Semiotics as Science: Theory Construction and Logic of Inquiry in the Semiotics of Umberto Eco

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Abstract

In light of the recent claims that semiotics has failed because of the inapplicability of scientific methods to the "human sciences," the semiotic theory of Umberto Eco is examined from the perspective of the "sophisticated methodological falsificationist" philosophy of science of Lakatos. Eco's *A Theory of Semiotics* and *Semiotics and the Philosophy of Language* are found to suffer from a faulty criterion for scientificity, misuse of symbolisms and calculi, and overall lack of clarity. Eco's central theoretical construct, the Model Q of "infinite semiosis," is found to be erroneously derived from Eco's sources, to entail several absurdities, and to be lacking in empirical content. Without detracting from Eco's essayistic and pragmatic merits, it is concluded that the failure of scientific semiotics is best ascribed to these faults, and that this failure should not be taken to discredit the scientific method in the human sciences.

Résumé

A la lumière des récentes assertions que la sémiotique a échoué en raison de l'inapplicabilité de la méthode scientifique aux "sciences humaines," *A Theory of Semiotics* et *Semiotics and the Philosophy of Language* d'Umberto Eco sont examinés selon la perspective de la philosophie de la science de Lakatos. Il est démontré que ces oeuvres souffrent d'un critère erroné de scientificité d'un abus de formalisme, et d'un manque général de clarté. L'élément central de la théorie d'Eco, le Modèle Q, se révèle être basé sur une interprétation fautive de ses sources, entraîner des absurdités, et être déficient en contenu empirique. Sans rien enlever aux autres mérites d'Eco, il est conclu que l'échec de la sémiotique scientifique est attribuable à ces défaillances, et qu'il ne devrait être perçu comme un discrédit à la méthode scientifique dans les sciences humaines.

Introduction

The scientific strain in semiotics,[1] after its heyday in the 1960s and 70s, is now often held to be in a decline, to have failed to fulfil its promise, to have been founded on a fallacy, or even to have been revealed as the last gasp of an outdated, naive, and ideological rationalism (cf., for example, Finlay 7 and 264, Polan 87, Derrida *passim*, and Angenot 9). While scientific semiotics is by no means extinct, newer approaches to the range of problems formerly widely acknowledged as the domain of semiotics abound, each claiming to have overcome the crippling deficiencies of the semiotic method.

Semiotics is taken by the majority of the practitioners of the various strains of deconstruction and of the many post-modern neo-Freudianisms to have failed because of its attempt to adapt the supposedly reifying, ahistorical, rationalistic methods of the natural sciences to a domain to which they were unsuited. In short, semiotics is held to have failed because it attempted to be the science of a domain which would admit of no such thing.

I shall claim in what follows that this view of the matter is mistaken. My argument will be that the quite real methodological failings of scientific semiotics are not those just cited; that, on the contrary, the shortcomings of scientific semiotics lie precisely in its nonconformity to the scientific ideal. More provocatively, my

claim will be that the failure of scientific semiotics ought to be put down to its having failed to be a science, rather than to its having tried to be one.

Although my intention is to make a point about an entire theoretical movement, I shall discuss the work of only one writer: Umberto Eco. Moreover, I shall not attempt an exhaustive coverage of Eco's writings, but shall restrict my attention to two books and a few articles. The rationale for this concentration is that Eco's position as a (if not *the*) preeminent theorist of the scientific semiotic movement, his reputation for rigour and exactitude (even for excessive exactitude, in the minds of some (e.g. Polan 87)), the generally acknowledged position of his *A Theory of Semiotics* as the single central work and the major modern synthesis of semiotic thought, the wide diffusion of his ideas to such varied disciplines as legal studies, economics, philosophy, sociology, and political science,[2] and the high frequency of citation of his works in the semiotic journals,[3] combine to make Eco the obvious choice of subject for this kind of inquiry.

Eco's range of theoretical interests is, however, extremely broad, and for the purposes of this thesis some narrowing was required. Rather than taking a chronological section of Eco's work, it seemed a sounder strategy to choose a major area of his theoretical concern. Eco's own segmentation of *A Theory of Semiotics* indicates that the major division in his theoretical output is between the

"theory of signification" (also "theory of codes"), and the "theory of communication" (also "theory of sign production"). It will be the former which receives the greatest part of my attention in what follows, for three reasons. First, it is in the theory of codes that Eco claims the greatest scientific rigour: "[t]he idea of the interpretant makes a theory of signification a rigorous science of cultural phenomena" (*TS* 70). Further, the "specific" semiotics for which Eco again explicitly claims the status of a science is the semiotics of signification of a specific code: "a specific semiotics is the 'grammar' of a particular sign system" (*SPL* 5). Second, it is here that Eco deploys the greatest concentration of the accoutrements of science: technical terminology, special notations, diagrams, and so on. Finally, it is in the theory of codes that Eco's work is most strongly tied to a number of related scientific disciplines, such as linguistics, Artificial Intelligence research, and cognitive psychology. The first of these points permits me to take Eco at what he would consider his scientific best; the second and third help to make my task somewhat easier, in that the apparatus of the philosophy of science can more easily be brought to bear on a subject with close methodological and substantive ties to better-established sciences.

Accordingly, it is to those of Eco's works that are most preoccupied with this aspect of semiotic theory (theory of codes), as well as to those works that explicitly

address questions of method that I shall attend in succeeding chapters These are first and foremost, *A Theory of Semiotics* (afterwards *TS*), *Semiotics and the Philosophy of Language* (afterwards *SPL*); the earlier "On Fish and Buttons. Semiotics and the Philosophy of Language" (afterwards "FB"); "The Sign Revisited" ("SR"); and finally "Semiotics: A Discipline or a Method?" ("SDIM") The most notable omission here is the collection of essays *The Role of the Reader*, examination of which will be omitted due to the fact that it is almost exclusively concerned with the theory of communication

It should be stressed that what follows is *not* an attempt to bring Eco's work into contact with the philosophy of language[4] (which Eco has himself attempted in *SPL*), or to criticize Eco on the basis of arguments from the philosophy of language. Neither is it an attempt to assess the internal coherence or the general fruitfulness of the whole of Eco's theoretical output It is rather an attempt to assess Eco's methodology from the perspective of a modern, realist, fallibilist philosophy of science. A large part of the interest of this project derives from the fact that little attention has thus far been paid to the validity of Eco's claim to scientificity. Apart from some brief remarks on Eco by F. W Galan, W. O. Hendricks, and Giulio Lepschy, a short general commentary by Paul Garvin on the relation of semiotics to the science of linguistics, a few remarks on Saussurean methodology by Naomi S. Baron, and a

short chapter on the logical structure of semiotic theory (in a longer work on concept formation in the social sciences) by Tadeusz Pawlowski,[5] I am aware of no treatment of scientific semiotics in these terms.

Two questions arise about such an enterprise. First, one may ask whether it is fair to demand of a semiotic theory that it adhere to the methodological strictures of the philosophy of science. However, Eco leaves no doubt that he considers himself (at least in part) to be elaborating the project of a *science* of semiotics. He writes that "a specific semiotics can aspire to a scientific status" (*SPL* 5), and that semiotics is "simply the science of [the layman's] culturally performed (if unexpressed) competence" (*TS* 72).[6] Obversely, as I have noted above, the standard "post-modern" criticisms of semiotics claim that scientific semiotics failed *because it was a science*. To this several rejoinders are possible: one might claim, against the evidence, that scientific semiotics has not failed; one might construct a successful scientific semiotic theory, thereby demonstrating that the failure of previous theories was due to other causes, an enterprise obviously beyond the scope of the present work; finally, one might show that this supposedly scientific semiotics was no science at all, as I propose to do here. If successful, the present attempt could therefore vindicate at least the possibility of a scientific semiotics.

Second, one may ask about the appropriateness of a particular philosophy of science, in this case a "sophisticated" fallibilist realism (Lakatos, *Methodology* 31ff), for this task This view is the latest philosophical response to the failure of justificationism (also called foundationalism), the demand for a final "ground" for knowledge Justificationism took many forms, including both classical intellectualism and classical empiricism, as well as scepticism, which accepted the justificationist criterion of knowledge, but held that it could not be met.[7] Earlier attempts to salvage the categories of knowledge and science from the failure of the justificationist program included probabilism (the belief that no theory can be known with certainty to be correct, but that various theories nevertheless have different degrees of probability of truth), which was refuted by Popper (who demonstrated that all scientific theories have zero probability, no matter what the evidence), and various forms of falsificationism.

Falsificationists hold that, although theories cannot be known to be true, it is nevertheless rational to hold a theory provided that it has not been falsified. The several varieties of falsificationism differ on what constitutes the criterion for rejecting a theory.

"Naturalistic" or "dogmatic" falsificationism held that pre-theoretical facts are available in experience, and that these may be called upon to disprove theories quite definitely. Early Popperian "naive methodological falsific-

ationism" abandoned justificationism entirely by recognizing that disproof is no more available than proof: it admitted that "conventionalist stratagems" (Popper, *Logic*, sections 19 and 20) are always available to save a theory in the face of any evidence whatsoever. Popper acknowledged "the need for decisions to demarcate the theory under test from unproblematic background knowledge" (Lakatos, *Methodology* 23). The results of attempted falsifications are therefore not wholly reliable: a "falsified" theory may yet be true. Popper argued that, if the beliefs which temporarily constitute the "background" (that is, those beliefs which a decision of the kind just mentioned has temporarily separated from the theory under test) are sufficiently well confirmed, a theory which has failed appropriately severe tests should be definitively and permanently rejected, since no better criterion for the acceptability of a theory can be had.

The latter Popper's and Lakatos' "sophisticated methodological falsificationism" (Lakatos, *Methodology* 31), a version of which I adopt here, attempts to reduce the risks of the "dare-devil" and "arbitrary" falsification rules of naive methodological falsificationism (*Methodology* 28, 30) by reducing its conventionalist element. This is done by providing stricter conditions which must be met before a falsification is accepted:

> For the sophisticated falsificationist a theory T is *falsified* if and only if another theory T' has

> been proposed with the following characteristics. (1) T' has excess empirical content over T that is, it predicts *novel* facts, that is, facts improbable in the light of, or even forbidden, by T. (2) T' explains the previous success of T, that is all the unrefuted content of T is included (within the limits of observational error) in the content of T'; and (3) some of the excess content of T' is corroborated... Sophisticated falsificationism thus shifts the problem of how to appraise *theories* to how to appraise *series of theories*. .. (32)

And further:

> Of course, there is nothing wrong in saying that an isolated, single theory is "scientific" .. as long as one recognizes that in this formulation we appraise the theory as the outcome of--and in the context of--a certain historical development (34-35)

Lakatos develops this into a theory of the rationality of science based on the notion of the "research programme" rather than the isolated theory. He does this in a conscious (and successful) effort to counter the psychologistic objections of Kuhn to Popperian falsificationism (cf *Methodology* 90-93).

I can offer only a rather local rationale for adopting this view in the present work, since the variants of fallibilism have been seriously debated in the philosophy of

science. On the other hand, while the case for fallibilism against th various forms of foundationalism or justificationism cannot be rehearsed here in any more detail, the arguments are very well known. Philosophers of science are no longer divided on this issue, and to some extent the smoke has cleared: Lakatos was able in 1970 to write a reconstruction of "the situation as it was in philosophy of science after the breakdown of justificationism" (*Methodology* 10) A debate continues, however, about "naive" (early Popperian) *versus* Lakatos' "sophisticated" versions of falsificationism, as does another about whether the sorts of assurances that can be provided by fallibilism are enough to warrant our continued use of notions like knowledge and science. I cannot argue this point here, and shall simply assume that they are.

As for scientific realism, this has, since Tarski's vindication of the correspondence theory of truth, been an integral component of the fallibilist views of both Popper and Lakatos rather than a separate position, and I list it separately only for clarity, since nonrealist positions are common in the human and social sciences. Realism remains, in its several modern variants, even though beleaguered by postmodernisms and relativisms of all sorts, the majority opinion in the philosophy of science.[8] A critical realism appears, moreover, to be Eco's own view. Although there are passages in Eco's work, especially in the Introduction to *SPL*, where he seems to espouse a relativist epistemo-

logy. his theoretical practice remains realist: he is able to write without self-consciousness of "facts" (*TS* 222) and "the actual state of the objective world" ("SDIM" 76), and to state blandly that a certain state of material affairs "is the case" (*SPL* 70). In discussing the "fallacy of reference," he writes that "one may easily admit that the signs transmitted through the Watergate Model have a corresponding 'object,' that is, the state of the water at the source" (*TS* 58). Again, in introducing Peirce's concept of the interpretant, he writes that "If one assumes that the *Bedeutung* is an actual state of the world, whose verification validates the sign, one must ask oneself how this state of the world is usually grasped or analyzed" (*TS* 61) These examples should suffice to show that Eco is a realist.

In the places just cited and elsewhere (e.g. *SPL* chapters 1 and 2 passim), Eco attacks the naive realist view that the world is wholly knowable Clearly, therefore, he holds that there are actual states of the world, some but not all of which may be known; this is critical realism in a nutshell.

My methodological perspective specifies a few more contours of my project, including the following: I shall devote special attention to Eco's contacts with semiotics' neighbouring sciences, especially linguistics and the work in Artificial Intelligence, as a means of exploring Eco's relations with the scientific mainstream. I shall be con-

cerned with substantive issues in Eco's theory only insofar as they prove relevant to methodological questions, or to the relations of his theory with its theoretical neighbours, or when Eco's handling of them can be criticized in purely logical terms. Finally, I shall not be concerned with criticism of Eco from perspectives other than that of the philosophy of science: in particular, I shall take notice of deconstructionist, "dialectical," relativist, or psychoanalytically oriented attacks on Eco's theory or method only insofar as they prove useful as diagnostic indicators of faults of the sort I am interested in.

It should be obvious that I am assuming answers to a number of questions which are to varying degrees contested. Although these issues are not explicitly discussed in this thesis, their influence can be felt throughout what follows. I shall try, therefore, quickly to point out a few of the more important ones. I cannot defend these views here, and, unlike Eco, I am not willing blandly to assert that my philosophical tradition is "highly reliable" (*TS* 7), thus resting the credibility of my philosophical views on an appeal to authority.[9] Each of the following positions is defensible on its merits, and each has been extensively defended.

Against the deconstructionists and certain relativists and dialecticians, I am assuming a correspondence theory of truth, and the adequacy of the cognitive safeguards built into the practice of science to protect it from reduction

to interests, desires, or economics.[10] I am assuming the reality of what is often misleadingly called scientific "objectivity," as against those writers of the "sociological turn" in the philosophy of science who claim that the notion of science is exhausted by some combination of extrinsic sociological and psychological factors.[11] Against a common psychoanalytic view, I am assuming the reducibility of mental states to neurological, and ultimately to physical, ones. Against some writers in the Continental dialectical tradition, I am assuming the supersession of "dialectical logic" by the ordinary kind

In Chapter One I review the critical literature on *TS* and *SPL*, paying special attention to matters of method In the next two short chapters I examine some of the general contours of Eco's work. Chapter Two examines Eco's view of the defining characteristics of a science and compares this to standard realist accounts, with special attention given to the roles of definitions, "posits," and theoretical terms. In Chapter Three, I examine Eco's use of formal devices (including symbolisms, diagrams, and graphs of all kinds). In the longer fourth chapter, I study in detail the complex of theoretical concepts surrounding the centrepiece of Eco's semiotics, his Model Q of semantic theory, including "interpretant" and "code," as well as the subsidiary concepts "cultural unit," "cultural world," "content-unit," and "expression-unit." After exploring the usefulness and coherence of Model Q, I raise some questions

about its status as an empirical theory. Finally, in the Discussion at the end of the thesis, I review the work presented in the earlier chapters and draw from it some conclusions regarding the scientific status of Eco's work, the prospect of a genuinely scientific semiotics, and the usefulness of the philosophy of science to young disciplines like semiotics

1 Critical Literature on Eco

1.1 Goals of this chapter

In this chapter I shall try to accomplish two things. My first goal will be to give evidence for a number of claims made in the Introduction, the acceptance of which is requisite to my taking a small number of titles by Umberto Eco as sole subject of what is meant as an argument about the scientific strain in semiotics as a whole. This will be done mainly by a survey of the internal critical literature on *A Theory of Semiotics* and *Semiotics and the Philosophy of Language*. By "internal criticism" I mean criticism originating from within the discipline of semiotics (though not confined to writers of the school I more narrowly defined as "scientific semiotics" above) as well as from within several disciplines related and sometimes sympathetic to the project of scientific semiotics. These include anthropology, sociology, communications, linguistics, speech communication, philosophy, aesthetics and art criticism, and consumer research.

The articles surveyed, it can be said with some confidence, include nearly all of the reviews of these books published between January 1976 and January 1988 in English or French.[12] As the number of these articles is somewhat large (24 reviews in all), I shall select representative examples for detailed presentation, and merely indicate the affinities of most others.

Since my goal here is to investigate the self-conception of scientific semiotics, as well as Eco's rank within it, in reviewing this body of work I shall attempt to isolate the points of critical near-consensus. Consensus, I shall try to show, obtains with respect to at least the following assertions: 1. that semiotics is a science, and has close and vital connections with other sciences, 2. that Eco is an exemplary and central theoretical worker of the school of scientific semiotics; 3. that the two works that will occupy most of my attention in the following chapters, *TS* and *SPL*, are basic texts of the school, and are important and successful syntheses of the work of Eco's most illustrious forebears, and that *TS* and *SPL* in particular do in fact constitute scientific research. These points, taken together, constitute the minimal basis needed to show that in the following chapters I shall not be tilting at windmills--that I have not imposed terms of debate altogether foreign to my subject, and that the positions I shall argue against are in fact held by many workers in the field.

Since what is wanted here is to establish the appropriateness of taking Eco as my subject in relation to my larger goals in the thesis, rather than a comparative assessment of Eco's individual works, I shall deal with criticisms of *TS* together with those of *SPL*.

In this section I shall attempt to refrain from comment on the views quoted, although, as will become clear later,

I regard most of these as ill-founded and misleading. For reasons mentioned earlier, substantive issues of semiotic theory (as opposed to methodological issues *about* semiotic theory) raised in the reviews will not often be discussed. The purpose of the first half of the present chapter is largely historical and pragmatic: I am concerned to establish some features of the subdiscipline I have been calling scientific semiotics. As discussed in the Introduction, my purpose is to review scientific semiotics from the external perspective of the philosophy of science, and internal theoretical disputes will be of interest only insofar as they illustrate features of theory or theorizing in scientific semiotics which are relevant from this perspective.

My second goal in this chapter will be to extract from this corpus of criticism on Eco some indications of possibly fruitful directions for a fuller methodological critique from the perspective of the philosophy of science, which I shall develop in later chapters. Since the number of works incorporating such a perspective is *extremely* small, I shall discuss each in some detail.

1.2 Survey of the Criticism of *TS* and *SPL*

1.2.1 The majority view

This group of critical writings on *TS* and *SPL* includes 15 of the 24, or 63%, of the works surveyed. As will be seen, their consensus is unmistakable.

Arthur Asa Berger's short review of *TS* manages in its 1000 words to give the flavour of the consensus in scientific semiotics about Eco and his work better than most others. Berger begins by informing the reader that Eco "has an international reputation, and has written brilliantly over the past decade on almost every subject conceivable," and then goes on to enumerate Eco's academic and professional accolades. *A Theory of Semiotics*, he asserts, "will be of consuming interest to a growing number of scholars interested in linguistics, structuralism, semiology, and related enterprises," and is a work of "awesome complexity and astonishing erudition," "highly technical and philosophically sophisticated" (217). Later Berger stresses the scientific character of the work, a quality which he apparently holds to follow from the formal precision of Eco's work. Berger argues that Eco carries forward the project for a science advanced by de Saussure, and concludes that *TS* is "the most significant text on the subject published in the English language that I know of" (218).

Hope Hamilton-Faria's review of *TS* in the *Modern Language Journal* carries on in much the same vein. *TS* is "an all-encompassing tour de force." Furthermore, the success of scientific semiotics gives hope to the literary scholar, who, "after having been seduced by the facile promises of formalism, structuralism, the New Criticism, and hermeneutics ... may raise his head once more and dare to hope that

methods and paradigms are at last available through 'semiotics'" (298).

Richard L. Lanigan, in evaluating *TS*, emphasizes its scientific precision and its relevance for scholars in the field of speech communication. Eco "makes the theoretical connection between communication theory and rhetorical theory . . with a systematic set of axioms grounded in empirical evidence" (345). Eco's chapter on code theory (a chapter about which I shall have much to say later) has, Lanigan writes, the character of a "painstaking proof" (345)

Robert E Innis' "Feature Book Review" of *TS* in the *International Philosophical Quarterly* praises *TS* as a major step in the "gigantic effort to bring a full-fledged science of signs into existence" (221), saying that this "marvellous" book, which is an attempt to "construct a comprehensive framework . . [for] semiotics," is "a goldmine of information, hints, clues, heuristic pointers, theses, arguments, and sophisticated questions bearing upon the semiotic project" (222). Innis especially appreciates Eco's work on the theory of codes: "Eco's discussion of the notion of a semantic system is brilliant and illuminating. . . Eco is extremely good on these issues" (227), as well as Eco's "attempts to generalize the best of contemporary linguistics into a general model of codes" (227-28).

Ronald L. Bogue's review of *SPL* in *Philosophy and Literature* praises this book as a worthy sequel to the "semi-

nal" *TS* (245), writing that it embodies "a compelling demonstration of the usefulness of semiotics as a framework for linguistic analysis and for understanding the history of the philosophy of language" (245). Bogue also stresses the synthetic character of Eco's work:

> The international and interdisciplinary scope of Eco's work, which coordinates diverse views within a theory that seeks formalization without totalization, is impressive and invigorating. Whatever the fate of his grand synthesis, we should be thankful that within his work the disparate and scattered voices of contemporary theory are brought together and engaged in a single discussion. (246)

Gilles Thérien's "*Semiotics and the Philosophy of Language* de Umberto Eco. Un sommet ou un temps d'arrêt?" points out what he calls "le caractère achevé, et d'une certaine façon terminale" of the book (125), and considers whether the thoroughgoing character of Eco's achievement in *SPL* might not preclude the possibility of Eco doing further work along the same lines, presumably because Eco's works exhaust the truths obtainable in the discipline. Later, he speculates whether the cause of this might not be that.

> [l]a sémiotique solidement ancrée dans le vingtième siècle avec ses considérations sur la biologie, la neurologie, l'intelligence artificielle, les sciences cognitives, l'analyse computationnelle, la proxématique, la kinésique et la zoosémiotique

n'est peut-être qu'une aberration optique de la philosophia perennis" (130)

W. C. Watt's review of *TS* in the *American Anthropologist* takes Eco and Thomas Sebeok as examples of the new "phase 2" of semiotics, which deserves to be taken seriously by anthropologists and which should soon "begin to produce a solid body of empirical work" (716)

Frank E. X. Dance, writing in *The Quarterly Journal of Speech*, lauds Eco for attempting "the cognitively Herculean task of outlining a broad theory which is capable of being partitioned and recast into testable hypotheses" (435)

Robert Scholes' short review of *TS* in the *Journal of Aesthetics and Art Criticism* acclaims Eco's as "the greatest contribution to the field" since Peirce and Morris. He praises Eco's "logic and lucidity," describing him as "superbly acute and sensible" (476). "Semiotics in his hands," he continues, "comes closer to being a reasonable and cumulative field of study than it ever has." Eco's findings, he writes, "rest upon a formidable edifice of observation, reason, and learning" (477)

Thomas E. Lewis' review of *SPL* is the setting for a lengthy and involved argument about the relationship between the theory of codes and the theory of sign production. Praising the book as contributing immediately to clarifying and developing some of the most fundamental areas of semiotic inquiry" (503), Lewis argues that *SPL* emends several aporiae in *TS* by recognizing that a sem

iotics of signification should be made to govern a semiotics of communication" (503)

Sollace Mitchell notes the confusion and dissent in the "theoretical base" (385) of semiotics, and hails *TS* as a step towards unity Virginia H Fry, John Lyne, William Ray, P Swiggers, and Thomas W Benson all write expository reviews of a generally sympathetic tone. Benson cites the "order, lucidity, and tact" of Eco's prose (214).

1 2 2 Dissenting opinions

The dissenting from the majority view are more heterogeneous than their antagonists. This group includes proponents of mutually exclusive philosophical outlooks, and it may be bisected into two subgroups. The first can be loosely labelled "postmodern," a term which I use in an inclusive sense to mean contemporary relativists, irrationalists, and deconstructionists of all sorts. I include here a few favourable reviews which praise Eco's work in terms to which, one might presume, a partisan of a scientific semiotics would object. I shall also mention under this heading a few remarks of a postmodern kind made by authors of the majority camp

The second group I have labelled "methodological perspectives from the philosophy of science." These writers consider Eco's claim to scientific status to be (to some extent at least) dubious, and they share my general philosophical position, they make only moderate and reasonable

demands of science, and find that it sometimes meets them; from this perspective, they point out failures of logic and design in Eco's work. These essays will form the jumping-off point for my later chapters

1.2.2.1 Postmodern Criticisms

Michael McCanles' "Conventions of the Natural and the Naturalness of Conventions" represents a relatively straightforward Derridean or deconstructionist critique of Eco's *TS*. Although I do not trust my own attempts to paraphrase argumentations of this type, McCanles seems to be claiming that Eco's notions of "natural" and "arbitrary" turn in a vicious circle of some kind, each requiring the "closure" (a term McCanles does not define) of the other. McCanles argues, in a manner which I can only reproduce verbatim, that this produces a contradiction in Eco's concept of unlimited semiosis:

> It is precisely the difference between signifier and signified that Eco seeks to break down, while at the same time he treats the signifier/signified differential as "natural." Unlimited semiosis is thus an unresolved paradox, a self-contradictory anomaly . . [T]he infinite circulation of signs necessarily requires that each relay become a point of naturalization, a closure of the meaning to which the previous relay points, otherwise the chain could not itself exist. (60-1)

This is not a fault of Eco's theory but a feature of the world, and one which has apocalyptic consequences:

> The aspiration to closure always finds its (illusory) fulfilment in a text, a closure that is not a closure, because meaning is always found, as Derrida says, elsewhere. *Because discourse continually aspires to closure it continually pursues unlimited semiosis....* So it would seem that whether we affirm or deny the process of unlimited semiosis we have in either case destroyed any understanding of how we constitute meaning through the creation of codes and the production of signs from these codes.... Ultimately *A Theory of Semiotics* speaks not in the mode of knowledge but of desire, a thwarted demand rather, that human sign systems reflect a determinate and natural order of things. (61-62, emphasis in original)

McCanles goes on to describe *TS* as an attempt at a "unified field theory of structuralism, post-structuralism, and semiology" (54);[13] later he draws an analogy between Eco's concept of unlimited semiosis (about which much more later) and "electrons in an electrical circuit" which "move forward through and by means of [sic] ... ohms of resistance" (60) Still later, he criticizes Eco's work as a remnant of a "pre-Einsteinian" world view (63).

This concern with physics and mathematics is not accidental, or unique to McCanles: Robert E. Innis writes that

Eco presents us with "the semiotic analogue of Godel's Theorem" (228). Teresa De Lauretis writes that the "semiotic approach is governed by a sort of indeterminacy principle" (380). David Glen Mick discusses at length "the semiotic challenge" to "the Received View" (207) in the philosophy of science, a view which Mick believes to embody a set of fundamental epistemological misconceptions about "forms of scientific research [other than semiotics], like experimentation or survey research," amounting to a "methodological dogmatism that seeks to govern what is knowable--hence, what can or cannot be true [sic]" (207).[14] Sebastian Shaumyan, in the chapter on the methodology of semiotics and the philosophy of science of his *A Semiotic Theory of Language*, states that "modern linguistics faces a semiotic problem that arises from situations defined by Bohr's Complementarity Principle. Originally this principle was formulated for quantum mechanics, but soon it became clear that it can be applied to other fields of human knowledge as well." (312)[15]

Innis approvingly argues that Eco's semantic theory entails a form of "framework relativism" (cf. Livingston, *Literary Knowledge* 22ff and 56). "Meaning is only determinate within a framework, but there is no ultimate framework to ground the semantic units.... Truth becomes truth within a framework." (Innis 288)

A related claim is made by Teresa De Lauretis in a lengthy review article on Eco's *TS* which is amongst the

most favourable. After summarizing Eco's general argument in some detail, De Lauretis then goes on to discuss "other aspects of the book that I consider just as important for their methodological, ideological, and metatheoretical implications which extend beyond the immediate interests of semiotics to all theoretical research and, in particular, to the fields of esthetics and criticism" (369) These are again primarily epistemological, and, after involved argument, De Lauretis concludes that "[t]he historical model takes precedence over the Kantian model, and semiotics establishes its claim to be a scientific discipline at the very moment in which it voluntarily rejects all aspirations to an absolute form of knowledge" (382).[16]

All these writers are concerned to show that Eco's work has in common with the most up-to-date work in modern science a social constructedness, a relativity to "forms of life" or "frameworks" (Innis, Bloor). Furthermore, this is held to have been demonstrated by work in the physical sciences as well as by work in semiotics: science is supposed, in standard deconstructionist style, to have brought about the demise of scientism.

Dana Polan's review of *SPL* stresses the reconciliation with deconstruction which Polan discovers in this book, in comparison with the more scientistic *TS*: Eco's latter work is "open to the fluxes and drives of subjectivity and history," "far from the clichés of [the] field as a ration-

alist, ahistorical, codifying system of reified analysis" (87).

Jane A. Nicholson, reviewing the same book, stresses what she sees as Eco's attempt to "combat those who would make a formalism [sic] or a science of semiotics" (105). This, I would claim, is the postmodern position (as we have seen it above) in embryonic form, before it dares to denounce the enemy. Scientism for Nicholson is delusive, but the distinction between science and something else still stands.

John A. Walker's review of *TS* is similar to Nicholson's in that Walker too retains (*pace* the deconstructionists) the traditional view that science and humanistic scholarship are fundamentally different sorts of activities. Walker, however, sees Eco as being on the side of science. He criticizes what he sees as Eco's ahistorical mode of theorizing (cf. Polan's opposite view above) and demands a theory "*for* art rather than *of* art, developed by concrete historical subjects," and argues that "theoretical views separated from practice, [and] criticism separated from production are alienated forms of activity" (319).

1.2.2.2 Methodological Perspectives from the Philosophy of Science

Three reviews of Eco's books attempt an assessment of them in the terms of the philosophy of science.[17] I shall sketch the arguments of each, and try to indicate the directions

in which I shall develop these criticisms in the following chapters.

F. W. Galan, after noting that Eco's *TS* is "the first comprehensive account of the problematics of signs and signification in English" (355), attacks it on several scores. Eco, claims Galan, "exhibits less skill as a theorist of semiotics than he does as its propagandist." Galan argues that Eco is unclear on whether semiotics is to be a nomothetic or an idiographic discipline, due to an "inability to work out a satisfactory definition of what constitutes the realm of the semiotic" (356). Eco's attempts at a definition of the semiotic are contradictory: his definition of the domain of semiotics as "everything than can be used in order to lie" (in *TS* 7) fails when applied to zoosemiotic phenomena, and Eco's alternative formulation that the semiotic is "everything subject to comic or tragic distortion" (in *TS* 64) fails when applied to "red spots on a patient's face which are the sign of measles" (Galan 355-57).

Eco's theory of codes is "a misguided project" (357): "although the book's organization largely corresponds to the competence/performance dichotomy, Eco sidesteps the concomitant dichotomy of deep and surface structures" (357). After criticizing one of many semantic theories (that of Katz and Fodor), Eco plumps for the Model Q of "infinite semantic recursivity," which "has the force and attraction of poetic vision, but can hardly be expected to

be of any explanatory value" (357). Cf. Chapter Four below for a discussion of the scientific credentials of Model Q.

Galan notes that Eco's key term "cultural unit" is poorly and contradictorily defined (358), and suggests that the "cardinal flaw" of Eco's book is that it is an attempt to build a metatheory prior to the existence of any satisfactory theory (358), making *TS* "a fruitless exercise in scholastic taxonomy, grouping various and related fields of endeavour under arbitrary headings" (358). This set of problems, concerning the relations of theory, metatheory, method, and philosophy, form the focus of my Chapter Two.

Finally, Galan claims that "Eco falls prey to what may be called 'diagrammania,' a belief that if something can be conveyed in a diagram it automatically acquires scientific validity" (358). This is the topic of my third chapter, which deals with the epistemological role of formalisms, including diagrams and symbolic notations.

Giulio Lepschy, writing in *Language*, also suggests that Eco's attempt at an overarching theory is premature: "more work on individual facets and aspects of semiotics is necessary before a satisfactory systematic treatise can be produced" (712). As noted à propos of Galan, I shall treat this matter in Chapter Three. Lepschy also points out the defects of Eco's special notation using slashes, guillemets, and double slashes to represent respectively expressions, contents, and "objects, images or behaviour intended as signs" (*TS* xi). Lepschy argues that this notation does

less well what the convention of putting a word mentioned, rather than used, into inverted commas accomplishes with greater economy of means. "Surely it is perverse," Lepschy writes, "to introduce a special notation for the word 'automobile' depending not on whether it mentions itself, but on whether the object it mentions is used as a means of locomotion or as a status symbol" (712).

William Hendricks, a linguist, shows that Eco has only partially understood the semantic theory of Katz and Fodor, omitting all discussion of the projection rules which control the amalgamation of the senses of polysemous lexical items, and which therefore determine "the form and content of dictionary entries in the KF theory" (293). I shall extend and generalize this claim in my fourth chapter, showing that Eco's misunderstanding of Quillian's work is of a kind similar to this, and that both misunderstandings point to a set of deeper conceptual errors.

Eco also underplays, Hendricks claims, the importance of double articulation, and disregards the empirical question of whether natural human languages are sign systems of a type different from that of other signifying systems (294). This question will be treated in Chapter Two, as an issue about "posits," or the *a priori* commitments of scientific theory.

2 Eco's Concept of Science

2.1 Eco's Sceptical Justificationism

In this short chapter I shall examine Eco's view of the characteristics of a science, taking it as established that Eco understands himself as elaborating a *science* of semiotics. I shall try to show that Eco's concept of science rests largely upon a somewhat erratic justificationism, and that it may therefore safely be replaced by a more adequate view when I later come to assess the scientific credentials of Eco's work.

Lakatos has discussed the intimate interrelation of justificationism and scepticism (*Methodology* 10, 166): scepticism is justificationism discouraged by the failures of its efforts to find ultimate grounds for certainty. It is along this axis that Eco's implicit philosophy of science wavers. He discusses the "methodological fault" of thinking that even a highly successful theory has "grasped the format of the world (or of the human mind, or of social mechanisms) as an ontological *datum*" (*TS* 47). This is a valid point against justificationist claims to certainty, but Eco ignores the falsificationist solution to the problem of knowledge, and lapses into a mild form of scepticism.

Eco warns against attempts to construct "crystal-like and unchanging model[s]" in semiotics, since semiotics deals with "social phenomen[a] subject to changes and re-

structuring" (*TS* 28-9), and at various places talks of the "indeterminacy principle" which "rules" semiotic research (*TS* 29, 129; *SPL* 5, "SDIM" 83). This constitutes an a priori limitation on the possible scope and precision of semiotics, one which is comprehensible only if one assumes that the alternative is a false justificationist certainty. A falsificationist methodology would preserve the possibility of exact theory by denying the need for and the possibility of certainty. Eco, in standard sceptical style, accepts the requirement of certainty, and denies that it can be met.

While a justificationist scepticism constitutes the core of Eco's philosophy of science, his view has many other interesting features. In the remainder of this chapter, I shall look in more detail at Eco's formulations of his position. Again, my goal shall be to show that Eco's view need not further be taken into account in my subsequent chapters.

2.2 Detailed Criticism of Eco's Philosophy of Science

In his short article titled "Semiotics: A Discipline or an Interdisciplinary Method" ("SDIM," later partly incorporated into the Introduction to *SPL*), Eco gives his most

explicit account of what he considers to be the essential characteristics of a science:

> What are, in fact, the criteria for a discipline? First, one needs to have a precise subject; and second, a set of unified methodological tools. We could also list among the requirements--since a discipline is a science--the capability of producing hypotheses, the possibility of making predictions, and--as in the hard sciences--the possibility of modifying the actual state of the objective world. (76)

I shall try to show that all three of these criteria are wrong, that is to say that each is based on a serious misconception, and that, even if a unique branch of real human activity is given by their conjunction, it is not what we would want to call science. Although I shall use these three points as a framework for my discussion of Eco's philosophy of science, I shall draw on the methodological remarks made by Eco throughout his works, and especially on the largely methodological Introductions to *TS* and *SPL*.

2.2.1 "A Precise Subject"

When Eco surveys the range of present day semiotic research, he concludes that "one realizes that in every case the core of the problem revolves around the process of referring back..." ("SDIM" 76). This remark occurs in the context of a discussion of the circumstances which justify

the commencement of semiotic research. In the more recent article "SR." Eco asks a series of questions about "the nature of the sign" (267). In both these cases, he takes as given the existence of, in the first case, a "process of referring back," and in the second of "the sign"; that is, of the phenomena which constitute the subject matter of his theory. These formulations make it plain that Eco considers the subject matter of semiotics to pre-exist the science.

In an important sense, however, a science *invents* its subject matter rather than presupposing it. As Fodor has pointed out, "the idea that it is possible to enumerate a priori the kinds of facts a scientific theory is required to account for," although it has an illustrious philosophical history, depends on the belief that the "facts" to be accounted for can be given in a pure, atheoretical data language. This is a positivistic position that had long ago to be given up ("Some Notes on What Linguistics is About" in Katz, ed. 147-48). In other words, Eco is entitled to claim that there exist "processes of referring back" or "signs" only insofar as his semiotic theory is successful in showing that there exist regularities to which he can then give these names. Theoretical notions like "sign" and "referring back" cannot form part of the conditions of existence of a science; on the contrary, they are, when well confirmed, some of its most highly valued *results*.

Eco has, however, a defense. In the first pages of *SPL*, he sets forth his distinction between a "special semiotics," which is the theory of some particular system of signs, and "general semiotics." A special semiotics "can aspire to a 'scientific' status" (5), but

> The task and nature of a general semiotics are different.... [T]he basic problem of a general semiotics splits into three different questions: (a) Can one approach many, and apparently different, phenomena as if they were all phenomena of signification and/or of communication? (b) Is there a unified approach able to account for all these semiotic phenomena as if they were based on the same system of rules (the notion of system not being a mere analogical one)? (c) Is this approach a 'scientific' one?
>
> If there is something which deserves the name of general semiotics, this something is a discourse dealing with the questions above, and this discourse is a philosophical one. A general semiotics is simply a philosophy of language which stresses the comparative and systematic approach to languages. (*SPL* 6-8)

Eco's distinction between science and philosophy appears to be that philosophy makes truth claims only in relative terms:

> What is "true" for Hegel is radically different from what is "true" for Tarski, and, when the Schoolmen said that truth is the *adaequatio rei et intellectus*, they did not describe entities that were recognizable as such before that definition. The definition decides what a thing is, what understanding is, and what *adaequatio* is. A philosophy cannot be true in the sense in which a scientific description ... is said to be true. A philosophy is true insofar as it satisfies a need to provide a coherent form to the world, so as to allow its followers to deal coherently with it. (*SPL* 11)

Furthermore, science depends on philosophy (as special semiotics does on general): every science "starts by positing philosophical categories" (*SPL* 11). Eco claims that even such basic theoretical terms of the natural sciences as "objects" are only (arbitrary) posits (*SPL* 11). The consequence of all this, which Eco does not explicitly draw, is the irrationalist view that every science is founded on decision.

This view rests again on a justificationist demand for certainty. What Eco calls "philosophical categories" are simply theoretical terms, and, although theoretical terms

cannot be "justified," good reasons can still be had for their acceptance or rejection they need not be merely "posited." This is a central falsificationist view Popper's famous "demarcation criterion" between science and metaphysics gives the form of such reasons: a theoretical term is to be preferred insofar as it exceeds its competitors in its ability to produce testable and well-confirmed assertions about states of the world.

This is also one of Lakatos' main concerns in his large body of work on scientific research programmes A research programme may have a "metaphysical" notion at its core (Lakatos, *Methodology* 41-42), but this can be evaluated, and accepted or rejected in the long run, according to whether it produces a "progressive problemshift" in the body of theory surrounding it. As Chomsky puts it in a different context, "the best way to clarify . . assumptions and to evaluate them is to construct specific models guided by them in particular domains, then to ask how these models fare when interpreted as explanatory theories" (*Rules* 3), this is possible because "any theory of language, grammar, or whatever carries a truth claim if it is serious, though the supporting argument is, and must be, inconclusive" (*Rules* 109). In short, what Eco calls "posits" are just proposals of possibly useful variables of state and of possibly real entities, and these are subject to the constraint that "[t]he viability of any variable [or entity--RR] depends ultimately on its potential to appear in some

generalization or other" (Bunge, *Life* 180). At one point Eco seems almost to recognize this, and states that "[t]he task of a general semiotics is that of tracing a single formal structure which underlies all these phenomena..." (*SPL* 38). Here the conjectural and empirical nature of the assumptions of general semiotics is nearly acknowledged

Furthermore, Eco's "specific semiotics," to which he attributes the status of special sciences, are nothing of the kind. If a specific semiotics "is, or aims at being, the 'grammar' of a particular sign system" (*SPL* 5), rather than the theory of some *kind* or *class* of sign system (e.g. visual sign systems), then it is a description, not a theory.[16] Since descriptions must be selective and can never be complete, description only makes sense as an assignment of values to the variables of state made pertinent by a theory. A major component of any theory is a specification of which features of phenomena are pertinent and which irrelevant Since Eco believes that a general theory of sign systems is possible (*SPL* 6-8), a specific semiotics will be unable to give descriptions without drawing upon this general semiotic theory. Any predictions it may yield will similarly depend upon the general theory. Only if no general semiotic were possible could a specific semiotics in Eco's sense count as a science. A special semiotics in the other sense (the theory of a class of sign systems) might, on the other hand, be counted as a science. The crucial difference is that a special semiotics of this

second kind would rest upon a testable conjecture (which would itself be a part of the general theory) · that systems of a certain class have common properties not shared by others.

Eco's distinction between general and specific semiotics thus fails: there is no more need for or possibility of two levels of semiotic theory in his sense than there is for a "general mechanics" and a plethora of separate and quasi-independent disciplines of "specific mechanics" · the mechanics of sticks, the mechanics of stones, etc.; or the mechanics of stick A, the mechanics of stick B, etc. What remains of his distinction is only the difference between a theory and its applications.

2.2.2 "Unified Methodological Tools"

Eco published a paper explaining in detail his view of the preconditions for the existence of a science of semiotics not long after *TS*. His paper "Semiotics: A Discipline or an Interdisciplinary Method?" constitutes his single clearest discussion in abstract terms of the nature of science and of his own discipline. In it, Eco gives a long list of the "methodological tools" he has in mind·

> Let me list among the paraphernalia of this methodological *koiné* the following· the overwhelming use of the synonymous terms *signans/signatum, signifiant/signifié, expression/content, sign-vehicle/significatum*, and so forth, to describe the

> semiotic relationship in any sign system: the linguistic criterion of *pertinence* as applied to other code systems from gestural to folkloristic: the psychological notion of *frustrated expectation*, the mathematical one of *information*, and the poetic one of *deviation from the norm*, applied together to the analysis of messages: the list of the *functions of language* from Buhler to Jakobson as applied to any form of communication: the extension of the notion of *binarism* to certain syntactic systems and even to structural semantics: the concept of *distinctive feature* working outside the domain of phonology, from visual signals to genetic units: the opposition between *selection and combination* to explain phenomena of various languages, from movies to music: the unified use of the Peircean notion of *interpretant*, which, even in linguistics, does not work if not viewed as an intersemiotic substitution of a sign by another sign and so on: and, finally, the pair *code/message* originally derived from the mathematical theory of communication.... The list could go on and include other categories that come from other disciplines and are widely used today as "pansemiotic" categories. ("SDIM" 81-82)

This is a list neither of methods nor of techniques:[19] it is a list of theoretical terms, each of which may be judged useful and acceptable, or otherwise rejected as ill-con-

firmed and useless, as just discussed. If Eco presents them as "methodological tools" which are presumably beyond dispute, he paints a picture of an orthodoxy, not of a science.

Even if an argument could be made for a necessary difference in method between the human and the natural sciences, which Rudner and many others have shown not to be the case.[20] Eco's is a list of substantive rather than methodological positions. To claim that the distinction "signans/signatum" is useful is to claim that a certain class of entities is composed of two sorts of things with different properties. This is an empirical claim, and as such must be established rather than presupposed by a science. Elsewhere Eco gives a list of the "methodological assumptions" of semiotics which begins "(a) meanings are cultural units; (b) these units can be isolated thanks to the chain of their interpretants as revealed in a given culture," and goes on to list four other equally substantive claims (*TS* 83).

It is clearly circular to take substantive claims which can only be the findings of a theory as the "methodological assumptions' of that same theory. Eco might respond that these are the results of *other* theories (in linguistics and other disciplines), but if semiotics is to be a science which carries on work begun in these disciplines it must not adopt their results dogmatically as "methodological assumptions," but must on the contrary accept them tent-

atively and subject to the possibility (and even likelihood) that they will be refuted or at least improved upon in the future. Borrowing from other theories is perfectly acceptable, but only so long as the epistemic status of the borrowed concepts is kept clear. For Eco to accept these borrowings unquestioningly is to foreclose the possibility of increased understanding. A borrowed theoretical term must either remain open to doubt, in which case it can hardly be taken as a "methodological tool," or else be mere dogma [21]

2.2.3 "Hypotheses," "Predictions," and "Modifying the State of the World"

These three requirements form a rather uneasy group. The first two seem in line with a falsificationist philosophy of science, although in Eco's article they are only loosely and inadequately specified. Both Popper and Lakatos require a scientific theory to be adventurous, to make bold and testable assertions. Although Lakatos is more inclined to be lenient towards young sciences, and does not believe that refutations can be as instantaneous as Popper seems to have it, both hold that the empirical content or verisimilitude of a theory is the property by which its worth may be assessed and compared to that of others [22]. Eco's formulation lacks the notion of this "quasi-measure-theoretical" (Lakatos, *Methodology* 101) criterion of the goodness of predictions. There is nothing

in his formulation which would prevent his requirement that a scientific theory ought to produce predictions from being met by vacuous or unfalsifiable predictions, e.g. pure existence statements

The 'ability to change the state of things of which it speaks" ("SDIM" 82), the third term of Eco's conjunction, however, has little to do with the scientificity of a theory. To see this, we need only notice that if this were a criterion of the scientificity of a discipline, we should have to say that astrophysics is not a science, since presumably astrophysicists cannot (yet) modify the states of stars and galaxies. In Eco's formulation this requirement appears to issue not from concerns about the criteria for adequate theory in his discipline, but from his apparently a priori belief that semiotics "is [or 'ought to be'--RR] a form of social practice," a "scientific carrefour . similar to medicine, which does not limit its task to a given state of affairs but attempts to improve it" ("SDIM" 76, 82).

3 Formal Devices Symbolisms and Calculi

3 1 Formalization

In this chapter I shall discuss Eco's use of formal devices, the chief ingredients of the "technical sophistication" and "precision" for which Eco has so often and so highly been praised (Berger 211, Lanigan 345, and many others; cf Chapter One above). I shall try to show that most of this praise has been misplaced: that Eco's ventures into formalism are generally defective and that, far from contributing to the "determinacy, universality, flexibility, and abstractness" (the four virtues of "the mathematical kind of language" listed by Wallace 116) of his work, they are fit to serve few purposes other than obscurantist ones. In so doing, I do not mean to suggest that full formalization is a *sine qua non* for the scientificity of a theory This is clearly false: a fallibilist holds that the conditions for the scientificity of a theory are those given by Popper's demarcation criterion of falsifiability and refined by Lakatos in his notion of progressive problemshift (cf. my Introduction above for references), and while falsifiability and progressiveness demand a certain explicitness and definiteness, they do not demand formalization. Nevertheless, there is no doubt that, wherever it is possible, formalization of a theory (or part of a theory) improves it.[25] I have shown (in Chapter One) that Eco's work has generally been understood within scientific

semiotics as embodying an appreciable formal component. What I shall try to show in this chapter is that Eco's ventures into formalization cannot be construed in a manner which lends credit to his theories.

In assessing Eco's uses of formalism, I shall make use of the standard criteria of formalization, and make no attempt to take account of the concepts of "partial formalization" sometimes encountered in the literature of the philosophy of social science. Rudner, although a proponent of the possibility of partial formalization in the social sciences, has given an eloquent account of the serious problems which must still be solved before this cloudy notion can be made sense of and usefully applied (47ff) [24] Pending solutions to these problems, it seems as well to let the concept lie.

As Lieb has pointed out, there are three relevant senses of "formal method":

a. Formal methods consist in the use (mostly informal) of existing mathematical theories.

b. Formal methods consist in the use of a formal (constructed) language or of a regimented form of a natural language.

c. Formal methods consist in systematic theory construction, in particular in theory construction including (i) or (ii) or both (i) and (ii):

i. formulating a theory in a formal (constructed) language or in a regimented form of a natural language.

ii. formulating a theory in axiomatic form.

I will not discuss those elements of Eco's work which fall under (a), since these are infrequent and somewhat incidental (they consist mostly in the occasional use or mention of mathematical information theory). On the other hand, as we shall see, none of Eco's work falls under (c), since nowhere does he attempt to frame a significant portion of his semiotic theory either in a constructed language or in axiomatic form. Eco does, however, frequently make use of formalisms of type (b): formalisms which are not models (in the logical sense) of a theory, but which are used as adjuncts, amplifications, or examples.

Of the three main types of formal device which appear in Eco's work--typologies, symbolisms, and diagrams--I shall discuss only the latter two here. The status of Eco's typologies as nominal or ordinal scales (that is, scales based respectively on a simple relation of identity or non-identity of items, with no ordering, and scales based on an ordering relation, but with no comparisons of intervals or magnitudes) is clear, and Eco does not in general attempt operations upon his typologies beyond the "permissible statistics" for these types of scale (Rudner 36ff, Stevens 142). Questions could be asked about the adequacy of the semantics of Eco's typologies, as well as

about their usefulness, but these issues cannot be gone into here.

Eco's use of symbolic calculi and of diagrams, however, exhibits grosser flaws, and these form the subject of the present chapter. I shall discuss them in turn.

3.2 Symbolisms and Calculi

Alonzo Church, in his article on "The Need for Abstract Entities in Semantic Analysis," gives the elements of an axiomatic system as follows:

> As the primitive basis of a logistic system it suffices to give, in familiar fashion, (1) the list of primitive symbols or *vocabulary* of the system (together usually with a classification of the primitive symbols into categories, which will be used in stating the formation rules and rules of inference); (2) the *formation rules*, determining which finite sequences of primitive symbols are to be *well-formed* expressions, determining certain categories of well-formed expressions--among which we shall assume that at least the category of *sentence* is included--, and determining (in case *variables* are included among the primitive symbols) which occurrences of variables in well-formed *expressions* are *free* occurrences and which are *bound* occurrences; (3) the transformation rules or *rules of inference*, by which, from the assertion of cer-

> tain sentences (the *premisses*, finite in number) a certain sentence (the *conclusion*) may be *inferred*: (4) certain asserted sentences, the *axioms*. (Fodor and Katz, eds., 437)

Rudner gives an essentially identical account (13-14), and notes that a formalism in an empirical science takes the form of an *interpreted deductive system*, which is an axiomatic system which fulfils certain technical conditions ensuring that

> [for] each possible interpretation of the calculus (by semantical rules) that makes the axioms true, every theorem (that is every *wff* derivable from the axioms in the calculus by applications of the transformation rules) likewise is true (18),

and which has been provided with an interpretation connecting theoretical terms to observables (17-18).[25] It is to this picture of formalism that I shall compare Eco's efforts.

The first point to be noticed about Eco's uses of symbolism is that they do not ever serve as theory: in no case does he establish a calculus for the purpose of obtaining a formal device permitting the deduction of new statements which are then interpreted as the predictions of an empirical theory. He introduces and subsequently drops a plethora of symbolisms: e.g. *TS* 49, where a new and unexplained notation is used for one line and then dropped; the several variations of his notation for componential analysis (*TS*

91, 114, 171 and *SPL* 70, 118-19), the temporary adoption of some symbols from the propositional calculus (*SPL* 18), and the cryptic diagrams of *SPL* 97 and 98 and "SDIM" 78. These are unrelated to one another, and serve simply as rather *ad hoc* and unsystematic amplifications of Eco's text as a sort of symbolic shorthand.

This is not as harmless as it may sound. While a translation to symbols of a point made in the text could potentially lend precision or clarity to Eco's argument, this would be possible only if he were to specify formally (for each calculus he meant to employ) the vocabulary of symbols, the well-formedness (formation) rules, and the rules of inference (transformation rules), as well as an interpretation of the calculus connecting it with the matters discussed in his text--in short, only if he were to make use of an interpreted deductive system as his symbolism. Eco does not do this, and the results are disastrous. In many cases he simply adopts a few vocabulary items from a well-known formal system, such as the propositional calculus, and employs them as if they possessed an intrinsic meaning which would somehow be preserved when they are applied in arbitrary ways.[26]

While examples could be multiplied, two will have to suffice. The first example occurs in Eco's discussion of the inferential quality of the sign.

> Let us attempt an analysis of a typical semiotic maze. A red flag with a Hammer and Sickle is equi-

> valent to Communism (p ≡ q) But if someone carries a red flag with a Hammer and Sickle, then that person is probably a Communist (p ⊃ q) (*SPL* 18)

Apparently we are to take p and q as variables standing for a red flag and Communism respectively (or for "a red flag" and "Communism". Eco does not tell us the type or level of these variables). Substituting for p and q in the first expression we obtain either "a red flag ≡ Communism," or ""a red flag" ≡ "Communism "" Whatever these mean, they clearly do *not* mean that a red flag and Communism (or "a red flag" and "Communism") satisfy the truth tables for ≡ or ⊃, since none of these items has a truth value at all, unless we take them as (non-standard) variables of the propositional calculus. This move is precluded by the fact that Eco is here discussing "signs," and variables of the propositional calculus are not signs in his sense. Since Eco gives us no deductive system within which the symbols "≡" and "⊃" are defined, their relationship is absolutely unspecified. Eco's usage of them provides contextual clues allowing us to conclude that "≡" is to be taken as a synonym of some sense of the English expression "is equivalent to," that "⊃" is to be taken as synonymous with the English construction "if ..then," and that "p" and "q" are to be taken as variables of some kind. If this is all we can make of the symbols, then they are entirely redundant, since each occurs adjacent to its definiens; therefore

Eco's introduction of the symbolism is gratuitous at best. At worst, it is seriously misleading, in at least two respects. first, it gives a false appearance of rigour and second it is if anything even *less* precise than Eco's formulation in English prose. since the symbolic expressions depend upon implicit (and therefore imprecise) definitions in terms of the already imprecise English text.

Another example displaying the same flaws even more clearly occurs in Eco's discussion of rhetorical code changing:

> Suppose . that there is an axis containing two semantic units (u_1 and u_2), that are usually considered mutually incompatible, because their first respective denotative markers are units derived from an oppositional axis (α_2 vs. α_1), but that, through α_1 , have a connotation (in common .
>
> Let us now suppose that, through a series of rhetorical substitutions a sememe can be named (and therefore rendered rhetorically equivalent a) either (i) by one of its markers (a case of metonymical substitution, represented by *mtn*, followed where necessary by the marker via which the connection is made), or (ii) by another sememe with which it shares a given marker (a case of metaphorical substitution represented by *mtf*, followed by the marker upon which the substitution relies) (*TS* 284-85)

This is then rendered as

$$((u_1\ (mtn\ \alpha_1) \equiv u_1) \cdot (u_1\ (mtn\ \alpha_2) \equiv u_2)) \rightarrow (u_1\ (mtf\ u_1) \equiv u_2)$$

which is explained as follows:

> Provided that the rules not of formal logic but of rhetoric are in play, then u_1 (because of its equivalence to u_2) acquires both markers α_1 and α_2, which were previously seen to be antonymically incompatible. (285)

There are several things to notice here. First, the symbols "$\cdot$" and "$\rightarrow$" are not even *informally* defined, despite the fact that the rules of formal logic are explicitly said *not* to be "in play." Second, the symbols "$\equiv$," "*mtn*" and "*mtf*" are given interpretations, but no formal definitions: they are said to represent "rhetorical equivalence," "metonymical substitution" and "metaphorical substitution," respectively, but the properties of the symbols "$\equiv$," "*mtn*" and "*mtf*" in the calculus are totally unspecified.[27] Third, the notion of "antonymical incompatibility" is said to be represented by this symbolism, but no symbol or combination of symbols is interpreted as standing for it.

It is quite clear here that the transformation rules of this symbolism are given only in terms of the supposedly *already clear* properties of "rhetorical equivalence," "metonymical substitution" and "metaphorical substitution": a user of this symbolism can know how to manipulate it only insofar as she already knows in detail the properties of these theoretical terms. The formation rules are even less

clearly specified. they are nothing but a fuzzy amalgam of the formation rules of the predicate calculus and the formation rules of English. A number of vocabulary items are listed. but since definitions of these must take the form of formation and transformation rules. they are no more precisely given than the rules are. Finally. Eco lists no axioms whatsoever

3.3 Diagrams

Eco uses diagrammatic devices even more often than symbolisms. but his use of diagrams succumbs to many of the same difficulties which plague his use of symbolisms While diagrams are in general merely heuristic rather than theoretical devices. and therefore less subject to rigorous conditions for their proper usage than calculi. this is not to say that they may be indiscriminately employed Analogously to the rules for calculi. at a minimum. for a relatively precise use of a diagram. the following should be given: 1. a vocabulary of graphical items. 2 a list of the features of graphical items and of the relations between graphical items which are to be considered pertinent (e g size. adjacency. position about an axis. or parallelism). 3. an interpretation mapping features of graphical elements and relations between graphical elements onto theoretical terms.

Eco repeatedly uses diagrams which meet few or none of these requirements. and which are thus so open to interpre-

tation (in the non-logical sense) as to be of no help in clarifying his theoretical statements. Examples could again be multiplied, but four should be sufficient. A good example is Table 30 of *TS* (142), Eco's often-reproduced diagram of the process of interpretation. At least the following are unspecified in Eco's use of this diagram: the theoretical concepts (if any) represented by the arrows, by adjacency of (or distance between) figures, and by the shapes of figures. One might also assume that some transformations take place each time an arrow traverses a figure, but this is nowhere made explicit. Figure 3.5 of *SPL* (98) leaves us to guess the reference of the arrows (do they stand for a relation of containment, reference, or something else?) and of the vertical placement of items. Even when Eco reproduces a diagram from another author who is more careful to specify the pertinent features and the interpretation in theoretical terms of the diagram, as is the case of Table 25 of *TS* (123), which is a reproduction of Figure 4-1a of M. Ross Quillian's article on "Semantic Memory" (Minsky ed. 225), he does not reproduce the discussion of these. In this case, he omits Quillian's Figure 4-1 (224), which is a careful enumeration of the pertinent graphical units and their theoretical interpretations, constituting a key to the diagram reproduced by Eco. He also omits Quillian's discussion and explanation of the two figures (223-26).[26]

In none of these cases does Eco even tell us which graphical features and relations are pertinent (much less what theoretical terms are represented by the pertinent ones), and we are left to guess the pertinent features by looking for systematic variations in the diagrammatic representations of different entities. Needless to say, this procedure is far from reliable. We have even less to go on when we attempt to guess the interpretations of these graphical items.

In general, Eco's use of diagrams is even looser than his use of symbolisms: he only rarely indicates any of the pertinent graphical elements, and he never bothers to specify either the pertinent relations between graphical elements, or the interpretation of these elements in theoretical terms.

4 Eco's Model Q

4.1 Inclusions and Exclusions

In this chapter, I shall examine Eco's Model Q of "unlimited semiosis" (*TS* 69, 71ff) or "infinite semantic recursivity" (*TS* 121ff), and the complex of concepts surrounding it, in some detail [29] Eco presents his Model Q as the culminating solution to the complex of problems he identifies surrounding the Katz and Fodor theory of a dictionary-like semantic competence. Model Q is his final move, which he introduces after having first proposed and then rejected the "Revised Model" (*TS* 105) which attempts to augment the KF account by means of an insertion of circumstantial and contextual selectors into the semantic analyses. Model Q remains central to Eco's most recent work (e.g. *SPL* 2, 82ff, 113-127)

Model Q seems therefore an appropriate element of Eco's semiotics to subject to closer and more detailed scrutiny than I have been able to afford in previous chapters, preoccupied as those chapters were with global features of Eco's work. By means of this detailed examination I hope to show that at a lower level Eco's work suffers from flaws related to the higher-level ones discussed in my previous chapters that his flawed logic of inquiry leads to inadequate and poorly thought out theory construction. The first part of this chapter consists in a comparison of Eco's Model Q to its source in the work of M. Ross Quil-

lian.[30] In this part I shall argue that Eco has misunderstood the work of Quillian and that many of the alterations to Quillian's model of a semantic memory which result from this misunderstanding are flawed or incoherent in various ways. In the following part I shall argue that many of the constituents of Eco's Model Q are in themselves unacceptable. These two parts consist largely of discussions of the many small and medium-sized aporiae in Model Q, its components, and the related concepts.

In the last part of the chapter I shall try to show that the adoption of a theory with certain of the features of Model Q (having to do with its relationship to its predecessors and its low empirical content) is disallowed on purely methodological grounds. This part will draw on the arguments of the preceding two parts to show that Model Q is not simply substantively incorrect, but in fact unacceptable as a (true or false, confirmed or disconfirmed) scientific theory.

In order to be able to concentrate on the subjects just mentioned, I will not touch on any of the following issues all of which deserve detailed discussion: Eco's theory of meaning and denotation, his position on semantic universals and the "structure of the Human Mind" (*TS* 126), questions of performance and competence (Eco takes Model Q to represent a portion of the "Semantic Universe" [*TS* 125], while Quillian's is a performance model [Quillian 220-21]), his somewhat surprising adoption of the well-aged Katz and

Fodor semantic theory as the sole representative of the enormous body of work in semantics by linguists, and his notion of model as distinguished from theory On this last point, I shall say only that since Eco's Model Q does not seem to be the sort of thing meant by any of the standard usages of the term "model" (cf Bunge *Formal* 79-83 for discussion and references), and since Eco does not specify what distinction he means, if any, I shall treat Model Q as theory, the form of which it resembles more closely

4 2 Relations of Model Q to Quillian's Model of a Semantic Memory

Although Eco cites Quillian's article on "Semantic Memory" as the source of the theory he calls Model Q and reproduces a diagram from this article,[31] and although Eco attributes Model Q directly to Quillian (*TS* 122), his Model Q has little in common with Quillian's model of a semantic memory beyond a superficial terminological resemblance due to Eco's adoption of a number of Quillian's terms (e.g. "associative link," "head node," "token node," "type node"). In fact, Eco's assertions about the properties of the network of associative links constituting the semantic model in many cases constitute direct contradictions of Quillian's, and in a number of other cases are incompatible with them. I shall examine a number of these conflicts.

4.3.1 Finiteness

Eco asserts that Model Q represents "a process of unlimited semiosis" and "everlasting recursivity" in a network of nodes the links between which represent a relation Eco identifies with Peirce's notion of interpretant.[32] The network is one with "infinite peripheries" in which starting from any node, "it is possible to penetrate from the center to the farthest periphery, the whole universe of cultural units" (*TS* 122).

In Quillian's exposition, however, all of the following are finite: the size of the network (221), the number of other nodes reachable by traversing links starting at an arbitrary node (226-27), and the number of types of associative link (229-30). These are not merely differences of opinion: as I shall show, these alterations introduce some severe defects into Quillian's model, and lead to absurd consequences.

If Eco's suggestion that the network is of infinite size is taken seriously, we are forced to accept at least one of two patently false views. Since in Eco's account the network is a competence model, and since competence is a component of performance,[33] we are left with the problem of how an infinite competence (not just the usual notion of a competence capable of generating an infinite number of sentences, but a competence which is itself of infinite size) may be realized in a finite mind. Clearly it cannot, and so Eco's suggestion forces us to conclude that the

human mind is of infinite size.[34] This conclusion can be avoided only by asserting that only a finite subset of the infinite total competence is realized in each speaker. This leads however to the conclusion that there is an infinite number of speakers, since the disjunction of a finite number of finite sets (the individually realized subsets of the global competence) could yield only a (possibly large but still) finite set. Eco's assertion that the network representing competence is infinite must therefore be rejected as entailing false consequences: either that the human mind is infinite, or that there are infinitely many speakers (of each language).

1.2.2 Accessibility of Nodes

Eco explains that the links in the network represent the relation interpretant-of (*TS* 122; cf. below for discussion) and he further asserts that every node is reachable by traversing the chains of interpretants beginning at any arbitrary node. (Let us call the dyadic relation obtaining between nodes a and b when node b is reachable by traversing an arbitrary number of interpretant links from node a "I-accessibility," written as "*I*ab." Eco's claim is then that, for all nodes a and b, *I*ab holds).[35] Quillian, on the other hand, specifies that the meaning (represented in the model by the "full concept") of the term represented

by a node is given by the subset of nodes I-accessible from it:

> Let us define a *full word concept*, as distinguished from its plane or "immediate definition," so as to include *all* the token and type nodes one can get to by starting at the initial type node, or patriarch, and moving first within its immediate definition plane to all the token nodes found there, then on "through" to the type nodes named by *each* of these token nodes, then on to the token nodes in each of *their* immediate definition planes, and so on until every token and type node that can be reached by this process has been traced through at least once. . .
>
> To summarize, *a word's full concept is defined in the memory model to be all the nodes that can be reached by an exhaustive tracing process, originating at its initial, patriarch type node, together with the total sum of relationships among these nodes specified by within-plane, token-to-token links.* (226-27, emphasis in original)

On pain of triviality, the set of nodes reachable in this way must be assumed to be, in general, a proper subset of the full set of nodes, since if in general every node were I-accessible from every other, all would, by this account, be synonymous. Eco, in asserting that *Iab* holds for all nodes a and b, owes us a new account of meaning, since

Quillian's representation of meaning by "full concept" can no longer be used, as it would render all nodes synonymous. This, however, Eco does not provide It would seem that a possible solution would be to drop the specification of the subset of nodes from Quillian's version of the "full concept" retaining only the configuration of nodes and the relationships between them specified by the token-to-token links Attempting this in Eco's version of the network, however, still leaves all terms synonymous: since the entire network is included in each "full concept," the configuration of nodes in each "full concept" is the total configuration of the network. Further, as we shall see below, Eco's version of the network is totally homogeneous, which means that even if he were to alter his theory so that *I*ab would not hold for all a and b, still no section of the network would have a unique configuration, and a great many distinct terms would still come out synonymous.

Quillian also limits the number of types of associative link to six (230). Eco, on the other hand, understands the links in the network to represent the interpretant relation, which, as we have just seen, he believes holds ubiquitously It is not clear whether we ought to take the interpretant relation to be a single relation of a very general kind, or the disjunction of a large (possibly infinite) number of more narrowly defined relations, such as those listed by Quillian. Whichever way we construe this, the extreme breadth of this notion causes certain further

difficulties for Eco's theory to which I shall return below.

4.2.3 Structure of the Network

As we have just seen, Eco's network is a mass of nodes connected by links of only one type, that which represents the interpretant relation. He states furthermore that individual nodes can be simultaneously both token- and type-nodes:

> The definition of a type A foresees the employment, as its interpretants, of a series of other sign-vehicles which are included as *tokens* (and which in the model are other lexemes). The configuration of the meaning of the lexeme is given by the multiplicity of its links with various tokens, each of which, however, becomes in turn a *type* B, that is the patriarch of a new configuration which includes as tokens many other lexemes, some of which were also tokens of type A, and which can include as tokens the same type A (*TS* 122)

This is again at variance with Quillian's version, in which token nodes have a special type of link back to the corresponding type nodes, but are not identical to them and do not function as type nodes for any concept (223). Eco also drops without discussion Quillian's quantity, number and criteriality qualifiers (231-32), as well as his parameter symbols (233). The result is that in Eco's version of the

network all nodes are of one kind Since there is also only one kind of link between nodes, and since there no reason to think that all nodes are not directly linked (Eco tells us nothing about the interpretant relation which would preclude this and in fact makes it seem inevitable. (f below), it seems that Eco's version of the network is totally homogeneous which is to say that it is totally unstructured and totally uninformative

4 3 "Code" and "Cultural Unit"

Eco writes that a code is constituted of the "crossings" of

> a vast series of partial content systems (or fields), which are matched in different ways with the expressive units Thus the system of semantic fields involved as it is in multiple shiftings, becomes crossed . by various paths from each sememe The sum of these crossings makes up Model Q. (*TS* 125)

Eco then elaborates on this relationship in the following way:

> A code as "*langue*" must therefore be understood as a sum of notions (some concerning the combinational rules of the content items, or semantic markers) which can be viewed as the *competence* of the speaker However, in reality this competence is the sum of the individual competences which constitute the code as a collective convention. What was called

> "the code" is thus better viewed as a *complex net work of subcodes* which goes far beyond such categories as "grammar" however comprehensive they may be One might therefore call it a *hypercode* (following the etymology of "hypercube") which gathers together various subcodes, some of which are strong and stable while others are weak and transient, such as a lot of peripheral connotative couplings (*TS* 125)

Elsewhere Eco explains that a code contains the body of knowledge shared by the members of a culture in the form of assignments of properties (in Eco's terms content units) to cultural units (*TS* 64) A code establishes a correspondence or mapping between expression-units and content units, and the latter take the form of "cultural units" which all together constitute a "cultural world" Cultural units give the meanings of lexemes (*TS* 61 62 66 68) As we have seen, however, the content-units and expression units which figure in the code are not distinct The meaning of a term (taken temporarily as an expression-unit) is specified in Model 2 by its interpretant-links to other terms (which are thus taken temporarily as content units), but each node in the network figures at various times both as definiens and definiendum (cf the passage from *TS* 125 reproduced above) Putting all this together we get a picture in which knowledge (of cultural units) is held directly in the form of expression-units to be consistent

Eco must believe that not only is there a language of thought but that it is quite simply English (or Italian, or Javanese or whatever).[36] This is a view which has been effectively refuted by the observation that non- and pre-linguistic organisms, such as dogs and infants, do in fact think (Fodor, *The Language of Thought* 56-58).

Eco somehow manages to avoid drawing this conclusion, although it follows from the views that I have just discussed as well in a parallel way from his belief in the irreducibility of cultural units (implicit in his whole argument against the possibility of Katz and Fodor-style semantic analysis): if meanings are given by irreducible cultural units, then these must be directly apprehended but cultural units are such things as "uncle town, blue (depressed) a hunch, the idea of progress, hope and art" (*TS* 87 quoting Schneider's *American Kinship: A Cultural Account*): they are lexical items of American English (for an American). Eco is forced to believe, then, that lexical items of American English are directly apprehended by the English-speaking American.

Eco's later development of the concept of code merits an aside at this point. Eco's chapter on codes in *SPL* constitutes a near-repudiation of the concept of code in favour of the concept 'encyclopedia," which "improves and better articulates the 'old' concept of code" (*SPL* 164). In this chapter, Eco reviews the history of the concept of code in order to explore "the reasons for which [it] en-

joved consensus and popularity as well as [its] perhaps still undiscovered fruitfulness" (*TPL* 165). The difficulties Eco now sees with the code concept have to do with the fact that a system (or system of systems) of correlations (or partial correlations) of expressions to contents cannot capture the reality of unlimited inferential semiosis:

> 'At its very birth the idea of code appears wrapped in ambiguity bound to a pancommunicative hypothesis. It is not a guarantee of communicability but, rather of structural coherence and of access between different systems. An ambiguity rooted in the twofold meaning of communication: communication as a *transfer* of information between two poles, and as *accessibility* or *passage* between spaces. The two concepts imply one another. Their confusion can be fruitful: maybe there are common rules for two distinct operations and these rules are not ineffable but can be expressed (maybe) by an algorithm. In other words they are *coded*. Most of the resistance against the notion of code was due to this fear of hyperrationalization. On the other hand, the popularity of the new category had all the characteristics of an exorcism: it constituted an attempt to force order upon movement, structure upon events, organization upon earth tremors. Speaking of codes meant for many to identify 'scripts' where, previously, only random,

> blind impulses, unspeakable creativity dialectical contradictions were recognized It was perhaps a short 'rationalistic' season, as soon as it was possible poststructuralism replaced codes with naked *desires*, pulsions, drifts (*SPL* 168)

Finally however, Eco effects a partial rescue a code

> is the whole of the encyclopedic competence, as the storage of what is already known and organized by a culture It is the encyclopedia, and therefore the Rule but as a Labyrinth A Rule which controls but which at the same time allows, gives the possibility of inventing beyond itself, by finding new paths, new combinations within the network
>
> A code is not only a rule which *closes* but also a rule which *opens*. (*SPL* 187)

This reworking of the concept of code, although superior to the old formulation in adopting the more realistic metaphor of encyclopedia for cultural competence which has excellent credentials from the filed of artificial intelligence: nevertheless does not get Eco out of the difficulties discussed in relation to his earlier formulations It is on the contrary a surrender to the contradictions arising from Eco's concepts of unlimited semiosis and cultural unit In Eco's earlier formulation a code was made up of an infinite network of interdefined items in perpetual and rapid flux, but now, even when considered as frozen

at a given instant it is furthermore to be understood as subject to 'creative' violation

This final move is sufficient to reduce the predictive power of the theory to zero." Any attempted falsification of a theory which incorporates this new notion to do (as all falsifications must be upon a prediction made by the theory, would be vitiated by Eco's escape clause of creativity.[38]

I shall return to this point below first, however, it is necessary to examine another of Model Q's dependent concepts

4.4 "Interpretant"

The interpretant relation is defined as representing the conjunction of the relations of denotation and connotation (*TS* 70, 124), and since these are nowhere well defined apparently anything goes. At one point Eco has a sign connoting the "negation of its antonym" (*TS* 101) (despite the fact that signs are not propositions and consequently have no truth values and so cannot be negated in any strict sense--presumably Eco means "antonym of its antonym") elsewhere the position of a chess piece connotes "a series of optional moves, a set of possible responses, a chain of foreseeable (or unforeseeable) solutions and therefore a series of new interrelational positions of the entire set of pieces" (*TS* 90), and elsewhere again a sentence about Napoleon connotes historical truth, while another about

Ulysses connotes legend (*TS* 65). Eco seems to value the breadth of the concept. He writes that

> The idea of the interpretant makes a theory of signification a rigorous science of cultural chemeras. The interpretant can assume different forms:
>
> a) It can be the equivalent (or apparently equivalent) sign-vehicle in another semiotic system. . .
>
> b) It can be the index which is directed to a single object, perhaps implying an element of universal quantification.
>
> c) It can be a scientific (or naive) definition in terms of the same semiotic system.
>
> d) It can be an emotive association.
>
> e) It can simply be the translation of the term into another language.
>
> Moreover, the interpretant can be a response, a behavioral habit determined by a sign, and many other things. (*TS* 70)

The last clause ("many other things") is hardly necessary; little if anything is excluded by the previous provisions. Since the interpretant relation apparently obtains not only between a sign-vehicle and every other sign-vehicle (which seems to be assured by the vagueness of item a above, since it is trivial to show that an equivalence class into which both of a pair of arbitrarily chosen terms will fall can easily be constructed), it also holds between a sign-ve-

hicle and *every other entity*. This is assured by item 1 and the concluding paragraph quoted above: "since anything whatever, be it a physical or an abstract object, can be the object of an 'emotive association,' a 'response' or a 'habit'." Since a relation which holds universally does not tell us anything whatever about the objects it relates, the concept of interpretant as defined by Eco can have no theoretical interest. This clarifies Galan's remark (quoted in Chapter One) that Model Q "has the force and attraction of poetic vision but can hardly be expected to be of any explanatory value" (357).[39] This observation brings me to my next subject, the explanatory value of Model Q--that is, the question of its empirical content.

4.5 Empirical Content

We have seen that Lakatos refined Popper's demarcation between science and metaphysics, recasting it as the criterion of "progressiveness" of a problemshift, or sequence of theories. We are now in a position to ask whether Eco's Model Q constitutes a step in a progressive problemshift, that is, whether it can be counted as a scientific theory. Lakatos writes that

> a theory is "acceptable" or "scientific" only if it has corroborated excess empirical content over its predecessor (or rival), that is, only if it leads to the discovery of novel facts. This condition can be analysed into two clauses that the new

theory has excess empirical content ("acceptibility$_1$") and that some of this excess content is verified ("acceptibility$_2$"). The first clause can be checked instantly by a *priori* logical analysis the second can be checked only empirically and this may take an indefinite time.

Let us take a series of theories, T_1 ... T_3 where each subsequent theory results from adding auxiliary clauses to (or from semantical reinterpretations of) the previous theory in order to accommodate some anomaly, each theory having at least as much empirical content as the unrefuted content of its predecessor. Let us say that such a series of theories is *theoretically progressive (or "constitutes a theoretically progressive problemshift")* if each new theory has some excess empirical content over its predecessor, that is, if it predicts some novel, hitherto unexpected fact. Let us say that a theoretically progressive series of theories is also *empirically progressive (or "constitutes an empirically progressive problemshift")* if some of this excess content is also corroborated, that is, if each new theory leads us to the actual discovery of some *new fact*. Finally, let us call a problemshift *progressive* if it is both theoretically progressive and empirically

> progressive, and *degenerating* if it is not (*Methodology* 32-34)

I shall show that Model Q has no excess empirical content over its predecessors (that it is not acceptable, or theoretically progressive)[40] and that therefore a painstaking empirical investigation of its acceptibility, is unnecessary, since Model Q fails to fulfil the logically prior requirement that it have excess empirical content[41]

In fact, I shall make a stronger claim that the empirical content of Model Q (as a semantic theory more on this qualification later) is zero[42] If this is true, then Model Q could not be considered a progressive step in a problemshift from *any* non-empty predecessor semantic theory This will enable me to avoid what might otherwise have been a somewhat difficult question about what ought to be taken as Model Q's predecessor or rival theories the Katz and Fodor semantics, or Quillian's model of a semantic memory (or indeed all current linguistic and logical semantic theory, as Eco's global denunciations of these fields might lead one to think)

Empirical content on Lakatos' account consists in the ability to predict novel facts Popper gives a more precise account of this in the technical addendum to *Conjectures and Refutations*

> from a theory alone no observational statement follows. (From "All ravens are black" we cannot derive any observational statement like "There is a

> black raven here now," although we can indeed derive the statement "There is no white raven here now."
>
> This is the reason why in defining empirical content I fall back on the idea that a *theory tells us the more about observable facts the more such facts it forbids*--that is to say the more such facts are incompatible with it. (385)

Eco's Model Q forbids no observations in its domain. Since as we have seen, the interpretant relation is defined by Eco in a way which admits as an instance of it any link however fanciful, between two words (or even between a word and an arbitrary object either concrete or abstract), and since the interpretant relation is, in Model Q, the only possible relation between semantic items, Model Q forbids no semantic relationship whatever. Since no possible semantic relationship is excluded, none can be predicted under any circumstances.[43] Since no fact can be predicted, a *fortiori* no *novel* fact can be predicted, provided only that we take as Model Q's theoretical predecessor any semantic theory which makes any prediction whatever, whether this prediction is corroborated or not (for example, we can choose the Katz and Fodor theory). Model Q therefore fails to be acceptable, (or equivalently, fails to be theoretically progressive) no matter what non-empty semantic theory we choose as its predecessor or rival.

This would be a rather strong conclusion. It would amount to a classification of Model Q, the centerpiece of Eco's semiotics, as sheer pseudoscience or metaphysics. Eco's theory is, however, saved from this fate, albeit in a rather surprising way. We have seen that Eco maintains that meaning is given in terms of irreducible "cultural units" which are identified with lexical items in natural languages, and I have argued that he must further be committed to the belief that these are directly apprehended: that is, that natural languages are the languages of thought. This is almost certainly a false claim (see the arguments against it cited above), but it is an empirical one. Model Q, therefore, although empty (without empirical content) as a semantic theory, appears to be a presumably false, but nevertheless non-empty, psychological one. Ironically enough, Eco's theory is saved from the charge of vacuity because it entails the existence of a "structure of the Human Mind" of precisely the type which Eco so abhors.

5 Discussion

5.1 Conclusions

In the Introduction, I stated that I would show that the failure of 'scientific semiotics' should be put down to its having been poor science rather than to either the purported inapplicability of scientific method to human affairs (the dualistic *Geisteswissenschaften* claim) or the supposed collapse into contradiction of the scientific epistemology (the claim which I discussed under the rubric "postmodern" in Chapter One). I have not argued directly against humanistic dualism, since this has been often and well done elsewhere[44] and I have only made a suggestion in passing that postmodernist criticisms of science are typically based upon an obsolete positivistic philosophy of science. I have instead addressed the other side of the question. Despite the obvious pragmatic essayistic, pedagogical, and political virtues of much of Eco's work, I believe I have shown that his semiotic theory falls short, in several ways, of what can reasonably be expected even in a young and relatively undeveloped *science*.

In Chapter One I tried to show that Eco's *TS* and *SPL* are accepted as exemplary works in scientific semiotics, that this discipline conceives of itself as a science, and that this makes it permissible to take stock of the discipline by evaluating these two theoretical works. I argued in Chapter Two that Eco's conception of science is incoher-

ent, and that a more meaningful assessment of his work can be obtained by evaluating it not in his terms but in those of a standard fallibilist philosophy of science. In Chapter Three I tried to show that Eco's use of symbolic notations and diagrams is seriously flawed and lends an appearance of precision to his work which is not warranted, since he uses these formal devices in ways which vitiate the virtues they have the potential to yield. Finally, in Chapter Four, I showed that Eco's Model Q, the central doctrine of his theory of codes, is based on a misreading of Eco's predecessor, entails absurdities, and finally has no empirical content in its intended role as a semantic theory.[45]

If these observations are well taken, I believe I have made a strong case for the claim I advanced in the Introduction. I have shown that Eco's work falls short in many ways of the fallibilist criteria for the scientificity of theoretical work. Since the argument I am opposing takes the scientificity of what I have been calling "scientific semiotics" as a premise, the argument does not go through if this is shown to be false. Insofar as Eco can be taken to be typical of scientific semiotics (which, I have tried to show, is to a high degree), I believe I have shown that it is in fact false.

If we grant that the failure of scientific semiotics cannot be put down to its having been a science (since it was not a proper science), another question arises about

the real causes of its failure. This can be taken two ways: first as a question about whether it was in fact its unscientificity and not perhaps some other factor which did in actual semiotics in and second as a question about the nature of this unscientificity. The latter is an empirical matter and one which would, I believe, prove a rewarding object for research in the sociology and history of science. On the other hand the former can, I believe, be answered a priori provided that we are allowed a reasonable and somewhat conventional premise. The premise I have in mind is a key ingredient of the modern world view: a neutral monist ontology. Given a neutral monism or in fact any monism dualistic *Geisteswissenschaftlich* arguments for the inapplicability of the scientific method to human affairs fail (cf. e.g. Bunge, *Life* 120-23) and there remain only practical difficulties, however substantial in the case of the human and social sciences. I can therefore answer a slightly modified form of the question in the affirmative: unscientificity is sufficient for the failure of a branch of human or social inquiry which aims at reproducible and communicable knowledge. It is not, of course, necessary: a science can fail due to financial neglect, ideological obstinacy, boredom, simple bad luck, political disfavour, or any of a host of other factors. We have then in its unscientificity, a sufficient cause for the failure of scientific semiotics. Although other (institutional and political) factors are likely to have con-

tributed to the rapidity of [illegible] decline we need [illegible] further for an explanation

5 2 Outlook

In his Introduction to the two volumes of his Treatise on Basic Philosophy devoted to the philosophy of science and technology Bunge discusses the potential usefulness of the philosophy of science and technology to scientists (and technologists pointing out that methodological self-awareness can help by

(a) digging up the philosophical presuppositions of ideas and practices

(b) elucidating and systematizing key philosophical concepts occurring in S & T (= science and technology)

(c) helping solve some scientific-philosophical problems, such as those of the nature of matter life mind, society culture and language

(d) proposing rational reconstructions of research projects, scientific theories or plans of action

(e) helping design S & T policies and plans (F[illegible] *mal* 7)

The importance of the philosophy of science is even greater in hatchling sciences like semiotics In these disciplines, which lack a history of intensive inquiry carried on by many, and have therefore not yet evolved the internal standards of communicability and testability of

research shared by other fields it cannot be taken for granted that the mechanisms of everyday research will promote methodologically adequate work The possibility of being seriously wrong is therefore great In such cases the discipline must absorb a high degree of inquiry from outside [illegible] since only disputed work can result if mine is [illegible] adopted If this is done unreflectively, an assumption that a reputed (often positivist or skeptical) [illegible] inquiry are likely to be adopted This appears to have been the case of Eco's 'scientific' strain in semiotics which seems to have adopted an uneasy hodgepodge of [illegible] empiricism and rationalism flavoured with a touch of phenomenology all this under the rubric of "scientific method" (cf for instance Eco's remarks on Occam's Razor [illegible])

It seems quite clear that a greater attention to the philosophy of science on Eco's part could have saved both his followers and his critics considerable trouble, and avoided contributing to what seems to be the increasing (and in my opinion of course undeserved) disrepute of scientific method in the social sciences [46] A genuinely scientific semiotics may well be possible Since the foundation of a new science requires the simultaneous invention of a set of concepts and the theories in which they figure, this will not be an easy task and may well require another Newton A necessary precondition for pursuing this aim, however, would be the renunciation of excessively easy

solutions like Eco's and of the belief that an empirical science can be created by fiat. It remains to be seen whether workers in semiotics will take up this challenge.

Notes

[illegible] Under the heading "scientific semiotics" I include [illegible] works devoted to the Peircean project of the [illegible] science of the sign [illegible] and aspiring to [illegible] exactitude of the natural sciences. This [illegible] for instance Kristeva's 'sémanalyse' which is designed rather as a hermeneutics than as a science. In what follows, the unqualified term "semiotics" should be taken in the sense of 'scientific semiotics'.

[illegible] examples in the critical literature on Eco [illegible] these aspects of Eco's preeminent status in semiotics [illegible], see Chapter One below.

[illegible] An informal survey of a sample of four issues of Semiotica [illegible] between 1978 and 1987 (issues 24.1/2, 33.1/2, [illegible] 1/2, and 63.1/2) found that of a total of 33 full-length articles with bibliographies (short reviews, bibliographic essays, articles with no bibliography, and articles by Eco himself excluded) 16 cited one or more works by Eco. Of these 16, seven cited *A Theory of Semiotics*. No other single work was cited as often in these 33 articles.

4 As distinct from the philosophy of linguistics, a

branch of the philosophy of science (see the introduction to Ware, *The Philosophy of Linguistics*).

5 Pawlowski gives a short list of works by members of the Warsaw school on the topic of semantic antinomies and argues that some of these authors do not concern themselves exclusively with issues of metalogic. As these works have not been translated, they remain unavailable to those who do not read Polish.

6 Of the cautions expressed at *PSA* '80 where Eco writes of the sort of indeterminacy principle which pertains to scientific research (cf. Chapter Two below) for a discussion of this sort of limitation on the scope of science.

7 In the following account of the genesis and substance of the sophisticated fallibilist view I follow closely, though in a summary form, the exposition and terminology of its greatest proponent Lakatos (*Methodology* 10–47 and 102–20).

8 For a survey of the range of non realist positions on this issue, especially those most popular among social workers in the social and human sciences, see the Introduction to George Levine's *One Culture: Essays in Science and Literature*. Levine, although apparently siding with the

relativists concede that realism remains the majority [illegible]

[illegible] *W*[illegible] discussion of the authoritarian [illegible] world [illegible]. This is [illegible] of the three types [illegible] holist, the others being the mystical [illegible] and the [illegible] rational.

[illegible] with Randall has written of science as an [illegible] of the intellect" (Randall 2[illegible]). Although I think this is clearly not a satisfactory definition of science, it does capture an important feature of the scientific method. [illegible] Lakatos *Methodology* 30.

[illegible] I mean philosophers like Michael Polanyi and in [illegible] later work Paul Feyerabend (Feyerabend was a staunch [illegible] for much of his career) as well as the many [illegible] of Thomas Kuhn. Kuhn himself has specifically disowned the view, which still bears his name more often than that of any other, that scientific knowledge can be fully accounted for in sociological terms; cf. the "Post-script--1969" to the second edition of his *Structure of Scientific Revolutions* (205-7). I include also sociologists working in the so-called 'sociology of knowledge' (better but less often named the "sociology of belief"), notably Barry Barnes and David Bloor.

12. The years of publication of [illegible] and [illegible] are respectively 1976 and 1984. The survey [illegible] all items listed in the following sources: *Abstracts of English Studies*, *American Humanities Index*, *Arts and Humanities Citation Index*, *Bibliographie de la philosophie*, *Bibliographie de linguistique*, *British Humanities Index*, *Bibliography of Philosophy*, *Bulletin signalétique 523 (Histoire et science de la littérature)*, *Bulletin signalétique 524 (Sciences du langage)*, *Canadian Literature Index*, *Communications Abstracts*, *Current Contents: Arts*, *Current Contents: Social Sciences*, *Humanities Index*, *International Bibliography of Social Science*, *Linguistics and Language Behaviour Abstracts*, *MLA Index*, *Philosopher's Index*, *Social Science Citation Index*, and *Year's Work in Modern Language Studies*. Five of the 29 articles found proved unobtainable given my limitations of time, and they are not included in the survey that follows.

13. Cf. Bunge *Understanding*: "In order to speak literally about a field there must be a field theory with definite and testable field equations" (32).

14. Livingston notes that this sort of outdated construal of science is prevalent in the attempts of the human and social sciences to come to terms with the dominant, which is to say scientific, modern form of knowledge (*Literary Knowledge* 22-23), and Rom Harré (in his "Introduc-

tion" to Knorr-Cetina's *The Manufacture of Knowledge*) has noted that much work in the sociology of knowledge has been characterized by an almost unacknowledged adherence to an old, and indeed exploded philosophy of science instrumentalism. [...]

[...] Chalmers adopts Bohr's philosophy of physics without discussion although Bohr's Copenhagen Interpretation of quantum mechanics was never widely accepted by philosophers of science and has now fallen even further in repute. Lakatos writes that "the Copenhagen Interpretation became one of the standard-bearers of philosophical obscurantism. Bohr's notorious 'complementarity principle' enthroned inconsistency as a basic ultimate feature of nature and merged subjectivist positivism and ontological dialectic and even ordinary language philosophy in one unholy alliance. This led to a defeat of reason in modern physics and to an anarchist cult of incomprehensible chaos. Einstein protested. 'The Heisenberg-Bohr tranquilizing philosophy--or religion?--is so delicately contrived that for the time being, it provides a gentle pillow for the true believer'." (*Methodology* 59-60). Cf. Michael Redhead's *Incompleteness, Non-locality and Realism* for a defense of a realist philosophy of quantum mechanics, and Bunge *Understanding* 84 for a critical discussion of the subjectivist interpretations of QM

Mick cites Fritjof Capra's popular work *The Tao of Physics* as an authority regarding the modern-day disrepute of the "Received View." Capra argues that modern physics demands a relativist epistemology. See my "Quantum Mechanics Appeal" for an analysis of the cognitive value and sociological implications of the appropriate mode of argument in the social sciences and humanities, and of Bunge (*Scientific Materialism*) for detailed discussion of the specific epistemological claim, concluding that "[e]very formula of the quantum theory refers exclusively to physical entities, none to knowing subjects" (150-51).

16 Again, this talk about "absolute form[s] of knowledge" seems to indicate that De Lauretis takes an obsolete justificationism to be the only possible philosophy of science and therefore chooses the relativist or sceptical deconstructionist alternative. Fallibilists also talk about the renunciation of the aspiration to absolute knowledge; they do not, however, represent this as an apocalyptic revelation. Cf. Lakatos' Introduction to *Methodology* for a conjecture that the demand for absolute certainty is a holdover from the epistemological criteria of 17th century theology, and see Popper's *Conjectures* for an argument that the social and human sciences are still in reaction against what was for 200 years the "dogma of almost stupefying power" of Newtonian physics (191).

17 A few other works which discuss scientific semiotic but not Eco in the terms of the philosophy of science, will be introduced in the course of the discussion in later chapters.

18 Cf Bunge's argument that grammars are not theories (*Life* 147-48). For a contrasting view, cf also Chomsky's discussion of the relationship between grammars and general theories of grammar "Methodological Preliminaries" 88ff (in Katz ed), and *Aspects* Chapter One.

19 Cf Rudner 4-5 and Nagel 9 for discussions of the distinction.

20 Cf Bunge *Life*, Chapter 4, Section 1, for a survey and rebuttal of the idealist arguments that the *Geisteswissenschaften* differ essentially from the natural sciences.

21. Other questions might also be asked about Eco's list of "methodological tools," which there is no space to go into here. One might ask by what means the items on Eco's list were included while others were excluded. One might also ask about the internal coherence of the list.

22 Lakatos follows Popper in defining verisimilitude as truth-content minus falsity-content: cf. Popper's *Con-*

jectures and Refutations* chapter [illegible] and Lakatos *Methodology* 101

23 Cf Wallace 116ff for a discussion of the advantages of formal language over ordinary language for the purposes of scientific theory construction and testing

24 Partial formalization should not be confused with formalization of only some parts of a theoretical work. The former refers to a kind of incomplete or half-way formalization, rather than to full formalization of certain portions of the work

25 The interpretation of a calculus is accomplished by means of a system of semantic rules taking the form of *reduction statements*. Chains of reduction statements connect theoretical terms to observables Cf Bunge *Formal* 20 and Wallace 109ff, who shows that the reduction statements have the form of an auxiliary theory Note that this is quite compatible with the theory-ladenness of observational terms.

26 This apparent belief of Eco's that diagrams can support only one interpretation (which therefore need not be spelled out) is somewhat surprising in light of his discussions of the polysemy of visual signs (e g *TS* 247) It can perhaps be explained as an aspect of a form of essentialism It might be argued that Eco believes (appro

priately "scientific") diagrams communicate the essences of concepts directly.

Other features of Eco's work which suggest a latent essentialism include his notion of definition, his almost instinctively etymological approach to the meanings of theoretical terms, and his apparent belief that posited (theoretical) entities pre-exist theories (cf. Chapter Two above). I have space here only for some brief remarks about the first of these subjects.

Eco writes that "a general semiotic theory will be considered powerful according to its capacity for offering an appropriate formal definition for every sort of sign-function" (*TS* 5). At another point he writes of a phenomenon which "a theory of codes can define but cannot structurally reproduce by means of a finite model" (*TS* 57), and elsewhere again asks how a certain kind of "interpretation" can be "semiotically defined" (*TS* 130). These are ill-formed speculations. If the phenomenon in question is a specified kind of something it has necessarily *already been defined*, presumably by a concealed component of Eco's own theory. Definitions are assignments of theoretical terms to classes of observables or other theoretical terms, and they yield no new knowledge (Popper, *Conjectures* 20). *Terms* are defined, not phenomena; phenomena are specified with the help of defined terms. A question about whether a phenomenon can be defined must be asked by an essentialist, since

only an essentialist believes she can grasp phenomena before applying terms to them, and that therefore the question of whether adequate terms can be found is moot.

Popper has described a doctrine he calls "Aristotelian essentialism": the belief that "a definition is a statement of the inherent essence or nature of a thing," and that "definitions are principles," that is to say they cannot be derived from other propositions "and] thus form the basis of every science" (*Conjectures* 30). As we have seen in Chapter Two above, Eco holds the very closely related belief that every science has a prelinguistic core of "posits."

27. If what Eco says about these symbols is to be construed as interpretation, "rhetorical equivalence," "metonymical substitution," and "metaphorical substitution" must be construed either as observables, or as low-level theoretical terms with associated observable indicators (Pawlowski 185ff, Wallace 106ff). Eco gives neither a means of recognizing these as observables nor any indicators.

The more charitable approach here is simply to overlook Eco's omission. Failing this we must conclude that Eco's symbolism is even more poorly specified than otherwise.

28. Cf. Chapter Four below for a discussion of some other flaws in Eco's appropriation of Quillian's model of a

semantic memory as his "Model Q" of "infinite semantic recursivity" (*TS* 121).

29. In what follows, I shall use the term "Model Q" to refer to Eco's appropriation of the work of Quillian and not to Quillian's work itself.

30. In its own discipline where it is considered a relatively minor work, Quillian's article on semantic memory, from which Eco derives his Model Q and upon which he builds much of his semiotic theory, never achieved anything approaching the degree of prominence that Eco gives it.

31. Cf. Chapter Three above for discussion of this diagram, and of Eco's failure to specify the interpretation of Quillian's formalism.

32. Note the confusion displayed by Eco's use of terms such as "unlimited" interchangeably with "infinite": arbitrarily large numbers are still finite.

33. Cf. Chomsky *Aspects* 4 for the standard account of the relation of competence to performance.

34. We can speak of physical size here, despite the fact that competence is an abstract informational object with no physical size, since the competence must be embodied in a physical brain in which the storage of each bit

of information requires a physical medium of some finite non-zero size

35 For convenience I will use the terms 'I-accessible' and "I-accessibility" to denote the relation of accessibility by traversing an arbitrary number of nodes in the network even when discussing Quillian's version of the network, although Quillian does not make use of the Peircean concept of interpretant, using instead the much more precise concept of associative link

36. Note that this does not follow in Quillian's version of the model in which the meaning of a node (its "full concept") is given by two abstract objects (the set of nodes accessible from it, and the structure of relations holding between the members of this set) rather than directly in terms of other nodes. This solution is not available to Eco, since he has abolished the distinction between token- and type-nodes, and has reduced all relations between nodes to the interpretant relation

37. But see the section on the empirical content of Model Q below an argument to the effect that the empirical content of Model Q was already nearly zero. This is foregrounded in Eco's later formulation.

38 At any rate Eco would have to claim that no predictions can be based on the infinite network, since

full knowledge of it would be impossible for a finite mind. I shall return to this point below.

39. This also completes the argument earlier in this chapter to the effect that Eco's version of the network is totally homogeneous, since he gives no reason why the interpretant relation should not hold between any and every pair of nodes. We have just seen good reason to think that the interpretant relation does in fact hold between any term and every entity.

40. It should be noted that this usage of the term "empirical content" is different from that of Bunge and corresponds to Bunge's "factual content." Bunge associates the term "empirical content" with the false (radical empiricist) claim that theories are only summaries of the data.

41. Lakatos' criterion of excess empirical content over its predecessors is an outgrowth of the deductive-nomological account of explanation due to Hempel and Popper. Bunge terms this mere "subsumption under a theory," and argues for a stronger account of explanation which does not "ignore [ ] the mechanisms of things" (*Understanding* 22-23). Since Bunge's criterion is strictly stronger than Lakatos' (it requires of a theory everything which Lakatos' does, and adds further requirements), my

argument would be unaffected by the adoption of Bunge's view of explanation.

42 This is a stronger assertion than that Model Q is false. A false statement can still have empirical content. Popper's classical example is that, when uttered on Monday, the statement "It is Tuesday," although false, still entails the true empirical statements "It is not Wednesday," "It is not Thursday," and so on (*Conjectures* 392). A statement with no empirical content entails no empirical statements at all.

43 Cf. Bunge *Understanding*: "anything that purports to explain everything actually explains nothing" (19).

44 Cf. Bunge *Life* (122-23) for a summary of the fairly conclusive arguments against this view.

45 The absurdities entailed are statements about psychology: these are what save Model Q from the charge of total vacuity, and produce the qualification "as a semantic theory" at the end of the last sentence of this paragraph.

46 In recent years the reputation of science in the social sciences has fallen so low that a spate of books and papers attempting a rescue have been elicited. For a thorough discussion of the general arguments cf. Papineau's *For Science in the Social Sciences*, and for the application

of these to semiotics and related disciplines, especially Chomsky's *Rules and Representations* and Livingston's *Literary Knowledge Humanistic Inquiry and the Philosophy of Science*

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