damit dem einfachen Satz $7+5=12$ verleiht, scheint uns künstlich und gezwungen. Jedenfalls sieht man, daß durch solch spitzfindige †Streitigkeiten† wie die von Zimmermann ausgeführten die synthe Natur unser nicht im mindesten in Frage gestellt wird, zumal die Einteilung der Urteile in anal. u synthetische

[2]Diesen Versuch, die synthetische Erkenntnis des Urteils in seinen Subjektbegriff hineinzuziehen und dadurch zu versteken, erkennen wir [gestrichen: auch] in dem weiter unten folgenden Angriff gegen die Kantsche Lehre wieder vgl pag [The page reference was either never added or has been destroyed by damage to the paper. The remainder of this footnote is exceedingly difficult to make out due to the crammed handwriting and the damage to the paper; indeed, some of it is simply missing.] die Bedenken [something added above the line] gegen die Bestimmtheit u Objektivität des Unterschiedes zwischen [at least one word missing due to damage to paper] a. u. s. Urteilen [unreadble word of 4-5 letters] garnicht sind

[XI]The explanation for the page-reference to Lange in the middle of this sentence is that this sentence is taken almost verbatim from (the second volume) of F.A. Lange's *Geschichte des Materialismus*. Lange's own words about Zimmermann are as follows: "*Zimmermann* behauptet, das Urteil $7+5=12$, welches Kant für synthetisch erklärt, sei nicht nur analytisch, sondern sogar identisch. Er will zugeben, daß man, um 7 und 5 zu vereinigen, sowohl über den Begriff von 7 als auch über den von 5 hinausgehen müsse, aber damit erhalte man noch nicht das Urteil, sondern bloß den Subjektsbegriff $7+5$. Mit diesem aber sei das Prädikat 12 schlechthin identisch.".

[XII]The reference is presumably to the last passage quoted from the *Kritik der reinen Vernunft*, i.e. A164/B205, where, immediately following the part quoted earlier, one indeed finds a lengthy parenthesis, which read: "(daß ich diese [die Zahl 12] in der *Addition beider* [i.e. 5 and 7] denken solle, davon ist hier nicht die Rede; denn bei dem analytischen Satze ist nur die Frage, ob ich das Prädikat wirklich in der Vorstellung des Subjekts denke)".

115 die Apriorität] Before these words, the words "die Behauptung" have been written, but struck out.

116 Die Vertreter] These words have been struck out, but then seemingly reinstated by a wobbly line drawn underneath them; in any case, no replacement has been added, so these words remain the only available grammatic subject for the sentence.

122 oben citirten] Before these words the letters "be" were written, perhaps as the first letters of "bereits", but struck out.

122 Worte] Before this word, the word "klaren" has been written, but struck out.

128 ausgeführten] Before this word, the letters "vorg" and the letter "b" has been written, but struck out.

129 unser] Before this word, two letters have been written, but struck out.

nicht auf der formalen Wortschaffung u =deutung, sondern, wie Kant selbst beton[t], auf dem gedanklichen Inhalte des Urteils beruht.[F]

Mit der Zimmermannschen Auffassungsweise im Wesentlichen übereinstimmend erscheint uns derjenige Standpunkt, welchen Ueberweg in seiner bezüglichen Anmerkung seiner Geschichte der Philosophie bei Besprechung der K. d. r. V. und mit besonderer Ausführlichkeit und Entschiedenheit Frege in der neuerdings erschienen Abhandlung "Die Grundlagen der Arithmetik"[XIII] vertritt. Ueberweg sucht die Zahlformeln aus den Definitionen des Zahlensystemes u die Rechnungsoperationen in analytischen Schlußfolgen abzuleiten, wobei er jedoch übersah, daß die Zulässigkeit u das Verständnis jener Definitionen ausschlißlich auf der Anschauung beruht. Aus demselben Grunde konnte das auch Frege trotz des Aufwandes der künstlichsten u complizirtesten Modifikationen [?] nicht gelingen, die Raum u Zeitanschauung ans der Arithmetik wegzuschaffen oder durch spitzfindige, der formalen Logik entlehnte Konstruktionsmittel zu ersetzen. Eine Widerlegung der Fregeschen Betrachtungen im Einzelnen wird doch nur auf die Widerholung der bereits im bisherigen zur Genüge herausgehobenen und für die Kantsche Lehre geltend gemachten Argumente hinauslaufen. 4'r

Schließlich nöthigen uns auch die von Hegel in seiner Logik gegen die Kantsche Lehre geltend gemachten Gesichtspunkte zu einer kurzen Entgegnung. Hegel giebt nämlich in den Theorieen der höheren Algebra und Analysis, keineswegs aber in den ihm vielmehr identisch erscheinenden Satz $7+5=12$ eine Synthesis zu. $7+5=12$ ist nach

[F] There is a mark in blue crayon here, and another in the right margin, next to which has been written (also in blue crayon) "gr. Einschub". This is presumably a reference to the text written on pages 4′r-4′v, where the same mark in blue crayon can be found in the top left corner of the page with "Absatz" written next to it, and the discussion of Zimmermann continues. The text, inserted now in its intended position in the discussion, is as follows.

[XIII] That Frege's book is here referred to as "recently published" may well be the best datum for dating this manuscript to the mid 1880s, i.e. to 1884 or 1885.

130 wie] Before this word, the word "auf" has been written, but struck out.

134 K. d. r. V.] Before this abbreviation, the letters "Kan" have been written, but struck out.

134 der] These words have been added above the line with an insertion mark. Below them a word of approximately 4 letters has been struck out so as the be illegible; it may be "einer".

137 die Zulässigkeit] Before thes words, the same words have been written once already, but struck out.

139 der formalen Logik] Before these words, the word "Konstruktionsmittel" has been written, but struck out.

143 gegen die Kantsche Lehre] Before these words, the words "in polemischer Weise" have been written, but struck out.

144 giebt] The manuscript originally had "erkannte", which has, however, been struck out and replaced by "giebt" above the line with an insertion mark.

144 Theorieen] The manuscript originally had "schwierigeren Fragen", which has been replaced above the line with "Theorieen". Before "schwierigere", the letter "d" has been written, but struck out; and before that letter, the words "höhere Fragen" have been written, but also struck out.

144 höheren Algebra und Analysis] Before these words, the word "Analysis" has been written once already, but struck out.

ihm überhaupt kein Satz der eines Beweises bedarf, sondern die Lösung einer Aufgabe, in welcher die Bestimmung der Summe der beiden Zahlen 7 und 5 verlangt wird. Da diese Bestimmung keine neue Begriffsbildung erfordert vielmehr auf der blos rechnerische außerlichen Thätigkeit des Weiterzählens beruht, so hält Hegel jene Zahlformel | 4'v
für rein analytisch. Zur Widerlegung genügen einige kurze Andeutungen. Ob jene Zahlformeln wie etwa $7+5=12$
150 Sätze oder Rechnungsresultate heißen, ist eine rein formale u daher für uns bedeutungs[los]e Frage. Sätze müssen bewiesen, Rechnungsresultate abgeleitet werden; in beiden Fällen ist die Zuhülfenahme der Anschauung unerläßlich. Mag das die Thätigkeit "durch 5 bestimmte Fortzählens von 7 an" noch so "gedankenlos, äußerlich, mechanisch" erscheinen, die bloße Anwendung der formalen Logik reicht zur Ermöglichung dieser Thätigkeit nicht aus. Die Zeit u Raumanschauung[3] ist †…†. Wie bereits oben ungedeutet, ging jedoch Hegel nicht so weit, auch in Theorien

[3]Die Verläugnung und Herabsetzung der "begrifflichen Rechnung" ist [struck out: Hege überhaupt] meiner Ansicht nach überhaupt Hegels †…†, welche ihn [gestrichen †…†] (vgl. seine Polemik gegen Newton 302) †…†(vgl. seine dreiste Aburtheilung über die bisherige Begründung der Differential u Integralrechnung pag 272, 278) und unterschiedlichen (vgl. die †…†pag 375) Behauptungen verbietet.

146 der eines Beweises bedarf] These words have been written in the margin and placed here by means of an insertion mark. Before the word "der" the word "welcher" has been written, but struck out.

146 welcher] Before this word, the words "nämlich der Aufgabe" have been written, but struck out.

147 wird] Between "verlangt" and "wird" 4 lines of text have been written and struck out. They read "†…†Bestimmung [above the line: †…†] ein die †…†, so hält Hegel jene [struck out: Fr] †…†".

147 keine] The manuscript originally had "weder", which has been struck out and replaced above the line.

148 vielmehr] Before this word, the words "noch irgend eine" have been written, but struck out.

148 der] The manuscript originally had "einer", which has been struck out and replaced above the line with "der".

148 außerlichen Thätigkeit] Before the words, the words "Thätig selbstständi" have been written, but struck out.

148 jene Zahlformel] Before these words, the word "je Zahl" have been written, but struck out.

149 Zur Widerlegung] Before these words, the words "Als An" have been written, but struck out.

150 Rechnungsresultate] Before this word, the word "Aufgabelösung" has been written, but struck out.

150 heißen] Before this word, the word "zu nenn" has been written, but struck out.

150 eine rein formale u daher für uns bedeutungs[los]e] Before these words, the words "formal u daher" have been written, but struck out.

151 abgeleitet] Before this word, the word "gefunden" has been written, but struck out.

151 in beiden Fällen] Before these words, the words "zu Beidem" and "für Beide" have been written, but struck out.

152 das] Before this word, the word "für Hegel" have been written (where "für" has been added above the line with an insertion mark), but struck out.

152 die Thätigkeit] These words have been added above the line.

152 durch 5] Before this word, the word "Fortzählen" has been added, but struck out.

153 zur] Before this word, the word "dazu" has been written, but struck out.

154 Zeit u Raumanschauung] Before these words, the words "Un†…†lichkeit der", but "Un†…†lichkeit" has been struck out. "der" has not been struck out, but makes no sense without the preceding noun to which it adds a genitive attribute.

154 in] The manuscript originally had "für", which has been struck out and replaced above the line by "in".

der höheren Analysis jede Synthesis zu leugnen, weil er die Annahme für †…† hält, und jener vermeintlichen Identitäten der Rechenkunst überhaupt auf rein analytische Weise eine †…† Wissenschaft voll †…†können. Aber diese †…†vollkommen richtigen Punkte †…†nun für Hegel ein Anlaß zu einer neuen †…†, indem er das Prädikat analytisch auf eine †…†, dagegen allen solchen †…†die synthetische Eigenschaft beimaß, bei deren Entwicklung †…†, un†…†oder in der Problemstellung unmittelbar nicht gegebenen Verhältnisse u Bestimmungen nothwendig sind. |[G]

Lange, in diesen Fragen ein sehr eifriger Vertreter des Kantschen Standpunktes, bespricht in seiner Geschichte 3r
des Materialismus ebenfalls den Zimmermannschen Einwand, antwortet jedoch auf denselben nur damit, dass er ein ihm besonders stark erscheinendes Argument, für die synthetische Natur der a. Urteile den Zimmermanschen Ausführungen gegenüberstellt. | 3v

Lange fährt nämlich an der betreffenden Stelle fort:

“Schade, dass Lange pag 25 sind ja identisch!”

Vielleicht würde ein Gegner d. syn. Natur d. arit. Urteile dem letzteren Ausführungen Langes gegenüber insofern eine teilwei [?] Bekehrung erfahren, als er für den unentwickelten Geist des Kindes eine Synthesis in den Zahlformeln zugiebt, dieselbe jedoch mit Rücksicht auf einen durchgebildeten Verstand verwirft.

Damit werden wir auf eine neue gegnerische Ansicht geführt, welcher wohl von Herder zum ersten Male ausge-

[G] This is the end of the long insertion on pp. 4′r and 4′v. The following paragraph is the one which begins immediately after the insertion mark in blue crayon on p. 3r.

155 weil] Before this word, the words “†…†” have been written, but struck out.

155 die Annahme] Before these words, the words “sich sich †…†” have been written, but struck out.

155 †…†] The manuscript originally had “undenkbar”, which has been struck out and replaced above the line by “†…†”.

155 und] Before this word, the words “†…†” have been written, but struck out.

155 vermeintlichen] Before this word, the words “†…†” have been written, but struck out.

156 Rechenkunst] The manuscript originally had “Arithmetik”, which has been struck out and replaced above the line with “Rechenkunst”.

156 †…†] Before this word, the word “†…†” has been written, but struck out.

156 Aber diese] Before these words, the words “†…†” have been written, but struck out.

157 †…†nun für Hegel] Before these words, the words “†…†nun Hegel” have been written, but struck out.

157–158 das Prädikat analytisch auf eine †…†, dagegen] These words have been written in the left margen and inserted here with an insertion-mark.

158 bei] Before this word, the word “welche” has been written, but struck out.

158 deren] Before this word, the words “welche nur” have been written, but struck out.

160 in diesen Fragen] Before these words, the words “wie †…†” have been written, but struck out.

162 ein] Before this word, the words “den Zimmermannschen Ausführungen” have been written, but struck out.

162–163 den Zimmermanschen Ausführungen] These words have been added above the line with an insertion mark.

164 an der betreffenden Stelle] These words have been added above the line with an insertion mark.

167 insofern] This word has been added above the line with an insertion mark.

168 mit Rücksicht] Before these words, the word “für” has been written, but struck out.

sprochen u vertreten ist. Herder erkennt nämlich den Kantschen Einteilungsprinzipe der Urteile in analytische und synthetische Urteile nur eine gerigfügige und †…†Bedeutung zu. In seiner "Metakritik, 1. Kapitel und Einleitung" heißt es

"Was soll die Eintheilung analytischer u synthetischer Urteile? An sich ist jedes Urteil eine Zusammensetzung (Synthesis) des Subjekts u Prädikats. — Will man also diejenigen Urteile, in denen das Prädikat etwas sagt, was nicht sogleich im Subjekt erscheint, synthetisch nennen, wohlan! nur wolle man damit nichts neues gesagt, auch keine wesentliche Einteilung der Urteile gemacht haben, indem dann einen neu sein kann, was dem andern nicht neu ist und einer die Verknüpfung der Begriffe schneller als das andre wahrnimmt. Wir werden also wohl thun wenn wir auf den Unterschied der Analyse u Synthesis bei einzeln Urteilen nicht achten, weil er ungewiß u relativ ist. —"[XIV]

Der hierin ausgesprochenen Auffassung liegt ein vollständiges Mißverständnis der Kantischen Meinung zu Grunde. Achten wir nämlich genau auf den oben citirten Wortlaut der von Kant gegenbenen Definitionen, so finden wir daß Kant ausschließlich diejenigen Urteile zu den analytischen rechnet, deren Prädikatsbegriff im Subjektsbegriff enthalten oder durch Zergliederung desselben hervorgegangen ist. Die analytischen Urteile sind demnach entweder identische Urteile A ist A, oder aus dem Satze des Widerspruches fließen: A ist nicht Nicht-A. Solche Urteile dienen zur Erläuterung oder Definition des Subjektbegriffes, können jedoch nie eine Erweiterung unserer Erkenntnisse | bewirken. Alle anderen Urteile dagegen, deren Prädikatsbegriff außerhalb des Subjektsbe- 3
griffs liegt, erweitern unsere Erkenntniß durch Synthesis und heißen daher synthetische Urteile. Ist nun ein Begriff

[XIV] This is indeed a quote from Herder's Metakritik, though more is omitted from the passage quoted than the dashes used as omission-marks here suggest. The passage from which these sentences are extracted extends over a page and a half (pp. 184f) of Herder's *Metakritik* as presented in volume 18 of *Herders Werke*.

178 wenn wir] These words have been added above the line with an insertion mark.

180 liegt ein] Before these words, the words "müssen wir mit aller Entschiedenheit entgegensetzen".

180 vollständiges] After the prefix "voll" the letters "kom" have been written, presumalby to spell "volkommen", but struck out.

180–181 Meinung zu Grunde] Before these words, the words "Sätzen zu Grunde" have been written, but struck out.

181 von Kant] Before the word "von", the letters "Kan" have been written, presumalby as the first letters of "Kant", but struck out.

184 fließen] After this word, the letters "de" have been written, presumably as an active participle ending, but struck out (even though the active participle would be stilistically preferrable!).

185 oder] Before this word, the words "des Subjektbegriffs" have been written, but struck out.

186–191 bewirken. Alle anderen Urteile dagegen, deren Prädikatsbegriff außerhalb des Subjektsbegriffs liegt, erweitern unsere Erkenntniß durch Synthesis und heißen daher synthetische Urteile. Ist nun ein Begriff durch eine Reihe von Merkmalen definirt und werden wir durch die fortschreitende Entwicklung unserer Erkenntniß dazu geführt, neue Merkmale in diesen Begriff mit aufzunehmen, so ist diese Aufnahme selbst eine Synthesis: die bezüglichen Urteile sind synthetische Urteile und bleiben es auch für denjenigen, welcher auf höherer Bildungsstufe stehend, jenes neu hinzugekommene Merkmal als notwendig mit dem Subjektbegriff verbunden erkannt hat.] This piece of text has been written on different paper and glued onto the sheet, It is also written in *markedly* more mannered handwriting than the rest and thus may not be written by Hilbert; the text in what is presumably Hilbert's handwriting continues immediately after the text on the sheet, seemingly without a break.

durch eine Reihe von Merkmalen definirt und werden wir durch die fortschreitende Entwicklung unserer Erkenntniß dazu geführt, neue Merkmale in diesen Begriff mit aufzunehmen, so ist diese Aufnahme selbst eine Synthesis: die bezüglichen Urteile sind synthetische Urteile und bleiben es auch für denjenigen, welcher auf höherer Bildungsstufe stehend, jenes neu hinzugekommene Merkmal als notwendig mit dem Subjektbegriff verbunden erkannt hat. Auf Grund solcher Vorgänge kann sogar die bloße Behauptung der Existenz des Subjektsbegriffes ein synthetisches Urteil abgeben. Steht es z.B. frei, neben dem Merkmal Ausdehnung noch das der Schwere in den Begriff des Körpers mit aufzunehmen, so ist das Urteil "Ein Körper ist" jedenfalls synthetisch und zwar aposteriori, da uns über die Existenz †…†ausgedehnten u zugleich Schwere allein die Erfahrung belehren kann.

Ueberhaupt kann nicht genug betont werden, daß die Entscheidung für die ana. oder s. Natur eines Urteils ausschließlich von seinem synthetischen Inhalte abhängig ist. Dadurch erklärt sich auch, wie ein Urteil unter Beibehaltung des Wortlautes durch die modificirten Definition seines Subjekts od. Prädikatsbegriffes aus einem analytisch zu einem synthetischen werden kann, Sobald sich damit sein gedanklichr Gehalt ändert. So ist z. B. bei den beide Urteilen

Alle Körper sind ausgedehnt

— schwer |

Das erste an. u das zw s. sobald wir den Begriff Körper durch das beigeordnete Merkmal der Ausdehnung

193 Steht es] The manuscript originally had the words "Ist es", but they have been struck out and replaced by "Steht es" above the line.

193 frei] The manuscript originally had "erlaubt", which has been struck out and replaced above the line by "frei".

193 Merkmal] The manuscript originally had the words "Begriff des", but they have been struck out and replaced by "Merkmal" above the line with an insertion mark.

194 ist] The word "ist" appears in the manuscript as the replacement of something else, which, however, has become illegible due to the physical deterioration of the paper.

194 über die Existenz] Before these words, the words "allein die Erfahrung ver der" have been written, but struck out.

196 für] Before this word, the word "gemäs" has been written, but struck out.

197 ausschließlich] The manuscript originally had "allein", which has been struck out and replaced above the line by "ausschließlich".

197 synthetischen] This word has been added above the line with an insertion mark.

198 Definition] The manuscript originally had "Auffassung", which has been struck out and replaced above the line with "Determination", which has also been struck out and replaced in between finally with "Definition".

199 zu] Before this word, the word "ein" has been written (as well as some other letters), and struck out.

199 Sobald sich damit sein gedanklichr Gehalt ändert] The manuscript had originally "indem dadurch sein Gehalt verändert wird", but "indem" has been struck out and replaced above the line by "sobald", "sich" has been added on the line between "indem" and "dadurch", the sillable "durch" has been struck out and replaced above the line by "mit" and "gedankliche" has been inserted above the line (twice, but one instance has been struck out), and the prefix "ver" has been struck out.

201 Alle Körper sind ausgedehnt] Before this sentence, a roman "(I)" has been written, but struck out.

202 — schwer] Before this sentence, a roman "(II)" has been written, but struck out.

definiren. Im Fall des alleinigen Zu†…†des Merk der Schwere wird jedoch das †…†a. des s. der Sachverhalt ist verändert, sobald wir etwa im ersten Fall das Wort Ausgedehnt in zweiten das Wort Masse an Stelle des Wortes Körpers einsetzen. Die Urteile

Alles Ausged ist ausged

u Alle Masse ist schwer

sind an. †…†

Alles Ausgedehn ist schwer

Alle Masse ist ausgedehnt

sind synthetisch. Obwohl der Form nach nämlich, sind doch die sind jene Paare von Urteilen inhaltlich vollkommen unterschieden. Das †…†Beispiel zeigt zugleich, daß die Entscheidung zwischen a. od. s. †…† nur bei mangelhafter Begriffsbestimmung d. h. bei unvollkommener Verständnisse d. Urteils=Inhaltes einer Willkürlichkeit unterworfen ist, keineswegs aber von der geistigen Entwicklungsstufe des darüber entscheidenden Individuums abhängt. Die oben als synthetisch aufgeführten Urteilen können selbst der Kritik eines höchst entwickelten Geistes gegenüber niemals ihre synthetische Natur verläugnen[H] Kein Absatz [I] der Fall endlich, der †…† der Synthesis durch

[H] A short line drawn with blue crayon appears at this point in the manuscript; its significance is not apparent.

[I] Another, similar short line drawn with blue crayon appears here, again with no clear significance.

204 alleinigen] This word has been added above the line with an insertion mark.

205 Masse] Before this word, the word “Materie” has been written, but struck out.

212 synthetisch] Before this word, the words “ihrer Aposteriorität wegen”.

212 sind jene Paare von Urteilen] The manuscript originally had “beide Urteile”, which was struck out and replaced (on the line behind these words) with “Urteil (I) von dem Urteil (II)”, which was also struck out and replaced above the line with “sind zwei Paare von Urteilen”.

213 zwischen] The manuscript originally had “ob”, which has been struck out and replaced above the line with “zwischen”.

213 †…†] These words have been added above the line with an insertion mark.

214 unvollkommener] The manuscript originally had “mangelhafter”, but that word has been struck out and replaced above the line by “unvollkommener”.

214 d. Urteils=Inhaltes] The manuscript originally had “d. Urteils seinem Gehalte nach”, but “seinem Gehalte nach” has been struck out and replaced above the line by “=Inhaltes”.

215 geistigen] This word has been added above the line with an insertion mark.

215 darüber entscheidenden] The manuscript originally had “und entscheidend”, which has been struck out and replaced above the line with “darüber entscheidenden”.

216 oben als synthetisch] The manuscript originally had “in (II)”, which has been struck out and replaced above the line by “oben als synthetisch”.

216 der Kritik] Before these words, the words “vom Standpunkt” have been written, but struck out.

217 gegenüber] The manuscript originally had “aus”, which has been struck out and replaced above the line by “gegenüber”.

217 der †…†] Before these words, the words “daß beide Merkmale des †…†Aus u Schw. in den Begriff” have been written, but struck out.

gleichzeitige Beiordnung beider Merk. zu dem Begriff Körper ist oben erledigt.

Nach den letzteren Ausführungen wird im Speziellen auch arithmetischen Urteile die Frage nach ihrer Natur einer obejtkiv bestimmten Antwort fähig sein. Wie bereits das des öfteren begründet schliesen wir uns in diesem Punkte vollkommen der Auffassung Kants an, welcher den Satz $7 + 5 = 12$ für synthetisch erklärt. Auch den höheren arithm. u. algebraischen Operationen beruhen im innersten Wesen (†…†) auf dem gleichen Konstruiren und Denken in der sinnlichen Anschauung, wie die niedrigen Rechnungsarten. Wie jene gilt daher betreffs ihrer Natur u Eigentümlichkeit dasselbe wie von diesem Satz sind Sätze wie:

"In der natürlichen Zahlenreihe giebt es unendlichvielPrimzahlen"

"Jede †…†Funktion d. Wurzeln einer allgebraischen Gleichung ist von den Koefficienten der letzteren in †…†abhängig."

"Die Irrationalzahl π ist nicht Wurzuel einer algebraischen Gl mit ganzzahligen Koefficienten." | 4r

Diese Tautologien oder bloße Identitäten nach den Gesetzen der formalen Logik, sondern insgesammt Urteile wesentlich synthetischen Charakters.

Daß es in der Arithmetik auch analytische Urteile giebt ist selbstverständlich. Kant selber erwähnt als Beispiel die beiden Sätze:

$a = a$

und $a \overset{>}{<} a + b \quad (b \overset{>}{<} 0)$

von denen das erste das Identitätsprinzip, das zweite den Satze des Widerspruches in arithmetischer Form zum Ausdruck bringt. Der Funktion analytischer Urteile entsprechend dienen beide Sätze auch in der Mathematik nur als

---

221 Auffassung Kants] Before the word "Auffassung", the word "Kantschen" has been written, but struck out. Between the word "Auffassung" and "Kants" the word "von" has been written, but struck out; the genetive "s" of "Kants" was presumably added when "von" was stricken out.

222 arithm. u. algebraischen Operationen] The manuscript originally had "Rechnungsarten", which has been struck out and replaced above the line with "arithm. u. algebraischen Operationen".

222 beruhen im innersten Wesen (†…†)] The manuscript originally had only "beruhen", which has been struck out and replaced by the same word above the line; the words "im innersten Wesen (†…†)" have been added in pencil with an insertion mark.

223 sinnlichen Anschauung] Before these words, the words "Anschauung rein" have been written, but struck out.

223 betreffs] Before this word, the word "das" has been written, but struck out.

224 Eigentümlichkeit] The manuscript originally had "Eigenschaften", but "schaften" has been struck out and replaced above the line with an insertion mark by "tümlichkeit".

225 In der natürlichen Zahlenreihe] Before these words, the words "Es gieb" have been written, but struck out.

228 nach den Gesetzen der formalen Logik] Before these worde, the words "sind †…†" have been written, but struck out.

228 insgesammt] The manuscript originally had "syn Si", which has been struck out, and replaced by "insgesammt", which has been written above the line with an insertion mark in pencil and in a diffrent handwriting.

229 wesentlich synthetischen] Before these words, the word "synthetisch" has been written, but struck out.

234 den] Before this word the word "†…†" has been written, but struck out.

234 in arithmetischer Form] Before these words, the words "†…†Aus" have been written, but struck out.

Bindemittel in der Kette der synthetischen Entwicklungen †…†Hervorholung Erläuterung bekannter Eigenschaften oder zur Definition von einzuführenden Beziehungen, während ein wirklicher Fortschritt in der Arithmetik durch jene Sätze allein ohne Hülfe der sinnlichen Anschauung unmöglich erscheint.

Hiermit ist die Untersuchung der Frage nach der Natur d. arith. Urteile zum[J] Abschluß geführt. Als Ausgangspunkt diente uns die Kantsche Lehre von der Synthesis apriori der mathem. Urteile. Dieselbe wurde im †…†Verlauf der Untersuchung †…†der mathematischen Urteile einer strengen Prüfung unterzogen und unter dieser Einschränkung als vollkommen begründet u zutreffend erkannt.

Die hohe philosophische Bedeutung der behandelten Fragen ist bereits in den einleitenden Worten zu Anfang dieser Untersuchung berührt. Ihre Beantwortung ist ein sehr †…†der gesammten philosophischen Lehre Kants einer nicht unwesentliche Dienst leisten, zumal einmalig die Er. sy ap. Sätze in die †…†u wohl mit Recht in Frage gestellte ist. Ein †…†suchung [?] Ex. wenigstens einiger sy. ap. Erkentnis ist nämlich eine notwendige Vorunt[ters]u[chung] damit die Frage nach den Gründen u Prinzipe [...][K] | †…†in der reinen Form der sinnlichen Anschauung †…†kann, 4v
uns letzteres die Bedingung ihres Gebrauchs, den Anfan[g?] u die Grenzen dieses Gebrauchs zu bestimmen sind die weiter bedeutungsvollen Probleme, mit denen sich die Kritik der reinen Vernun. u die Prolegomen beschäftigen. —[L]

So ist die Auflösung der Gl. $x^m - 1 = 0$ †…†V, 2 pag 277 selbst ist diese Hegelsche Unterscheidung der †…†in zwei getrennte Klassen ist keinesfalls zu billigen, weil sie einerseits einer prinzipiellen Begründung entbehrt, andrerseits vielfach in das unterscheidenden Individuen gesetzt ist. †…†z. B. siehe die Umkehrung unser math. Satzes nämlich $12 = 7 + 5$ nach den Hegelschen Bestimmungen an. od synth.? Wie verhält es sich ferner mit den Sätzen

$$12 = 0^2 + 2^2 + 2^2 + 2^2$$

$$12 = 1^2 + 1^2 + 1^2 + 3^2$$

[J] A mark in red crayon has been inserted here; its significance remains unclear.

[K] The last line or two written on this sheet have become illegible due to damage to the paper: Too many pieces are missing at the bottom of the sheet to make out anything more than *that* something was written there.

[L] After the dash here, a reference to “Prolegomena pag 28” has been added.

236 Hervorholung] Before this word, the words “Erläuterung †…†” have been written, but struck out.

236 Eigenschaften] Before this word, the word “Merkmale” has been written, but struck out.

243 Die hohe] Before these words, the words “Dass mußte [?]” have been written, but struck out.

245 Sätze in] Before these words, the word “Erkenntnisse” has benn written, but struck out.

246 wenigstens] Before this word, the word “von” has been written, but struck out.

247 Form] Before this word, the word “Anscha” has been written, but struck out.

248 letzteres] Before this word the words “seine”, “dieselben” and some word starting with “d”, which is illegible due to a piece of the margin having been turn away, and “Bedingen” [sic] have been written, but all (separately) struck out.

249 bedeutungsvollen] Before this word, the word “tiefere” has been written, but struck out.

251 V, 2] This reference has been added above the line with an insertion mark.

252 zu billigen] Before these words, the same words have been written once already, but struck out.

die letzten Formeln sind Exemplificationen des $^{\dagger}\ldots^{\dagger}$:

Jede größten Zahl ist als Summe von 4 Quadraten darstellbar

Ist dieser Satz in Hegels Sinne an. od synt? u. s. w.

Appendix D

Behmann's Presentations on Russell

Editorial Introduction

Early on in his career on at least two occasions Behmann presented Russell's work to more or less specialist audiences: The first such presentation took place in the Colloquium of the mathematical society in Göttingen in 1914, a second such presentation was given in 1927 at the invitation of Heinrich Scholz to a general audience in Kiel.

Of the 1914 presentation a very clean manuscript in Behmann's handwriting survives (it is dated Dec 1 1914 and comprises 15 sheets which have been continuously paginated by Behmann himself), while what likely were Behmann's preparatory notes for the 1927 presentation in Kiel survive in typewritten form (this typescript is not dated, comprises 16 sheets which have been paginated (by hand) by Behmann).

Both of these texts are reproduced here.

Texts

The manuscripts edited here are held by the Staatsbibliothek zu Berlin – Preußischer Kulturbesitz. They are found in box 9 of Nachlass 335 (Heinrich Behmann) and within that box constitute units 8 ("Über mathematische Logik (1914)") and 29 ("Die Russellsche Theorie der Typen"). The text of both lectures appears here with kind permission of the Handschriftenabteilung, Referat Nachlässe und Autographen of the Staatsbibliothek zu Berlin – Preußischer Kulturbesitz.

D.1 Über mathematische Logik (1914)

Wie ich gleich im voraus bemerken möchte, beabsichtige ich mit meinen Ausführungen, Sie in die Grundgedanken 1.
eines im Laufe der letzten Jahre erschienen größeren Werkes einführen, das eine ebenso tiefgründige wie erschöpfende

Darstellung alles dessen bietet, was man unter dem Namen "Mathematische Logik" zusammenfassen kann. Ich meine die "Principia mathematica", wie von den beiden englischen Logikern B. Russell und A.W. Whitehead verfaßt sind und von dem ich Ihnen hier den ersten im Jahre 1910 erschienen Band vorlege. Wenn ich Ihnen sage, daß alle drei bisher erschienen Bände durchschnittlich denselben Umfang haben und zusammen etwa zweitausend Seiten ausmachen, so ergibt sich daraus von selbst, daß ich an dieser Stelle aus der gewaltigen Fülle des Stoffes nur ein paar wesentliche Punkte, sozusagen einige Grundsteine er Theorie, herausgreife und daran die ganze Art der Behandlung klar zu machen versuche, wobei ich natürlich manche tieferliegende Fragen nur kurz berühren kann.

Bevor ich zu meinem engeren Thema übergehe, will ich noch wenige Worte über das Wesen und die bisherige Entwicklung der mathematischen Logik sagen. Wir bemerken zunächst, daß der Ausdruck "Mathematische Logik", der mir diese ganze Forschungsrichtung am besten zu bezeichen scheint, nicht eindeutig ist. Er kann nämlich einerseits als eine mathematische Durchbildung der allgemeinen Logik – sozusagen als eine "Mathematik der Logik" – verstanden | werden die dieser vor allem die Vorteile einer mathematischen Bezeichnungsweise und Formelsprache 2.
zugute kommen läßt – in diesem Sinne spricht man auch von "Symbolischer Logik" –, auf der anderen Seite aber kann man an das Verhältnis von Logik und Mathematik zueinander an die Rolle, die die Logik im Aufbau der einzelnen mathematischen Wissenschaften spielt, denken und würde also in diesem Sinne von einer "Logik der Mathematik" zu reden haben. Da bis jetzt noch durchaus keine Einigkeit darüber besteht, bis zu welchem Grade die Mathematik überhaupt von der Logik abhängt, würde also diese Seite der Behandlung für die extremste Ansicht, die hier möglich ist auf den Neuaufbau der gesamten reinen Mathematik allein aus reiner Logik hinauskommen. Das ist in der Tat der von Russell und Whitehead vertretene Standpunkt. Ich halte es nicht für wünschenswert, diese Zweideutigkeit in dem einen oder anderen Sinne aufzulösen; vielmehr scheint mir die eigentliche Bedeutung der mathematischen Logik erst in der Vereinigung beider Aufgaben zu bestehen.

Dies bestätigt sich auch, wenn wir von hier aus einen Blick auf die bisherige Entwicklung der mathematischen Logik werfen. Wir finden nämlich, daß die Anfänge dieser Entwicklung fast ganz unter dem Einfluß der ersten Auffassung stehen, so z.B. G. Boole in seinen "Laws of Thought", ebenfalls E. Schröder – der Titel seines dreibändigen Werkes über die "Algebra der Logik" verrät ganz deutlich diese Auffassung –, auch bei G. Peano herrscht sie vor, wenn dieser auch schon eine Mittelstellung einnimmt, – um nur einige der wichtigsten Vertreter zu nennen. Als eine Vorarbeit im Sinne der zweiten Auffassung können wir z.B. B. Bolzanos "Wissenschaftslehre" und seine "Paradoxien des Unendlichen" ansehen. Sie hat dann eine | eingehendere Bearbeitung erfahren in G. Freges "Grundlagen 3.
der Arithmetik" und in seinen "Grundgesetzen der Arithmetik" – auch hier sind die Überschriften bezeichnend –. Meines Wissens sind die "Principia mathematica" das erste größere Werke, das alle wesentlichen Ergebnisse

4 sind] The text originally had "ist", which has been struck out and corrected below the line with an insertion mark.

19–20 für die extremste Ansicht, die hier möglich ist] These words have been written in the right margin and placed here by a "V"-shaped insertion mark.

dieser Forschungen zu einer Einheit zusammenfaßt – ganz abgesehen natürlich von der Fülle seiner neuen und tiefen Gedanken – und sie damit zu einem gewissen vorläufigen Abschluß bringt. Seiner Tendenz nach gehört es durchaus der zweiten Richtung an, immerhin wird es wegen seiner Vielseitigkeit auch der ersten Richtung sachlich durchaus gerecht, und es verdient daher für den, der sich einen Einblick in das Gebiet verschaffen will, in allererster Linie Beachtung.

Wenn wir einen Band des Werkes aufschlagen, so bemerken wir, daß es nur zum kleineren Teil in gewöhnlichem Englisch geschrieben ist. Es besteht zum größten Teile aus einer ganz neuartigen Formelsprache, in der alle Beweise geführt und alle bewiesenen Sätze ausgedrückt werden. Der erste Eindruck ist ein so fremdartiger, daß wohl ein jeder , der das Buch zum ersten Mal aufschlägt, sich zunächst mit einigem Entsetzen abwenden wird. Aber es ist, wie eine vielfältige Erfahrung lehrt, tatsächlich unmöglich, irgend ein Gebiet mit mathematischer Strenge zu bearbeiten, ohne eine der mathematischen ähnliche Formelsprache zu benutzen; die gewöhnliche Wortsprache wäre dazu viel zu verschwommen und unbeholfen.

Die Darstellungsform des Werkes ist die axiomatische, d.h., es werden alle Sätze aus neunzehn Grundsätzen (primitive propositions) abgeleitet. Als Grundbegriffe (primitive ideas) brauchen wir dabei zunächst die des Dinges, des Satzes und der Satzfunktion. Zu den | Dingen rechnen wir alles, was uns als Gegenstand unmittelbar in der 4.
Wirklichkeit gegeben ist, also nicht etwa Mengen oder ähnliche reine Gedankengebilde (individuals). Wenn uns ein Satz gegeben ist, so können wir darin irgend ein bestimmt angegebenes Ding durch die Unbestimmte oder Veränderliche x ersetzen und erhalten dadurch eine sogenannte Satzfunktion (propositional function). So entsteht z.B: aus dem Satz: “Sokrates ist sterblich” die Satzfunktion “x ist sterblich”. Ebenso lassen sich natürlich auch Funktionen – wir wir die Satzfunktionen kurz nennen wollen – von mehreren Veränderlichen bilden. Dabei können die Argumente der Satzfunktion selbst wieder Sätze oder Funktionen sein, wenn ich z.B. den Satz aufstelle:

“A behauptet, daß die Funktion $\phi\hat{x}$ für jedes x richtig ist.”

Ist nun irgend eine Funktion $\phi\hat{x}$ gegeben, z.B. “x ist sterblich”, so können wir daraus umgekehrt wieder Sätze bilden, indem wir die Veränderliche x durch bestimmte Dinge ersetzen. [A]Diese Sätze brauchen natürlich nicht alle richtig zu sein; z.B. Sokrates ist sterblich, Zeus ist sterblich, wenn wir für den Augenblick Zeus als ein Ding der Wirklichkeit ansehen wollen. Es kann nun sein, daß die Funktion für alle oder doch für mindestens ein x richtige Sätze ergibt. Wollen wir dies ausdrücken, so schreiben wir z.B. $(x).x$ ist sterblich, $(\exists x).x$ ist sterblich, d.h. “Alle Dinge sind sterblich”, “Einige Dinge sind sterblich”. Von diesen Sätzen ist der erste falsch und der zweite richtig. Hier haben wir es mit Sätzen zu tun, in denen die Veränderliche x auftritt, die aber natürlich keine Funktionen von

[A]Behmann has here begun writing the following sentence, which has been left unfinished and struck out: “Hat nun eine Funktion $\phi\hat{x}$ die Eigenschaft, daß alle so ents[stehenden].”.

33 natürlich] Above the line Behmann has written in pencil “freilich” perhaps as a replacement or alternative for “natürlich”.

33 und tiefen] These words have been added above the line in pencil and placed here with an insertion mark.

x sind. Nach einem von Peano übernommenen Sprachgebrauch bezeichnen wir das | x, wenn es als Argument einer 5.
Funktion auftritt, als echte Veränderliche (real variable); hängt dagegen der Ausdruck, in dem es auftritt, nicht von ihm ab, so heißt es eine Scheinveränderliche (apparent variable). So ist z.B. in dem Ausdruck $\int_a^x f(y)dy$ x eine
65 echte Veränderliche und y eine Scheinveränderliche.

Der Begriff der Satzfunktion ist einer der wichtigsten, aber auch einer der schwierigsten der Logik. Dies liegt auf der einen Seite an der ungeheuren Vielseitigkeit dieses Begriffes und an den verwickelten Formen, die die Satzfunktion unter Umständen annehmen kann. Man braucht nur zu bedenken, daß allein sämtliche mathematischen Sätze zu ihnen gehören. Andererseits aber ist es nicht von vornherein klar, wie man mit diesen Funktionen operieren muß,
70 ohne gelegentlich auf widerspruchsvolle oder sinnlose Sätze zu stoßen. Ein berühmt gewordenes Beispiel hierfür ist das griechische Sophisma von dem Kreter Epimenides, der bekanntlich behauptet, daß alle Kreter Lügner seien. Dies bedeutet im Sinne des Sophismas, daß jede von einem Kreter ausgesprochene Behauptung falsch ist. Da der Sprechende aber nach Voraussetzung sich selbst zu den Lügnern rechnet, ist sein Ausspruch ein Satz, der seine eigene Falschheit behauptet. Das bedeutet – in die Sprache der symbolischen Logik übersetzt –, wir haben es hier
75 mit einer Funktion zu tun die sich selbst zum Argument hat, also von der Form $\phi(\phi)$ ist.

Da nun alle derartigen Sätze selbstverständlich für die Logik unbrauchbar sind und auf irgend eine Weise ausgeschlossen werden müssen, so stellen Russell und Whitehead die Forderung auf, daß an die Argumentstelle einer Funktion niemals die Funktion selbst | treten darf oder etwas, was nur mit Hilfe dieser Funktion ausgedrückt 6.
werden kann. Dies ist das sogenannte Zirkelprinzip (vicious-circle principle). Aber diese Forderung ist allein noch
80 nicht hinreichend; sie erweitert sich daher in den "Principia" zu der "Lehre von den logischen Stufen" (theory of logical types). Wenn ich das Wort "type" durch "Stufe" übersetze, so geschieht dies in Anlehnung an Frege, der in ganz ähnlichem Sinne Begriffe erster und zweiter Stufe unterscheidet. Diese Stufentheorie besteht nun in der ganz allgemeinen Forderung, daß an die Argumentstelle einer Satzfunktion nicht beliebige Ausdrücke treten dürfen, sondern nur solche, die einer gewissen Stufe angehören, und daß auch die Funktion einer bestimmten Stufe angehört
85 und zwar einer höheren als jeder der Ausdrücke, die in ihr vorkommen. So baut sich eine ganze Rangordnung von Funktionen (functional hierarchy) auf, die nicht etwa eindimensional ist, sondern einen äußerst verwickelten Bau hat, zumal noch die Unterscheidung der echten und der Scheinveränderlichen hineinspielt. Ich sehe geradezu eine Hauptschwierigkeit für das Verständnis der Principia darin, sich von dieser Rangordnung ein einigermaßen klares Bild zu machen. Es wird allerdings auf diese Weise erreicht, daß jede Möglichkeit eines Widerspruchs von
90 vornherein ausgeschlossen wird, vorausgesetzt, daß jeder einzelne Logische Schritt in Übereinstimmung mit der Theorie ausgeführt wird.

Aber wenn auch der praktische und erkenntnistheoretische Wert dieser Theorie unbestreitbar ist und man auch sonst noch einleuchtende Gründe für sie anführen kann – insofern haben wir es hier mit einer durchaus vernünftigen Theorie zu tun, so bleibt | es doch eine Frage, ob sie nicht gegenüber der verhältnismäßig harmlosen Sophismen, 7.

die in der Logik der Sätze und Funktionen auftreten können, ein etwas zu schweres Geschütz darstellt. Durch die Beschränkung jedes Satzes auf eine einzelne Stufe werden ja auch Sätze ausgeschlossen, die kaum irgendwie bedenklich erscheinen, wie z.B. der Satz vom ausgeschlossenen Dritten, wenn man ihn in der folgenden Form ausspricht:

"Alle Sätze sind entweder richtig oder falsch."

Die Stufentheorie erklärt diesen Satz für sinnlos, da nicht alle Sätze derselben Stufe angehören, und dieser Standpunkt läßt sich auch folgendermaßen begründen. Der Ausdruck "alle Sätze", der in dem genannten Satze auftritt, bezeichnet nämlich eine gewisse Gesamtheit von Sätzen. Ein Satz, der diese Gesamtheit schon voraussetzt, kann ihr aber selbst nicht angehören, da er ja sonst sich selbst voraussetzen würde. Dieser Einwand, der sich wesentlich gegen den Ausdruck "alle Sätze" richtet und der ja in der Tat die Unmöglichkeit einer Gesamtheit aller Sätze beweist, läßt sich aber kaum noch aufrecht erhalten, wenn man den Satz so schreibt:

"Wenn p ein Satz ist, so ist p entweder richtig oder falsch."

Denn hieraus sieht man, daß der Satz in Wirklichkeit gar keine Aussage über "alle Sätze", d.h. die Gesamtheit der Sätze, darstellt, sondern vielmehr die einzelnen Sätze selbst zu Gegenständen hat. Er ist ja auch richtig, was für einen Satz man auch für p einsetzen mag, selbst dann, wenn p selbst den Satz vom ausgeschlossenen Dritten bedeutet. So möchte man wenigstens vom Standpunkt des gesunden Menschenverstandes den obigen Satz | in seiner 8.
allgemeinen Form für zulässig erklären. Die Stufentheorie kann ihn dagegen nur so gelten lassen, daß p auf irgend eine bestimmte Stufe beschränkt ist. Ich glaube, daß wir es hier mit einer noch offenen Frage zu tun haben, zu der ich im Augenblick noch keine bestimmte Stellung nehmen kann.

Ich hatte vorhin die Widersprüche nach der Art des Sophismas vom Epimenides für einigermaßen harmlos erklärt. Diesen Standpunkt kann ich jedoch nicht mehr aufrecht erhalten, wenn ich mich nunmehr zu den Widersprüchen wende, die es mit dem Begriff der Menge zu tun haben. Ich brauche nur an das bekannte Russellsche Paradoxon zu erinnern oder an das Burali-Fortische von der Ordnungszahl aller Ordnungszahlen, das ja geradezu zum Schreckgespenst der Mengenlehre geworden ist. Hier haben wir es wirklich mit einer ernstlichen Schwierigkeit zu tun, die nur durch ein tieferes Eindringen in das Wesen des Mengenbegriffs behoben werden kann. Ihre Erledigung muß geradezu als eine Lebensfrage der Logik und vor allem der Arithmetik angesehen werden. Die Antwort, die Russell nach vielen vergeblichen Versuchen schließlich auf diese Frage nach dem Wesen der Menge gegeben hat, ist eine völlig unerwartete; sie mutet uns faßt an wie das Ei des Klumbus, denn sie verblüfft gerade durch ihre Einfachheit. Sie besagt nämlich einfach: Es gibt überhaupt gar keine Mengen. Es ist klar, daß dieser Satz, falls er richtig ist, alle Schwierigkeiten und Widersprüche des Mengenbegriffs mit einem Schlage aus dem Wege räumt. Damit ist in kurzer, allerdings paradoxer, Form der Inhalt derjenigen Auffassung bezeichnet, die in der Literatur als die Russellsche "no-class | theory" bekannt ist. Unsere nächste Aufgabe ist nun, diesen Satz uns so zum Verständnis 9.

123–124 falls er richtig ist,] This qualification has been added in the right margin and placed here with a "V"-shaped insertion mark.

zu bringen, wie er wirklich gemeint ist. Wir wollen es nicht so machen wie Poincaré, der sich allerdings wohl nur auf frühere Veröffentlichungen Russells stützte und der die Russellsche These dahin auslegte: man solle ein Lehrbuch der Logik hernehmen und darin alle Sätze ausstreichen, in denen das Wort "Menge" oder "Klasse" vorkommt. Was dann übrig bleibe, sei die allein wahre Logik.

Um die Russellsche These nun aber richtig zu würdigen, will ich mich eines einfachen Beispiels aus der Geometrie bedienen. Bekanntlich lernt man schon in der Schule, es sei für bestimmte Zweige der Geometrie vorteilhaft, der geraden Linie einen unendlich fernen Punkt "zuzuschreiben"; man "stellt sich vor", daß jede gerade Linie außer ihren im Endlichen liegenden Punkten noch einen unendlich fernen Punkt besitzt, durch den zugleich alle ihre Parallelen hindurchgehen. Wenn einen diese Lehre vom Unendlichfernen zum ersten Mal in dieser Darstellung – und ich glaube, es ist die übliche – geboten wird, so ist sie durchaus geeignet, beim Hörer lebhaften Widerspruch zu erregen – ich erinnere mich wenigstens, daß es mir so gegangen ist –. Man fragt sich bei einige Nachdenken: Woher mag es nur kommen, daß die beiden Parallelen, die doch überall den festen Abstand a haben, plötzlich im Unendlichen den Abstand Null haben, da doch der unendlich ferne Punkt nicht von sich selbst den Abstand a haben kann, und wie erklärt sich das Wunder, daß die Gerade allein durch das Hinzukommen dieses einen Punktes zu einer geschlossenen Linie | wird und dabei die Eigenschaft einbüßt, die Ebene in zwei Gebiete zu zerlegen? Ich glaube, 10.
daß selbst der eine oder der andere gründliche Kenner der projektiven Geometrie solchen Fragen eines Anfängers gegenüber um eine klare und bündige Antwort verlegen wäre. Ein Logiker – oder vielmehr eine gewisse Klasse von Logikern – würde natürlich sagen: Das ist alles Unsinn, der unendlich ferne Punkt wird nicht "zugeschrieben" oder "vorgestellt", sondern man erschafft ihn einfach, sobald man ihn braucht. Es soll allerdings Leute geben, die auch nach dieser Erklärung gerade so klug sind wie vorher.

Trotz alledem bietet die richtige Auflösung all dieser geometrischen Widersprüche, sobald sie einmal gegeben ist, dem Verständnis keine erhebliche Schwierigkeit dar. Sie besagt nämlich einfach, daß die idealen Elemente nicht im eigentlichen Sinne Gegenstände der Geometrie sind, sondern zunächst einmal nur Worte und als solche Teile von Ausdrucksweisen, deren man sich in der Geometrie bedient, um eine gewisse Klasse von Sätzen auf eine möglichst einfache Form zu bringen. So besagt z.B: der Satz:

"Die Geraden a und b schneiden sich im Unendlichen"

nicht mehr und nicht weniger als der andere

"Die Geraden a und b sind parallel."

Der zweite Satz ist nichts weiter als eine für manche Zwecke bequemere Form des ersten Satzes und bringt sachlich nicht das geringste Neue hinzu. In genau derselben Weise läßt sich nach bestimmten Vorschriften jedem Satz über uneigentliche Elemente ein logisch vollkommen gleichwertiger an die Seite stellen, in dem die uneigentlichen Elemente überhaupt nicht vorkommen. Damit | ist gezeigt, daß uns die Frage nach der Existenz der uneigentlichen 11.

128 dahin] The manuscript originally had "so", which has been struck out and corrected above the line with an insertion mark.

Elemente als geometrische Gegenstände nicht im mindesten zu kümmern braucht und daß sie deshalb auch keinen Anlaß zu irgendwelchen Schwierigkeiten in der Auffassung geben können.

Wir brauchen das Ergebnis der vorausgehenden Untersuchung nur zu verallgemeinern, um auch die Russell-Whiteheadsche Klassentheorie zu verstehen. Das Wort "Klasse" ist nur ein anderer Ausdruck für "Menge". Diese Theorie verfährt nämlich mit den Klassen genau so, wie wir eben mit den unendlich fernen Punkten verfahren sind. Sie kümmert sich nicht darum, was eine Klasse ist und ob es Klassen gibt. Sie erklärt "Klasse" einfach für ein Wort, daß in bestimmten Ausdrucksweisen vorkommt, deren wir uns zur bequemeren Darstellung gewisser Tatsachen bedienen; daß wir diese Tatsachen aber auch aussprechen können, ohne irgend ein solches Wort wir "Klasse", "Menge", "Gesamtheit" überhaupt zu gebrauchen. Nach den Feststellungen von vorhin ist ein Satz wie dieser:

"Der unendlich ferne Punkt einer Geraden gehört auch allen parallelen Geraden an"

nur scheinbar eine Aussage über den unendlich fernen Punkt, da in diesem Satz zunächst einmal die Definition des Ausdruckes: "Ein unendlich fernen Punkt gemein haben" einzusetzen ist, genau so, wie der Satz:

"Keine Katze hat acht Schwänze."

nur scheinbar eine Aussage über "keine Katze" ist. Wir dürfen aber nicht das Wort "Klasse" selbst definieren wollen, gerade so, wie wir auch den Ausdruck "unendlich ferner Punkt" nicht selbst | definiert haben, sondern 12.
müssen uns darauf beschränken, die Ausdrucksweise, in denen das Wort "Klasse" vorkommt, zu definieren. Bei dieser Gelegenheit erinnere ich daran, daß wir, wie schon vorher, unter einer Definition stets eine reine Festsetzung zu verstehen haben. Eine Definition kann daher niemals etwas aussagen.

Wir bemerken zunächst, daß, wenn wir von irgend einer Klasse α sprechen wollen, diese Klasse durch irgend eine Eigenschaft festgelegt sein muß, die allen Elementen von α, aber keinen nicht in α enthaltenen Dingen zukommt, denn ohne irgend eine derartige Bestimmung würde sie völlig in der Luft schweben. Eine solche Eigenschaft wird symbolisch durch eine Satzfunktion $\phi\hat{x}$ dargestellt, die stets einen richtigen Satz ergibt, wenn wir x durch ein Element von α ersetzen, für andere Werte von x aber stets falsch ist. Dies ist im wesentlichen auch die Cantorsche Definition der Menge. Nun ist es klar, daß zu ein und derselben Klasse viele Funktionen gehören können, daß aber mit der Funktion auch die zugehörige Klasse schon gegeben ist. Aus diesem Grunde ist es möglich, jede Aussage über eine Klasse in eine Aussage über irgend eine die Klasse definierende Funktion zu verwandeln. Um ein Beispiel aus den "Principia" zu gebrauchen, so können wir statt des Satzes:

---

179 allen] Before this word the word "nur" has been written, but struck out.

180 ohne irgend eine derartige Bestimmung] The manuscript originally had just "andernfalls", which has been struck out and (being the last word in the line) replaced in the right margin.

185 verwandeln] Before this word, the word "erset[zen]" has been written, but struck out.

"Die Klasse der Apostel hat zwölf Elemente"

auch sagen:

"Die Funktion: '$\hat{x}$ war ein Apostel' wird durch zwölf Argumente befriedigt."

Dies ist eine Eigenschaft oder Funktion der Funktion "$\hat{x}$ war ein Apostel" von der Form $f(\phi)$ oder vollständiger $f(\phi\hat{x})$. Und zwar | ist f eine extensive Funktion von ϕ, d.h. wir dürfen ϕ durch irgend eine andere Funktion ψ 13. ersetzen, wenn ψ nur dieselbe Klasse definiert oder – anders ausgedrückt –, wenn sie durch dieselben Argumente wie ϕ befriedigt wird.

Wir bezeichnen nun diejenige Aussage, die aus $f(\phi\hat{x})$ entsteht, indem wir sie in eine solche über die durch $\phi\hat{x}$ definierte Klasse umformen, mit $f\{\hat{z}(\phi z)\}$. Der Wechsel des Argumentbuchstabens soll nur zum Ausdruck bringen, daß es auf ihn nicht weiter ankommt. Dann können wir von diesem Ausdruck etwa folgende Definition geben:

$$f\{\hat{z}(\phi z)\}.=:f(\phi\hat{z}):\psi x\equiv_x \phi x.\supset_\psi .f(\psi\hat{z})\quad \text{Df}$$

D.h., wenn wir sagen, der durch $\phi\hat{x}$ bestimmten Klasse kommt eine gewisse Eigenschaft zu, die wir symbolisch mit f bezeichnen, so soll diese Ausdrucksweise bedeuten, daß die die Klasse definierende Funktion $\phi\hat{x}$ die Eigenschaft f hat und daß, wenn wir ϕ durch irgend eine Funktion ψ ersetzen, die durch dieselben Werte wie ϕ erfüllt wird, auch ψ die Eigenschaft f hat. Durch diese Übereinkunft wird also das Wort "Klasse" oder "Menge" vollständig entbehrlich.

In Wirklichkeit ist dies noch nicht die Russell-Whiteheadsche Definition, da in der eben betrachteten von den Funktionen die Rede war, die durch dieselben Werte wie ϕ erfüllt werden. Diese Funktionen gehören aber der Stufentheorie zufolge nicht alle derselben Stufe an. Infolgedessen wird in den "Principia" ein besonderes Axiom eingeführt, das eine Definition ähnlich der obigen trotzdem ermöglichen soll. Dies ist das Axiom der Reduzierbarkeit. Es besagt, daß zu jeder Klasse, die überhaupt irgendwie angebbar ist, es auch eine Funktion der niedersten Stufe gibt, die | eben diese Klasse bestimmt. Diese Funktion niedester Stufe sind diejenigen, die eine reelle Veränderliche, aber 14. keine Scheinveränderliche enthalten. Man bezeichnet sie als prädikative Funktionen und deutet diese Eigenschaft durch ein hinter das Funktionszeichen gesetztes Ausrufungszeichen an. Das Axiom sagt also aus, daß in dieser einen Stufe schon genügend viele Funktionen vorhanden sind, um alle überhaupt möglichen Klassen zu definieren; es läßt sich daher auch als eine Art Vollständigkeitsaxiom für die prädikativen Funktionen auffassen. Das Axiom der Reducierbarkeit, über dessen Zulässigkeit oder Notwendigkeit zu sprechen hier zu weit führen würde, schreibt

187 "Die Klasse der Apostel hat zwölf Elemente"] Behmann originally formulated the example as "Es gab zwölf Apostel", but struck it out and replaced it with the formulation in the text.

195–196 Der Wechsel des Argumentbuchstabens soll nur zum Ausdruck bringen, daß es auf ihn nicht weiter ankommt.] This remark has been added in the right margin and placed here with a "V"-shaped insertion mark.

199 $\phi\hat{x}$] The symbolic notation has been inserted above the line with an insertion mark.

200 und] This word has been inserted above the line with an insertion mark.

204 war] The manuscript originally had "ist", which has been struck out and replaced above the line.

207 daß] The manuscript mistakenly has "das".

sich folgendermaßen:

$$\vdash: (\exists f) : \phi x.\equiv_x .f!x \quad \text{Pp.}$$

Dieses Axiom wird nun in einer ganz merkwürdigen Weise, deren richtige Auffassung durchaus nicht leicht ist, mit in die Klassendefinition aufgenommen, die dann die folgende Form annimmt:

$$f\{\hat{z}(\phi z)\}.=: (\exists\psi) : \phi x.\equiv_x .\psi!x : f\{\psi!\hat{z}\} \quad \text{Df}$$

Das heißt, $f\{\hat{z}(\phi z)\}$ soll bedeuten: Einer gewissen prädikativen Funktion $\psi!\hat{z}$, die durch dieselben Werte wie ϕ erfüllt wird, kommt die Eigenschaft f zu. Daß es stets eine solche Funktion $\psi!\hat{z}$ gibt, verbürgt eben das Axiom der Reduzierbarkeit.

Wenn auch diese letzte Wendung der Auffassung einige Schwierigkeit macht, zumal noch nebenbei die Bedingung, daß f eine extensive Funktion von ϕ ist, fallen gelassen wird, so ist in den vorangehenden Betrachtungen, wie ich glaube, doch das Wesen des Klassenbegriffs insofern genügend zum Ausdruck gebracht, als gezeigt ist, daß sich alle Schwierigkeiten, | die sich aus dem Klassenbegriff ergeben könnten, von vornherein und mit einem Schlage beseitigen 15.
lassen, sobald es nur gelingt, die Verwendungen des Wortes “Klasse” oder “Menge” einwandfrei zu definieren. Wie sich der wirkliche Aufbau der Arithmetik und der Mengenlehre dementsprechend im Einzelnen zu gestalten hat, ist natürlich eine andere Frage. Sie macht eigentlich den Hauptinhalt des ganzen Werkes aus.

Damit hätte ich die Behandlung des ersten grundlegenden Begriffes der mathematischen Logik wenigstens zu einem gewissen Abschluß gebracht. Ich hätte noch gern einiges über die weitere Entwicklung der Theorie gesagt, insbesondere über den nicht minder wichtigen, aber ungleich vielseitigeren Begriff der Relation und den Zahlbegriff; aber dazu würde die Zeit wohl nicht mehr ausreichen. Auf jeden Fall aber möchte ich die bisher behandelten Fragen zunächst einmal zur Diskussion stellen.

D.2 Die Russellsche Theorie der logischen Typen (1927)

Bedeutung der *Widerspruchsfreiheit* für irgend ein theoretisches Gebäude, or allem für die *Logik*. [1]

Widersprüche schon im Altertum. Lügender Kreter usw. Als ein typisches Beispiel das *Zermelo-Russellsche Paradoxon*. Für *Klassen* und für *Begriffe* (Begriff = Aussagefunktion.)

Sehr viele Versuche Russells, die Schwierigkeit zu beseitigen. Gewisse *Einschränkung* der vom rein formalen Standpunkt für das Denken vorhandenen Möglichkeiten.

“Mathematical logic as based on the theory of types” (Am. Journ. of Mathematics 1908.)

Dieser Ausweg durch die Widersprüche selbst nahegelegt. Einteilung der Begriffe in *Typen*.

Als Begründung bei Russell das *Zirkelprinzip* (vicious-circle principle): “Whatever inovlves *all* of a collection

225 insofern genügend] Before these words, the word “genügend” had been written once already, but struck out.

8 das] Before this word the typeskript has “Das”, which has been struck out (typographically with minuses).

must not be one of the collection." Oder: "If, provided a certain collection had a total, it would have members only definable in terms of that total, then the said collection has no total."

Nicht ganz klar. Was heisst "involve"? Das "only" ist verdächtig. Setzt das Ding selbst oder nur die *Angabe* des Dinges die Gesamtheit voraus? Je nach der Entscheidung dieser Frage hat man zwei Grade der Typentheorie.

Auch die Begründung ist etwas fadenscheinig. "the values of a function are presupposed by the function, not vice versa. … Thus for example the proposition 'Socrates is human' can be perfectly apprehended without regarding it as a value of the function 'x is human'. It is true that, conversely, a function can be apprehended without its being necessary to apprehend its values severally and individually. Etc." Man setze hier "*Begriff*" statt "Funktion"! | 2

Bessere Begründung: Prinzip des "*robusten Wirklichkeitssinnes*". Liegt schon der Typentheorie der PM zu Grunde. Zuerst ausdrücklich ausgesprochen in der "Einführung in die mathematische Philosophie". "Der Sinn für Wirklichkeit ist für die Logik eine Lebensfrage. … Bei der Durchführung einer Analyse von Sätzen mit Einhornen, goldenen Bergen, runden Quadraten und anderen derartigen Pseudo-Objekten (gegen Meinung!) braucht man einen sehr robusten Wirklichkeitssinn." (S. 171.) "Ich möchte behaupten, dass die Logik ebensowenig ein Einhorn zulassen darf wie die Zoologie; denn die Logik befasst sich geradesogut mit der realen Welt wie die Zoologie, wenn auch mit ihren abstrakteren und allgemeineren Eigenschaften." (S. 170.)

Was hat dies mit unserem Problem zu tun? Die *Wirklichkeit* ist natürlich *widerspruchsfrei*, d.h. die reale Welt, die *vor* dem Denken da ist, above the line with an insertion mark.noch nicht die "ideale" Welt der Begriffe, Zahlen usw. *Konkrete Sinnenwelt*, soweit sie noch nicht "von des Gedankens Blässe angekränkelt" ist. Von irgend zwei sinnvollen kontradiktorisch entgegengesetzten *Behauptungen über die reale Welt* ist notwendig *die eine wahr und die andere falsch.*

Beschränken wir uns auf Behauptungen über die reale Wirklichkeit, so haben wir *keinen Widerspruch zu befürchten.* Solange wir diesen *festen Boden unter den Füssen* haben, sind wir unbedingt gesichert.

Das Reicht der Ideen kann uns narren. *Kant*, transzendentale Dialektik. "Ein Kerl, der spekuliert, ist wie ein Tier, auf dürrer Heide von einem bösen Geist *im Kreis herumgeführt*."

Ist es möglich, mit Behauptungen über die reale Welt auszukommen? Oder ist dies vielleicht sogar notwendig? Natürlich steht eine derartige Erfahrungsbezogenheit *nicht im Widerstreit zur Apriorität.*

Kant: "Die Möglichkeit der Erfahrung ist also das, was allen unseren Erkenntnissen a priori objektive Realität gibt." "Folglich ist uns keine Erkenntnis a priori möglich, als lediglich von Gegenständen möglicher Erfahrung." "Also beziehen sich alle Begriffe und mit ihnen alle Grundsätze, so sehr sie auch a priori möglich sein | mögen, 3 dennoch auf empirische Anschauungen, d.i. auf Data zur möglichen Erfahrung. Ohne dieses haben sie gar keine

9 had] The typescript has erroneously "dad", which has been corrected in Behmann's handwriting.

26 noch] This word has been added (in Behmann's handwriting

34 dies] The typeskript has only "es", the "di" has been added by hand.

objektive Gültigkeit, sondern sind ein blosses Spiel, es sei der Einbildungskraft oder des Verstandes, respektive mit ihren Vorstellungen." "Alles Denken aber muss sich, *es sei geradezu oder im Umschweife*, zuletzt auf Anschauungen, mithin … auf Sinnlichkeit beziehen, weil uns auf andere Weise kein Gegenstand gegeben werden kann."

Wirklichkeitsnahe und -ferne Begriffe. *Flieger*! Wirbel bis auf die Erde. Auch in der dünnsten Luft der Abstraktion trägt uns der feste Boden der realen Wirklichkeit. *Rangordnung der Begriffstypen* je nach dem Grade der Abstraktion, also der Wirklichkeitsferne.

Enthypostasierung der Logik und der Mathematik. Verankerung in der Realität.

"*Immanente und transiente Mathematik*" (Klein). Voss: (aus einer Kritik Kants): "Durch dieses Vermögen der reinen Anschauung … wird nun allerdings die Frage beantwortet, wie die mathematischen Ideen … zugleich immanent und transient, d.h. nicht nur absolut gewiss sind, sondern auch eine widerspruchslose Gültigkeit für jede mögliche Erfahrung besitzen." (An sich könnte hiernach etwas theoretisch richtig, praktisch aber falsch sein!)

Schopenhauer geht in seinen Aeusserungen noch über Kant hinaus und rechtfertigt die Typentheorie im voraus:

"Wie aus dem unmittelbaren Lichte der Sonne in den geborgten Widerschein des Mondes, gehen wir von der anschaulichen, unmittelbaren, sich selbst vertretenden und verbürgenden Vorstellung über zur Reflexion, zu den abstrakten, diskursiven Begriffen der Vernunft, die allen Gehalt nur von jener anschaulichen Erkenntnis und in Beziehung auf dieselbe haben." "… so hat die abstrakte Vorstellung ihr ganzes Wesen einzig und allein in ihrer Beziehung auf eine andere Vorstellung, welche ihr Erkenntnisgrund ist. Diese kann nun zwar wieder zunächst ein Begriff, oder abstrakte Vorstellung sein, und sogar auch dieser wieder nur einen ebensolchen abstrakten Erkenntnisgrund haben; aber nicht so ins Unendliche: sondern zuletzt muss die Reihe der Erkenntnisgründe mit einem | Begriff 4 abschliessen, der seinen Grund in der anschaulichen Erkenntnis hat. Denn die ganze Welt der Reflexion ruht auf der anschaulichen als ihrem Grund des Erkennens." (Das Wort "Erkenntnisgrund" passt nicht recht[A]; es handelt sich nicht um das Erkennen, sondern um das *Verstehen*!) "Da nun … die Begriffe ihren Stoff von der anschauenden Erkenntnis entlehnen, und daher das ganze Gebäude unserer Gedankenwelt auf der Welt der Anschauungen ruht, so müssen wir von jedem Begriff, wenn auch durch Mittelstufen, zurückgehen können auf die Anschauungen, aus denen er unmittelbar selbst, oder aus denen die Begriffe, deren Abstraktion er wieder ist, abgezogen worden: d.h. wir müssen ihn mit Anschauungen, die zu den Abstraktionen im Verhältnis des Beispiels stehen, belegen können. Diese Anschauungen also liefern den realen Gehalt alles unseres Denkens, und überall, wo sie fehlen, haben wir nicht Begriffe, sondern blosse Worte im Kopfe gehabt. In dieser Hinsicht gleicht unser Intellekt einer Zettelbank, die, wenn sie solide sein soll, Kontanten in Kassa haben muss, um erforderlichenfalls alle ihre ausgestellten Noten einlösen zu können: die Anschauungen sind die Kontanten, die Begriffe die Zettel."

[A] In the left margin next to this line, Behmann has written "Gehalt!". Although that addition is not clearly inserted here, it makes sense to see it as Behmann's suggestion of a, to him, more satisfactory piece of terminology.

65 im] The typeskript has "Im", but the capital "I" has been corrected in Behmann's handwriting.

Die Paradoxien der Logik und die Antinomien der Philosophie als *Inflationserscheinungen*!

Der erste Grad der Typentheorie. Zwar nicht verbürgt, aber als Durchgangsstufe vorauszusetzen.

Bereich der ausserlogisch gegebenen *Dinge* (individuals). $a, b, c; x, y, z$. "We say that x is 'individual' if x is neither a proposition nor a function." (!) C. hielt hiernach früher auch die Klassen für "individuals"!

"Aussagefunktionen" mit Dingen als Argumenten. Ich sage dafür *Begriffe* von Dingen. *Eigenschaften* und *Beziehungen.* $f_x, g_{xy}; \phi_x, \phi_{xy}. | f\hat{x}, g(\hat{x}, \hat{y}); \phi\hat{x}, \chi(\hat{x}, \hat{y})$.

Begriffe von Begriffen von Dingen. Z.B. "mindestens einem Ding zukommen" ist eine Eigenschaft einer Eigenschaft Usf.

"Bäume" mit Dingen oder Aussagen an den Zweigenden. Die Dinge und die Aussagen bilden je einen Typus. | Näheres in "Mathematik und Logik". 5

Jede Variable bedeutet ein *Ding*, eine *Aussage* oder einen *Begriff von einem bestimmten Typus.* Es gibt also keine Aussage über Begriffe im allgemeinen oder Eigenschaften im allgemeinen.

Künstlicher Ausweg: Verwendung der *freien Veränderlichen* (real variable) in der Aussage. Dies widerspricht dem Wesen der Aussage.

Alle nicht den Typenvorschriften gemässen Formen werden *als sinnlos erklärt.*

Die *Umfangslogik* ist nach Russell nichts weiter als eine *Uebersetzung der Inhaltslogik* in einer anderen Sprache. (Richtiger: eines *Teils* der Begriffslogik.)

Dinge, *Klassen* und *Zuordnungen* (relations).

Die Typen genau wie vorher, nur dass an den Zweigenden lauter Dinge stehen. Der Aussagentypus hat hier keine praktische Bedeutung.

Elemente einer Klasse haben stets gleichen Typus, ebenso *homologe Glieder einer Zuordnung.*

Auch die *Nichtelemente*! $u \varepsilon v$ ist *sinnlos*, falls u nicht den Typus der Elemente von v hat.

Scheinbare Ausnahmen sind die *logischen Verknüpfungen* (Begriffe von Aussagen) *nicht*, *oder* usw. *Systematische Vieldeutigkeit.*

Damit ist der Forderung des "*robusten Wirklichkeitssinnes*" (zum mindesten in gewisser Hinsicht) bereits genügt. Jede überhaupt zugelassene Aussage, gleichgültig von welcher Ordnung, bezieht sich letztlich auf die *vor* dem Denken gegebenen realen Dinge, auch wenn ein Begriff (Klasse, Zuordnung) grammatisches Subjekt ist.

Insbesondere ist das *Zermelo-Russellsche Paradoxon* hierdurch *ausgeschlossen.* Von einer Klasse kann nicht sinnvoll ausgesagt werden, dass sie sich selbst als Element enthält oder nicht enthält.

Dagegen bleiben, wie es scheint, noch *andere Widersprüche möglich.*

Als Beispiel das *Paradoxon von Richard* (nach der von | *Berry* angegebenen Form). "Die kleinste nicht durch 6
weniger als hundert Buchstaben in deutscher Sprache angebbare natürliche Zahl".

---

85 einer anderen] The typeskript has "einer andere", which could be corrected either to "eine andere" or "einer anderen".

Natürliche Zahlen sind gewisse Begriffe zweiter (oder höherer) Stufe; wir können aber hier der Einfachheit halber so tun, als ob es sich um *Dinge* handelte. Wir können uns ja eine Tafel, auf der alle natürlichen Zahlen aufgeschrieben sind, widerspruchsfrei denken. Wenn wir lieber wollen, nur die Zahlen bis 30 hoch 100, unterhalb deren die fragliche Zahl sicher liegen würde.

Durchmusterung aller Kombinationen von höchstens 100 Buchstaben, *Markierung* aller derjenigen, die einen Sinn haben und zwar eine natürliche Zahl bedeuten. Dies können wir aber nicht *nicht immer entscheiden*. So würden wir beim Buchstaben D an die Kombination "—" gelangen. Also ein *Zirkel*.

Anderes Beispiel: *Paradoxon von Grelling und Nelson*. Scheinbar eine andere Einkleidung des Zermelo-Russellschen. "*Autologisch* und *heterologisch*".

Die Eigenschaftswörter können wir ebenfalls (mit einem gewissen Gewaltakt) als *Dinge* betrachten (*Wörterbuch*!), etwa ein Verzeichnis von ihnen angelegt denken.

Also *weitere Unterteilung der früheren Typen*. Z.B. die Eigenschaften von Elementen desselben Typus, etwa von Dingen, gehören nicht mehr zum gleichen Typus.

Der zweite Grad der Typentheorie. 1. Verschärfung. Im "*Am. Journ.*" und in der *Einleitung* der PM.

[B] Jeder *alte Typus* mit Ausnahme desjenigen der Dinge zerfällt in eine abzählbar unendliche *Folge von neuen Typen*. *Ordnung*snummern, jeweils von einer gewissen niedersten anfangend. Die *Dinge* haben die *Ordnung* 0, unter den Aussagen nur die "*elementaren Aussagen*", d.h. solche, die keine Variablen bzw. nicht "alle" oder "es gibt" enthalten. | 7

Allgemein bestimmt sich die *Ordnung* einer Aussage als um 1 höher als die Ordnung der *höchsten in ihr auf*tretenden (freien oder gebundenen) *Veränderlichen*. Diese Einteilung nach "Ordnungen" kreuzt sich mit der alten Einteilung nach Typen; hierdurch ergeben sich die neuen Typen. (Nicht streng richtig, da *Einschränkung d. Variablen*!)

Als Beispiel betrachten wir den früheren Typus der *Eigenschaften von Dingen*. $f\hat{x}$.

Enthält $f\hat{x}$ ausser x keine (gebundene) Veränderliche, so heisst es nach Russell eine "*elementary function*" (elementare Eigenschaft). Entsprechend ist $f(\hat{x},\hat{y})$ ein elementarer Begriff.

Sind f und g elementare Begriffe, so sind $(y).f(\hat{x},\hat{y})$[,] $(\exists y).f(\hat{x},\hat{y})$, $(y):(z).f(\hat{x},\hat{y},\hat{z})$ usw. nicht mehr elementar. *Alle* haben aber die Ordnung 1, d.h. alle derartigen Eigenschaften von Dingen (natürlich mit Einschluss der elementaren) bilden *einen Typus*, nämlich den der *Eigenschaften 1. Ordnung von Dingen*.

Wie x, y, z veränderliche Dinge sind, so mögen $\phi_1\hat{x}$, $\phi_1\hat{y}$ usw. *veränderliche* Eigenschaften 1. Ordnung von Din*gen* (d.h. natürlich: *Leerstellen* für derartige Eigenschaften) sein.

Dann gelangen wir zu formen wie

[B] Before this paragraph, another has been begun but struck out. It reads: "Als Beispiel betrachten wir den früheren Typus der Eigenschaften von Dingen. $f\hat{x}$.".

$(\phi_1).F(\hat{\phi_1}, \hat{x})$, wo $\phi_1 = \phi_1\hat{x}$,

$(\phi_1) : (\exists y).F(\hat{\phi_1}, \hat{x}, \hat{y})$ wo $\phi_1 = \phi_1\hat{x}$ oder auch $= \phi_1(\hat{x}, \hat{y})$

Alle diese bilden den Typus der *Eigenschaften 2. Ordnung von Dingen*. Diesen können wir seinerseits von einer Variablen $\phi_2\hat{x}$ durchlaufen lassen und damit zu *Eigenschaften 3. Ordnung von Dingen* aufsteigen usf.

Man beachte, dass mit dem Typus einer "Funktion" $f\hat{x}$ noch *nicht der Typus der "Werte"* von fx gegeben ist. So erhält man durch Einsetzen in eine Eigenschaft 1. Ordnung von Dingen *Aussagen 0. Ordnung oder Aussagen 1. Ordnung.*

Infolge davon kann z.B. ein Begriff von Begriff (eine Funktion von einer Funktion) *Werte* verschiedener Ordnung, also *verschiedenen Typus* haben. Es sei etwa $F(\phi\hat{x}) = \phi a$. Einsetzung der elementaren Eigenschaft $f\hat{x}$ gibt die elementare Aussage fa, während Einsetzung ei|ner nicht elementaren Eig. 1. Ordnung eine Aussage 1. Ordnung 8 ergibt.

Es gibt elementare Begriffe (nicht elementare Eig.en) von beliebiger Ordnung. Ein solcher Begriff heisst *Matrix*. *Prädikative* Begriffe. *Durchweg prädikative* Begr.

Die *Aussagentypen*. Abzählbare Folge, den Ordnungen entsprechend.

$(p).p\vee \sim p$ bedeutet nicht mehr dem *allgemeinen* Satz vom ausgeschlossenen Dritten. Dieser ist, wie fast alle Regeln der üblichen Logik, keine zulässige Aussage mehr. Höchstens $p\vee \sim p$. Sachlich überflüssige Unterscheidung von *all* und *any* (Frege!).

Paradoxien der Aussagenlogik. Z.B. "Lügender Kreter": "Das, was ich jetzt sage, ist falsch." Oder:

"Zweimal zwei ist fünf.

Der Friedensvertrag von Versailles ist ein Akt der Gerechtigkeit.

Alles, was auf dieser Tafel steht, ist falsch."

Bei den *Klassen* ist der Typus nicht mehr durch den der Elemente bestimmt. Es gibt jetzt *Klassen beliebiger Ordnungen von Dingen*, usw. Die Beziehungen $\subset$ und $=$ sind natürlich auch für Klassen verschiedener Ordnung sinnvoll, falls der *Rang* derselbe ist. Ebenso die *Beziehung sm*, bei der nicht einmal der Rang übereinzustimmen braucht. Natürlich sind $\subset$, $=$, *sm* usw. *systematisch vielsinnige* Beziehungen.

Praktisch natürlich *grosse Unbequemlichkeiten*. Bereits starke *Einengung* der Ausdrucksmöglichkeit und des Schliessens.

Es seien z.B. α und β zwei Klassen (etwa 1. Ordnung) von Dingen. Wir können uns denken, dass die verfügbaren Beziehungen 1. Ordnung von Dingen nicht ausreichen, um α auf β eineindeutig abzubilden, sondern etwas nur α

---

138 *oder*] The typeskript originally had "und", which has been struck out and replaced in Behmann's handwriting. Along with the "und" two occurrences of "teils", one before each occurrence of "Aussagen", have been struck out.

140 Begriff] The manuscript originally had "einem Begriff", but "einem" has been struck out.

156 der *Rang* derselbe ist] Before these words, the words "die Ordnung oder vielmehr" have been written, but struck out.

161 nicht ausreichen] The typeskript had initially "weder ausreichen", which has been struck out.

auf einen Teil von β, während es eine Beziehung 2. Ordnung von Dingen gibt, die α auf das ganze β abbildet. Dann wäre relativ zu abbildenden Beziehungen 1. Ordnung $\alpha << \beta$, relativ zu abbildenden Beziehungen 2. O. dagegen $\alpha sm \beta$. | 9

Von besonderer Bedeutung z.B. für die *vollständige Induktion* und die Arithmetik und Analysis der *reellen Zahlen*. Im ersten Fall kann man vermittelst eines Umweges durchkommen, im zweiten nicht. Es würde ein wesentlicher Teil der Mathematik abgeschnitten werden. *Weyl* und *Brouwer*.

Ausweg Russells: *Axiom der Reduzibilität*. Heute anscheinend von ihm aufgegeben.

Dieses Axiom wird u.a. in kaum einwandfreier Weise in die *Definition der Klassen und der Zuordnung* hineingearbeitet und dadurch beim Uebergang von der Begriffs- zur Umfangslogik die Unterteilung der Typen *künstlich wieder verwischt*, so dass man dann für die Praxis wieder auf den ersten Grad der Typentheorie herauskommt.

Der dritte Grad der Typentheorie. Im *Hauptteil* der PM. Weitere Verschärfung, nicht wegen der Paradoxien, sondern aus Gründen des *formalen Aufbaues*.

Wir sagen schon, dass die Elemente desselben Typus für die Theorie *nicht alle dieselbe Rolle* spielen (z.B. Eigenschaften 1. O. von Dingen!) und nicht einmal die Werte derselben dem Typus nach bestimmten "Funktion" *unter sich denselben Typus* zu haben brauchen. Die tiefere Veranlassung war aber wohl die folgende:

Fassen wir etwa die *Negation* $\sim p$ ins Auge! Es gibt ebensoviele Funktionen $\sim \hat{p}$, wie es Aussagentypen gibt. Nun möchten wir nicht gern unbegrenzt viele Grundbegriffe $\sim$ haben. Man wird also versuchen, mit der *Negation der elementaren Aussagen* als Grundbegriff auszukommen und die übrigen Negationen auf diese zurückzuführen. Entsprechend natürlich für die *Disjunktion*.

Im Falle der *Negation* verfährt man so: Grundbegriffe sind $\sim \hat{p}$, wo p eine elementare Aussage ist, (x) und $(\exists x)$. Man definiert nun:

$$\sim (x).fx :=: (\exists x). \sim fx \quad \text{Df},$$

$$\sim (\exists x).fx :=: (x). \sim fx \quad \text{Df}.$$

Ist f eine elementare Eigenschaft, also fx eine elementare Aussage, so kommen rechts nur Grundbegriffe des Systems vor, und die Negation ist damit vor Aussagen 1. Ordnung mit nur *einer* Veränderlichen erklärt. | 10

Die Definitionsformeln sollen nun auch dann gelten, wenn $f\hat{x}$ eine gebundene Veränderliche enthält. So erklärt sich z.B. $\sim (x) : (y).f(x,y)$ als $(\exists x) : (\exists y). \sim f(x,y)$, usf. Hatten wir eben zwei Definitionsakte, so kommen nun noch *vier* neue hinzu. So gelangt man schrittweise zu jeder endlichen Anzahl von gebundenen Veränderlichen innerhalb der Aussage 1. O.. (Es ist hier vorausgesetzt, dass die "alle" und "es gibt" stets am *Anfang* stehen; dorthin geraten sie aber von selbst, wenn man den entsprechenden Definitionsschematismus auch für die *Disjunktion* durchführt.

187 nun] This word has been added in Behmann's handwriting above the line with an insertion mark.

191 durchführt] The typeskript has "durchführen", which is not congruent with the grammatical subject "man".

(Neuerdings verwendet Russell nach *Sheffer* nur die eine Grundverknüpfung $p|q$ "nicht p oder nicht q".) Da man das Zeichen $\sim$ in $\sim(x):(y).f(x,y)$ durch das in $(\exists x):(\exists y).\sim f(x,y)$ erklärt hat, sind also, wenn man nicht einen *Zirkel* begehen will, die drei $\sim$ als *sachlich verschieden* anzusehen. Ebenso die in den beiden ersten Definitionen,
195 falls man *stückweise Definieren* (Frege!) vermeiden will.

Da es technisch nicht möglich ist, durch *einen* derartigen Definitionsakt die Negation für *beliebige Aussagen 1. O.*. zu erklären, also in der Form $\sim p$ die Veränderliche p im allgemeinen *nur einen Teil* ihres Typus im bisherigen Sinne durchlaufen kann, weil ja eine und dieselbe Negation $\sim$ nicht für alle diese Aussagen passen kann, bleibt nichts übrig, wie es scheint, als *die bisherigen Typen von neuem zu unterteilen* und die Wertbereiche der Veränderlichen
200 entsprechend weiter zu beschränken.

Wollen wir Zirkel im Definieren vermeiden, so haben wir (im Fall der Aussagen 1. O.) nur solche Aussagen zum gleichen Typus zu rechnen, die in der *Anzahl der* (gebunden) *Veränderlichen* übereinstimmen; wollen wir uns auch vor dem Vorwurf des "stückweisen Definirens" sichern, so haben wir überdies den *Charakter der "Operatoren"* gemäss ihrer Reihenfolge (auf die Buchstaben als solche kommt es ja nicht an!) in Betracht zu ziehen.

205 *W. und R. tun das erste, aber nicht das zweite!* Vgl. ihre Definition of [sic.]"being of the same type".

Den *Uebergang vom zweiten zum dritten Grad* der Typentheorie stellen sie als unbedeutend hin; er ist aber | 11
in Wirklichkeit *sehr einschneidend*. Beim Studium der Typentheorie hat man stets sehr darauf zu achten, ob man gerade die *Einleitung* oder den *Hauptteil* vor sich hat. Sonst gerät man in die grösste Verwirrung.

Gewisse Bedenklichkeiten ergeben sich insbesondere für das *Axiom der Reduzibilität*. Für die Umfangslogik und
210 die Arithmetik wird auch diese Verschärfung durch den schon erwähnten Gewaltstreich unschädlich gemacht.

Zur Würdigung dieser Schwierigkeit und Verwicklungen bedenken Sie, dass die Russellsche Definition der *Anzahl 1 eine* (gebundene) *Veränderliche*, die der *Anzahl 2 zwei Veränderliche* usf. enthält. In einem Typus hat also jetzt genau genommen nicht mehr als *eine* endliche Kardinalzahl Platz. Der Begriff $m < n$ *ist also unzulässig* und eine Aussage wie diese, dass, wenn m kleiner als n ist, n grösser als m ist, gemäss dieser zweimal verschärften Typenthe-
215 orie *gar nicht ausdrückbar*! W. und R. kommen hier gar nicht um einen *groben Verstoss gegen die Typenvorschriften* herum; doch bringen die diesen so versteckt an, dass der Nichteingeweihte ihn nicht leicht aufspürt.

Mit diesen Andeutungen möchte ich mich bezüglich des dritten Grades *begnügen*, da mehr die Zeit nicht erlaubt und ich glaube, dass Ihnen hiernach das Studium der Typenlehre der PM *kaum ernstliche – wenigstens keine*

---

192 (] The opening parenthesis, whose closing mate is placed at the end of this sentence is missing in the typskript. I have supplied it here.

197 im allgemeinen] These words were originally written one line below, but have been moved here with an insertion mark.

213 $m < n$] The formulas was initially written as "$\mu < \nu$", but has been stuck out and replaced above the line; both formulas are handwritten (by Behmann).

218–219 – *wenigstens keine grundsätzlichen* –] This parenthetical qualification has been added (in Behmann's handwriting) in the margin with a "V"-shaped insertion mark placing it here in the text.

grundsätzlichen – Schwierigkeiten mehr bereiten wird. Sagen möchte ich noch, dass der Ausdruck "*prädikative Funktion*" im *Hauptteil* natürlich etwas anderes bedeutet als in der *Einleitung*, da es sich jedesmal um einen *Typus* handelt. Er bedeutet nämlich in der Typentheorie dritten Grades nichts anderes als *Matrix*; d.h. ein *prädikativer Begriff* ist hier ein solcher, der *keine gebundenen Veränderlichen* enthält.

So ist die *Widerspruchsfreiheit der Logik und der Mathematik gerettet*, aber *um welchen Preis*? Ein künstlich aufgeführtes *Gerüst*, dessen Verständnis sehr viel schwieriger ist als das der Sachverhalte der Theorie selbst, die dadurch gestützt werden soll. Eine "*Metalogik*" über der eigentlichen Logik, die sich der beabsichtigten *Formalisierung und Mechanisierung* der logischen Reflexion notwendig entzieht und das Arbeiten viel *schwieriger* macht, als es vordem ohne Symbolik und Formalismus war. | 12

Damit ist der "*Aufbau*" der Typentheorie erledigt und ich kann nun zum "*Abbau*" übergehen.

Natürlich hat man viele *Einwände* gegen die Typentheorie wie überhaupt gegen die Russellsche Logik gemacht. Z.B. lehnt die *italienische Schule* der symbolischen Logik jede Typentheorie wie auch die Frege-Russellsche Erklärung des Zahlbegriffs strikt ab.

Ein Vorstoss von L. *Wittgenstein*. "Logisch-philosophische Abhandlung" (mit einem Vorwort von Russell), abgedruckt in Ostwalds "Annalen der Naturphilosophie" 1921 (letzter erschienener Band). Schüler und Freund Russells. R. ist in der letzten Zeit stark, ich möchte sogar sagen, bedenklich in W.s Fahrwasser geraten.

Verdienst Russells Betonung und Herausarbeitung des Standpunktes der *Inhaltslogik*. Umfangslogik nur als *Spezialisierung* und *zugleich Umformung* von ihr, während die frühere symbolische Logik (vor allem *Schröder*) allzu "extensional" eingestellt war und infolgedessen bei den Philosophen (wenigstens *mit* aus diesem Grunde) stark *in Misskredit* stand. Vom Standpunkt der *Mathematik* hat nämlich gerade die *extensionale Einstellung* unzweifelhaft manches für sich.

Denken wir uns irgend zwei Klassen α und β! Was heisst, dass beide *gleichzahlig* sind? "Es gibt eine eineindeutige Zuordnung (Beziehung) mit α als Vorbereich und β als Nachbereich." Unter den uns verfügbaren inhaltlichen *Beziehungen* oder den "an sich" existierenden *Zuordnungen*?

Oder nehmen wir eine *Klasse elementfremder Klassen*, unter denen die leere Klasse nicht vorkommt! "Gibt es" dann Klassen, die aus jeder dieser Klassen genau ein Element enthalten? Man wird sagen: An sich muss es notwendig solche *Klassen* geben; nur können wir im gegebenen Fall nicht immer eine derartige Klasse "*definieren*", d.h. durch eine *Eigenschaft* festlegen, mithin auf keine Weise für sich herausgreifen. Vom *Begriffsstandpunkt* wird man sagen: *ja*, vom *Klassenstandpunkt*: *nein*.

Dies ist nach meiner Ansicht die richtige Entscheidung. Das *Auswahlpostulat* hat in der Begriffslogik nichts | zu suchen und ist vielmehr beim *Uebergang zur Umfangslogik* als weiteres logisches *Axiom* hinzuzunehmen. 13

Die *Zweiheit von Inhalts- und Umfangslogik* besteht durchaus zu Recht. Es gibt eben *verschiedene* berechtigte "*logische Weltansichten*". Wir können gar nicht die Logik oder sonst eine Wissenschaft völlig *undogmatisch* aufbauen.

Irgendwo müssen wir nun einmal anfangen. Denken wir z.B. an die *Geometrie*! Punkte, Geraden, Ebenen. Wenn der Geometer sich erst lange den Kopf zerbrechen wollte: "Was ist eigentlich ein Punkt? Kann man immer entscheiden ob zwei Punkte noch verschieden sind? Was ist eine Gerade? Weg des Lichtes? Im homogenen Medium! Drehungsachse eines starren Körpers? Was ist dieser?", so würde er niemals zu dem Gebäude der Geometrie kommen, da er die eben gelegten Fundamente immer wieder unterwühlen und dadurch den schon begonnen Bau ins wanken bringen würde. Natürlich sind alles das interessante und wichtige Fragen, aber sie gehören eben *nicht in die Geometrie als solche*, sondern in eine andere Wissenschaft.

Aehnlich ist es in der Logik. Nehmen wir z.B. den Begriff des *Dinges* (als Erfahrungsdinges). Was wird nicht alles dagegen eingewandt! "Ist es etwas Objektives? Oder ist es ein Komplex von Empfindungen? Kann man immer eindeutig angeben, ob etwas ei Ding ist oder nicht? Ist ein Schlüsselloch ein Ding? Wieviel Dinge sind in diesem Raum?[C] Muss jedes Ding durch einen Begriff festgelegt sein? Was bedeutet, dass das Ding a mit dem Ding b identisch ist; kann man dieser Identität gewiss werden und auf welche Weise? Sind nicht statt der Dinge die Empfindungen das eigentlich Gegebene (und Wirkliche? Oder vielmehr die letzten Bestandteile der Dinge?"[D] Usw. Alle derartigen Fragen, so berechtigt sie an sich sein mögen, dürfen sich *nicht in den Aufbau der formalen Logik* hereindrängen. Vielmehr ist man gezwungen, *irgend eine bestimmte Einstellung*, mag sie auch nicht in jeder Hinsicht angemessen sein, zu wählen und diese folgerichtig durchzuführen, ohne sich mit allzuviel erkenntnistheoretischen Skrupeln und Zweifeln zu beschweren. Ob etwas Rechtes oder Brauchbares zustande gekommen ist, sieht man erst am fertigen Gebäude und ebenso, ob der Anfang der richtige war. Daher *keine falsche Scheu vor* | *dogmatischem* 14 (bewusst dogmatischem) *Anfangen*!

Es kann somit durchaus *mehrere gleichberechtigte "Einstellungen"* und folglich Systeme geben. Zwei solche gleichberechtigte Einstellungen sind nun die *intensionale* und die *extensionale*. Beide liegen nicht eindeutig fest, sondern sie lassen sich abschwächen oder auch auf die Spitze treiben. Das letzte hat *Wittgenstein* mit der extensionalen Einstellung getan. Er *extensionalisiert* bewusst *auch die Begriffslogk*. Er denkt sich die gesamte Tatsächlichkeit der Raumzeitwelt aufgelöst in *lauter Elementarsachverhalte*, von denen irgend zwei i ihrem Bestehen oder Nichtbestehen von einander unabhängig sind. Jede überhaupt mögliche Behauptung soll sich dann als Konjunktion aus solchen Elementarsachverhalten und Negationen von solchen atomar zusammensetzen. p und q sind dann identisch dieselbe Aussage, wenn sie in genau der gleichen Weise atomar aufgebaut sind, also die abstrakten Möglichkeiten des Weltzustandes und des Weltgeschehens in genau der gleichen Art einzuschränken.

So ist z.B: $p. \supset .p \supset q$ dieselbe Aussage wie $p \supset q$. Die *logischen Gesetze* sind dadurch ausgezeichnet, dass sie

[C] The typeskript originally had "Usw." here, but it has been struck out.

[D] The opening quotation mark closing here is missing. I have supplied one at the start of the series of questions.

278 sind] Before this word, the letters "si" have been written once already, then the word "scheinen", both of which have been struck out.

die realen Möglichkeiten überhaupt nicht einschränken. Sie sind folglich *alle identisch* und behaupten alle dasselbe, nämlich *nichts*.

Diese Theorie ist augenscheinlich *stark dogmatisch*, was freilich ihr gutes Recht sein mag. Aber sie ist nach meiner Ansicht für den praktischen Denkgebrauch *völlig ungeeignet*. In gewissem Sinne ist auch dies eine berechtigte Logik, aber eine Logik für allwissende Wesen, nicht für uns endliche Menschen. Für jene sind die Sätze der Logik und der reinen Mathematik tatsächlich nichtssagend, d.h. sie brauchen überhaupt keine Logik, haben gar keine Verwendung dafür, und insofern ist ihr System der Logik in der Tat das einfachste überhaupt denkbare.

Es versteht sich, dass zufolge dieser Theorie die *Ordnung* einer Aussage oder eines Begriffes keine Bedeutung hat, da irgend eine Aussage oder ein Begriff ja auf sehr viele Arten symbolisch geschrieben werden kann. So ist z.B. $(\exists x).x = a.fx$ (1. Ordnung) dieselbe Aussage wie fa (0. Ordnung). | 15

Infolgedessen ist mit der Wittgensteinschen Auffassung *allein der erste Grad der Typentheorie* verträglich.

Diese Folgerung hat F.P. *Ramsey* ("The foundations of mathematics". Proc. London Math. Soc. 1925) thatsächlich gezogen.

Wie aber steht es dann mit den *Paradoxien zweiter Art*? Hierfür hat *Ramsey* eine mit Wittgensteins Theorie verträgliche Auflösung gegeben (a.a.O.). Diese kommt darauf hinaus, dass *die Sprache* oder irgend ein Zeichensystem eine gewisse empirisch aufweisbare umkehrbar eindeutige Zuordnung zwischen allen "Gegenständen" im weitesten Sinne und einem gewissen Teil von ihnen darstellt oder vielmehr darstellen soll, dass diese aber aus bestimmten innerne Gründen (vor allem wegen des unvermeidlichen Durcheinanderwirkens von *suppositio realis* und *suppositio nominalis*) niemals streng eindeutig und somit *einseitig* durchführbar ist. Um dem Wesen dieser Abbildung beizukommen, zerlegt Ramsey die problematische allgemeine Zuordnung zwischen Zeichen und Bezeichneten in Teilzuordnungen, besser: er weist eine Folge verschiedener derartiger Zuordnungen auf, also *unter sich verschiedener Bedeutungen des Wortes "bedeutet"* selber, von denn jede in sich ordentlich und vernünftig ist, die man aber *nicht mit einander verwechseln* darf. So "bedeutet" z.B: "heterologisch" in gewissen Bedeutungen des Wortes "bedeutet" "autologisch", in anderen "heterologisch".

Bemerkenswert ist hierbei, dass diese Analyse der sprachlichen Abbildung formal völlig dem *zweiten Grad der Typentheorie* entspricht. Nur betrifft diese hier eben allein den sprachlichen Abbildungsvorgang, ohne dass die formale Logik als solche damit behelligt wird.

Ich weiss nicht sicher, ob ich die Darststellungen Wittgensteins und Ramseys in den Einzelheiten richtig aufge-

281 sind] Before this word, the letters "behaupt" have been written, but struck out.

283 sein mag] The typeskript originally had "ist", which has been struck out in pencil and replaced above the line in Behmann's handwriting.

298 bestimmten] The second "m" has been added by hand.

303–304 in gewissen Bedeutungen des Wortes "bedeutet"] Before the words the words "im" and "einen" have been written, but struck out.

fasst habe, sondern kann nur sagen, dass ich sie mir wie eben geschildert zurechtgelegt habe.

Es entsteht die Frage, ob nicht auch der erste Grad der Typentheorie entbehrlich, diese also überhaupt *vermeidbar* ist.

Nicht zu verkennen, dass die Typentheorie an sich wertvoll ist und *ausserordentlich zur Klärung beigetra|gen* 16 hat. “Incomplete symbol”, “definition in use”, z.B. in Anwendung auf den *Zahlbegriff*. Zahlen *nicht als Dinge* einer idealen “Gedankenwelt”, wie sonst üblich.

Hat man sich an die Typenlehre erst gewöhnt, so widerstrebt es einem, nun wieder konkrete Dinge, Begriffe, Zahlen usw. sozusagen in einen Topf zu werfen und zu einem allgemeinen Brei zu verrühren, wo “Dreieck, Wehmut und Schwefelsäure” in der gleichen Weise als “Gegenstände” eingehen. *Aristokratie* und *Demokratie*. Aber es würde sich beim Aufgeben der Typentheorie natürlich nicht darum handeln, dass man *von dieser Gleichberechtigung innerlich überzeugt ist*, sondern, dass man sie *praktisch anerkennt*. Es bildet sich ja auch kein vernünftiger Mensch ein, dass in einem demokratischen Staat die Menschen alle *wirklich* gleich und gleichwertig wären.

So soll nicht die Rede davon sein, dass der *Gedanke* der Typentheorie, der ja zweifellos durchaus einleuchtend und vernünftig ist, aufgegeben werden sollte, sondern allein, dass wir auf seine *ausdrückliche Beachtung* bei allem logischen Schliessen verzichten würden.

Dies würde praktisch, und zwar schon im Fall des ersten Grades der Typentheorie, eine *ganz bedeutende Erleichterung* sein. Es gäbe dann nur *eine* leere Klasse und nur *eine* Anzahl 1, und wir hätten sogar das *Recht, dies zu behaupten*, während wir früher von den verschiednen leeren Klassen und den verschiedenen Einsen nur “metalogisch” reden durften.

Der entscheidende Punkte ist natürlich, ob wir die Paradoxien durch irgend eine *andere, weniger einschneidende Massregel* unschädlich machen können. Und zwar handelt es sich hierbei wesentlich nur um die *Paradoxien erster Art*, da wir für die Paradoxien zweiter Art die von *Ramsey* gegebene Auflösung annehmen können, ohne uns darum seiner “extensionalen” Einstellung anzuschliessen.

Eine solche Auflösung der Paradoxien erster Art *ohne Inanspruchnahme* (natürlich nicht darum mit Verwerfung!) *der Typentheorie* scheint mir nun tatsächlich möglich zu sein.

---

310 nicht auch] Before these words, the word “dann” has been written, but struck out.

319 *ist*] This word has been added by hand above the line with an insertion mark.

321 durchaus einleuchtend] The typeskript originally had “etwas durchaus E[inleuchtendes”, but “etwas” and “E” have been struck out.

330 uns darum] Before the word “uns” the word “darum” has been written once already.