HENRI POINCARÉ'S THEORY OF CONVENTIONALISM

by

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PREFACE

Jules-Henri Poincaré (1854-1912) is universally acknowledged to have been one of the greatest scientific minds of the nineteenth century. The development of his genius from childhood precociousness was unusually smooth. By the end of his life he had been accorded virtually every international honour in the field of science.

A list of his philosophical writings is contained in the bibliography at the end of this thesis. However, to give the reader some idea of the vast range of his scientific creativity, included below is a list of his principal scientific works exclusive of several scores of articles and communications.

1. Calcul des Probabilités, Georges Carré, Paris, 1896.
2. Capillarité, Georges Carré, Paris, 1895.
3. Cinématique et Mécanismes, Cours publiés par l'Association amicale des Éleves et anciens Éleves de la Faculté des Sciences de l'Université de Paris (CPA), 1886.
4. Électricité et Optique (2 vols), Georges Carré, Paris, 1890, 1891.
5. Figures d'Équilibre d'une Masse Fluide, C. Naud, Paris, 1902.
6. La Théorie de Maxwell et les Oscillations Hertziennes, Georges Carre, Paris, 1899.
7. Lecons de Mecanique Céleste Professées à la Sorbonne (3 vols), Gauthier-Villars, Paris, 1905, 1907, 1909.
8. Lecons sur la Théorie de l'Élasticité, Georges Carré, Paris, 1892.

9. *Les Méthodes Nouvelles de la Mécanique Céleste* (3 vols), Gauthier-Villars, Paris, 1892, 1894, 1899.

10. *Les Oscillations Électriques*, Georges Carré, Paris, 1894.

11. *Potentiel et Mécanique des Fluides*, CPA, Paris, 1886.

12. *Théorie Analytique de la Propagation de la Chaleur*, Georges Carré, Paris, 1895.

13. *Théorie des Tourbillons*, Georges Carré, Paris, 1893.

14. *Théorie du Potentiel Newtonien*, C. Naud, Paris, 1899.

15. *Théorie Mathématique de la Lumière* (2 vols), George Carré, Paris, 1889, 1892.

16. *Thermodynamique*, Georges Carré, Paris, 1892.

TABLE OF CONTENTS

INTRODUCTION

THE PROBLEM OF HENRI POINCARÉ

This dissertation is concerned with the logical foundations of science. The history of science has been punctuated by a number of crises. These crises have occurred when the evolution of scientific theory appeared to move in a new and unsuspected direction. The scientific novelty seems, at such times, to signify more than a casual modification of the established tradition. It is as though there had been a qualitative change in the nature of science. Such crises are comparatively rare. The names of a few great figures in the history of science tend naturally to come to mind - Copernicus, Galileo, Newton, Maxwell, Planck, Bohr and Einstein.

The crises brought about by the scientific discoveries of these men produced a flurry of activity among philosophers. At such times, the questions: "What is science?," "What are the logical foundations of a scientific theory?" and "What is the relationship between a scientific statement and the world?" are posed.

Henri Poincaré was the philosophical interpreter of such a crisis in the history of science. In this case the crisis was in the field of geometry. Prior to the development of non-Euclidean geometry in the mid-nineteenth century, geometry had been regarded as perfect and unchanging. With the advent of non-Euclidean geometry, philosophers and scientists were compelled to reappraise the status of geometry and, in particular, its supposed relationship to physical space.

Let us trace briefly the development of non-Euclidean geometry during the nineteenth century, which paved the way for the philosophic theories of Poincaré.

The beginning of mathematics in the modern sense of that word is in the work of Euclid. His monumental achievement was to originate the axiomatic or postulational method. He discovered that all the known geometrical relationships, as well as many new ones, could be logically derived from a few simple assumptions or axioms. This endowed geometry with a marvelous simplicity and certainty. More particularly, the problem of verification was vastly simplified by Euclid's achievement. Prior to the work of Euclid, every geometrical proposition stood by itself. Euclid showed, however, that the truth of all geometrical theorems depended solely on the truth of the few axioms from which they could be derived. Thus, Euclid could be said to have reduced geometry to these axioms. Hence, the verification of geometry is simply resolved into the problem of verifying the axioms. In fact, this was no problem at all for Euclid in that he regarded his initial assumptions, the axioms, as self-evident or intuitively certain.

Poincaré points out that actually the initial assumptions of Euclidean geometry are by no means of a uniform type[1]. Specifically, some of them are purely analytic in character and do not properly belong to geometry as, for example, "Things which are equal to the same thing are equal to each other." The other group of axioms is peculiar to geometry, and of those one in particular stands out. This is Euclid's celebrated "parallels" postulate. It asserts that through one point only one parallel may be drawn to a given straight line.

The postulate of parallels had attracted the attention of mathematicians for many years. It has perhaps stood out as somewhat unsatisfactory because it leads to the notion of infinity.[2] Thus, it lacked the complete intuitive certainty that was expected from a Euclidean axiom. Hence, the system of Euclidean geometry which depends on this axiom has the shadow of doubt cast upon it. Undoubtedly, Euclidean geometry might have been improved in elegance and simplicity if this postulate could have been deduced as a theorem of the system. Many mathematicians attempted to demonstrate this postulate, i.e. to derive it

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[1] Science and Hypothesis, p. 35f. (All references will be to the Dover edition, New York, 1952).

[2] H. Reichenbach, Philosophy of Space and Time, p. 3.

from the other axioms, but all such attempts had to be ranked with the attempts to trisect the angle or to square the circle.

The birth of so-called "non-Euclidean" geometry came about when certain mathematicians, notably Lobatschewsky, Bolyai and Gauss, discovered that a consistent geometrical system could be constructed without the inclusion of this dubious postulate.

Lobatschewsky did not regard his geometry as a serious rival to the Euclidean system. In fact, it served principally to show the impossibility of demonstrating the postulate of parallels. He reasoned that if this postulate could be derived from the other axioms, then were one to reject it while retaining the other axioms as they are, self-contradictory theorems should follow as consequences. Thus, he assumes that through a point an indefinite number of straight lines may be drawn parallel to a given straight line. When this was combined with the traditional axioms of Euclidean geometry, there resulted a system of theorems which differed in many respects from Euclidean geometry but which appeared to possess complete internal consistency. For example, the angle sum of a triangle was found to be less than two right angles[3]. Moreover, the amount of the defect varied with the area of the triangle.

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[3]Cf., A. D'Abro, _The Evolution of Scientific Thought_, p. 35.

The next major development along these lines was made by Riemann. Lobatschewsky had constructed his system analytically by varying one of the fundamental postulates of Euclidean geometry. Riemann, on the other hand, approached the problem in a different manner. He realised that the most fundamental definition of a geometrical system is the definition of congruence. Once this definition has been given, the rest of the geometry must follow necessarily from it.

In ordinary experience, our definition of congruence is in terms of the behaviour of rigid bodies. A distance AB is regarded as congruent with a distance CD if a rigid measuring rod which is just equal to AB is also just equal to CD after it has been transported through space. We assume that the measuring rod has not contracted or "squirmed" during the transportation. However, one may very well ask how one can be certain that the measuring rod has undergone no deformation. Again, in everyday experience an answer is ready. A rod is regarded as rigid provided that it determines various distances as congruent in such a way that the resulting geometry of the region measured turns out to be Euclidean. It should be obvious that such a criterion is arbitrary.

Reichenbach[4] has pointed out some of the difficulties involved in determining congruence. It is impossible to

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[4] *Op. cit.* p. 16ff.

determine whether a rod has been transformed during transportation if this change were caused by universal forces[5] affecting the rod. If two rods were found to be equal when placed side by side, and were then transported to a distant region of space, by different routes, and were again found to be of the same length when placed side by side, it would not necessarily follow that they had been of the same length at all times during the transportation. "An expansion that affects all bodies in the same way is not observable because a direct comparison of measuring rods at different places is impossible."[6] It would be useless to appeal to optical experiments since then a similar assumption would have to be made about the propagation of rays of light.

The immediate consequence of the foregoing considerations is that the determination of congruence is not an empirical problem at all. That is to say, we do not "cognize" congruence but simply "define" it. Two rods which are normally regarded as equal might be defined as unequal such that one could be treated as though it were twice as long as the other. Doubtless, such a system would greatly

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[5] A universal force, in this case, would be one which permeated the whole of space; which could not be stopped or varied by a material barrier; and which would affect all material substances in precisely the same manner.

[6] Reichenbach, Op. cit., p. 16.

complicate our measurements. Nevertheless, from the logical standpoint, the new system would be just as admissible as the normal one. Thus, it emerges from the considerations of Riemann that congruence is determined by definition. It is a matter of convention. Mathematical space is completely amorphous. The system of geometry which we employ to describe space depends on our initial convention. This doctrine is referred to as the "relativity of geometry" or "the relativity of space."

Thus, the mathematician is presented with a number of different systems of geometry from which to choose the geometry of physical space. The situation was profoundly different from anything envisaged by Kant. For Kant, there was a single geometry which was imposed on phenomena *a priori*. The evolution of mathematics in the nineteenth century revealed that Euclidean geometry is merely one among many. What then is the relationship between geometry and the world? Can we attach any meaning to assertions that "space is Euclidean?" Those were the problems faced by Henri Poincaré.

In the following chapters we shall consider these and other related problems in greater detail, and attempt to clarify Poincaré's solution of them, about which there have been certain misunderstandings in the current literature of philosophy.

CHAPTER I

MATHEMATICAL AND PHYSICAL CONTINUA

Geometry may be described as the study of the spatial continuum. Thus, Poincaré develops his philosophy of science by first exhibiting the manner in which the spatial continuum is formed. This is prior to any considerations of the metrical properties of space.

The most elementary form of continuum is a series of numbers. We may begin with the series of rational or commensurable numbers. Each term in the infinite series is different from every preceding and succeeding term. Hence, strictly speaking, this series is not a continuum. That is to say, we cannot travel imperceptibly from one term to the next. If every point on a straight line could be represented by a rational number, it would follow that a straight line would not be continuous but would be an aggregate of separate, discrete points. Then it would be conceivable that two lines could intersect without a point on one line coinciding with a point on the other. For example, the hypotenuse of an isosceles right-angled triangle[7] would not intersect the

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[7]i.e., the diagonal of a square.

other two sides. If each side of the triangle were represented as being of unit length, then the hypotenuse, according to the celebrated theorem of Pythagoras, would be represented by $\sqrt{2}$. But $\sqrt{2}$ is an irrational number.

Such considerations led mathematicians to regard the irrational numbers as real, to admit their existence on the line. Thus, between each term of an infinite series, another infinite series of irrational numbers is interpolated. In this way we arrive at the notion of a mathematical continuum.

Poincaré now[8] considers the relationship between the mathematical continuum and the physical, i.e. sensory, continuum. One might at first suppose that the notion of a mathematical continuum were simply drawn from ordinary experience. Poincaré, however, succeeded in showing that there is a profound difference between them. Moreover, this difference is the result of the crudeness of sensory experience.

It has been established experimentally by Fechner that we are unable to distinguish the sensation caused by a weight of 10 gr. from that caused by one of 11 gr. Similarly, we cannot distinguish the sensation of 11 gr. from that of 12 gr. However, the sensation of 10 gr. is quite distinct from that of 12 gr. Thus, we find that sensory continua exhibit a curious relationship between their elements, which may be expressed as follows: $A = B$, $B = C$, $A < C$. This,

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[8] Science and Hypothesis, p. 22.

Poincaré writes, "may be regarded as the formula of the physical continuum."[9]

The above simply stated formula is plainly at variance with the principle of contradiction, and has necessitated the invention of the mathematical continuum which is free from any such contradiction. The mathematician will maintain that the contradiction is simply the result of the crudeness of our senses. Poincaré infers from this that, while the mathematical continuum is not imposed by our experience, experience has, nevertheless, suggested it to us. This, as we shall later see, is the germinal idea of his philosophy of the exact sciences, an idea which has been curiously overlooked by many modern writers who have quoted from his works.

The first stage in the creation of a mathematical continuum is to intercalate between A and B a discrete number of terms. However, if we had recourse to a more refined instrument of observation such as a microscope, the same difficulty would reappear. Under the microscope A and B would now appear to be distinct, but a new term D would appear which could not be distinguished from either A or B. To escape the contradiction we would postulate still more terms separating D from both A and B. But then a more powerful microscope would reveal new elements which could

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[9] <u>Idem</u>.

not be distinguished from the postulated intermediary terms. Thus we are led to interpolate more and more terms, ad infinitum. Sensory experience will always exhibit this inherent characteristic of the physical continuum with its associated contradictoriness.[10]

There is yet another difficulty connected with the physical continuum. A given length is indistinguishable from half of that length doubled by the microscope. Thus, the whole and the part are, in this sense, homogeneous. To resolve this contradiction it is postulated that there is a continuum of an infinite number of terms. Thus, the aggregate of whole numbers is equal to the aggregate of even numbers. At the basis of our mathematical reasoning is the assumption that an operation which has been performed once may be repeated, in the same way, an indefinite number of times. This is the principle of mathematical induction which Poincaré regards, curiously enough, as a genuine synthetic a priori principle.[11] Thus, once we have intercalated terms between two consecutive terms of a series, we feel that this operation may be continued without limit. Thus, Poincaré speaks of mathematical continua of various orders. The continuum of the first order would be represented by the aggregate of rational numbers. By intercalating the series

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[10] The Value of Science, p. 42. (All references will be to the Dover edition, New York, 1958).

[11] Science and Hypothesis, Chapter I, Passim.

of irrational numbers, we arrive at the continuum of the second order and so on.

We have already anticipated Poincaré concerning the origin of the mathematical continuum of the second order. He points out that, strictly speaking, it is only the latter which may be called a mathematical continuum.[12]

To summarise, we have shown that experiment has led the mind to construct a particular system of symbols which is called the mathematical continuum. The physical continuum contains inherent contradictions which suggest the necessity of a mathematical continuum. Thus, there is a relationship between the two. However, it should not be thought that the mathematical continuum is imposed on the mind. The mind freely constructs the mathematical continuum as a result of perceiving the contradictions inherent in the physical continuum. This, it should be noted, is not an *a priori* necessity. Furthermore, the physical continuum is imposed *a priori* no more than the mathematical continuum. The former is simply a contingency which arises from the crudeness of our sensory perception. Thus, contrary to the views of Kant, Poincaré would not regard the space-time form of the manifold as an *a priori* form of sensibility, but simply as a convenient and natural method of ordering the data which come to us from that faculty.

The next aspect of the continuum which we shall

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[12]*Ibid.*, p. 27.

consider is of great importance. It is, however, one of the more difficult aspects of the foundations of qualitative geometry, namely the problem of dimensionality. Poincaré deals with this subtle subject with masterful simplicity. However, the subject is inherently complicated and its exposition is not made easier by the fact that Poincaré gradually developed and improved his ideas on the subject throughout his works. The basic concepts are provided in Science and Hypothesis. The special problem of the origin of the three-dimensional character of physical space is dealt with in The Value of Science. The latter subject is taken up once more and fully developed in his posthumous Last Thoughts.

Poincaré developed the notion of multi-dimensional continua from the preceding considerations on the nature of the physical continuum. More specifically, it is based on the simple fact that any two aggregates of sensations are either distinguishable or indistinguishable.

We may now mention the technical terms which Poincaré introduces for the exposition of this subject.[13] A single aggregate of sensations will be called an "element." A "continuum" will be constituted by a linear series of elements, provided that it is possible to travel from any element in the series to any other via a series of connected

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[13] Ibid., pp. 31-32.

elements no one of which can be distinguished from its predecessors. An aggregate of elements is called a "cut", provided that the removal of this aggregate would be such that the remaining elements would no longer constitute a single continuum. Not every removed aggregate of elements would constitute a cut. If one of the remaining elements were indistinguishable from an element of the removed aggregate, then the latter would not have subdivided the former.

The number of dimensions of any continuum C will be defined by the necessary number of dimensions of the corresponding cut. In any continuum of n dimensions, the cut will be of n-1 dimensions. If the cut is composed of a finite number of distinguishable elements, the continuum will be of one dimension. The continuum which is of particular interest to the geometer, the three-dimensional continuum, could therefore only be subdivided by a cut which is itself a two-dimensional continuum.

Poincaré's treatment is highly abstract and it would facilitate our understanding if we considered a simple, concrete illustration. Let us imagine a series of musical notes. Each note is so close to its neighbouring notes as to be indistinguishable from them. This would satisfy the definition of a sensory or physical continuum. That is to say, it would be possible to travel from any note or element of the series to any other by way of an imperceptible series of transformations of pitch. Let us now imagine that one note of this series is removed. Then obviously it would no longer be possible to make the trip by imperceptible degrees. A discernible jump from one note to the next would be required to bridge the gap left by the missing note. Thus, the removed note would be a cut. Now this single note is obviously not a continuum. It has zero dimensionality analogous to that of the geometrical point. Thus, the original series of notes would be a continuum of one dimension.

Let us now imagine that in addition to variations of pitch, there are also variations of intensity or loudness in the aggregate of musical notes. For any pitch there are several degrees of intensity such that one could travel from one degree to any other through a series of imperceptible changes. We would now have a continuum of two dimensions. That is to say, it would be impossible to subdivide this continuum by the removal of a single note. Let the series of pitches be represented by consecutive letters of the alphabet A B C ... . Now let the series of sound intensities be represented by consecutive integers 1 2 3 ... . Let us suppose that the single note K7 were removed. Would it now be possible to go by imperceptible stages from J7 to L7? Obviously it would be. There is an indefinite number of possible routes which would, in fact, satisfy the condition of a sensory continuum. For example, from J7 the ear could travel imperceptibly to J6, then to K6, then to L6 and finally to L7. We can readily see that to render the transition from J7 to L7 impossible, it would be necessary to remove either all of the K's of all intensities or all of the 7's of all pitches. In either case, the cut would itself be a continuum, either of sound intensities or of pitches. This cut could be subdivided by the removal of a single note, and would therefore be one-dimensional. Hence, the original continuum must have been two-dimensional. One might then consider the addition of differences of tonality in the aggregate of notes. Then the cut would have to be a two-dimensional continuum of the type which has just been described. The total aggregate of notes would then be a three-dimensional continuum.

Finally, to justify this mode of defining the dimensionality of a continuum, we must consider whether it is compatible with the idea of dimensionality as actually employed by geometers.

> "Usually they begin by defining surfaces as the boundaries of solids or pieces of space, lines as the boundaries of surfaces, points as the boundaries of lines, and they affirm that the same procedure can not be pushed further.
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> "This is just the idea given above: to divide space, cuts that are called surfaces are necessary; to divide surfaces, cuts that are called lines are necessary; to divide lines, cuts that are called points are necessary; we can go no further, the point can not be divided, so the point is not a continuum. Then lines which can be divided by cuts which are not continua will be continua of one dimension; surfaces which can be divided by continuous cuts of one dimension will be continua of two dimensions; finally space which can be

divided by continuous cuts of two dimensions will be a continuum of three dimensions."[14]

The point of Poincaré's special language is that it is adapted to apply not to mathematical continua but to physical continua, for example to physical space, which, as the only representational space, is of much greater intrinsic, epistemological interest than mathematical space.

So far, no reference has been made to the notion of measurement, although the properties of the spatial continuum depend on this. Poincaré's four works on the philosophy of science are largely based on separate articles written at different times. It may sometimes, therefore, appear that his philosophy is unsystematic. This is not truly the case. As one instance of this, we may consider the germinal idea of the relativity of space which is hinted at, in passing, in his initial discussion of the continuum. Poincaré states that a criterion must be formulated to enable us to compare the intervals separating two terms in the continuum. That is to say, a definition of congruence must be provided. This, he tells us, requires the convention that the interval separating two terms A and B is equal to the interval separating C and D. By convention, it is assumed that all elements of the continuum are equidistant. "This definition is very largely, _but not altogether_, arbitrary."[15] Provided that the commutative and associative laws of addition

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[14] _The Value of Science_, p. 44.

[15] _Science and Hypothesis_, p. 28. (The italics are mine).

are satisfied, the choice is indifferent. We thus see how the consideration of the continuum leads naturally to his views on the relativity of space and geometry.

CHAPTER II

THE GENESIS OF THE VISUAL CONTINUUM

When we speak of space, we should make the distinction between physical or representational space and geometrical or mathematical space. This distinction is merely that which holds between the sensory and the mathematical continuum.

Poincaré distinguishes the most essential properties of mathematical space which are that it is continuous, infinite, three-dimensional, homogeneous and isotropic.[16] Visual space is found to differ fundamentally in its essentials from mathematical space. Poincaré considers an image on the retina of the eye. Admittedly, this image is continuous. On the other hand, it is enclosed within a limited framework and, moreover, it is not homogeneous.

The last point is very important and we should pause to consider it in greater detail. The mathematically naive person who considers the bizarre idea of a non-Euclidean geometry will probably argue that such geometrical systems may be of academic interest, but, nevertheless, Euclidean geometry has a privileged status as the geometry of the space

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[16] Science and Hypothesis, p. 52.

which we observe - real, physical space. He does not consider that we must learn to use Euclidean geometry. We must learn to adapt ourselves to it.

Primitive visual perception appears to have a metric of its own which differs from the metric of Euclidean geometry. This fact is actually quite well known but profoundly ignored not only by laymen but also by many philosophers. Kant, himself, would have done well to have considered it. I refer to the phenomenon of optical illusions.

A certain class of optical illusions depends on the fact that the retina of the eye has a non-homogeneous lattice structure. Hence, visual perception yields results which differ in a fairly definite manner from those of Euclidean measurements. If we observe a round disk such as a coin, it will appear to be slightly flattened at the top and bottom. When two straight lines are drawn side by side of equal length, one horizontal and the other vertical, the former will appear to be longer than the latter. We take these direct intuitions to be illusory when we find that they do not conform to the measurements which we make with our rigid Euclidean rulers. That is to say, we reject the only "natural" geometry there is, the geometry of primitive, visual perception. In its place, we select the more convenient geometry with its constant metric (for rigid bodies) which makes no distinction between the horizontal and the vertical.

Thus, we see, contrary to the beliefs of the mathematically naive, that if any geometry qualified as a privileged, "natural" geometry, one of its characteristics would be its use of a metric for a heterogeneous space.

To return to Poincaré, he appears to admit that representational space, like mathematical space, is three-dimensional. However, the third dimension or distance obviously cannot impress itself on the two-dimensional surface of the retina. We are able to appreciate distance through the effort of accommodation which we must make to focus on a distant object and also through the angle of convergence of the two eyes. That is to say, our awareness of the third dimension results from a muscular sensation, so that representational or visual space, unlike mathematical space, is not isotropic.

Poincaré finds it striking that the effort of accommodation and the convergence of the eyes are in complete agreement or harmony. In mathematical language, "the two variables which measure these two muscular sensations do not appear to us as independent."[17] "If two sensations of convergence A and B are indistinguishable, the two sensations of accomodation A' and B' which accompany them respectively will also be indistinguishable."[18] However, Poincaré maintains that we can only know this as an empirical fact.

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[17] _Ibid._, p. 54.

[18] _Idem._

There is no a priori necessity attaching to it. It is quite conceivable that convergence and accommodation could be independent. Then, so to speak, there would be an additional independent variable. For example, a being with senses like our own might be placed in a world in which light would have to pass through complex refracting media before reaching his eyes. Then convergence and accommodation would yield different results. Under such circumstances the visual, spatial continuum would be four-dimensional.

It appears that Poincaré is in error regarding the last point. Doubtless, he has frequent recourse to popular physiological considerations as the result of the medical background in his family.[19] However, he seems to reveal little appreciation of the evolutionary factor of adaptation which cannot be separated from any physiological question. An elaborate criticism of Poincaré's argument would have to be undertaken by a skilled physiologist, and is certainly beyond the scope of the present thesis. However, one or two extremely elementary remarks would not be out of place.

Firstly, there is no necessary mathematical relationship holding between the effort of accommodation and the angle of convergence. Moreover, for practical purposes, accommodation might be regarded as a purely qualitative sensation. Thus, it is surely obvious that we simply learn to associate a certain convergence with a given

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[19] His father was a medical doctor.

accommodation. Thus, in all cases, we should expect the two to be in complete harmony. The amount of effort which we must make to focus on a distant object will actually depend on the curvature of the lens of the eye. This, in fact, differs widely from one individual to another. A myopic or near-sighted individual will have to make a greater effort than a normal-sighted person. But this does not lead him to attribute a fourth dimension to representational space. If we could assign a set of values to the accommodation "variable," we should find that they corresponded to one set of angles of convergence for the myopic person and to another set for the normal-sighted individual. As the amount of myopia increases slowly during an individual's lifetime, he will gradually make the necessary adjustment in his association of ideas. It is sometimes the case that one eye is quite myopic while the other is normal. An individual so afflicted may wear spectacles to correct the discrepancy. If he wears them all day and then removes them at night, his visual perception will be confused. Even then the necessary re-adjustment may be made in a matter of seconds without resorting to a fourth dimension.[20]

Finally, if it were the case, as Poincaré seems to think, that there were a definite relationship between accommodation and convergence, amounting to more than mere association, the organism's power to adapt would ensure that

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[20] A personal experience of the writer.

the lens of the eye were of the right shape for the two to be in harmony. An individual in a hypothetical world where all light passed through refractive media would have a differently shaped eye from our own. On the other hand, if someone from this world were transported to the hypothetical world, he would immediately know that his confused perception of distance was the result of the presence of some such medium and would register no more surprise than we do when we perceive that a straight stick becomes bent when a part of it is placed in water. Fortunately, however, Poincaré's mistake does not affect the validity of his fundamental position that our intuition of the three-dimensional continuum is empirical and not given *a priori*.[21] In fact, he may have been aware of the difficulties involved since he offers a brief but more subtle argument for the three-dimensionality of visual space in *The Value of Science*.[22]

It should be noted that Poincaré has not really shown why the visual continuum is of three dimensions. He has merely argued that it must be less than four. Two of the three dimensions of space have simply been determined by the fact that the retina is a two-dimensional surface. This of course is clearly to beg the question, for one might then

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[21] Poincaré's lapse is all the more curious since, on the following page, he deals with motor space in terms of the association of ideas along the lines I have suggested. Cf., *Science and Hypothesis*, pp. 55-56.

[22] *Op. cit.*, p. 52ff.

ask: "Why do we represent the retina to ourselves as a two-dimensional surface?"

The complete solution to the problem of the three-dimensional character of the spatial continuum is not given through a consideration of pure visual space. Poincaré regards visual space as an abstraction, an "artifice."[23] Nevertheless, we shall see what he has to say about it.

Poincaré begins by considering the abstraction from the visual manifold of all those sensations which are red. These sensations will differ only as regards the point of the retina which they affect. Let us now imagine a line drawn across the retina so that it divides these sensations into two groups. Now all the red sensations which are on this line or which are so close to it as to be indistinguishable from them will obviously form a cut which divides the manifold of red sensations. That is to say, one could not pass continuously from a red sensation on one side of the line to one on the other side without choosing a route which would include one of the sensations in the cut. Let us call this cut C. Now, the aggregate of sensations affecting a single point on this cut would constitute a second cut C', since it would divide the aforementioned line. Poincaré argues that if C' has n dimensions, then C will have n + 1, and visual space will have n + 2.

If all the sensations affecting a single point could

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[23] Cf., The Value of Science, pp. 53&54.

be regarded as identical, this aggregate would not constitute a continuum. It would be of zero dimensions, in which case complete, visual space would be of two dimensions. Thus we see that a key question to be answered is whether or not it is possible to distinguish two sensations which are qualitatively indistinguishable and which, furthermore, affect the same point on the retina. The answer, of course, is that it is possible, provided that their perception involves differing sensations of convergence or of accommodation. Thus, it follows that C' is a continuum. The reader may note that in this later argument, Poincaré refers to convergence and the effort of accommodation but does not require their harmony.

We may now consider the cut C'' which would be the aggregate of sensations of C' accompanied by a given effort of accommodation or sense of convergence. This element would not be a continuum since its constituent sensations would be indistinguishable.[24]

One last point should now be considered. Poincaré does point out that if convergence and accommodation were not equivalent, it would follow that visual space were four-dimensional. His treatment of this matter in The Value of

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[24]Of course, they could in fact be distinguished by their temporal succession. Then a single sensation would be a cut C'''. This would give rise to a four-dimensional space-time continuum. The Value of Science, of course, predated the great work of Minkowski.

Science is, however, in line with the criticisms which I levelled against Science and Hypothesis. He asks the reader to consider whether his arguments show that experience has taught us that space has three dimensions. Poincaré answers this question in the negative. He admits that an optician could give us spectacles to create a fourth-dimension if this were true. Experience has simply taught us that it is convenient to attribute three dimensions to space. Now, of course, everyone knows that very well. It would seem, at first, that Poincaré's entire discussion of the dimensionality of space has been superrogatory, since the nature of space is purely conventional. Has this long discussion revealed nothing more than that we assume that space has three dimensions? This interpretation would naturally be unthinkable. Careful reading[25] will reveal that Poincaré simply meant that the harmony of convergence and accommodation is not experimentally given but is a useful convention. In short, it is the simplest convention which is suggested by the nature of experience.

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[25] Vide, The Value of Science, p. 54. Last paragraph but one.

<u>CHAPTER III</u>

THE GENESIS OF MOTOR AND TACTILE SPACE

According to Poincaré, complete representational space is more complex than visual space which is merely a part of it. More fundamental than visual space is what he chooses to call "motor" space. All of our movements are accompanied by muscular sensations. The framework to which we refer these sensations is motor space. Now, to each muscle there corresponds a specific sensation. Thus, it would appear that there would be as many variables connected with this space as there are muscles in the body. That is to say, if, for example, there were <u>n</u> muscles in the body, then space would be n-dimensional. The Kantian would argue that if the notion of space were dependent on movement, it is because a definite sense of direction is inherent in all our movements; that this sense of direction is imposed, <u>a priori</u>, on our muscular sensations as much as on our visual ones. Poincaré explicitly denies this view[26]. He argues that the sense of direction is not imposed <u>a priori</u> but arises through the association of ideas. Such an association is very complicated since the contraction of a muscle in my

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[26] <u>Science and Hypothesis</u>, pp. 55-56.

fore-arm, for example, may correspond to any direction depending on the general disposition of my body. It is, Poincaré maintains, the result of habit based on a large number of experiments. No single sensation could give rise to the concept of space. It is only through studying "the laws by which these sensations succeed one another," that we arrive at this notion.

The most pervasive feature of sensory experience is change. In general, we distinguish two fundamental types of change - change of state and change of position or displacement. Both types of change are conveyed to the mind in the same way, as changes in an aggregate of sensations. There is nothing in the nature of the sensory evidence itself to suggest what sort of change has actually taken place. Thus, if I perceive a circle which becomes a triangle, I may attribute the change of impressions to forces which have compressed the circle into a triangle or I may equally well attribute the change to the rotation of a cone. How, then, do we in fact distinguish a change of state from a displacement?

Let us first answer this question simply and directly. In the case of any displacement, the original aggregate of sensations may be restored through the appropriate voluntary motion. The motion will be such as to restore the object to its original position relative to oneself. The modification in the sensations is balanced by an inverse modification which will restore the original aggregate.

It follows that we may pass from the aggregate of sensations A to the aggregate of sensations B in two different ways. The modification will be either voluntary or involuntary. The voluntary modification corresponds to a displacement of the observer, whereas the involuntary modification, unaccompanied by muscular sensation, corresponds to a displacement of the object.

We are now in the position to understand the incomplete character of any account of space which limits itself to the visual continuum. On the basis of visual sensation, it would be impossible to distinguish between changes of state and changes of position. Let us imagine an observer who is incapable of any motion. He would be presented with a two-dimensional visual manifold. In this manifold, he would note that some sensations are more or less permanent while others undergo frequent modification. Let us now imagine that one day our hypothetical observer became aware of certain muscular sensations which accompanied changes in the form of the hitherto permanent, visual sensations, while, on the other hand, the changing sensations became relatively permanent. Our observer would have changed his original position and would eventually recognize this and interpret the novel phenomena in terms of objects' varying their distance from him in a third dimension.

So far we have made the distinction between changes of state and changes of position. We have noted that changes of state cannot be reversed by a voluntary movement whereas changes of position or displacements can be. This distinction is not altogether satisfactory. Poincaré draws a further fundamental distinction between what he calls "internal" changes which are voluntary and accompanied by muscular sensations and "external" changes which have the opposite characteristics.[27] Among external changes, some are capable of being corrected by an appropriate, voluntary motion of the body whereas others are not. It is in this way, specifically, that we distinguish between changes of state and changes of position.

Poincaré considers the following illustration. A sphere has one red hemisphere and the other blue. We are first presented with the blue hemisphere, and then the sphere rotates so that we are now presented with the red hemisphere. Let us now compare this situation with that of a spherical vase containing a blue liquid which becomes red as the result of a chemical reaction. We have received similar visual sensations, yet we interpret one as a displacement and the other as a change of state. In the first case, I am able, by walking around the sphere, to reestablish the original blue sensation, whereas in the second case I cannot.

Let us now consider a second sphere having a hemisphere of yellow and one of green. Originally a blue sensation was replaced by a red sensation. Now, after the

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[27] The Value of Science, p. 48.

rotation of the second sphere, a yellow sensation is replaced by a green one. Thus, we are presented with two distinct series of sensations, yet we regard them as the outcome of a similar displacement, the rotation of a sphere. Obviously, we are able so to do not because we have the right to set up a correspondence between blue and yellow and between red and green, but because in both cases the original sensation can be reestablished by a similar movement accompanied by similar muscular sensations. It is important to note that the similarity of muscular sensations in the two cases suffices for the conclusion. It is not necessary to know anything of geometry or to represent the motion of one's body in geometrical space.

Poincaré offers a second illustration.[28] Let us imagine that an object is in motion so that its image was first formed at the centre of the retina and subsequently at the border of the retina. The two sensations must be qualitatively distinct or one could not distinguish between them. How, then, is one led to postulate that the two distinct sensations are really one and the same image which has undergone a displacement? Simply because the object may be followed by the eye. It is possible to bring back the image to the centre of the retina, to reestablish the primitive sensation by a voluntary motion accompanied by a muscular sensation.

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[28] *Ibid.*, p. 49.

If the image of a red object moves from the centre A of the retina to the border B, and then the image of a blue object passes from A to B, one concludes that the two objects have undergone a similar displacement because precisely similar muscular sensations will accompany the two eye motions which are necessary to reestablish the original sensations. If the eye were incapable of being moved, I should not be in the position to state that the relation between red at the centre and red at the border was equivalent to that holding between blue at the centre and blue at the border.

> "I should only have four sensations qualitatively different, and if I were asked if they are connected by the proportion I have just stated, the question would seem to me ridiculous, just as if I were asked if there is an analogous proportion between an auditory sensation, a tactile sensation and an olfactory one."[29]

So far we have been considering external changes or those which arise without any voluntary motion of the body. Now we must consider Poincaré's treatment of "internal" changes. Firstly, Poincaré distinguishes between a simple displacement of the body in which the various parts of the body retain their initial positions relative to each other and those changes in which the parts of the body undergo a modification in their relative positions. The latter may be called a change of "attitude."[30] The two may be distinguished

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[29] *Ibid.*, p. 50.

[30] *Idem.*

by virtue of the fact that the former can correct an external change whereas the latter may bring about, at best, a partial correction. Poincaré stresses once again that this may be ascertained as an item of direct experience without any prior knowledge of geometry being presupposed.

It has sometimes been suggested that Poincaré has supplied the contemporary philosophy of science with its formal element while such thinkers as Ernst Mach supplied the empirical basis. It should by now be clear to the reader of this exposition that Poincaré believes that the fundamental notion of the spatial continuum has an experimental basis. It is much more than a formalistic convention. We shall later see how this is connected with the formalistic aspect of his philosophy which has been called "Conventionalism."[31]

To repeat, Poincaré denies that geometry is presupposed by these elementary, experimental facts. However, an elementary geometrical explanation could be provided if it were desired. An external object undergoes a displacement. If we desire the various parts of the body to resume their initial positions relative to this object, it would be necessary that they retain their original positions relative to each other. If the position of the eye changes relative to a finger, the eye can still be displaced in such a way that the original visual aggregate of sensations is restored.

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[31] Cf. Infra, p. 79.

However, the relative positions of the finger and the external object will then have been modified so that the original tactile sensations would not have been restored. Only the internal changes which correspond to the restoring of the original relative positions of the eye and the finger could be accompanied by the restoration of the original aggregate in all its aspects. This is, of course, an explanation which presupposes a good deal of geometry. Poincaré's point is that the awareness of the facts themselves requires no geometry.

A second pertinent consideration which Poincaré raises is that the same external change may be corrected by more than one internal change and therefore be accompanied by different sets of muscular sensations. This again is a primitive experimental fact. However, the following is a geometrical explanation of the fact. To move from position A to position B, one may take different routes. To one of these routes there will correspond a set of muscular sensations S while to another there will correspond a set S'' which may be completely different from S. Now, how is it that I am able to know that both correspond to the one displacement AB? The two series S and S'' will have but one common feature; both are capable of correcting the same external change. Thus, I may walk in a straight line from A to B. I then return by a straight path to A, so restoring the original aggregate of sensations. I then execute a series of pirouettes around the room until such time as I become aware, once again, of the second aggregate of sensations. I then know that I have once more displaced my body from A to B.

A more complicated situation is now considered.[32] Consider two different external changes as, for example, the rotation of the half blue, half red sphere and the rotation of the half yellow, half green sphere. Let us call the two displacements _a_ and _b_. They will be represented to us by two quite different changes of sensation, the passing of blue into red and yellow into green. Now we consider two series of internal changes S and S'' accompanied by sets of muscular sensations having nothing in common. Now, I happen to be in the position to assert that _a_ and _b_ correspond to the same displacement and that S and S'' also correspond to the same displacement. How is this possible? It is because I discover that S can correct both displacements _a_ and _b_, and that _a_ can be corrected by both S and S''. Now we may consider the following question: "If I have ascertained that S'' likewise corrects _b_?"[33] In answering this question, Poincaré states his position with greater force. He maintains that experiment alone can teach us whether or not the law is verified. If it were not at least approximately

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[32] _The Value of Science_, p. 51.

[33] _Idem._

verified there would be no geometry.[34] In fact, we would have no interest in making the distinction between a change of state and a change of position, and we would have no concept of space.

We may note with interest the difference between the somewhat contrived Kantian position and that proposed by Poincaré. The Kantian would argue that the law is verified by virtue of an *a priori* condition of sensibility which makes it so. According to Poincaré, on the other hand, it is an empirical fact that the law is true. What, then, for Poincaré is the relationship between experience and geometry? Poincaré refrains from adopting the radical empiricist position that geometry is learned from experience. The student of Kant will be fully aware of the untenable nature of this position which has been proposed by some empiricists such as Hume and John Stuart Mill. Geometry, as a formal system, requires the truth of this law. This entails neither that the law is *a priori* nor that geometry is empirical. It simply means that geometry can be applied in practice without fear of contradiction. In fine, experience does not teach us geometry but it does teach us that geometry is useful. Thus, once again, we see Poincaré's general position in the process of emerging. Geometry is a convention. But it is not an arbitrary one. It is a convention which has been

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[34]It is not however universally valid. If it were, there could be no geometry. Cf. *Science and Method*, pp. 110ff. (All references will be to the Dover edition, New York, no date.)

suggested by experience![35]

So far, in tracing the genesis of physical space, we have considered the visual continuum which was found to be a continuum of three dimensions. However, it has been pointed out that the consideration of a purely visual continuum is artificial and, to some extent, even arbitrary. We have just now analysed what Poincaré describes as the continuum of displacements. An element of this continuum was an internal change capable of correcting an external change. It has the property of a physical continuum since two internal changes may be so close as to be indistinguishable. The continuum or group of displacements is related to space but it cannot serve as an analogue of space since it is not three-dimensional. Poincaré[36] states that this continuum in fact, has six dimensions, although, unfortunately, he does not undertake the rather tedious task of demonstrating this. Thus, the genesis of the familiar notion of space is still not completely accounted for. To do so, we must first make a detour to consider certain questions regarding the notion of a point.

The spatial continuum is a manifold of points in three-dimensions. Each point is an element of the continuum. Adhering to what has already been said about continua in general, it follows that points in space must be normally

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[35] Science and Hypothesis, p. 50.

[36] The Value of Science, p. 57.

distinguishable but sometimes indistinguishable. But what do we mean by the identity of two points? How can we distinguish two points? What, in fact, is a point?

In the first place, it is not possible to represent a point to oneself, at least not in the simple manner that some people might think possible. When these people think that they are representing a point, they are, in fact, visualizing a very small object such as a tiny chalk spot on a blackboard. However, while there is indeed a difficulty here, it is not the most fundamental one. The crux of the problem concerns the representation of a specific point.[37] For example, if we agree that a point in space may be designated by a chalk spot on a blackboard, in what sense can one be in the position to say that the mark occupies the same position or is located at the same point after a period of time has elapsed?

Poincaré is of course making a simple reference to the relativity of position. The chalk mark will have travelled 30 kilometres from its original position after a period of one second as a result of the earth's motion. It is impossible to determine whether an object has retained its position in space during any period of time. In fact, the question is meaningless. Thus, we may only consider the relative position of the point. The most primitive consideration would be whether the point has retained its

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[37] *Ibid.*, p. 46.

relative position to oneself. If the sensations produced by the object differ from the original aggregate, as we have seen, the object has undergone a displacement or a change of state. If we can voluntarily restore the original aggregate, we conclude that the object did in fact undergo a displacement. Furthermore, if two objects have retained their relative position to one's own body, one may conclude that they have retained their position relative to each other. The latter consideration, as we have already stated, presupposes a knowledge of geometry, so that fundamentally we are only able to speak of the position of points relative to our own body.

Thus, it appears that a point may only be defined with reference to a coordinate system attached to one's own body. The localization of a point in this way does not, however, presuppose the notion of space. All that is required is that one represent to oneself the movements which are necessary to reach it. More precisely, one would represent the muscular sensations which would accompany such a motion. The muscular sensations, as such, would certainly not presuppose space. Hence, if two different objects successively occupied the same point in space, the impressions associated with them might be totally different. One feature, however, which they would share is that similar muscular sensations would accompany the movements necessary to reach them.

At this point, however, a possible difficulty appears to arise. As we have already noted, there are several different series of movements which could transport the body from A to B. The muscular sensations of the various series might have nothing in common with each other. How, then, can one know that the several representations are of a single, self-identical point? To have recourse to visual sensation is extremely tempting but, as Poincaré rightly points out,[38] this would actually multiply our difficulties. That is to say, we should then have to show how our visual apprehension of a given point corresponded to our motor apprehension of the same point. The problem of the identity of two points is obviously more difficult than it would first appear.

Let us suppose that during the interval between two instants of time, _a_ and _b_, the relative position of the various parts of my body have remained the same. At the instant _a_ a point in space had been occupied by object A; at instant _b_ that same point is occupied by object B. Now, what are the conditions which make such knowledge possible? Poincaré must necessarily introduce visual sensation into his considerations, since it is normally only through that medium that we can distinguish between two distinct objects.

At time _a_ I receive visual sensations which are transmitted through a fibre of the optic nerve. I attribute

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[38] _Ibid._, p. 47.

these sensations to object A. At the same time, I also receive tactile impressions of that object via a tactile nerve in one of my fingers. Similar considerations would apply at time b to object B. That is to say, impressions of B would be transmitted by the same optic nerve fibre and the same tactile nerve. The two sets of sensations corresponding to the two objects, A and B, may be qualitatively quite different. By what right then, do we suppose that they have been transmitted by the same nerves?

We shall shortly be in a position to see that tactile space is more important than visual space, so that a solution to this problem is not mandatory. However, Poincaré does offer a simple hypothesis which could suffice to explain the above point. He supposes that the object A produces two simultaneous sensations, a which is purely luminous, and a' which is coloured. Similarly B will produce the luminous sensation b and the coloured sensation b'. If a and b affect the same point on the retina they will be identical sensations. If a and b affected different points on the retina, we would say that objects A and B were in different regions of space, provided, of course, that the attitude of the eye was the same in both cases. However, a' and b' possess qualitative differences, so we would distinguish them in either case, therefore knowing that we were dealing with two distinct objects. However, the fundamental point is that even if this hypothesis were faulty, and neither Poincaré nor the present writer has any great desire to defend it, there must be something in common between a and a', on the one hand, and b and b' on the other. It is an experimental fact that some objects may be regarded as though they have successively occupied the same point of space while others may not be so regarded.

In order that we may judge two points to be identical, there are certain conditions, both visual and tactile, that must be fulfilled. However, the visual condition while necessary is not sufficient. The tactile condition is both necessary and sufficient. That is to say, the visual condition might be met without the two points being coincidental. The tactile condition could not, in this case, be fulfilled. The explanation in this case while still elementary is of a geometrical nature. Hence, it should only be regarded as a footnote to Poincaré's doubts about the aforementioned hypothesis. Lest the reader forget, it should be emphasized that the notion of physical space is not yet complete. It is not until it has been accounted for that one can even begin to discuss mathematical, i.e. geometrical, space.

O is a point on the retina where an image of object A is formed at time a. At instant a, the object is at a point M in space. Similarly, object B occupies a position M' in space at time b. The problem is to determine the visual and tactile conditions which must hold for M and M' to be identical. Now, vision is capable of acting at a distance. Consequently, the points M and M' could be identical provided that O, M, and M' were on a single straight line. However, M might be five feet from the eye while M' were five yards from it. Thus, while this condition is indispensable for the identity of M and M', as we stated above, it is obviously by itself insufficient. However, let us suppose that the finger is at point P in space at time a. It is discovered that P and M coincide. Then, at time b, it is found that the finger which has remained at P now coincides with M'. Since touch does not operate at a distance, it is concluded that M and M' must be identical. Hence, tactile space is more fundamental than visual space, at least for the determination of the identity of two points. However, at the primitive, experimental level, we may only determine that when the visual condition is fulfilled, the tactile condition may or may not be. But whenever the tactile condition is fulfilled, the visual condition invariably is.

Since these conditions are only experimental, it is possible to conceive that the positions of sight and touch might have been reversed. We would then conclude that touch can operate at a distance whereas sight cannot.

Our knowledge of the spatial continuum is enhanced by the fact that, in practice, we normally make use of more than one finger. At instant a, my first finger receives an impression which is attributed to object A. My body is then displaced with the corresponding series of muscular sensations S. After this displacement, my second finger receives a tactile impression which is also attributed to A. Later at instant b, after no physical displacement, my second finger receives an impression which is attributed to object B. Now my body undergoes a displacement corresponding to a series of muscular sensations S' which is completed at the time b'. Experimental evidence has assured me that S and S' are mutually compensating. That is to say, following S, the original aggregate of sensations will be re-established after a series of movements corresponding to S', and vice-versa. Now, the question which Poincaré considers is whether, at instant b', my first finger would receive impressions which could be attributed to object B.

A little elementary reflection will reveal that the answer to this question will be affirmative, provided that the objects A and B have not moved. I will not burden the reader with the details of the considerations which Poincaré provides at this point.[39] The important point to note, however, is that these considerations are of a geometrical nature. From the experimental standpoint, we recognize the truth of the conclusion but, at the same time, realize that a different conclusion is conceivable. The latter would merely modify our opinions concerning the use of sight and touch.

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[39] Ibid., pp. 60-61.

Before leaving this point, to anticipate Poincaré, for one moment, it should be noted that the condition attached to the geometrical reasoning is of the nature of a convention. We refer to the proviso that the objects under consideration should not have moved. In other words, if any experiment suggested that physical space were not a three-dimensional continuum with the familiar metrical properties of Euclidean space, we would counter with the assumption that the objects must have been displaced in the course of the experiment. This is an important point of which more will be said later.

CHAPTER IV

TACTILE SPACE AND THE AXIOM OF TRI-DIMENSIONALITY

In the Introduction, it was pointed out that non-Euclidean geometry originated from considerations concerning Euclid's postulate of "parallels." Consequently, this postulate receives a great deal of attention from philosophers of science, including Poincaré. However, there is another postulate of Euclidean geometry which is as fundamental and as deserving of attention for its philosophic implications. This is the postulate (or axiom) that space has three-dimensions. This is certainly at least as well embedded in our convictions about the nature of space as the "parallels" postulate. Poincaré is, therefore, obliged to show how this postulate is suggested experimentally in the genesis of tactile space.

So far, we have learned, in the first place, that it is possible to recognize the identity of two points at successive moments, provided that the body does not move. In the second place, even if the body does move during the interval between the two sets of impressions, provided these movements are accompanied by two sets of muscular sensations (S and S'), it would still be possible to recognize the identity of the two points, treating the body as though it had remained motionless.

Poincaré now proceeds to show that, given the aforementioned experimental conditions, it would follow that tactile space is a continuum of an indefinitely large number of dimensions. He then demonstrates the factors which permit us to reduce it to a three-dimensional continuum.

I distinguish two points in space occupied by the objects A and B by virtue of my finger's touching A at time _a_, and B at time _b_. The method by which I compare the two points is to consider the muscular sensations Z which have accompanied the movements of my body during the interval _ab_. From what we have noted in Chapter I about physical continua in general, it follows that the totality of different series Z would form a physical continuum with as many dimensions as there are Z's. On the basis of earlier considerations, we need not distinguish between the two series Z and Z+S+S', since S and S' cancel each other. However, the number of Z's will still be very great. To each of the series Z, there corresponds a point in space. That is to say, after a given movement, the tip of my finger will be at a definite point in space. Among these many points, some will be distinct, others identical.

Apart from the special case where Z = Z'+S+S', the cases where the points would be identical, there are those where the finger itself does not move. Thus, Poincaré distinguishes a sub-group of the Z's which he calls z. z represents the series of muscular sensations accompanying a bodily movement in which the finger remains motionless. Poincaré maintains that tactile space will have but three dimensions provided that we do not regard as distinct the series Z and Z+z. In ordinary language, this means that a series of movements corresponding to the displacement of my finger from A to B will be regarded as indistinguishable from an identical series of movements plus a further set of movements in which the finger is motionless.

Once again, Poincaré begins by offering a geometrical explanation which should, in any case, be obvious to the reader. Consider a surface A in space. On the surface A let there be a line B, and on the line B let there be a point M. Let C_0 represent the aggregate or totality of Z's. C_1 represents the totality of Z's in which the finger-tip remains on surface A. C_2 is the aggregate of Z's in which the finger-tip remains on line B, while C_3 represents the aggregate of Z's in which the finger-tip remains at the point M.

It is apparent that C_1 is a cut which divides C_0. That is to say, if we removed C_1, it would not be possible to move from any point in space to any other point. Similarly, C_2 is a cut which divides the surface A so that one could not move from any point on the surface to any other point. In like manner, C_3 will be a cut which divides C_2. Again, following what was determined in the preceding chapter, if C_3 is a cut of n dimensions, C_o, which is the aggregate of possible motions in space during the interval ab, will have n+3 dimensions. Obviously, our task is to show that C_3 is of zero dimensions.

Now the reader will recall that C_3 will only fulfill this condition provided that it does not consist of a series of elements such that the difference between two adjacent elements would be imperceptible. That is to say, C_3 must not be analogous to a series of musical notes of the same intensity and tonality but of varying pitch. In short, C_3 must not be a continuum of elements. Now, this condition is fulfilled by C_3 provided that we agree to treat Z and Z+z as indistinguishable. Then all of the several series of sensations in which the tip of the finger remained at M would be indistinguishable. C_3 would not be a continuum, and C_o or tactile space would have three dimensions!

This ingenious derivation of the number of dimensions of space is obviously geometrical. From the primitive, experimental standpoint, why should we have singled out the series z? The answer is of the utmost simplicity. The series z stands out by virtue of the experimental fact that the tactile sensation received at the beginning of such a series of muscular sensations will usually be identical with the tactile sensation at the end of such a series. Thus, if I touch a piece of silk, I receive a characteristic tactile impression. I then execute a series of movements at the end of which the same characteristic feeling of silk remains. I touch a piece of glass. I then execute an identical series of movements and the characteristic impression of glass is found to persist. After much experiment, I conclude that the series of muscular sensations z, corresponding to these movements is such that it does not alter the tactile impressions which are received by a given finger.

There are, of course, those cases where the original impression does not persist. We should explain this geometrically by saying that the piece of silk or piece of glass was displaced. We are not, however, entitled to offer this explanation prior to any knowledge of geometry. Poincaré is content to point out that so long as the experimental condition usually holds, it is sufficient to induce us to regard z as corresponding to a special type of displacement.

In the final paragraph[40] of this section, Poincaré inserts a comment to the effect that while muscular sensations inform us of the movements of the body, the final position of the body depends not only on these movements but also on the position from which it began. However, there is no sensation to inform us of the initial position. This, in itself, suffices to make the relativity of spatial position apparent. We shall have more to say of this in the following

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[40] The Value of Science, p. 65.

chapters.

It would be appropriate at this point in our exposition to consider how one might characterize the position held by Poincaré. Poincaré has discussed the genesis of the notion of space in a manner which would seem to place him directly in the tradition of the empiricists. He has, in effect, shown that the notion of space arises through a complex association of ideas. More precisely, he has shown that we associate various muscular sensations with external impressions. The correlation of these two sets of data is readily accomplished through the medium of a three-dimensional continuum. Are we then to classify Poincaré's position with that of Hume, Mill and Spencer? We should defer any definite conclusion until we have learned more of his philosophy. This, however, should be carefully considered. Neither Hume nor Kant made the explicit distinction between representational and mathematical space. Poincaré has so far been discussing purely representational space and, thus far, his position has indeed been empirical. However, this space has no definite metric. We cannot call it Euclidean or Lobatschewskian. It is a continuum formed by the correlation of tactile, visual and motor impressions. Thus, before passing any final judgement, we must consider the conditions and manner of endowing space with a metric. We shall see that it is here that Poincaré reveals features of his philosophy which set him apart, albeit subtly, from the classical empiricists.

To resume our exposition of Poincaré's views, we must now consider the manner in which the various aspects of representational space are correlated. That is to say, we must learn how Poincaré treats the relationship between visual and tactile space. Actually, Poincaré does not deal with this question explicitly. He merely shows how one should set about it. From what he has said of tactile space, it follows that each of our fingers generates a three-dimensional, spatial continuum. Poincaré shows how we arrive at the identity of two tactile continua in a manner which is consistent with his other views. This, at least, gives us the clue to understanding how tactile space could be correlated with visual space.

Poincaré considers two three-dimensional, physical continua C and C' which are generated by two fingers D and D'. An element of such continua is a point in tactile space. To each of these elements there corresponds a series of muscular sensations Z. There will also be series of sensations of the type Z+z corresponding to the same point or element. Similarly, in the continuum C', there will be a series Z' corresponding to each element, and also a series Z'+z'. We distinguish z from z' because z preserves the tactile impressions of D while z' preserves the tactile impressions of D'. Finally, as before, S and S' are inverse or mutually correcting series of sensations.

Let us now consider the following experimental data. The finger D' receives a tactile impression A'. I execute movements corresponding to the series S. Then finger D feels the impression A. I then execute movements corresponding to _z_. I continue to receive the impression A through my finger D. I now make the motions corresponding to S. Hence, once again, finger D' feels the impression A'. In other words, the series of movements corresponding to S+_z_+S' (_in that order_) preserves the impressions of finger D'. By definition, we note that S+_z_+S' belongs to the series _z_'. _Mutatis mutandis_, S'+_z_'+S will be a series of type _z_. Provided that S is suitably chosen in the series S+_z_+S', by varying _z_ in every possible way, we may obtain every possible series of the type _z_'.

As an aid to the reader, it would be appropriate to consider a concrete example of such an experiment. Before me is a narrow strip of silk. By stretching my right arm straight out, the tip of the index finger (D') of my right hand comes in contact with the strip of silk, receiving the impression of it (A'). I then move my arm slightly to the left, experiencing a series of muscular sensations (S) in so doing. My middle finger (D) is then in contact with the strip of silk which transmits a second tactile impression (A). I bend my knees slightly and, at the same time, tilt my right arm upwards so that my middle finger remains unmoved, in contact with the silk. The bending of the knees together with the upward tilting of my arm is accompanied by a series of muscular sensations (<u>z</u>). Then I move my arm slightly to the right, experiencing more muscular sensations (S'). My index finger (D') is once more in contact with the silk strip, which again causes its characteristic, tactile sensation (A').

Poincaré offers the usual, geometric explanation. It is not necessary for us to repeat it, since it is implicit in the concrete illustration. The essential point to note is that, on condition that the strip of silk has not moved, we would naturally suppose that the tip of the index finger has occupied the same point in space as the tip of the middle finger. In other words, <u>for any point in a given spatial continuum, there will be a corresponding point in a second spatial continuum</u>.

To resume the exposition in Poincaré's own language, there is a series of muscular sensations Z which corresponds with a point M in the first tactile space. To the series S+Z+S', there corresponds a point N of the second space. Poincaré now must show that M and N are corresponding points. Since these are arbitrarily selected points, this would amount to demonstrating that <u>every</u> point in the continuum C has a corresponding point in the continuum C'. In other words, C' would be a transformation of C.

The notion of a geometrical or co-ordinate transformation is readily understood. Let the continuum

C be represented by the surface of a flexible rubber ball. Let us now squeeze the ball, without stretching it, so that its shape is deformed. The new surface will represent C'. The reader will immediately perceive that for every point on the first surface there is a corresponding point on the second surface. The second surface is then a transformation of the first. Had we drawn a triangle on the surface of the ball, then the geometrical form on the ball, after being squeezed, would be a transformation of that triangle.

Poincaré's intention is now more readily understood. He wishes to show that the space engendered by one finger is identical with that engendered by a second finger. Firstly, however, he must show that one is a point transformation of the other.

To establish that M and N are corresponding points in the sense we have considered above, the following condition must be satisfied. It must be shown that whenever two points M and M' are identical in the first space, the two points N and N' will be identical in the second space. Since S and S' are mutually correcting series, it follows that $S+S' = 0$.

Hence, $S+S'+Z = Z+S+S' = Z$ I

Also, $Z+S+S'+Z' = Z+Z'$ II

These relationships are obviously not of the commutative variety. The order of the corresponding movements is significant. Hence, we are not entitled to assert the following:

$$S+Z+S' = Z$$

It is necessary to show that Z and Z' correspond to the same point M = M' in the first space. To do so, it is sufficient to show that $Z' = Z+\underline{z}$.[41] We should then be able to write:

$$S+Z'+S' = S+Z+\underline{z}+S' = S+Z+S'+S+\underline{z}+S' \quad \ldots\ldots\ldots\ldots\ldots\ldots \text{III}$$

The reader may verify the above relationships by choosing a simple movement of the finger for each symbol. He will find that after each of the three series of movements, his finger will be in the same place.

It was shown above[42] that $S+\underline{z}+S'$ was a series of the type $\underline{z}'$. Substituting $\underline{z}'$ for $S+\underline{z}+S'$ in III, we obtain

$$S+Z'+S' = S+Z+S'+\underline{z}' \quad \ldots\ldots\ldots\ldots\ldots\ldots \text{IV}$$

That is to say that S+Z'+S' and S+Z+S' correspond to the identical point in the second space.

The conclusion of this deduction is a crucial one. It is therefore, necessary to review the reasoning with care. Poincaré wishes to show that two spatial continua are isomorphic. If they are, whenever two points in the first space are identical, two corresponding points in the second will also be identical. We have agreed, by hypothesis, to regard S and S' as two series of sensations which are inversely related or mutually correcting. Thus, the relationships I and II are self-evident.

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[41] The reader will recall that $\underline{z}$ is a series of the type which preserves the aggregate of sensations.

[42] *Supra*, p. 54.

We recall that Z corresponds to a point M in the first space, and Z' corresponds to a point M' in the first space. S+Z+S' is an arbitrarily chosen series of impressions corresponding to a point N in the second space, while S+Z'+S' corresponds to a point N'. Hence, Poincaré must show that whenever Z and Z' lead to an identical tactile impression, the series S+Z+S' and S+Z'+S' will also result in the identical tactile impression.

If Z and Z' correspond to the same point (M=M') in the first space, then Z' = Z+<u>z</u>. This, again, is true by definition. Let us then assume that Z and Z' are such series. On the basis of this assumption, the set of relationships III is a necessary consequence through the simple algebraic substitution of Z+<u>z</u> for Z'.

It has already been established that S+<u>z</u>+S' is one of the series of the type <u>z</u>'. Moreover, it was noted that by varying <u>z</u> in every possible way, one could obtain every possible series of the type <u>z</u>'. Hence, the substitution of <u>z</u>' for S+<u>z</u>+S' is legitimate. By making this substitution in III, we obtain the relationship of IV.

S+Z'+S' = S+Z+S'+<u>z</u>'

But <u>z</u>' is, by definition, a series of sensations which preserves the initial impression. Hence, it may be disregarded. We conclude that S+Z'+S' and S+Z+S' correspond to the same impression or the same point in space N = N'.

This demonstration proves that whenever M and M' are identical points in space C, N and N' will be identical points in space C'. In other words, these two tactile spaces are isomorphic.

For the benefit of the sceptical reader, let us resort once more to an intuitive illustration. Let us begin, as before, with my right arm outstretched and my index finger extended to touch a strip of silk. To attain this position I have undergone motions corresponding to the sensations Z. Then, as before, I bend my knees and tilt my arm slightly upwards. These movements are accompanied by the sensations <u>z</u>. It should be noted that <u>z</u> could have been a movement of my index finger. For example, I could have moved it in such a way that its tip would have described a full circle whose plane is normal to the plane of the piece of silk. In any case, the reader can see that both the movement corresponding to Z and that corresponding to Z+<u>z</u> will result in my index finger's touching the piece of silk. We give Z+<u>z</u> the name Z'. Experience teaches us that Z and Z' are equivalent.

I now perform a similar experiment with my middle finger. The second experiment is slightly more complicated by virtue of the presence of S and S', but these are any suitable motions which are self-cancelling as, for example, one step backward and one step forward. I ascertain that whenever Z and Z' correspond to the same point for my index finger, S+Z'+S' and S+Z+S' will correspond to the same point for my middle finger. Suppose that to the left of the strip of silk there were a strip of glass. If Z corresponded to the tactile sensation of silk in my index finger, while Z' corresponded to the tactile sensation of glass, then I should also find that whenever S+Z+S' corresponded to the feel of silk in my middle finger, S+Z'+S' would correspond to the feel of glass in that finger. Such experiments would lead me to believe that a point correspondence existed between the space of my index finger and that of my middle finger.

The foregoing intuitive considerations have actually anticipated Poincaré's final conclusion. Not only do I regard the spaces as corresponding but also as identical. I have learned experimentally that a series of movements which preserves an impression at point M for my index finger will also preserve an impression at point N for my middle finger. Sometimes, however, the impression will not be preserved. In these exceptional cases I assume that the object has moved. Moreover, I find that whenever the series of movements fails to preserve a tactile sensation for my index finger at M, it likewise fails to preserve a tactile sensation for my middle finger at N. I assume that the two sets of tactile impressions are caused by one and the same object which must occupy both M and N. In other words, M and N are one and the same!

CHAPTER V

CONVENTIONALISM AND THE GEOMETRY OF SPACE

We have so far learned how Poincaré accounts for the origin of space in terms of primitive, experimental data. All we know of it is that it is a physical continuum of three dimensions in which we represent physical objects and their movements. This primitive space is, so far, devoid of all metrical properties. Poincaré has not told us how to measure distance or, for that matter, how to determine direction. However, the most fundamental and crucial question for the philosophy of science is whether space is relative or absolute.

This problem is partly concerned with the locating of objects in space. We say that an object is here or there. What do we mean? If the position of an object is determined by its relationship to space itself, then space is absolute. If all material objects were removed from it, absolute space would continue to exist unchanged. If, on the other hand, we are only able to assign positions to objects relative to other objects or to ourselves, then space is relative. If there were no material objects, the concept of space would be devoid of significance.

It would perhaps be in order at this point to interpolate a comment about the term "relativity." This word has a number of distinct meanings, philosophical, mathematical and physical. In traditional philosophy, the distinction was made between the relative and the absolute. A relative quality would be one whose existence depends on a relationship with something else. Absolute existence or reality exists in and through itself. In traditional metaphysics, the absolute was often called "substance."

The philosophical distinction between the relative and the absolute will not, however, enter into our considerations. More important for us is the distinction between mathematical and physical relativity. In the following chapters, we shall have occasion to refer to both, and it would be advisable to be certain of which type of relativity we are speaking. Mathematical relativity is primarily, although not wholly, relativity of position. The position of an object or point is said to be relative to a system of coordinates. It should be noted that this type of relativity is not discoverable. In a very real sense, it is given *a priori*. This distinguishes it from physical relativity which is empirical. Whether a given physical magnitude is relative or absolute depends on experimental investigation. We cannot, as Einstein has clearly shown, treat a physical magnitude as absolute on *a priori* grounds.

Let us consider a simple illustration. Weight is regarded as a relative magnitude since it varies with the distribution of matter in its immediate vicinity. For example, an object which has a given weight at the equator will have a slightly different weight at the poles. Its weight would be considerably less if it were transported to the moon. In more technical language, the weight of an object will depend on the gravitational potential in the region of space which it occupies. Hence, the reader will see that one cannot make an absolute assertion about the weight of an object. One can only make a statement about the weight of the object relative to the potential of the gravitational field where the object is located.

On the other hand, classical physics regarded mass as an absolute quantity, an inherent feature of matter. There were excellent grounds for this belief at the time, since no variation in the mass of an object had been detected. Then, in the late nineteenth century, it was found that electrons moving with high velocities did undergo an increase in mass. This was one of the many experimental facts which eventually led to the abandonment of classical physics. The classical physicists were not wrong to have supposed mass to be absolute. Their error was to suppose that the principle of the conservation of mass was a necessary truth. It was in fact only an experimental truth which was eventually falsified. This, then, is the crux of the distinction between mathematical and physical relativity.

Unfortunately, Poincaré does not seem to have made so explicit a distinction. However, he did speak of both types of relativity. In fact, it is even possible that to some extent he confused them. In any case, in our exposition of Poincaré's philosophy, we shall try to separate the two as clearly as possible. However, in line with the general nature of this thesis, which is concerned with epistemological problems and not with pure mathematics, we shall be particularly interested in the problem of the relativity of physical space.

In fact, the relativity of physical space follows directly from the observations of the preceding chapter. We saw that the physical continuum is generated by the laws of succession of our sensations. Since it is obviously conceivable that the concatenation of sensations be quite different from what it actually is, it follows that the nature of the physical continuum which we call representational space might differ from what it actually is.

> "There is nothing, therefore, to prevent us from imagining a series of representations, similar in every way to our ordinary representations, but succeeding one another according to laws which differ from those to which we are accustomed. We may thus conceive that beings whose education has taken place in a medium in which those laws would be so different, might have a very different geometry from ours."[43]

Poincaré gives an elaborate illustration of a hypothetical world in which there are such differences.

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[43] Science and Hypothesis, pp. 64-65.

This world is enclosed in a sphere in which there is a continuous variation of temperature. It is greatest at the centre and decreases to absolute zero at the surface. The law by which the temperature varies is a simple one in which, if R is the radius of the sphere and r the distance from the centre, the temperature will be proportional to R^2-r^2. Furthermore, it is assumed that all bodies have the same coefficient of thermal expansion. Thus, the linear dilation of any body in this universe would be proportional to its absolute temperature. Finally, it is assumed that a body in motion is in instantaneous thermal equilibrium with its surroundings. Obviously, as a material object moves towards the surface of the sphere, it will grow smaller.

The inhabitants of such a world would suppose it to be infinite since, as they approached its boundary, their limbs would contract and they would take successively smaller steps. We have already seen that a visual continuum of three dimensions is arrived at when it is apprehended that certain primitive sensations can be restored by an appropriate movement. It is thus that we distinguish between changes of state and changes of position. Now, in Poincaré's hypothetical universe, the inhabitants would similarly be presented with changing aggregates of sensations. Would these beings be able to restore their sensations as we do? Not in quite the same way. The objects which we regard as undergoing simple displacements are called solid or <u>rigid</u>

objects. The displacements which they undergo are Euclidean displacements. That is to say, the shape of an object after such a displacement would be congruent to its shape before the displacement, according to the Euclidean definition of congruence. Thus, when we regard our physical continuum as similar to a Euclidean mathematical continuum, our conclusion is closely geared to the way in which certain objects are displaced. To restore a primitive aggregate of sensations, our bodies must undergo Euclidean displacements. In the hypothetical world, an aggregate of sensations could only be restored by a non-Euclidean displacement of the body. That is, it would be a displacement in which the observer actually dilates in accordance with the law given above. In short, such people would develop a non-Euclidean geometry as the most natural or simplest geometry.[44]

At first, it would appear that Poincaré has argued for the non-relativity of space, in a certain sense. From what he has said, it would seem that only one geometry, namely Euclidean, is possible to describe the physical continuum of representational space. But this is merely the error of the naive. We must, in fact, ask ourselves what would happen if a person from our universe were transported to the hypothetical universe. Would he decide that Euclidean geometry is no longer true? If he wished, he could so conclude, but it is highly unlikely. He would find it

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[44] Science and Hypothesis, p. 68.

difficult to adjust to another system of geometry. It is more likely that he would retain his accustomed system of geometry but note that the new universe has the curious characteristic of lacking rigid objects. That is to say, he would note that objects which undergo a displacement actually "squirm." But he would be able to describe such "squirms" in the language of Euclid. Hence, we see that the geometry of physical space is not experimentally decidable but depends on a choice based on convenience.[45]

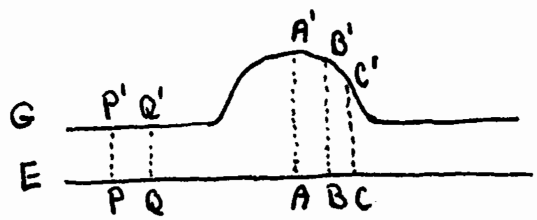

Fig. I

The principle of the relativity of physical space has been discussed more cogently by Hans Reichenbach.[46] Reichenbach considers a great glass hemisphere which gradually merges into a glass plane. A cross-section would present the aspect G in figure I. Parallel to the plane of G and underneath it is an opaque plane E. Vertical light rays will pass through the glass, casting shadows onto E of all objects situated on G. If human beings lived on the surface of G, they would soon discover by simple geodetic measurements that G is a plane with a hemispherical hump in the middle. Their

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45 Ibid., p. 71

46 The Philosophy of Space and Time, p. 11ff.

measuring rods would cast shadows on the surface E which would be deformed in the central area of E. Let us now suppose that there are also inhabitants on E. An invisible force affects the measuring rods of the E-men such that, as they are moved, their length is always equal to the corresponding shadows of measuring rods on G. Obviously, the E-people would obtain precisely the same results from geodetic surveys as those of the G-people. Would they conclude, therefore, that they were living on a world with a hump or would they prefer to postulate an invisible force?

Actually, such a question is, strictly speaking, meaningless. As Reichenbach puts it, "We may just as well say that G is the surface with the 'illusion' of the hump and E the surface with the 'real' hump. Or perhaps both surfaces have a hump."[47]

Poincaré argues at some length in Science and Hypothesis to establish the point that there are alternative descriptions of physical space which are theoretically equivalent. If geometry were an experimental subject, it would be inexact and provisional. In fact, we should have to say that Euclidean geometry is false, since there is no rigorously rigid object. Thus, we are back to the dilemma of the Introduction. The statements of geometry cannot be synthetic *a priori* truths, nor can they be empirical generalizations. What, then, are they? It is to this

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[47] *Op. cit.*, p. 13.

question that Poincaré gives his famous reply: "They are conventions."[48] We adopt Euclidean geometry because it is the most convenient description of the world. However, it is not, in any sense, "truer" than a system of non-Euclidean geometry.

Poincaré's exposition admittedly seems confused. We have examined, at length, his elaborate account of the experimental origin of representational space. We have noted that our belief in the Euclidean character of that space stems from our experience of the displacements of rigid bodies. Such accounts would surely have proved pleasing to a Hume or a Mill. Yet they must be contrasted with such statements as: "whichever way we look at it, it is impossible to discover in geometric empiricism a rational meaning."[49]

So it would appear that Poincaré believes that the Euclidean metric is of experimental origin and, at the same time, that it is of a merely definitional character. Actually, this does represent Poincaré's position quite accurately. But as we shall see, there is no real contradiction involved in this. He considers, for example, the phenomenon of stellar parallax. If the geometry of Riemann were true, this parallax would be negative. If the geometry of Lobatschewsky were true, the parallax of a distant star

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[48] Science and Hypothesis, p. 50.

[49] Ibid., p. 79.

would be infinite. Surely, then, in principle, the truth of a geometry should be determinable by the appropriate astronomical measurements.[50] But, in astronomy, the straight line is actually the path traversed by a light ray. Therefore, in either of the above cases, we could retain our system of Euclidean geometry and modify the laws of optics so that light would be considered to be propagated along curvilinear paths. "Euclidean Geometry, therefore, has nothing to fear from fresh experiments." Poincaré's general point is that geometrical experiments only provide information about the mutual relationships between bodies or between bodies and light rays. We cannot design an experiment to convey information concerning the relationship between a physical object and space itself.

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[50]*Ibid.*, p. 72.

CHAPTER VI

RECENT CRITICISMS OF POINCARÉ'S INTERPRETATION OF GEOMETRY

In the present century, since the development of the general theory of relativity, it has become the custom to regard geometry as a branch of physics. That is to say, the position of geometric empiricism has found a powerful evidential basis. Thus, many thinkers, not including Einstein himself, have been critical of Poincaré's so-called <u>conventionalism</u>. The most consistent critic has been Hans Reichenbach. In <u>The Rise of Scientific Philosophy</u>, he writes that:

> "Space is not subjective, but real - that is the outcome of the development of modern mathematics and physics. Strangely enough, this long historical line leads ultimately back to the position held at its beginning: geometry began as an empirical science with the Egyptians, was made a deductive science by the Greeks, and finally was turned back into an empirical science after logical analysis of highest perfection had uncovered a plurality of geometries, one and only one of which is the geometry of the physical world."[51]

Reichenbach accepted Poincaré's conventionalism up to a point but appeared to feel that it was an overstatement.

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[51] <u>Op</u>. <u>cit</u>., p. 139.

He admits that there are alternative geometrical accounts of a single empirical state of affairs. He calls them "Equivalent descriptions." However, he argues that there are sets of equivalent descriptions which could not refer to a single observable world.

The following would be regarded as equivalent descriptions:

a) The geometry is Euclidean, but there are universal forces distorting light rays and measuring rods.

b) The geometry is non-Euclidean and there are no universal forces.

But Reichenbach argues that the foregoing must be distinguished from the following:

a) The geometry is Euclidean, and there are no universal forces.

b) The geometry is non-Euclidean, but there are universal forces distorting light rays and measuring rods.

> "Conventionalism sees only the equivalence of the descriptions within one class, but stops short of recognizing the differences between the classes. The theory of equivalent descriptions, however, enables us to describe the world objectively by assigning empirical truth to only one class of descriptions, although within each class all descriptions are of equal truth value."[52]

Reichenbach then goes on to point out that we do not normally employ classes of descriptions to refer to the geometry of the world. It is customary to select a single

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[52] Ibid., pp. 136-137.

description which is taken to be the <u>normal</u> system. This <u>normal</u> system is the system of <u>natural</u> geometry. The criterion for choosing the <u>natural</u> geometry is that it is the one in which universal forces vanish. However, there is no *a priori* necessity for a class of equivalent descriptions to contain any such normal system. There would be no such system if, for example, the geometry of light rays differed from the geometry of rigid bodies. "That the natural geometry of the world of our environment is Euclidean must be regarded as a fortunate empirical fact."[53]

Such opposition to Poincaré's position from one of the greatest philosophical exponents of the theory of relativity deserves careful attention.

The reader will recall Reichenbach's illustration of the two surfaces G and E. This was apparently in support of Poincaré's general contention anent the relativity of physical geometry. But Reichenbach has more to say about it. It was postulated that the measuring rods, etc. on the surface E were subject to a deformation by invisible forces. If these forces were in all respects unobservable, then it would not be possible to determine the geometry of surface E.

Now we must consider the question, under what conditions would such forces be absolutely unobservable? It is easy to imagine a physical force which would deform

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[53] *Ibid.*, p. 137.

the measuring rods in accordance with the conditions of Reichenbach's illustration but which would, nevertheless, be observable. Heat, for example, concentrated in the central area of E would cause the measuring rods to expand in that area. The presence of this "force" could be determined by virtue of its being what Reichenbach calls a "differential force."[54] That is to say, a variation in temperature affects various materials differently. In Reichenbach's illustration, however, the hypothetical invisible force was of the type he calls "universal." Universal forces have two principal properties:

a) They affect all materials in the same way.

b) There are no insulating walls.

It is obvious that the presence of such forces would under no circumstances be directly observable. If it is also the case that universal forces are inaccessible to indirect verification of any kind, it follows that we can make no categorical assertion about the metrical properties of physical space, and Poincaré's thesis would be established. Referring to Fig. I, we may reduce the question to its most elementary form, are the distances AB and BC "really" equal? In other words, is it possible to give an objective definition of congruence?

Reichenbach points out that metrical relationships, such as congruence, can only be determined after a "co-ordinative" definition has been made. For example, before

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[54] Philosophy of Space and Time, p. 13.

making measurements, we must define our unit of measurement. We may take it to represent a certain fraction of the earth's circumference or even the wave length of krypton gas. The co-ordinative definition serves, then, to relate a concept, in this case metrical, to a physical object or state of affairs. It is simply what Bridgman has called an "operational definition."[55]

Once the unit of length has been established, we have the problem of defining congruence. To determine the equality or congruence of spatial distances we are bound to transport one or more measuring rods. Thus, if two measuring rods, R_1 and R_2, are placed side by side, let us suppose that they are found to be of equal length. R_1 is transported to a distant region of space where it is found to correspond to the distance AB. R_2 is transported to another region of space where it is found to correspond to CD. Then AB and CD would be regarded as congruent.

However, an assumption has been introduced to the effect that the rods have not been deformed during their respective translations. The most that could be done would be to bring the two rods together again to determine whether, when placed side by side, they are still of equal length. Thus, the only cognitive knowledge we have is to the effect that R_1 and R_2 are always of equal length when in the same region of space. If the rods have been affected by universal

[55] The Logic of Modern Physics, passim.

forces, there is no means of discovering the effect.

Therefore, to determine congruence, we must base our decision on the physical fact that the rods are locally of equivalent length <u>and</u> on the definition that when in different regions of space they are still equal. This distant equality is not cognitive. It is purely definitional. If the factual relationship of local equality did not hold, it would still be possible to define congruence, but a separate definition would be required for every region of space. Conversely, in the actual world it would be possible to formulate a more complicated definition of congruence such that, for example, two rods whose respective lengths coincided would be defined as of unequal length. With such a definition of congruence, all of our metrical determinations would be greatly complicated. But, strictly speaking, all definitions are conventional and, hence, epistemologically equivalent.

> "It is again a matter offact that our world admits of a simple definition of congruence because of the factual relations holding for the behaviour of rigid rods; but this fact does not deprive the simple definition of its definitional character."[56]

Thus, it is clear that the question as to whether the surface E is a plane or has a hump in the middle depends on its inhabitants' choice of a coordinating definition of congruence. However, we still need to determine whether there is any reasonable basis for our decision. But beforehand

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[56] <u>Philosophy of Space and Time</u>, p. 17.

we must investigate more closely the notion of the rigid body which is employed in the usual definition of congruence.

In everyday life we make frequent use of the concept of rigid body. When we say that the ceiling of our room is a plane, that the floor is rectangular or that a taught string is straight, we are presupposing the idea of rigidity. However, it is almost a commonplace that none of these objects is perfectly rigid. They are all subject to various kinds of forces which cause slight deformations. Scientific physics endeavours to avoid the imprecision of the physics of everyday life.

It would, of course, be circular to define the rigid body as one which undergoes no change of shape. But such circular reasoning is not necessary. The rigid body may be defined as follows:

> "Rigid bodies are solid bodies which are not affected by differential forces, or concerning which the influence of differential forces has been eliminated by corrections; universal forces are disregarded."[57]

That is to say that the universal forces are set at zero by definition. Without such a stipulation no rigorous definition of the rigid body would be possible, since any object which was called rigid might actually be deformed by such a force. Of course, in physics all of the forces that are dealt with are of the differential kind.

Solid objects actually possess various internal

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[57] *Ibid.*, p. 22.

forces or tensions which resist the change of shape of the body. A rigid body is realized when the external forces are vanishingly small relative to the internal forces.

We are now in a position to return to our original fundamental question: what criteria do we employ as a basis for a decision concerning the geometry of the physical world? Mathematically, we know that a point transformation is possible for all congruence geometries. In the language of physics this means that:

> "Given a geometry G' to which the measuring instruments conform, we can imagine a universal force F which affects the instruments in such a way that the actual geometry is an arbitrary geometry G, while the observed deviation from G is due to a universal deformation of the measuring instruments."[58]

This states clearly the principle of the relativity of physical space. In the first place, we are assured by it that a Euclidean geometry is always possible. In the second place, however, it asserts that any other geometry will be equally acceptable. Reichenbach would agree with Poincaré that on the above principle the question of the absolute truth of any geometry is meaningless.

Reichenbach, however, now suggests precisely why he disagrees with Poincaré in the final analysis.

> "We obtain a statement about physical reality only if in addition to the geometry G of the space its universal field of force F is specified. Only the combination G+F is a testable statement."[59]

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[58] Ibid., p. 33.

[59] Idem.

But we have already agreed to accept the coordinative definition of a rigid body in accordance with which F = 0. That is to say, the physicist adopts that geometry which enables him to assume that measuring rods are not transformed. Poincaré, on the other hand, has supposed that, all geometries being equal, we shall always prefer the Euclidean geometry ($G = G_o$). Einsteinian geometry is said to be the geometry of physical space because it does not require the assumption of unobservable universal forces.

Poincaré is then supposed to be in error because he failed to see that, in spite of the principle of geometrical relativity, objective statements about space are still possible.

> "This is a misunderstanding. Although the statement about the geometry is based upon certain arbitrary definitions, the statement itself does not become arbitrary: once the definitions have been formulated, it is determined through objective reality alone which is the actual geometry."[60]

> "The objective character of the physical statement is thus shifted to a statement about relations. A statement about the boiling point of water is no longer regarded as an absolute statement, but as a statement about a relation between the boiling water and the length of the column of mercury. There exists a similar objective statement about the geometry of real space: it is a statement about a relation between the universe and rigid rods. The geometry chosen to characterize this relation is only a mode of speech; however, our awareness of the relativity of geometry enables us to formulate the objective character of a statement about the geometry of the physical world as a statement about relations. In this sense we are permitted to speak of physical geometry."[61]

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[60] Ibid., p. 37.

[61] Idem.

Essentially I am in agreement with Reichenbach's position. However, that does not place me in disagreement with Poincaré. Reichenbach is plainly mistaken when he attributes to Poincaré the view that the conventions of geometry are arbitrary. In fact, it has become commonplace to regard Poincaré as the proponent of the view that the geometry of physical space consists of "arbitrary conventions." Poincaré, however, is absolutely explicit in his denunciation of such a notion.

It has already been noted that Poincaré seriously maintained that the geometry of physical space arises from the study of rigid bodies. Consequently, "Our choice among all possible conventions is _guided_ by experimental facts."[62] "Experiment guides us in this choice which it does not impose on us. It tells us not what is the truest, but what is the most convenient geometry."[63] Again, "We have chosen the most convenient space, but experience guided our choice."[64]

Reichenbach has maintained that once the co-ordinative definitions have been clearly stated, it is possible to formulate objective statements about physical space. Let the reader compare this with Poincaré's assertion that:

> "A statement of fact is always verifiable, and for the verification we have recourse either to the witness

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[62] _Science and Hypothesis_, p. 50.

[63] _Ibid._, pp. 70-71.

[64] _Science and Method_, p. 115.

of our senses, or to the memory of this witness. This is properly what characterizes a fact. If you put the question to me: is such a fact true? I shall begin by asking you, if there is occasion, to state precisely the conventions, by asking you, in other words, what language you have spoken; then once settled on this point, *I shall interrogate my senses and shall answer yes or no.*"[65]

Furthermore, Reichenbach asserts that geometrical statements can be objective when it is kept in mind that they are merely statements about the *relationships* holding between measuring rods and the world. Let the reader compare this with Poincaré's own position that:

"Therefore, when we ask what is the objective value of science, that does not mean: Does science teach us the true nature of things? But it means: Does it teach us the true relation of things?"[66]

Also compare:

"It is only the relation of the magnitude to the instrument that we measure, and if this relation is altered, we have no means of knowing whether it is the magnitude or the instrument that has changed."[67]

The preceding profusion of quotations must be excused as necessary. The reader may draw his own conclusions from them. The fact, however, appears inescapable that all of the fundamental ideas which Reichenbach ventures in refutation of Poincaré were actually insights first developed by Poincaré himself!

Although the position of Poincaré is in essential

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[65] *The Value of Science*, p. 118. Italics are mine.

[66] *Ibid.*, p. 138.

[67] *Science and Method.*, p. 97.

agreement with that of Reichenbach, there are, of course, accidental differences. Poincaré was arguing from the state of pre-relativity mechanics. The non-Euclidean space of Einstein is a product of the general theory of relativity of 1915. Reichenbach pointed out that the non-Euclidean space follows as a consequence of relativity theory when $F = F_o$. It would be possible to retain Euclidean geometry in principle, but the ensuing complexities would baffle even the greatest of mathematicians.

It does not therefore follow, however, that the value $F = F_o$ is divinely ordained; that it must be granted, a priori. Reichenbach seems to treat it as though it had a privileged status. The reader will recall that Reichenbach refers to the geometry corresponding to $F = F_o$ as the "natural" geometry. What does this really mean? Actually, it means precisely what Poincaré intended when he held Euclidean geometry to be the most "convenient." If Poincaré were alive today, we are convinced that he would accept Reichenbach's stipulation. He would regard it as the best convention for relativistic mechanics as he had believed it to be for classical mechanics. Poincaré believed that Euclidean geometry was not only the simplest geometry in itself but also the simplest account of experience.[68] Were he alive today, he would probably continue to maintain that Euclidean geometry could still be applied if anyone desired to take

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[68]Cf., Science and Hypothesis, p. 50.

the trouble. The theory of relativity could not refute this general contention. It is for this reason that Einstein himself remarked, "Sub specie aeterni Poincaré, in my opinion, is right."[69]

Now, with regard to Reichenbach's example of two sets of equivalent descriptions which are not equivalent to each other, it is implied that the stipulations $G_o + F_1$ and $G_1 + F_o$ are equivalent. Also, $G_o + F_o$ and $G_1 + F_1$ are equivalent. However, Reichenbach would argue that the two sets of descriptions could not be applied to the same physical world. In this regard, Reichenbach is correct, but he forgets that any system of geometry G could be employed to describe any world provided that suitable adjustments are made to the system of physical laws F. It is this undeniable truth and nothing else which is the central principle of Poincaré's opposition to geometric empiricism!

To return to the illustration of Fig. I, Reichenbach would argue that if the measuring rods of the G-people were deformed, while those of the E-people behaved like Euclidean solids, there would be an empirical difference between the two worlds G and E. Would this then mean that the people of G could not possibly employ the language of Euclidean geometry? Not at all. It would simply mean that they would

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[69]"Geometry and Experience" in Readings in the Philosophy of Science, (ed. H. Feigl and M. Brodbeck), p. 192.

not be able to retain Euclidean geometry and still choose between F_0 and F_1. But they could still describe their world by the stipulation $G_0 + F_2$, where F_2 is any system of physical laws which would be necessary to retain G_0. Of course there would be an empirical difference between the two worlds, but it would be the same order of empirical fact which teaches us that Euclidean geometry is the most convenient description of our own world. This much, Poincaré naturally admits.

A. d'Abro is one of the few writers who seems to have understood Poincaré's position:

> "If we consider the problem in its present state, we see that it is the physical behaviour of material bodies and light rays which is in the final analysis responsible for our natural belief in absolute shape. But this realisation brings with it the assurance that space itself has eluded us entirely in our discussions. Such was indeed Poincaré's stand. He maintained that though for purposes of convenience it was only natural for us to measure space as we do, yet if needs be we could disregard the behaviour of material bodies entirely, adopt non-Euclidean standards and proceed as before."[70]

We may conclude that Reichenbach's criticism of Poincaré on behalf of geometric empiricism is based to a very great extent on a misunderstanding of Poincaré's position. Reichenbach attributed to Poincaré a doctrine of radical conventionalism which the latter, in fact, never held. Poincaré's position is in agreement with that of Reichenbach to the effect that we can only make objective statements about the conjunction G + F. However, the almost

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[70] *The Evolution of Scientific Thought*, 52f.

trivial difference in principle between them is that whereas Reichenbach would adopt the convention $F = F_o$, Poincaré, on the basis of existing physics, adopted the convention $G = G_o$, i.e. that geometry is Euclidean. In either case, the fact remains that no cognitive statement can be made until a convention of one sort or the other has been stipulated.

A far more bitter polemic, in the name of geometric empiricism, has recently been made by H. P. Robertson.[71] While we may forgive Reichenbach for having misunderstood Poincaré, we can find no justification for either the nature or the tone of Robertson's attack.

Robertson points out[72] that in spherical geometry the sum of the interior angles of a triangle exceeds two right-angles. The amount of this spherical "excess" is given by the formula $\sigma - \pi = K\delta$, where δ is the area of the triangle, σ is the angle-sum, and K is the constant of curvature, given by $1/R^2$ where R is the radius of a sphere on the surface of which the triangle could be placed without distortion.[73]

It is obvious from the above formula that the curvature of space (K) could be determined by an angular

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[71] "Geometry as a Branch of Physics" in Albert Einstein, Philosopher-Scientist. (ed. P. Schilpp), pp. 315-332.

[72] Ibid., p. 318.

[73] The last part of this sentence is an addition of the present writer to aid the reader in understanding intuitively what is meant by the "curvature" of space.

measurement of a suitable triangle. If K is very small, i.e. R is very great, the experiment attempted by Gauss on the triangulation of mountain peaks would be of no value. However, Robertson refers to the work of Gauss' successor at Göttingen, K. Schwarzschild who proposed a more refined experiment in which:

> "A triangle determined by three points will be defined as the paths of light-rays from one point to another, the lengths of its sides a, b, c, by the times it takes light to traverse these paths, and the angles a, b, c will be measured with the usual astronomical instruments."[74]

Robertson suggests that such a procedure could be applied to a triangle ABC, in which A is the position of a star while B and C are successive positions of the earth which are, for example, six months apart. Robertson goes on to point out that:

> "...the value for us of the work of Schwarzschild lies in its sound operational approach to the problem of physical geometry - in refreshing contrast to the pontifical pronouncement of H. Poincaré, who after reviewing the subject stated:
>
> 'If therefore negative parallaxes were found, or if it were demonstrated that all parallaxes are superior to a certain limit, two courses would be open to us; we might either renounce Euclidean geometry, or else modify laws of optics and suppose that light does not travel rigorously in a straight line.
>
> 'It is needless to add that all the world would regard the latter solution as the more advantageous.
>
> 'The Euclidean geometry has, therefore, nothing to fear from fresh experiments(!)'"[75]

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[74] Über das zulässige Krümmungsmaass des Raumes, Vierteljahrsschrift der astronomischen Gesellschaft, vol. 35, pp. 337-347. Quoted by H. P. Robertson, Op. Cit., p. 323.

[75] Op. cit., pp. 324-325.

It seems to the present writer that it is not Poincaré but Robertson himself who is guilty of the "pontifical pronouncement." Poincaré's position was clearly stated when he wrote that, "to ask what geometry it is proper to adopt is to ask, to what line is it proper to give the name straight? It is evident that experiment can not settle such a question."[76]

In short, Poincaré is simply reiterating the point made by Riemann that we cannot assign a particular metric to space until we have given a definition of congruence or, what is almost the same thing, until we have settled on a definition of a straight line or geodesic.

Robertson claims that Schwarzschild proposed an operational method for determining the metric of space. However, the metric would not be revealed by any internal evidence regarding the nature of space itself. The proposed experiment could only be performed after a suitable definition of the spatial metric had been given. Obviously, Schwarzschild chose a definition of the geodesic which is based on the time taken by a light ray to go from one point to another. This is undoubtedly the most convenient definition in view of the fact that astronomy is perforce based on optical experiments. However, there is no *a priori* necessity in choosing such a definition. We would repeat that it is not Poincaré but Robertson who has made a "pontifical pronouncement."

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[76]*The Value of Science*, p. 37.

The most illustrious of Poincaré's critics is undoubtedly Bertrand Russell. In An Essay on the Foundations of Geometry, Russell offered a brief criticism of the conventionalist interpretation of geometry in which he clearly allied himself with the proponents of geometrical empiricism.[77] His remarks led Poincaré to make a formal criticism of Russell's book.[78]

Poincaré begins by reiterating several of his well known views concerning the basis of the geometrical axioms, but with greater force than hitherto.

> "I believe that Mr. Russell is wrong in attributing an empirical character. . . to Euclid's postulate.
>
> "Moreover, the word 'empirical', in such a context as this, seems to be devoid of meaning."[79]
>
> "If one were to discover a star whose parallax was negative, would one thereby conclude that our geometry is false? No; it would surely be more natural to conclude that the light rays emanating from this star were not rigorously propagated in a straight line. I have stated this before, but I do not hesitate to repeat it, in view of the fact that people still contest this truth which is to me so obvious"[80]

Poincaré also reiterates the point that,

> "Our knowledge of the movements of solid objects cannot supply the basis of geometry; they are merely

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[77] Cf., Op. cit., pp. 30-31 and p. 113.

[78] Des Fondements de la Géométrie, Revue de Métaphysique et de Morale, v. 7, 1899, pp. 251-279.

[79] Ibid., p. 265. All quotations from this article have been translated by the present writer.

[80] Idem.

suggestive of such a basis. They play an important psychological role but no logical role whatsoever.[81]

"If anyone remains unconvinced by these considerations, let him produce an actual experiment which could be interpreted in the Euclidean system but which could not be interpreted in the system of Lobatschewsky.

"As I know that this challenge will never be taken up, I may conclude:

"No experience can ever be in contradiction with Euclid's postulate; by the same token, no experience will ever contradict Lobatschewsky's postulate."[82]

With regard to Poincaré's view of the conventional character of distance, Russell wrote,

"It is open to us, of course, if we choose, to continue to exclude distance in the ordinary sense, as the quantity of a finite straight line, and to define the _word_ distance in any way we please. But the conception, for which the word has hitherto stood, will then require a new name, and the only result will be a confusion between the _apparent_ meaning of our propositions, to those who retain the association belonging to the old sense of the word, and the _real_ meaning, resulting from the new sense in which the word is used."[83]

Poincaré replies:

"To illustrate the pure folly of this criticism, I shall take a rather extreme example. Suppose I said that, 'I am entitled to say that a triangle has four sides, for no one can prevent me from giving the name triangle to the shape that you would call a quadrilateral.' You would reply: 'But you are wrong in giving the name of triangle to something which everyone else would call a quadrilateral.' This advice is certainly sound, but does it imply that the statement 'the triangle has three sides' is an axiom or theorem rather than a mere definition?"[84]

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[81] _Idem._

[82] _Ibid._, p. 267.

[83] _Op. cit._, p. 33.

[84] _Des Fondements de la Géométrie_, p. 273.

Poincaré's simple but penetrating criticism of Russell's theory of metageometry elicited a charming reply from the latter[85] in which he pays particular attention to the notion of congruence:

> "Poincaré's thesis led him to the view that a distance implies an equality, i.e. an equality of two distances. This point is fundamental, since it implies that the determination of a distance depends on a measurement. But what is it that one measures? If it is distance that one measures, it must have existed before the measurement. This point brings out the essence of the confusion. It seems to be believed that since a measurement is necessary to <u>discover</u> equality and inequality, there can be no <u>equality</u> or inequality without measurement. However, the proper conclusion is precisely the contrary. That which one can discover by any operation must exist independently of that operation. America existed before Christopher Columbus, and two portions of space must be equal or unequal before being measured. Any method of measurement is good or bad depending on whether its result is true or false. Poincaré, on the other hand, believes that measurement creates equality and inequality. It follows that all methods of measurement must be equally good. But there is still another implication that he does not appear to have realised, that (on his theory) there is nothing left to measure and that equality and inequality become words which are devoid of meaning."[86]

In short, Russell is adopting a "factualist" interpretation of geometry. How he was able to find such an interpretation epistemologically tenable we do not know. For example, the definition of congruence is not only fundamental to the measurement of distance but also to the determination of amount of curvature. Russell puts himself

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[85] <u>Sur les Axiomes de la Géométrie</u>, Revue de Métaphysique et de Morale, v. 7, 1899, pp. 684-706.

[86] <u>Ibid.</u>, p. 687f. This quotation is translated by the present writer.

in the hopeless position of maintaining that a line is "really" straight or "really" curved prior to any measurement. But this is a naive view which clearly misses the undeniable truth of the principle of spatial relativity. It is true that there is a difference between a good measurement and a bad one, but only after the nature of the coordinate system has been specified. Russell misses the point that a measurement can be called good or bad even though it is relativistic.

Russell apparently holds the view that there are factors internal to space itself which uniquely determine the metrical properties of that space. Russell would argue that the word "congruence" has the unique meaning of spatial equality. This is quite true. "Congruence" does refer to the equality of spatial intervals. However, the axioms of geometry are such as to admit an infinitude of different interpretations of the equality of two intervals. A spatial interval or distance has no special metrical properties which permit us to single it out as the type of interval which is specified by the primitive term "congruent." The criterion on the basis of which we regard two intervals to be equal must be an external standard. When we are concerned with actual measurements the standard will be some sort of instrument as, for example, a measuring rod. The rigidity of this standard when it is in motion is clearly a matter of stipulation as even Reichenbach would admit. We fail to see how the consideration of space itself would enable us to single out any one standard as uniquely determined. That is not to say, however, that experience does not suggest an appropriate congruence standard. It does. But the standard depends on the behaviour of material objects and has nothing to do with the intrinsic nature of space.

CHAPTER VII

CONVENTIONALISM AND MECHANICS

I-ABSOLUTE SPACE AND MOTION

The bearing of Poincaré's philosophy of science on the doctrines of theoretical mechanics is actually more interesting than its bearing on geometry. It is more interesting because it is more critical. Many scientists tend to disapprove of the philosophy of science because its results are either false in the face of real scientific practice or trivial in their implications.

While philosophers of science have sometimes claimed that their doctrines contain genuine heuristic principles which scientists would do well to heed, such claims are often specious or, at least, dubious. I do not think that the primary aim of the philosophy of science is to discover heuristic principles but, undoubtedly, its validity as an intellectual discipline would be the more readily accepted if it could.

We may determine to what extent the conventionalist interpretation of mechanics is a heuristic theory by considering its application to Einsteinian relativistic mechanics. Poincaré's remarks on the conventional character of mechanics were, of course, addressed to pre-Einsteinian science. But if they possess genuine heuristic value, they will be applicable to the theory of relativity. This, we think, would be an excellent test of the validity of Poincaré's thesis.

We must begin this chapter with a digression into the history of mechanics. This is necessary to provide a backdrop against which to consider the philosophy of Poincaré. However, no claim is made to present a scholarly account of the history of science in the following pages. It would be the greatest conceit for any man to claim to do this in a few pages, not to speak of the limitations of the present writer. The following observations on classical mechanics are in general common coin. However, the literature on relativity theory, ranging from the popular to the highly technical is so immense that the writer could not possibly give any account of it in these pages. Perhaps arbitrarily, but with sufficient justification, it has been decided to rely, for the most part, on a single elementary source for the theory of relativity, namely, Einstein's "Relativity, The Special and General Theory."

In his greatest achievement, the "Principia", Newton enunciated the principle of inertia as the first law of motion. It was the fundamental axiom of his theoretical system. It asserts that every body will persevere in a state of rest or uniform, rectilinear motion unless acted on by external unbalanced forces. Now it is apparent that such a statement could have no determinate meaning unless a frame of reference be given in the form of spatial coordinates. For example, if a ball is thrown in the air in a uniformly moving vehicle, it appears to go straight up and then straight down to any observer at rest in that vehicle. However, its path would appear to be a parabola to any person at rest on the surface of the earth. Again, with respect to the earth, the motion of the falling stone appears to be accelerated. If the frame of reference were falling at the same rate, the stone would appear to be motionless. In short, the phrase "uniform, rectilinear motion" is only meaningful with respect to a frame of reference.

If we grant the fundamentally amorphous character of space, it would follow that there is no privileged frame of reference and, therefore, Newton's law of motion would be quite arbitrary. It will be true provided that the appropriate frame of reference is selected. But laws of much greater complexity than the classical laws of motion could be arbitrarily chosen and shown to be true with respect to the appropriate system of spatial coordinates.

Notwithstanding the amorphous character of space, Newton discovered that frames of reference are distinguishable. In some we find strange forces acting on us and on moving objects, while in others we do not. Those frames of reference in which no disturbances, other than of the gravitational variety, are found were called Galilean or inertial frames. With respect to such inertial frames, not only were the laws of motion found to be true but also the related Keplerian laws of planetary motion.[87] Furthermore, it was discovered that the strange, unsymetrical forces of the non-Galilean frame could be directly related to the inertial system. It was found that an "inertial" force appeared in a frame which is in a state of rectilinear acceleration. When the non-Galilean frame is rotating with respect to the Galilean frame, the resultant forces are of the centrifugal and Coriolis types.

It should be noted that the earth was not taken to be an inertial system. According to the principle of inertia, the stars should describe rectilinear motions, but due to their great distance, their displacements would be imperceptible. However, to an observer on the earth, the fixed stars appear to follow curved paths around the pole-star. Hence, it must be assumed that the earth is a rotating system. The presence of rotational forces was, in fact, finally confirmed by the rotation of the plane of Foucault's pendulum. We see, then, that any Galilean frame may be defined operationally as a system at rest or in

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[87]In fact, this is only approximately the case, but the writer wishes to avoid needless complications in such an elementary account.

uniform motion with respect to the system of fixed stars.

It has already been noted that the distinction between inertial and non-inertial systems is incompatible with the amorphous character of space. Nevertheless, such a distinction does appear to be necessary. If all frames were equivalent, the law of inertia would be meaningless. Newton could have resolved the difficulty by accounting for inertial forces in terms of external influences or, more specifically, with reference to the fixed stars. However, according to his theory of gravitational attraction, the distance of the fixed stars was considered to be too great for them to have any perceptible terrestrial effects. Thus, he maintained that the forces found in non-Galilean systems would occur even if the system were in isolation from all the matter in the universe. This being the case, it follows that the accelerations and rotations of non-Galilean frames are "real" or "absolute." In other words, inertial forces imply the reification of absolute space! In fact, it also follows that, if bodies accelerate and rotate in absolute space, then bodies must also undergo uniform translations with respect to the same absolute space. In short, classical mechanics turns out to be wholly incompatible with the doctrine of the relativity of space.

As it happens, however, the real or absolute uniform motions or velocities cannot be detected by any mechanical means. That is to say, we are unable to determine mechanically which inertial systems are at rest in space and which are in uniform motion with respect to them. Thus, in practice, velocity remains relativistic. In fact, the relativity of velocity may be deduced as a consequence of Newton's second law of motion which asserts that the force acting on a body is equal to the product of the mass of that body and its acceleration. In classical mechanics, mass was regarded as an invariant quantity. Similarly, an acceleration will be invariant in all Galilean frames since any velocity must appear in the equations of motion as a constant and will disappear after differentiation. In other words, the acceleration is independent of the initial velocity. Consequently, the product of mass and acceleration must be invariant. Therefore, the second law $f = ma$ is true for all Galilean frames. If velocity were mechanically discernible, one would have to conclude either that $f = ma$ is not invariant or that mass is relative. Either alternative would be disastrous to classical mechanics.

Hence, there is a principle of relativity in classical mechanics which asserts that it is impossible for a Galilean observer to ascertain mechanically the state of rest or uniform, rectilinear motion of the system in which he is situated. Moreover, the mathematical expression of this principle of relativity is the invariance of the laws of mechanics throughout all Galilean frames.

The factors which led to the extension of classical relativity by Einstein pertain to theoretical difficulties of nineteenth century electromagnetics and optics. It was noted in the preceding paragraph that the principle of relativity requires the invariance of physical laws. One of the great achievements of the nineteenth century was the development by Maxwell of the equations of the electromagnetic field. However, while these equations described a host of physical phenomena, which made their abandonment virtually unthinkable, they were of such a type that their form was modified by Galilean transformations. This seemed to suggest a dualism between the space of mechanics and the luminiferous ether of electrodynamics. Since the hypothetical ether was stagnant or motionless, scientists believed that it would be possible, after all, to determine the real velocity of the earth by electro-magnetic means. Stated crudely, since electro-magnetic laws vary from one Galilean frame to another, a suitable experiment should reveal the absolute velocity of the frame in which the earth is at rest. The nineteenth century was replete with such experiments. However, the most famous is the experiment of 1887 performed by Michelson and Morley.

To reduce it to its simplest terms, the foundation of this experiment is that if two light rays depart from the centre of a sphere at the same time and are reflected by the inner surface of that sphere, they should return to their point of origin at precisely the same time, <u>provided</u>

that the earth is at rest in the ether. However, since the earth possesses an orbital velocity, a time difference between the two journeys should be discerned. This difference would reveal, therefore, the absolute velocity of the earth. As we know, the Michelson-Morley experiment yielded negative results, as did many similar experiments. The task of theoretical physics in the latter part of the nineteenth century was to explain the null effect of these experiments.

The most adequate explanation was provided by Fitzgerald. He postulated that the effect of the earth's motion through the motionless ether would be the contraction of all bodies at rest on the earth in the direction of that motion. That is to say, a translatory motion would produce a deformation, so that what is a sphere at rest would become distorted during a translatory motion into an elipsoid. Thus, the light rays of the Michelson-Morley experiment would actually travel at different speeds depending on their direction, but the distances travelled would also vary in such a way that the one effect precisely compensates for the other.

Lorentz accounted for the Fitzgerald contraction by means of an elaborate hypothesis of the electronic structure of matter. Furthermore, Lorentz worked out a set of transformations, differing from Galilean ones, in which the contraction of length would occur and, moreover, in which the invariance of electro-magnetic laws would be preserved.

Thus, Lorentz and Fitzgerald showed that while the earth does have a real velocity, nature, through apparent caprice or malevolence, has conspired to hide it.

While the theory of Lorentz adequately accounted for most experimental results in a general way, it was far from perfect for many reasons. In the first place, it required a set of complicated *ad hoc* assumptions about the electrical constitution of matter, concerning which virtually nothing was known at the time. Secondly, it deprived classical theory of its elegant generality by requiring one set of transformations for electro-magnetic phenomena and another set for mechanical phenomena. But from the philosophical standpoint, its greatest limitation is that it postulated physical effects which were held to be unobservable in principle.

In 1905, A. Einstein unravelled this tangled skein with a few simple but daring generalizations. On Lorentz's theory, real motion, while unobservable, had been preserved in principle. There was an objective or privileged frame of reference, namely the stagnant ether. The Fitzgerald-Lorentz contraction would not occur in this privileged frame but only in other frames which are in motion with respect to it. Einstein suggested that since an overwhelming weight of experimental evidence reveals that no privileged frame is discoverable, it would be better to dispense with the notion altogether. In other words, we should assume that all Galilean frames are equivalent for both mechanical and electro-magnetic laws.

In this case, we would no longer have any reason to differentiate between mechanical and electro-magnetic phenomena, so that a single set of transformations must be adopted in which both types of law are invariant. Moreover, the complete relativization of uniform motion will require the invariance of the velocity of light. But with respect to Galilean transformations, the principle of the composition of velocities requires that the velocity of light will vary with the velocity of its source. However, although Lorentz had not attached too much significance to it, the velocity of light does indeed appear as a constant in the Lorentz transformation equations. What Einstein required was a set of transformations having the above properties plus the additional feature of preserving the invariance of the classical laws of motion for low velocities. The Lorentz transformations filled all of these requirements. Thus, the special theory of relativity was formulated. We should now consider very briefly some of its consequences.

Firstly, the apparently self-evident law for the composition of velocities had to be abandoned. Let us take a simple example. Suppose a man on a train moving in the x direction with a velocity v throws a ball with the velocity w also in the x direction. An observer on the embankment will, according to classical principles, find the ball to travel with the velocity W = v + w. The invariance of mechanical laws would be preserved in classical physics by the preceding formula. In relativistic mechanics, the composition of velocities does not take place by simple addition or subtraction. Instead the formula will be:[88]

$$W = \frac{v + w}{1 + vw/c^2}$$

Even more remarkable is the relativization of time, given by the following formula:

$$t' = \frac{t - v/c^2 . x}{\sqrt{1 - v^2/c^2}}$$

The startling physical significance of the above equation is that time is no longer an absolute but a function of relative velocity. In other words, all clocks will slow down in a frame in uniform, translatory motion. Lorentz had distinguished between absolute or real time on the one hand and relative time on the other. But in the special theory of relativity there is no privileged frame, so that one cannot speak of a real time interval or congruence.

Furthermore, the Fitzgerald-Lorentz contraction, the reader will recall, was explained in terms of a physical hypothesis concerning the impact of electrically constituted matter against a motionless ether. Lorentz calculated that a rod which measures one metre at rest will contract when

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[88] All the mathematical formulas in this chapter are from Einstein, Op. cit., p. 39ff.

moving with a velocity v to $\sqrt{1-\frac{V2}{C2}}$ of a metre.

According to Lorentz, the latter is a real contraction. For Einstein, however, the mathematical formula for the contraction is correct but it does not signify a real contraction. What it actually represents is the relativization of distance. That is to say, Einstein's theory requires no _ad hoc_ postulates concerning the constitution of matter. The contraction is the result of the relative motion between the object which is measured and the measuring instrument. Therefore, if we consider two observers in different Galilean frames, each will consider the measuring rods of the other to have undergone a contraction.

It has, from time to time, been suggested that Lorentz, not Einstein, was the original author of the special theory of relativity. It is hoped that this brief review has made it quite clear that Lorentz merely modified the mathematical form of classical mechanics without introducing the radical reinterpretation which must be credited to Einstein alone. In the theory of Lorentz we find the absolutism of Newton still lingering on. If we could define the fundamental difference between Einstein and Lorentz in a single sentence for the sake of philosophers, it would be that Lorentz believed real motion to be meaningful but unobservable whereas Einstein maintained that it is altogether devoid of physical meaning. The by no means simple question which is now before us is to determine whether

Poincaré was a genuine precursor of Einstein.

According to Newton, the laws of mechanics are precise descriptions of the physical world. According to Poincaré, they have a definitional or conventional character akin to the propositions of geometry. The enunciation of classical laws presupposed the absoluteness of space and time and the Euclidean character of space. We have already discussed some of the problems connected with space. Later, we shall expound Poincaré's treatment of time. But Poincaré maintained that while the relativity of space and time make it clear that classical mechanics is conventional, one could reach this conclusion independently of these considerations.

Let us begin with a consideration of the principle of inertia. If this principle is anything more than a definition, it must be either an experimental law or an *a priori* principle.[89] Poincaré argues that it is obviously not given *a priori*, for not only is it possible to doubt it but in the past it actually has been doubted. The Greeks, notably Aristotle, believed that motion ceases when the cause of that motion ceases. This is surely as appealing to reason as the Newtonian law. It is unnecessary to labour the point, since surely no one with an elementary knowledge of physics could today argue that an alternative to the first law of motion is inconceivable.

Is this principle, then, an experimental fact? In

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[89] *Science and Hypothesis*, p. 91.

the first place, it is obvious that a body on which no forces are acting has never been experienced. That is to say, no direct confirmation of the principle is possible. But the scientist would argue that it may be verified indirectly by its consequences.[90] Poincaré points out that this is actually a loosely phrased argument. He maintains that what is really intended is that we may verify a more general law of which the principle of inertia is a special case. Poincaré proceeds to formulate that law which he calls the generalized principle of inertia: "The acceleration of a body depends only on its position and that of neighbouring bodies, and on their velocities." In mathematical language, this means that the laws of motion will have the form of differential equations of the second order.

Let us suppose that the true law of nature differs from the preceding law. For example, we might assume that when no force is acting, the position of the body is unchanged. Again, we might suppose that it is the acceleration of the body which is unchanged. The generalized principle of inertia corresponding to the first assumption would be that the velocity of a body depends only on its position and the position of neighbouring bodies. In the other case, it would assert that the <u>variation</u> of acceleration depends on its position and on the positions, velocities and accelerations

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[90] <u>Ibid.</u>, p. 92.

of neighbouring bodies. In mathematical language, the first assumption would mean that the laws of motion are differential equations of the first order, while the second means that they would be differential equations of the third order.

Now we must consider whether it would be possible under any circumstances for such foreign principles to be adopted. Poincaré offers a simple hypothetical example of a physical situation to which the former alternative could be applied. If by chance the solar system were such that the orbits of planets had neither eccentricity nor inclination and, furthermore, that their masses were so small that perturbations would be indiscernible, then scientists would conclude that the orbits of planets must be circular and parallel to a certain plane. The reader will readily perceive that under such conditions, which are after all free from self-contradiction and physically conceivable, it would be possible to determine the orbit of a planet from its present position alone. In other words, a Newton of this hypothetical world would conclude that when no force is acting on a body, its position remains constant. In fact, of course, the Keplerian orbits led the real Newton to formulate the law of inertia in its present familiar form. Poincaré makes the point that it is extremely unlikely that we have been led into a monstrous error of the same kind as our hypothetical astronomer. Nevertheless, it must be added that such an eventuality is indeed possible.

If we grant the validity of the law of inertia insofar as we suppose that no chance coincidence of circumstances has led us to adopt it, the next question to consider is whether this law could be refuted under any circumstances. In physics we frequently have recourse to hypothetical entities to explain phenomena. In fact, this is more obviously true today than it was in Poincaré's own time. Suppose we were examining a system of n molecules and found that their 3n spatial coordinates satisfy a set of 3n differential equations of the fourth order. Would we then abandon the present law of inertia? Obviously we could but it would be most inconvenient to do so. A set of 3n differential equations of the fourth order can be expressed by 6n equations of the second order by introducing 3n auxiliary variables. Then it is a simple matter to postulate that the 3n auxiliary variables represent the spatial coordinates of n invisible molecules, and the law of inertia is saved. The foregoing reasoning may seem a trifle abstract but it merely asserts that scientists prefer to abandon facts than to abandon theories.[91] Let the reader reflect how he would react to a situation in which a falling object failed to obey the law of gravity. He would surely not abandon that time-honoured principle. He would likely suppose, short of resorting to miracles, that some force which is so

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[91] Cf. Pierre Duhem, The Aim and Structure of Physical Theory, Passim.

far unknown has acted on the body to produce this apparent anomaly.

> "To sum up, this law, verified experimentally in some particular cases, may be extended fearlessly to the most general cases; for we know that in these general cases it can neither be confirmed nor contradicted by experiment."[92]

Poincaré now turns to a consideration of the second law of motion. If this principle is experimental, it should be possible to measure acceleration, force and mass. Poincaré points out that it is possible to measure an acceleration if we assume a measurable order of absolute time. Granting this, we are still faced with the problem of measuring mass and force. Before we can measure them, we must know what we are measuring. We must, therefore, begin with suitable definitions of force and mass. We may say that mass is the product of volume and density. But it is equally proper to say that density is the quotient of mass by volume. Similarly, force may be defined as the product of mass and acceleration, but we may also say that mass is the quotient of force by acceleration.

Let us begin by determining what is meant by the equality of two forces. The standard definition is that two forces are equal when they give the same acceleration to the same mass. Let us suppose two forces F and F' which are acting vertically upwards on two bodies C and C' respectively. A body of weight P is attached first to C and then to C'. If

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[92] Science and Hypothesis, p. 97.

there is equilibrium in both cases, we conclude that F and F' are equal to P and are, therefore, equal to each other. But such a definition lacks mathematical rigour, since it is assumed that the weight P remained constant when transported from C to C'. In fact, of course, there is a minute variation in weight from place to place. More important, however, is that we cannot simply assert that the weight of P is applied to C, keeping the force F in equilibrium. The situation is really more complex than this. It is the action A of P which is applied to C. Similarly, there is a reaction R of C on P. F and A are equal because they are in equilibrium. A and R are equal by Newton's third law of the equality of action and reaction. R and P are equal because they are in equilibrium. Hence, we may deduce the equality of P and F. It is apparent that the equality of two forces depends on our acceptance of the third law of motion. The latter, therefore, enters our considerations not as an experimental law but as a convention. In all, there are three assumptions on which we base our conclusion: the equality of action and reaction, the equality of forces in equilibrium and the constancy in magnitude and direction of weight. The last of these assumptions is indeed an experimental law but, as we have seen, it happens to be inaccurate.

We are forced to return to the definition of force as the product of mass and acceleration. But we are now compelled to regard it as a definition and not as an experimental law. Furthermore, it follows from the principle of action and reaction that the motion of the centre of gravity of an isolated system will be uniform and rectilinear. The position of the centre of gravity depends on the values of the various masses, so that it should be possible to define mass by assigning values which are consistent with this rule. But, in practice, the only isolated system is the entire universe. It is absurd to suppose that one could actually determine the centre of gravity of the universe as a whole. We are compelled to conclude, therefore, that, "*Masses are co-efficients which it is found convenient to introduce into calculations.*"[93]

If the laws of motion are merely definitions, it might be asked of what use they can be. Surely, it will be argued that they must be devoid of physical significance. This is by no means true according to Poincaré. The laws of motion are in the first place suggested by experiment. But experimental rules are only approximate. Consequently, we restate them rigorously, but then they lose their experimental character, and are no longer experimentally falsifiable. Of course, the weight of much additional experimental evidence could lead us to withdraw them for purposes of convenience, but that is a different matter. "If a principle ceases to be fecund, experiment without contradicting it directly will nevertheless have condemned

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[93] *Ibid.*, p. 103.

it."[94] "Thus is explained how experiment may serve as a basis for the principles of mechanics, and yet will never invalidate them."[95]

Widespread misunderstanding of Poincaré's position has been engendered by the failure to grasp the last particular point. Physical geometry is conventional because there are no absolutely rigid bodies. Nevertheless, there are bodies which are approximately rigid in the Euclidean sense, so that our physical geometry is a useful convention. Precisely the same situation holds in the case of mechanics. Mechanical laws are indeed conventional but are not arbitrary. However, so often has the view been attributed to Poincaré that the laws of mechanics are arbitrary conventions that he should be permitted to speak for himself.

> "Are the laws of acceleration and of the composition of forces only arbitrary conventions? Conventions, yes; arbitrary, no - they would be so if we lost sight of the experiments which led the founders of science to adopt them, and which, imperfect as they were, were sufficient to justify their adoption. It is well from time to time to let our attention dwell on the experimental origin of these conventions."[96]

Above all, it should not be supposed that there is the slightest artificiality in Poincaré's account of the precise relationship between conventions and experiment. It is unfortunate that many philosophers are characterized by

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[94] The Value of Science, p. 110.

[95] Science and Hypothesis, p. 105.

[96] Ibid., p. 110.

by their desire for neatness and system. They all too often distort science when they apply their Procrustean systems to it. It may well be that Poincaré's position has been misunderstood because philosophers would like to have read in his works that scientific laws are experimental <u>or</u> that scientific laws are conventional. But Poincaré, in effect, is saying that they are a little of each. This detracts from the dramatic impact which some philosophers like so much to convey but it is eminently sound as anyone who has had direct and practical contact with science will know. For example, suppose that in engineering thermodynamics an aspect of the performance of a jet engine could be represented by plotting its thrust against its internal temperature. The results will be a series of points which cannot be joined by any smooth curve. But a smooth curve is drawn, nevertheless. It is the curve which joins as many of the experimental readings as possible and departs as little as possible from the rest. We have to admit that the smoothness of the curve is after all conventional. But is the curve itself arbitrary? One would hardly go to the trouble of conducting lengthy and expensive engine tests if it were.

It should not be supposed that Poincaré's elaborate demonstration of the definitional character of mechanical laws was intended as a criticism of classical mechanics. Poincaré would have argued that the laws of relativistic mechanics are equally conventional. Euclid's postulate is a convention, but that does not suggest its abandonment. To use the language of Reichenbach, Poincare has shown that the laws of mechanics require coordinative definitions in terms of the behaviour of rigid bodies before they can be employed. Furthermore, Einstein's criticism of Newtonian mechanics is not to be construed as a rejection of the system itself. Einstein rejected Newtonian mechanics because when we seek to apply it to nature, we find that no coordinative definition is possible. This is particularly apparent with regard to the absolute time of classical mechanics.

In the general theory of relativity of 1915, Einstein extended the principle of relativity by showing that it is not only impossible to detect a real velocity but equally impossible to detect a real acceleration or a real rotation. The principle of relativity is then expressed in its most radical form as advocated by Ernst Mach. It completely denies the physical significance of space. It is remarkable that Poincaré should have anticipated Einstein to such an extent as to recognize this. While discussing the problem of relative and absolute motion in <u>Science and Hypothesis</u>, Poincaré expresses some surprise that the principle of relativity applies to velocities but not to accelerations.

> "Why is the principle only true if the motion of movable axes is uniform or in a straight line? It seems

> that it should be imposed upon us with the same force if the motion is accelerated, or at any rate if it reduces to a uniform rotation."[97]

Poincaré now proceeds to discuss another hypothetical world. This one is like our own earth but is surrounded by dense clouds so that its inhabitants would be unaware of the existence of the stars and planets. Would these people imagine their world to be motionless? Poincaré suggests that they would have to wait much longer than we for a Copernicus. But eventually one would turn up. The scientists would be unable to account for Foucault's pendulum experiment, for the flattening of the poles, and for the general lack of symmetry in nature (centrifugal forces). The Copernicus of this imaginary world would reach the conclusion that all of these arbitrary and isolated mysteries could be accounted for on the single assumption that the earth rotates. Just as our own Copernicus explained to us that the laws of astronomy can be expressed in far simpler language on such an assumption, so the hypothetical Copernicus would point out that in this way the laws of mechanics admit of much simpler expression.

Poincaré now makes the remarkable point that this discovery would by no means confer any absoluteness on space.

> "And hence this affirmation: 'the earth turns round,' has no meaning, since it cannot be verified by experiment; since such an experiment not only cannot be realised or even dreamed of by the most daring Jules Verne, but cannot even be conceived of without contradiction; or, in other words, these two propositions, 'the earth

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[97]*Ibid.*, p. 113.

> turns round,' and, 'it is more convenient to suppose that the earth turns round,' have one and the same meaning. There is nothing more in one than in the other."[98]

The foregoing was precisely the conclusion which Einstein reached through the most abstract mathematical reasoning. This should suffice to suggest the heuristic value of the thesis of conventionalism.

A later proponent of conventionalism, Pierre Duhem, took this point a little too far. As a Roman Catholic, he saw it as a possible justification for the fate of Galileo at the hands of the Inquisition. Poincaré was far too sensible to employ his doctrine for the reintroduction of Ptolemaic astronomy. He stresses the point that conventions are not the free creation of the scientist. The scientist is inevitably constrained by experience. Thus, our conventions do convey information about the world. However, this information is only concerned with relations. "To affirm the immobility of the earth would be to deny these relations, that would be to fool ourselves."[99]

> "The truth for which Galileo suffered remains, therefore, the truth, although it has not altogether the same meaning as for the vulgar, and its true meaning is much more subtile, more profound and more rich."[100]

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[98] Ibid., p. 117.

[99] The Value of Science, p. 141.

[100] Idem.

CHAPTER VIII

CONVENTIONALISM AND MECHANICS

II ABSOLUTE TIME AND CAUSALITY

It has already been noted that Newton, who was well aware of the difficulties involved in the notion of absolute space, felt no misgivings about the absoluteness of time. The first breach in this concept was made by Lorentz. However, Lorentz believed the time transformation applied only to electro-magnetic phenomena. Moreover, as we have seen, he distinguished between local, relative time and real, absolute time. Thus, the doctrine of the relativity of time may truly be regarded as the creation of Einstein. Let us proceed to consider the main features of Einstein's argument.

Einstein expounded his interpretation of time in the following simple illustration.[101] Let us suppose that lightning strikes the rails on a railway at two places A and B which are far apart. We are told that the two events occurred simultaneously. This, at first, would appear to be a meaningful statement, but if it has any physical

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[101] Relativity: The Special and General Theory, p. 25ff.

significance it must be accessible to experimental verification. But what sort of experiment would verify a statement about the simultaneity of two events?

After some reflection, the following definition of the simultaneity of the two strokes of lightning might be given. Connect the line AB and carefully determine its mid-point M. The observer should be placed at M with two suitably arranged mirrors. If light rays from the two events reach his mirrors at the same time, then the two events are simultaneous. This definition would be quite proper, according to Einstein, provided it be recognized that it is based on what he calls the "stipulation"[102] that the light travelling along the path AM has the same velocity as light travelling along BM. This could not be an empirical determination, since no method of measuring time may be presupposed. Similarly, physics may define time by placing clocks at the points A, M and B, whose hands are simultaneously set. It is "stipulated" that the several clocks are going at the same rate. In other words, it is possible to define physical time, provided that we begin by adopting various conventions about the behaviour of clocks and light rays.

So far, we have reached a definition of time with respect to a particular coordinate system, the railway embankment. In accordance with the methods of physics, we must now discover whether our definition is invariant, i.e.

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[102] *Ibid.*, p. 28.

whether it transforms into itself with respect to other coordinate systems.

Let us imagine that a long train is moving along the track with a constant velocity <u>v</u> in the direction AB. The train will constitute a second Galilean frame. Accordingly, we set up an experimental arrangement on the train similar to the one on the embankment. Let M' be the midpoint of AB on the moving train. When the flashes occur at A and B as judged from the embankment, M' will coincide with M. The second observer at M' will move towards the light ray from B and away from the light ray from A. Consequently, he will observe the light from B <u>before</u> he observes the light from A. In short, with respect to the train, the two events will be judged to be successive rather than simultaneous. We must conclude that every Galilean frame has its own temporal order. That is to say, time is a relativistic concept. Let us see how closely Poincaré's pre-relativity analysis of time accords with Einstein's theory.

Poincaré points out that we must distinguish between subjective, "psychologic" time which is given to us, and the objective time order of physical events in which there is no consciousness.[103] Poincaré distinguishes two questions which follow from this distinction:

> "1. Can we transform psychologic time, which is qualitative, into a quantitative time? 2. Can we reduce

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[103] <u>The Value of Science</u>, p. 26f.

> to one and the same measure facts which transpire in different worlds?"[104]

Poincaré begins by considering the problem of temporal congruence, i.e. the equality of two separate intervals of time. He points out that there is no direct intuition of such an equality. Is there, then, any physical determination of temporal equality? One might resort to the use of a pendulum, assuming that all the beats of the pendulum define equal intervals. But such a definition would lack precision since the period of the pendulum will vary with barometric pressure, temperature and so forth. Thus, scientists must turn to the sidereal day for a definition of time. Then all our terrestrial clocks will be corrected in accordance with the time taken by the earth to complete one full rotation about its axis. But we are then assuming that the rotational velocity of the earth is absolutely uniform, and we have no evidence for such an assumption. In fact, some astronomers believe that the angular velocity of the earth is gradually decreasing.

Is it at least possible to conceive of a perfect physical clock? Poincaré points out that the employment of any clock, be it the rotating earth or a pendulum, as a basis for the objective measure of time must rest on one initial postulate, namely, "*that the duration of two identical phenomena is the same*," or, "that the same causes take the

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[104]*Ibid.*, p. 27.

same time to produce the same effects."[105]

Let us trace the implications of this postulate. Suppose that in a certain region of space an event a occurs which produces, after a certain interval of time, the effect a'. In another region of space, very distant from the first, an event b occurs with the effect b'. Let us now suppose that a and b are simultaneous and that a' and b' are also simultaneous. Let us suppose that under roughly similar conditions the event a occurs once more and that simultaneously b is also reproduced. The two events are followed by a' and b' respectively, as before. Finally, we shall imagine that a' occurs perceptibly before b'. If we were witness to such a state of affairs we would be bound to admit that our postulate is absurd. Yet there is nothing self-contradictory about the foregoing suppositions. We must, therefore, conclude that there is no *a priori* basis for our postulate.[106]

Poincaré proceeds to point out that the postulate faces a further difficulty in that it assumes that a single discriminable cause produces a certain effect. In fact, however, this is rarely the case. For example, the period of

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[105] *Ibid.*, p. 28.

[106] It is quite obvious that the postulate is really a definition. Although Poincaré failed to say so, his general position would have been strengthened had he pointed out that we may adopt the postulate as a convention, in which case the observed discrepancy between aa' and bb' could be attributed to a difference in the rates of the two clocks which were employed to measure the two intervals.

a pendulum is due _almost_ solely to the earth's contraction. In all rigour, however, even the attraction of Sirius would have some effect on the pendulum. In the final analysis we must, therefore, modify our postulate to assert that, "Causes almost identical take almost the same time to produce almost the same effects."[107]

But these approximate rules are surely not adopted by astronomers when they suggest that the earth is slowing down. On what basis do they posit such an hypothesis? For one thing, they would argue that the friction of the tides will produce heat and so destroy _vis viva_.[108] Again, they might argue that the secular acceleration of the moon is greater than what is predicted by Newton's laws. In practice, then, astronomers define time in such a way that the laws of motion are preserved. But if we treat the laws of motion as experimental truths, the definition of time is still only approximate. Suppose that some other method of measuring time were adopted. The experimental basis of Newton's laws would be unchanged, but the enunciation of those laws would be greatly complicated.

> "So that the definition implicitly adopted by the astronomers may be summed up thus: Time should be so defined that the equations of mechanics may be as simple as possible. In other words, there is not one way of measuring time more true than another; that which is generally adopted is only more _convenient_.

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[107] _The Value of Science_, p. 29.

[108] i.e. kinetic energy.

> Of two watches, we have no right to say that the one goes true, the other wrong; we can only say that it is advantageous to conform to the indications of the first."[109]

Poincaré now proceeds to discuss the problem of simultaneity, although he correctly points out that this is really another aspect of the preceding discussion. We habitually speak of the simultaneity of phenomena as, for example, when we say that two psychological phenomena occurred simultaneously in two separate minds. What is meant by this? Furthermore, what do we mean when we say that a physical phenomenon which is not a part of any consciousness occurred before or after a certain psychological phenomenon? For example, in 1572 Tycho Brahe observed a new star. The light from this star took at least two hundred years to reach him. Therefore, the birth of the new star occurred before the discovery of America. When we say that this great phenomenon, which occurred unwitnessed, preceded the visual image of America in the consciousness of Columbus, what do we mean? Poincaré suggests that such assertions only acquire their meaning on the basis of a convention.

In the first place, how are we able to represent so many different worlds in a single frame which we call the external universe? It seems that we form the conception of an infinite intelligence which could represent all the events in the universe in its own time. Surely, some such hypothesis

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[109] The Value of Science, p. 30.

is unconsciously adopted whenever we speak of a time in which all the events in the universe take place. However, the notion of an infinite intelligence is obviously unsuitable as a basis for scientific assertions.

Let us consider some examples. I write a letter to my friend. Subsequently my friend reads that letter. Two visual images have occurred in two impenetrable consciousnesses. Yet, under no circumstances, would we hesitate to assert that one phenomenon is prior to the other. This is obviously because we regard one event to be the cause of the other. Again, I infer from the sound of thunder that an electrical discharge has occurred. I do not hesitate to assert that the physical phenomenon is prior to the psychological one, because it is its cause.

In other words, it would appear that time is defined in terms of causation. However, when we find that two phenomena are constantly conjoined, how do we determine which is the cause and which the effect? Surely, the anterior phenomenon is regarded as the cause of the other. That is to say, we define the causal relationship in terms of time! Thus it would seem that we are guilty of a *petitio principii*. "We say now *post hoc, ergo propter hoc*; now *propter hoc, ergo post hoc*; shall we escape from this vicious circle?"[110]

I must interrupt this exposition to point out that

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[110]*Ibid.*, p. 32.

while I am in general agreement with the position which Poincaré upholds anent the problem of time, his argument does appear to be a trifle weak in the last detail. While this does not actually harm his thesis, it would seem that Poincaré might have made his point in a much less complicated way; moreover, in a way which is fully consistent with the doctrine of conventionalism.

Specifically, Poincaré has been misled by an inadequate philosophical conception of the causal relationship. I refer to that type of treatment of causality which was made so famous by Hume. Hume defined causality in terms of the constant conjunction of two phenomena, A and B. He argued that if this conjunction is observed a sufficient number of times, we will eventually come to attribute a necessary connection to the two events, A and B. Hume appears to have been thinking of the common sense notion of causal events as, for example, when I strike a match and the match ignites.

This description of causality obviously depends on the significance of the notion of the recurrence of the phenomenon A. A is taken to be repeatable. But how do we know that a particular situation actually reveals the recurrence of state A? When we consider the universe as a whole, we are surely entitled to assert that it never repeats the same state twice. We would, of course, have to make the reservation that in defining the recurrence of state A, we disregard those circumstances in the universe which are irrelevant to A. But how do we know whether a particular circumstance is relevant or not? We can only determine whether a particular fact must be regarded as relevant to or, what is the same thing, a part of state A according to whether it is followed by state B. In short, we are saying that A and B are causally related if a recurrence of A is invariably followed by a recurrence of B, and that a recurrence of A is defined as a state which is followed by a recurrence of B. In other words, Hume's definition of the causal relationship turns out to be tautologous as soon as we attempt to define it in operational terms.

Does this imply that the causal laws of science are really vacuous tautologies which tell us nothing about the world? Not at all. It simply means that the form of the causal law is not such that it expresses the constant conjunction of two phenomena. With the present exception of thermodynamics, physical, causal laws have the form of differential equations. These equations express how one or more variables vary in value with respect to another variable which is time. In other words, a causal law does not describe several identical instances of a static configuration but the evolution or manner of change of a single physical system in time. We may say that there is a causal relationship between A and B when we observe that a state A_1, which is very close to A, is accompanied by a state B_1, which is very close to B, and that a state A_2 which is very close to A_1 is accompanied by a state B_2 which is very close to B_1. Therefore our causal law asserts the following relationship.

$$\begin{array}{lll} A_1 & B_1 \ldots\ldots\ldots\ldots\ldots\ldots & t_1 \\ A_2 & B_2 \ldots\ldots\ldots\ldots\ldots\ldots & t_2 \\ \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot \\ A_n & B_n \ldots\ldots\ldots\ldots\ldots\ldots & t_n \end{array}$$

It should be noted that the above series is linear. If it were not, the differential equations of mechanics would be of a much more complicated and difficult type. Hence, we may conclude that the distinction between cause and effect is defined in such a way that the laws of mechanics may be expressed in the form of uncomplicated differential equations.

Not only does the foregoing interpretation fit very neatly into the framework of conventionalism but, moreover, is compatible with the Einsteinian conception of time. According to Einstein, the relativity of time cannot be such that the order of cause and effect is reversible from one coordinate system to another. The reason for this is obvious. The velocity of light, according to the Lorentz transformations, is the maximum velocity which is physically attainable. Consequently, a causal influence cannot be transmitted more rapidly than the velocity of electro-magnetic propagation. Therefore, if two distant events are causally related, their order of succession will be the same in all coordinate systems. However, if two events are so far apart in space but occur so close together in time that a causal connection between them is precluded, we may <u>stipulate</u> that they occurred simultaneously. In that case their order of succession could be changed from one coordinate system to another. It would seem that Poincaré's treatment of the problem of time could have been improved in this particular regard. That is not, however, to detract from his overall position or his brilliance in its exposition.

Poincaré prefers to attack the problem in terms of a psychological analysis.[111] I perform an action A which is followed by the sensation D which I regard as its consequence. Moreover, I suppose that D is not the direct effect of A but related to it through the external circumstances B and C, i.e. B is the effect of A, C is the effect of B, and D is the effect of C. But why, Poincaré wonders, do we insist on the order A, B, C, D? I regard A as the initial cause because it is accompanied by the sense of my being active. Similarly, D is regarded as the final effect because it is a passively received sensation. The order of B and C appears to be more arbitrary. We would tend to justify it by asserting that in our experience, we invariably perceive B before C. However, we are faced with a certain difficulty because we have no <u>direct</u> experience of B and C but only the experience of

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[111] <u>Idem</u>.

the corresponding sensations B' and C'. We know intuitively that B' precedes C' and suppose, therefore, that B precedes C. While this is admittedly a natural enough criterion, there are exceptions. For example, we may perceive a near flash of lightning before a distant one although the nearer of the two is actually later.

There is still another difficulty to be faced in our attempt to define the temporal series in terms of causality. If we grant the causal interdependence of the various parts of the universe, a given effect must be the product of an infinitely complex cause. Let us, however, consider a case which is somewhat less than infinitely complex. Take three bodies such as the Sun, Jupiter and Saturn. Furthermore, let us suppose that they constitute an isolated system of three mass points. Their positions and velocities at one time will suffice to determine their positions and velocities at all times, past and future. Their positions at time t will determine their positions at $t + h$ and $t - h$. Moreover, the position of Jupiter at time t and that of Saturn at $t + h$ together suffice to determine all positions of Jupiter and Saturn at all times. If we carry this further, we may say that the position of Jupiter at $t + e$ and of Saturn at $t + a + e$ are connected through a complicated law with the position of Jupiter at time t and of Saturn at $t + a$. It should then be possible to call one of these aggregates the cause of the other, in which case t and t + a would be regarded as simultaneous. Poincaré points out that the reasons against the adoption of such a procedure would merely be "convenience and simplicity."[112]

Along the lines of our earlier suggestion, it should be noted that a differential equation does not refer to discrete causes and effects. We may arbitrarily select two states A and B which are as close in time as we choose. We could then say that A is the cause of B. However, it will be possible to choose a state which is temporally situated between A and B which we may regard as the effect of A or the cause of B. The point is that there is no physical state which carries the label, "I am a cause," or "I am an effect." However, when Poincaré states that the criteria of "convenience" and "simplicity" determine the selection of causes and effects such that the laws of mechanics will be as simple as possible, i.e. of Newtonian form, it would seem that his position is, after all, not very far from what we have suggested to be the proper application of conventionalism to this particular matter.

> "The simultaneity of two events, or the order of their succession, the equality of two durations, are to be so defined that the enunciation of the natural laws may be as simple as possible. In other words, all these rules, all these definitions are only the fruit of an unconscious opportunism."[113]

Poincaré clearly maintains the position, later

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[112] Ibid., p. 34.

[113] Ibid., p. 36.

adopted by Einstein, that two distant events are not observed to be simultaneous but that it is fruitful in the description of the physical world to stipulate their simultaneity. The ascription of simultaneity is conventional. However, the convention is not arbitrary. The precise conditions under which such a stipulation is made are determined by experience.

This is precisely the conclusion which was reached by Einstein. The fact that the velocity of electro-magnetic propagation is finite compells us to abandon the attempt to observe simultaneity, i.e. to define it in operational terms. Moreover, the actual magnitude of this velocity determines the circumstances in which such a stipulation may be made. To put the matter as simply as possible, if the space-time coordinates of two events are such that the events must be causally independent, then we may stipulate that they occurred simultaneously. However, this convention is suggested by the finitude of electro-magnetic propagation. The latter, in the theory of relativity, is not a convention but an empirical fact which could be experimentally falsified.

Furthermore, as the nature of the Lorentz transformations clearly reveals, such considerations are only physically significant when we deal with distances and velocities which are so great that they are significant compared with the velocity of light. Otherwise, relativistic effects are too minute to be taken account of, and we may fruitfully revert to the classical mechanics.

It is striking that from what was largely a philosophical theory, Poincaré reached precisely the same conclusion as Einstein.

> "Perhaps, too, we shall have to construct an entirely new mechanics that we only succeed in catching a glimpse of, where, inertia increasing with the velocity, the velocity of light would become an impassable limit. The ordinary mechanics, more simple, would remain a first approximation, since it would be true for velocities not too great, so that the old dynamics would still be found under the new. We should not have to regret having believed in the principles, and even, since velocities too great for the old formulas would always be only exceptional, the surest way in practise would be still to act as if we continued to believe in them. They are so useful, it would be necessary to keep a place for them."[114]

We know of no greater philosophically based prophecy than what is embraced in the last quotation!

[114] *Ibid.*, p. 111.

CHAPTER IX

CONVENTIONALISM AND MECHANICS

III RELATIVITY THEORY

Superficially, it would seem that the thesis of conventionalism as it applies to mechanics is not precisely the doctrine which Poincaré enunciated regarding the nature of the axioms of geometry. The reader will recall that Poincaré maintained that under no circumstances would experience impose the necessity of abandoning our Euclidean conventions. But now Poincaré tells us that it may be that we shall have to abandon our Newtonian conventions for others which are more precise. Surely, if the classical laws of motion were conventional, there would be no question of their abandonment.

Poincaré was fully aware of this potential objection to his position. He himself expressed it as follows:

> "Have you not written, you might say if you wished to seek a quarrel with me - have you not written that the principles, though of experimental origin, are now unassailable by experiment because they have become conventions? And now you have just told us that the most recent conquests of experiment put these principles in danger."[115]

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[115] *The Value of Science*, p. 109.

We shall quote his reply at length.

"Well, formerly I was right and to-day I am not wrong. Formerly I was right, and what is now happening is a new proof of it. Take, for example, the calorimetric experiment of Curie on radium. Is it possible to reconcile it with the principle of the conservation of energy? This has been attempted in many ways; but there is among them one I should like you to notice; this is not the explanation which tends to-day to prevail, but it is one of those which have been proposed. It has been conjectured that radium was only an intermediary, that it only stored radiations of unknown nature which flashed through space in every direction, traversing all bodies, save radium, without being altered by this passage and without exercising any action upon them. Radium alone took from them a little of their energy and afterward gave it out to us in various forms.

"What an advantageous explanation, and how convenient! First, it is unverifiable and thus irrefutable. Then again it will serve to account for any derogation whatever to Mayer's principle; it answers in advance not only the objection of Curie, but all the objections that future experimenters might accumulate. This new and unknown energy would serve for everything.

"This is just what I said, and therewith we are shown that our principle is unassailable by experiment.

"But then, what have we gained by this stroke? The principle is intact, but thenceforth of what use is it? It enabled us to foresee that in such or such circumstance we could count on such a total of energy; it limited us; but now that this indefinite provision of new energy is placed at our disposal, we are no longer limited by anything; and as I have written in 'Science and Hypothesis,' if a principle ceases to be fecund, experiment without contradicting it directly will nevertheless have condemned it."[116]

We see, then, that by some method, albeit devious, it is always possible to retain our convention. However, the criteria of convenience and simplicity may persuade us to adopt an alternative. It should be noted that we are persuaded but not compelled. We feel fairly confident that

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[116] *Ibid.*, p. 109f.

if this feature of conventionalism were more widely understood, there would be far less criticism of the doctrine. Some of this criticism would appear to treat conventionalism as though it were the attempt to reduce science to some sort of word game. Plainly, this was not Poincaré's position. One of the great advantages of Poincaré's philosophy of science is that it is quite consistent with the various advances which have been made in both theoretical and experimental science. Here lies one of the superiorities of Poincaré's philosophy over, for example, the Kantian philosophy of science which obviously would have tended to inhibit the development of science had it been taken very seriously by practicing scientists.

If Poincaré is correct, we are bound to conclude that the special theory of relativity is not imposed on us by empirical data but is merely the most convenient way of representing them. To show this would be to complete the defense of this most important aspect of Poincaré's thesis.

As a theory of mechanics, relativity theory makes assertions about the behaviour of clocks and measuring rods. It maintains that clocks which are in motion slow down and that measuring rods which are in motion contract in the direction of that motion. While Lorentz did not believe that clocks are "really" affected by motion, he did believe the contraction of measuring rods to be a "real" contraction.

Einstein, on the other hand, maintained that both of these effects were apparent. For example, a moving clock does not slow down for an observer who is at rest with respect to it. Consequently, if we consider two observers O and O_1 at rest respectively in the systems S and S_1 which are in rectilinear motion with respect to each other, O will hold that the clocks in S_1 are running slow, while O_1 will hold that the clocks in S are running slow. To those of us who are more familiar with metaphysics than with physics, it would be tempting to say that O and O_1 cannot both be right. However, the physicist who, for very good reasons, does not concern himself with the "ultimate" nature of things would say that each observer has performed a correct measurement. It is merely that every measurement is restricted to some particular coordinate system. O and O_1 are not in real disagreement. They have merely selected different coordinate systems. The reader will recall that Poincaré suggests that in the genesis of the notion of space one tends to regard oneself as the fixed axis of the system to which we refer our sensations. O has chosen the coordinate system S while O_1 has chosen S_1. The essence of relativistic physics, Einsteinian and pre-Einsteinian, is that there is no means of selecting one system rather than another; all systems are equivalent. In other words, we may choose the system which we happen to prefer. In general, the most convenient choice will be that system with respect to which we are at rest. In everyday matters this will be the surface of the earth.

For the physicist, who is concerned with inertial and centrifugal forces, it will be the fixed stars. But in all cases, the choice is a matter of convention!

Similar considerations pertain to the contraction of measuring rods. As we have already noted, Lorentz believed that there were <u>real</u> contractions with respect to the ether. It is for this precise reason that Lorentz cannot properly be said to have anticipated Einstein. The latter discarded the idea of the ether but showed that the contraction can only be defined with respect to a system of coordinates. Again, the choice of a coordinate system is a matter of convention.

The relativistic increase of mass is even more clearly a conventional postulate. Experiment reveals that a constant force does not produce a constant acceleration at velocities which are significant fractions of the velocity of light. In other words, the relationship f = ma appears to break down. But Newton's "quantity of matter" must be defined as the ratio of accelerations. Hence, we are faced with a difficult alternative. On the one hand we may abandon Newtonian mass and adopt the Lorentz formula, $m' = m/\sqrt{1 - \frac{v^2}{c^2}}$ or we may abandon the principle of the conservation of momentum. <u>Experiment tells us that we must make a choice, but does not tell us what it should be.</u>

Philipp Frank has stated the position with the utmost clarity.

"Actually, the question of which is the 'legitimate successor' of the Newtonian 'quantity of matter' cannot be decided. The 'rest-mass' has inherited the property of 'constancy' while the 'relativistic mass' that is defined by f/a has inherited the property of being the ratio of force to acceleration. Hence, the question of which of them should be declared the 'legitimate heir' of the Newtonian mass can only be decided *upon the grounds of convenience, simplicity and similar types of consideration.*"[117]

"All these considerations show us that if we introduce 'mass' as the object which has as many as possible of the properties of the old Newtonian mass, this is the only possible justification for the introduction of statements like 'mass is not constant,' or 'mass can disappear.'"[118]

In short, the principle of conservation of momentum is so fundamental to physics that it would be most inconvenient to abandon it. We prefer to adopt the convention of postulating a relativistic increase of mass.[119]

Perhaps the most striking defense of the conventionalist interpretation of relativistic mechanics was made by L. Rougier.[120] Rougier's argument is to the effect that if Poincaré's interpretation of geometry is correct, i.e. if we are justified in holding that Euclidean axioms are suggested by experience, then one should expect that under the right circumstances some other set of axioms might

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[117] *Philosophy of Science*, p. 147. The italics are mine.

[118] *Ibid.*, p. 148.

[119] This incidentally suggests the clearly conventional character of the conservation laws.

[120] *De l'utilisation des Géométries non-Euclidiennes dans la physique de la relativité*, *L'Enseignement Mathématique*, Jan. 15, 1914, pp. 5-18.

be suggested.

This in fact is precisely the outcome of relativistic mechanics. We have seen that bodies contract in the direction of their motion in accordance with the Lorentz formula. In short, the material objects which constitute the basis for a Euclidean physical geometry no longer exist.

Rougier shows that such a contraction may be assigned to solid objects in translatory motion in a space of negative curvature of the Lobatschewskian type. Moreover, just as there can be no parallelogram of velocities in relativistic mechanics, so in Lobatschewskian geometry there are no parallelograms.

Thus, it can be shown that by selecting the appropriate constant of curvature for space, the Einsteinian equations are transformed into classical equations! It should be noted that Einstein expressed his theory in terms of Euclidean geometry while postulating a deformation of material objects. This presents us with an elegant illustration of the applicability of the thesis of conventionalism. We may treat natural solids as rigid bodies of the Lobatschewskian type or as deformable bodies of the Euclidean type. The two alternatives are equivalent. The implication of the theory of relativity is, surprisingly, that the Lobatschewskian geometry would be the simpler. This, of course, Poincaré failed to anticipate. But his failure was not the outcome of a defect in his theory but merely of an inability to foresee some rather startling experimental results.

Finally, let us consider the general significance of the Lorentz equations in the theory of relativity. For Lorentz himself, as we have seen, these equations were supposed to represent genuine physical hypotheses about the behaviour of clocks and measuring rods. But this is not the role which they play in the special theory of relativity.

We may understand their significance for Einstein if we consider why he retained them. If they do not assert that rods are contracted or clocks slowed down in any absolute sense, what was Einstein's reason for retaining them? It is quite clear that they are only retained as transformation rules, i.e. to express the relationship between the various inertial systems. But if they are merely transformations without genuine physical significance, why did not Einstein retain the much simpler Galilean transformations? The point is that while they are merely transformations they are nevertheless not devoid of physical significance. They are the only transformations for which the laws of mechanics and electro-magnetism are invariant. If we apply the Galilean transformation rules to Maxwell's electro-magnetic field equations, the latter will undergo a fundamental change of form. We would not be entitled to say that therefore the Galilean transformations are false; it is merely that they would make the enunciation of physical laws too unwieldy and complicated.

In short, we retain the Lorentz equations because of their systematic simplicity. That is to say, they are the most convenient rules for the description of physical data. Once again, we are led to Poincaré's central theme which I beg the reader's indulgence to repeat. The laws of mechanics are conventional, but they are conventions which have been suggested by experience.

Probably one of the most celebrated of the consistent critics of the conventionalistic interpretation of mechanics was Moritz Schlick. Like Reichenbach, Schlick seems to be labouring under certain misapprehensions about the meaning of conventionalism. Certainly he understands the doctrine in a way which is foreign to the ideas of Poincaré.

I believe that Schlick would accept the conventionalist's interpretation of geometry. Euclidean geometry, considered as an axiomatic system, is merely the grammar or syntax which we adopt to describe certain features of the world.

> "The language in which we speak of physical relations must after all also have its own grammar and there is no doubt that this is determined by convention. Are natural laws these conventions perhaps? Do the natural laws represent nothing else but the grammar of the natural sciences, i.e., in the last analysis, of physical language in general?"[121]

Again,

> "The difference between a stipulation and a genuine proposition obviously lies in the fact that the

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[121] Schlick, M., "Are Natural Laws Conventions?" in <u>Readings in the Philosophy of Science</u>. (H. Feigl & M. Brodbeck, Ed.) pp. 181-182.

> validity of a convention is of our own making. After a stipulation has been made, we can maintain it under any circumstances. Experience might well suggest but can never compel its abandonment, for the validity of a convention remains in our power. It is well known that facts of nature can be described by means of Euclidean geometry, if we stubbornly insist upon it."[122]

Schlick holds that if natural laws could be interpreted in the same manner as geometrical axioms, the conventionalist position would be established. However, he argues that the belief in the conventional character of natural laws rests on a grave, logical error.

Specifically, Schlick argues that the conventionalists have failed to understand the distinction between a sentence and a proposition. He defines a sentence as, "a sequence of liguistic signs with the help of which something can be asserted."[123] A proposition "is to be viewed simply as the set of rules which are stipulated for the actual application of the sentence, that is, for the practical use of the sentence in the representation of facts."[124]

In short, a proposition is what a sentence "means." Now, we may stipulate a sentence to mean anything that we choose it to mean. Moreover, it is true that natural laws are expressed in sentences. However, Schlick suggests that if a natural law were no more than a sentence, then we could draw the absurd conclusion that it is legitimate to write

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[122] Ibid., p. 182

[123] Ibid., p. 185.

[124] Idem.

down any sentence and call it a natural law. In fact, natural laws are sentences which express propositions. We may vary the sentence but we cannot alter the proposition which it contains. The latter is determined by the essential character of the physical world.

> "Once the rules are fixed, i.e., once agreement is reached concerning the grammar of the scientific language, then there is no longer any choice about how to formulate any facts of nature. After this there is only one possibility, only one way of formulating the sentence which will fulfill the purpose. A natural law can then be represented in only one quite definite form and not in any other."[125]

But Poincaré never intended his doctrine to mean any more than what Schlick asserts in the last quotation. The extreme form of conventionalism, which Schlick may have had in mind, what Poincaré calls "nominalism," is the subject of vigourous attack by Poincaré himself.[126] For example, he writes that, "I can not admit that the scientist creates without restraint the scientific fact since it is the crude fact which imposes it upon him."[127] "...*when I have laid down the definitions, and the postulates which are conventions*, a theorem henceforth can only be true or false."[128] Poincaré would regard as fantastic the claim, "that the facts of daily

[125] *Ibid.*, p. 187.

[126] *The Value of Science*, p. 112ff.

[127] *Ibid.*, p. 116.

[128] *Ibid.*, p. 118.

life are the work of the grammarians."[129]

In short, Poincaré is obviously exempt from the criticisms which Schlick levels against the doctrine of conventionalism. In fact, he would undoubtedly have joined with Schlick in making them. Poincaré was attempting to show that the language in which we express the laws of nature cannot be taken to convey anything about the real constitution of nature itself. This is the real significance of what Reichenbach called Poincaré's theory of "equivalent descriptions." It is also what Schlick meant in his distinction between sentences and propositions.

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[129] Ibid., p. 119.

CHAPTER X

CONCLUDING REMARKS:

CONVENTIONALISM AND RECENT DEVELOPMENTS

IN THE PHILOSOPHICAL FOUNDATIONS OF SCIENCE

We hope that it is by now abundantly clear to the reader that Poincaré's philosophy of science is not one more museum piece in the history of nineteenth century thought. We have shown not only its applicability to the classical mechanics but also the way in which it could serve to interpret the twentieth century conceptions of relativistic mechanics.[130] However, we propose now to show that Poincaré's views are directly in the mainstream of contemporary philosophic opinion.

The most active and vocal movement in recent philosophy of science has been logical positivism. While the writer admits to some sympathy for both the aims and

[130]In the preceding chapter we limited our discussion to a consideration of the theory of relativity. We would like to see someone in the future do the same sort of analysis with regard to quantum mechanics. For example, Schroedinger's wave mechanics and Heisenberg's matrix mechanics are two completely diverse mathematical theories which, nevertheless, yield identical empirical results about the mechanical behaviour of atomic objects.

achievements of this movement, it is not our purpose to defend the doctrines of logical positivism in this dissertation. We wish simply to show that Poincaré must be counted among the principal precursors of the movement. In fact, he may be said to have been more than its precursor. In his treatment of the structure of science he seems, with Ernst Mach, to have been among its first members. In short, then, we wish to show the striking similarity between the philosophy of Poincaré and what is possibly the most important philosophical interpretation of science in the present century.

In fact, Poincaré's relationship to logical positivism has not gone unrecognized. It is difficult to state precisely when this movement began. Those who point to Schlick as its founder base their opinion on somewhat arbitrary criteria. The movement was first known as "the Vienna Circle" due to the fact that its members held their weekly discussions in a Viennese coffee house. These meetings began in 1907. At this time the leading light of the Vienna Circle was the theoretical physicist Philipp Frank. If one man is to be regarded as the founder of the movement, Frank, it seems to us, has a greater claim than Schlick. However, it is unlikely that any member of the movement would press such a claim, as the cooperative spirit of its members has always been unusually high. In any event, Frank has pointed out that the three thinkers who were most widely discussed at the early meetings in the coffee house were Kant, Mach and Poincaré.[131]

The Kantian epistemology was, of course, rejected by the Vienna circle. However, Ernst Mach was the darling of these thinkers who eagerly embraced the latter's radical empiricism. But it was felt that the "sensationalism" of Mach was inadequate as a basis for scientific theories of broad generality. In describing the historical development of logical positivism Frank writes,

> "We felt very strongly that there was a certain gap between the description of observations, necessarily consisting, in physics particularly, of a small number of concepts (like force, mass, etc.) linked by statements of great simplicity. We admitted that the gap between the description of facts and the general principles of science was not fully bridged by Mach, but we could not agree with Kant, who built this bridge by forms or patterns of experience that could not change with the advance of science.
>
> "In our opinion, the man who bridged the gap sucessfully was the French mathematician and philosopher Henre Poincaré. For us, he was a kind of Kant freed of the remnants of medieval scholasticism and anointed with the oil of modern science."[132]

Frank proceeds to specify the nature of Poincaré's contribution to the germinal ideas of the Vienna Circle:

> "The traditional presentation of physical theories frequently consists of a system of statements in which descriptions of observations are mixed with mathematical considerations in such a way that sometimes one cannot distinguish which is which. It is Poincaré's great merit to have stressed that one part of every physical theory is a set of arbitrary axioms and logical conclusions drawn from these axioms. These axioms are relations

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[131]P. Frank, Modern Science and its Philosophy, pp. 1-52.

[132]*Ibid.*, p. 8.

> between signs, which may be words or algebraic symbols; the important point is that the conclusions that we draw from these axioms are not dependent upon the meanings of these symbols. Hence this part of the theory is purely conventional in the sense of Poincaré. It does not say anything about observable facts, but only leads to hypothetical statements of the following type: 'If the axioms of this system are true, then the following propositions are also true,' or still more exactly speaking: 'If there is a group of relations between these symbols, there are also some other relations between the same symbols.' This state of affairs is often described by saying that the system of principles and conclusions describes not a content but a structure. Hence, this system is occasionally referred to as the structural system."[133]

In the subsequent development of the philosophy of the Vienna Circle, the external influences were many and various. Not the least of these was the philosophical writings of Bertrand Russell. Russell developed a system of symbolic logic with the aid of which he showed that the statements of pure mathematics are reducible to formal logic. Although not strictly a logical positivist himself, his work came to have considerable bearing on the future of the movement.

Russell paid considerable attention to the study of logical paradoxes, as a result of which he developed his theory of logical types. According to this theory, every class is of a higher logical type than any of its members. Similarly, any statement about another statement is of a higher type than the statement which it is about. Russell pointed out that the confusion of logical types will lead to the meaningless grouping of symbols. While meaningless, the

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[133] _Ibid._, p. 12.

structure of such groupings would be considered correct from the point of view of rules of traditional grammar. Thus, Russell pointed out that sentences may be divided into three groups: the true, the false and the meaningless. It was fundamental to the logical positivists' hostility to and eventual elimination of metaphysics that grammatically correct sentences could be regarded as logically meaningless.

The new logical techniques of Russell were applied to the problems of epistemology as early as 1914.[134] _Occam's Razor_, the principle of parsinomy, was of major importance: _Entia non sunt multiplicanda praeter necessitatem_. Russell restated this principle as follows: "Whenever possible, substitute constructions out of known entities for inferences to unknown entities."[135] Russell also pointed out that,

> "_In so far_ as physics or common sense is verifiable, it must be capable of interpretation in terms of actual sense-data alone. The reason for this is simple. Verification consists always in the occurrence of an expected sense-datum. ...Now if an expected sense-datum constitutes a verification, what was asserted must have been about sense-data, or, at any rate, if part of what was asserted was not about sense-data, then only the other part has been verified."[136]

The foregoing amounts to a careful restatement of the empirical doctrines of Ernst Mach.

However, of all the external influences, the

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[134] Cf., _Our Knowledge of the External World_, p. 12.

[135] B. Russell, "Logical Atomism," in _Contemporary British Philosophy: Personal Statements_, First Series, p. 363.

[136] _Our Knowledge of the External World_, p. 89.

greatest and most profound was the Tractatus Logico-Philosophicus of Ludwig Wittgenstein. Virtually all of the matters of concern to logical positivism are to be found, although sometimes obscurely, in the pages of this one book.

> "Wittgenstein claimed bluntly that the problems of traditional philosophy are merely verbal problems. Our ordinary language, which has grown up to describe the facts of everyday life, is not adapted to the task of expressing and answering problems put to traditional philosophy. If we try to use our ordinary language in this way, we get into trouble. The real problem is to find out what one can actually say clearly. The world of facts can be described in our ordinary language; therefore, says Wittgenstein, 'to understand a proposition means to know what is the case if it is true.'"[137]

In short, Wittgenstein was primarily concerned with the relationship between language and the world. As Russell puts it, Wittgenstein's problem is the following: "What relation must one fact (such as a sentence) have to another in order to be capable of being a symbol for that other?"[138]

According to Wittgenstein, the objectives of propositions are "facts". Such facts, being the fundamental elements of the world, cannot be defined without circularity. However, "objects" which these facts compose may be defined in terms of the set of facts in which they occur. The totality of atomic facts is the world. All compound facts are therefore reducible to atomic facts. The atomic facts are various groupings of objects. The nature of the grouping is the

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[137] P. Frank, Op. cit., p. 32.

[138] B. Russell, Introduction to the Tractatus Logico-Philosophicus, p. 8.

"structure" of the fact.

With regard to the relationship between facts and propositions, Wittgenstein adopts a correspondence theory of truth. That is to say, he holds that a true proposition "agrees" with a fact, while a false proposition is one which is in disagreement with a fact. The notion of agreement and disagreement in this context is perhaps vague. However, Wittgenstein is unequivocal in his usage. A true proposition, he holds, is a "picture" of a fact. Now, in what sense did Wittgenstein intend the term "picture"?

For Wittgenstein, a proposition is a fact in its own right. In other words, it is a physical phenomenon such as a noise, a neural event or a set of physical symbols. The relationship which Wittgenstein postulates to hold between propositions and the world is thus somewhat easier to understand. It is a matter of the relationship between two facts. The proposition will "picture" the fact by virtue of its common logical form with that fact.

The point that we wish to bring out in this unduly cursory exposition of Wittgenstein's philosophy is that he held that the significance of a statement about the world is in its logical structure and nothing more. Wittgenstein brings out the point clearly when speaking of the propositions of mechanics. The physical world, he writes, may be regarded as a picture in black and white. We may superimpose on this picture a network of squares. Then we could describe the picture by asserting which squares are black and which are white. The form of the network is arbitrary. It might, for example, have been easier to work with a mesh of triangles or hexagons.

> "To the different networks correspond different systems of describing the world. Mechanics determine a form of description by saying: All propositions in the description of the world must be obtained in a given way from a number of given propositions - the mechanical axioms. It thus provides the bricks for building the edifice of science, and says: Whatever building thou wouldst erect, thou shalt construct in some manner with these bricks and these alone.
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> "(As with the system of numbers one must be able to write down any arbitrary number, so with the system of mechanics one must be able to write down any arbitrary physical proposition)"[139]

We would particularly like to draw the reader's attention to Wittgenstein's conclusion from the preceding considerations. The following quotation from the Tractatus Logico-Philosophicus might very well have been taken from the works of Poincaré.

> "So too the fact that it can be described by Newtonian mechanics asserts nothing about the world; but this asserts something, namely that it can be described in that particular way in which as a matter of fact it is described. The fact, too, that it can be described more simply by one system of mechanics than by another says something about the world."[140]

Wittgenstein's general position is identical with that of Poincaré. The latter, the reader will recall, had maintained that the objective knowledge which we have of the

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[139]Wittgenstein, L., Op. cit., p. 175.

[140]Ibid., p. 177.

world is about relations. Moreover, these relations may be expressed in a variety of ways. The particular mode of expression which we adopt is a matter of convention. However, it is natural to select that mode which affords the simplest account of the world. All of these ideas are to be found in the preceding quotations from Wittgenstein!

Wittgenstein's Tractatus was by no means the definitive work for the Vienna Circle. It might be said that Wittgenstein's application of empiricism was too rigorous (or possibly too obtuse) even for the logical positivists. Very briefly, Wittgenstein held that all propositions are truth-functions of elementary propositions, and that elementary propositions have an exclusively empirical reference. In short, all meaningful statements are empirical statements. But the statements in Wittgenstein's book, especially his statements about the use of language, are not empirical. Thus, his philosophy turns out to be self-stultifying. Wittgenstein, grasping the bull by the horns, recognizes this and concludes his Tractatus with what is surely one of the most remarkable passages in the literature of philosophy.

> "My propositions are elucidatory in this way: he who understands me finally recognizes them as senseless, when he has climbed out through them, on them, over them. (he must so to speak throw away the ladder, after he has climbed up on it.)
>
> "He must surmount these propositions; then he sees the world rightly.
>
> "Whereof one cannot speak, thereof one must be

silent."[141]

Obviously, if logical positivism was to be a viable movement, it could not rest content with the final outcome of Wittgenstein's analysis of language. One of the most significant events in the history of the Vienna Circle was the arrival in Vienna of Rudolph Carnap. It was he who gave the required solution to the foregoing difficulty. Moreover, to Carnap more than any other man, we owe the classical, definitive expression of the philosophy of logical positivism.

However, before dealing with his linguistic theories, there is another matter which concerned him in his early period, which should be mentioned for its bearing on the views of Poincaré.

An important task for logical positivism was to clear up an ambiguity in the notion of knowledge.[142] In German there are two words which signify knowledge, "Erkenntnis" and "Erlebnis." In English we may express the distinction by the phrases: "knowledge by description" and "knowledge by acquaintance." "Erkenntnis" is communicable knowledge. The logical positivists hold that communicable knowledge expresses nothing but the formal structure of experience. The content of experience is essentially private and, therefore, incommunicable. Its presence may be indicated by various

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[141] Ibid., p. 189.

[142] Cf., M. Schlick, Erleben, Erkennen, Metaphysik, Kant-Studien, 31, 1926, pp. 146-158.

demonstrative words such as "I", "here", "now", "this", "that", but this is as far as one can go. However, there are formal relations holding between _qualia_ which may be communicated. For example, consider the possibility that in the experience of two people, the qualities _red_ and _green_ were systematically interchanged so that whenever the one experienced red the other experienced green, and vice-versa. The two individuals would encounter no special difficulties in communicating with each other. In fact, all colours throughout the entire compass of experience might be interchanged without our communicable, scientific knowledge being affected in the least way. Such considerations are, of course, applicable not only to colours but to all of the traditional "secondary qualities." Hence, so far as we are concerned with communicable knowledge (_Erkenntnis_), the essence of a quality is of no significance. What is of importance, however, is the unique set of relations which holds between it and other qualities. The most systematic treatment of this matter was undertaken by Carnap in an early work, _The Logical Structure of the World_. Very briefly, Carnap attempted to outline a system whereby all the concepts of empirical science may be "constituted" or constructed from purely formal operations on a single primitive concept which is an indefinable. formal relationship.[143]

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[143]R. Carnap, _Der Logische Aufbau der Welt_. The details of this phase of Carnap's work are not pertinent to this dissertation.

We have drawn attention to the logical positivists' conception of the nature of scientific knowledge since it once more reveals the remarkable extent to which a contemporary positivistic doctrine was anticipated by Poincaré. Like the logical positivists, Poincaré held that objective, i.e. scientific, knowledge is essentially communicable. However, he points out that sensations cannot in themselves be communicated, but only the relationships between them.

> "The sensations of others will be for us a world eternally closed. We have no means of verifying that the sensation I call red is the same as that which my neighbour calls red.
>
> "Suppose that a cherry and a red poppy produce on me the sensation A and on him the sensation B and that, on the contrary, a leaf produces on me the sensation B and on him the sensation A. It is clear we shall never know anything about it; since I shall call red the sensation A and green the sensation B, while he will call the first green and the second red. In compensation, what we shall be able to ascertain is that, for him as for me, the cherry and the red poppy produce the <u>same</u> sensation, since he gives the same name to the sensations he feels and I do the same.
>
> "Sensations are therefore intransmissible, or rather all that is pure quality in them is intransmissible and forever impenetrable. But it is not the same with relations between these sensations.
>
> "From this point of view, all that is objective is devoid of all quality and is only pure relation. Certes, I shall not go so far as to say that objectivity is only pure quantity (this would be to particularize too far the nature of the relations in question), but we understand how some one could have been carried away into saying that the world is only a differential equation.
>
> "With due reserve regarding this paradoxical proposition, we must nevertheless admit that <u>nothing is objective which is not transmissible, and consequently</u>

that the relations between the sensations can alone have an objective value."[144]

The most mature phase of Carnap's work, and what is most typical of logical positivism in its fully developed state, may be described as the logical analysis of language. Actually, there are two distinct aspects to the logical analysis of language, formal syntax and formal semantics.

The formal or logical syntax of language was investigated by Carnap for the purpose of showing that it is possible, contrary to the belief of Wittgenstein, to discuss language in a meaningful way. The logical syntax of a language is the formal structure of that language apart from all considerations of meaning. For example, "The house is large," may be described as a sentence which contains an article, a noun, a copula and an adjective in that order. If, however, it is said that the sentence is about a house and that the last word designates a degree of magnitude, the description is no longer formal, since it concerns the meaning or sense of the words in the sentence. By language, in this context, Carnap means the formal rules whereby meaningful sentences may be constructed. There are two types of rules: formation rules and transformation rules. A formation rule of the English language is that four words in the order of article, substantive, verb and adverb constitute a sentence in that language. The formation rules

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[144] The Value of Science, p. 136. (The italics are mine).

of a natural language such as English are too numerous and complicated to be completely laid down. However, artificial languages, in which symbols replace words, may be constructed where all the formation rules are given. For example, an expression consisting of a predicate and a variable constitute a sentence such as $\phi(x)$. Two such expressions joined by a connecting sign together constitute a sentence such as $\phi(x) \vee \psi(x)$.

The transformation rules of a language are those which are commonly known as the rules of inference. By these is determined the number of sentences which can be inferred from a collection of sentences. In any language S, a sentence of S is defined by the totality of formation rules of S, and a direct consequence in S as the totality of transformation rules of S. Normally, "true" and "false" cannot be defined syntactically since the truth or falsity of a sentence will depend on the meaning of the symbols contained in it. However, it sometimes happens that sentences are true or false by virtue of their syntactical form. Such sentences would be called <u>valid</u> and <u>contravalid</u> respectively. These terms are syntactically definable.

The language so far described is noticeably barren and could hardly be described as a tool for the advancement of physical knowledge. This is due to the fact that all of its transformation rules and primitive sentences are of a purely logical or mathematical character. None of its terms have any extra-logical significance. They are what Carnap calls "L-terms." Similarly, all of its transformation rules are called "L-rules."[145] However, it is possible to incorporate into the language as primitive sentences what Carnap calls "P-rules" or physical rules of transformation. For example, we might include Newton's laws of motion or Maxwell's equations of the electro-magnetic field. Then a sentence C which is a consequence of a class P of premises on logical grounds alone is called an "L-consequence" of P. If it is necessary to apply P-rules also, the sentence is a "P-consequence" of P.

Even with the inclusion of P-rules, our language is still a purely formal structure and therefore not a complete scientific system. A scientific theory or system is an abstract calculus such as the one described above plus a further set of rules for its use. For example, we may deduce various consequences from Newton's laws of motion, but these consequences are not scientific predictions unless we know what they mean. That is to say, we require rules to relate our abstract calculus to the world. These rules would determine the conditions for the truth or falsity of a statement. But to know whether a statement is true or false is to know its meaning. Hence, the scope of our discussion must be broadened to include semantics.[146] The

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[145] Carnap, R., Philosophy and Logical Syntax, p. 50f.

[146] R. Carnap, "Logical Foundations of the Unity of Science" in International Encyclopedia of Unified Science, Vol. 1, pp. 42-62.

semantical rules of a language are simply what Reichenbach called "coordinative definitions" and what Bridgman called "operational definitions."

> "...we shall say that we understand a language system, or a sign, or an expression, or a sentence in a language system, if we know the semantical rules of the system. We shall also say that the semantical rules give an interpretation of the language system."[147]

Carnap stresses the point that there is a degree of freedom available in the formulation of semantic rules. That is to say, semantic rules are not unambiguously determined by a set of linguistic facts.

> "Suppose we have found that the word 'mond' of B was used in 98 per cent of the cases for the moon and in 2 per cent for a certain lantern. Now it is a matter of our decision whether we construct the rules in such a way that both the moon and the lantern are designata of 'mond' or only the moon. If we choose the first, the use of 'mond' in those 2 per cent of cases was right - with respect to our rules; if we choose the second, it was wrong. The facts do not determine whether the use of a certain expression is right or wrong but only how often it occurs and how often it leads to the effect intended, and the like. A question of right or wrong must always refer to a system of rules."[148]

> "We found earlier that the pragmatical description of a language gives some suggestions for the construction of a corresponding semantical system without, however, determining it. Therefore, there is a certain amount of freedom for the selection and formulation of the semantical rules. Again, if a semantical system S is given and a calculus C is to be constructed in accordance with S, we are bound in some respects and free in others."[149]

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[147] R. Carnap, "Foundations of Logic and Mathematics" in International Encyclopedia ... Vol. 1, p. 152f.

[148] Ibid., p. 148f.

[149] Ibid., p. 166.

According to Carnap, the construction of a scientific system may proceed in two ways. In the first way, we would begin with the semantical system S. That is to say, we would classify the kinds of signs which we want and the rules determining the forms of the sentences which we wish to employ. Then we would lay down the rules of semantical designation. This would involve selecting the objects and properties which we wish to speak about, and then choosing the signs we wish to employ to designate these objects and properties.[150]

The other method is to begin with the construction of a formal calculus C, and then to lay down the set of semantical rules S which interpret C. This method is the more important since, according to Carnap, it is the procedure which science does in fact follow. We shall, therefore, quote Carnap's account of it at length.

> "We begin again with a classification of signs and a system F of syntactical rules of formation, defining 'sentence in C' in a formal way. Then we set up the system C of syntactical rules of transformation, in other words, a formal definition of 'C-true' and 'C-implicate.' Since so far nothing has been determined concerning the single signs, we may choose these definitions, i.e. the rules of formation and of transformation in any way we wish. With respect to a calculus to be constructed there is only a question of expedience or fitness to purposes chosen, but not of correctness.
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> "Then we add to the uninterpreted calculus C an interpretation S. Its function is to determine truth conditions for the sentences of C and thereby to change them from formulas to propositions. ...

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[150] *Ibid.*, p. 167.

"Finally we establish the rules SD for the descriptive signs. We have to take into account the classification of signs. We choose the designata for each kind of signs and then for each sign of that kind. We may begin with individual names. First we choose a field of objects with which we wish to deal in the language to be constructed, e.g., the persons of a certain group, the towns of a certain country, the colours, geometrical structures, or whatever else. Then we determine for each individual name, as its designatum, one object of the class chosen. Then, for each predicate, we choose a possible property of these objects, etc. In this way, a designatum for every descriptive sign is chosen."[151]

Carnap's account of the structure of a scientific system is in perfect accord with the views of Poincaré. Poincaré, for example, held that we begin with a system of geometrical axioms. We are completely free to choose any set of axioms we desire. It is true that he held the Euclidean axioms to be preferable on pragmatic grounds but this is merely accidental, and in any case is covered by Carnap's reference to "experience or fitness." However, we cannot say of our set of axioms that it is true or false. The axioms do not describe the world. The question of truth only becomes relevant once we have defined the signs of our axiomatic system. For example, we may have the sign "straight-line" designate the path traversed by a light-ray. But here again, of course, our choice is free. In short, Poincaré held the view, which has since come to be regarded as the distinctive property of the logical positivists, that a scientific system is an abstract, logical calculus which

[151] Ibid., p. 167.

is interpreted by semantical rules. Furthermore, it should be noted that Carnap did not fail to see the conventionalistic implications of his account of the structure of a scientific system.

> "Are the rules on which logical deduction is based to be chosen at will and, hence, to be judged only with respect to convenience but not to correctness? Or is there a distinction between objectively right and objectively wrong systems so that in constructing a system of rules we are free only in relatively minor respects (as, e.g. the way of formulation) but bound in all essential respects? Obviously, the question discussed refers to the rules of an interpreted language; *nobody doubts that the rules of a pure calculus, without regard to any interpretation, can be chosen arbitrarily.* On the basis of our former discussions we are in the position to answer the question. We found the possibility - which we called the second method - of constructing a language system in such a way that first a calculus C is established and then an interpretation is given by adding a semantical system S. *Here we are free in choosing the rules of C. To be sure, the choice is not irrelevant; it depends upon C whether the interpretation can yield a rich language or only a poor one.*
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> "We may find that a calculus we have chosen yields a language which is too poor or which in some other respect seems unsuitable for the purpose we have in mind. *But there is no question of a calculus being right or wrong, true or false. A true interpretation is possible for any consistent calculus.*"[152]

Carnap concludes that the rules of a scientific system "can be chosen arbitrarily and hence are conventional."[153]

Now it may be objected that Poincaré made no explicit distinction between a formal calculus and an interpreted system, in which case the comparison with Carnap is specious. In all rigour, this objection is partly warranted. As we

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[152] *Ibid.*, p. 169. (The italics are mine)

[153] *Ibid.*, p. 170.

remarked in an earlier chapter, Poincaré failed to make explicit the distinction between mathematical relativity and physical relativity. We might now rephrase this criticism in a new context by saying that Poincaré failed to make explicit the distinction between our freedom in the choice of syntactical rules and our freedom in the choice of semantical rules. It is, in fact, due to his obscurity in this regard that his doctrine of conventionalism has been widely misinterpreted. However, this obscurity is really quite natural. For example, if we followed Carnap to the letter, our mode of formulating the axioms of pure geometry would be quite different from our mode of formulating the axioms of physical geometry. Take the following axiom: For any two points there is a straight line on which they lie. In a pure geometry the foregoing might be expressed as:

> For every x, y [If (x is a P_1 and y is a P_1) then, for some z(z is a P_2 and l(x,z) and l(y,z))]

Carnap, however, points out that a scientific system is actually a "nonlogical calculus"[154] which consists of two parts. There is the basic logical calculus and in addition a specific partial calculus which will vary from one science to another. Since the basic calculus is virtually the same for all systems, we tend not to mention it. "What usually is called an axiom system is thus the second part of a

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[154] Ibid., p. 179.

calculus whose character as a part is usually not noticed."[155]

Normally, a mathematical calculus is purely logical. In the case of geometry, however, it may be an interpreted calculus, which is intended to be descriptive. When we employ such terms as "point", "straight line", etc., we treat them as interpreted signs, i.e. as descriptive. Thus, the axioms of geometry become factual propositions about the world. Carnap points out that it happens to be the custom to employ the same symbols in mathematical as in physical or interpreted geometry.

> "The distinction between mathematical geometry, i.e., the calculus, and physical geometry is often overlooked because both are usually called geometry and both usually employ the same terminology. Instead of artificial symbols like 'P', etc., the words 'point', 'line', etc., are used in mathematical geometry as well, ... and hence there is no longer any difference in formulation between mathematical and physical geometry. . ."[156]

It is the custom of employing the one language for mathematical geometry and physical geometry which has led to the dispute concerning the status of geometry in relation to the world, especially following the development of non-Euclidean geometries.

> "Mathematicians regarded all these systems on a par, investigating any one indifferently. Physicists, on the other hand, could not accept this plurality of geometries; they asked: 'Which one is true? Has the space of nature the Euclidean or one of the non-Euclidean structures?' It became clear by an analysis of the

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[155] *Ibid.*, p. 180.

[156] *Ibid.*, p. 195.

discussions that the mathematician and the physicist were not aware of this in the beginning. Mathematicians have to do with the geometrical calculus, and with respect to a calculus there is no question of truth or falsity. Physicists, however, are concerned with a theory of space, i.e. of the system of possible configurations and movements of bodies, hence with the interpretation of a geometrical calculus."[157]

Although it is obvious that Poincaré did not make the distinction clear, he did reach the correct conclusions which are implied by such a distinction. Moreover, it was obviously implicit in his philosophy as he did distinguish between mathematical and representational space.

Finally, a brief reference should be made to the fact that, as we saw in the preceding chapter, Poincaré treated mechanics in precisely the same way as geometry. This again is perfectly consistent with the views of Carnap.

"The method described with respect to geometry can be applied likewise to any other part of physics: we can first construct a calculus and then lay down the interpretation intended in the form of semantical rules, yielding a physical theory as an interpreted system with factual content."[158]

We may conclude that on all major points Poincaré's interpretation of science is essentially similar to that of the logical positivists. In the first place, Poincaré is a thoroughgoing empiricist, in the tradition of Ernst Mach, as is plainly revealed by his treatment of the spatial continuum. Secondly, like the positivists, he regarded objective, scientific knowledge to be concerned with relations rather

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[157] Ibid., p. 196.

[158] Ibid., p. 199.

than with qualities. Finally, while not stating the matter as explicitly as he might have, he anticipated Carnap's interpretation of a scientific system as a formal calculus which is interpreted by semantical rules.

Some readers may find the close analogy between Poincaré's conventionalism and the rigorous empiricism of the logical positivists very difficult to accept. It may, for example, be said that by no means all of the logical positivists have been as generous as Philipp Frank in acknowledging their indebtedness to Poincaré. We have already seen that such positivists as Reichenbach and Schlick were openly critical of his position. In the aforementioned cases it has been shown that the differences are largely apparent and due to a misunderstanding, albeit a natural one, of Poincaré's actual doctrine.

However, let us pursue the matter a little further. Victor Kraft, an eminent logical positivist, has criticized the thesis of conventionalism on the ground that it lays open the possibility for the retention of any scientific theory in the face of conflicting facts through the introduction of appropriate auxiliary hypotheses.[159] As an example, he cites the Lorentz-Fitzgerald contraction as an auxiliary hypothesis introduced to make the Michelson-Morley experiment compatible with existing theory. In Kraft's opinion, we must choose between conventionalism and empiricism, the two being clearly

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[159] Victor Kraft, The Vienna Circle, p. 140.

incompatible.

> "But if we do not wish to give up empiricism in favour of conventionalism, we must allow this way of solving contradictions between a consequence of the hypothesis being tested and an accepted basis sentence only under definite conditions. We must not allow the introduction of arbitrary auxiliary hypotheses or modifications of our presuppositions which serve no other purpose than to remove these contradictions and are otherwise unfounded. Such remedial assumptions are arbitrary if they are not either capable of independent verification, in terms of new observations, or deducible from propositions already established."[160]

In the first place, it should be noted that Poincaré was attempting to describe the nature of scientific theory. It would be wrong to suppose that he regarded conventionalism as prescriptive. He distinguished certain conventional aspects of scientific theories and drew attention to them. At no time and in no way did he advocate the extension of these conventional aspects. He was not arguing for an unrestricted conventionalism. He made it abundantly clear that he did not recommend that scientists abandon their experiments in favour of ingenious linguistic exercises.

What Poincaré did say, however, and on this point Kraft would agree, is that there will always remain the logical possibility of retaining a theory through the appropriate modification of its parts. However, when he cites examples of such attempts, it is quite clear from the context that it is only to show how absurd they are. He argued that while experience could not falsify such theories

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[160] Idem.

it will clearly lead to their abandonment by showing them to be fruitless. Kraft, himself, agreed, as is obvious from the above quotation, that auxiliary hypotheses are admissible provided that they are not arbitrary. But, as the reader knows, this is precisely the point taken by Poincaré.

A similar criticism has been levelled at Poincaré by Karl Popper. While Popper is not a logical positivist, he is decidedly an empiricist, and has been closely associated with the Vienna Circle. He maintained that a scientific theory is characterized by its falsifiability.[161] Stated as simply as possible, Popper's position is to the effect that an experimental investigation is carried out as an attempt to refute a scientific theory. It is not the object of the scientist to find ways and means of preserving his theory but rather to assure himself that the theory is not vulnerable to attack on experimental grounds.

> "According to my proposal, what characterizes the empirical method is its manner of exposing to falsification, in every conceivable way, the system to be tested. Its aim is not to save the lives of untenable systems but, on the contrary, to select the one which is by comparison the fittest, by exposing them all to the fiercest struggle for survival."[162]

It would certainly appear that Popper's brand of empiricism is incompatible with the conventionalistic interpretation of scientific theories. For example, he

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[161]Karl Popper, The Logic of Scientific Discovery, p. 40ff.

[162]Op. cit., p. 42.

writes that, "the <u>empirical</u> <u>method</u> shall be characterized as a method that excludes precisely those ways of evading falsification which are logically admissible."[163]

It is beyond the scope of this thesis to make a direct refutation of Popper's elaborate treatment of the logic of science. Popper, however, admits that conventionalism is logically defensible but accuses it of certain "stratagems."[164]

> "In order to formulate methodological rules which prevent the adoption of conventionalist stratagems, we should have to acquaint ourselves with the various forms these stratagems may take, so as to meet each with the appropriate anti-conventionalist counter-move. Moreover we should decide that, whenever we find that a system has been rescued by a conventionalist stratagem, we shall test it afresh, and reject it, as circumstances may require."[165]

Popper goes so far as to admit that conventionalism cannot be rejected on theoretical grounds.

> "attempts to detect inconsistencies in it are not likely to succeed. Yet in spite of all this I find it quite unacceptable. Underlying it is an idea of science, of its aims and purposes, which is entirely different from mine."[166]

It would seem that Popper treats conventionalism with some injustice if he means by this that the conventionalist regards science as the art of linguistic and logical manipulation. Poincaré has clearly stated that the object

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[163] <u>Idem</u>.

[164] <u>Op</u>. <u>cit</u>., p. 80.

[165] <u>Op</u>. <u>cit</u>., p. 82.

[166] <u>Ibid</u>., p. 80.

of science is to discover objective relationships in the structure of the world. Surely, Popper does not conceive of any other role for science.

In all, Popper distinguishes four conventionalist stratagems to be guarded against.[167] In the first place, the conventionalist will introduce _ad hoc_ hypotheses to preserve a theory. Secondly, he may modify the "ostensive definitions" of the theory. Thirdly, he will be sceptical in his attitude to the reliability of an experimenter whose results threaten the theory. Fourthly, he will question the theoretical acumen of the scientist.

The third and fourth reasons may justly be ignored as trivial. It is noteworthy that Popper frequently refers to the views of an "imaginary conventionalist."[168] This turn of phrase is well advised, since we know of no conventionalist who has suggested either of these as grounds for the retention of a theory. Certainly, there is not the barest suggestion of any such idea to be found in the works of Poincaré. Therefore, we shall limit our considerations to "stratagems" one and two.

Popper would allow the introduction of _ad hoc_ or auxiliary hypotheses insofar as they do not diminish the degree of testability of the theory, in which case they are even to

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[167] _Ibid._, p. 81.

[168] _Idem_, Popper does cite the names of H. Poincaré, P. Duhem and A.E. Eddington but fails to examine their actual writings in even the barest detail.

be encouraged.[169] However, when an ad hoc hypothesis is imported for the specific purpose of retaining a theory in the face of conflicting facts, it has diminished the degree of testability of that theory and is to be rejected as a mere stratagem.

All this, of course, is simply a matter of stating the obvious. Let us imagine any actually existing theory. Let us now suppose that a novel fact has been observed, which is incompatible with that theory. The old theory is then enlarged for the specific purpose of taking that fact into account. The new theory will then possess a higher degree of testability than the old one because at least one more testable statement will be deducible as a consequence of it. Poincaré would argue that it would be logically possible to import a type of assumption which merely explains the fact away, for example the postulation of an unobservable force. Popper is correct in suggesting that this would diminish the testability of the theory and is therefore to be avoided. The crucial point, however, is that Poincaré would clearly have agreed with Popper in this regard. "If a principle ceases to be fecund, experiment without contradicting it directly will nevertheless have condemned it."[170]

Popper was probably misled into attacking Poincaré's thesis by placing undue stress on the latter's analysis of the

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[169] Cf. Ibid., p. 83.

[170] The Value of Science, p. 110.

foundations of geometry. Physical geometry is a special case of physics in that no novel facts are discoverable. The "given" of geometry is the amorphous spatial continuum. In demonstrating the conventional character of geometry, Poincaré stressed the point that the several systems of metrical geometry are formally equivalent. That is to say, in Popper's language, all such systems of geometry possess precisely the same degree of testability. Hence, experience cannot compell the adoption of one system of geometry rather than another. In the case of mechanics, however, new facts are discoverable which render one theory more acceptable than another. Popper seems to believe that Poincaré held the view that any system of mechanics, like any system of metrical geometry, is as good as any other, Popper rightly finds such a view of science to be unacceptable. But clearly this view is far from Poincaré's conception of the nature of mechanical description.

The foregoing considerations apply equally to the second stratagem of conventionalism, namely the modification of ostensive definitions. An ostensive definition in this context is what Reichenbach called the "coordinating definition" and what Carnap calls the "semantical rule." Popper appears to be of the opinion that there is something in the nature of an artful subterfuge involved in the modification of an ostensive definition in the light of fresh experimental evidence. This is attributable to the fact that Popper treats a scientific theory from the artificial standpoint of the professional logician of science. That is to say, for Popper, a scientific theory is a finished product. Popper's legitimate task as a logician is to determine the degree of empirical justification pertaining to that finished product. However, such an approach to science, while fruitful in itself, has certain shortcomings. In particular, it overlooks the fact that scientific theories develop gradually from the collective experience of generations of scientists. Popper appears to suggest that once a primitive sign has been semantically defined in a theory it is dishonourable to change the definition. If this is so, then the history of science is replete with dishonourable intentions. For example, the word "atom" played a specific role in nineteenth century physics. But in the present century Niels Bohr profoundly modified the meaning of "atom" to take account of fresh experimental evidence. Surely, Popper would not suggest that Bohr was guilty of cheating, of employing a stratagem to retain the atomic theory of matter. The point, once again, is that in some instances such changes are fruitful while in others they are not. As a conventionalist, Poincaré would merely argue to the effect that there is nothing to prevent a scientist from modifying a definition in either case. However, he would certainly agree that, in the second case, nothing fruitful will have been gained by it.

The foregoing considerations lead directly into my final point. Once more, the reader may object to the stress that has been placed on the similarities between logical positivism and conventionalism on the ground that the sharp distinction between an abstract calculus and an interpreted scientific system made by Carnap is not to be found in the philosophy of Poincaré.

The difference between the formal presentations of the two thinkers is to be attributed to a difference of perspective. In the case of Carnap, we find the perspective of the logician, concerned with the finished product, its formal structure and the grounds for its justification. Carnap, in short, was not concerned with the psychology of scientific discovery. He would certainly not suggest that the scientist actually begins by working out a logical calculus and then proceeds to interpret that calculus by the conscious formulation and introduction of semantical rules.

For Poincaré, on the other hand, scientific theorizing was a personal matter. He was aware of the gradual and intermingled growth of the syntactical and semantical aspects of a scientific theory. Consequently, he was lead to present the two as being of a piece, as an admixture, which they are in the mind of the working scientist. Something is to be said for each approach. They differ but are by no means incompatible. On the contrary, they complement each other.

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