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REALISM, RELATIVISM AND REFERENCE

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Dedicated to

my father, Vasilios, and my mother, Demetra, .

for there is no possible world containing this work

and not containing them together with their many qualities,

a modality for which I am deeply grateful.

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(Order No.)

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The central thesis of this dissertation is that the recently proposed Causal Theories of Reference by K. Donnellan, H. Putnam, and especially S. Kripke provide support for Scientific Realism as developed in the theories of J.J.C. Smart, H. Putnam, and especially R. Boyd, on the face of the most serious challenge to scientific objectivity contained in the writings of the Relativists S. Toulmin, N.R. Hanson, T.S. Kuhn, and P. Feyerabend. I have argued accordingly that all the relativistic arguments are either weak or are reducible to a strong formal argument about the reference of scientific terms and the categoricity of scientific systems. This argument is shown to be even stronger because of the important role that reference plays in the objectivity of science. I proceed to show then that the employment of this argument by the Relativists rests on the Traditional Theory of Reference. In the final part, I argue that the Traditional Theory of Reference has become untenable, both on account of internal difficulties and because of advances in Modal Logic that show some of its basic tenets to be erroneous. In its place a new theory has been proposed by Kripke, the Causal Theory of Reference which avoids the internal difficulties of the

Traditional Theory and is sensitive to modal distinctions. My conclusion is that the new theory of reference offers not only negative support to Scientific Realism, by undercutting the argument of its most serious challenger, but provides positive support as well by way of foundation and clarification of its major tenets. As a result a new conception of science is beginning to emerge, in which the notions of necessity, cause and essential property play a key role.

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INTRODUCTION

The Problem, Its Origin and Its Significance

The central thesis of this dissertation is that the recently proposed Causal Theories of Reference (Donnellan 1966, 1974, Putnam 1970, 1973, 1975 and especially Kripke 1971, 1973) provide support for Scientific Realism (Smart 1963, Putnam 1976 ms, 1979, and especially Boyd 1975 ms) on the face of the most serious challenge to scientific objectivity contained in the writings of the Relativists S. Toulmin, N.R. Hanson, T.S. Kuhn, and P. Feyerabend. I have argued accordingly that all the relativistic arguments are either weak or are reducible to a strong formal argument about the reference of scientific terms and the categoricity of scientific systems. This argument is shown to be even stronger because of the important role that reference plays in the objectivity of science. I proceed to show then that the employment of this argument by the Relativists rests on the Traditional Theory of Reference. In the final part I argue that the Traditional Theory of Reference has become untenable, both on account of internal difficulties and because of advances in Modal Logic that show some of its basic tenets to be erroneous. In its place a new theory has been proposed by Kripke, the Causal Theory of Reference which avoids the internal difficulties of the Traditional Theory and is sensitive to modal distinctions. My conclusion is that the new theory of reference offers not only

negative support to Scientific Realism, by undercutting the argument of its most serious challenger, but provides positive support as well by way of foundation and clarification of its major tenets. As a result a new conception of science is beginning to emerge, in which the notions of necessity, cause and essential property play a key role.

The investigation of this problem was undertaken in the belief that it is a most significant problem involving the combined fields of Epistemology, Ontology, Philosophy of Logic, Philosophy of Science, and Philosophy of Language. I believe moreover that since its focus is on the question of the objectivity of human knowledge, it has also repercussions for other fields of philosophy such as philosophy of mind, ethics, and aesthetics. Apart from these wide implications, however, the advantage of studying the problem of objectivity in this formulation is that it gives us the opportunity to critically examine three general theories that occupy a central spot in the frontiers of contemporary philosophical research.

Speaking from a more personal view point, my main motivation for undertaking this inquiry was the need to resolve a conflict between two philosophical stands. On the one hand, after many philosophical wanderings I came to the realization that my most firm and honest beliefs, in thought and in action, could be identified as Scientific Realism, not just as an abstract endorsement of the rationality of scientific practices but as the grounds for a progres-

sively accurate description of the world. It is this belief, serving as the ground for an intellectual commitment to philosophy and science as one unified truth-seeking enterprise, that was seriously challenged by the recent Relativistic Movement. Despite the latter's exaggerated claims, that were checked early by other inquirers far more competent than myself, I took the Relativistic Position as a serious and well established movement with secure foundations that traced their origins to important philosophers and scientists of the 19th and 20th centuries. This rich tradition, represented in our times by W.V.O. Quine, could not be repudiated without serious thought and argument. Of course, the recent Relativists indeed turned against the spirit of their philosophical precursors: they attempted to show that the objectivistic positivist analysis of science, based on extensional logic and appeal to purely observational foundations, was erroneous. Their conclusions however were extended beyond this initial critique to involve a full scale attack on objectivism. The last step of this escalation is the advocacy of methodological anarchism in science by Feyerabend. As a Realist, while agreeing partly with the first part, the critique of verificationism and the return to a view of science as ontological, I found the general attack on the objectivity of science unacceptable. My efforts have been to find the basis of the relativistic argument and to investigate its plausibility.

Parallel to my concern to investigate my basic commitments with a view to clarifying and defending them, in the face of serious challenge, was another concern. I wanted to study the problem of reference as a way of understanding philosophy of language, a dominant topic in recent philosophy, which is presented as an autonomous (and rather "rootless") field. My investigations have led me to the recognition of the rich connections between philosophy of language and philosophy of science and to the belief that the former cannot be studied apart from the latter.

A. The Components of Our Problem and Their Historical Origin

As I have formulated it, the problem of objectivity is the result of the coming together of three problematic strands: (i) The problem of reference, the problem that is, of the relations between word (or concept, in other formulations) and object. (iii) The Theory of Scientific Realism as an apology for scientific knowledge, which goes beyond the methodological claim of objectivity and argues that some scientific theories are true. (iii) The Relativistic Challenge as the most recent part of a long systematic sceptical tradition which claims that there are good, formal, and informal arguments to show that there are important and insurmountable limitations to the attempts of science to determine the nature of reality in a systematic way, and as a result a strong belief in science

or in the rationality of its practices, as a privileged road to truth is unfounded.

The historical origins of my problem then are traceable to Kant, for it is in his theory that the three problematic strands come together for the first time explicitly. Kant takes his task as an apology for certain scientific theories held at the time (Euclidean Geometry and Newtonian Mechanics), on the face of radical systematic scepticism (contained in the writings of Hume). He also formulated his problem as one of reference [Letter to M. Hertz, Feb. 21, 1771] and the solution he offers to the problem in the Metaphysical Deduction of the First Critique, is through reference. Objecthood, and thus objectivity, is determined by the categories which are nothing but the conceptual counterparts of the logical constants of judgment together with some modal indicators taken as statement operators applying formally to the way a statement is asserted. The significance of this move is that it makes reference to objects a result of the synthetic activity of judging. Ironically enough, this idea, popularly known as Kant's Copernican Revolution, helped launch a new tradition of Relativistic Arguments. Especially as the theories that Kant took to be as absolutely true (necessary and universal), Euclid's and Newton's, came under criticism on foundational grounds.

After Kant the three problematic strands are pursued, either separately or two at a time, by the thinkers that followed him. The defense of science passed into the hands of the philosophers-scientists of the XIX Century, who, on the face of the aforementioned developments, re-examined the nature of scientific knowledge and showed the Kantian account of science as synthetic a priori knowledge was incorrect. This line of thinkers starts with Frege in Mathematics and Mach in Physics, and proceeds through the works of thinkers like Peano, Russell, Poincaré, Hilbert, Duhem, and Weyl to the philosophers of the Vienna Circle, who undertake a systematic investigation of the nature of scientific knowledge and a conservative defense of its objectivity that rests, on the one hand, on the validity of its formal structure and, on the other hand, on the empirical groundedness of some of its material claims.

Parallel to the above strand, and often intertwined with it even in the writings of single thinkers, we find post-Kantian scepticism, which is now not a blanket empiricistic scepticism, but a formal result of the aforementioned systematic investigations. This respectable scepticism tries to formally reveal the limits of the scientific determination of reality. This tendency, downplayed in the work of some thinkers is clear in the writings of Poincaré and Weyl.

Finally, the problem of reference became explicitly a problem in its own right, due to Frege's advances in Logic. Its philosophical

import is clearly recognized in the Frege-Hilbert Controversy over the foundations of Geometry, as well as in the Logical Atomism of Russell and Wittgenstein.

Of all the philosophical works of this century, Wittgenstein's Tractatus comes closer to capturing the problematique of Kant as well as some of the spirit of the Kantian solution. Wittgenstein used the Theory of Descriptions that Russell proposed in 1905 as a general theory of reference that could serve as the basis for a theory which could at the same time delimit human knowledge and defend the objectivity of some parts of scientific knowledge. What followed this time was even more ironic, since it is Wittgenstein himself who overturns his previous position and moves in the direction of Relativism. The simple correspondence theory of meaning that recognizes its holistic (and thus also its social) character. As a corollary of the above consideration, semantics together with other human sciences are shown to be indeterminate.

It is to this Wittgensteinian critique that the Relativistic Movement of the Fifties and the Sixties returns (Hanson, Toulmin) and it is here that our problem begins.

B. The Present Problem and Its Significance

Wittgensteinian and other types of holism were both the inspiration and the theoretical basis of the Relativists. A second

important factor that explains their movement by providing their program, is the opposition to the Positivist Philosophy of Science. Using standard extensional logic and some much contested semantics about the empirical groundedness of scientific claims, the Positivists attempted a "stripped-down-to-the-hull" objectivism. Admittedly confirmation, a key concept in the Positivist Philosophy of Science, was problematic, but here a good theory of probability was expected to fill the gap.

The Relativists noted that not only confirmation is limited by logic, and falsification by the systematic nature of scientific knowledge, but even a loose, non-formal idea of testing against experience was suspect on epistemological and semantic grounds. They argued that cognitively significant observations can only be theory-laden. This undercuts the Realist tenet of a cognitively independent reality against which theories can be tested, even inadequately.

The second important step in the Relativistic Challenge was aimed against the Nomologico-deductive Model of Explanation. The D-N Model, logically indistinguishable from prediction, could only be used in trivial cases. Real scientific explanations involve a totality of metaphysical beliefs: they are, to use Toulmin's phrase, "new ways of seeing old regularities." Thus two theories cannot be compared as explanatory vs. non-explanatory, even in cases where the one was abandoned for the other. Theories simply

differ in what they consider worthy of explanation, and this is a matter of total metaphysical outlook, in much the same way that anomalies are system-relative.

The above two steps place scientific communication, and scientific rationality together with it, under doubt. The ideal of unambiguous communication based on rigorous definitions and clear statement of primitive terms was shown to be unfulfillable even on the level of observation sentences. This demotes the natural sciences to the level of the human sciences. Understanding a theory cannot be spelled out by a series of clear definitions; we have to settle for the vague idea of "sharing an outlook."

The Relativists were also able to explain away all appearances to the contrary by a careful examination of the history of science. Here Kuhn's Theory of normal science as puzzle-solving, and paradigm-changes as revolutions, was the key. Actual scientific practices were shown to be a lot more political and a lot less abstractly rational than they were claimed to be.

Kuhn's Theory also had implications for another tenet of Realism: the idea of truth-by-approximation that was supported by the idea of cumulative progress in science. The history of science, according to Kuhn, was not a smooth process of accumulation guided by rational choice, but a history of revolutions and overthrows of one paradigm by another. Since paradigms differ in total outlook they are incommensurate, and there can be no rational choice between them:

only conversion, based on rhetoric and politics, that eventually results in a general "Gestalt-switch."

All the demythologization of science as a rational institution also led to the stripping of its title as a democratic institution, a characteristic stressed by the Pragmatists. The idea that the method of science remains "shock-protected" while theories are replaced by one another (as are governments in a democracy) is shown to be only part of the general myth. According to Feyerabend the only normative lesson that the history of science gives us is that of methodological anarchism.

C. Possible Responses to the Problem

There can be four kinds of responses to the Relativistic Challenge. The first is outright rejection. The unacceptable conclusions that the Relativists were led to can be used as a reductio proof against their theoretical bases. This is a facile response but it points to an important consideration: the Relativists owe to us an explanation of the success of science.

The second response is conservative and piecemeal. It distinguishes the normative character of the Positivistic Philosophy of Science and argues that the Relativists have taken too far certain truths about the actual scientific practices. A more careful evaluation of many scientific practices will show them to conform to the normative positivistic methodology, with the necessary

allowances for any application of normative rules. As for scientific discovery, an account of it is surely desirable but it should not be allowed to overshadow the account of justification at hand. The weakness of this position is that it treats Relativism not as a strong unified position with a long historical tradition, and, as a result, it does not take the Relativists at their strongest. Thus, even if these responses are partly successful, at best they amount to a return to the Positivistic Philosophy of Science coupled with theoretical support from theories like Naturalism or Pragmatism. Such theoretical mergings cannot be easily dismissed as they retain much of the rigor of the positivist analysis. At the same time however, the force of the best relativistic arguments is left intact. The best known example here is that of Quine whose position, depending on the interpretation, can be read alternatively as providing the best argument the Relativists ever had, or as the most careful reformulation of the Positivistic account of science, that utilizes both Naturalism and Pragmatism. The other two examples here (Scheffler 1967, Shimony 1970, 1975) are not ambiguous in their commitment to Positivistic Philosophy of Science, to Pragmatism or Naturalism, as well as in their opposition to Relativism.

When we release ourselves from the Positivistic Philosophy of Science, two responses become immediately available. The first

accepts the serious part of the Relativistic Argument (i.e., not methodological anarchism) and tries to revise Philosophy of Science accordingly. The task is difficult because the holistic import of the Relativistic Argument requires that we find a suitable unit of analysis that falls outside what we already have from formal extensional logic. Proposals here abound: "Paradigms" of Kuhn, Goodman's "entrenchment", Lakatos' "Research Programs", Laudan's "Research Traditions", McIntyre's "Narratives", H.I. Brown's "pre-suppositions."

The final response to Relativism comes from Realism. Realism claims that the critique of the Positivist Philosophy of Science, and the critique of verification does not lead one automatically to Relativism. It is indeed possible to criticize the Verificationist Methodology from a Realist standpoint. It is unfortunate however, that many Scientific Realists limit themselves to a critique of Positivism (Smart, Sellars, and up to a point Boyd) and in this way leave the road to Relativism open. What they usually have to offer by way of a positive argument is that Realism is the best explanation for the success of science, while Positivism makes the success of science almost a miracle. What they have missed is that in their critique of Positivist Methodology there are ideas that can be used positively to establish a viable Realistic Methodology, which has been shown by Boyd and Harré, and negatively

to defeat the Relativistic alternative which has been recognized by Putnam in his attack on Conventionalism. Both of these options are not available, however, unless one takes Realism not only as a best explanation, but also as a Theory of Reference, and it is here that the Theories of Putnam and Kripke become relevant.

The kind of Realism that I propose in this dissertation is neither a social theory about the behavior of scientists, nor a best explanation argument for the success of science, but combines Boyd's observations about the ontological component in scientific methodology with the Causal Theory of Reference proposed by Kripke. In terms of exposition, this kind of Scientific Realism proceeds by noticing the significance of ontology and reference for science. Then it examines the arguments of the Relativists as its chief rival among theories that take science to be ontological. Its main argument here is that the Relativistic Arguments are based on an erroneous theory of reference, the TTR. The next step is to replace the TTR by a more accurate account of reference and trace the implications of the latter for our conception of science.

The core of our argument then, centers around the problem of reference. We have promised to show that the Relativistic Arguments are in fact reducible to one major argument which is formally correct; the use of the argument, however, in matters of science requires a theory of reference of scientific terms and theories,

which in view of modern developments appears erroneous. These same developments open the way for a new theory of reference which can be shown to be in support of Scientific Realism.

This preliminary exposition has left untouched two points of contention. In the first place I have not defined Scientific Realism explicitly, even though I have on occasion mentioned some of its basic tenets. This omission was purposeful, because the name 'Realism' has been used to describe so many different theories or aspects of theories that one cannot help thinking that it serves more as a regulative principle in philosophical theory-making than as a name for a concrete theory. An important aim of this dissertation is to define and circumscribe a theory of Scientific Realism, and this aim will be fulfilled throughout the essay. In the first chapter I will deal with the relations between Realism and the Correspondence Theory of Truth.

The second point is of even more immediate concern. At this point of our inquiry, one cannot help wondering about the central role that we have assigned to reference. Even if one accepts the claim about the relations between Relativism and the Traditional Theory of Reference, and the relations between Realism and the Causal Theory of Reference, still one may object to this whole enterprise on the ground that the topic of reference is too formal and too peripheral to be of such importance to the scientific endeavor.

In our approach reference is treated like the Key-stone whose removal sends the whole edifice of Relativism collapsing. We shall begin then by examining the importance of reference for science. Once this becomes clear, both the Relativistic Argument and our counter-argument will become relevant.

CHAPTER I

SCIENCE, CORRESPONDENCE, AND ONTOLOGY

Why and in what way does reference matter to science? I will eventually support the thesis that reference matters to science because, and to the extent that, science is ontological. Yet before proceeding to establish and explain this thesis in the second part of this chapter, in this first part I will examine a somewhat different but often-heard answer. According to this alternative answer, reference matters to science only to the extent that science is aiming at truth; given that any correspondence theory of truth explains truth ultimately by appeal to reference, the connections between science and reference are easily established. This approach has become especially favorite recently because of the heavy endorsement that Tarski's Theory of Truth has received, if not as a correspondence theory, at least as capturing whatever is significant about any correspondence theory of truth.

My reasons for approaching the topic by a detour via Tarski's Theory of Truth are three-fold. In the first place, I hope that the detour will make my thesis clearer by bringing in focus a contrast between two conceptions of science that accompany the two different approaches. In the second place, I need to examine Tarski's Theory as well as the correspondence theories in general as a way of

clarifying Realism: according to many thinkers the common tenet of many different schools of Realism is the adherence to a correspondence theory of truth. Beyond this general tenet, however, thinkers disagree about the philosophical significance of Tarski's Theory, especially in regard to the problem of Scientific Realism. Does Tarski's Theory provide support for realism? Any careful account of Scientific Realism has to examine and evaluate this lead. Finally, it would be instructive to start this inquiry by considering two closely related views on Realism that have been recently proposed by two philosophers of science who have dealt with the problem for the last thirty years: Popper of the Objective Knowledge and Putnam of the John Locke Lectures. Contrary to these thinkers I shall maintain that while reference does figure primarily in science, it does so not by way of Tarski's Theory, but rather by way of the ontological aspect of science which is as important in the context of explanation as it is in the context of truth.

There are three arguments that can be brought against the thesis that reference is important to science as an explanans of truth by way of Tarski's Theory. In the first place, it is questionable whether Tarski's Theory is an explanation of truth. Secondly, even under a wide understanding of 'explanation' that would include Tarski's Theory, it is questionable whether the Tarskian account, or any such formal account of correspondence for that matter, could capture the notion of truth as it figures in science (as a truth-seeking activity). Finally, if the only way that reference figures in science

is by way of Tarski's Theory, then it is not of specific importance to science but to all human knowledge and discourse. To be sure, Tarski's Theory is applicable only to formalizable, semantically open, notationally complete systems, but except for a very few parts of science such systems are only artificially constructed, often in the context of exposition of Tarski's Theory. On the other hand, if we insist on a loose reading of Tarski's achievement, and go liberal on a few of his requirements, the Disquotation Criterion does not in any way single out science as its domain of application. In view of these obvious objections why would any thinker consider Tarski's Theory as the key to supporting Realism? Let us begin by concentrating our attention to Popper who came closest to such a Tarskian defense of Scientific Realism.

1.1 Popper's Realism and Tarski's Correspondence

Dedicated to Alfred Tarski, Popper's Objective Knowledge is a mature attempt to modify and place his (Popper's) previous theories within a serious general framework in philosophy of science and epistemology. Taking into account many of the recent developments in the field of History and Philosophy of Science, Popper undertakes to defend Realism, a task that would appear almost contradictory to his early falsificationism because even the most conservative versions of the former consider truth as not only the aim of science but also its achievement. Upon a closer examination however, the

two positions do not appear antithetical. This is due to a certain reading of Realism as well as to Popper's marked departure from previous stands. In his new position three elements clearly stand out. Contrary to his early absolute falsificationist claims, Popper tries to spell out a theory of verisimilitude, which is formally a lot clearer than his early idea of corroboration. Secondly, he acknowledges his debt to Tarski for making clear the correspondence notion of truth, and in this way allowing him (Popper) to defend a realistic position without "philosophical embarrassment." Thirdly, and probably in response to the aforementioned developments in the fields of History and Philosophy of Science, he seeks to reformulate the old epistemological problem (formulated explicitly by Kant) by proposing that the old mind-world dichotomy is misleading and in need of supplementation with a third "objective" reality, the world of developing ideas, the "Third World" as he calls it.

My aim is not to examine Popper's reformulation of the epistemological problem, but only the role that Tarski's Theory plays in it. It is nevertheless necessary to point out the connections between these three elements, otherwise Popper's reading of Tarski will not be obvious. Upon a first glance, the significance of the Third World of Ideas is that it supplements Popper's theories by acknowledging explanation to be a major aim of science, in addition, that is, to the truth-seeking of science. Specifically, this Third World of interconnected developing ideas replaces Popper's old model where any conjecture is or can be proposed as a hypothesis, on the

basis of unrealistic (pun intended) criteria like "boldness." Popper, therefore, recognizes that on a purely conceptual level theories are not just proposed; they are developed and the laws of their development cannot be captured by a simple conjecture-and-refutation model which allowed any and every theory to be proposed. This old model was formal, i.e., empty, in the sense that it was only truth-sensitive, but had little to do with explanation. This becomes clear in considering the problem of theory choice (or hypothesis choice). Popper's attempted analysis would either place it in the context of discovery (or conjecture) or would try to explain it by appeal to some aspects of the truth of theories.

The Third World is also proposed in the context of truth as an attempt to reformulate the problem of objectivity in a more complete way. One does wonder here whether this addition is in fact a "one-step-forward, three-steps backward" type of advance, since now we have to worry about three sets of relations rather than one as the old problem demanded. Presumably, this Third World instead of generating difficulties should operate as a missing link in the problem. Nevertheless, the obscurity inherent in any concept of an objective world of ideas makes the solution problematic.¹ On the side of Popper, we can say that his new idea makes us understand better why there is a problem. By generating a Third World, Popper characterizes knowledge as a special kind of object: not just another part of the objective world, nor just a series of happenings inside an organism. This allows him to discuss the other two worlds,

especially the mind as part of the natural world without falling into an egocentric or a semanticalist predicament. Yet semanticalism is not so easy to eliminate; at least it cannot be bypassed in the same way as the egocentric predicament is eliminated: by appeal, that is, to naturalism, to an evolutionary "epistemology without a knowing subject." Let us see why. The Third World of Popper, if it has any significance beyond the obvious metaphor, it is to be found in the connections that it has to Popper's ideas about rationality and criticism. In Popper's view that which makes our knowledge the subject of criticism - and makes us worry about objectivity - is a tacit recognition of the independence of the Third World. As he remarks in an eloquent passage:

...he (the scientist) is consciously critical of his theories which, for this reason, he tries to formulate sharply rather than vaguely. But the amoeba cannot be critical vis-a-vis its expectations or hypotheses; it cannot be critical because it cannot face its hypotheses: they are part of it. (Only objective knowledge is criticizable: subjective knowledge becomes criticizable only when it becomes objective. And it becomes objective when we say what we think; and even more so when we write it down or print it.) [1972. p. 25]

What is the philosophical import of this passage? In the first place it is leading to semanticalism: after all, what is it about this Third World that sets it apart from the others? certainly not just the fact of objectification: our activities and movements generate

a lot of objective changes in the world; we do not consider them as part of another world unless we take them as symbols: it is this way that our footprints become objects of reflection. Popper owes to us to clarify the semanticalism implied by his position.

Secondly the Third World, in the passage becomes an Ur-principle analogous to Kant's Transcendental Unity of Apperception: the "I think" that stands before every judgment, separating the judgment formulated from the activity of the thinking being. I do not mean to suggest that Popper is a transcendental idealist: the egocentricity of the Kantian "I think" principle is not the same as the semanticalism of the Third World of Popper; yet they are alike qua ultimate principles, and the passage quoted above makes this clear: we face our judgements, they are not just parts of us, for Popper we do not just think, we know (are aware of) that we think.

Principles of such generality and profundity may help us in "understanding" our problem, or our "predicament" ("the glory and agony of being human" and other such things); they may even, but not only at best, suggest in a vague way the aspects of the problem that may yield a solution. Yet they do not advance solutions. What we are left with in Popper's treatment are two considerations: a lurking semanticalism and the hope that this semanticalism will be eliminated by some proper account of truth. Popper as a matter of fact endorses such a hope. Realism in his view, is neither demonstrable nor refutable [1972, p. 38] but its plausibility is grounded

in the referential function of language: the simple fact that statements are about the world or of something in the world [1972 p. 41].

The way from the above considerations to Tarski's Theory is clear, yet before proceeding to discuss Popper's use of this theory, let us also say a few words about the connection between the Third World and the other important element in Objective Knowledge. As we emphasized before in the Third World, Popper pays respect to the development of theories out of other theories in science. This new orientation in his philosophy make the growth of scientific theories the main problem of Epistemology: "I see the problem of knowledge in a way different from my predecessors. Security and justification of claims to knowledge are not my problem. Instead by problem is the growth of knowledge." [1972 p. 37] In response to this new aim of epistemology Popper develops a formal theory of verisimilitude. This is a more clearly defined concept than corroboration: corroboration was based on the 'accidental' history of testing, while verisimilitude affords a clear criterion that may be applied to compare any two theories. This in turn would allow for a theory of scientific progress, and, of course, for Scientific Realism, since one of the basic tenets of traditional Scientific Realism is the belief that science approximates truth.

It is at this point that Popper introduces Tarski's Theory because his definition of verisimilitude, always according to Popper, is based on two of Tarski's ideas: (i) the notion of the truth-content of a statement (the consequence class) and (ii) the general

Tarskian Definition of Truth. In many ways then, Popper's later theory is based on Tarski's Theory of Truth. Verisimilitude determines the development of the Third World of ideas and becomes the aim of science as well. How accurate is the grounding of verisimilitude on the Tarskian Theory? The first idea, that of a truth-content is certainly used by Popper. Concerning Tarski's Definition, however, there are two caveats. For one thing, it is not used directly but in a roundabout way: according to Popper, Tarski made it possible to introduce truth as correspondence to facts. More importantly, however, there are serious misunderstandings in Popper's reading of Tarski. Let us take these matters one at a time after we have noted one positive aspect connecting Popper's ideas and Tarski's Theory: it has to do with the Third World of ideas. Implicit in the introduction of this Third World is the recognition that the problem of truth is a human problem of modest proportions; that is to say a problem which would accept a simple solution unclouded by psychological (Second World) or metaphysical (First World) considerations. Tarski's Theory is indeed a modest theory, in the sense that it does not attempt (and does not feel that it has to attempt) to uncover the nature of the world or the nature of the knowing mind before offering an account of what truth is.

Passing to the critique of Popper, the important question to ask first is, "Can Tarski's Theory of Truth ('TTT' from now on) provide what Popper expects of it?" Popper's reading of the TTT can be analyzed into the following three theses:

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Passing to the critique of Popper, the important question to ask first is, "Can Tarski's Theory of Truth ('TTT' from now on) provide what Popper expects of it?" Popper's reading of the TTT can be analyzed into the following three theses:

1. The TTT is a correspondence theory of truth, because it explains truth as correspondence with the facts.
2. The basic achievement of Tarski is the explanation of truth, an admittedly problematical semantic notion in non-semantic terms. Specifically Popper maintains that the TTT in effect allows us to eliminate all the semantic terms of the object language by reducing them to syntactical term in the meta-language.
3. Even though Realism (and its opposite, idealism) are, strictly speaking, irrefutable and indemonstrable, still Tarski's Theory lends support to it by establishing one of its basic tenets: a correspondence theory of truth, as correspondence to facts. This "rehabilitation" of the correspondence theory is, for Popper, Tarski's most important contribution. [1972, p 323]

How accurate is the above reading of Tarski? Even at a first glance, Popper's interpretation of Tarski's achievement is suspect on two counts. In the first place thesis 1 above runs against thesis 2, at least initially: how can a theory of truth be at one and the same time a correspondence theory and a purely syntactical theory? The whole point of correspondence theories is, presumably, that truth is not a matter of language alone: it is the relation of language to what is extra-linguistic. Thus Popper's claim has the paradoxical air of saying that Tarski explained correspondence to facts without ever talking about facts. The reading would have been more accurate

had Popper dropped the first 'facts' from the previous sentence, yet Popper is adamant on this score [1972 p. 44-46, also p. 323-327].

Secondly, and with respect to the third thesis above we have Tarski's own repeated claim that his theory is epistemologically neutral. Judging this latter point of course, cannot be done without discussing TTT and its claims to capture all that is significant to correspondence. This task will be undertaken in the next section (1.2). For the time let us concentrate of Popper's reading of the TTT. We saw that the TTT figures in verisimilitude only in a meta-theoretical level by making talk of truth acceptable (in case someone objected that any mention of 'truth' will lead to semantic paradoxes). Most importantly, it is an open question whether Tarski's Theory meets the requirements for a traditional correspondence theory; it can better be described as setting some formal limits for all correspondence theories. The important question here is why would Popper insist on such a reading of Tarski?

I believe that Popper's reading of the TTT is to be explained by appeal to his earlier theories. It is problems with his earlier theories that lead Popper to embrace the TTT and to tailor his reading of it accordingly. Specifically, this early theory rested entirely on falsification. Falsification in its turn was defined as disagreement with the facts. This rather commonsense notion had resisted analysis: Wittgenstein's account was rather obscure and the Vienna circle discussions in the Thirties between Neurath and Schlick were

inconclusive: as a result the concept of fact was philosophically suspect. Popper was therefore placed in the "funny" position of advocating an ultrarigoristic view of science which was based on a notion that was philosophically suspect. If facts were spelled out as observation sentences then the theory of truth would be entangled in the controversies surrounding such sentences. If they were to be explained as entities of some kind, then a theory of metaphysics (of unfalsifiable metaphysics, that is) would be placed at the heart of Popper's theory. These are general objections that accompany any employment of the concept of fact. In the case of Popper, however, the situation is even more difficult. According to Popper's early radical falsificationism, the practice of science does not require that anything be accepted as true. This implies that presuppositions cannot be included in Popper's analysis of science, since according to the best account of presupposition (Strawson's) 'B presupposes A, just in case A has to be true in order for B to have a definite truth value'. Any theory of facts, or any specific facts would have to serve as a presupposition to doing science, and this runs contrary to Popper's views.

There are two alternatives at this point: one can consider facts as assumptions. The result would be that facts appeared in conjunction with the theories to be tested. But this solution is certainly anti-realistic for it would destroy the independence of facts as theory-falsifiers; the other alternative is fully rationalistic: to expect that a theory of facts could serve as a principle that would lead deductively to a complete account of science. Such a

Leibnizian alternative is not easily available for any twentieth century philosopher of science.

There is also a third alternative: if Popper could show that facts and correspondence or disagreement with them form no part of the synthetic (falsifiable) part of science, then he would have managed to place them in the realm of logic, which is immune from falsification. Something like this is attempted in the Logic of Scientific Discovery. He begins by offering a formal definition of facts that amounts to 'class of sentences equivalent to a true sentence' [1959, p. 89] and continues:

The purpose (of these rules of translation) is to give an interpretation of the realistic mode of speech which makes intelligible what is meant by saying, for example, that an occurrence P_k contradicts a theory t . This statement will now simply mean that every statement equivalent to P_k contradicts the theory t and is thus a potential falsifier of it.

In Popper's view this enables him to talk of basic sentences corresponding to such facts, and attempts to decipher the structure of such sentences from two formal and one material requirements: a basic statement should have (i) deductive independence from theories to be tested (ii) ability to contradict statements of theory (iii) reference to observables. His candidate is the existential statement which contradicts universal statements and yet is independent from them deductively. [1959 p. 100-102] As for the observability condition he is shrugging off the charges of psychologism and claims that it should be taken as a primitive. [1959, p. 103]

This first attempt at determining a suitable account of facts fails on many counts. The formal conditions certainly do not determine the nature of facts nor do they shed light on correspondence. The appeal to realism is unexplained; and the so-called definition is purely nominal, and leads quickly to circularity since facts are defined by the appeal to "true." Finally, the above description of falsification hardly ever applies to science. Theories are not tested just by a 'yes' or 'no' that an existential sentence yields but in interesting instructive ways: things that we would expect to turn one way, turn another way. These new turn of events are certainly connected with theory since they often suggest directions in research. Indeed a falsifying instance is not often taken as such before we make sure that an explanation for it cannot be found or before a new theory is advanced that can account for it.

Given the failure to find a formal account of facts for his early falsificationism, Popper remained in philosophical embarrassment over a crucial aspect of this theory. One can understand then why he would embrace the Tarskian Theory, and tailor his interpretation of it in such a way as to fit the desiderata left unfulfilled earlier. He needs a correspondence theory of truth that does not commit him to accepting the truth of a previous ontology or of a set of statements. In Popper's reading the TTT is both a correspondence theory but at the same time a non-synthetic one; indeed his appeal to syntax means that he takes it to be a logical theory: one that reduces

semantics (a discipline that still has to talk about the world since it discusses reference to the world) to syntax (a theory that has to deal with language alone).

Popper's reading of the TTT then fits exactly his purposes, yet it cannot be accepted as an accurate reading. Let's take the points one at a time. The first point of contention is that the TTT is not a correspondence theory but a semantic theory of truth. Of course, given that semantics deals with the relations between symbols and objects, one could call it a correspondence theory but certainly not a correspondence-to-facts theory, as Popper wishes it to be. The point here is not to quibble about terminology, it is one of philosophical principle. What is wrong with correspondence-to-facts theories has to do with the concept of 'fact' which is shown upon analysis to be either trivial or suspect. It is trivial when it is taken as a semantic substitute for 'true' as Popper did in the passage quoted before. It becomes suspect when it leads philosophers to investigate facts as if they were some kind of entities, committing thus a category mistake. Quine [1960, p. 246-8] and Davidson [1973, p. 80, 1967, p. 752] have shown that little sense can be made of the concept. Davidson has even offered a proof that shows Frege's original intuition to be correct: if facts are needed for true statements to correspond to, then there can be only one fact and thus we are back to explaining truth as correspondence (agreement) with reality.² If, on the other hand, one treats facts as intensional

objects, as Scheffler does in his effort to eliminate them from the context of explanation [1963, p. 57 ff] the road is more promising as we can begin with an analysis of the expression 'the fact that...'. But even in this approach, admirably pursued by R.M. Martin [1967, p. 180 ff], we will find that other intensional objects are certainly more promising either because they have more structure to reveal or because they promise more by way of positive further research. Propositions for example reveal grammatical structure, and modalities reveal logical structure. Beliefs, on the other hand, offer the possibility of further psychological research and so do representations. But when we turn to facts the impediments are insurmountable: what is the structure of facts? What could future research reveal about facts that we do not already know? One could hope that philosophy could make a break in the analysis of facts analogous to Russell's break in the theory of descriptions [Russell, 1903] but in that case we were aware of the logical puzzles involved and thus we had criteria by which to judge the success of our analysis. In the case of facts we cannot even tap our intuitions for one positive lead: are there general facts corresponding to general statements, negative facts to negative statements? Does a conjunction correspond to one fact or to many? What are the criteria of identity of a fact?³

Popper's second claim about the TTT is less controversial at the start. He maintains that TTT offers an adequate explanation of truth by reducing all semantical terms into syntactical ones. The claim is certainly not unfounded at least in its first half,

since Tarski has claimed TTT to define truth by using non-semantical terms [1943, p. 351; 1969, p. 68]; there is also good ground for asserting the second part. What is questionable is the claim that the TTT is an explanation of truth. The philosopher who has been able to voice this objection most succinctly is H. Field in his 1973 paper "Tarski's Theory of Truth." We shall have the opportunity to examine his position in detail later but for the time a brief summary of his general position is in order. According to Field, Tarski's explanation of truth consisted primarily in reducing it to satisfaction, that is reference, which is a semantic notion. Concerning the recursive characterization of satisfaction, this is done in non-semantic terms, yet it is not a real explanation. It aims merely at extensional equivalence and represents no real reduction. In effect then, Field presents the Tarskians with the following dilemma: either Tarski did explain truth but not in non-semantical terms, or he did offer a non-semantic account of truth, but which cannot be called an explanation.

This attack of Field, although it is unfair to Tarski, as I will show, is especially effective against Popper and all those thinkers who expect from the TTT an endorsement of realism, because it shows that the TTT does not represent a real advance in explaining reference. Field does however agree part of the way with Popper. In interpreting the TTT, Field brings textual evidence to show that Tarski's real aim was to clear semantics of the paradoxes of semanticalism and to show that it is in principle possible to do

physicalistic semantics: at least one would not have to consider semantics irreducible to other more rigorous disciplines as the semanticalists required. This point is essentially in agreement with Popper's claim that Tarski "rehabilitated" truth. But he did not explain truth in ways that would support Realism.

Popper's point about syntax has a kernel of truth, but not the whole truth. Popper wishes to have a purely syntactical theory to avoid placing a theory of facts as a presupposition to the practice of science. Whatever appeal to syntax there is the TTT certainly does not, and cannot, achieve this aim. Instead what the recursive definition of truth does is that it reveals in the construction of sentences the syntactical elements that are truth-sensitive. The TTT does not turn correspondence-to-facts to a matter of syntax alone. Instead it isolates and uncovers the truth-relevant syntactical combinations and in this way tells us in what way syntax matters to truth. Even for mathematical theories it is required by the TTT that we know the truth of the atomic sentences.

Finally, with respect to Popper's third thesis, the relations between TTT and Realism, we are back to square one. When one strips the TTT of the title "correspondence-to-facts-theory" what is left can hardly be called a Tarskian endorsement of realism. The most Popper can say is that language is about the world and Tarski's theory is not needed to bring this point home.

To sum up then, Popper's reading of the TTT is inaccurate on almost every count. It is however, still possible that his general

stand on the matter is correct while his specific arguments are erroneous. Seen in this light, Popper's attempt becomes an instructive failure: we have to examine his realism and to investigate the reasons for its failure.

There are two main components to Popper's later Realism: (i) the correspondence theory of truth, and (ii) a theory of Scientific Progress. Popper thought erroneously that both could be established by appeal to the TTT. It turns out that his reading of the TTT is erroneous and that neither of these two components can be established by appeal to the TTT. What remains for us to investigate is two-fold: in the first place is Popper's hunch about Realism correct? Secondly, is there perhaps a way of reading Tarski, other than Popper's that could help Realism. We shall investigate the first question here and the second we shall leave for Part II of this chapter.

Popper's first thesis about Realism (the correspondence theory of truth) as we argued, boils down to a vague claim about reference. All language and theory are about the world. The second thesis is more fruitful: scientific progress is certainly central to any theory of Scientific Realism. Popper is also correct in recognizing that what is significant about progress is to be found in the development of scientific ideas - the Third World. What is missing from his theory however are examination of this progress of ideas. Progress cannot be studied without stipulating a criterion or at least a basic structure with respect to whether progress is achieved. One

would expect that his bringing up the independent world of developing ideas would lead Popper to measure progress in terms of conceptual innovation; that he would focus on the content of science and reveal to us the conceptual tendencies that are latent in the development of science: the tendency towards the discrete, towards mathematization, towards de-anthropocentrization etc. Of course, these tendencies of themselves do not constitute progress without a criterion: one has to show why all these tendencies result in progress. The road here is open in the direction of explanation. The history of science abounds of examples of this sort: the flight from teleology, from mentalism, from potentialities, form-matter, are normally defended by appeal to better vs inadequate explanations. This is by no means the only way of dealing with progress. Larry Laudan, for instance, in a recent book takes problem-solving as the measure of progress, rather than explanation. Popper on the other hand, abandons his early lead, as well as all the recent developments in History of Science, and uses truth as the yardstick of progress. On the surface the idea is certainly valuable: the Realists could use a theory of approximation of truth. Or do they? The Realist's concern is not approximation to truth but the the truth. If all of our concern in science was to match up truths we would hardly need to progress beyond Ptolemy or Aristotle. Even the idea of matching up the maximal number of truths fails to square with the actual development of science. Popper's attempt then to explain progress by verisimilitude

and verisimilitude by Tarski's Theory of Truth mistakes the real import of the Realist's belief in a gradual approximation to The Truth. Instead of pursuing the development of concepts in the Third World, Popper barks up the wrong tree, Tarski's Theory, leaving the right tree, namely the relations between explanation and ontology, untouched.

The errors here are compounded: an erroneous reading of the TTT, a wrong representation of progress in verisimilitude, as well as an erroneous theory of verisimilitude⁴ and a short-changing of explanation. Popper's approach to explanation amounts to nothing since his account of explanation is merely an appeal to conjecture: science is explanatory to the extent that it involves conjectures. There is no mention of the content or ontology of explanations. This empty theory has as a result the blurring of two key distinctions that Scientific Realism insists on drawing: the distinction between common sense and science (blurred because conjectures figures as much in common sense as it does in science) and the distinction between observational and theoretical knowledge (again because conjecture, the supposed basis of explanation cannot discriminate between the two). After all synthetic knowledge, including simple perceptual judgments involves a large element of conjecture).

The most serious error by far involves the misunderstanding of Realism as far as correspondence and approximation to a true description of reality are concerned. Popper took the TTT as the only way this tenet of realism can be made clear and established.

There is no denying that Realism has a deep and serious commitment to truth, but this is the case with all human endeavors that are susceptible to rationality. We do not call the court's commitment to truth "realism." This shared commitment to truth can be dealt with by TTT, but it is only a minimal necessary condition in defining science as a truth seeking activity: it only tells us that the scientist is "rational" (again in the minimal way that his logic - contained in his syntax - is describable in a material way by the Tarskian account). Yet there are specific commitments of science that go beyond this minimal commitment and these commitments have to do with explanation. While an adequate account of explanation is still pending, still we recognize that it is connected partly to the content of a theory, its ontology. Is truth connected in an essential way with this ontology and if so can it then be dealt with in terms of the TTT? On a first glance it would appear that no such significant connection exists: while science is after the true ontology, still the connection between the choice of ontologies and truth is not clearly established: we still choose between theories that are truth-equivalent. Put in an Aristotelian manner - while science does (like all human endeavors) care about the question "is it (the case that...)?" it specifically cares about the questions "what is it?" and "why is it?" Of these two questions the first is clearly ontological, while the second has at least two components: a material component - connected to the first question - and a formal component

connected with inference in general. It is to the latter component that the TTT is connected. What about the first component however? In some ways the choice of ontologies is a matter that no formal theory of truth can handle. The idea that ontologies are chosen as to match more and more truths is not born out by the history of science. Popper's attempt to reduce the ontological component to a truth component (either in its early form "boldness of conjecture" or in its late form "verisimilitude") fails, with or without Tarski's assistance. At the same time however a common link between Tarski's Theory and the problem of scientific ontology does exist for it is part of Scientific Realism that the terms that make up their ontologies refer. The question is now whether the reference that the scientific realist has in mind has any connections with reference as an explanans (or more correctly explicans) of truth on Tarski's Theory. This is the central problem of our next section.

1.2 Tarskian Correspondence and Realistic Correspondence

What is the philosophical significance of Tarski's Theory of Truth especially with respect to the establishment of Scientific Realism? There is little doubt that Tarski's Theory did achieve a lot. When we pass however to the philosophical significance of the Theory the situation becomes complicated to the point where it would safe to claim that philosophers could not rationally discuss Tarski. We have on the one side philosophers, of repute, who are claiming for this theory unique powers for curing philosophical

ills, and on the other side we have philosophers who are wondering if they would be called children if they openly stated what they honestly believed: the Emperor has no clothes. So what did Tarski achieve?

There is little disagreement about one aspect of Tarski's achievement: his theory that it is possible to talk about truth, a problematic yet all important notion, without having to apologize for its paradoxical nature. There is also no denying that to be able to talk about truth without the pains of paradox is significant for many reasons. In the first place the concept of truth is important in discussing matters of logic: we need to be able to state logical laws involving the concept of truth without running the risk of paradox. Truth is also needed in the context of logical validity as an independent way of establishing the consistence of axiomatic systems. Without an adequate theory of truth there cannot be an adequate formal semantics. Truth then, before Tarski, presented foundational problems to formal semantics in much the same way that the concept of set of sets presented problems in the foundations of arithmetic. The semantical paradoxes that were generated certainly mattered to logic and formal semantics. They would also matter to any attempt at developing a purely formal theory of science, since one cannot do epistemology or logic of science without some concept of truth. Given that the one viable alternative would be Ramsey's (and Kotarbinzki's) redundancy theory, we can see why Tarski saved truth without eliminating it. Ramsey's account is inadequate

to handle the use of truth in the context of meta-theory or even in simple contexts where a set of sentences called by a summary name is characterized as true. Tarski takes the paradoxes seriously as symptoms of a fatal flaw and proceeds to offer a 'purification' of truth: we can now do formal semantics without fear of contradiction.

There are three questions that must be asked here: 1. Is the TTT an adequate solution to the paradoxes? 2. Does the TTT capture all that is significantly involved in our encounter with the concept of truth? 3. Is the avoidance of philosophical and logical embarrassment all that there is in the TTT? The first question falls outside of our interests. Our task is not to examine formal semantics; moreover, the liar paradox has little to do with epistemological concerns.⁵

The second question addressed to Tarski's Theory must be postponed till we have dealt adequately with the third: we must know more about what the TTT has to offer before we are able to assess its epistemological value. It should however be emphasized here that on the one hand, epistemology has only a cursory interest in the truth-paradoxes. On the other hand, Tarski himself claimed his theory to be epistemologically neutral. This should have settled the dispute had it not been for an additional clause that can be taken as implied by his work: the TTT is epistemologically neutral, (and here comes the clause) as all adequate theories of truth ought to be. This is clearly a point of disputation.

By his own admission, Tarski takes the liar paradox seriously, as the telling sign (symptom) of an underlying problem. The paradox furnishes him with a formal condition (to avoid inconsistency) and with a hint as to the form of a general theory. The proper definition for "true" has to be limited to a language and has to be developed in a meta-language, otherwise the paradox can be revived. By being semantically closed ordinary language allows for such paradoxes. Since its semantical closure does not adequately distinguish between language and metalanguage, the definition of 'true' for ordinary language is not attainable by Tarskian means. The paradox of the liar is important to solve not merely from a strictly formal standpoint. If it proves insurmountable to solve or if its solution turns out ontologically expensive then the result would be antithetical to reductionism and physicalism in the following way. If the paradox cannot be bypassed then the aetiology of it (ie. as based on 'self-reference') would give impetus to semanticalism as a kind of mentalism, where the 'emergent' characteristic would be the 'capacity of self-reference'. A neuron that fires cannot be involved in 'contradiction' arising out of self-reference. The hope for a physicalist account of semantics would face the problem of "interpretation of symbols" because clearly the mind can 'take' objects as symbolic and can even symbolize its own symbols and talk about them (thus turn self-referential).

In addition to the above methodological consideration (as well as the formal consideration) Tarski set out to offer a theory of

truth, which although 'formal' still had to be a theory. In addition to consistency theories must match material conditions, otherwise they would be about nothing. The intuitions that a theory of truth has to account for (the 'facts', so to say), according to Tarski, are contained in the traditional correspondence notion: "A sentence is true if and only if it corresponds with the facts." Given the vacuousness or vagueness of the right-hand side of this bi-conditional, one has to spell it out concretely. In the case of every language and every sentence of that language one can say that the above vague biconditional is equivalent to a biconditional of the general form

'p' is true if and only if p

where 'p' in quotation marks is the name of the sentence that appears on the far right. The schema above cannot be raised on to a level of general theory without quantifying over statements and without guarding that the liar paradox is not introduced in the place of 'p'. This would again lead us to semanticalism for we would have to admit propositions as entities. Tarski's solution is ingenious. Imagine an impoverished language of about 4 sentences. We would know how to construct a theory which would imply the schema above applied to each specific sentence. The theory would contain all the applied schemata joined by alteration. Languages that are worth their name however are infinite in terms of grammatical constructions (and if we trust Chomsky even indenumerable). Furthermore, truth figures in them specifically by way of grammatical (syntactical) structures. They would be unlearnable otherwise. For a few classes

of these languages Tarski has been able to define 'true-in-L' in a significant way that can meet the material adequacy criterion W. He utilizes recursion that allows him to reduce the truth of complex sentences to the truth of simpler sentences and so on down to the atomic sentences. The truth of the atomic sentences then can be established in accordance with the schema as well as a carefully inductive account of their possible combinations in such a way that no allowable grammatical structure is unreachable by starting from simples. Given the theory then there is no sentence whose T-schema instance is unreachable (in this case provable). In this way the desideratum was fulfilled, but in a formal way.

Tarski's Theory is extensional because it seeks only to prove for each sentence of the language the corresponding T-schema instance: it has to capture ultimately the true sentences. Its extensionality matters in two respects. In the first place, it merely matches the true sentences. It does not identify in a non-trivial way what they have in common (if that can be done): specifically if two sentences are assigned by the theory the same valuation then from the standpoint of Tarski's Theory they are indistinguishable. Secondly, the general interest of this theory lies in its form, because it shows where truth matters in grammar. Since all other theories of truth have to meet the W-condition close attention must be paid to this. Should another predicate of sentences (besides the T) prove to match the 'T' sentence for sentence then it is a truth predicate no matter what it connotes (ie. 'provability' or 'prepos-terousness' etc.).

The previous sketch is rough, and the TTT has more subtlety than described above. In the preceding exposition, for instance, we stopped at the level of atomic sentences while we know that even they can be broken down to truth-significant combinations. The process however is similar for instead of name of sentence and sentence we can have sequences of objects satisfying open or closed sentences. 'Satisfaction' which is again a semantic relation can be recursively characterized in similar ways so that eventually all truth is definable by means of satisfaction by sequences of objects. Another subtlety which will prove important later, is the distinction between the generalized theory which defines truth generally by satisfaction-by-a-model and the absolute theory which is a special case of the former tuned to the actual sequences of objects (infinite sequences) available in the world. The latter appeals to philosophers since it purportedly shows the TTT as establishing a genuine correspondence between language and objects of the word.

The TTT's achievement then extends beyond the simple solution to the paradoxes: it saves the physicalistically minded semanticist a lot of embarrassment. The semantic paradoxes had generated the impression that an adequate physicalistic semantics could not be given. The impression was justifiable because it is obvious that the paradoxes are generated by some form of self-reference. This in turn gave rise to the belief that semantics had to presuppose semanticalism (ie. irreducible semantics: or, to use a more understandable locution, the semantic properties are emergent). I owe

this interpretation to H. Field who quotes Tarski's "Establishment of Scientific Semantics" [p. 406] "...it would be difficult to bring [semantics] into harmony with the postulates of the unity of science and of physicalism (since the concepts of semantics would be neither logical nor physical concepts)."

Both of these contributions, the avoidance of paradoxes and of semanticalism, important as they are, are nevertheless not fully positive contributions: they do not offer a genuine insight into truth but simply remove impediments to its proper employment or its proper study. To fully understand Tarski's contribution one has to look more closely in two directions: first in the specifics of Tarski's Theory and then in the history of the attempts to define truth. Let us take the second task first.

Defining 'truth' as 'correspondence to the facts', that is to say as a relation faces the obvious problem of determining first the nature of facts and then the nature of the correspondence relation. We have given reasons why the first task is impossible, if desired at all. The Austin-Strawson debate shows why discussion of correspondence is equally thankless. [See Pitcher ed. 1964] One would think here that our problems could be bypassed if we defined truth not as a relation but as a property of sentences (a property characterizing some sentences, the true ones). This idea faces at the start a fundamental objection proposed initially by Kant in the First Critique:⁶ if we made a general property out of correspondence then, in order to define truth as correspondences, we have to find

what is common in the instances of this property that all true sentences share. Yet what makes the sentence "Snow is white" true is different from what makes "grass is green" true. This, in effect, particularizes all talk of truth as the property of correspondence-to-facts, to such a degree that the meaning of 'true' in "'snow is white' is true" is different from the meaning of 'true' in "'grass is green' is true." The net import of this consideration is the realization that 'true' as a property-word becomes syncategorematic and thus we have to abandon the hopes for a general theory of truth. Put in more simple terms: 'true' means different things in different cases. The alternative here is equally - if not more - uninformative: go back to the relation of correspondence and claim that true sentences correspond generally to reality.

In my view, Tarski's great achievement was the discovery of a clever way of combining the correspondence relation with the truth property and in this way provide a general definition of truth. There are not so many cases where philosophers managed to have their cake and eat it too, so Tarski's achievement deserves special attention.

Let us start by pointing out to all those who insist that truth for Tarski is a relation of correspondence, that Tarski always insisted upon calling truth a 'property' [1969 p. 64, 1943 p. 345]. In addition to the above considerations there is an important formal reason why truth in the TTT cannot be taken as a relation as Davidson points out [1973 p. 78]

the logical grammar of T-sentences shows that it is essential that the truth predicate not express a relation: if it did, there could not be the crucial disappearance, from the right branch of the T-sentence biconditional, of all semantic concepts, and indeed of everything except the very sentence (or translation of the sentence) whose truth condition it states."

The property of truth then is defined recursively for all significant logical constructions in such a way that the truth of complex sentences is reduced to the truth of the constituent sentences. Tarski has as a necessary aim of his definition the ability to prove for all true sentences in a language, a T-sentence stating this truth. Leaving aside for the time the charge that such a requirement is merely meant to match by extensional equivalence all the true sentences and in such a way claim universality for the theory of truth, let us concentrate on a significant detail that bears the previous considerations. While truth is defined recursively as a property, ultimately the open sentences which though not true make up the true sentences, are brought into the theory by way of satisfaction by a sequence of objects. This ultimate definiens of truth is not a property but a relation, and it is here that the TTT stakes its claim to be a genuine correspondence theory of truth. Satisfaction in its turn is characterized recursively and thus disappears as a semantic property. The result is amazing: (i) a general definition of 'truth' as "satisfaction by all sequences"; (ii) a non-semantic definition of truth (by the recursive characterization of

satisfaction) that maintains what is significant about semantics (relations between language and world) but at the same time eliminates semanticism: a purported full explanation of semantics, in non-semantic terms; (iii) a definition of truth with as much generality as the property of truth affords and as much specificity as the relation of correspondence would require. This is an important point. Though a true sentence is satisfied by all sequences, still ascribing truth to a sentence requires that we pay attention to the open sentences that make up the closed sentence in question and specifically to the satisfaction of this open sentence by sequences of objects. In the case of each true sentence then spelling out its property of truth requires running through the recursion backwards to the sequences of objects that satisfy the corresponding open sentences. Tarski's Theory, as his proponents would say, return truth to where it belongs: syntactical construction on the one hand, and things in the world, on the other.

As with all clever card-tricks, before the applause settles down, doubt settles in: how accurate and how adequate is this theory? Tarski announced at the start that his aim was for a theory that is formally correct (to avoid the paradoxes) and materially adequate, that is to say capable of handling the occurrences of the term 'true'. The formal correctness of the theory is beyond doubt, although the restrictions that safeguard this correctness are thought by Tarski's opponents to be rather artificial, on two counts: the distinction language-metalanguage and the requirement of a semantically closed

object language. It is however with respect to its material adequacy that the TTT has generated controversy. According to Tarski a materially adequate theory of truth for a language ought to imply all instances of the schema: "The sentence S is true if and only if P" where S is replaced by the name of a sentence in the meta-language and P is replaced with the sentence itself or a translation of the sentence in the metalanguage. There are good formal reasons why the schema cannot be generalized to be made into a definition.⁷ According to Tarski however, all the instances of the schema T for a language L can be thought of as partial definitions of truth. Had L a finite number of statements Tarski would have a complete definition of truth by conjoining all the above instances of T for L by disjunction. Short of this, recursion achieves the same effect. These partial definitions of truth however, generate two basic difficulties. In the first place, Kant's fears are vindicated: the TTT is at heart not a general theory of truth. Secondly, Tarski's Theory simply achieves an extensional equivalence but it is not a real theory of truth: because it provides no real explanation of truth, it just offers a recursive way that allows us to reach deductively the instances of the T-schema.

There is little doubt that no real explanation is offered by a theory that simply aims at extensional equivalence. To give a general theory of 'man' in the form 'to be human is to be either Tom, Dick, Harry...' can only be taken as a joke. Of course, Tarski's Theory is not as simplistic as the above example suggests but a

more elaborate argument can be constructed leading to a similar conclusion. Hartry Field has done exactly this in the already mentioned 1973 paper of his. Field argues that Tarski correctly reduces truth to Reference (satisfaction); but then he proceeds to show that the recursive characterization of satisfaction is in fact, trivial and thus non-explanatory. To make this clear he argues by analogy as follows: he likens the present situation (truth reduced to reference) to the situation at the end of the previous century in physical chemistry when the scientists had knowledge of the valence of a lot of elements but had little or no idea what valence was. He then proceeds to offer a "Tarskian" Theory of Valence as follows:

(VE) (Vn) (E has valence n $\equiv$ E is potassium and n is +1,
or... or E is sulphur and n = -2).

It is true that even if the above definition covered all the existing elements it would not constitute, it would not be called, a theory or an explanation of valence. The analogy is especially apt because (i) valence at that time was as important to chemistry as truth is to semantics today, (ii) valence was used as the basis for a proper reduction of chemistry to physics avoiding thus emergent-chemicalism in much the same way that we expect out of 'satisfaction' to obtain a non-semantical account of truth and a rehabilitation of semantics into physicalism.

Field's argument though analogical carries a lot of weight. Its philosophical core traces its origin to similar arguments offered in the history of philosophy: one can find traces in Aristotle's

critique of the Platonic forms, but the best example of its use can be found in James' Critique of Fechner's reformation of Weber's Law: James claims that Fechner's logarithmic formulation of the Weber Law of the Constant Ratio of JND's to original stimulus, simply tells us what we know in another way without advancing a real explanation (which presumably should be in physiological terms). Field's claim is similar. Tarski's theory of satisfaction is a pseudo-theory, which adds nothing to the understanding of truth or reference and misleads us away from a proper investigation of truth: a causal theory of reference that would really anchor semantics in the physicalist framework of modern science. To put it in a traditional idiom, Field's claim is that Tarski's Theory fails for the most part to reveal the nature of truth.

Field's challenge to Tarski is the best realistic attack on the TTT, allowing us to discuss Tarski's achievement in a critical light that adjudicating between these two theories will generate.

Let us start with the condition of material adequacy: the condition W of Tarski. It would be wrong to focus on the many instances of schema T as just partial definitions of truth for a language L. Of course Tarski's ontological parsimony does not allow us to consider the schema T as anything more than a schema, but in the interest of explanation of truth we can take a small step into second-order logic and reformulate the schema as

$$T(p) \equiv p$$

The significance of this reformulation becomes obvious when we realize that Condition W is a necessary condition that any theory of truth ought to fulfill. What Tarski is in effect proposing here is a challenge to any proposed definiens (or definitional substitute) of the true-predicate.⁸ It has to be such that allows for the above equivalence to hold. This is not only a good guide for the search for definiens of the true-predicate, but offers a glimpse into the nature of truth. When we return from the second-order logic to ordinary semantics we can say that Tarski's Challenge is indeed the unique property that the true-predicate has among all predicates that apply to sentences: this property is that it allows for dis-quotation.

The major tenet of Field's objection is that the TTT fails to uncover, or even to direct us properly toward uncovering, the nature of truth, and as such it is not a genuine theory. This claim cannot be judged unless we say a few things about the nature of truth: when one claims that a certain theory does not reveal the nature of an object he must have a relatively clear idea about what kind of theory he is after. There is little doubt that the "Tarskian" Theory of Valence does not reveal the nature of valence, but truth and valence are different kinds of concepts. In the case of valence we knew well enough that there were things about valence that we did not know at the time. In the case of truth it is questionable that there is such additional information to be found. Truth is certainly a problematic concept but its problematic character is

not easily attributable to our ignorance as in the case of valence. It is difficult to imagine what kind of research would reveal more things about the concept of truth. Field, of course, is not entirely unjustified here since Tarski speaks of physicalistic approach to semantics and even cites research (in the forms of poles taken) in support of his theory, but here Tarski is simply wrong in overstating his case.

We have argued in the previous paragraph that truth and valence are different kinds of concepts and as such deserve different explanations. In what kind of category of concepts and explanations of concepts does truth and the TTT belong? It would be better in my view to classify the truth-concept together with concepts like infinity or even space and time, because only then the plausibility of the TTT becomes apparent. This of course, is a circular argument for the plausibility of the TTT, but one should consider that all the concepts mentioned are not just fundamental ur-concepts but they are problematic because of their paradoxical nature. In all such cases the proposed explanation does not move in the direction of further research that would remove the paradox. Instead we seek a theory which will cover adequately what we know about the concepts, paradoxical nature and all. For example, we do not worry about the real nature of space or time, instead we concentrate on theories that capture a few key relationships such as incidence, betweenness, congruence, or equal duration and simultaneity. In all of these cases our success depends on two factors: whether we have captured

all the pertinent characteristics of the concept in question and whether we have done so correctly. The additional demand to reveal the nature of space or time is difficult to understand: are we supposed to offer a picture that ends up being metaphorical and a category mistake? The concept of infinity offers an equally good example of a paradoxical concept 'explained'. Dedekind turned the explanandum (the "paradox" that infinite sets were equal to some of their proper subsets) into a definiens by his famous Definition 64.⁹

The key then to these types of explanations is to collect the "telling" characteristics (facts, intuitions) of the concept in question and to construct a theory that can account for all of these facts, even if this theory is little distinguishable from the collection of these facts. The proper critical approach to these types of theories is to question either their material adequacy: "Are the telling characteristics that the theory has selected to account for sufficient to cover all our intuitions about the concept in question?" or their formal correctness: "Does the theory account for these telling characteristics without leading into error?"

In my view the proper way of interpreting Tarski's Theory of Truth is as a theory of the kind described above. Field's error is to demand of Tarski something different: a reductionistic theory of truth that would uncover the nature of truth. I shall not wander off into the problem whether truth is in fact a concept like space, time, or infinity rather than like valence, nor will I engage in hermeneutics to show that Tarski's real intention was to provide

a theory of the type that I am proposing. My aim is to discuss realism and correspondence and specifically whether Tarski's Theory offers support for Realism or not. I return therefore, to the material adequacy problem in Tarski's investigation of truth. The telling characteristics of truth that the TTT sets out to capture are the following three:

- (1) Disquotation: The probably unique property of the predicate "true" to allow us to infer 'p' from "'p' is true" and vice versa. As we explained earlier, when we talked about Tarski's challenge, the condition W is meant by Tarski to capture all there is to the Traditional Correspondence intuition: what he calls "the Aristotelian Definition of Truth."
- (2) Truth-sensitivity of syntax: In its specifics the TTT also captures the fact that truth figures primarily in logical constructions (logical syntax). When we follow carefully the recursion in the TTT we notice that Tarski's reconstruction mirrors in some way the (extensional) way of assessing the truth of compound sentences from the truth values of the constituent sentences. To put the matter simply: a good account of truth should capture the way in which syntax affects truth.
- (3) Satisfaction and reference: Although the definition of truth as satisfaction by all sequences sounds unfamiliar and implausible, closer attention to the recursive way in which satisfaction is characterized and to the process by which the satisfaction functions relate sequences of objects to open sentences, first,

and then closed ones, reveals that in the TTT there is clear recognition that truth is based on reference. Statements are not true or false on their own; they are true or false because they are about the world or more specifically about objects in the world which happen to have or not to have the properties that the sentences say they do.

The above are the telling facts about truth that Tarski set himself to capture by the TTT and he did capture them. I shall not discuss whether he captured them in the best possible way, but rather whether these telling facts exhaust all that is worth capturing from our encounters with truth. To begin with let us note that Tarski does not wish to capture all that is relevant in our encounters with truth (certainly not the pragmatic aspects of lying or truth telling, etc.) but only as much as goes into correspondence. For any Tarskian, therefore, (1)-(3) cover completely and adequately our encounters with truth as correspondence. Davidson has made this claim explicitly in his articles "True to the Facts" and "In Defense of Convention T." Up to a point, he is right: no other conditions have been clearly proposed to suggest that (1)-(3) fail to capture an important aspect of truth as correspondence. Of conditions (1)-(3) the Tarskians would insist on (1) and (3) as the key to understanding truth as correspondence. Yet despite the elegance of disquotation and the exhaustiveness of the recursion, one cannot help the feeling that there is more to correspondence than what the TTT captures. There have been many attempts to put the finger on this elusive quality contained in correspondence: Field, the

Pragmatists, Austin & Strawson. Let us vaguely characterize it by a metaphor. Our encounters with truth contain a success-story: achievement in the interaction with the world. To push the metaphor further, while many people think of our dealings with truth as a kind of arrow-shooting of sentences onto the world, Tarski would have us believe that we shoot the arrows first and paint the targets later. Enough with the metaphors, let us now try to spell out concretely the material inadequacy of the Tarskian correspondence.

I shall proceed by way of contrasting the Tarskian account of correspondence (1-3) with a paradigmatic case of correspondence: if there is any inadequacy in the Tarskian account it is here that we should expect to find it. After all, one has to ask why has correspondence been associated with truth? Is it merely an ill-fitted metaphor? The most successful case of truth-related correspondence that I can think of is that between a region mapped and the (or a) map of the region. Let us call this correspondence 'map-correspondence' and the (1)-(3) conditions as the Tarskian correspondence. There is little doubt that the map-correspondence has the elusive quality of achievement that we mentioned as vaguely residing in any correspondence account of truth: anyone who has used or even contemplated a map knows the feeling involved in knowing that this map corresponds to reality. So what is there in map-correspondence that is missing from the Tarskian correspondence?

Sl. Isomorphism. The first suggestion that offers itself is that the map-correspondence is superior to the Tarskian correspondence because of the isomorphism that exists normally between the map

and the mapped. Contrary to the Tarskian correspondence in which sequences of objects satisfy open sentences in map correspondence in addition to the objects we have an account of the relation as well. To use Davidson's example here, maps capture what the opponents of Tarski are expecting his theory to capture: "Thus 'Dolores loves Dagmar' would be satisfied by Dolores and Dagmar (in that order) provided Dolores loved Dagmar. I suppose Dolores and Dagmar is not a fact either - the fact that verifies 'Dolores loves Dagmar' should somehow include the loving. This 'somehow' has always been the nemesis of theories of truth based on facts." [1967 p. 748]. In the case of maps we can have the 'loving' too. A map not only informs that Brighton is between Newton and Boston, but also depicts Brighton between Newton and Boston.

This view of correspondence has been prominent and disastrous in theorizing about facts; the most celebrated example being the picture theory of meaning in Wittgenstein's Tractatus. This idea of map isomorphism cannot be applied to language. In the first place, it does not help even in the case of maps: even the most "realistic" maps are isomorphic to the mapped only according to some readings, while any map in some way preserves some isomorphism to the mapped, if only the number of items that it intended to map.¹⁰ We could then imagine a language tuned to preserving some such isomorphisms but it would quickly prove to be of little use for the functions for which languages are normally used. Even if some clear

sense could be made of the map isomorphism, it would be absurd to expect of Tarskian correspondence to seek to capture it.

S2. The Ontological Difference Between Map and Mapped. One of the sources of dissatisfaction with Tarski's Correspondence is derived from the feeling of triviality surrounding the Schema T. Although Tarskians keep repeating that "'snow is white'" inside quotes, is different than "snow is white" without, still this perceived similarity is taken as rather trivial: after all, isn't truth as correspondence a relation within the world? At this point the rational inquirer is wondering whether the objector expects white snow on Tarski's paper, yet the point is clearly made: maps correspond to regions; the two objects are of radically different nature.

As a critique of Tarski, however, the point is unfairly taken. Maps can certainly be drawn on sand, which in effect means that what is important is not the ontological status of map and mapped but the symbolic function that allows for this correspondence that leads from symbol to object. Tarski's correspondence cannot be accused of forgetting the symbolic function of words (i.e., satisfaction) and cannot be expected to produce 'objects' for the words to correspond to. As for the feeling of triviality that surrounds the Schema T here one has to go with Quine. Tarski's disquotation allows us to talk about language in ontologically acceptable ways while at the same time reminding us that it is the world that we are talking about.

S3 Informativeness. Map correspondence could be considered superior to Tarskian correspondence, a suggestion might be, because of this informativeness: one can certainly use a map to reveal unknown regions or to systematize one's knowledge of a region. Tarski's recursive account coupled with the requirement of a richer meta-language may give the feeling that Tarskian correspondence is thoroughly uninformative. The T-sentence: "'snow is white' is true if and only if snow is white" tells us nothing about language, snow, reality, or whatever. At most it tells us about how quotation marks are used when they are coupled with the expression 'is true'.

Understood in one way this suggestion turns out to be similar to the previous one, and involves a misunderstanding of truth. Language is as informative (if not a lot more informative) than maps, and it offers information about the world. One should not expect however of Tarski's theory to tell us about the world; Tarski instead aims at capturing the notions of truth and falsity. Applying this to map-correspondence it is obvious that maps can hardly fare better than T-sentences: if someone gave us a map and said "this map is true" all that this would tell us is that somewhere there is something (maybe a region, maybe a painting, maybe a building, maybe just another map) to which this map corresponds in some way. Again: if one says "Do not go by this map; it is inaccurate" again he is not telling us anything more than a T-sentence would tell us.

The suggestion however is open to another interpretation. It could mean that the visual mode of representation is more readily

accessible to the understanding that the linguistic mode, and at any rate it is more universal. This could be on account of two possible factors: either maps capture more of the mapped than language does, in which case we are back to the first suggestion S1; or, it is because the visual symbols, being more universal, carry their meaning on their sleeve. But one cannot seriously expect a theory of truth to offer us a theory of meaning as well, let alone a universal theory of meaning (Davidson notwithstanding).

Another way of presenting this same objection about informativeness is more fruitful. Maps are informative and knowing one correspondence between the map and the mapped allows one to establish a lot of others. Language on the other hand is informative but knowing one Tarskian correspondence leaves us absolutely in the dark. This is certainly a clearer formulation of that vague feeling that we were trying to spell out in S1-S3. What accounts for the difference? The earlier suggestion that pictorial symbolism is more readily understandable than linguistic is either leading to an absurd demand for meaning or is simply false. Goodman in the Languages of Art has shown such easy claims to be false; there are extremely intricate pictorial representation, and much depends on the way a pictorial representation is taken. Another suggestion would be to say that maps cannot be compared to sentences since they normally pack a lot more information in them than the location of two cities: arrow pointing North, some scale, other cities as well. This consideration adds to the previous point: maps must be read in some ways and they

are rich in content. Yet this richness and the availability of universal conventions (arrow, scale, etc.) simply allows us to find things about the map pertaining to the meaning of various symbols. In the case of sentences however, meaning cannot be individuated, as Quine has shown. Moreover, Tarski's Theory requires that we know the metalanguage before defining a truth predicate for a language L translatable to the metalanguage M. Yet, if I know a region, I certainly do not need a map.

It would be fair to say so far that the comparison between map-correspondence and Tarskian correspondence has not revealed anything significant. At best the classification turns out to be caused by an absurd demand for meaning. In other words, the feeling that correspondence should tell us more than we know arises out of the richness, the indeterminacy and the system-relativity of meaning, while truth itself is a rather unexciting concept easily capturable by disquotations and satisfaction.

At this point it would be valuable before proposing our suggestion to consider Putnam's 1976 theory that is in fact a compromise solution that manages to hold onto both, the Tarski's Theory of Truth and a richer concept of correspondence. Putnam begins by arguing that truth is indeed as unexciting as Tarski takes it to be: indeed there is no other theory of correspondence than Tarski's. Furthermore, this theory is not committed epistemologically. The Realist has no theory of correspondence to offer other than Tarski's. To establish this point Putnam shows that even if one replaces the

logical connectives involved in Tarskian semantics with constructivist counterparts, still the resulting truth theory would be Tarskian; the only difference would be that "truth" will become "warranted assertibility" (this of course, is not a definition but just an "understanding"), and existence will be defined intra-theoretically. Thus, the TTT form of correspondence does not come in support of realism nor does the realist have to offer a rival theory of correspondence. What he has to offer is a certain understanding of the TTT that can be spelled out as 'taking the connectives in their classical sense'. Putnam argues that this understanding of the connectives and consequently of the TTT, leads to Fallibilism as an important tenet of Realism: "A statement p may be false though it follows from my best accepted theories." He suggests that any non-realistic understanding of the connectives and of the TTT would lead to understandings of truth as 'warranted assertibility' and this understanding of truth would reduce the above statement of fallibilism to nonsense.

Putnam's suggestion is an important defense of TTT as a Realistic Theory, because it manages to hold onto the neutrality of the TTT and by shifting from theory to understanding it manages to show the Realistic aspect of the Tarskian correspondence. Could this addition have been the missing element of correspondence from the TTT that we have been so patiently searching for by comparing it to map-correspondence? It is highly unlikely that the realistic aspect of correspondence can be revealed by adding to the TTT a certain understanding of the correctives that the TTT reveals as

truth sensitive. The fallibility condition at best tells us that truth depends not on our knowledge alone but on reality and that certain statements are definitely true or false though we may never be in a position to establish it. This has little to do with map-correspondence and is already captured in (1) and (3) of Tarskian correspondence.

We have reached a kind of impasse with respect to our problem of the relations between Scientific Realism and the TTT. Specifically we find that the TTT does not really fulfill the requirements that the Realist has in mind while at the same time the Realist cannot tell what is wrong with the TTT. When he was looking for was a good account of correspondence, because as a Realist he believes in a correspondence theory of truth. The TTT provides some account of correspondence which, according to the Realist (and according to Tarski) does not fulfill his requirements, but when we ask about what the Realist has in mind for his correspondence, the result is vague. In almost every respect map-correspondence (as a paradigmatic case) is shown to be as weak as the Tarskian correspondence. The only exception, and a vague one at that, is the idea that in the case of maps one needs to know a few correspondences and by using the map can establish more: if one knows the 'atomic details' of the map he does not need it. But even in this case Tarski's correspondence allows us to pass recursively from established correspondences to new correspondences that are logically complex. The observation here requires further elaboration. But in order to do this we must

bring together the various strands of our argument: Popper's failure, Tarski's theory, the demands of Realism and the two kinds of correspondence.

Popper's instructive failure can certainly help us clarify the Realist's demands concerning the correspondence theory of truth. There are two kinds of expectations that the Realist has when he is out shopping for a good correspondence theory. He expects in the first place that the theory of truth will help him understand the method of science and especially the success of the method. His second desideratum is more specific: he expects that the correspondence theory will help him establish a viable theory of reference which will uncover the way in which scientific terms refer. Let us take the two desiderata one at a time.

The first desideratum occupied Popper's approach. A convinced Rationalist, Popper tried to show the rationality of the Scientific Method by appeal to truth alone. If on the basis of the TTT we could show that the scientific method is the most reliable truth-seeking instrument then the rationality of science would be clearly established. If one takes this demand too strictly one is bound to land on falsificationism. Yet falsificationism, despite its purity, does not adequately portray science. There is a lot of positive content in the development of scientific ideas that is uncapturable by the conjecture and refutation model. At this stage of the game the TTT merely adds credence to logic (by legitimizing semantics: that is by clarifying the notion of logical consequence and thus providing another instrument of approach for the logician).

The most serious problem in Popper's rationalistic project is to account for scientific progress by connecting method and truth. As we argued, however, the Third World of the development of ideas cannot be explained by verisimilitude (with or without aid from the TTT). Once again the positivistically minded philosopher of science is faced with his old nemesis: explaining the choice of theories ultimately by appeal to nothing but truth (verifiable truth, that is). If this project fails, does this show that the choice of theories is irrational or not strictly rational? A positive answer to this problem lands us into the relativistic camp. The Realist on the other hand sees truth (verifiable truth) as only a necessary condition for the rationality of science. The development of ideas and the choice of theories on the other hand are both based on explanation, and explanation in its turn is not to be based on truth alone (by the D-N model) but is governed by other criteria, chief among which is the ontology of science. The Realist maintains, furthermore that there has been progress in ontology. Can he show this? To answer this question one has to go over all this dissertation yet a good case can be made by appeal to the history of science either in its entirety, in which case certain trends can be established, or by considering specific instances in that history (real or imaginary) in which theories were abandoned or should be abandoned on the basis of ontology alone: either the ontology involved was too far-fetched ("action at a distance" for example) or was not promising. Consider as an example of the latter the case in which faced with a choice

between the theories of Freud, Skinner, Piaget, Chomsky the scientist chooses one and discards the others because the latter do not provide him with any hint of where to look for a more basic explanation.

We will argue later that science is ontological, yet still we can make an even stronger case for the Realist by examining the problem of the rationality of scientific method in light of R. Boyd's theory [ms.]. Contrary to the positivistic analysis Boyd shows that the scientific method is not a formal and empty truth-seeking tool but is guided even in its formal structure by ontological considerations. We will have the opportunity to examine his theories further in Chapter IV, yet this suffices to provide a contrast between the positivistic-falsificationist rationalistic project for understanding scientific progress and scientific rationality, and the realist approach to the same problem which appeals to ontology and explanation primarily and to truth secondarily.

On the basis of the above considerations it is easier to understand the second desideratum that the realist has in mind concerning the right kind of correspondence theory. He expects of correspondence theory to explain the reference of scientific terms and especially theoretical terms. Is this a rational demand? Unless one accepts relativism with respect to reference (see Chapter II) the demand is rational from any standpoint: how else can one explain truth except by reference? The TTT while initially appealing, because it explains truth by satisfaction, eventually leaves the Realist where he started. What is wrong with the Tarskian recursive characterization of

satisfaction? And furthermore, is there any aspect of reference in the map-correspondence that is superior to Tarski's satisfaction? The answer to this two-fold problem is complex and involves appeal to the extensionality involved in the TTT, and the two versions of the TTT, so let us proceed with caution and patience.

We explained earlier in this section the two senses in which the TTT is purely extensional. The first-sense, clearly apprehensible, follows from the fact that the condition W is a necessary condition for any definition of Truth but at the same time, and here comes the extensionality: any predicate 'P' which functions logically like the predicate 'T' (that is to say, allows for disquotation) is according to the TTT a truth predicate. The Realist however expects from a theory of correspondence to distinguish between these prospective candidates (for the role of T) the one that is related to the scientific method. Given that the TTT cannot carry out this discrimination, one can see why from a Realist standpoint Popper's project is doomed from the start. The point is one of principle since no such predicate (even the predicate "provable") has so far filled the role of T. Yet even if there is one to be found, still the fact that, even if it is de facto unique, still it cannot be discriminated from the possible newcomer who fills the bill (functions like the T) and has little to do with science, makes the TTT of only marginal interest to the Realist. At best the TTT may be said to explain an abstract form of Realism by isolating a standard form of correspondence, but this is only a necessary condition. The Realist does not question the

validity of classical logic. He is however committed to science, not to any science in abstracto but to this science that we have developed from the ancient times to our days.

Similar attitudes can be found in the Realist's response to Tarskian 'satisfaction'. Field's paper articulated the realistic dissatisfaction with Tarski's 'satisfaction'. We have to go deeper here however, by appealing to the second kind of extensionality involved in the TTT. To do so we have to distinguish the generalized from the absolute TTT, and take each one at a time. In the generalized TTT truth is defined as satisfaction by a model. A model is an interpretation of a theory conceived as a formal set of sentences. The interpretation is a structure consisting of a domain of elements and a function that assigns to the formal signs of the theory elements from the domain. Models then are the interpretations which make the theory come out true. To any monadic predicate ' P_1 ' the model assigns $V(P_1) =$ a subset of domain D , to any dyadic predicate ' P_2 ' the model assigns $V(P_2) =$ a subset of ordered pairs of the domain D , and soon. The extensionality of the TTT means in this case that if the model assigns the same subsets of D to two different predicates then from the standpoint of Truth (as defined by TTT) the two predicates are identical. In this way "to have kidneys", and "to have a heart" are identical predicates (since they differ only in intension but not in extension). The Tarskian satisfaction by a model then is such an assignment of elements or, if you like, objects to predicates, so that:

' P_2xy ' is satisfied by $\{\langle k_1, k_2 \rangle, \langle k_2, k_3 \rangle \dots\}$ if and only if $P_2k_1k_2, P_2k_2k_3$ etc.

The Realist's dissatisfaction with this account of satisfaction is captured by Field: it merely systematizes in a formal way what we already know, so that the T sentences come out right at the end, but this is a trivial slight-of-hand trick which results in us obtaining whatever we had at the start. The charge is not fair to Tarski and, by the way, to Field since the latter concentrated his critiques not on the generalized model-theoretical account but on the absolute account. But I am interested here in explaining the general problem. To make it clearer let us cite the source of dissatisfaction with the absolute theory. Leaving aside the undoubtedly trivial "truth as satisfaction by all sequences" (since this merely utilizes the idea that only closed sentences can be absolutely true or false which leaves no place for real satisfaction by objects), we can say that what the Realist finds inadequate is the impoverished notion of reference contained in satisfaction. In the absolute version of the TTT the only parts of the world which figure in correspondence are sequences of objects. What makes science true is sequence of objects in the world. This maybe so but it does not help the Realist's quest for reference for his theoretical terms of science. Especially, given the second kind of extensionality outlined above the Realist finds that Tarski's Theory does not allow him to discriminate in-the-world the different kinds of objects that his science talks about.

Indeed, if the assignment of subsets of objects are similar for two predicates the Realist has lost the distinctions his science utilizes.

Much of the impact of this demand of the Realist will become clear when we discuss the ontological nature of science and the hidden essentialism in the Realist's use of models. For the time we are closer to putting our finger on the elusive aspect of correspondence that the map-correspondence possesses and that the impoverished satisfaction-by-a-sequence lacks.

We said that maps allow us to establish more correspondences once we know a few plus the statement that the map is accurate. The TTT does something similar but only in ways that are uninteresting to the Realist: it merely allows us to establish correspondences of sentences that are logically compound. In the TTT ultimately there is one and only one kind of reference: to sequences of objects. In a map of reference is established to many different kinds of things closely interrelated. It is this multiple and differentiated reference that allows for the use of maps to proceed from known to unknown correspondences. On the other hand one does not need a map if one knows all the corresponding referents: he simply knows the region. Of course, there are problems involved in this "knowing of the region" and it is a fair bet that if we try to unpack it some form of TTT correspondence will surface. Yet, going back to the Realist we can now pinpoint his feeling that Tarskian correspondence is minimal. It is required by the TTT that one knows the truth of all the atomic sentences of the language L before he knows the theory

of truth-in-L. Whatever value this may have for mathematical languages L (truths of arithmetic and such) for the Realist, it is his on-going natural science that he cares about and for which he does not know the truth of all atomic sentences. Ideally, one would say there are as many truths as there are facts but this does not capture for the Realist his view of science as a truth-seeking activity. Indeed, James put this matter clearly by claiming that any correspondence theory of truth is a mockery of the scientist's activities and involvement with truth. The realist is expecting out of correspondence to capture the way in which science proceeds from knowing a few truths and correspondences to new atomic truths and in his view this is intimately connected with the reference of theoretical terms. Science in the realistic conception is not interested in a simple truth by truth matching of verifiable truth; instead the interconnections of theoretical terms and the search for their referents generates new ways of seeing old truths and even new truths.

All this is rather vague and has to remain so for the time. At this point we have to ask if the Realist's complain against the TTT is justified. There is little doubt that if one stays within the absolute version of the TTT, the Realist is justified because the satisfaction by a sequence of objects is clearly impoverished: except for the objects the only other discernible structure in the referents is order. The Realist expects more kinds of structures in the referents. Concerning the generalized TTT however, the situation is different because in some respect the Tarskian correspondence fares

at least as well, if not better than the map correspondence. The reason is that the model itself is a structure which in addition to the domain of elements contains properties, relations, etc. in the form of subsets, or subsets of ordered pairs, ordered triples, ordered n-tuples. The model-structure now allows us to establish new correspondences in the following manner: once we have established (by TTT) one model, i.e., one correspondence between the theory and a structure, then we can in principle establish many others that are isomorphic to it, and in this way extend the theory in a referential manner. This consideration certainly shows the TTT as capable of 'informativeness' because the new structures can help us find things about our theory (consequences) that we had not known from the start. The point goes even deeper: one can find in science instances where a structure was applied to a new field (consider a set of wave equations or even an analogue of Newton's Theory applied to a new field: Culomb's Law).

This point bears emphasizing because it indicates a significant difference between the formalistic and the realistic accounts of science. Clearly the above model-extension of the theories does not occur in maps. It is irrational to apply a map to more than one region simply because it worked for one region, unless one had explicit instructions about the similarity of the two regions. Maps, somehow, carry with them a categoricity assumption: they are true of one unique object (normally, because diagrams can be applied to many similar structures: the diagram of a Greek temple can be used as a guide to

many Greek temples). In this observation lies an important realistic demand for science that will be examined in detail in the Frege-Hilbert dispute [Section 2.1] and in the discussion of categoricity [2.2]. What we can say for now is that the Realist does not see science as an abstract set of predicates in interrelations whose reference can only be established by formal models: he expects from his science to establish its reference uniquely. So how would the Realist respond to the above claims?

In the first place the Realist disagrees with the requirement that all the atomic truths within a model must be known. Only mathematical science can proceed this way - and this is not indisputable. The Tarskian models then are mathematical. At the same time, however, in science, the extension of one theoretical abstract framework to a new field of application, a new model does not happen automatically. Only a Platonist or a Hegelian who think that "all Nature is akin" - whatever this means - can believe that he can apply what he knows about one domain of objects to another without a preliminary ontological analysis. The Realist on the other hand insists that all such extensions are based on a preliminary identification of kind, and only if the two sets of objects are alike-in-kind can the extension of the theory occur. For instance, extending the gravitation formula to magnetism is based on the recognition that in both cases we are dealing with forces which have similarity in action (they both attract - the latter also, however, repels). Again, applying wave equations to a new field of phenomena requires a lot of ontological footwork

concerning what is the object or the medium that undergo this type of change. Concerning the specific use of models in science, M. Hesse's account of the three different kinds of analogies is on the whole correct, as is M. Wartofsky's sixth model function. My concern is for the explanation of this use of models are in my view the extension of models is not based simply on similarity of form but similarity of kind. It is this ontological consideration that allows for further research.

What makes possible this use of models? Going back to the maps we can say that there is an important characteristic of maps that has been missed by investigators. In my view it is this characteristic that allows for the informative use of maps. This significant property of maps is not just the general truism that items in the map refer to (or correspond to) things in the world but the more specific claim that different kinds of items in the map refer in different modes to different kinds of things in the world. In other words map correspondence does not just specify sequences of objects but also different modes of reference. Tarski's absolute account is deficient in this score because it specifies only one kind of referring: satisfaction by a sequence of objects, without specifying kinds of objects or ways of referring.

The distinction then between map-correspondence and Tarskian Correspondence is based on the distinction between differentiated vs. uniform reference. The Tarskian account of reference discriminates at most two structures in the referent: objects and order

(sequence). On the other hand even the most impoverished map contains at least three different modes of reference.¹¹ Consider the schematic map offered at some toll-booths as an example: it gives you the cities in sequence (objects and order like Tarskian correspondence) plus the existence of a road. Unless some such third kind of referent be given, the 'map' is not a map but a list. Even the verbal instructions given ("you will encounter a, b, c, d, e on the road") contains three kinds of referents plus the "you are here now" referent which is a fourth kind.

We can sum up the previous observations in the form of a principle about map-correspondence, and call it the Fundamental Map Principle (FMP):

Every map-correspondence contains at least three different modes of referring corresponding to different types of entities in the mapped.

On the basis of this principle we can set up an order of correspondence that starts with the list (orderless list) which contains only one mode of reference (numerical identity) and proceed through the sequence (objects and order) to the map (objects, order, plus a third kind of referent depending on the map) and from there on to more and more sophisticated models or maps. In its absolute form the Tarskian correspondence reaches the second type of correspondence. In the generalized form it can reach higher, but as we have argued, there are other reasons why the Realist would object to the generalized form of the TTT. It is not only the account of sciences as a Leerform

of connected predicates which he objects to, as we will see, but also his belief in one independent reality: he is found to choose the absolute TTT, yet it turns out to offer an impoverished correspondence.

If we agree as to the distinction between differentiated and uniform reference, then we have to ask whether the Realist is justified in considering science as involving the former than the latter. In other words how and why does the FMP apply to science? Let us begin by dispelling a possible misconception. The differentiated reference that the Realist requires is not an appeal to fact. If anything it is the Tarskian atomic sentences that come closer to facts than the Realist's differentiated referents. With this in mind let us now proceed to sketch the Realist's answer to the problem in two steps. In the first step he argues that language clearly contains differentiated reference and science in fact contains an even more highly differentiated reference which not only distinguishes modes of referring and kinds of referents, but also establishes interconnections between these many modes and these many kinds.

On the level of language differentiated reference is apparent in the theory of categories. I do not intend here to go into this vast philosophical problem; for my purposes it is significant to note that the history of the problem reveals results that are consistent with what I am proposing above. Whether one opts for the ten categories of Aristotle (which are in fact four plus one), or for the twelve categories of Kant (which are classified in four groups), or for the infinite categories of Ryle, or for the three categories

of Peirce, one has to recognize in the first place that the categories are not parts of the theory of meaning but of the theory of reference. Secondly, one has to recognize the consistency of the solutions with the FMP. Finally, even on a least sympathetic approach to categories (Quine's) which associates categories with grammaticality, the notion of category is connected with the syntax of a language. Using this observation we can see in what way linguistic reference is differentiated rather than uniform. The multiplicity of the syntactical structures guide the speaker into recognizing that there are different modes of referring to different kinds of things. In fact, one can even point to a very strong similarity with map correspondence: even when we do not know the meaning of a word, the syntactical structures in which it figures is a guide to understanding the kind of thing that is referred to. Of course, language is often confusing (in the sense that it leads to category mistakes) but the situation is easily controlled by considering more contexts or by shifting to a scientific language.

At this point one may seriously object that appeal to differentiated or "eidetic" reference (i.e., reference to kinds) is unwarranted and runs against the demand for simplicity: isn't it better to let only sequences of objects bear the weight of reference, while all the differentiation of reference can be expressed not by referential functions but by interrelation of predicates on the level of a theory. Why talk of kinds of entities when we can spell out kinds by predicates: after all, is there anything more to a kind than a

group of properties? The advantage of this countersuggestion is clear: ontological parsimony. All that we need in the world are objects, numerically distinct, which are arranged into sequences in accordance with whether they satisfy or not our interrelated predicates. Why force onto the world a more complex structure, especially since by the Ontological Relativity arguments (which rest on the Inscrutability of Reference) we know that such structures cannot be established in a theoretically satisfying manner? This powerful argument will occupy us later; for now we can say that it rests on a Traditional Theory of Reference according to which extension (reference) is established only by way of intension (meaning). Kripke in Naming and Necessity has argued successfully that this view of reference is erroneous and in this way has brought essentialism (and differentiated reference) back into the philosophical picture.

A second more significant reply is to point to scientific theories as best examples of differentiated reference. Even a cursory look at science reveals the multiplicity of referents that its various terms correspond to: objects, properties, magnitudes, fields, forces, directions, time, space, etc. The scientific formulas that express these entities do not only establish strict differentiations of kind but also establish strict interconnections between them, in such ways that new magnitudes can be generated on paper before they are even detected in the laboratory. In this way we distinguish velocity from acceleration from momentum from energy recognizing that each term refers in different ways to different objects in the world. Again

question of the physical counterpart, the referent, is vitally important. We do not rest with a set of equations describing how two objects act in a gravitational field. Instead we seek to uncover the nature of gravity, field, matter, etc. We also use referential considerations negatively, against theories that employ concepts whose reference is indeterminate: what are the 'stages' of Piaget ontologically? what kind of thing is a 'potential'? a disposition? and so on. Even in the case of non-exact sciences (like natural history) we employ differentiated reference by distinguishing species and genera plus other factors. The idea that science can be reduced to a set of interrelated predicates all on equal footing has gradually been phased out: After a long entanglement with Platonism and its troubles (philosophy of) science returns to its father Aristotle.

The above considerations, in our view is what bars the TTT from being a Realistic theory. Before expanding on this point it would be useful to put it in the proper frame with a brief summary of the steps that we have taken so far. We started with the problem "why reference matters to science" and for the most part we have produced negative conclusions. We examined the hypothesis that reference matters to science in exactly the way that Tarski's Theory of Truth says it does, because of the old claim that the aim of science is truth. We followed therefore, Popper's late suggestions that Realism is based on the Correspondence Theory of Truth, that Tarski 'rehabilitated'. We found that Popper's solution is based on a misinterpretation of Tarski. We then investigated, independently of Popper's

sentences. What has often been missed in this account is that science does not pose 'yes' or 'no' questions to Nature and Nature in its turn does not offer 'yes' 'no' or 'maybe' answers back. Instead both the success and the failures of science are instructive and lead to further research. This is due to the fact that science is a complex systematic map of the world in which different types of concepts refer differently to different types of entities. Verificationism manages to ignore this ontological aspect of science by placing it in the context of discovery. Falsificationism in its turn, despite its late interest in the Third World, again threw explanation into a similar fate: conjecture. The net outcome of both moves is that the ontological theoretical part of science is taken as either heuristic or as a mathematical calculus. In the first case, it is indeed either by psychology or history of ideas, in the second case, we have meta-theorems to show either that it is dispensable (Craig's Thesis) or that this axiomatic system does not really specify uniquely the objects that it is about. This makes reference of theories indeterminate as there is no fact of the matter to be right or wrong about. Coupled with this account of science is an uninteresting, formal account of explanation (Hempel's D-N model) which makes explanation virtually indistinguishable from prediction (again the old obsession with truth - only on pragmatic grounds can the two concepts be distinguished as Scheffer has shown).

The Verificationist Conception of Science, as we stated in the Introduction is under attack and is replaced by new conceptions of science still in the process of formulation. These new conceptions

are not unified except in two respects: opposition to the Positivist Model and the belief that science is ontological or at least has an important ontological component. In other words that science purports not to match or predict the truth of observation sentences but to give us a systematic and unified representation of the world. Beyond these two seminal ideas, the various approaches diverge: one road takes the systematic character of science and couples it with holistic epistemological considerations leading either to Relativism or through Relativism to new socialized, historical, etc. conceptions of science. The other road re-examines whatever is correct in the Positivist Theory of Scientific Method but from a Realistic standpoint and brings out the ontological aspects of the scientific method.

The best example in this second line of thought is the work (mostly unpublished) of R. Boyd. He manages to show that many of the traditional rules of the scientific methodology make little sense and are unusable unless they be supplemented with ontological considerations, that is to say, the realization that what science presupposes are a host of underlying entities and mechanisms to which the theoretical terms of typically mature science refer. In the introduction to his work, Boyd explains that the aim of his enterprise is to show primarily that epistemology cannot be divorced from metaphysics. The point is well taken. Our enterprise here, however, is to provide foundations for a Realistic Philosophy of Science by examining the Reference of Scientific Terms. Boyd takes the major tenet of

Scientific Realism to be that "theoretical terms of mature sciences refer." My point, on the other hand, is that the theoretical terms of science - as distinct from pseudoscience - refer eidetically. It is the spelling out of this eidetic reference that will guide our conception of science. But I am running ahead of my story.

What we have managed so far is to spell out in a fruitful way an old puzzle concerning Realism. Realists are supposed to accept the Correspondence Theory of Truth. Tarski's account of correspondence, at best, gave us the vague idea that language is about objects: this is what makes our utterances true or false. We have shown that this minimal explanation is inadequate to account for the Scientific interest in Truth as correspondence. The correspondence that the Realist has in mind is not explainable as blanket-satisfaction of predicates by sequences but rather in terms of differentiated eidetic reference. This eidetic reference is connected not only to scientific truth, but also to scientific explanation, since as I will argue later on, explanation proceeds by appealing to kinds of things. It would be helpful here to remember the map parallel: The eidetic reference of map-correspondence is what makes a map an instrument of understanding: it puts together a complete picture which can guide us to new places or even to old places in new ways or even help us decide to seek new places. Metaphors aside, we have reached a first determination of Scientific Realism and a glimpse of the reasons that makes it care for reference: reference matters to science because science is ontological.

The claim that science is ontological is only partially explained by the map-correspondence that I have developed above. Is science ontological and what does the characterization mean? In what specific ways is ontological science related to reference? I shall deal with the first question in the last part of this chapter. I shall begin the second chapter by considering the second question.

1.3 Science as Ontological

Many ways of characterizing science have been proposed, accompanied by criteria to be used in distinguishing it from pseudo-science and from other cognitive enterprises such as technical and practical knowledge, religion, ideology, and art. The most common difficulty that such attempts have run into is due to the fact that science, with its long history is too diverse an enterprise to be easily captured by one or two criteria. If they are not drawn strictly, such criteria fail to discriminate adequately and thus allow other enterprises to pass as science. On the other hand, the attempts to spell out these criteria formally and thus restrict their application result, more often than not, in keeping out of science proper large parts of it which have historically been taken as science. My aim in examining these characteristics is not to deal once again with the old problem of demarcation. In the context of the preceding discussion about reference, I propose that the key characteristic of science, which is often ignored, is its ontological nature. I will argue accordingly that without this characteristic the other characteristics

that have been proposed are inadequate for the purposes of distinguishing science from other cognitive enterprises.

There are two theses involved here, a weak thesis and a strong one. The weak thesis is that the ontological nature of science is the missing characteristic which together with the rest forms a jointly sufficient characterization of science. The strong thesis accepts the weak one and goes one step further by claiming that one needs to consider the ontological nature of science because it is the only way that the other characteristics that are vague can be properly spelled out and explained. Once the two theses are defended and explained, two roads are opened to our inquiry: in the first case it will be clear now why reference matters to science; the topic will be further pursued in Chapter II. Secondly, this exposition will be the first step in determining the conception of science that the Scientific Realist is proposing. The complete exposition of this conception is a task for the whole book.

What then distinguishes science from other cognitive enterprises? The first distinguishing mark of science that is proposed is its systematic nature. Leaving aside the vagueness of the term "systematic" for a moment, it is clear that the claim that what distinguishes science is its systematic nature falls short of the truth on two counts. It certainly is not a sufficient condition: many non-scientific or even pseudoscientific enterprises are systematic: astrology is the most common example. On the other hand, coming back to the vagueness of the term "systematic", if we specify a rigid criterion

of systematicity, such as an axiomatic structure that no astrologist would be capable of imitating (without giving up astrology for mathematics as a result) then we would have to exclude from science almost all of the history of science and a large part of what rightfully passes at present as science. Unless then the characteristic of the systematic nature of science is explained properly, it is neither necessary nor a sufficient distinguishing criterion.

At this point, it is customary to bring up the evidential sensitivity of science as a way of eliminating the pseudoscientists who have managed to imitate systematicity. Although we realize that science has theoretical problems both with verification and falsification (Hume and Duhem respectively), still we can say that science involves a deep commitment to objectivity, which is based on its sensitivity to evidence: failure to match the evidence or to predict accurately a further observation or the outcome of an experiment are taken seriously. This characteristic may effectively eliminate the most naive astrologist, yet faces problems of its own. Consider the case of common sense, of technical knowledge, and above all, of practical knowledge. The practical common lore knowledge of the illiterate old fisherman often allows him to predict the weather a lot more accurately than the meteorologist. I have personally encountered such a case of an old fisherman who by watching the midday shift in the winds, the evening appearance of the sea, and the clouds at the top of Mount Olympus was capable of outperforming in accuracy the official meteorological predictions.

Even though many philosophers of science would claim that the fisherman's knowledge is not to be sharply distinguished from the meteorologist's (both historically and up to a point formally) still I take it that some such distinction ought to exist. The normal way that such distinction was attempted was to add to science the characteristic of universality: The fisherman and every practical knower can even outpredict and outobserve the meteorologist but his knowledge covers only a small region (in the Aegean in our case). The meteorologist on the other hand, has universal knowledge. Again however the trouble with universality is vagueness. To some degree the practical knower has universal knowledge: the simple fact that he projects to the next day or that he remembers previous seasons etc. warrants the claim. If the criterion is drawn strictly as to dissociate universality from any mention of a particular region, or individual object, then even the meteorologist will find that his limitations (to Earth climate) would place him out of science. On the other hand, such a strict criterion would place even Kepler's Laws outside of science, clearly an undesired consequence.

To the above difficulties we have to add the problem of the sophisticated pseudoscientist: for example, the astrologist who claims his predictions to be of a statistical nature, and his willingness to revise his astrological laws in accordance with evidence. To counter both cases, one could propose as a criterion the explanation value of scientific theory. Despite the fisherman's good predictions he does not know why the rules he has devised to predict

the weather work. Again, even the astrologist who has consistently had good predictions does not deserve the title of "scientist" since his aim is not explanation: he can tell us nothing about why the course of the stars influence human lives. The trouble this time is that the concept of explanation in science is problematic. When we construct it along the formal D-N model, either we have to include the practical knower and 'his' laws or we have to spell out completely 'law-likeness', a task that has resisted analysis. On the other hand, we have a challenge here also from religion and ideology who do not find science to be explanatory enough. Finally if one seeks to block both the above cases (the practical knower and the believer-ideologue) by an appeal to the intuitive explanatoriness of science, the clarity and the plausibility of scientific explanations, then one faces a serious challenge from art. With respect to intuitive explanatoriness, a good case can be made that the account of emotions, or character that one finds in certain works of literature are more explanatory than the accounts provided by many a psychological theory. I take this challenge seriously, for one can see in the works of certain perceptive writers concrete accounts about what kind of life history will generate a certain kind of character or what actions and thoughts will generate what kinds of passions. The point here is that literary knowledge of man is not explanation just because it is 'deep', i.e., 'profound'. It can be taken as an extremely accurate and causally informative account. To put the matter bluntly, one learns a lot more about human nature from Chekhov or Dostoyefsky or Iris Murdoch

than from Skinner or Freud, and there is an intuitive feeling that the literary accounts are 'true' while about the scientific ones there is a lot of uncertainty.

Before proceeding to resolve this difficulty we should also mention another characteristic of science that is emphasized in Peirce's "The Fixation of Belief." In that work Peirce contrasts science as a method for reacting belief to other methods like the a priori method of philosophy, the tenacity of fanaticism, the method of authority, and finds that in the case of science, failure does not lead us to doubt the method, as in the case with all other methods. This characteristic of science, let's call it "shock-protectedness" (following Scheffler), while it does distinguish it from a lot of other cognitive enterprises (even from literature, as we often reject altogether a certain author's view of man on the basis of error- or growing up), still suffers from two ills. It is an appeal to subjective trust and in the absence of good reasons for it, it seems arbitrary. I have argued elsewhere ¹² that, from a pragmatist standpoint, the best explanation for the 'shock-protectedness' of the methodology of science is to be found in its systematicity: simply because it is the interconnectedness of science that allows us to learn from our failures. This may very well be, yet as we said in the treatment of systematicity, the criterion is rather vague. We need a better explanation.

We have listed five criteria-characteristics to distinguish science from its rivals: systematicity, evidentiality, universality,

explanatoriness, shock-protectedness of its method. We also found that these characteristics either individually or collectively are not adequate for the task. On this score the most common problem we faced was the vagueness of the characterization primarily and secondly their problematic nature: they have not yet been investigated fully. One would expect nevertheless that an adequate account of the Logic of Science would help make the criterial precise enough for the task. The most systematic effort here, the Positivist's enterprise has failed on this score. Are we to conclude together with Feyerabend that science is too diverse an enterprise to be captured by any methodology? too anarchistic to have a logic? Such claims cannot be established simply on the disparity between official methodology and actual practice: it is normal to expect such a disparity between normative and the descriptive. What the critics of the Logic of Science owe to us is to show that the existing analysis failed to capture a fundamental element, without which it is difficult to understand science. I propose then that the missing element in the general analysis is the ontological character of science. But let us examine first in what way this solves our problem of distinguishing science from the other cognitive enterprises.

The claim that science is ontological means loosely that science has as a basic commitment to uncover the nature of reality. This characterization is vague and contains a metaphor. Let us make it more explicit by applying it to the task at hand: how does it help to distinguish science from the other cognitive enterprises. We

have argued earlier that, as far as explanatoriness goes, a case can be made that literature, or at least some literature, can go a long way towards offering a good account of emotions or characters. On the other hand, the science of psychology, whether we call it young or old, is not only plagued by controversy, but has not yet offered a good account of complex situations involving human emotions. Despite the fact that Dostoyevsky, or Chekov, or Murdoch can tell us a lot about emotions, what is missing from their "explanations" is an ontological component: an account of what kind of thing an emotion or a character is. The scientific explanation, on the other hand, proceeds by way of considering the nature of the object at hand. A scientist of course may be a lot less sensitive to characters and emotions, and one may learn a lot more about man from "writers" rather than from the scientific account.¹³ However, the scientific account has independent validity, not because of its "accuracy", or objectivity, but rather because it explains by uncovering the nature of the object in question. As such then, it is to be preferred from the literary account because it is really informative (i.e., the explanans and the explanandum are essentially different) and allows for further progress: once we have explained the nature of X by showing it to be a Y kind of object, we must now explain the nature of Y, and so on.

This characteristic also helps us distinguish science from practical knowledge. The fisherman may be extremely sensitive to regularities in the weather but he knows nothing about the nature of the

weather: what is a cloud? and what is the wind? Moreover, his account does not proceed by way of ontology, but only by way of regularities. Finally, he does not even feel the need to examine the ontology of the terms of his account. Why should he investigate the nature of clouds? Similar things hold for technical knowledge: the engineer cares about the nature of cement only to the extent of distinguishing kinds of cements fitted for a special purpose.

At this point some thinkers may claim that we are unfair to the ancestry of science. Putting this possible objection in stronger form, it may be claimed that although a particular practical knower has no interest in the nature of the objects of his knowledge, this characterization is too vague to be of any value: for what is the nature of these objects anyway, and specifically, is there anything in the nature of the objects that cannot be captured by regularities? Although the fisherman does not care much about the nature of clouds and winds he should care enough to investigate his regularities further and come up with more precise knowledge of these regularities, in terms of more regularities or more adequately drawn regularities. The reason that should send him in search of more would presumably be certain failures of his old regularities: a more precise account would give him not only abilities to predict more precisely but also would help him recognize his old faults: did he take the effect as in fact a cause? Did he take two effects as cause-effect relation?

This objection is purely positivistic in nature: it brings out not only the connections between science and practical knowledge,

but it also brings forth the dependence of Positivism on the Humean analysis of causation: though science purports to uncover the nature of reality, in fact it does so by proposing laws. Laws in their turn, are indeed carefully drawn regularities (not essentially distinct from the regularities of practical or technical knowledge or even of art), which describe causal dependencies. Causes in their turn are to be analyzed as relations between occurrences connected temporally (A before B) spatially (some form of cognition), plus an inexplicable constant conjunction: "it always happens this way."

This objection misrepresents science in many ways. In the first place, the continuation of research is unaccountable by this model: once a regularity is discovered and established what is the use of investigating further. Secondly, there is no need for a unified science. If all we have is accurate laws describing regularities what is the point of reducing one science to the other. The ontological approach insists on the need for a unified science: either the objects of a science have to be shown to be reducible to another science or an accurate account of their emergence must be found. Thirdly, as is well known, true universal regularities do not capture the spirit of scientific laws: lawlikeness is a key ingredient which cannot be spelled out by the Humean account of causality. On the other hand, the causal necessity involved in law-likeness (i.e., counterfactual necessity) involves appeal if not to necessity or dispositions at least to underlying entities and structures.

The above three objections to the Positivistic account of laws are common, and the Positivists have sought to overcome them by recognizing a place for ontology: ontological models of underlying realities, etc. are useful as heuristic devices. My claim however, goes deeper: ontology figures not only in the discoveries of science but even in the setting up of sciences (for our purposes science is born, "spear, shield and helmet" out of the head of Aristotle). One would be in a difficult position to explain the existing sciences without appeal to ontology. Consider for example why there is no science of "big things" or science of "holes" (as McIntyre suggested). The only way available here is the Aristotelian account: only proper genera can become subject-matters for science. A 'big thing' is not a genus nor is a 'hole' a genus; both characterizations are ontologically dependent: a big what? a hole-in-what?

Ontology figures not only in the setting up of the sciences (i.e., in delimiting the subject-matters for the sciences) but in the important innovations that change the manner in which the science is pursued. Consider here the two great innovators in medicine: Claude Bernard and Louis Pasteur. In both cases what is achieved is not a more accurate description of regularities but a new ontological conception of the subject matter. In the Lecon sur le Diabete et la Glycogenese Animale, C. Bernard introduces the idea that the nature of a disease is not to be explained in ways that depart from the usual physiological accounts of the normal workings of an organism. Instead every disease is to be thought of as corresponding to a normal function that has been altered for some reason. This "homeostatic"

account changed medicine in all its aspects: its subject matter was seen now as not distinct from biology and its methods as well as its principles underwent a similar change. It is significant to note here that Bernard starts with an ontological discussion of the nature of diseases. Similar considerations are brought up in the work of Freud: Bernard's ontological conception of disease showed pathology and physiology to be essentially connected; Freud's account of mental disorder (as based on 'wish') destroys the sharp barrier between normal behavior and abnormal behavior, and leads to new types of research. Pasteur's account uncovers a new aspect of disease by showing an underlying mechanism to be at work in the original aetiology of disease.

In all these cases, especially in the case of Bernard and Freud and to a smaller degree in the case of Pasteur, what is at stake is not more regularities or more accurate regularities but radically new concepts of the subject matter at hand; the differences between pre-and post-Bernard medicine and pre- and post-Freud psychology are ontological in character: they have to do with the definition of disease, with a new conception of the nature of what the science is about. Of course, the new conception leads to new methods and principles for the science.

It would be wrong at this point to think that such ontological investigations take place only during scientific revolutions. Consider for example the recent accounts of the function of mitochondria. It has been suggested that these organelles are in fact hold-overs from bacterial invasions of primitive eukaryotic cells. This new

hypothesis is certainly not the beginning of a scientific revolution, yet it suggests a new conception as to the nature of cells, since the powerhouse of the cell, the mitochondrion, is now seen as an original "invader." Furthermore, the fact that the hypothesis is "historical", strictly speaking, does not make it irrelevant to present research: it allowed us to make an analogical inference that lead to a conception of chloroplasts in plant-cells in a similar way.¹⁴

Another objection to our conception of science as ontological would argue that not all scientists are aware of such an ontological commitment, while most of them are aware of the laws and regularities that have been discovered in the field. I do not wish to contest this claim, as I do not wish to contest the claim that many scientists are bad scientists. Especially in a field like psychology we often have rehashing of common-lore knowledge in fancy scientific terms. My point however, is that, even granting this unfortunate fact about the practice of science, still any scientific treatise affords more of an ontological insight into the nature of its object than either a technical manual or the rules of the practical knower or the novel of the sensitive writer. The reason is that all scientific treatises have to offer definitions, or other kinds of determinations (formulas, implicit definitions) of their fundamental concepts: these definitions or determinations spell out the nature of the objects in question; it is at this point that the ontological character of science is apparent.

The definitions to which we are appealing here should not be seen simply as stipulative ones, it would be better if we used the general term "scientific identifications." Their significance will become obvious as we proceed; for the time it is important to keep in mind that science proceeds to investigate objects in accordance with such identifications; a different kind of research is dictated when one defines disease as a "homeostatic recovery process due to a lesion" (Bernard), from the kind of research that is followed when one defines disease as "a function that leads to death." Similar considerations hold in case a phenomenon is identified as wave-phenomenon.

Only a complete account of the ontological conception of science could provide adequate explication of the other five characteristics that we listed. By way of a preliminary approximation we can say that under this new conception, explanation is not to be left on the level of universal statements describing regularities. The laws that these regularities describe are to be taken as causal laws, by appeal that is the underlying entities, mechanisms and processes. Similarly universality in the sciences is to be defined by appeal to the kinds of things that a science is about. Even more dramatic is the importance of the ontological conception for a correct understanding of the evidential aspect of theories. Here, the work of R. Boyd is crucial; to give one example, Boyd has argued that ontology figures in the testing of hypotheses because of the many possible variations that are to be tested in experiments we select the ones that our

ontological conception guides us to examine as the ones most likely to reveal errors in our theories.¹⁵

One last objection must be taken up before concluding this chapter. I loosely characterized the ontological aspect of science as its commitment to uncover the nature of reality. In this case, one may wonder, why not talk about the metaphysical aspect of science rather than the ontological? The disagreement here over the two rival characterizations is not simply a matter of choice of terminology. Some of the points I have made place science on either side, specifically the idea that different conceptions as to the nature of reality lead to different kinds of research. The difference in my view lies with how inclusive the ontological aspect is. A metaphysical outlook refers to a total view of the world: the total framework. Ontology on the other hand, takes a piecemeal approach and concentrates on specific terms. My explanation, that it is the key definitions and identifications in science that carry the ontological weight, should be sufficient here to distinguish what I have in mind. Of course it may very well be the case that the identifications that a scientist is proposing for his key terms are ultimately dependent on his metaphysical outlook, his way of seeing the world. Yet neither I nor the other scientists can do anything about this, nor do we have to.¹⁶ The ontological aspect of science is certainly public: the key-identifications are openly proposed and become the subject of disputation, clarification, and further research: as the example of Claude Bernard showed.

In the claim that science is metaphysical or ontological we have the agreement of many thinkers including Aristotle, the Relativists, the Naturalists, Quine, and in general the thinkers who see science and philosophy as continuous and who take philosophy as a theory not as an activity (a "know that" type of knowledge than a "know how" one whose aim is to offer a true theory). It is essential however that a distinction be drawn between the metaphysical and the ontological, not only because "metaphysics" is a bad word (which it is not) nor just because my aim is to differentiate my position here from the Relativistic (which it is). The distinction has wider value and can help us understand science.

The distinction ought to be drawn on two grounds. The first was already briefly mentioned. Ontology deals with the nature of reality on a piecemeal basis. It does not have to give a complete account of reality as metaphysics does. Frege for example is carrying out an ontological task when he seeks to define the nature of number; he does not, at the same time, have to offer a theory about time, or matter, or mind. A metaphysical system, contained say in a religion, has to account for all the elements that are talked about as parts of reality. But if this is so, what is to be said about the unity of science: doesn't it make our science from ontological metaphysical? This charge is avoided when we add to ontological science a second element connected with the first. In its determinations of relation science and ontology can tolerate uncertainty; these determinations are proposed often with the provision that further

determinations are in order. Metaphysics on the other hand has an air of finality about it.

I have already mentioned that according to Realism, ontology figures in science in the definitions and scientific identifications. It is clear that ontology thus operates in science in a few nodes of the system and in a piece-meal fashion. Moreover, such determinations can change and sometimes they do change. There can be progress in ontology partly because it is open to inter- and intra-theoretical criticism. In opposition to this, metaphysics does not admit of piecemeal critique, at least before interpretation. To understand what Hegel meant one has to cover the whole theory and to have a viable interpretation. It is still possible to attack the ontology of a theory or even a part of the ontology of a theory or the ontological implications of a single definition without having to share the opponents (or colleagues) total theory. The fight between Skinnerian Behaviorists and Classical Behaviorists can be seen as an ontological disagreement.

Of course, there is no denying that some scientists or philosophers who do engage in piecemeal scientific or ontological work have made commitments to wide-ranging philosophical movements like physicalism or naturalism or mechanism. Such metaphysical biases or preferences are not normally discussed as part of science but from an ontological point of view such general commitments matter only in the way they figure in our theory: in the way they contribute to our definitions or, in other words, in the way they lead to

characterizations of various entities: Aristotle, Freud, Skinner were all naturalists, yet for the ontologically minded philosopher what is significant has to do with the different psychological theories they produced, and it is here that he can begin his work.

This latter consideration shows the intimate connections between ontology and method. Methodological discussions are based on ontological considerations since we select our methodological nets by virtue of the kinds of fish we expect to catch. An excellent example of the ontological metaphysical distinction and of the significance of the first for methodology is contained in the opening lines of B.F. Skinner's "Behaviourism at 50":

Behaviorism... is not the scientific study of behavior but a philosophy of science concerned with the subject matter and methods of psychology... The basic issue is not the nature of the stuff of which the world is made or whether it is made of one stuff or two but rather the dimensions of the things studied by psychology and the methods relevant to them. [in Wann, ed. 1964, p. 79]

The distinction I am drawing is not the vague and grotesquely value-infested distinction between descriptive and revisionary metaphysics that P.F. Strawson draws ("Descriptive metaphysics is content to describe the actual structure of our thought about the world, revisionary metaphysics is concerned to produce a better structure"). Instead I am concerned to delineate within science the areas where considerations of the nature of the object under study enter not only as abstract beliefs but as subjects of real disputation that affects

scientific research immediately. There are instances where ontologically minded thinkers have tended to blur the distinction as Quine does in the "Two Dogmas of Empiricism" where he places the 'Gods of Olympus' on the same footing as the theoretical entities of modern physics, yet I believe that this is due to rhetorical exaggeration. At this point Quine is interested in arguing (contra Carnap) for the continuity of science and philosophy by considering the "metaphysical" aspect of the former. In other writings Quine is clear as to his ontological commitments (and very reasonable about them).

If the reader is still unconvinced and detects a lot of grey area between metaphysics and ontology let him consider a weak pragmatic argument, connected with our opposition to Relativism. Contrary to the anti-metaphysical and anti-ontological Positivists, true Relativists have argued for this additional aspect of science but they hurried to call it metaphysical by emphasizing that in scientific revolutions we have "complete changes in the way of seeing old regularities" [Toulmin 1960, p. 116 ff]. They have even used charged words such as "conversion", "propaganda", "Gestalt-switch" to emphasize the fact that what is involved is a total metaphysical outlook. This however is unfair to the ontological aspect of science which is contained in the key identifications of the sciences and which is clear enough to become the subject of intelligent disputation. As I will show in the next chapter, it is these key-identifications that show the importance of reference for science.

CHAPTER II

REFERENCE, SCIENCE AND RELATIVISM

In the previous chapter we asked the question "why does reference matter to science?" and we answered it by arguing that reference matters to science because, according to the Scientific Realist, science is ontological. We shall begin this chapter by asking the converse question "Why does science matter to reference?" The reasons for asking this question are not stylistic. In modern discussions, the problem of reference is treated as an independent problem of semantics: how to best account for certain oddities that Frege and Russell pointed out in relation to reference. What is missing from this account, in my view, is not to be found in the search for an adequate semantic theory but rather in the attempt to understand science. I shall accordingly devote the first part of this chapter in an attempt to isolate the aspects of the traditional problem of reference that have to do with philosophy of science.

Closely connected with the above aspect of the problem of reference is another all important aspect that is connected to the core of our argument. We claimed that Relativism is based on the Theory of Reference that is no longer tenable. I shall devote the second part of this chapter to making the above claim explicit. This will not only facilitate the main contention of my dissertation (Chapters III and IV), but it will also show the Relativists as the latest

offshoots of a rich tradition that reaches all the way to Kant through the philosopher-scientists of the turn of the century. I shall show that for Frege, Russell, Weyl, Poincaré, and even Kant the problem of scientific objectivity and the problem of reference were one problem.

Finally, the whole chapter must be thought of as another stone in the construction of a Realistic Theory of Science which will spell out the ontological conception of science.

2.1. The Traditional Problem of Reference and Its Connections to Science

With the possible exception of Kripke and Putnam, contemporary treatments of the problem of reference take it primarily as a Twentieth Century philosophy of language problem. Specifically, the problem of reference is treated as a group of puzzles that stand at the heart of constructing an adequate semantic theory. These puzzles are Frege's puzzles of identity and Russell's puzzles of non-existence. More specifically Frege's puzzle concerned the difference between "The Morning Star is the Morning Star" and "The Morning Star is the Evening Star". Russell's main puzzle had to do with the problem of the truth-value and meaningfulness of statements about non-existent entities such as "The present King of France is bald" and its negation; he also dealt with a variant of Frege's problem which involved identity in intensional contexts: the difference between "King George wanted to know whether Scott was the author of *Waverly*" and "King

George wanted to know whether Scott was Scott." Finally Russell considered also the paradoxes associated with statements of existence and especially non-existence: how can a statement of non-existence be true and significant?

For a lot of writers these four paradoxes or variants on them defined the problematique of reference. Substantive contributions to the problem often offered new solutions or disputed old solutions. Some more ambitious works took a view of the larger problematic framework (Strawson 1959) and tried to arrive at a better theory of the reference of individual terms: names and descriptions. Survey works limited themselves to anthologizing the previously mentioned articles (Rosenberg & Travis, 1979; Copi & Gould 1967). Other survey works traced the solutions of the paradoxes from Frege onward. A paradigmatic case here is Linsky who in all three of his books on Reference took the four paradoxes as the key to understanding the problem, and even in his post-Kripke work he took the adequate solutions to the four paradoxes as the sole criterion of an adequate theory of Reference (1976, p. 11, 1977, introduction, p. xv).

There is no denying that paradoxes in philosophy are important ways of signaling that a problematic concept is used, and of inviting philosophical analysis for the said concept. They can also serve as criteria for an adequate analysis of the concept. Nevertheless, one ought not to lose sight of the relevance of these paradoxes with respect to the main problem. Why is reference a problem? Why is it a philosophical problem?

On this score contemporary treatments took a limited view. They took the problem of reference as a problem of constructing an adequate theory of semantics. Of course, one might object here that this task belongs to the linguists, yet two responses were quickly proposed. A school close to Davidson would claim that it is philosophy's task to offer a good semantic theory [Davidson 1979, p. 242]. Carnap's school would distinguish pure semantics from empirical semantics [1942, p. 12] and argue that since the time of Tarski the development of pure semantic theory should fall into the hands of the philosopher. Other schools offer variations of these basic themes, such as the British School of Ordinary Language who have carved for themselves the "formal" aspects of the third division of Semantics: Pragmatics.

What is the problem then according to these schools? Since the time of Wittgenstein we were made aware that language matters to philosophy. Linguistics, and especially its fully theoretical part Semiotics, is the systematic effort of approaching language. The philosophers required a good theory in semantics that would encompass all parts of the use of symbols: syntax, semantics, and pragmatic use. Of the three parts the area of semantics appears to be the most problematic; it is divided into a theory of meaning (on intension) and a theory of reference (extension) in accordance with the old triad: symbol-concept-object. One would expect out of a theory of semantics to offer an account of meaningfulness, of sameness of meaning, of truth, of sameness of reference, of how symbols connect to significant statements that can be true, etc. Of the two candidates for study, meaning and reference, it was thought that our best approach would

be to systematize the latter: two reasons here were obvious: meaning, previously known as "connotation", was largely dependent on an individual's history of learning the language: it is a triviality to say that different words have different connotations for different people. Reference on the other hand, being just the relations between symbol and object was thought to offer a more objective footing for a start. As a matter of fact it was often thought that reference could be used to explain meaning as well: no matter in what ways different people come to learn words, still language is about our encounter with the world. The road of referential semantics then was preferred though not without objections. Both Carnap and Frege thought that meaning was objective and not a subject for psychology. Later investigations however by Quine showed the road of meaning as hopelessly entangled with mentalism and indeterminacy, and thus initially vindicated the road of reference. Concerning a theory of reference it made sense, methodologically, to start with the simplest cases of denotation and proceed carefully to the more complex ones. We knew that since the times of Plato that the denotation of predicates is problematic. It seemed better to start with the simple theory of naming, the relations between a name or a description and the object named; and it is here that our problems began. The four paradoxes of reference stood as a stumbling block in our first step: unless these were solved, one could not expect to proceed further.

The previous considerations show the general problematique behind the problem of reference and specifically the obsession with the paradoxes. It is my contention that these do not really capture the

full-fledged problem that Frege and Russell were facing. I am not denying that the development of Referential Semantics is of great importance to us and to Frege and Russell, but they are not the whole story. Specifically they do not show the connections between the problem of reference and the philosophy of science which in my view were the major motivation behind the Frege-Russell problem. Unless these are brought up one cannot understand both the significance of the modern developments (post Kripke) and the connections between reference and Relativism both in the works of the Kuhn-Feyerabend-Toulmin-Hanson group and in the works of Quine as well as in the works of the precursors of modern Relativism: Poincaré & Weyl.

It may be protested here that some of the contemporary accounts of the problem of reference go back to Brentano and Meinong. Yet they only do this in order to show that Russell changed his mind about reference and about philosophy in general between 1900-1903 (the time of writing The Principles of Mathematics) and 1905 (On Denoting).

My aim is to show that the problematique of reference initially involved the problem of objectivity for formalized science, and I will argue this point by appealing to Frege's writings on Definition and his disagreements with Hilbert on the Foundations of Geometry. In the case of Russell one has to point out the many places (besides "On Denoting") in which the Theory of Descriptions figures especially his Introduction to Mathematical Philosophy and the two passages in the Principia (Chapter of the Introduction and the Theorems of Chapter 14). We will have opportunity to talk about the Theory of Reference

(Frege-Russell-Strawson-Wittgenstein-Searle) with the Causal Theory of Reference, (Kripke-Putnam-Donnellan) thus we will concentrate here more on Frege and less on Russell.

We claimed that the construction of Referential Semantics does not exhaust Frege's and Russell's Problematique. How much of Frege's and Russell's problem belongs to the development of an adequate Referential Semantics? There is no denying that both thinkers cared much about philosophy of language. Their investigations into formal logic had placed the taken-for-granted concept of reference into a new problematic light. Frege's "On Sense and Reference" starts with the problem of identity statements (how can they be both true and significant) but proceeds to a semantic solution that distinguishes sense and reference: the truth of an identity statement has to do with sameness of reference (or extension); their informativeness has to do with the differences in sense between the two names connected by the identity sign. This semantic solution goes deeper. Frege concerns himself not only with singular terms but with predicate terms and with sentences. Predicates are defined as "unsaturated" expressions which when provided with referring singular terms result in sentences whose reference is The True (Reality) and whose sense is a complete 'thought'. One should try to avoid reading psychologism here (if he can) and concentrate on the fact that we have here the beginnings of a semantic theory. Not only an account of reference, but even an account of how different words come together to form a sentence. Yet the judgment here is premature: the account is not

only obscure but even circular: if the aim is to show how sentences can arise, out of words, such that can be true or false and contain a complete thought, it fails because the definition of "unsaturated" includes statements to begin with: a predicate is unsaturated because alone it does not make a sentence. There are other arguments, offered by Davidson,¹ about the inadequacy of this approach. Russell's dealings with the problem can be equally considered as a significant attempt to deal with the project of Referential Semantics. Let's take one of his puzzles to illustrate the point: according to Russell a significant statement has to be about something. The term which carries this function is aptly called the "subject" of a sentence. Russell's problem with statements of inexistence is how can we have a true such statement since what it is about does not exist: the statement "Pegasus does not exist" is on the one hand about Pegasus and also about nothing: this shows that if it is true it must lack significance (about nothing) and if significant (about something) then false. Pre-1905 Russell took reference in a mentalistic way borrowing from Meinong and Brentano's idea of intensional inexistence and object-directedness of mental phenomena. Russell's 1905 theory analyzed the problem away, since the denoting expressions of the form 'the so-and-so', and many names (which in fact were descriptions in disguise) were shown to be not proper subjects but fake subjects to be analyzed away in-context, by a conjunction of two separate sentences: one affirming the existence of an object another affirming the uniqueness of it. This presented no problem as both characterizations were

parts of logical discourse, not predicates of their own: uniqueness is shown by identity and existence by the existential quantifier. Russell later introduces in the Principia the 'E!' predicate of existence but this cannot be taken as a regular synthetic predicate: it is obvious in the Principia that it follows from every predication since it is a theorem that:

$$\phi[(\lambda x)\psi x] \supset . E!(\lambda x)\psi x$$

Frege's view though not exactly identical is roughly similar: as it is obvious in his correspondence with Hilbert he takes existence not as a predicate but as a kind of meta-predicate, a second order predicate which states that a certain predicate (or concept for him) has instances.

Russell's theory cannot be taken as the basis of a viable semantics because in the first place it was not proposed as universal. Russell still maintained that some names were logically proper names while others were descriptions-in-disguise: much of it depended on the specific speaker and his history of acquaintance with the world. Secondly, Russell's account does not seek to explain predication or how sentences are formed out of referring and non-referring parts. Later in his career Russell tried a Wittgensteinian approach of speaking about facts, and even maintained a place for universals like 'similarity' in order to make sense of predication.

In the meantime, Frege's semantical ideas were superseded by Carnap in his works of semantics in the 40's. Carnap managed to capture Frege's distinction between sense and reference. He does so

by developing a general theory of intension and extension. Specifically, Carnap defined identity conditions for both extension and intension for certain limited languages, for which it is possible to enumerate their state-descriptions. Using this appeal to Leibnizian possible worlds, Carnap was able to define equivalence and L-equivalence and these in turn can establish identity of extension and identity of intension respectively. This theory allows Carnap to capture an important aspect of the Fregean concept of 'sense': the asymmetry between identity of reference and identity of sense: the latter implies the former but not vice-versa.

The above theory captures most of Frege's secure contributions to semantics. There has been, of course a lot of scholarly interest in Fregean semantics especially his conception of sense, in the hopes of discovering the seeds of a theory that would give us a good formal account of meaning. The reasons for this hope are obvious. Frege rejected the psychologistic accounts of sense and maintained to the end that senses were objective. For our purposes, we need to take note of one important aspect of sense that captures the "objective" aspect of sense. Frege notes that in cases of oratio obliqua (indirect speech) it is the customary sense of a word or an expression that becomes its reference. The argument for this position is not difficult to furnish, in fact it is based on intersubstitutivity. When reporting the opinions or beliefs of others, while substitution of terms by other terms with the same reference will not preserve the truth of the whole statement, a substitution with a term that

has the same sense would do so. Take for instance the statement 'John believes that the Capital of Argentina is a port'. Replacing the expression 'the capital of Argentina' with expressions with identical reference such as 'Buenos Aires' or 'Borges' city' will not preserve the truth of the statement. But if we replaced it with 'the city where the government of Argentina resides', which has the same sense as 'the capital of Argentina', truth is preserved.

I wish to focus attention on two implications of the above position for reasons that I will make explicit later. In the first place, Fregean sense is objective. This is born out not only by the sameness of sense but by our normal expectation that sense should have a subjective component. I explain: concerning the example above, one would expect that identity of sense is in fact appeal to synonymy i.e., sameness of meaning; and furthermore than synonymy requires, in this example, an appeal to what the speaker believes. Yet Frege is categoric on this point. His intersubstitutivity of sense is not predicated on a speaker's understanding of expressions. This is a very strong statement about the objectivity of sense. Senses for Frege are objects about which identity can be decided, not by appeal to one's perception of identity. The obvious objections can be for the time brushed aside by appeals to the vagaries of ordinary language as opposed to the ideal of a carefully regimented logical language.

The second important point is the idea that identity is handled by appeal to reference. Notice that Frege is not speaking just of identity of sense but is adding that in indirect speech ordinary

sense becomes reference. In other words, he believes that the criterion of intersubstitutivity (salva veritate) is a referential criterion. He is following here same Leibnizian principles, yet it is significant because there was a way available that follows a different route: to argue, namely, for a new criterion of intersubstitutivity of sense defining it for oratio obliqua alone: "two expressions have the same sense if and only if they are intersubstitutable without change of truth value in cases of oratio obliqua." Instead he choses to stay within the traditional road of reference and claims that sense becomes reference.

These points are important to our general argument because of the bearing that they have on Frege's account of definition. It is here, I believe, where reference matters for Frege and for the philosophers of science. To put the matter into an even more general frame, I claimed earlier that the problem of reference is erroneously limited to the problematique of constructing an adequate theory of Referential Semantics, while in fact the real problem for Frege and Russell, who initiated our concerns with reference, was related in an immediate way to the sciences. Specifically I intend to show these connections by focusing on the connections between reference, formalism, definition, and objectivity as they figure in Frege's thought as well as in the thought of his contemporaries.

Let us take a second look at his paradox of reference: how can identity statements be both informative and true (which is the central problem of "Über Sinn und Bedeutung")? I claimed that Frege should

not be seen as just the originator of one school of linguistic philosophy that runs through the Twentieth Century. Specifically, Frege himself stands as the intellectual heir to a rich German Tradition that runs back to Leibniz and Kant. With respect to Kant, his theories were not only targets for Frege (the Grundlagen attack on the synthetic a priori of Mathematics), but also set up important parameters for the problematique of Frege. I have argued elsewhere² that the best way of reading Kant reveals that for him the central problem of objectivity of knowledge is in fact a problem of objecthood (i.e., no objectivity without an object), and furthermore that objecthood is based on categories (forms of judgments) that are indeed referential functions. Frege takes this point one step further: objecthood is analyzable in terms of identity: "no identity conditions, no object."

The other side of Frege is this most celebrated contribution: the logical system of the Begriffsschrift. There are two aspects of this contribution that must be emphasized here. First and foremost is the idea of a complete formalization of all science (not just of some sciences) since logic now could be formalizable in an axiomatic Leerform. The word "Leerform" here is important since the emptiness of the formalization must somehow come to terms with the objecthood (objectivity) of science: i.e., what science is about. The second aspect is that the discovery of a logical form underlying ordinary language in ways that were not obvious shows reference to be a problem: if the statement 'All men are mortal' is not about men but instead

an 'if... then...' statement about variables, this shows that our simplistic views of reference are not warranted.

These two aspects and the concern with identity come together in Frege's problem of definitions. Science must be both formal and about the world: this 'aboutness' must be spelled out formally as well, if it is to be part of science. Identity, a formal function, is the key to objecthood, yet identity is problematic. Definitions are what connects science to the world (thus they cannot be true or false) but strangely enough they are statements of identity. Moreover, identities are discovered, they are not just found in dictionaries.

Frege's solution to the puzzle in "Über Sinn und Bedeutung" is to distinguish sense and reference. The statement "The Morning Star is the Evening Star" is an identity statement but it is different from 'a = a' or 'The Morning Star is the Morning Star' because the two terms 'The Morning Star' and 'The Evening Star' have different senses. Yet there is a problem that arises here. The discovery involved in 'The Morning Star is the Evening Star' is a discovery about reference primarily and only secondarily about sense. Presumably, what we found out was that we were referring to the same object by two separate terms. As for sense we argued earlier that in Frege's view sense is objective and recognizable by any speaker since it is substitutable in oratio obliqua. So what has sense got to do with anything concerning identity? There are two ways out here: the first is to claim that senses can change as we learn more so that scientific discoveries become part of the sense of a word. This way out is

certainly viable yet there are marked difficulties: in the first place it does not appear to be too Fregean; according to Frege senses are not only objective (concepts) but are also recognizable as such because as we said in oratio obliqua, they are substitutable salva veritate. Furthermore, to each pupil that learns that "The Morning Star is the same as the Evening Star" the revelation is not a piece of lexicography but a scientific discovery: and it is about reference: the star which we saw at dusk last night is the same object as the star that we saw three months ago in the morning. To say that the informativeness of an identity statement is due to the different senses is erroneous.

The second way out which in my view is closer to what Frege had in mind can be best understood by comparing the following three identities:

- (1) "Mary's father is Mary's father"
- (2) "Mary's father is the man whose daughter is Mary"
- (3) "Mary's father is the first violin in the B.S.O."

In case (1) the two sides of the identity have the same reference and the same sense, but trivially so: we learn nothing from it. The second case is an identity between expressions with the same reference and the same sense. One could argue that it is trivial but still it is not as trivial as (1): it tells us something about language: specifically about the synonymy of the expressions 'x is the father of y' 'y is the daughter of x'. It is case (3) that attracts Frege. Let us contrast it to case (2). The main difference here is not just

that 'Mary's father' has a different sense from 'the first violin in the B.S.O.' but that the informativeness of the latter helps us identify, i.e., establish the reference of 'Mary's father'. Case (2) on the other hand tells us nothing about reference. To say then that the informativeness of an identity statement consists in identifying two senses that are different is misleading. What is informative about an identity is that what is identified is the reference. Yet senses are important in identity, not in themselves, but as keys to reference: if people did not know the sense of 'the first violin in the B.S.O.' statement (3) would not help them in learning about the reference that the identity (3) is establishing. People have interpreted Frege's point to be just that there is an asymmetry between reference and sense since expressions with the same sense have the same reference but expressions with the same reference do not have the same sense. From this it was only a short step to saying that identity statements are informative because they identify two different senses. The first step here is only a necessary, not a sufficient condition. The second however, is misleading, and should be eliminated. In its place we ought to say that identities of the form (3) are important because they establish reference (identity of reference) but also that for any word to have reference it must also have sense that is understood. Hence the birth of the Fregean Dogma: "Reference is established through sense: one cannot establish the reference of a term without first knowing its sense."

Why call it a "dogma" when it is as common sense as daylight? Consider its implications when applied to proper names: if in order to have reference a term must have a clearly understood sense, then all proper names cannot have reference unless they have sense. Given that for Frege sense is objective, and not just a set of random associations on the part of a speaker, it follows that proper names must have 'meanings'. Here however we have J.S. Mill's established position that proper names do not have connotation but only denotation. The point is significant here because Russell is continuing in the Fregean line trying to find meanings for proper names while Kripke returns to Mill and attacks the whole Fregean Dogma.

In the case of science however, proper names have a minimal (if not null) role to play. Instead scientific identities (identifications) hold normally between general terms which according to tradition running back to Plato, have meanings and thus their reference is accessible through identities. Identity figures in science primarily by way of definition. It is here that our conclusions about Frege's solution matter because Frege sees definition not as establishing identity between two different senses (this would be a task for lexicography) but as establishing identity of reference between two terms whose reference was so far taken as different due to differences in sense. In other words, for Frege, definitions are not just stipulations about meanings. Their importance lies on the fact that they identify the referents: definitions for Frege are real definitions:

for Frege are real definitions: they are supposed to reach out and hit the object, so to say, they determine extensions or they determine the objects.

To understand the importance of this point for Frege one should read his correspondence with Hilbert - which we shall go into. Yet even before this correspondence, which comes late in Frege's career, one can see the importance of definitions for Frege by looking at the Grundgesetze der Arithmetik. Frege takes an ontological view of science and in this case mathematics: unless it (mathematics) is able to define objects about which it is, it is about nothing, and thus it is not an objective science. Many people miss the point in Frege because they are misled by history books which claim, correctly yet inadequately, that Frege reduced arithmetic to logic, and since logic has no subject matter therefore... arithmetic is about relations of ideas. This is a mistake both for Frege and for Russell: logic is about the world and has Truth as its subject matter (see Russell 1917, p. 169 and Frege in Klemke 1968, p. 507). Truth in its turn, for Frege is not just a predicate of a metalanguage, but is a synonym for Reality and is spelled out by way of objects to which the terms of sentences refer (Grundgesetze p. 51). For mathematics, then, there must be mathematical objects, otherwise it is not a science. This point is very clear in Frege's response to Russell's Paradox:

And even now I do not see how arithmetic can be scientifically founded, how numbers can be conceived as logical objects and brought under

study, unless we are allowed - at least conditionally - the transition from a concept to its extension. Is it always permissible to speak of the extension of a concept, of a class? And if not, how do we recognize the exceptional cases? Can we always infer from the extension of one concept's coinciding with that of a second, that every object which falls under the first concept also falls under the second? These are the questions raised by Mr. Russell's communication. ... It is not just a matter of my particular method of laying the foundations, but of whether a logical foundation for arithmetic is possible at all.

[Grundgesetze, Vol II, Appendix p. 254]

His problem then is whether, after Russell's paradox we can consider classes as objects any more, because his own definition of number that utilized "classes of classes" led to paradox. Remember also here that for Frege identity is the criterion of objecthood: later in the passage he considers the possibility of altering identity but rejects the idea:

We might try to escape this by assuming that a special sort of identity for improper objects, but that is completely ruled out; identity is a relation given to us in so specific a form that it is unconceivable that various kinds of it should occur."

[Grundgesetze, Vol II, Appendix p. 254]

and finally his suggestion:

There is no alternative at all but to recognize the extensions of concepts, or classes, as objects

in the full and proper sense of the word, while conceding that our interpretation hitherto of the words "extension of a concept" is in need of correction

[Grundgesetze, Vol II, Appendix p. 254]

One can see then the significance of defining 'number' for Frege. He is not merely interested in an axiomatic of arithmetic but in establishing a correct theory of 'number': he seeks to uncover the nature of number and unless he can show that his system properly determines its objects, he believes that the whole edifice collapses since it is about nothing. Why is this a problem?

To understand the problematic force of the above claims one should examine Frege's long fight against Formalism. From our standpoint the controversy is significant for two reasons: first because it shows the important connection between science and reference with regard to objectivity and secondly because aspects of this controversy are implicit in the Relativistic Argument.

Frege's disagreements with Formalism have a strong dosage of irony in them because it was Frege's work that contributed most to the development of Formalism. In some way then Frege ended up fighting against what he had helped to create, taking it for a monster. Metaphors aside, what I mean to emphasize is that Frege himself was clearly ambivalent about formal aspects of science. We know that he required of his science to be axiomatic and at the same time to be about the world, about objects. And here comes the problem: is this latter part of science (the aboutness, the reference to objects) to be

handled so, then it could be shown, and was shown, that a formal system cannot really determine its reference; at best it can determine its objects only up to an isomorphism between models that interpret the system, and even this case represents a rather uninteresting optimum, because categoricity has completeness as a necessary condition. Frege's troubles with this problem can be seen first in his "Review of Husserl's Philosophie der Arithmetik":

The objection that it is not the concept but its extension that is defined really touches all mathematical definitions. For the mathematician it is no more right and no more wrong to define a conic section as the line of intersection of a plane with the surface of a circular cone than to define it as a plane curve with an equation of the second degree in Cartesian co-ordinates. Which definition he chooses - one of these two, or some other again - depends entirely on reasons of convenience, although these expressions neither have the same sense, nor evoke the same images. I do not mean by this that a concept and its extension are one and the same; but coincidence in extension is a necessary and sufficient condition for the occurrence between concepts of the relation corresponding to identity between objects [because identity property speaking does not apply to concepts: cf. my essay "On Concept and Object"].

[Greach & Black ed. p. 80]

and he continues that he agrees with Husserl that the definition of identity as substitutivity salva veritate is not a definition but for different reasons:

my reasons however are different. Since definition
is an identity, identity itself cannot be defined.
 [ibid. Underlinings mine]

Frege's point then, is that definition's main aim is to establish identity of referents, and not identity of senses. On this point, translators and interpreters often go astray because of the previously mentioned confusion about sense and reference and because of a passage in the Grundgesetze which stipulates that a proper definition should be an identity between expressions that have both the same sense and the same denotation [Grundgesetze Vol. I, Section 27, p. 45]. Yet in this case Frege is talking about the introduction of a novel term. In other places he limits the identity of a definition to reference, yet both Furth and Austin insist on translating "Gleichbedeutend" as 'same in meaning' rather than 'same in reference'. The point is especially crucial in § 33 which is entitled Principles of Definition.

According to Frege then it is definitions which establish the reference of a theory or of a science in general. His requirements concerning definition however bring him in opposition to formalism. Specifically, Frege requires that a definition should completely determine the reference of a term so that one can decide on the basis of the definition alone whether an object falls under the concept or not. This is his completeness requirement mentioned in Grundgesetze, Vol I, p. 51 but explicitly stated in Vol II, § 56:

A definition of a concept (of a possible predicate) must be complete; it must unambiguously determine, as regards any object whether or not it falls under the concept" (*italics mine*)

This sets him in a collision course with both the formalists and the constructivists. As he notes in § 57

It follows that the mathematicians' favourite procedure, piecemeal definition, is inadmissible.

The attack here is against the implicit definitions that were in favor since Gergone: this is precisely his point of disagreement with Hilbert's axioms. His ambivalence however is surfacing again in a passage that runs counter to his critique of Husserl, mentioned earlier:

...we may define a conic section as the intersection of a plane... once we have done this we may not define it over again et. as a curve whose equation in Cartesian co-ordinates is of second degree.
 [§ 58]

He has similar objections against creative definition which is favored among the constructivists [Grundgesetze, Vol. I, § 139-144]. Consider here for purposes of contrast Weyl's account of creative definition:

For the mathematician it is irrelevant what circles are. It is of importance only to know in what manner a circle may be given (namely by O and A - for a circle about O through A) and what is meant by saying that a point P lies on a circle thus given.

[1949, p. 8]

Frege's specific objections against the constructivists' creative definition is that the only limitation by which we can proceed in construction: namely the requirement that the properties of the object to be considered not be mutually inconsistent, cannot be satisfied unless we already presuppose the object: for there is no other criterion of consistency of such properties but to find an object that possesses all of them: "but if they can do that, they need not first construct an object" [Grundgesetze, Vol II, § 143].

Frege's disagreements with the Formalists shows clearly the importance that he attached to definitions in science as keys to its objectivity. Frege maintains that the primary function of definitions (a task that cannot be carried out by any other part of theory) is to establish the reference of the terms of the theory. Without such a determination of reference the statements involving the term in question lack sense and cannot be called true or false [First Letter to Hilbert in Kluge ed., p. 8]. Frege demands then that science should be formal but at the same time it should uniquely determine its object. This demand brings him in direct disagreement with Hilbert and Korselt (who defended Hilbert's formalistic account). Frege believed

furthermore, that this unique determination of the reference of a theory should be part of the formal structures, as definitions were supposed to be used for proofs in the formal system. Frege's charges against the formalists can be summarized as follows: Hilbert and Korselt confuse axioms and definitions and offer unacceptable accounts of both. Their theory if taken as true, has to tacitly presuppose an informal theory of objects, otherwise one would not know the truth of axioms. If on the other hand, one insists that no such previous reference is to be part of the formal theory, then the formal theory turns out to be about nothing and thus it is not only not-objective but even meaningless. At any rate there is a serious confusion in the Formalistic Theories because they mix parts of two ontologically distinct theories: a first level theory which contains first level concepts, such as lines and points, which subsume objects, and a second level theory about second level concepts, such as existence and independence, which can properly subsume only first level concepts. The above summarizes Frege's attack on Hilbert and Korselt. We need now to go into the specifics of his attack and we shall separate the examination into two parts: at first we shall substantiate our claims about Frege's views on definition, we shall then proceed to discuss the connected topic of the objectivity of science as it figures in the disagreement. In both parts our aim is to show the key role that is played by reference.

There can be little doubt that the sole purpose of a definition for Frege is to establish the reference of a sign. The usual traditional claim that to define is to allow us to eliminate by substitution of synonyms (definire est eliminare) is only the result of the primary function that establishes reference. Definitions can do this because they are identities and thus they can establish reference. Only if reference is identical can substitution be effected. This is certainly a controversial view but Frege is categorical on this score.

Any definition must... contain a single sign
[to be defined], and fix the reference of this
sign.

[Grundgesetze v.ii in Black & Geach, 171].

Every definition contains a sign (expression
word) which previously has had no reference and
which is given a reference only through this definition.
[First Letter to Hilbert, in Kluge p. 7].

The same point is almost verbatim repeated in his Paper on Geometry
[Kluge, p. 23]. Also in discussing explications Frege writes:

They resemble definitions in that in their case too
what is at issue is the stipulation of a reference
of a word (sign).

[ibid. p. 8]

Again, in discussing axioms and theorems he writes:

To definitions that stipulate something I oppose
principles and theorems that assert something.
The former contain a sign that is still to receive
a reference by means of them; the latter contain
no such sign.

[Foundations of Geometry in Kluge p. 50-51]

His objection then to Hilbert's Foundations of Geometry is that
the axioms of Hilbert cannot define the concepts that they are
supposed to establish because

Hilbert's so-called definitions do not give references
to these words.

[Foundations of Geometry, in Kluge p. 80]

Hilbert's reply on the matter is that any search for definition
of fundamental concepts of a theory would be pointless, beyond
examining the axioms that are true of these objects:

If someone is looking for other definitions of
"point", etc. perhaps by means of circumscriptions
such as "extensionless" then I would oppose such
an enterprise. In this case one is looking for
something that can never be found because there
is nothing there, and everything gets lost, becomes
confused and vague and degenerates into a game
of hide-and-seek"

[First reply to Frege in Kluge p. 12]

Earlier when we were discussing sense and reference in Frege's
account of identity we explained how definitions as identities could
carry out the task of establishing reference. Here we ought to
reconcentrate on the content of the disagreement. On the surface

there is a two-fold disagreement involved. The first disagreement has to do with differences between axioms and definitions. The second disagreement has to do with the adequacy of implicit definitions.

Frege's argument against the use of axioms for the purposes of definition is clearly stated in his first letter to Hilbert [Kluge, p. 8] and in his critique of Korselt [Kluge, p. 51, p. 55]. Axioms cannot contain undefined terms because such terms would lack reference. Without reference the whole axiom is rendered senseless and thus it cannot be taken as true. An axiom that cannot be taken as true is simply not an axiom. To further substantiate his claim Frege offers an Hilbert-type of axiom that is supposed to define implicitly the imaginary term "anej": "Every anej bazet at least two ellah." He then argues that this sentence which cannot be taken as true or false is similar to Hilbert's axioms if one strips from them terms like 'point' 'line' 'between' the usual connotation as Hilbert asks us to.

The argument is persuasive yet fails to establish Frege's point completely because we recognize implicit in it an absurd demand for a definition of every term in the system. We know however, that such a demand is unfillable and we can claim therefore that Hilbert's ideas about implicit definitions by axioms applies primarily to the primitive undefinable terms. What has Frege to say on the matter? Frege would readily admit that primitives cannot be defined, but if so, he argues, they should not be defined by

axioms either; furthermore, to claim that one defines them in the above manner is misleading, circular, and simply wrong. Instead he notes two things: in the first place even in the case of primitives we require that reference be kept constant: that the same term always refer to the same thing [Kluge, p. 59]. Then he argues that in place of a definition we should have 'explications' that aim at fixing the reference of primitive terms. Explications are like definitions in that they aim at establishing the reference of the term, yet they do not formally belong to the theory but the propaedeutic preamble to the formal theory [Letter to Hilbert in Kluge, p. 8].

The disagreement here however runs deep. Frege's view of explication is necessitated by the demand for reference which in his opinion makes communication possible:

My opinion is this: we must admit logically primitive elements that are undefinable. Even here there seems to be a need to make sure that we designate the same thing by the same sign (word). Once the investigators have come to an understanding about the primitive elements and their designations [Bedeutung] agreement about what is logically composite is easily reached by means of definition. Since definition is not possible for primitive elements something else must enter in. I call it explication. It is this that serves the purpose of mutual understanding among investigators, as well as the communications of sciences to others.

[Kluge, p. 59, italics mine]

In other words, the sameness of reference is needed so that "objecthood" is defined inter-theoretically: there can be only one concept

of 'point' for all theories and sciences even if we define it by different definitions. Contrast this to Hilbert's account which claims that the meaning of "point" changes with the theory and the reference is indeterminate anyway. For Hilbert objecthood is intra-theoretical.

On the other hand, in my estimation it is impossible to give a definition of a point in three lines since it is only the whole axiom structure that gives the complete definition. After all each axiom contributes something to the definition, and therefore each new axiom alters the concept. "Point" is always something different in Euclidean, non-Euclidean, Archimedean, non-Archimedean geometry respectively. Once a concept has been completely and unequivocally fixed, then in my opinion the addition of any axiom whatever is entirely impermissible and illogical - a mistake that is made very frequently, especially by physicists.

[Letter to Frege, in Kluge, p. 13]

The contrast between the two passages is striking. The disagreement clearly reminds one of the contemporary disagreements about relativism and the system-relativity of reference. It is equally significant to note that Hilbert does not expect of his definitions to capture the object 'point' nor does he insist on one concept of point. Finally, while Frege insists on interdisciplinary communication based on sameness of reference, even in cases where we are faced with incomplete determination of reference (and thus continued research is in order), Hilbert appears to draw a line between formal theories and a fortiori between disciplines.

In all fairness to Hilbert it should be noted here that his account is certainly clearer than Frege's. Frege has troubles with his definitions: on the one hand he claims that they do not add anything to our knowledge and are just abbreviations [ibid. p. 24]. At the same time however, he demands of Hilbert that his implicit definitions capture the definienda adequately: Hilbert's account ought to allow one to 'grasp' the notion of 'betweenness' and they do not [ibid. p. 30].

Frege's claim about definitions appears contradictory to his earlier claims on another score as well: he argues that definitions as opposed to axioms are not true or false. [ibid, pp. 23, 30, 61]. We know on the other hand, that in the "Über Sinn und Bedeutung" article and in the Grundgesetze he claims that identities can be both true and informative. The way out of this trouble is to claim that definitions 'become' assertions once they have completed their task of introducing the novel term. This make-shift solution takes care of the truth of definitions as they are supposed to figure in proofs. As for their informativeness we have to fall back on his earlier claims and his idea of 'explication.'

Frege's disagreements with implicit definitions is two-sided. In the first place he argues that the axioms which serve as implicit definitions would lack truth value. In the second place, he claims that such definitions do not uniquely determine their referents. The first disagreement is best summarized by Hilbert as follows:

You write 'Axioms I call positions... From the fact that axioms are true it follows that they do not contradict one another? I was extremely interested to read just this proposition in your letter, because for as long as I have been thinking, writing, and lecturing about such things, I have always been saying the opposite: if the arbitrarily posited axioms together with all their consequences do not contradict one another, then they are true and the things defined by these axioms exist. For me this is the criterion of truth and existence.
[Letter to Frege, Kluge, p. 12]

Frege views implicit definitions in much the same way as he views algebraic systems of equations with more than one unknown. He expects out of definitions a complete unambiguous determination of the extension of what is defined. Unless a system of equations has one and only one solution it is indeterminate. [Second Letter to Hilbert in Kluge, p. 18; also Vol. II of the Grundgesetze § 66 in Black and Geach, p. 170]. Frege finds that such a procedure is objectionable as procedure of definition because one's grasp of the reference of a concept is not immediate but depends on further investigation of the concept itself. Furthermore, he is unwilling to settle for indeterminate reference, "a sign without determinate reference is a sign without reference" [in Kluge, p. 62].

To understand exactly Frege's position on the matter one has to return to his earlier writings in Grundgesetze. In discussing definitions Frege claims that the function of a definition is to 'apprehend' or establish reference to the object. Without this the theory is about nothing.

If there are logical objects at all - and all the objects of arithmetic are such objects - then there must also be means of apprehending them, of recognizing them. This service is performed for us by the fundamental law of logic that permits a transformation of an equality holding generally into an equation. Without such a means a scientific foundation for arithmetic would be impossible.
[Grundgesetze, § 147].

The same ontological claim is repeated at the end of the Appendix to Volume II of the Grundgesetze.

The prime problem of arithmetic may be taken to be the problem: How do we apprehend logical objects, in particular numbers? What justifies us in recognizing numbers as objects? Even if this problem is not yet solved to the extent that I believe it was when I wrote this volume, nevertheless I do not doubt that the way to a solution has been found.

The above is clearly an ontological account of science. It should be contrasted to Hilbert's formalistic accounts as it is described in his letter to Frege:

You say that my concepts, e.g., 'point', 'between' are not unequivocally fixed... But surely it is self-evident that every theory is merely a framework or schema of concepts together with their necessary relations to one another and that the basic elements can be construed as one pleases. If you think of my points as some system or other of things, e.g., the system of love, of law, or of chimney sweeps... and then conceive of all my axioms as relations between these things, then my theorems, e.g. the Pythagorean

one, would hold of these things as well. In other words, each and every theory can always be applied to infinitely many systems of basic elements... The state of affairs just indicated can never be a shortcoming of a theory and in any case is unavoidable.

[Kluge, p. 13-14; italics mine]

The two quotations show clearly that the differences between Frege and Hilbert with respect to definition are due to the radical differences concerning the nature and aims of formal science. We can now understand Frege's objections to implicit definitions: they fail to uniquely determine their object. Since definition is the key to determining objects such an error makes science impossible. This demand for what Frege calls 'completeness of definition' is explicitly stated in the Grundgesetze [Black & Geach, p. 159-160]. Frege's system cannot guarantee this unique determination of objects. Instead the simple Euclidean explication of 'point' by Euclid (as 'that which has no parts') allows Frege to decide that his pocket watch is not a point [Kluge, p. 18 and 62]. Ontologically speaking then, the Elements of Euclid is closer to Frege's idea of proper science than the Festschrift of Hilbert.

What is at stake here then is the problem of formalism in science. Specifically for Frege the problem of reference is essentially the problem of the objectivity of formal science: what a theory is about. Does a theory determine its object of application as it should? His examination of Hilbert's work leads him to believe that his system represents an empty formalism. It would be easy to dismiss

Frege's charges at this point. Even the celebrated argument of Frege's pocket watch is not conclusive because it deals only with a negative case: what is not a point (the clock that is made up of parts). Euclid's definition not only does not determine what is a point (i.e., what falls under the concept 'point') but neither does it figure in proofs of theorems where one would expect it to figure (i.e., theorems about lines intersecting in one or two points). Such a quick dismissal however would rob us of an important idea of science and of an attempt to draw the limits to formalism.

According to Hilbert and to Korselt "it is both inexpedient and unfair to demand of a formal system that it give a determinate reference to the figures which it has constructed on the model of proper names or concept-names." Frege demands of his definitions that they serve as hard and fast criteria that would enable one "to judge of any object whatever whether it falls under the concept or not" [Kluge, p. 63]. As he explains however this does not mean that one can decide the question "on the basis of the definition alone, without the help of perceptions." The formalist however would have no qualms with this provision: it is part of his view of science that it can be interpreted and applied. Yet there are two great differences with Frege here; the first is Frege's emphasis on definition as the key to the interpretation of a formal system. The second point concerns the problem of interpreting formal systems. According to Korselt there are many interpretations of a formal system; according to Frege "the word 'interpretation' is objectionable

because, when properly expressed, a thought leaves no room for different interpretations" [Kluge, p. 79]. The uniqueness of interpretation is based on definition and specifically on the requirement of completeness [ibid., p. 82]. Frege agrees with Korselt that the objectiveness of a formal system (and its consistency) is based on exhibiting objects of which the formal system is true [ibid., p. 91], but they differ as to the determination of these objects (by definition or not) and also as to the nature of the formal system. Frege argues that inference is not a matter of transformation of signs but is based on relations of 'thoughts' which have to be determinate [ibid., p. 82].

...Mr. Korselt is quite correct in claiming that we should not from the very start demand objectiveness of a formal system... But... the charge of being on empty playing with signs may justifiably be leveled against certain formal theories these, however, are quite different from general theorems of the kind considered here. For in the latter we always have a sense. Those other formal theories however proceed in the manner of Dr. Ironbeard. Since the sense occasions difficulties now and then, it is simply exorcised. What remains is of course the inanimate sign. The originator of such theories does not want to express thoughts with his signs, but merely wants to play with them according to certain rules. Consequently it cannot be truth that is here at issue. The word 'theory' is really quite inappropriate; we ought to say 'game'. At least, it would be so if the execution of the enterprise were consistent. But this is never the case: the formalists want to have their cake and eat it too. They empty the signs so as to escape inconvenient questions; but then they refuse to acknowledge that the signs are really empty."

[Kluge, p. 93-94]

Frege is again bringing up his old theme that the truth of the formalistic axioms is based on intuitions borrowed from non-formalistic theory. It is significant however to note here Frege's view as to the formal nature of axiomatic systems: his view of science holds that no part of science is completely formal, including logic. Instead science is ontological, every science dealing with its own objects which it has to determine by definitions or explications:

One may now be tempted to appeal to the formal nature of the laws of logic according to which, as far as logic itself is concerned, each object is as good as any other and each concept of the first level as good as any other and can be replaced by it, etc. But this would be overly hasty, for logic is not as unrestrictedly formal as is here presupposed. If it were then it would be without content. Just as the concept 'point' belongs to geometry, so logic too has its own concepts and relations; and it is only in virtue of this that it can have a content. Toward thus what is proper to it, its relation is not at all formal. No science is completely formal; but even gravitational mechanics is formal to a certain degree, insofar as optical and chemical properties are all the same to it. To be sure so far as it is concerned, bodies with different masses are not mutually replaceable; but in gravitational mechanics the difference of bodies with respect to their chemical properties does not constitute a hindrance to their mutual replacement. To logic, for example, there belong the following: negation, identity, subsumption, subordination of concepts.

[Kluge, p. 109]

This Aristotelian view of science expressed by the thinker who made Formalism possible is significant in helping us understand

the importance of definitions as determinants of reference. It also gives us a way to connect Frege's enterprise with Russell's. According to both philosophers arithmetic even if reduced to logic must still proceed by defining 'number'. Furthermore, this definition must uniquely determine its object. We know how Russell's paradox affected Frege's definition of number. Russell's troubles with reference, in my view, are similarly connected to definition and objectivity of a theory. Leaving aside the specifics of Russell's definition of number and his progress beyond Frege, we can say that it is the same problem of what a theory is about that motivates Russell's troubles with denoting expressions. Specifically, his argument in the Introduction to Mathematical Philosophy is that the five axioms of Peano do not uniquely determine their objects [1917, p. 9]. What is needed then is a way of determining what the formalism is about. Given that both the definition of natural number and any such definition that anchors a theory onto reality uses the denoting expression "the so-and-so" Russell has to investigate the capacity of such expressions to refer: hence the chapters of the Introduction and the Principia that are devoted to the puzzles of denoting expressions.

We will have the opportunity to examine Russell's theory closer in Chapter III. My aim in this first part of the chapter was to show that the traditional problem of reference was not just a problem of developing adequate theory of referential semantics as part of a general philosophical theory of Pure Semantics. Instead, it arose

primarily as a problem in the philosophy of science, and specifically as the problem of objectivity of formal science. Both Frege and Russell who originated the problem of reference were committed to the formal ideal of science; yet at the same time, keenly aware of sciences ontological nature, they were puzzled about how can formal science be objective, not in terms of applications alone, but in terms of determining the nature of the objects that it was about. In my view this is the real problem of reference, and not the post-Wittgensteinian problem of dealing with the puzzles of reference out of context. The proper discussion of reference moves from Frege and Russell, to the theories of Poincare, the Frege-Hilbert, Korselt disagreement, and the great work of Hermann Weyl. When one considers reference in this context then one can properly understand the debt that Relativism owes to reference (both the Relativists of the Kuhn-Feyerabend School and especially Quine). One can also see how the modern Relativists despite their pronounced attacks on Positivism, in fact share with Positivism a formalistic account of referring: they use Hilbert's idea that the reference of a term depends on the theory in which it figures. This idea of referring will become the focus of my attack in Chapter III. I have to show now that the Relativists do in fact base their position on an argument about reference.

2.2 Relativism and Reference

The content of the Relativistic attack against objectivism in science was developed in the Introduction. The aim in that second part of the Introduction was to show the existence of a serious problem in contemporary thought. What was developed, consequently, were the stands that the Relativistic movement took with respect to science. What was left untouched (or was merely hinted at) were the arguments in support of the Relativistic contentions. In this section my aim is to argue that most of the Relativistic Arguments are reducible to one argument about the indeterminacy of reference. Specifically I will argue that the arguments of the Relativists either are outright weak (and they have been shown to be weak) or can be made strong only in connection with a formal argument which shows that science cannot determine formally its object of reference. Given the ontological character of science, to which both Relativists and Realists subscribe, the argument is serious. It also connects the Relativists with important traditions in the Philosophy of Science: Poincaré, Weyl, Hilbert, Quine. My concern is to attack Relativism on what I consider its strongest grounds, instead of following a Fabian tack of chipping away at its outskirts, I shall argue then that while the argument for the indeterminacy of reference is formally correct and while it is certainly a relevant argument since it is the aim of science to determine the nature of reality, still the application of the formal argument to science is based on an erroneous view about how reference is achieved in science. What I have in

mind to contrast here is not the usual discrepancy between referring-in-principle and referring-in-practice. I am prepared to argue instead on the basis of what I have established so far that the ontological account of science requires an altogether different theory of reference than the traditional theory on which the Relativistic Argument is based. In the next chapter then, I shall contrast the Traditional Theory of Reference with the new Causal Theory of Reference. I will argue that the correct theory of reference (the Causal) not only undercuts the Relativist's use of the Indeterminacy Argument, but moreover lends support to the Realist's position.

It is customary and not altogether unfair to deal with the Relativists Hanson, Kuhn, Toulmin, Feyerabend, as one philosophical school. There are certainly important distinctions between their various theories that must be kept in mind for an adequate account of their position. Hanson, for example, is interested in offering a groundwork for the proper understanding of modern physics and proceeds to show by appeal to earlier Wittgenstein that a great source of troubles (in understanding modern physics) is an erroneous view of the relation between scientific theories and the observations that support them. Toulmin takes a beginning in later Wittgenstein and argues that science (or at least its more theoretical and less descriptive-taxonomic part) is aiming at new ways of seeing old regularities. These new ways of seeing are as distinct from one another as different language games. Kuhn in his turn has provided a more systematic view: he proposes a certain view of the history

of science, as a history of revolutions rather than of cumulative progress based on rational rules and he proceeds to back up this diagnosis with a general account of science-as-practiced and a justification of the latter by appeals to considerations of system-relativity of the meaning of basic scientific terms. Feyerabend finally takes Kuhn's theoretical argument one step further and supplements it with a theory of incommensurability of scientific theories. From his position an even more radical view of the history of science follows connected with an even more radical theory of scientific methodology: science is and should be an anarchistic enterprise; besides the absence of method there is no other method for science.

The Relativists however, despite their differences, do form a tradition. Specifically, they share three unifying traits. In the first place their works were developed within the framework of holism, specifically Wittgensteinian holism. In the second place they all engage in critique of the Positivistic Philosophy of Science. Finally they all engaged in their work as part of the attempt to understand the developments of modern physics. These three common elements have philosophical significance that runs deep. The first element for instance represents an attack on the methods of traditional philosophical analysis. Of the second element we have already spoken: it is in fact the recognition that science has an all important metaphysical-ontological dimension. Finally the third element, the concern over quantum physics is significant because in the first place it illustrates an important Relativistic point that was forgotten

by the Positivists: that science can engage in philosophical controversy - contrary to the "hypotheses non fingo" attitude that used to reign previously. It also represents according to some participants in the debate a direct challenge to Realism. Furthermore, there is the related point that quantum theory brings back into science epistemological concerns as part of science, forcing us to rethinking not only concepts like cause and object but even more 'stable' notions such as representation, model, understanding, and even our background theories of logic and language.

These unifying traits which I outlined are only the beginning points of the Relativist's programme. To establish anything like a unified epistemological position one has to look behind the implications of their arguments because it is true that the bulk of the Relativistic works is composed of critiques of objectivism.

The Relativists have offered two general types of arguments:

(1) Historical Arguments and (2) Philosophical Arguments. Under the first kind we can properly place all the 'inductive arguments' which can be divided in two subclasses: (a) Properly Historical Arguments, that is to say arguments which appeal to the History of Science (b) Arguments pertaining to the actual practices of the scientists.

Concerning both of these types of 'historical' arguments while it is true that some attention to the history and the practices of scientists was long overdue, one should not at the same time overestimate the ability of these appeals to history to dislodge

the Positivist a-historic account. They require supplementation by philosophical arguments. Let me explain here. Given that the Positivist programme is essentially normative, arguments of the form 'this is not what the scientists in fact do' or 'this is not what the scientists have done in the past' would not take one very far unless he could show that the historical component was an essential ingredient in scientific analysis (which was done by Lakatos) and furthermore that there is indeterminacy inherent in this historical component. The Relativists were able to shift the emphasis from discussions of the Logic of Science (i.e., the empty formalism) to discussions of concepts and evolution of concepts. The legitimacy of this shift can be established only in philosophical not on historical grounds. More importantly, however, for this move to have an effect on scientific objectivism appeal to history or even to the fact that we are just another stage in history will not help. As long as we can justify the existing methodology on normative grounds the relativistic attack becomes merely an attack on the sloppiness of a few scientists. There is even a precedent here in the case of Logic. If we simply tried to codify by logic the ways in which people actually think, logic would hardly have developed in any interesting or legitimate ways. On the other hand to expect of history to establish an epistemological point not only begs the circularity point but also begs the clarity conditions: we expect out of the obscure to illustrate the clear! Consider for instance the common argument of Feyerabend and Kuhn about the re-writing

of history by scientific textbooks of normal science. Feyerabend claims that present scientific education 'simplifies the participants in the history of science' by presenting them as undimensional thinkers engaging in one type of pursuit and leaving aside preoccupations like religion, metaphysics, sorcery, sense of humor, etc. [1975, p. 1]. Presumably considerations of these forms will show the history of science to be a lot more complex than presented.

Such arguments from history, even when they spill over into normative historiography carry little weight unless supplemented by proper philosophical argument. For one thing, they become quickly entangled in matters of interpretation. Some principle of "charity" is often used by which the development of ideas can appear as coherent or noncoherent. Once we recognize this, the choice between a chaotic and complex history and a goal-directed history is pragmatic. There are reasons for seeking coherence in the development of ideas, especially if history is seen as pedagogic: to help us understand the present activities under a more rational framework. In the same way, it helps in military history to present a war as more rationally conducted, rather than focus on the chaos or on the accidents, if one is to teach military science. Again it does not hurt to expect of our lives to cohere into chapters and plots leading up to ends, even if no life history really develops in such manner. By the same token, if only for consolation, the opposite tactic is to be followed.

By the above considerations I do not mean to belittle objective historical research but merely to show the functions that interpretation can fulfill. On the score of objectivity, however, the Relativists have made many contributions to the history of science but not all of them are considered "paradigms" of historical research. If anything the Relativists choice of historical instances and of interpretations is clearly biased and often in an intentionally misleading way. H. Stein has made a very convincing case against Kuhn and Scofield which shows not only the theoretical bias in favor of the paradigm-interpretation but also the systematic deception involved by uncovering the misquotations of Newton's work by the aforementioned authors. In fact, according to Stein, Newton was not only fair to his 'opponent' Huygens, but even provided arguments in support of the wave-theory of light. Similar observations held for Maxwell's views of his opponent's criticism against his mechanical models.

Convincing as H. Stein may be, I am not involved in upholding his claims against Kuhnian historians. My point is a general point about the limitations of the 'historical' arguments. The Kuhnians must establish that there are no underlying themes or ideas and no progress in such underlying ideas. To do so they require philosophical arguments as much as the existence of such traits would require philosophical arguments as well.

The philosophical arguments of the Relativist's are divided into two categories. (i) Arguments about perception and (ii)

Ontological and semantic arguments about the meaning and reference of scientific terms. I will try to show that the former group is reducible to the latter in the sense that the former group requires the support of the latter, otherwise they fail either in themselves or as constituents of a serious epistemological position.

The typical relativistic argument from perception claims that scientists from different paradigms (i.e., with different general theories) simply perceive the phenomena under consideration differently. There is a variation of this argument in every relativist under consideration: Hauson's well-known example of Tycho and Kepler, Kuhn's claim that "after a scientific revolution scientists are responding to a different world", Toulmin's claim that science primarily provides new ways of seeing old regularities, and finally Feyerabend's claim that facts are not independent of theories. Let us examine these arguments. What is their aim?

It is difficult to know exactly what the Relativists are attacking by the arguments from perception. There seem to be three distinct targets. In the first place they want to attack sensationalism and specifically the idea of neutral observations: there are no perception simples and no innocent eyes to perceive these simples. Using a questionable step then they expand the scope of their attack: objectivism is the target. The objectivity of science cannot be warranted by observation since observation is determined by scientific theories: observation is theory-laden. The final step has even wider scope and is rather bold for it goes beyond being an attack

to stating a whole epistemological position. The target here can only by Scientific Realism since it is now argued that the concept of an independent reality against which theories are tested and developed is a myth.

This confusion in scopes always works in favor of the Relativists because they often borrow forces (and victories) from their limited claims and bring them to bear on their more radical claims. To use an example, their attack on sensationalism is certainly accepted since even in the times of British Empiricism it was recognized that the concept of simple idea (our modern "sense-datum") is a problematic derivative of a theory of perception which more recent theories showed to be erroneous: as a matter of fact sensationalism was not defeated by Relativism but had almost collapsed from within. Then the Relativists shift their attack on sensationalism into a more general attack on objectivism, without really providing a new argument but only generalizing the old argument and adapting it to a scientific observation situation. What is this major argument?

The claim that observation is not theory neutral but theory-laden [Hanson, 1958, 1971] rests on two grounds: It is argued in the first place that whatever is neutral in observation is certainly unusable for purposes of theory building. It is then argued conversely that whatever is so useable is already formulated in statements and these statements are influenced by the general theoretical framework. In general then cognitively significant observations can only be theory-laden. How are we to deal with this argument?

In the first place there is an aspect of the argument that is correctly taken. Even since the times of Duhem it was recognized that due to the systematic nature of scientific claims and theories even the logically correct falsification ran into the difficulty that it could not genuinely test one isolated theory. It is obvious that auxiliary assumptions enter the hypothetico-deductive method. It can even be argued that something of what the Relativists had in mind is certainly correct. The readings of a voltmeter cannot be used as absolutely independent touchstones for theories of electricity: the movement of the needle of the voltmeter is insignificant theoretically without the theory which even produced the voltmeter in the first place. Hanson is right in noting that what a scientist observes in a laboratory is rather conceptually sophisticated: he does not observe needles but currents, or particles, and what he sees is certainly more than what the novice sees. This simple and correct point however is elevated to a status of epistemological principle, by arguing that "different thinkers see different worlds." Contrary to this position Sheffler has shown that simply to see something now as P and then as Q requires that there be some fundamental agreement about what is seen, that is to say the reference of P_x and Q_x [1967, p. 41]. Shimony on the other hand has argued that even the weak methodological claim against sensationalism does not always apply to science: synthetic perception is often abandoned for analytic perception of sense data if the case at hand calls for such an observation (as it often does since color is used often

in analysis in biochemistry, astronomy, optics, etc.) or if the disagreements between the various reports of what the scientists have seen are significant enough to become the subject of a debate [Shimony: manuscript]. Given these objections, what is left of the Relativistic Argument from perception?

The Relativists consider perception as indeterminate because they claim, the conceptually significant units of observation are theory-laden. This claim of course by itself does not make perception indeterminate. As a matter of fact various studies in perception have shown that there can be significant variations in perception across cultures or as a result of training; also simple studies in physiology tell us that a hawk sees the world differently than a frog and differently than a human being. These causally determined variations are not what the Relativist has in mind. The relativistic point can be made strong if it can be shown that (a) Theories of Science are incommensurate and (b) Theories of Science involve total metaphysical outlooks. This is obvious in the argument of Hanson about Tycho and Kepler: the two astronomers watching the sunrise see different things. While the claim is almost trivially correct one is left wondering why they cannot share a view or even share a view of their differences, and also whether there are some common elements in their observation to form a basis of agreement. To be a relativists one has to answer these questions in the negative; once we admit there is a difference there is no way of containing the difference: there is in principle no way of establishing how

far the difference in Weltanschauung goes. What this sceptical argument does is to make scientists Leibnizian metaphysical islands. If left in this simplistic form the argument is certainly ludicrous since it is a simple crude appeal to the village scepticism of "how do you know?" type. To find the full force of the argument we must look into its presuppositions.

Hanson's major argument here is that what is perceived is neither an object "I see x", nor an aspect of an object "seeing x as y" but instead a fact "seeing that x is p." All 'seeing' then is 'seeing that'. There are two ways of reading this argument. The first way is by concentrating on the concept of 'fact'. As I explained earlier this is an intensional concept that leads us back to the problem of private meaning: indeed we hardly progress beyond the crude village-scepticism of the previous paragraph. The second way is to interpret the claim as one having to do with individuation of units of significance in observation. Hanson is saying that the smallest significant unit in observation is not an object nor a property (predicate) but a complete state-of-affairs. As a matter of fact, he later spells this out by claiming that every observation involves not only a state of affairs but also a retrodiction and a prediction. In other words we do not see isolated states of affairs (like frames in a movie reel) but objects-in-time: we see more than hits the retina. This "more" is determined by our knowledge. This much is clear even in the voltmeter example: we do not see a needle dancing up and down we see currents and voltage between the two

probing poles. However expanding the claim to cover all of scientific knowledge requires additional scaffolding.

The first element in this scaffolding is to see science as ontological or, in the case of relativists, as metaphysical. Science in other words aims at a complete investigation of the nature of reality. I explained earlier why it is essential that this characteristic of science for the Relativists be 'metaphysical' rather than 'ontological'. Here it becomes even more obvious that what the Relativist requires is not a view of science as a piecemeal investigation of reality but as a total investigation: otherwise the claim of indeterminacy cannot be established.

To avoid the obvious objection that much of science is piecemeal investigation the Relativists have to concentrate on certain types of scientific theories. Kuhn for instance concentrates on Scientific Revolutions, while Feyerabend and Hanson concentrate on theories that are vastly different in such a way that different metaphysical conceptions are involved: it is easy to argue that Aristotle's conception of motion is radically different from Newton's in ways that show the two general theories to be incommensurate. If we show indeterminacy to exist in such extreme cases (of 'radical translation') then we can form an in principle argument that brings indeterminacy back home, but provided always that science is after an ultimate representation of reality. The best distinction here is provided by Toulmin. He separates two types of science the Descriptive Sciences ('natural history' types of science) which

systematize the phenomenal world and the Explanatory Sciences like 'physics' and chemistry which seek reductions. It is the latter types of science that show precisely the metaphysical aspect of science. This distinction is important to establish Toulmin's view that progress in science simply involves not new regularities but new ways of seeing old regularities. The distinction runs deep since these new ways of seeing determine new methods of investigation. According to Toulmin the descriptive scientist shares the language of commonsense, his categories do not represent a departure from ordinary categories. The explanatory scientist however is, by virtue of his quest, involved in language shifts where new discoveries force us to reclassify even the subject matter that we had initially sought to investigate.

There are two results following from the above observation. In the first place it is obvious that the explanatory scientist is involved in an indeterminate process: there is no part of his science that can be considered constant: subject matter, method, principle, and ways of interpreting phenomena are all connected. At best one can hope to share the scientist's way of seeing, his 'understanding' of the world, but that cannot be guaranteed.

In the second place, it is instructive to note the summary of the distinction that Toulmin offers: Descriptive (natural history) Science investigates the regularities of given forms (meaning by 'given' that they are shared with ordinary language) the explanatory sciences investigate the forms of given regularities [1953, p. 53 ff].

The distinction is important because in the first place it establishes that not all facts in physical theory can be taken as of equal significance: facts have significance in accordance with the theory. It follows from the above that negative instances which matter a lot in natural history cannot play similar roles in physics: much depends on the significance of the falsifying fact, and this in turn is determined by the whole theory. Finally we ought to keep in mind here the particular view of science implied 'a formal construction' or rather one possible formal construction to fit a given number of regularities.

The indeterminacy that the Relativists have in mind is closely tied to their metaphysical view of science. The point is clearly made when in addition to the above considerations we consider the Relativist's analogy of theories and maps. In Chapter 4 of his Philosophy of Science, Toulmin considers the analogy between maps and theories and is initially favorable to the likening in much the spirit that we outlined as a 'realist' account of correspondence [Section 1.2]. Yet he finds, with good justification, that scientific theories differ from maps in correspondence, because in map-building we start from an initial agreement about two all important elements. In the first place there is agreement as to the nature of the objects-to-be-mapped: the map can provide no additional theoretical (explanatory) information as to the nature of these objects. Contrary to this, in the explanatory sciences, the aim of the theory is to provide a representation of the nature of the objects in question and there

cannot be prior agreement - otherwise science would be redundant. In the second place and related to the first, there is a second prior agreement in ordinary map-making about the conventions of representation. In science however the mode of representation depends on the findings or the conjectures of the scientist concerning the nature of the objects in question: different theories differ not only in substance but in form of presentation.

This great metaphor of Toulmin shows clearly the point of the Relativist position. To the two-fold indeterminacy above (i.e., that scientific theories seek to uncover the nature of the mapped and that, depending on this nature, the principles of the scientific mapping depend) we only need to add that that which-is-to-be-mapped, reality, must be taken as unknown or rather unknowable, so that what the scientific map is compared to is not reality but rather another map, and we have the full force of the relativistic position. The task of science is to determine the nature of its object, yet science is unable to do so in a categorical way; much of what counts as object for science to refer to is inter-theoretically determined, while what science is ultimately supposed to correspond to, observations, are equally determined by the theory: the map determines our vision of the area, so to say.

Metaphors aside then, the Relativistic argument from perception borrows its force from ontological semantic arguments about indeterminacy of scientific theories. The same can be said mutatis mutandis

for the inductive arguments. This indeterminacy of scientific theories is the well known incommensurability. Once again however the term does not apply to one single argument. There are at least three types of incommensurability in the writings of the Relativists. The first kind is the incommensurability of theories with respect to explanatory relevance, that is to say, the fact that different theories differ with respect to what they consider worth explaining. The second kind is the incommensurability of meaning which exists because the key scientific terms of theories are defined implicitly, by the theory. The third kind is the incommensurability or rather indeterminacy of reference, which leads to the thesis that the major task of science is left necessarily indeterminate since science cannot determine its object in any determinate way. The first two kinds of incommensurability create a lesion on the ideal of scientific communication cross theories. The last, which in my view is the more serious, hits at the heart of science since it assails the ideal of scientific objectivity. It is my aim to argue that the strongest relativistic argument is the last one which not only stands well on its own but also provides support for the rest.

The first kind of incommensurability, that of explanatory relevance was developed by Kuhn [1970, p. 170-172]. According to him two theories are incommensurate not primarily because we cannot develop and compare their competing claims concerning each possible observation instance but because different theories disagree as to what is worth explaining, that is to say what are the telling

facts about the subject matter at hand. Although this point can only be established not by formal argument, but by appeal to the history of ideas - and therefore it is bound to involve some hermeneutical footwork - it is nevertheless a very serious point for it reduces the important scientific debates to debates not about substance but about values. Furthermore, given science's professed avoidance of values, the debate becomes subject to rhetoric and propaganda. It is easy to see that most of Kuhn's sociological observations about the institution of science are based on this type of incommensurability.

To my knowledge, the earlier critics of Kuhn have not provided an adequate reply to him. Scheffler in his Science and Subjectivity [pp. 84-86] argues that there are two types of criteria that Kuhn confuses: the internal criteria by which a paradigm determines the legitimacy of problems and solutions to these problems and the external criteria by which paradigms themselves are evaluated. I find this solution inadequate because except for post-Kuhnian science it is difficult to see scientific debates that are clearly of the latter kind. Of the post-Kuhnian debates, the Chomsky-Skinner debate is, perhaps close, but it must be admitted that much of Chomsky's success was modeled on Kuhnian paradigms. Other important, philosophical debates in science (which are the most likely to be about external criteria) are mixed with debates on substantive points. Consider for instance the Bohr-Einstein debates from the earlier Solvay Conferences to the Einstein-Podolsky-Rosen paper: although

one senses clearly that the disagreement runs deep into the discussion of what are the requirements for an adequate science, still the points discussed are concrete thought experiments. Similar things can be said even about the debate that Galileo sets up between himself and his Aristotelian opponents. Furthermore, this type of distinction which was, in another form, proposed by Carnap in his Empiricism, Semantics and Ontology was shown to stand on questionable grounds by Quine in the Two Dogmas: there is no criterion of analyticity or of empirical significance that can distinguish the pragmatic external criteria for adopting a general ontology from the non-pragmatic criteria for establishing within the adopted ontology framework the existence of an entity. Similar considerations apply to our case as well. Are we then to admit that Kuhn's Incommensurability of Relevance Thesis is correct? Again our claim is that this Thesis does not stand alone.

Up to a point the thesis is correct and common-place. Aristotle's Physics or De Motu Animalium is not easily comparable to modern treatments on physics or on biology not because there is no ground on which to compare their concrete claims (after all, sad to admit, Aristotle did make a few false claims) but because the problems that Aristotelian biology or physics sets up to answer are different from the problems that modern biology or physics consider important. One can try to handle this problem by appealing to the interest-relativity of explanation as Putnam has done in the Locke Lectures.

Yet unless one adopts some kind of more basic theory, like Naturalism, by which to deal with this 'interest-relativity', this is only a sham of a solution: it simply repeats the problem in other terms.

Another reading of the thesis returns us to our previous problem about metaphysical outlooks: what people wish to explain is what they find surprising, that is to say the 'telling facts about a subject matter'. What they do find surprising is a function of what they do not find surprising, that is to say: problems are children of theories. Examples here abound. Freud thought that dreams were worth explaining while the psychiatric investigators before him thought that they should only be explained away. It is significant at the same time to note however that Freud had to make a case for the efficacy of dreams: a simple theory of interpretation based on 'wish' would not be accepted unless it was also shown that dreams do function in the causal lines of behavior.

Thus, even if one grants to Kuhn that theories do differ as to what they consider worth explaining, still one has to realize that above every other knower or thinker the scientist argues for his problematique, often enough not merely to persuade his colleagues but to persuade himself. The fact that new ideas often do not succeed in persuading the scientific establishment has to be evaluated in contrast to the opposite claim, and even if the first claim turns out to hold in the majority of cases one has to grant that it is not due to the neglect on the part of a scientist to lay bare his

problematique: Aristotle's opening dialectic in the treatises is a splendid example of this.

Moreover this supposed incommensurability does not by itself lend support to relativism as an epistemological position. One has to argue also that science as a whole does not have unifying traits that cut across theories and paradigms. The trouble begins if one considers the method as the only unifying trait and specifically if one takes the Positivist account of the method as the canonic one. Under this misconception whole eras in the history of science will become absurd or incommensurate with later developments. As I argued earlier however the Positivist account of the method can only be taken as a first approximation, a necessary and normative account for reconstructing in simple extensionally-minded logic certain moments in science. One certainly has to look deeper into the ontological component of the method. To establish the relativistic contention one has also to argue that in terms of the ontology of explanation there are no unifying traits in the history of science; one has to argue that there is no generally accepted demand for mathematization, (for instance) or acceptance of atomism, or a general shift towards the concrete. More recent works, like L. Landan's Progress and its Problems and H.I. Brown's Perception, Theory and Commitment have tried to show that there is not so much chaos in the metaphysical outlooks of different scientists. Both these thinkers have argued that problems are not as indeterminate as the relativists have claimed. Instead they are

generated within broader metaphysical positions shared by many scientists (Landan's Research Traditions and Brown's Presuppositions).

The conception theories as islands of different metaphysical archipelago, and the implied first incommensurability cannot stand alone. What is required is an argument in principle that will show the communication between scientists to be subject to indeterminacy. This ushers in the second type of incommensurability that is due to the indeterminacy of meaning of scientific terms. The central idea here is that scientific terms as far as their meaning is concerned, are determined only by virtue of the place they occupy in total theoretical systems. This implicit definition of theoretical terms results in indeterminacy since any change in theory has to result in change in meaning. The claim obviously has strength when applied to theoretical terms and primitive terms. Both cases of terms are of crucial importance to science: most scientific theories worth their name involve primarily theoretical terms or terms which were initially theoretical. As for primitive terms it is obvious that the process of definition requires primitive terms in terms of which other terms are ultimately defined.

The import of this thesis is equally obvious. It provides support for the first incommensurability thesis by showing that in principle there can be no unambiguous scientific communication. This, in turn, places the idea of progress in question since one of the presuppositions for rational choice of theories, communication,

is shown to be problematic: rival theories are not commensurate from the standpoint of meaning.

Variations of this basic idea are hardly novel or even effective. Ever since the time of the Sophists we have recognized the existence (and the value of) ambiguity in communication. It is true that science always did its best to avoid ambiguity since it was taken correctly as a factor leading to subjectivity rather than objectivity. It is also true that the scientific insistence on the methodical introduction of new terms (by listing the primitives, or by definition) eliminated much of the ambiguity from the presentation of scientific theories. Whatever excess-meaning the scientific terms carried beyond systematic determination or definition, that is to say, the different connotations of the terms for different scientists, can be taken to be not only harmless but even productive, because ambiguity in this form (connotation) has heuristic value: it functions in the context of discovery. So why should we worry about ambiguity now?

Moreover, even if we admit that the key terms in science are implicitly and not explicitly defined, and consequently that the meaning of such terms changes from theory to theory or even within one theory as it is developed, still why should this undermine the objectivity of science? Implicit definitions are certainly less preferable than explicit ones, yet they are systematic enough so that scientists can manage with them. Especially in view of the

mathematization of science, the idea of a Scientific Babel is, to say the least, questionable, unless supplemented by additional considerations. These considerations, in my view have to do with reference. But let us examine the case more closely.

Why should one take the system-dependence of meaning as leading to such a radical result as incommensurability of scientific theories? Meaning, since the times of Frege was recognized as a problematic concept: Russell's theory of knowledge by acquaintance vs. knowledge by description can be used to account for the vagaries of meaning. These considerations at best can lead us to scepticism about the possibility of a rigorous science of semantics, and maybe, by extension, to scepticism about the possibility of rigorous sciences of man. One can even grant that the system-dependence of meaning does create indeterminacy of meaning. Disquieting as this result is, it is still explainable and acceptable given the success of communication: it is distressing to think that we may never be certain about what Aristotle meant, yet we often forget how amazing it is that we can understand as much of Aristotle as we do. To be sure there are a lot of interpretations of Aristotle but let's not forget that Aristotle was a philosopher and furthermore that this multiplicity of interpretations simply shows a general fact about meaning: maybe Aristotle was not himself clear about what he meant. What I am hinting at is that when we make too much of the indeterminacy of meaning we are implicitly committed to the thesis that meanings have to be determinate, or, to use Quine's metaphor, to the Museum

Theory of Meaning. Even with indeterminate meanings (as I believe they are) communication is perfectly possible. Is the incommensurability thesis based on an irrational demand just like all sceptical theories?

There is a plausible interpretation of Feyerabend, by Hacking [1976] that shows Feyerabend and Quine to be diametrically opposed on the question of meaning. Whereas Quine maintains that there can be an infinite number of translation manuals between two systems, Feyerabend and the relativists claim that there can be none. Hacking's interpretation must be elaborated further: the fact of communication should lead us to modify Feyerabend's claim from "there can be no translation manual" to "there can be no accurate translation manual" and this certainly presupposes, in the form of a demand, the determinacy of meaning.

This reading of the 'meaning' incommensurability thesis squares well with the 'relevance' incommensurability and shows how the latter thesis also rests on a philosophically suspect idea of private determinate meanings: since scientific terms can only be understood implicitly, i.e., by the totality of laws in which these terms figure, and given that every theoretical system represents a metaphysical outlook (first incommensurability) which in turn has to be understood in terms of a total world-view of the theorist, there is no way of understanding a theory without knowing the complete Weltanschauung of the proponent of the theory. Furthermore, since one

cannot talk of 'knowing' that Weltanschauung at best we can opt for sharing the outlook in question. If one grants the in principle impossibility of understanding this ultimate background metaphysical framework, then we are clearly implying that we are faced with a private language that is inadequately translated into a public one.

Contrary to these claims one can argue that (a) we are not clear about our ontological-metaphysical world-view and (b) we embark in the development of theories knowing fully well the social character of our discourse. As one proposes a theory or a view one is well-aware of the indeterminacy involved; the fact that we can always say something more to 'correct misinterpretations' should not be taken as an indication that there is something determinate (a private world view) that is only incompletely conveyed: that would be the 'book in the mind' view of theories which is as erroneous as the Museum Theory of Meanings. It is not the case that world-views are first developed and then a way of communicating them must be found; instead world-views are developed-in-communication. If there are more than one interpretations of a given theory, we can ask for information that will lead to elimination of some of the rivals in favor of one or few remaining ones. If no such information can be given in principle then the competing interpretations cannot be really competing. If no such information is forthcoming, because, say Aristotle is dead, and no new works of his are discovered, then we can sadly accept the indeterminacy and rest contented with the richness of the theory.

In summary then, the second incommensurability thesis is weak because to be effective it requires that meaning be considered determinate. Once this demand is removed, the system-relativity of meaning not only does not become a fatal impediment to scientific communication but becomes an element of progress. There is however an aspect of the second thesis which is clearly leading to relativistic conclusions. While indeterminacy of meaning is acceptable, indeterminacy of reference is not. If, moreover, meaning determines reference then the indeterminacy of meaning results in indeterminacy of reference. One should note, however, before going on the preceding antecedent: for if Relativistic argument to work one needs the ancillary thesis that reference is determined by meaning, at least in science. Let us then re-examine the argument.

We started by saying that at the heart of the Relativistic argument lies the observation that the key terms in science are defined implicitly by the laws of the theory in which these terms figure. This gives rise to indeterminacy because any change in theory results in change of meaning of the terms in question: the term 'electron' has different senses in the works of Rutherford and the later writings of Bohr. That much is almost too trivial to admit. The problem is how can we get from this to incommensurability of theories, especially in view of the fact that implicit definitions, though less preferable than explicit ones, are still adequate and moreover necessary?

The success of implicit definitions rests heavily on the consistency of the statements that define, implicitly, the terms in question. To say that the set of sentences defining a term implicitly is consistent is to say that there can be objects denoted by the term in question: this is a first step. It is also required that these sentences form a categorical system, otherwise the terms in question cannot be unambiguously defined. To illustrate the point let us return to Frege & Hilbert controversy and specifically the suggestion that implicit definitions be thought of analogously with algebraic systems of equations. We require that the system of equations (a) have a solution and (b) have at most one solution. Failure to meet condition (a) results in impossibility. Failure to meet condition (b) results in indeterminacy. Similar conditions should hold for a scientific theory: it should specify one, unique model; inconsistency means the absence of any possible model, non-categoricity means indeterminacy or inability to establish a unique model.

Before proceeding we should take note of three important observations. In the first place it is clear that the controversy surrounding the Relativistic movement is nothing new; at the heart of this recent controversy lies an issue similar to the one involved in the Frege-Hilbert disagreement. The second important thing to note is that while the argument is based on the definitions of scientific terms, it is obvious, by the equation analogy as well as from the Frege-Hilbert controversy, that what is at stake is not a problem of meaning but rather a problem of reference. Finally a word of

caution concerning the conditions of adequacy for implicit definitions. We required that the set of sentences be consistent and categorical. One may wonder why not require that it be a set of true sentences. This suggestion while it would guarantee the consistency of the statements, and thus the existence of a model, still it would quickly run circular: if it is required that for an adequate implicit definition (determination) of 'electron', all the statements containing it be true, then we already imply that the term 'electron' refers: that much is required by the meaning of 'true'. It is obvious then that what we are faced with is a problem not of meaning but of reference of scientific terms.

The circular claim of course is never seriously advanced. What is often advanced however, is a non-circular variant of it. It is suggested that since the truth of scientific theories is tested by the truth of certain observation sentences, that lie on the periphery of scientific theories, it is then argued that the reference of scientific terms is exhausted by the truth of the observation sentences of a theory. This instrumentalist view of theoretical terms can lead then to a new version of the old theoretician's dilemma: assuming that the ontology of a theory is determined by assignment of entities that would fulfill the theoretical part of science, which is presented as a Leerform (a calculus with variables instead of objects) then any possible re-arrangement of the theoretical parts of science that would match the original observation sentences would generate a new ontology. And here comes the dilemma: either

we cannot observationally distinguish between the two ontologies and thus there is no rational choice between them (except for such aesthetic criteria like simplicity), or we can, in which case the terms in question are not theoretical any more.

Variants of this argument have been used by Poincaré³ (against Maxwell's models) and by Bridgman.⁴ In the present context using the argument as part of the Relativistic attack would involve us in taking a curious double stand. The argument borrows from the Positivistic account of science some elements, omits certain others of crucial importance, and still provides other anti-positivistic elements. Specifically what is borrowed from positivism is the idea of science as a formal calculus made up of laws that are in fact interrelations between predicates (the idea ultimately borrowed from Poincaré). What is abandoned from the positivistic account is in the first place the aim of science. In place of prediction and control of observables, a new anti-positivistic aim is provided: the obligation to provide a representation of reality, that is to say the metaphysical-ontological component of science as a necessary condition for any adequate scientific explanation, since the D-N model of explanation is taken as trivial. Finally what is also abandoned from the Positivistic account are the correspondence rules (coordinative definitions) which are meant to relate theoretical terms to observational terms and thus allow a theory to lead to experimental terms. It should be noted here that the positivists

did not take the correspondence rules as definitions of theoretical terms: the definitions of theoretical terms were supposed to be given implicitly by the calculus.

The relativistic variant of the argument then focuses first on the implicit definitions of the theoretical terms by the calculus, it proceeds then to ignore correspondence rules on the grounds that no clear distinction can be established between theoretical and observational vocabulary of a theory. Against this the positivist would claim that experimental laws normally do not lose their truth or their meaningfulness with theory shifts, as opposed to theoretical laws that do, but the relativists could afford to ignore this claim by utilizing the argument that observation is theory-laden, which we showed to be in error. We can now see an additional reason why such an argument fails: while it is true that theories change, it is also true that some theories have become, by the advance of technology and of observation techniques, as stable as experimental laws: one can use as examples here theories that have become virtual certainties such as the theory that organisms are composed of cells.

In examining the argument closer, however, we see a serious argument lurking in the background. If one accepts the view of science as an axiomatic system and at the same time assigns to science an ontological aim, then the question becomes which part of science is to carry the ontological weight. Surely not observations, according to Relativists. It is here that the concept of a model

has to be introduced, and it squares well with the relativistic idea of science as providing a representation of reality. It is the model that provides an interpretation of the formalism and thus provides the reference for theoretical terms.

Before proceeding to examine models from a relativistic standpoint let us stop to note two things. (i) The argument about alternative ontologies fitting the same observational data is really, according to what is said above, an argument about the reference of theoretical terms. They are supposed to be defined implicitly by the theories in which they figure while their reference is again determined by the models that serve as interpretations of the abstract theories. (ii) It is also essential to note the idea of science as a formal system where abstract predicates are interconnected in postulates and theorems: science as an axiomatic Leerform. With these two observations in mind let us return to our central argument.

In discussing implicit definitions we compared them to algebraic systems of equations and argued that their success or failure depends on whether the system determines a set of values and whether this set of values is unique. Exactly the same should hold for the reference of scientific theories. The whole relativistic argument then hinges on the problem whether science is capable of specifying its object. This question is of utmost importance on two counts. First because science is ontological: it is its main task to determine the nature of its object. Secondly, because little

sense can be made of the objectivity of science-in-a-vacuum: there can be no objectivity without an object; method alone will not do.

The answer to our main question then is that conceived as a formal system science is not capable of uniquely specifying its object. The reason has to do with the categoricity of formal systems. Two strong theses can be shown. In the first place that no formal system can determine its object (i.e., what it is about) except at best to an isomorphism, that is to say a formal system at best, if it is categorical, will be interpreted by models that are isomorphic to each other. This certainly represents a limitation since no set of objects is determined, yet one can still claim that the objects determined by the different isomorphic models are theoretically similar. We can still say that science determines forms: not sameness of object (identity) but sameness of form (isomorphism). The second thesis is even more radical: completeness is a necessary (but not sufficient) condition for categoricity. Even if science turned out to be a complete formal system this would not guarantee its categoricity. The fact of its incompleteness however guarantees its non-categoricity. In other words science cannot determine its object uniquely even up to an isomorphism. The grounds for such a claim can be many even though very few parts of science have been developed as axiomatic systems (about which the question of completeness can be asked meaningfully): there are two formal arguments at hand: the one utilizes Gödel's Incompleteness Theory and the

other utilizes the Löwenheim-Skolem Theorem. There are also informal arguments to the same effect that are of equal importance.

It is certainly beyond the scope of this essay to discuss the significance of Gödel Incompleteness Theorem or the Löwenstein-Skolem Theorem for science: the task would clearly require two additional volumes. It should be mentioned however that the Gödel theorem can be used to prove non-categoricity of at least recursive arithmetic and by extension of any scientific system that includes recursive arithmetic, as well as any formal system sufficiently rich to express recursive arithmetic. It does not matter that the self-referring Gödel sentence (that turns out to be either true and unprovable or provable and false) is not a statement about numbers, as long as we focus on the formalism. Given that the Gödel sentence in question is expressed in the language of arithmetic (by Gödel numbering) and that the two languages, arithmetic and Gödel-numbered translation of the formal metalanguage (in which the Gödel sentence initially belonged) are isomorphic, it would be futile from the standpoint of the formalism to distinguish the Gödel sentence from the truths of arithmetic. In a complete system the Gödel sentence if true should be provable, but since it asserts its own improbability, any proof of it would mean that either the axiomatic system is inconsistent (since it yields a falsity) or that the proof procedure by which the Gödel sentence was proven is not valid. In the first case the inconsistency means that the axiomatic system has no model;

in the second case the invalidity of the proof means, semantically, that there is at least one model that makes the Axiomatic System true and the Gödel Sentence false. The first case is for our purposes uninteresting. The second case however can be used to argue for the non-categoricity of the Axiomatic System because in addition to the aforementioned model there are models which make the Axiomatic system true (since it is consistent) and the Gödel sentence true (since it is true but not provable): this follows in accordance with the semantic account of consistency. The point of the argument is that now we have clear proof that an incomplete system would be non-categorical since the former kind of model is non isomorphic to the latter (in the former G is false, in the latter G is true).

I am bringing forth the Gödel Incompleteness Theorem not as a substantive claim for the incompleteness of science but in order to show the formal point about the connections between incompleteness and non-categoricity. The point is often forgotten and one has to go back in history to realize that categoricity was the old substitute for completeness. It remains to be seen that the scientific systems if axiomatized will lead to such results in ways that would be significant for the Relativist-Realist dispute: in that case the line of skirmish will be drawn around the previous isomorphism between arithmetic and the metalanguage in which the Gödel sentence was expressed. It may even be the case as Nagel and Newman conclude

that "the resources of the human intellect have not been and cannot be fully formalized, and that new principles of demonstration forever await invention and discovery" [1958, p. 101].

An equally, if not more disquieting result concerning the limits of formalism with respect to the determination of its object is to be found in the Löwenheim-Skolem Theorem. There are two ways in which the Löwenheim-Skolem Theorem affects our problem. In the first place the theorem itself sets limits on the capacity of any formalism to determine its object. Secondly the theorem when applied to the ideal of a formalized science implies that the models that are meant to spell out the ontology of a formalized theory cannot be isomorphic and as a result, except for trivial cases, science cannot be categorical. Let us take these claims one at a time. Our aim is not to examine the proof of the theorem but its implications for ontology and philosophy of science. For the exposition of the theorem I am indebted to Quine [1973], Delong [1970], and Beth [1959], while for a discussion of its significance I am indebted to the Symposium discussion between George Berry and R. Myhill.

The Löwenheim-Skolem Theorem states that if a set of sentences or sentence schemata expressed in the first-order predicate calculus has a model, that is to say, is syntactically consistent, then it has a denumerable model: in other words it also comes out true in the universe of positive integers. The trouble is that the above theorem leads us to recognize a limitation on the formalism because

it follows that no formalization is capable of capturing non-denumerability and this in turn matters because of the non-denumerability of the continuum: no formal system can capture the continuum, or if it does the formalized continuum is not a continuum any longer. Why is this so? To capture any property by formal means one has to devise a set of postulates which are consistent (otherwise they would be true of nothing) and whose every model shares the property in question: this latter would be the criterion of having captured that specific property. But now the Löwenheim-Skolem Theorem tells us that if our set of postulate is consistent then it has a denumerable model. This places indenumerability forever outside our reach: there can be no set of postulates that expresses indenumerability because not all the models of the set will share the said property of indenumerable cardinality: if the set is consistent there has to be a denumerable model.

This conclusion itself is certainly important because of the key role that the continuum has occupied in our views of nature. It is true however, as I will argue later that there is a general tendency towards the discrete especially in modern science, and I shall have the opportunity to examine it in the fourth chapter as a key element in explanation connected with reference. At this point what is important is to recognize that behind the formalistic inability to deal with indenumerability lies an even more significant problem for our inquiry concerning the nature and the limitations of interpretation. Let us examine the situation more closely.

Why can we not simply say, on the face of the Löwenheim-Skolem Theorem, that the indenumerable will always remain outside of our formal tools and rest contented with this? After all since the time of Pythagoras and Zeno we have been constantly reminded of our troubles on the matter. The problem is that one cannot ban the continuum and the indenumerable out of our theories, because there are theories that have as a proven theorem the existence of the indenumerable. Moreover, these theories can be formalized by set-theory. In set theory we have one primitive predicate the epsilon ' ϵ ' or more correctly ' $\textcircled{1} \epsilon \textcircled{2}$ ' where the circled numeral slots are to be appropriately filled with variables. Of course the predicate epsilon is now to be defined implicitly by the axioms of set-theory. The paradoxical result which follows from this is that on the one hand these 'defining' axioms can be used to prove within set-theory that there are non-denumerable sets of objects (by an adaptation of Cantor's proof). At the same time however and on a meta-theoretical level the Löwenheim-Skolem Theorem tells us that formalized set-theory has to have a denumerable model. One may be tempted to say here, correctly, that no such paradox exists as long as we keep in mind that the two contradictory corollaries belong to different levels: theoretical vs. metatheoretical. This is certainly correct, yet one has to take into account the problem of implicit definition of epsilon, because both operations the theoretic axiomatic and the metatheoretic development of models have as an aim the

spelling out of our intuition about epsilon. More specifically if the epsilon is to be understood in the first case in terms of the axioms of set theory then part of its meaning involves the proof of non-denumerability (which has to be combined in the axioms from which it was proven). If on the other hand it is to be understood in the formalistic sense in terms of models drawn in the metatheory then its definition certainly involves the denumerability that the Löwenheim-Skolem Theorem implies. This is the real import of the Skolem paradox and is based on the preformalized account of non-denumerability vs. the formalized account of epsilon that is denumerable. The non-isomorphism (by definition of isomorphism) between the denumerable and the non-denumerable leads to the conclusion that the theory at hand cannot be categorical: our theory doesn't know what it is talking about! To give a relativistic twist on the argument, our main predicate has two different implicit definitions: we cannot simply forget indenumerability for it is involved in defining the epsilon and vice versa.

The source of the paradox could lie either in the pre-formalized concept of the indenumerability, or on the formalization that excludes indenumerability from full categorical treatment. Or on the interpretation that connects the preformal with the formal conception. The majority of logicians focus on this third element and argue that the meaning of 'denumerable' changes from the preformal to the formal (together with the meanings of the other terms such as the epsilon

of 'is a member of' and even the conception of natural number). This maybe so, but the problem turns self-referential very quickly as soon as we realize that a significant part of the aim of formalization and models is precisely in order to deal with the vagaries and indeterminacies of interpretation. We turn to formalism precisely because we recognize the in principle possibilities of such indeterminacy. It is instructive here to see in quote Myhill's discussion of the matter, which I do not endorse in full, but which captures accurately the significance of the paradox for Relativism. Myhill draws a distinction between the private and the public aspect of formalism and proceeds:

...Formalism in its private aspect is a computational device for avoiding 'raw thought' - we operate with symbols which keep their shape rather than with ideas which fly away from us. All real mathematics is made with ideas, but formalism is always ready in case we grow afraid of the shifting vastness of our creations. Ultimately formalism is an expression of fear. But fear can lend us wings and armor and can penetrate where intuition falters, leading her to places where she can again come into her own. The Skolem 'paradox' thus proclaims our need never to forget completely our intuitions. We could shift to a formalism indistinguishable from set theory and it would be something other than set theory. It only remains set theory as long as the intuition of membership has not slipped away from us. It could be formally the same and have a grotesquely different meaning. The astonishing thing is perhaps less the Skolem paradox that formalism apart from prior interpretations does not completely determine its object, than the fact that an uninterpreted formalism can determine its object at all. At least, even if our intuition of membership perishes entirely,

we can rely on set-theory not to turn overnight into a theory of some finite group, even though we cannot guarantee that it will not turn into a theory about some complicated arithmetical relation. If it did it would look the same in print, though the motivation would puzzle the reader.

As to the public function of formalism, Myhill notes:

In those cases where formalism does adequately determine its object, we may consider that communication has been established. (But even this is really a matter of degree; for we presuppose a standard interpretation of truth-functional connectives even in arbitrary models)... But there seems to be no formal means of assuring that our conception of membership any more than our perception of a particular sense quality is the same as another person's. For no finite or even infinite number of formal assertions agreed on by us both could be evidence that his set-theory was not in any sense denumerable. Of course it would not be 'denumerable' in his sense, but I would not know if he meant by denumerable what I meant by 'denumerable' unless I knew that he meant the same as I meant by 'membership'. The second philosophical lesson of the Skolem-Löwenheim theorem is that formal communication of mathematics presupposes an informal community of understanding.

[1951, p. 50]

The connections with the relativistic position are now obvious. It is not just that 'natural number' changes in meaning across theories but rather a more important result (after all the relativists have not insisted on a mathematized or a formalized science). The point is that both private and public 'understanding' cannot be formalized to yield categoricity. If they are presupposed, they

still are indeterminate. The relativist has at hand a powerful argument in the form of a dilemma: there are two ways of conceiving science and scientific understanding: either as an empty formalism or as a representation of reality (models are common to both approaches, though they have a different meaning in each case). Yet both ways of taking science lands in indeterminacy. Since science is supposed to determine its object and can do so only implicitly, science is condemned to this indeterminacy of its object.

One cannot avoid the argument by drawing a sharp distinction between the mathematician's models and the physicist's. This move has been practiced (by Hesse, Harre, Wartofsky, and others) but has only pragmatic import. To say that the physicist uses models as analogies or as pictures or as representations, while the mathematician has a stricter account of them as 'true interpretations' (by means of a universe of discourse and interpretations of the predicate letters) misses the point that mathematics too is a science, and runs the danger of making more obscure a picture that is already obscure enough. For my part, I take it that the Skolem paradox shows that such a distinction cannot be drawn. The quotation from Myhill shows that intuition and understanding are as much a problem for the mathematician as it is for the physicist. Whatever problems the so-called 'physicist's models' present can be adequately captured by the indeterminacy of intuition and understanding. As for the analogical use of models in physics I am not persuaded that mathematicians are not involved in similar patterns when they milk their

intuitions towards the discovery of new theorems or rather towards the intuitive realization that some 'truth' not so far proven should be proven. How else but via intuition is a mathematician to choose the next 'truth' to prove? The whole point in this erroneous distinction rests on a supposed 'emptiness' of mathematics which in the light of the history of mathematical science and mathematical logic is certainly unwarranted. If anything the mathematician cares too much about the nature of his objects, as the previous discussion of the Frege-Hilbert controversy shows.

If a distinction is to be found, it has to do with attitudes towards 'isomorphism'. The mathematician will rest satisfied if he can determine his objects up to an isomorphism, because his categorical theory will have determined an identity of form, while the physicist is maybe seeking more than isomorphism, uniqueness of objects. This distinction, which is beyond my scope of examination, is one of subject-matter: the mathematician's objects are forms. Furthermore, it does not bypass the Skolem paradox: both the mathematician and the physicist have to face indeterminacy.

Before proceeding to connect these theoretical points with the Relativistic argument let us introduce one last aspect of the Löwenheim Theorem that has a strong bearing on our problem. The Theorem allows us not only to offer denumerable models of indenumerable ontologies but also the reverse (proven by Tarski [1942]). A system with denumerably infinite models can have indenumerably infinite models. Among the countable models then we have a distinction between the models that involve finite ontologies and the

ones that involve denumerably infinite ontologies. The latter kind clearly implies the non-categoricity of the system since by definition a denumerable and an indenumerable model cannot be isomorphic (there can be no one-to-one correspondence of elements otherwise the indenumerable would be denumerable). What is left, the system with finite ontologies, can be categorical, but the victory is only Pyrrhic: we could not rely on a system that requires a finite ontology for our investigations of the universe. On the other hand we could expand this system by Quine's method of hidden inflation [Quine 1936, p. 135] into a countably infinite system. We come thus to the same point emphasized by E. Beth that the Löwenheim-Skolem theorem reveals that no set of axioms can ever be categorical [Beth 1966, p. 490; also Henkin in Edwards, p. 72]. Beth proceeds to connect the point to the Frege-Hilbert controversy for in that same passage he cites the theorem in support of Bernays' argument of 1942 [review of M. Steck's edition of Frege's Letter to Hilbert] that "the axioms of Euclidean plane geometry cannot be taken the notions of a point or a straight line in the sense of Euclidean plane geometry; they rather define the set of all systems of entities which fulfill these axioms, that is to say the set of all models of these axioms." From this to the non-categoricity of Euclidean geometry is only a short step that is offered by the Löwenheim-Skolem Theorem.

One may argue that our procedure, if it is meant as an exposition of relativism prior to a critique of it, is unfair to the Realists,

for no where in their writings is there any appeal to the Löwenheim-Skolem Theorem, or to the Frege-Hilbert controversy. My general response is that whatever I am constructing is not a straw man out of an iron man but rather the other way round: I am building up relativism to its strongest before I offer arguments against it. I have argued that the majority of the relativistic argument is either in error or borrows its strength from a more basic argument. I have then argued that this more basic argument, the incommensurability argument is vague and thus affords three readings of which the first two are easily refutable while the third is indeed similar to the arguments that can be found in the Frege-Hilbert controversy and the discussions about categoricity following from the Löwenheim-Skolem Theorem. I believe that my point is also historically correct, because I believe that the main Relativistic argument has actual precursors in the twentieth century philosophy of science: it is this previous discussion that made the relativistic argument both possible and plausible. I am not however advancing a historical thesis here. To do so one would have to look at the indebtedness of the Relativists towards Quine and via Quine towards Bridgman, Poincaré, and Weyl. The point is that even if the Relativists did not make this argument explicitly, this is the argument they should have made, because it is their best chance.

To put the same point in another way, the claim about incommensurability can only be read as a point about indeterminacy of reference due to the implicit definition of scientific terms. One

however must establish this indeterminacy somehow, and the preceeding arguments is the best way, in my view of doing so: assuming that the relativists are maximally rational, I consider then this interpretation to be correct.

The point is worth developing further. We are interested in Relativism because we see in it an important epistemological challenge. This challenge cannot be established, if one takes Relativism simply as a historiographical thesis or simply as a thesis about actual scientific practices that fall short of a much advertised ideal. Their point is deep and an in principle one. One then has to seek out and establish this in principle point.

The indebtedness of the Relativists to these precursor arguments can be established in yet another way. The most complete attempt to answer the Relativistic claims was made by Scheffler in Science and Subjectivity. In that book Scheffler argues successfully against the historical claims and claims about scientific practice by offering counterclaims of the form "it is not always so" or "it is not necessarily so." Concerning the arguments from perception Scheffler shows that once one abandons the sense-data theory, the Relativistic point is hardly worth making. Most importantly however Scheffler grants that the meaning of theoretic terms is indeterminate (or rather system determinate and therefore indeterminate) yet he brings two relevant points to bear on the matter: (a) meaning is not supposed to be determinate anyway (and in any case); (b) but even with indeterminate meaning scientific objectivism, communication, and

progress is possible as long as reference is determinate. His claim is that reference is determinate. Scheffler's work then should have effectively quieted the Relativist's qualms about the objectivity of science, had it not been for his position on reference. After the publication of Science and Subjectivity, Quine wrote the Dewey Lectures on Ontological Relativity which effectively undermines Scheffler's basic contention, because reference now is shown to be as indeterminate as meaning. I do not believe that the aim of Quine was to support the Relativists I have in mind. As a matter of fact in the context of Quine's general theory, it is obvious that the Ontological Relativity Thesis (together with the related theses of the Indeterminacy of Translation and the Inscrutability of Reference) is meant primarily as an impossibility proof against certain philosophical and linguistic schools (present and past). The interpretation of Quine is a difficult task due to his skillful evaluation of position and his admission of circularity: for example, the ontological relativity thesis can only be drawn on the basis of his stimulus theory of meaning and this in turn presupposes naturalism and the ontology that accompanies naturalism. However the point remains that one can use Quine's argument rests heavily, by Quine's own admission, on Weyl's argument about isomorphism and non-categoricity. Indeterminacy of reference then is the natural last but strong bastion for relativism. But even without Quine's indeterminacy theses, there is an escape route via reference always available to the relativist. Scheffler's main contention

consisted of admitting that meaning was indeterminate (or system-determinate) as it should be, but insisting at the same time that reference was not. If the second part of the claim (about reference) was meant as a basic contention, it certainly lacked theoretical backing. It must then be read as a challenge to the relativists. Unless they show reference to be indeterminate their challenge to the objectivity of science fails. Yet, further reflection on the indeterminacy involved in implicit definitions would have led one to the relativistic conclusions. If meaning is indeterminate, so must reference be as well, because, after all reference is determined through meaning, or is it?

In the next chapter we will argue that it is not, but this claim comes as a result of the causal theories of reference developed after 1970. Before that time the claim that reference was determined by way of meaning was accepted and endorsed by tradition as true. As we saw in Frege, while the informativeness of identity statements lies in the fact that they connect different senses, and while definitions provide new senses, still the aim of both kinds of statements is to establish reference. Russell's theory of descriptions is drawn in accordance with these basic guidelines and finally Quine pushes this line to the extreme by arguing for the elimination of all singular terms claiming primacy for predicates (rather than names) and for the semantic relation of denotation (true of ___) rather than naming. Within this tradition it is clear that meaning determines reference and that naming can be subsumed under denotation

(in its traditional not its modern sense). If this theory holds then relativism really survives Scheffler's objections but only as long as every other argument that the relativists have offered (and Scheffler has countered) hinges on what I have called the main Relativistic Argument about the reference of scientific terms.

Before proceeding further let us summarize the whole development of the Relativistic argument that leads to the non-categoricity of science. The Relativists criticized the Positivistic conception of science by arguing, correctly, that science is not just a formal instrument for prediction and control of appearances. Instead the aim of science proper is to uncover the nature of reality by offering representations of reality. Central to these representations are the theoretical entities that are needed to explain the phenomena. Given that observations are theory-laden, the choice of different theories (different representations employing theoretical terms) cannot be based on observation; neither can an observational account of theoretical terms be given for that same reason. The theoretical terms have meaning and reference by virtue of the theory in which they figure: changes or differences in theory result in differences in scientific ontology. The success however of any such implicit definition is based on the capacity of a theory to determine one, unique set of objects of which it is true: the ontology of a theory is to be determined by the model(s) that make the theory true. Yet upon closer examination we find that there are limitations to the determination of ontology by a theory. Science at its formal best

determines its objects only up to an isomorphism of many models. Indeed even this is a virtually unattainable ideal. It is this radical indeterminacy in ontology which lends weight to the relativistic epistemology. Borrowing a Goodman-style metaphor we can say that science as a map (representation) is incapable of determining the mapped absolutely but can only at best aspire to an isomorphism with another map; in fact we are condemned to compare maps to other maps rather than to reality since in the scientific map-making there is no prior agreement as to the nature of the mapped: the whole purpose of the 'map' is to determine the nature of this unknown reality.

The idea that science cannot determine what it is about, and the explanation of its obvious correspondence with the world as still another function of the system has a long history traceable in modern philosophy to Kant who despaired over the possibility that science might describe "things in themselves." The Kantian argument however can be left to the side as based on a psychological theory: its significance lies in the influence that it exerted on subsequent philosophers of science. A better statement of the problem was introduced for the first time in the Frege-Hilbert correspondence over the function of definitions and the capacity of axioms to serve as implicit definitions. We have examined this already as the first obvious precursor of the modern problem: in the controversy over definitions reference is explicitly brought up as the heart of the problem. Another precursor to the modern relativistic argument

is to be found in the theories of H. Poincaré. Specifically the Relativists are indebted to Poincaré in two ways. In the first place his general theory of science is one that eliminates ontology in favor of formal structure. In his view science was unable to determine the nature of things but only the relations holding between predicates true of things. The second place where Poincaré's theories coincide with modern Relativism is the former's critique of Maxwell. Poincaré argued against Maxwell's valiant attempts to develop mechanical models for electromagnetism by insisting on the one hand on the need for formal axiomatic science [in his review of Maxwell - 1905, pp. 210-224] and by offering a proof that if one model can be developed to fit the observational data, then an infinity of them is available [Preface to Electricité et Optique]. We have argued that this argument in its turn is reducible to the categoricity argument. It is nevertheless used in anti-realistic arguments by Bridgman in The Way Things Are (p. 195) and The Logic of Modern Physics (p. 48-49).

The Frege-Hilbert controversy brought up the problem of indeterminacy of implicit definitions, while Poincaré introduced the problem of indeterminacy of models. The two ideas are brought together and their epistemological significance is traced in H. Weyl's Philosophy of Mathematics and Natural Science.

... the idea of isomorphism... is of fundamental importance for epistemology. ... Isomorphic domains may be said to possess the same structure...

These considerations induce us to conceive of an axiom system as a logical mold ("Leerform") of possible sciences. A concrete interpretation is given when designata have been exhibited for the names of the basic concepts, on the basis of which the axioms become true propositions. One might have thought of calling an axiom system complete if in order to fix the meanings of the basic concepts present in them it is sufficient to require that the axioms be valid. But this ideal of uniqueness cannot be realized, for the result of an isomorphic mapping of a concrete interpretation is surely again a concrete interpretation. Hence the final formulation has to be as follows: an axiom system is complete or categorical, if any two interpretations of it are necessarily isomorphic.

[1949, p. 25]

Concerning this last clause we have found that it holds true only for categoricity (by definition) but it is only a necessary not a sufficient condition for completeness. All the same the epistemological significance of the point is clear:

A science can only determine its domain of investigation up to an isomorphic mapping. In particular it remains quite indifferent as to the 'essence' of its objects. That which distinguishes the real points in space from number triads or other interpretations of geometry we can only know by immediate intuitive perception. But intuition is not blissful repose never to be broken, it is driven on towards the dialectic and adventure of cognition. It would be a folly to expect cognition to reveal to intuition some secret essence of things hidden behind what is manifestly given by intuition. The idea of isomorphism demarcates the self-evident, insurmountable boundary of cognition. This reflection has enlightening value too for the metaphysical speculations about a world of things in themselves behind

the phenomena. For it is clear that under such a hypothesis the absolute world must be isomorphic to the phenomenal one... Thus even if we do not know the things in themselves, still we have just as much cognition about them as we do about the phenomena.

[1949, p. 26]

These two passages which echo in the later works of Quine contain the best statement, in my view, of the basic Relativistic argument. I am not claiming that Weyl was a relativist. Later in the book when he offers his own version of constructive cognition he demands that the outcome of the cognitive operations be uniquely determined by the 'given' [1949, p. 37]. Yet this trust on intuition comes under fire in the period that followed the writing of this work. Once the 'given' is removed as a determinant of uniqueness the road to relativism is open: hence the importance of the theory-ladenness of observations, for the Relativists.

What we have shown then so far is that at the heart of the Relativistic positions lies a very strong argument that supports all the other arguments and turns Relativism into a serious epistemological position that challenges the scientific enterprise. Furthermore, it is shown by this argument and the history of this argument that recent Relativism has behind it a long tradition of anti-realistic thought. What is one to do with this argument?

Let us start by noticing its plausibility. It is not easy to dismiss the argument by simply appealing to the success of science, for what is at stake is not just the success of prediction and control

but the determination of the nature of things. On that score one could not dismiss the argument by claiming that surely scientists, if anyone does, know what they are talking about: they know, in other words, the reference of this discourse, the ontology of their theory. Pointing to the actual success of scientific communication does not help as long as the point is one of principle: How do we make sure that we know what we are talking about in science? How does science determine its object? Since we cannot rely on 'understanding' or 'intuition' to carry this task (this would beg the in principle question) we have two roads available. The first road is the road of formalism: surely in the axiomatic method we have one cognitive tool that is not subject to the vagaries of private understanding or intuition; whatever we are talking about is captured in the formalism, any excess meaning or connotation if not formalizable can be left on the side. Concerning the ontology of such formal systems again we have to make some commitment to the concept of truth and then proceed to develop the said ontology formally by models that make the theory come out true. The second road, which is found in Frege (and Aristotle) is that of the definition (or determinations). According to this view it is the definitions that carry the ontological weight of a scientific theory.

If one reduces the way of definitions to the first way, that of formalism then clearly science becomes ontologically indeterminate. This is done by insisting that the key definitions in science are

implicit: in other words there are no ontological definitions in science over and above the axioms of the system: it is the system as a whole that determines its ontology. Since the only legitimate way it can do so is formally, by models, science is subject to the non-categoricity proofs outlined before. This clearly shows the plausibility of the argument. One should keep in mind however that the Aristotelian-Fregean account of definitions must be abandoned in favor of the implicit definitions, in order for the argument to be effective.

The argument that science as a formal system cannot determine its reference because it cannot be categorical cannot be fought on formal grounds: it is formally correct. What can be (and must be) challenged are the underpinnings of the argument that allows science to be considered as such a formal system. Let me explain here. The non-categoricity proof works only if we are considering uninterpreted formal systems. It is these systems for which it is impossible to determine unique models. The question is whether science can be conceived in this manner, i.e., as just a formal axiomatic system, a calculus that remains uninterpreted and whose reference or ontology is provided by models that can at best be isomorphic to one another. What we are questioning there is the use of this argument in the case of science.

Here one may be tempted to quickly claim that there is a difference between mathematical and physical models and furthermore

that in the physical sciences we care not about every model but about the intended models. Even though there are elements of truth behind each one of these claims, it would be too hasty to reply and up to a point counterproductive. The supposed distinction between mathematical and physical models cannot be accurately drawn simply because of the vagueness that surrounds the so-called physical models. By 'physical model' we cannot mean just 'mechanical model' because even though 'mechanical model' is a clear notion (a model that utilizes only the laws of mechanics) it is too narrow. The other characterizations as 'representation' or 'picture' or 'analogy' are by their own nature vague: we are running the risk of explaining the clear in terms of the obscure misled by a supposed familiarity with these vague terms. On the other hand the 'intended' model suffers from similar troubles together with additional troubles of mentalism and meaning: the intended model is supposedly the model that the scientist 'had in mind'. This would lead us straight back into the hands of the relativist.

Before utilizing such distinctions one ought to clarify them sufficiently, and select out of the distinctions whatever is both clear and true. As I have argued earlier the basic distinction in kind exists not between mathematical and physical models but between logical and scientific models; the latter including the models of both mathematics and physics. This distinction is drawn along the lines of uninterpreted vs. interpreted systems. The distinction between mathematical and physical models is minor and

relates to the tolerance towards isomorphism: the mathematician would be satisfied with a categorical system, the physicist, if he is a realist, should insist on a unique model.

This observation has repercussions for the concept of 'intended' model as well, because now it is possible to speak of intended models without going into mentalism. To do so we have to utilize Henkin's idea of 'standard' model vs. non-standard models [Henkin, 1951]. Standard models are developed on the basis of prior interpretations. By utilizing standard models one can bypass the Löwenheim-Skolem noncategoricity proof. The important question then is whether science operates with standard or non-standard models, and we will argue that it operates with the former. But let us explain standard models from the beginning.

In discussing the Löwenheim-Skolem Theorem we said that for any property to be formalized we must construct a set of postulates all the models of which should have the property in question. Following Myhill's exposition we showed that some properties concerning the cardinality of the domain cannot be formalized (eg. indenumerability). Apart from indenumerability a formal system can express some cardinality properties: it can express for instance the property of having at least a countable number of members. It is significant however that we cannot by purely formal means express the property of having at most a certain finite number of elements. Having the capacity of expressing the 'at most' property is essential in establishing the 'uniqueness' of either an object or of a set of objects.

Consider for example how we show uniqueness in Russell's Theory of Descriptions: we have to show that there is at least an object fulfilling the set condition and that there is at most one such object. This proof was known since the time of Kant's metaphysical proofs of the existence of one space and one time.⁵ How is this achieved in Russell? While the 'at least' property is carried out formally by the ' $(\exists x)$ ' quantifier, the 'at most' property requires at least two variables (or $n + 1$ where uniqueness of n is to be shown) and the relation of identity. The problem is that in purely formal uninterpreted systems we cannot include identity because it would be an interpreted predicate; what we can include is equivalence as an isomorphic equivalent of identity, but this does not allow us to capture the 'at most' property any more because equivalence is a wider logical property than identity: equality of length, weight, etc. are all equivalence relationships but they are not the identity relationships.

As long as identity is not introduced as an interpreted predicate, we cannot avoid the non-categoricity of the formal system. But if we consider a system where the predicate of identity ' Ixy ' is considered interpreted as ' $x = y$ ' then we can achieve categoricity for the rest of the system (which includes other predicates besides I ① ②). It is on the basis of this proof of Henkin that we can establish the distinction between standard and non standard models as the key to describing the ontologies of formal systems. Specifically the standard models are the ones which are in agreement with

a preassigned interpretation of a predicate (in this case as in most cases the interpretation of the equivalence predicate as identity). Non-standard models are then defined as the ones which do not agree with the pre-assigned interpretation [Myhill, p. 49]. It is significant to note here that some writers want to make the standard models narrower by limiting the preassigned interpretation specifically to the identity predicate [Hunter 1971, p. 198 ff]. Henkin expresses the matter in yet a different way but which amounts to the same thing: the trouble originates in that we cannot formally distinguish, if we stay within first-order predicate calculus, identity from any other equivalence relation. But if we are allowed to move to a higher-type then we can do so by Leibniz's Law which quantifies over predicates (two objects are identical if they have exactly the same properties and vice versa). If then we are allowed a theory of types then it is possible to formalize identity as a unique relation (distinguishable from equivalence) in a higher-order calculus. This then would allow us to establish the completeness and categoricity of some formal systems by drawing the legitimate now distinction between models that are standard relative to identity and models which are not standard relative to identity. It is this proof that is established by Henkin in the 1951 paper "Completeness and the Theory of Types". We can make use of the proof then in the way of Myhill.

One can still claim however that this new categoricity is achieved by a sleight of hand because the formal system cannot be considered as completely uninterpreted: or, putting the matter in another way: standard models are relative to a preassigned interpretation. This is correct but what is worth asking is whether science as a formal system proceeds by preassigned interpretations of some of its predicates and establishes reference for the rest of its terms by standard models relative to these preassigned interpretations or whether it proceeds in a thoroughly formalistic way by first developing an uninterpreted calculus of predicates and then seeks models in order to establish its reference tout court (rather than piecemeal). I will argue that not only scientific reference but all reference generally proceeds in a manner different than the formalistic. Indeed this has been one of the major tenets of the recent (1970 onward) theories of reference. But I am running ahead of my story.

What can be gained then from Henkin's contribution? In the first place his distinction between standard and non-standard models makes the notion of 'intended' model more understandable and more subject to our formal analysis. To insist on 'intended' models without explication other than 'what the scientist had in mind' takes us back to the relativistic position, only this time it is the various 'intended' models that are incommensurate. Still the notion of a 'preassigned' interpretation carries with it that element

of a private conception, yet the problem is contained now to one element not to a whole system. We will examine this difficulty further.

The second element of Henkin's way out is that not only does it leave us a way open out of the non-categoricity predicament, but moreover it guides us into that way! I explain: my use of Henkin's contribution is not meant to be a simple counterexample to the in principle non-categoricity claim; I maintain that the view of reference that is contained in the idea of standard models is essentially correct not only in the case of science but also in all cases of reference. Let us take the claims one at a time.

One could claim that Henkin's contribution does not help us a lot because, while categoricity is achieved on one level, on a different level indeterminacy is preserved or even assumed, since standard models are drawn relative to a preassigned interpretation. To make the case even stronger consider scientific formal theories which contain one predicate as in the previously discussed example of set theory. Since the epsilon is the only predicate of set theory one could not speak of standard models helping us establish the categoricity of set theory. As a result Myhill's conclusions (quoted at length) follow: mathematics rests on intuition and previous understanding among mathematicians. We said that this conclusion lends even more support to relativism. Such a conclusion however is available only in extreme cases where one basic predicate is employed.

As primitive predicates are multiplied the situation does not become more confused but rather clearer: it is easier to decode fifty sentences than to decode one. Specifically Henkin finds a way of limiting the available models relative to an interpretation.

One can take the claim even further and argue that reference in science is never achieved 'post-formalization'. Even in scientific revolutions new terms are never introduced in a vacuum and never has a theory originated as a formal uninterpreted calculus. Even the most philosophically problematic terms are introduced after a lot of discussion (remember Aristotle's preliminary dialectic), while all terms indeed are established by interconnections with previously established terms. Sometimes new magnitudes are developed simply by combining mathematically previously well established other magnitudes (energy, momentum, etc). Thus even if one agrees that terms acquire reference by interconnections with other terms, still one has to establish distinctions between terms which are accepted as referential previously and terms whose reference is not yet clearly established. Reference in science then is not a holistic all-or-nothing affair but rather a piecemeal effort in which we erect less stable structures on more stable ones.

We haven't yet answered the in principle argument however, because now the indeterminacy is moved to the preassigned interpretations: the Relativist can claim that still different scientific theories are referentially incommensurate because their preassigned

interpretations to key predicates are referentially indeterminate and inscrutable. Before despairing let us note what we do have: if nothing else we have at least limited the indeterminacy problem, we have 'contained the fire' so to say, yet at a high cost: if we cannot do anything about this indeterminacy we are bound to return to the previous predicament because standard models are relative to these preassigned interpretations. At best we will have achieved more interpretations, more structure, since in addition to the many non-isomorphic models we will have a whole host of additional sets of models that are standard relative to one or another preassigned interpretation.

It is important here to note the crucial role that identity plays. The preassigned interpretations normally revolve around this terminal concept. If one is permitted to use a 'classical' understanding of identity (Leibniz) the non-categoricity predicament is avoided. One can understand now why it is important for Quine to argue in Ontological Relativity that the conceptual apparatus for establishing identity is indeterminate and can be systematically distorted to square with any conceivable ontology (events, objects, detachable and undetachable parts of objects). Let us also note here the connections that identity has with (a) definitions and (b) reference, as we established in our discussions of Frege. An examination therefore of identity is a key to understanding reference, and specifically scientific reference. As we will see Kripke's theories come hand in hand with a new analysis of identity.

The trouble with identity, as Frege quickly recognized, is that it appears as a trivial concept. If it is a relation it is one which an object can only have with itself (otherwise it would be false). How can such a trivial relationship play such a crucial role in science? We saw that it can formally make the difference between categoricity or non-categoricity of a system, but can its significance expand beyond the formal and into our views of the world? We can have two schools here: on the one hand we have thinkers who consider identity as a primitive concept open only to intuition (as far as it is used in our view of the world): Quine would be a good example here. On the other hand thinkers like Ruth Barcan Marcus claim that there are many different conceptions of identity even on a formal level. She has argued, as we will see in the next chapter, that one can spell out identity by stronger or weaker equivalence. If these two alternatives formed an exclusive dilemma we would certainly be in trouble because either way identity is shown to be indeterminate, either cognitively or formally, and it would remain as a rather trivial relation.

What is missed in the previous dilemma is that in the first place identity is not a trivial relationship: while it is a necessary relationship, still it is not an a priori or an analytic relationship (this is the main import of Kripke's contribution). We can say then that in addition to a common formal structure the identity relations share the feature of necessity. The problem is how is this necessity spelled out in each case in such a way that identity

statements are true and yet informative (about the world). Frege's solution about establishing reference (the idea of different senses is questionable), is only the first step. As with the other great concept of science, causality, identity too is spelled out differently as our knowledge of the world progresses. To give a rough example: identity of species is different before and after Darwin, because after Darwin to say that two species found in different continents are identical or belong to the same genus implies a whole host of facts about the evolution of the said species (as well as facts about geography-geology, etc.). Even a more banal example if one knows that a certain flu virus causes the patient to have the same symptoms twice over, then the claim that one is suffering from the same illness the second time has a different meaning: if one didn't know the fact, suffering from the same illness would mean that one has contacted the same disease twice (different viruses) and so the implied causal lines would go to two different occasions where one was in proximity with such viruses, while in the second case the 'same disease' means 'same viruses'. The examples can be extended: prior to Darwin it would make no sense to say that two species with identical characteristics found in different parts of the globe have a different evolutionary history and thus belong to different more general species. As our knowledge of the world increases our judgments of identity change, but always necessity accompanies them. Identity then is not an indeterminate relation: we can always spell it out by appeal to origin or molecular structure. We will have

opportunity to discuss the problem further in our discussion of Kripke's theories, so let us continue with Henkin's contribution, keeping in mind for the time the crucial role that identity and identification plays in definitions and by extension in reference.

What I consider important in Henkin's contribution is an alternative model of reference in science (which can afford us categoricity). We apply Henkin's model to our own problem by arguing that all reference and especially scientific reference is not a uniform, holistic affair when a whole formal system of uninterpreted predicates is given reference via models. Instead the process is highly differentiated: some terms have reference, certain rules for referring are laid out and for more terms reference is achieved both by the use of prior terms and of prior rules. In all these various processes of definition, rather than satisfaction by a model is the key, and by definition one should understand a wider notion that includes identities, identifications and determinations (like "water = H_2O ").

The anti-realistic conception of science then takes science to be a system of predicates. Predicates are all considered to be on a similar level: any differentiation among them should be expressible as further relations between predicates. From the standpoint of reference all of these interrelations must be taken to be meaning postulates, spelling out the meaning of the key terms of the system. Reference is indefinite and carried out by variables that are attached to these predicates: the way reference is to be formally carried out is by models. Truth in its turn, which is

a function of reference is spelled out by satisfaction of the predicates by objects: there is to be no differentiation of the different kinds of objects except for their place in a sequence: all cognitively significant differentiation is to be expressible by the predicate relations.

This formalistic conception of science has holes in it. Consider for instance the generalization formula that is supposed to be the key to explanation. It cannot distinguish between the accidental and the lawlike generalization. Except one is willing to accept kinds, as we do in science, this problem will not easily be bypassed. Simply put, all predicates are not on a level according to our science, nor is it possible to express this point formally without recourse to a good account of identity. In the realistic conception of science, from Aristotle onward, predicates are taken as distinguishable by categories with kinds playing a key role in scientific explanations. Further research has shown the initial intuitions about the many natural kinds as correct and have furnished us with new means of discussing their identity. Formally such an approach of course requires the introduction of modalities for one way of distinguishing predicates is by appeal to essential and accidental properties. As we will see in the Third Chapter the anti-realistic argument that claims necessity to be a function of the way an object is described (de dicto rather than de re modalities) is not a sound argument. Reference in the realistic conception is not only piecemeal and drawn on the basis of identity but is also highly differentiated.

As is the case with maps, in science as well terms fall into kinds and it is clear that reference is to different kinds of things. Furthermore, as in ways, these kinds are interrelated: one recognizes acceleration to be a different kind of thing from velocity (and there is a corresponding formula to show the difference in kind), or from energy or from momentum. More importantly, in the multiple ways in which these magnitudes are expressed there is a clear correspondence between the various determinations. It is this determination of kinds of things and magnitudes that often generate conceptions of new kinds or of new magnitudes. These magnitudes that are first drawn on paper and are then sought in nature point to a scientific conception of truth as correspondence much richer than the correspondence contained in the Tarskian account between strings of signs and sequences of objects. Keeping in mind the dangers of using analogies, one could liken the scientific correspondence to the map correspondence discussed in Chapter 1, since in maps one gauges and explores new and unknown regions on the basis of the known ones. Much of the success of this enterprise, as I have argued, rests with the fact that reference in maps is highly differentiated into kinds of things and the kinds of things differentiated in maps are interrelated.

In science, to a high degree and in language to a lesser degree, reference is not uniform but highly differentiated and 'coherent'. These differentiations in kind cannot be taken as 'meaning' relationships: they have enough structure to enable one to distinguish

kinds of things even if one is unaware of the precise meaning of the terms involved. Specifically reference is differentiated in accordance with 'categories' (plus syn-categorematic terms which lack proper reference). In science we try to keep our differentiations clear and our interconnections coherent; ordinary language on the other hand has only grammar and syntax as means of differentiation and often falls prey to category mistakes and to vagueness. Ordinary discourse does not have to be ontological, or ontologically correct.

There are two objections to the view of scientific reference that I am proposing. The first comes from the Relativist camp. Using Toulmin's distinction between descriptive (Natural History) and explanatory (Physics) science one may claim that while kinds and differentiations of kinds is important in the first type of sciences, it is the second type which has philosophical significance for the Relativistic argument. The second objection comes from the formalist and positivist camp and claims that whatever scientific knowledge there is about kinds, essential properties, etc., should be expressible by further relations of predicates: "One should explain 'kind' by appeal to properties. If no such explanation is forthcoming then one ought to abandon talk of kinds and essential properties." Once again the realist is put in front of a dilemma to either cash in his knowledge of kinds and categories for predicates (that will lead him to the non-categoricity proof) or to abandon this knowledge altogether.

Despite their different origins the two objections are fundamentally similar because they are based on a same view of reference. According to Toulmin, the natural historian starts from a position of agreement with common sense as to his categories and the only thing he seeks is further regularities. These further regularities cannot really serve in explanation, I suppose, for two reasons: in the first place because the D-N model in which particulars are explained by appeal to the kind (Toulmin's example: Chi-chi is black because he is a raven and all ravens are black) is only a trivial explanation, and in the second place because the prior agreement as to categories would turn any such explanation circular in cases where we are dealing with deeper probes into the nature of reality, since part of the aim is precisely the explanation of such categories. If one has to assume the categories at the start one is simply reproducing on a different level (microscopic or macroscopic) reality as we know it. This second reason is the old dilemma between the requirement that the explanans be familiar (since the only way one can be said to understand is by way of what he knows already) and the requirement that the explanation be a real reduction and therefore unlike the explanandum (otherwise the explanation would be circular). Of course, no one makes such errors like explaining the color of the object by appealing to the color of the atoms, yet the point still stands. What is wrong then with the objection?

In the first place it takes a limited view of natural kinds. While it is true that kinds of organisms have attracted man's

curiosity more than other things, one should also consider as kinds a whole host of other things. A particle is a kind and so is a wave. The scientific determination 'sound is a wave' is a genuine explanation even though both 'kinds' are rather well known. In the second place the objection takes a limited view of our knowledge of natural kinds as consisting of generalizations alone. The important thing however in our knowledge of kinds are not generalizations but essential generalizations (i.e., generalizations plus necessity). Here the fallacy is one of misrepresentations: by focusing on the rather silly generalization of the ravens (which is silly because we know that plumage in birds does vary) one extends the notion to cover cases where it simply does not fit: no one claimed blackness to be an essential property of ravens. There are however characteristics of ravens (about their DNA structure, about their physiology, about their evolution) which occupy a more important role in our knowledge of ravens and in these cases if we find unexpected results our knowledge not only of ravens but of other organisms as well will have to be revised: consider the difference between finding a white raven and a raven that reproduced not by laying eggs but in the manner of mammals. In the latter case there are a lot of serious implications concerning not only ravens but a lot of other organisms. Even under such a moderate approach to modalities, crucial characteristics like the above came to be called essential as affecting our knowledge not only of the species but of the genus as well.

This realistic position however is easily assailable as long as one does not possess an adequate modal logic that could distinguish essential predicates from non-essential ones; the above criterion is rather vague. Consider for instance the example above: while it is true that ravens are birds and all birds lay eggs it is not true that all birds are black; so the objection goes, talk of essential properties is simply reduced to talk of genera, but even there our only course is to proceed by way of laws that combine predicates. Furthermore, one could claim, 'genera' are simply constructs: a set of interrelated predicates. Similarly with the claim that sound is a wave: there is no such kind as a wave, there are wave equations. Both waves and birds must be thought of as forms and there is no other way of spelling out these forms except by systems of predicates that exhaust hopefully the 'intension' of the general term in question. If these systems of predicates happen to be true of a phenomenon, then the phenomenon is classified under them.

It is at this point that the two objections run a similar course. The Relativists and their precursors as well as the Positivists share the traditional view that reference is established by way of meaning, that intension determines extension. According to this traditional view each term is associated with a group of predicates either by a nominal definition (establishing synonymy) or by a group of sentences. These 'meaning postulates' are the key to reference because on the basis of these postulates we can seek objects

fulfilling these postulates and it is in this manner that the reference of a term is established. To refer to dogs, for example, one must have, ideally, a set of predicates true of all and only dogs and then on the basis of this list one can judge object *x* or *y* for 'doghood' or 'non-doghood'. Similarly to judge Fido to be a dog implies that one has applied the characteristics to Fido successfully.

This idea of reference has all the blessings of tradition. Leaving aside individual objects with their infinite characteristics it focuses on general terms as paradigmatic cases of proper referring. These general terms are paired with predicates, all on a par, that exhaust the meaning of the term, and if these predicates jointly have denotation (i.e., are true of an object) then the general term refers. Notice how helpful is the idea for formalization: one can dispense with reference now and concentrate on predicates that can now be put into a system. We can even employ an abstract axiomatic system to turn our knowledge into a deductive system. Yet still the problem of proper names persists: how can one turn our acquaintance with individuals into general knowledge. The answer is provided by Russell's Theory of Descriptions. If we can find a predicate that can be considered synonymous with the proper name then we can extend the above procedure that applies to general terms to proper names. Descriptions came to fulfill this role. Russell claimed all non-logically proper names to be descriptions in disguise; Quine proclaimed all names to be eliminable by way of descriptions. In

this way all referring, including 'naming' is reduced to denoting (being true of). This abandoning of the direct referential relation of naming allows us to turn our knowledge completely formal: we need nothing but predicates. Yet the cost is rather high: with naming out of the way the relations between our knowledge (a system of predicates) and the world becomes problematic, especially when it can be shown that the system of predicates in question is non-categorical: i.e., is not uniquely true of one set of objects. The problem is even more acute for theoretical terms because their meanings do not have the benefits of the pre-theoretical acquaintance with the referent. Since the meaning of these terms is completely a function of the system, and since they are in this manner understood as 'forms', the proof of non-isomorphism hits at them the hardest. These 'forms' do not refer because the system has no isomorphic models, and thus it cannot determine the sameness of forms, for the theoretical terms to refer to.

The problem at hand then can be reduced to two problems: (i) How is reference of scientific terms achieved? (ii) How is reference in general achieved? Contrary to the Traditional Theory of Reference the new Causal Theory of Reference (CTR) takes naming as the paradigmatic referential relationship, and instead of names being treated on the model of general terms, many general terms are treated as proper names. Species are referentially conceived in the manner of individuals. Typically one does not establish the extension of such general terms by way of their intension. According to the

CTR, when one attributes a kind-term to an object, it is not done on the basis of a list of predicates: instead what takes place is an appeal to a causal line that connects the present speaker via the community of speakers to the initial encounter with the species in question, as well as to a causal line that runs through the species to the present member. While meaning postulates do matter in actual establishing of reference, no set of meaning postulates can exhaust reference. There are however characteristics that are crucial to a kind, in the sense that any object which would lack any one of them would not belong to the kind: any body of liquid that is not H_2O will not be water despite possible similarities in all other respects.

At this point, and before proceeding further with reference, it would be instructive to pull together all the strings of the book.

1. In the first chapter we argued that the kind of correspondence that science cares about (the kind of correspondence that the realist has in mind) is similar to map-correspondence because it involves eidetic reference, that is to say reference to different kinds of things interrelated in many ways. The Tarskian correspondence in its absolute form by contrast involves just two modes of reference to sequences of objects without differentiating kinds, as far as reference goes. Any differentiation in the Tarskian Theory is not done as part of reference but only as part of meaning: on the level of predicates. The Tarskian theory is not a theory of categories.

2. We also argued in the first chapter that reference (and specifically eidetic reference) matters to science because science is essentially ontological: it seeks to uncover the nature of things. It does so by scientific determinations (subsumptions, identities, and identifications) which include definitions. It is these determinations that establish reference for scientific terms, by showing the interconnections between kinds of entities.

3. We have also argued (Chapter II part 1) that the problem of reference should not be thought of as a group of puzzles whose solutions is a criterion for the construction of an adequate semiotic theory of referential semantics. Instead a careful examination of the problem of reference in its genesis reveals that it was initially formulated by Frege and Russell as a problem of objectivity for formal science: how can formal science determine its object (i.e., what is it about)? Moreover, it is this aspect of the problem that is relevant to our work because it is this problematic tradition that reaches to the modern Relativists by way of the Frege-Hilbert controversies, the work of Poincaré and Weyl and the work of the Positivists. Reference is important as the key to ontology and thus to objectivity of science.

4. We also found that most of the Relativistic arguments are weak and have only partial application which makes them unable to dislodge the Positivistic account of science from its normative stronghold. There are however two strong points in support of relativism. In the first place they have argued for a metaphysical

conception of science (vs. a verificationist one). Secondly they have one strong argument about the indeterminacy of reference: the idea that science as a formal system is unable to determine its object except, at best, up to monomorphism. As a formal system science leads to an indeterminate ontology since it is not categorical.

The four strings are connected in obvious ways. To say that science is ontological is to isolate the form of scientific determinations and definitions as vehicles not just of reference (Frege) but of eidetic reference. How can eidetic reference take place is the subject of our next chapter. In what remains of this chapter here I wish to show the connections between the fourth and the first and second strands, which is the only aspect of the interrelations that I have not developed yet.

We said that the Relativist agrees with the Realist part of the way: in opposition to the Verificationists they both affirm that science aims at offering us a representation of reality, and not merely a way of organizing into formal systems sets of statements about appearances for the sake of controlling and predicting further appearances. There is a vast difference however between the Relativist and the Realist with respect to this idea of 'representation'. Both take it as a referential function, which it is, but they differ as to their account of reference in such a way that their views of scientific representation become radically opposed in virtually every point of comparison. To begin with, the Relativist takes

the scientific representation to be 'metaphysical' while the Realist takes it to be 'ontological'. I have argued in I.3 that this distinction must be drawn because of its far-reaching impact: the holistic character of the metaphysical representation, as opposed to the piecemeal and causal account of the ontological representation, leads to the conclusion that scientific theories are incommensurate because metaphysics is inscrutable: one cannot establish or explain a metaphysical outlook; one can only acquire one or 'share' one. The ways in which these new theoretical-metaphysical outlooks are transmitted are not carefully analyzable processes, which can lead to secure determination of the objects involved, but rather conversion, propaganda, perception shifts, revolution, rhetoric, and forgetfulness. This view of thinkers as separate metaphysical islands is rather extreme for it is based on views about privacy of meaning and determinacy of meaning, and is often based on a certain reading of the history of science (which if not outright biased at least is not uncontestible), however there is a good argument behind all these appeals to the inscrutability of metaphysics as we move from meaning to reference.

Representation, we claimed, is a referential function. The paradigmatic case of such a referential function of representation is to be found in maps or diagrams that represent a region. The Relativist and the Realist differ in their understanding of the referential function of representation. According to the Relativist the scientific representation must be seen as essentially different

from the map-representation because in the former case there is no prior agreement as to the categories of the mapped; science creates its categories, and therefore its mapping rules, as it proceeds. Scientific reference, for the Relativist has two fundamental characteristics (a) it is established holistically (tout court): instead of terms being connected to objects, a whole system is the proper referring unit, (b) it is established from within the system. In other words, we do not start with two elements a system and an independent world that we have to match but rather we have at hand a system whose boundaries are vague (no demarcation with observations) and we inquire as to what kinds of objects the system determines. In this respect the Relativists go beyond the Positivists for they have no independent observational facts to connect theories to since observations for them are theory-laden.

The results of these two characterizations of scientific representation are obvious: 1. In the first place meaning becomes the sole determinant of reference (and consequently indeterminacy of the former results in indeterminacy of the latter). 2. Theories are incommensurate because there cannot be two theories about the same things, as there can be two maps of the same region. To put the matter more dramatically, since scientific representations determine both their objects and the mode of referring to these objects, one is always in the predicament of comparing one's map not to reality, but to other maps drawn on the basis of the first.

While the above positions appear as extreme, it is important to note its serious side which touches on the idea of human limitations. The problem at hand is the determine what science is about as a preliminary to scientific objectivity and communication. If one is not to rely on intuitive understanding and agreement about what-a-scientific-theory-is-about then one has no other road but to look into the theory itself and the axiomatic method as vehicles for establishing reference. Since the only way that a whole scientific theory can be shown to refer is by way of models of the theory, one can demand that for a theory to be referentially determinate means to have a unique model. It turns out however that in most cases science not only does not determine a unique model, but it cannot even determine a set of models sharing a unique form. If we abandon the formal approach for the intuitive understanding of what science is about, we jump from the frying pan into the fire, for there is absolutely no in principle way in which this private intuitive understanding can be shown to avoid indeterminacy.

The Realist on the other hand focuses on intended models, but he is careful to avoid the indeterminacy of 'intended' by the formal tool of standard models which are based on preassigned interpretations. While this move may appear simply as pushing the indeterminacy-in-principle one step back, it is important to note three advantages gained. In the first place categoricity of the system is achieved. Secondly, the preassigned interpretations of key predicates simply allow for a cumulative progress model of science

where further theory is built on previous reference. Thirdly, and in close connection with the points developed above, the Realists preassigned interpretation is normally limited to a strong notion of identity as a necessary yet not a priori relation. Identity is a crucial relationship in two respects: it allows us to deal with it formally, and to minimize thus the risks of indeterminacy. It also enters into actual scientific progress for new advances in science allow for different kinds of identifications: even inter-theoretically we establish some identities on the basis of more fundamental identities (consider as an example the phenotype-genotype distinction in biology where identities or differences in the former are established on the basis of identities in the latter). Above all however, identity is the key to reference. The Realist sees definitions and other scientific determinations as the key to the reference of scientific theories. In his view then reference is not a holistic determination but is rather a piecemeal affair where some terms, either because of previous accepted advances or because of other characteristics (like involvement in causal lines of research) bear the weight of reference of the whole system. This is the first characteristic of the Realistic Conception of Reference. The second characteristic is also diametrically opposed to both the Relativistic and the Positivistic: reference is seen not as a conceptual (meaning) relationship drawn from within the system but as a causal relationship that is established between the inquirer or the speaker, the community of inquirers or speakers and the world.

CHAPTER III

TWO THEORIES OF REFERENCE

3.1 Puzzles and Criteria for Theories of Reference

We have to examine in this chapter the Traditional Theory of Reference (from now on "TTR") and the Causal Theory of Reference (from now on "CTR"). We will argue specifically that the CTR effectively displaces the TTR both because the TTR itself runs into difficulties and because the CTR provides us with a better account of reference. The significance of this discussion of theories of reference should by now be obvious: we have established so far that (a) Relativism stands or falls with the TTR because the central argument of Relativism, although formally correct, rests on taking the reference of scientific terms as established in a holistic manner, by way of the meaning of these terms - which is system relative and (b) the main point of contention between Realists and Relativists is the problem of reference of scientific terms. Their disagreement over the objectivity of science is in the final analysis the problem of categoricity of science, that is to say, the problem of the inability of science as a system to determine its object, what it is about. The Relativists here, as well as the Positivists, follow

the traditional theory while the Realists opt for a causal account of reference.

I have claimed in the previous paragraph that the CTR provides a better account of reference than the TTR. To establish such a claim, it is obvious that criteria are needed. Here however we run into difficulties because different theorists consider different criteria as crucial for the evaluation of theories of reference. The source of the difficulties is traceable to the fact that there is no agreement as to where the problem of reference properly belongs. For some theorists reference is a problem of psychology [Quine 1974], in which case different theories of reference are to be judged in terms of how well they account for the facts of reference. But what are the facts of reference that theories are supposed to match? There is no agreement here, because the so-called facts about reference turn out to be vague intuitions or conjectures about how the human organism develops its referential apparatus. It is equally doubtful that further research will reveal many more facts about reference, because reference occupies too central a position in philosophy; the search for facts will normally be based on some theory (such as naturalism) and this certainly prejudices the outcome of the search and renders our theories viciously circular.

Similar problems surround us when we take the problem of reference as a problem of linguistics. In both cases, the disciplines are in foundational crisis and much of this crisis is related to reference. The source of the trouble is the fact that reference

is neither a relation between words, nor a relation between objects (except in the trivial sense in which words are objects too), but a relation between symbols and objects. The centrality of symbolism to all thinking makes the problem almost unapproachable. Descriptive traditional linguists (of the Bloomfield school) approach the problem armed with naturalistic-psychological presuppositions, while modern linguists, who focus on the formal aspect of linguistic theory (generative and transformational grammarians like Chomsky) normally produce accounts of reference and meaning which turn out to be restatements of rules of grammar and syntax.¹

The preceding considerations might lead one to take reference as an essentially philosophical concept which can best be dealt within the context of philosophy of language and philosophy of logic. This is more or less the traditional approach, which takes the puzzles of reference as the basic criterion of judging theories of reference. Indeed, one has to take into account these puzzles because many of the theories of reference were developed solely as responses to them. Yet, as I have argued earlier in 2.1, the puzzles of reference form only the tip of an iceberg that runs deep into the philosophy of science. Taken in isolation, the puzzles of reference are not only non-paradoxical, they can even be said to have an air of silliness, which is another way of saying that they do not seem to promise much by way of developing a theory. How seriously ought one to take these puzzles in evaluating theories of reference?

The thinkers who would take the puzzles as crucial might argue along the following lines. To begin with, given what we said above, that reference is such a crucial (primitive) semantic notion one should not expect to find important 'facts' about reference, so as to construct his theory with a view towards explaining these facts; in fact the best we can expect in that score are a few paradoxes. It follows then that one ought to take reference as belonging to the same kind of concept as truth, and consequently, as we argued in 1.2, treat it in much the same way as other Ur-concepts like time, space, infinity, meaning. Of such concepts one cannot offer a theory that will be an explanation, that is to say, a proper reduction, but rather an explication. The success of such 'explications' is based on 'covering' the paradoxes or puzzles generated by the concept and showing a way for avoiding them. The best we can hope for by way of an explication for a semantic concept is to offer an elaborate system that will allow us to make the problematic concept disappear upon analysis leaving behind the characteristics that make it significant for discourse in the first place. Thus 'truth', or rather 'true', can be made to disappear by analysis leaving behind the recursion, the connectives by which the recursion was made possible, and disquotation. With a little thought now we recognize the source of error that led us to the liar antinomy. We ought to expect then a similar account for reference: in this respect the solution to the puzzles of reference is as crucial to the theories of reference as the antimony of the liar is to the

theories of truth. We can expect therefore that philosophical semantics can provide a theory which will cover all the relevant 'facts' about reference, that is to say, the puzzles, without necessarily uncovering any other facts about referring. At this point we can place a weak demand that the theory to be developed not run contrary to psychology. Given that language is acquired, it should be possible to establish connections between the formal theory of reference and the causal account of language acquisition. Nevertheless this is always - and only - a weak demand, because if reference is, naturalistically speaking, inscrutable, no amount of evidence will allow us to support any theory about how reference is 'actually' established during language acquisition. It follows that one ought to keep one's causal facts separate from his formal facts, the puzzles, in the same way that the theories of number are under no obligation to match psychological theories about how we learn to count.²

Let us pause for a second here to note that if one accepts these methodological guidelines, the connection between reference and objectivity is lost for good. The weak demand can be fulfilled, and has been fulfilled, by Russell's Theory of Descriptions, and specifically by the connections between the TTR and the epistemological distinction between knowledge by acquaintance and knowledge by description. This empiricist distinction however, functions differently in different epistemological theories, while the theory of reference remains intact. Even more importantly, when the epistemological theory involved came under doubt there was no

corresponding rejection of the theory of reference, a fact which shows the connection to be that of weak affinity, and not a true logical connection.³

Returning to the traditional approach, if the nature of the concept of reference is as described two paragraphs above, then what is required is an analysis of naming and denoting that will allow us to eliminate the problematic concepts by analysis, and in this way solve the paradoxes and provide a foundation for the development of referential semantics, in much the same way that we obtained a theory of truth from Tarski's definition of truth, by recursion. Seen in this light, Russell's Theory of Description is a good candidate for the bill. It identifies a set of puzzles concerning non-existence, and responds also to the puzzles that were proposed by Frege, concerning identity. It then provides an account of singular statements employing descriptions and names, which permit us to eliminate such denoting expressions by analysis. The theory clearly avoids the puzzles of reference, it can be generalized,⁴ and even has the weak connections with empiricist theory of learning, which we already mentioned.

Upon a closer examination however the methodological requirements outlined in the last three paragraphs do not hold. In the first place reference is not only 'like' truth but is related to it. Both under Tarski's account and according to what I have been proposing in 1.2, truth is to be explained in terms of reference (either by 'satisfaction by a sequence' in Tarski's account or by

a theory of categories in 'eidetic reference'). In order to treat reference in the same way as truth it would be required of the theorist to either provide us with a more primitive semantic concept, and none is forthcoming, or to discuss reference in terms of truth, which is certainly circular. A recursive account of truth was possible both because of the satisfaction relation and because of clearly established, extensionally-understood, syntactical, truth-functional structures. By appeal to these structures we could reduce the truth of complex sentences to the truth of the component simpler sentences, and ultimately the truth of atomic sentences. At this level 'satisfaction' took the place of truth. A recursive account of 'satisfaction' then would eliminate the predicate 'true' and allow us to obtain a general definition of truth as satisfaction by all sequences. In the case of reference however both avenues are closed: there is no more primitive semantic relation and the reference of terms does not have the clearly defined syntactical structure on which to base our recursion. Finally, not all occurrences of a term are referential and to decide which ones are, one has to utilize truth (intersubstitutivity salva veritate).

The second objection to the puzzle-problematic is that while the paradoxes of truth (as well as of other such problematic notions) are clear-cut antinomies involving contradictions, the puzzles of reference are 'oddities', that is to say 'mere' puzzles. Consider, for instance, the Frege puzzle of identity: it has to do with knowledge, not an outright contradiction. Also Russell's 'George wanted

to know if Scott was the author of Waverly', which leads to 'George wanted to know whether Scott was Scott', certainly involves intentional contexts. In general the Russell 'paradoxes' are based on certain presuppositions about the meaning of sentences (namely that unless the grammatical subject designates an object, the sentence is meaningless).

Finally in the case of truth, solution to the paradoxes is not the sole criterion, but only a telling symptom. The more important criterion is that a clear account of semantics is needed that can be checked against the logic of implication. By spelling out clearly the semantics of truth, Tarski was able to elucidate the notion of consequence in such a way that his semantic theory could match the already developed axiomatic, syntactically-based theories. But as we turn our attention to reference, we recognize that, besides the puzzles, there does not seem to be any established independent theory for our theories of reference to agree with. Similarly, facts about reference are hard to find, establish, interpret or evaluate. There are 'intuitions' concerning reference but given the problematic nature of the concept the value of such intuitions is questionable and choice between rival intuitions (if they can be shown to be rival) is itself a problematic task.

If the above claims are correct then one is led to the following dilemma: either a theory of reference is needed simply for the solution of the puzzles or no theory of reference is needed (since there are no facts, theories or fixed intuitions for the theory

to match). Both suggestions have been made, the latter, which is more controversial, by Davidson.⁵ The former suggestion, which is the one that we are arguing against, is the most traditional and is clearly held by Linsky. In defense of the puzzle-problematique, the traditionalist may claim that the puzzles are significant philosophically as steps in the development of an accurate semantic theory based on referential notions. Specifically, they may use the puzzles to show that our conceptions of identity and substitution are not clear. Frege's paradox, and especially Russell's, show that some laws of predicate logic and specifically the laws of Existential Generalization and Universal Instantiation do not hold indiscriminately. To take one example, the valid law $(x) (Fxv-Fx)$ does not support all instantiations, for if we take 'a' to designate 'the present King of France' we have to conclude by UI that he is either bald or not bald, neither of which is the case. If then we adopt Russell's Theory a way out of the paradox is shown, and thus the Theory of Descriptions becomes a proper part of logic (the fourteenth chapter of the Principia).

There is some truth to this claim, but it is neither accurate, nor helpful on the whole. If the sole aim of a Theory of Reference is to solve the puzzles, then Russell's Theory is more than sufficient. What reason would we have for developing other theories? On the other hand, the 'puzzles' of reference do not really constitute a confrontation to identity, or predicate logic itself but to its application to ordinary language and discourse. The puzzles of

reference would never lead anyone to seriously question EG or UI. Instead, what is problematic is the application of EG or UI or identity to all contexts or all terms that appear syntactically to belong to the category of individual terms (proper names and descriptions). Syntactically, there is no distinction between 'Pegasus' and 'Plato' or between 'the present King of Norway' and 'the present King of France'. The aim then would be to regiment ordinary language in such a way that the puzzles are avoided and this can be done in either one of two ways: either to find a way in which the appropriate distinctions can be established, or to argue that such distinctions do not exist. One can then proceed to show how in fact all proper names and all definite descriptions have a complex structure that does not allow for simple applications of EG or UI or identity laws. Russell opted for the first way, Quine for the second. In either case however, it should be noted, we are not dealing with a completed theory, but the beginnings of a theory. It is important to ask at this point whether such a theory of semantics is possible and also what is required for its completion. It is clear that syntax is not enough: what distinguishes semantically the expression 'the present King of France' from the expression 'the present King of Norway' is a matter of our knowledge of the world. Furthermore this knowledge of the world cannot be taken as constant since for some people the present King of Norway is the object of acquaintance, while for others the object of a description. If one wishes to avoid the conclusion that there cannot be a general semantical

theory, one has to side with Quine's theory which treats all names and descriptions as similar and eliminates them in favor of predicates by using Russell's theory of descriptions.

Similar considerations apply to other terms which are syntactically similar. Consider for example mass terms like 'milk' and referentially individuated terms like 'apple' or terms that are indeed adverbs like 'happiness' or 'mind'. Quine certainly made a valiant effort in Word and Object to achieve a unified system of regimentation, leaving to pronouns (the linguistic counterparts of variables) the task of carrying out reference. The significance of this move cannot be over-emphasized: reference becomes uniform, 'indeterminate', and can be formally handled by Tarskian satisfaction. As for our claim that terms refer to different kinds of things, Quine would insist that all such distinctions do not, or should not reflect distinctions in reference but only distinctions in meaning, in the relation between predicates. To be sure the process is artificial because even though there is, upon first examination, no syntactic distinction between 'happiness', 'apple', 'cattle', 'milk', 'humanity', when we consider these terms as parts of other expressions we recognize distinctions. Some of these expressions are acceptable but others are unacceptable. Such distinctions are clearly syntactic, as Quine himself would admit, because they can be drawn by appeal to the referential apparatus of individuation ('some', 'all', 'every', plural, 'a part of', 'many', 'one', 'is the same

as', 'is different than', etc.). One might argue here that such distinctions clearly illustrate that reference cannot be taken as uniform, since even if one does not know the meaning of a term, (and barring - some how - metaphorical usage), one can still get a good sense of what kind of object the supposed referent is. At this point, however, Quine's Thesis of Inscrutability of the Referential Apparatus comes into play. Quine would argue that the syntactic distinctions outlined above cannot be used as a basis for a theory of categories of things (for different kinds of terms to refer to). In his Philosophy of Logic Quine argues the point in a round-about way by considering the notion of 'grammaticality' which he finds obscure.⁶

The above considerations bring up the point mentioned earlier, that the positivist-empiricist approach blurs the distinction between meaning and reference. It requires that any distinction in kind or category be expressible as a relation of predicates (what Carnap would call, correctly, meaning postulates). Quine of course is not an outright positivist empiricist and as we well know rejects meanings; on the negative side he would claim that reference is every bit as indeterminate and problematic as meaning; on the positive side however, he implicitly draws a distinction within reference between 'truth' and 'reference'. Truth and its attendant categories are somehow exempt from radical indeterminacy. This is done in three steps. First, the Tarskian Theory of Truth is taken as canonical: it does spell out 'truth'. Secondly, from an epistemological

standpoint, observation sentences are privileged as "the cornerstone of semantics." Finally, concerning objects and categories, the only ontological structure required by Tarski's Theory is that of a sequence of entities. As a matter of fact we do not even need to accept many sequences, since we can analyze all sequences by appeal to the ordered pair of objects $\langle a, b \rangle$ which is properly spelled out as $\{\{a\} \{a, b\}\}$. Given that all sequences and n-tuples can be ultimately analyzed as ordered pairs [by the well known method outlined in Word and Object, pp. 257-262] the only categories needed are object and ordered pair, or, even simpler, ordered pair.

This approach, which assigns referential import to the most indefinite elements of our discourse (pronouns, variables), indeed forces us to think of reference only by way of meaning. An x by itself or a y by itself cannot be said to refer to anything; it is by virtue of the predicate that we can even ask the existence question. The slogan 'to be is to be the value of a variable' is a variation on the old Platonic maxim 'to be is to be something' (i.e., to have something predicated of x). This principle, to which very little attention is normally paid, is even explicitly mentioned by Quine as the Principle of Primacy of Predicates in his Methods of Logic. It is offered as a kind of justification for eliminating proper names.

There are sound empiricist reasons behind this approach which takes reference as in-principle indeterminate. In the first place, one would be hard pressed to show any distinction between meaning and

reference: what is it that distinguishes our understanding of 'blue' or 'biped' from our understanding their categories (their mode of individuation)? Since both have to be learned in context and one cannot be learned without the other, the quest for a distinction may very well be futile. Secondly, and more importantly, the view of reference as indefinite agrees with the empiricist principle of fallibilism: we can tolerate indeterminacy of meaning and error in theory, but reference if it was determined would have to be a relation between word and object, extralinguistic object, that is. The empiricist then would have to give up his liaison with scepticism and claim that once reference has been established, at least in some cases, it is established for good without possibilities of revision.

There are three counterarguments to the above account of reference. In the first place, it is not clear that Humean type of scepticism is to be applied to matters of reference. Kripke has argued that with at least a few terms (rigid designators) reference can be fixed by appeal to necessity. These terms are not limited to proper names (as in Russell's logically proper names) but extend to natural kinds and scientific identifications. To do so, one does not have to abandon Humean or human modesty (and claim infallibility) but only to draw a distinction between epistemological categories like a prioricity and certainity and metaphysical categories like necessity.

The second reason for rejecting such an empiricist approach has to do with scientific reference (and by extension linguistic reference). It is not accurate to say that meaning and reference to kinds and categories are indistinguishable. They are indistinguishable, only if one takes a strict empiricist line of stimulus meanings. Ordinarily however, people are able by appeal to syntax to understand things about the referents of discourse even though they are not aware of the meaning of the terms in question. The syntactic structure is often a guide to understanding the kind of thing referred to. Possibilities of error do exist and ordinary language certainly gives ground for many category mistakes. Nevertheless a good case can be made for the old Kantian position that sees elements of syntax as determining categories. But even if one abandons this route and accepts that the differences between referentially-significant structures and meaning-significant structures is only one of generality (and thus a matter of degrees), still one cannot ignore what I called the 'eidetic reference' in science [1.2]. Especially with the mathematization of science, reference to different kinds is assured and determinate: introducing, for instance the gravitational force between two masses as $K \cdot \frac{m_1 m_2}{r^2}$ clearly differentiates the force from the masses or from distance and uniquely determines the kinds of magnitude units that can be used to detect it in reality. This differentiation and interdependence of kinds is so pervasive in science that often new magnitudes and

new kinds are discovered and defined on the blackboard before any actual empirical investigation into the kind is launched.

Finally even if one is to accept the general strategy of Quine that opts for a general account of reference which regiments ordinary language for the purposes of science, still one may question the choice of logic that this referential regimentation is to take as canonical. Quine and many of the thinkers that follow the strict puzzle-problematic take normal predicate calculus of the first order logic as the logic that any theory of reference has to argue with. Yet, as I have argued, the puzzles of reference are not a challenge to predicate calculus but to the application of this calculus. Furthermore, it is possible to require of a theory of reference to be sensitive to modal distinctions. Why chose ordinary, extensional, first-order logic over modal logic as a standard for developing theories of reference? What are we to do if an account of reference does not preserve modal distinctions? We have two alternatives: we can revise the theory of reference or we can challenge modalities. Quine does the latter, the CTR theorists do the former.

It is clear, from what has been developed so far that the task of comparing and choosing different theories of reference is not by any means easy. Different theories were developed in response to different problematicues. They sought to achieve different goals and to preserve different types of commitments (epistemological, ontological, and logical methodological commitments).

Earlier I also sought to dissociate my inquiry from the simple puzzle problematic. This task has to be done because by now many reference theorists have accepted the puzzle problematic as the key to reference. Much of this is due to certain fundamental metaphilosophical errors in certain influential schools of modern philosophy of language. The discussion of these errors falls outside the scope of my present inquiry, yet, given the centrality of reference in all philosophical matters, the significance of such errors cannot be underestimated. Much is due to Linsky who in his two books Referring and Names and Descriptions assumes the role of the historian of the problem of reference. It is not only that he takes the puzzles of reference as the absolute criterion of theories of reference, but, since he hardly investigates the significance of the paradoxes, he often pronounces judgments of interpretation which, more often than not, miss the forest on account of a tree. He argues for example, that Kripke in Naming and Necessity attributes to Frege and Russell tenets that they never made explicitly or even tenets for which they provided exceptions. As historical claims, these critiques of Kripke's lack of scholarship are accurate but in a near-sighted sense. Frege never stated that extension is determined by intension, Russell did draw a distinction between proper names which were descriptions-in-disguise and logically proper names. One might even claim that Russell's logically proper names are close to Kripke's idea of names as rigid designators. Yet all these claims will have missed Kripke's

perceptive problematique of reference which leads to a more accurate assessment of the TTR. I have shown in what way Frege's theory is committed to the claim that reference is determined via sense. Concerning Russell, we will show in what ways he was aware of the limitations of his position, yet the distinction between logically proper names and other proper names is certainly drawn in different ways than the rigid-non rigid designator distinction of Kripke. The former is drawn on the basis of whether the existence question can be significantly asked, while the latter is drawn on modal grounds. The significance of this difference will be shown in the discussion that follows.

My main objection to the puzzle problematique is that it took the problem of reference as a limited problem in the philosophy of language, and moreover treated the latter as an autonomous philosophical field. A more careful examination will show that there is no autonomous field of philosophy of language and, most important for our theory, that the problem of reference is linked to the problem of objectivity of science in such ways that the former should not be discussed without taking the former into account.

It follows from this stand, that our criteria of judging theories of reference will involve the puzzles of reference, but, as in our discussion of Frege's puzzles, we will concentrate on the spirit of the puzzles (the significance of the puzzles) rather than on the letter. We will also take modal contexts seriously, but not

without argument (which will hopefully link modality to philosophy of science).

3.2 The TTR and Its Troubles

In the previous section we discussed the general problematic framework of criteria by which the various theories of reference are to be judged. This task was necessary, if our inquiry is not to turn circular. It would turn circular without such independent criteria, because we would in effect be claiming that the TTR is to be rejected because it supports relativism and defies realism, while at the same time we would be citing the failure of the TTR as an argument against relativism and in favor of realism.

Concerning these criteria however, as we explained, one cannot expect that they be precisely drawn, because of the centrality of the problem of reference, the absence of clear intuitions, and the already existing philosophical discussion. Without a semantic theory at hand, we have to proceed using the puzzles, the intuitions, ordinary language, yet above all, we should not lose the underlying significance of all these elements for the problem of the objectivity of human knowledge in general and science in particular. The puzzles do form a clear starting point, so they will be brought in the discussion, yet without an understanding of their significance one is left at a loss, especially with respect to evaluating different solutions to the puzzles and appeal to intuitions simpliciter will

not do: how are we to judge whether the solution of Russell agrees with our intuitions about the reference of definite descriptions? At the same time abandoning the descriptive common sense intuitions for the sake of coherence with the logic of science is equally problematic because there are many logical systems in the first place, and, in the second place, there is a lot of disagreement about which of these systems is appropriate (either correct or adequate) for doing science. If one follows strict extensionalism in logic, or Humean empiricism in philosophy of science, one is bound to eliminate all talk of modality from formalized science. He will have to reduce our 'modal' intuitions to a meta-level, claiming all modality to be a de dicto modality. Following this strategy he will not require of his theory of reference to be sensitive to modal distinctions. The opposite would happen if he considered modality (specifically, necessity) as an irreducible element of scientific thought (to be found in nomic universality, for example).

Given all these difficulties, one way of proceeding would be to state one's prejudices at the start and then proceed to develop the theories either immediately or after he has adequately defended his prejudices. In this case, we would require that one states his prejudices with respect to three subjects of dispute: (a) intuitions about referring; for example, an adherence to naturalism, or semanticalism; (b) preferences with respect to logic as, for example, the acceptability of higher order logics or many-value

logics, intensional vs. extensional logics, etc.; (c) preferences with respect to analysis of science such as positivistic-empiricist analysis vs. realist analysis vs. essentialistic analysis.

A second way of proceeding is to focus on the central problem and proceed carefully, exposing one's commitments (prejudices) as one goes along, after argument has failed to establish them. I will follow the second route, because of the nature of my inquiry: I am in the middle of investigations, and I am not sure yet what commitments I have to make in discussing reference. On the other hand, in the two preceding chapters I have made some of my tentative commitments clear.

We shall begin then by investigating the puzzles associated with the TTR, but in all cases we shall try to uncover the significance of the puzzle, before offering a solution, and in light of that significance we shall evaluate the proposed solutions. Following this tactic, then we recognize at the start that a large part of our work has already been done. We have explained generally the significance of the problem of reference (from Kant onward) and specifically in 2.1 we have analyzed one of the paradoxes, proposed by Frege in Über Sinn und Bedeutung, by placing it in the wider context of Frege's Philosophy of Science and Mathematics. We will start with a summary of these results and proceed to the generalized TTR by Russell.

Frege's well known puzzle about the identity of the Morning Star and the Evening Star was based on the problem of understanding identity. Given that identity is a relation that an object can have only within itself, how can an identity statement be informative? It either has to be true and uninformative (as in ' $a = a$ ') or false and informative. Yet there is no doubt that identity statements are informative because they are discovered by research and thought, not by analysis. The significance of the puzzle is multiple: consider for example an attempt to eliminate it by claiming that identity is a 'degenerate' case of relation, sitting at one end of a spectrum of relations and having all the characteristics of equivalence relations plus the additional characteristic that it remains an equivalence relation no matter in what context it figures, simply because of its degenerate nature (relation of an object to itself). This view would lead to understanding identity as thorough-going substitutivity based on equivalence plus the 'degenerate' nature of identity, for, clearly, what can be lost by substituting an object with itself: it is hardly even a substitution. The truth is however, that identity does not allow for a thorough-going substitution: the statement "the Babylonians discovered that the Morning Star was the Evening Star" is clearly different from the statement "The Babylonians discovered that the Morning Star was the Morning Star."

It is essential then to develop a theory of identity that would be sophisticated enough to explain and regulate substitutivity.

That much of the significance of Frege's puzzle was always recognized. It is obvious that Frege's contribution to logic, especially predicate calculus, required a proper understanding of identity, for it is virtually impossible to do predicate calculus without some form of substitution. What was not so easily recognized was the significance of the puzzle for the philosophy of science and this can be brought up only by considering the whole of Fregean Corpus and in particular his disagreements with the formalists Hilbert and Korselt concerning the subject matter of mathematics and the function of definitions and axioms. To understand his general stand one should keep in mind that for Frege (and for a lot of other philosophers) identity is the key to objecthood. In mathematics identity looms large in all the operations that involve equality. Some form of explanation for its informativeness had to be found. Hand in hand with the use of identity is the form of the definite description, since, by virtue of the mathematical equality, we can speak of definite objects that are the result of operations (functions, in Frege's terms) as in 'the product of x and y ', 'the square root of z ', etc. But even more than understanding equality, Frege took identity as important for two reasons: (a) identity was involved in all definitions, and (b) identity was important in determining what a theory was about. These two reasons were obviously interrelated, since Frege assigned to definitions the task of establishing the reference of an axiomatic system.

His well known solution to the puzzle was to distinguish the sense and the reference of a term and to show that while the truth of an identity statement rested on the identity of reference of the terms, the informativeness was based on the difference of senses of the terms. That much is well-known and appreciated. What is however missed is the aim of an identity statement for Frege. It is not to establish identity of two senses, or between two senses. If it had been so, then identity statements would belong to lexicography not to science! After all, for Frege, the two senses are (a) objective and (b) not identical. Instead the whole aim of identity statements is to establish reference. It is at this point that Frege becomes committed to the theory that reference is established by way of sense, that is to say, extension is determined through intension. Given that he also believed in the objectivity of sense, the task of identity statements was not just to facilitate communication between two people with different ideas but to abstractly establish reference in an objective way. Definitions as a result had to be real, and serve as the key to the objectivity of scientific systems. Without clear, real definitions (identity statements) science would be without object (about nothing). Frege took all science to be ontological (i.e., offering an account of the world). Even logic, according to Frege, had to have a subject matter.

These logicist-realist concerns of Frege were clearly shared by Russell as I argued in 2.1. After abandoning his early Hegelian and Meinongian beliefs, Russell argued for a system of logic and

mathematics that would be about reality, while at the same time realizing the formal and indeterminate nature of mathematics. His constant attempts to define 'number' properly must be seen in this light. Concerning the puzzles of reference, Russell managed to follow on Frege's footsteps by both generalizing the problem and the solution.

The first way in which Russell can be seen as developing the work of Frege is the following. According to Frege, reference is determined through (objective) sense: if a term lacks sense then one cannot establish its reference. This however would place our most clearly referential terms (proper names) in the funny position of lacking reference by virtue of lacking sense. It had been a long tradition, from Aristotle through the Scholastics and onward to J.S. Mill, to maintain that proper names have denotation but lack connotation (sense). This seems obviously correct, since proper names are not translated. Definite descriptions on the other hand, do have sense because they are general functions of the form 'the father of ____', 'the author of ____'. 'the tallest mountain in ____', etc. If we assume that they are unquestionably referential, then there is no end to the objects that can be created as referents for them: 'the present King of France', 'the present King of USA', 'the father of the present King of USA', etc. On the other hand, a good case can be made that the reference of a definite description can only be established by virtue of the proper name that becomes an argument for the descriptive function. Except

for descriptions in mathematics, where unique reference can be established by the 'the so-and-so' construction employing general terms alone (example: the x such that $x^2 = x$ uniquely identifies the number 1),⁷ in all other cases no individual can be referred to except by appeal to another individual. These considerations should be kept in mind, in order to understand both Russell's Theory of Descriptions and its extension by Quine and finally Kripke's innovation (the return to J.S. Mill).

The second way in which Russell extended the work of Frege on reference was by generalizing Frege's problem. Frege's puzzle stemmed from the problem of understanding identity. Russell generalizes the problem by taking it one step back: instead of asking "what is an identity statement about?" he asks "what is any statement about?" which is equivalent to asking about how the reference of a sentence is established. If we answer, uncritically, that a sentence is about the object denoted by the grammatical subject of the sentence, then we run into a two-fold difficulty. In the first place, there are many statements about imaginary, non-existing objects. It would follow that the statement is about something that does not exist. At this point, we have two ways of understanding this: either to say that the statement is about nothing or to say that it is about something that 'exists' in some other manner ("subsists", perhaps). Either way is unacceptable: the first does not allow for distinctions among imaginary entities, the second creates one too many realities for the ontologically parsimonious.

The second difficulty has to do with non-existence sentences: how can a sentence like "Pegasus does not exist" be even significant, let alone true, when it fails to be about anything? If the statement is true then it is meaningless because there is no subject for it to be about; if it is meaningful (i.e., it is about something) then it is false, because it attributes non-existence to an existent. The puzzle of non-existence statements and of statements about non-existents lies behind almost all of Russell's puzzles.⁸

One may object at this point to the whole puzzle by claiming that it is based on a simple-minded view of the reference of sentences. It presupposes that the statements are about the referent of the grammatical subject and that unless there is a referent for the grammatical subject the sentence is meaningless. Without making excuses for Russell, one can see however the plausibility of his approach. If we are to discuss what sentences are about, we might start from the simplest cases of singular sentences (i.e., statements involving proper names as subjects). Clearly the statement "Nixon resigned the Presidency" is about Nixon and attributes a property to him, which happens to be true of him. Why not treat "Santa Claus does not exist" in a similar way? Russell held onto the principle that the sentence is about the referent of the grammatical subject. The problem for him then was to show how can non-existence statements be both true and meaningful, in view of this principle.

His solution represents a generalization of Frege's solution in three senses: (i) it can deal with both non-existence and identity

statements, (ii) it fulfills the Fregean requirement that a sense be found for proper names and descriptions so that reference for such terms be established, (iii) it is a solution to the general problem "what are sentences about?" One should keep these three items in mind for a full appreciation of the significance of Russell's solution.

Russell's solution is developed with a view to accomodating definite descriptions and is then extended to include proper names. The reasons for this will become apparent as we proceed, but, for the time, let us note that all of Russell's puzzles utilized definite descriptions. More importantly, however, the description has a grammatical form that allows it to "create" objects (so to say): "the so-and-so." If we are to find a general solution to the puzzle of non-existence, we better concentrate on a denoting expression that exhibits form. Alternatively one could avoid the puzzles involving names by considering not the objects intended as referents, but the context in which these names were brought up. In this manner "Hamlet does not exist" could be analyzed as "the hero of Shakespear's Hamlet is merely a fictional character." Clearly no general theory of referring can be derived from this approach.

Above all however, one should consider the fact that proper names do not figure in science.⁹ Descriptions on the other hand are basic vehicles of reference and cross-reference for all hard sciences, and mathematics in particular. This is obvious in both the Principia, where two chapters are devoted to descriptions, and

in the Introduction to Mathematical Philosophy. Russell needs the theory of descriptions to make sense of science. The fact that the theorems of Chapter 14 of the Principia are all devoted to descriptions bears witness to the Russelian idea that descriptions carry a logically significant structure that is of importance to science. Consider for instance statements about "the square root of 2", or "the highest prime"; one should have a way of analyzing their reference, if one is not to take them as names.

The first step in Russell's solution is to claim a "syncategorematic" quality for descriptions. Despite their appearance as denoting terms or even as functions, descriptions of the form "the so-and-so" do not have either denotation or meaning by themselves. They can have significance only as parts of other sentences. To understand the reasons for this move, consider the alternatives. If one takes them as names, then they have to have denotation; if one takes them as functions, i.e., predicates, then they have to be true of an object.¹⁰ In either case we land back on the problem of non-existence. This interpretation of descriptions as context dependent shall become even clearer later.

The second step then is to analyze descriptions as parts of larger statements. Using Russell's notation we can write ' $(\lambda x)\phi x$ ' for the descriptions and claim that in general our analysis will be of statements of the form ' $\Psi(\lambda x)\phi x$ ' or even more vaguely "... $(\lambda x)\phi x$..." Russell's suggestion is that whenever we are

confronted with an expression of the form "...(ιx) ϕx ..." we ought to analyze the ' $(\iota x)\phi x$ ' in that context by a conjunction of two statements, one asserting existence and the other uniqueness:

$$(1) (\exists x)\phi \text{ and } \phi x \cdot \phi y \equiv_{x,y} x = y$$

This conjunction is equivalent to Russell's definitions of ' $(\iota x)\phi x$ ' in a context

$$(2) \dots (\exists b) (\phi x \equiv_x x = b) \dots$$

which in the case, say, of a context like

$$(3) \Psi(\iota x)\phi x$$

becomes

$$(4) (\exists b) (\phi x \equiv_x x = b : \Psi b)$$

where (3) and (4) are definitional equivalents.

It is clear now that the statement "the so-and-so does not exist" can now be completely analyzed since it provides a context for the description:

$$(5) -(\exists b) \therefore \phi x \equiv_x x = b$$

On the basis of this meaningful and true sentence we can discuss the other problematic cases, like the statement "the present King of France is bald", which is translated into (4) above. In that case the sentence is meaningful but false, since one part of the conjunction, namely (2), is false. (2) would also be false in case there were more than one ϕ as in "the U.S. senator is bald."

Let us pause for a moment to appreciate the formal ingeniousness of this solution, for it is one of the few cases where one formally has his cake and eats it too. One should compare the

informal (1) with the indefinite (2) and with (4) and (5) which are closed sentences. (1) is only informal, because one cannot replace every occurrence of a description with two statements in conjunction. Had this been possible, then every time one uttered 'the present King of France' he would be making two statements (that could be true or false). Yet surely, the expression 'the present King of France' is not a statement. The addition, however, of 'does not exist' turns it into a statement as in (5). How are we to understand this having of a cake and eating it too (i.e.: "K is not a sentence - K is a sentence")? After all, how would we say that "the present King of France exists" except by a negation of "the present King of France does not exist." Here Russell introduces another "funny" logical predicate the existence predicate 'E!' and the statement

$$(6) E!(\iota x)\phi x$$

becomes

$$(7) (\exists b) \therefore \phi b \equiv_x x = b$$

which is the contradictory of (5).

We will have the opportunity to explain why (6) does not commit Russell to the idea that existence is a predicate, but for the time, we should note that what Russell has achieved is to show that the expression 'the so-and-so' is not a denoting (categorematic) expression, but a piece of formal apparatus! Like all logical constants then, it has to be explained or defined implicitly by paraphrase-in-context in the same way that 'v' is explained in

terms of '&' and '-' by the paraphrase of 'pvq' as ' $\neg \bar{p}q$ '. In a similar way, existence becomes part of the logical apparatus, carried out by the existential quantifier. Even the 'E!' predicate is to be understood semantically as a metalogical statement 'has instances.' Its logical qualities are obvious, since 'E!' has specific theorems attached to it. One should understand it as similar to identity, which is a relation but also a logical relation.

The ingenuity of the logical solution however pales before the significance of the solution for semantics, ontology, epistemology, and metaphysics. Consider, first of all, that Russell's solution fulfills the Fregean requirement that reference be established by way of sense. The concept of sense was an embarrassment to Fregean semantics, because it was required of it that it be 'objective' without at the same time, having to do with reference. This double requirement would lead us necessarily to 'objective meanings in the mind' in much the way of the Brentano-Meinong (and later - Chisholm) intentionally inexistent objects. Russell showed however that the meaning of some singular terms does not have to be parasitic on reference (naming), which would lead us to claim that a general theory of semantics is impossible. Instead he provided an analysis of the meaning of some singular terms, and this analysis could be extended to cover the totality of singular terms.¹¹ This would allow us to extend the application of logic to cover singular inferences, at least as far as science was concerned. Given that science, in its theoretical part (not in its application),

is interested in the universal, and discriminates among the singular terms, the ones that have one foot on the universal (i.e., descriptions), we can understand the significance of Russell's theory for the analysis of science: we can discuss objects in science as long as we introduce them properly as descriptions.

At the same time an age old problem was solved. Terms designating individuals were always a sticky point for traditional logicians. According to Aristotle and the later followers of his logic, individuals were indefinable in the same way that ultimate categories (like 'being') were. Ultimate categories were indefinable because there is no higher genus to be used in their definition; in scholastic terms 'being' has extremely great (infinite, according to some) extension, but null intension. Individuals on the other hand, have a genus but lack a proper differentia because they have an infinity of properties, and so they cannot be defined (in scholastic terminology, they have null extension but infinite intension). In both cases, without a definition, neither kind of terms can become a part of science. As a result, they can only be dealt with by non-science: either direct acquaintance (for individuals), or metaphysics (for ultimate genera) which is dialectical and non-demonstrative ('no demonstration, no science'). Russell's solution manages to define both the individual's ' $(\exists x)\phi x$ ' and 'being' ('E!') by analyzing them as parts of the logical apparatus. This certainly extends the boundaries of demonstrable science and limits the area of speculative metaphysics.

This approach to metaphysics by way of logic (i.e., ontology) is found not only in Russell, but in Aristotle, Leibniz and Frege. Russell however is the first to offer a clear logical statement for 'individual' and 'being'. After Russell, other logicians attempted similar feats. Consider here P. Lorenzen's Methodical Thinking in which he argues that the terms 'object' and 'the world' are pseudo-denotative terms: the first is a pseudo-predicate and the second a pseudo-name. While the relations here are the reverse of Russell's ('the so-and-so' for individual objects, and 'exists' as a predicate), still the way of elimination is similar: by methodical reduction to logic, the cumbersome terms disappear upon analysis.¹²

Russell's theory applied primarily to descriptions, and for the purposes of theory, descriptions are all we need. For the purposes of epistemology however, and general semantics, we need to deal with names as well. Here Russell, in the spirit of empiricism, draws a distinction between logically proper names which are the objects of acquaintance and other proper names which are known by description. The latter are in fact descriptions-in-disguise (or "telescoped" descriptions), and are to be analyzed as such. In this way we can analyze away 'Pegasus' in 'Pegasus does not exist' in the manner of (5). At the same time however this move helps Frege since names now acquire a 'sense' that can be used in establishing their reference: proper names can be taken as synonymous with descriptions. The identity statement that is

constructed is the key to establishing reference, as in the example

(8) 'Aristotle = the teacher of Alexander the Great'

There is an element of indeterminacy here since there are other descriptions true of Aristotle like "the Stageirite", "the father of Nicomachus", "the father of science", etc. but this is due to the peculiar epistemological nature of description: some people came to learn about Aristotle in one way and others in other ways. The distinction, however, between logically proper names and names which are descriptions in disguise, can be established on clear logical grounds. If it makes sense to ask "Does c exist?", then 'c' is not a logically proper name, because, presumably, objects of acquaintance have to exist; only objects known by description can be meaningfully questioned with respect to existence. In other words, to say of a logically proper name c that 'c exists' is to utter a tautology, while 'c does not exist' amounts to a contradiction. Using this criterion we find that there are very few logically proper names. As Russell admits in the Lectures on Logical Atomism, the only logically proper names are the demonstratives 'this', 'that'. This is important to realize because it helps us understand Wittgenstein's Tractatus: 'this' and 'that' are the simples of the Tractatus, while every other object was to be understood as analyzable by Russell's theory of descriptions.

The trouble is, of course, that the so-called logically proper names are not names at all, because, as A. Noah [1973] has suggested 'this' and 'that' are logically proper names only in the context

of a specific utterance involving an ostension. Thus the logically proper names are, like paper cups, used once and thrown away. In this manner they do not fulfill the elementary requirement for names: that they allow for repeated reference and for cross-reference.

These considerations make clear the necessity for a uniform treatment of individual terms, and specifically the plausibility of Quine's approach that eliminates individual terms altogether, in favor of predicates, by utilizing Russell's theory. Quine constructs artificial predicates out of proper names (is-Pegasus', or the cacophonous, 'pegasizes') and using these predicates, he constructs the corresponding definite descriptions ('(1x)Pegasizes x' = 'the one and only x that is - Pegasus') which are analyzed by Russell's method. As a result a whole class of cumbersome terms are tamed and can become part of our formal system, should we wish to talk about them in our formal language. What is gained here is not only logical clarity, but ontological parsimony as well; we do not have to refer to Pegasus in order to say something about him (it?). Indeed the only vehicles of reference are now the variables (corresponding to pronouns), while existence is asserted by capturing the variables. "To be is to be the value of a variable," as Quine argues in "On What There Is" in the Methods of Logic.

F.P. Ramsey characterized Russell's Theory of Descriptions as "that paradigm of philosophy", and what I have written above certainly bears witness to the characterization. To make the characterization ever more plausible, consider the following three

aspects of Russell's discovery. In the first place it was methodologically important: it showed how in philosophy we can eliminate problems that have been vexing us for centuries, by logical analysis of language. Of course, one could liken Russell's achievement to Hume's analysis of causality, and a case can be made that the two kinds of analysis are similar. Still however Russell's appeal to logic and language certainly showed the power of analysis. One must also see in this discovery a philosophical innovation analogous to Kant's transcendental argument. By showing that the purported subject disappears upon analysis, Russell initiated our search after semantic concepts. We became aware that some problems are not due to either reality or language but to the relations between the two. This insight gave us one more dimension for philosophical analysis; problematic concepts may very well be semantic concepts in disguise. The Russellian treatment could thus be extended to other concepts like 'truth' or 'cause'.

Finally to some people Russell's theory, and especially Quine's extension of it, seemed like an endorsement of a negative theory of substance. The best example here is Quinton's On the Nature of Things. By eliminating individual terms in favor of predicates Quine, in Quinton's view, resolves the old conflict, by showing the empiricist theory of negative substance to be perfectly plausible. This interpretation of Quine is for the most part erroneous. Quine's elimination of singular terms is not done in the context of doing traditional metaphysics. In fact, his Ontological Relativity Thesis

shows the fruitlessness of doing traditional metaphysics. Furthermore the so called individuating-substratum is not lost by the Quinian move, since it is transferred now to the predicate ('is-Pegasus'), while the referential vehicle (the pronoun, or variable) still carry a lot of the "heaccaity", the "thisness" of the traditional primary substances. Yet despite these glaring errors in the interpretation of Quine, still Quinton has a point which can be made more accurately in the following manner. Quine's elimination of singular terms does not allow us to decide whether things are just bundles of qualities or more than just bundles of qualities. What is achieved instead is the transference of all referential import to variables which are indeterminate and can only be made sense of by predication. Science thus is conceived as a system of predicate relations that has reference, but only indeterminately. This certainly agrees with one aspect of empiricism, since it provides for radical changes of the scientific system which would have been inexplicable had reference been determinate. To see the significance of this point, consider two other related ideas. In the first place Quine's claim that there is no distinction between science and philosophy and no place for a first philosophy is clearly in agreement with the previous claims. There can be no first philosophy (separate from science) because there is no separate subject matter for it to be about. That is to say, there are no privileged predicates (that have reference) which we can call 'substance' so that we have to develop a special theory for them (i.e., a theory of

substance); indeed science and philosophy are about the world and there is not sharp distinction between the two, as there is no part of human knowledge that is immune to revision. The second point would be to contrast Quine's view with Strawson's, who draws a sharp separation between metaphysics and science and argues that the former not only has a separate subject matter but has elements that are immune to revision since they are the cornerstone of all intelligibility and reference. It is not surprising that they turn out to be the Aristotelian individuals and the kinds under which they are subsumed. One should also mention here, that Strawson's work is based on a critique of Russell's Theory of Descriptions.¹³

Given the status of the Russell-Frege Theory of Reference, one could conclude that the presupposing of this theory by the Relativists should be the least controversial part of their position. We could even make a stronger case for them, by claiming an intuitive clarity for their position on the matter. Given that theoretical terms are not by definition objects of acquaintance, their reference (or their extension) has to be determined by way of their meaning (their intension). According to the TTR, this is true of all terms including singular terms and natural kind terms. It must be true a fortiori of the theoretical terms of science. There is no "electron" or "magnetic field" or "positron" or "virus" or "elasticity" apart from some theory that introduced these terms, and, put in a referential mode, if one is wondering whether x is an electron or

not, one simply has to ask whether x fulfills the criteria of electronhood that are outlined by the theory.¹⁴

This most-uncontroversial part of Relativism, however turns out to be highly questionable. Doubts arise not only about the claims of the Relativists concerning theoretical terms, but also about the whole picture of reference painted by Russell and his followers. It is argued specifically that the TTR presents both a simplistic and an inaccurate account of what is involved in referring. The task of the rest of this part is to show the ways in which the TTR is criticized. In the final section we will discuss its replacement by the CTR.

The criticism of the TTR falls into two categories: A. Criticism of the TTR from the inside, and B. Criticism of the TTR from the outside. The former concentrates on anomalies of the theory and seeks to amend them without questioning the fundamental assumptions of the theory, specifically the Fregean doctrine that reference is determined by way of meaning. The latter kind requires that the range of application of the theory of reference be extended to new contexts (like the modal context) and is primarily interested in questioning the fundamental assumptions of the TTR. We shall examine both criticisms in the above order.

The internal criticisms of the TTR focus on the relation between names and descriptions. In what way can names be considered synonymous with descriptions? Can the 'sense' of a name be a description? Are there any asymmetries between the two kinds of terms? Frege,

Russell, and common sense required that names have some sense, because otherwise one would not be able to explain how people managed to refer to objects. Even ostension would be unsuccessful without certain presuppositions concerning the boundaries and the nature of the ostended object, as well as the activity of ostension. Notice here, the connections with the Relativistic arguments about perception and objectivity: not even ostension can establish the identity of the referent, because two disagreeing astronomers while pointing to some object (from our standpoint) are still in disagreement with respect to the presuppositions concerning the nature of the object point to, and the epistemic value of the ostension. What can we do about the doubter who refuses to accept the evidence provided by the microscope or the telescope, because he disagrees with the method of ostension involved ("You are only pointing to an image not an object!")?

Concerning the asymmetries between names and descriptions, it is to Russell's credit that he anticipated many of the difficulties that occupied his followers. These difficulties were of two kinds: formal difficulties (dealing with the logic of names and descriptions) and material difficulties (dealing with the semantics of names and descriptions).

The formal difficulties (elaborated in the Principia in Chapter 3 of the Introduction and Chapter 14 of the proofs) arose from the fact that definite descriptions were often involved in ambiguities of scope. The problem of scope ambiguities for descriptions is

of crucial importance in the modal use of descriptions, as we will see later. We are generally acquainted with such problems in predicate logic when we distinguish between "every" and "any" in the contexts where scope matters.¹⁵ In this specific case the '(1x)φx' description occasionally presents problems especially when the object designated does not exist. Consider the statement

(9) The present King of France is not bald

which is initially translated in the formal language as

(10) $\neg\Psi(1x)\phi x$

This proposition is ambiguous and its ambiguity is traceable to the ambiguity of the scope of '(1x)φx'. If we take the description as having a large scope then we take (10) as affirming the predicate '¬Ψ' of '(1x)φx':

(11) The predicate '¬Ψ' is true of (1x)φx

which in our example means that

(12) The present King of France is a non-bald thing.

Alternatively, we can take (10) as denying the truth of the sentence 'Ψ[(1x)φx]', in which case the scope of the description is taken as narrow (applying to the closest predicate 'Ψ' and not the largest '¬Ψ'):

(13) 'Ψ(1x)φx' is false

which means the same thing in our example as

(14) It is not the case that the present King of France is bald.

This distinction matters formally and this can be shown in two ways. Consider in the first place the formal equivalents of (11) and (13) respectively:

(15) $(\exists c)(\phi x \equiv_x x = c : \neg\Psi c)$

(16) $\neg (\exists c)(\phi x \equiv_x x = c : \Psi c)$

It is obvious upon inspection that (15) and (16) are different extensionally. To see this consider the case in which what '(1x)φx' purports to refer does not exist. According to Russell (15) in that case would be false while (16) would be true. Given that both (15) and (16) are legitimate translations of (10), the above consideration shows both the ambiguity and its import.

Russell's way out of the ambiguity was to introduce a scope operator which utilized the '(1x)φx' in brackets and which functioned in the same way that quantifiers function to indicate scope. In this way we can decide about the proper analysis (and thus about the truth-value) of the statement involving a distinction. In the absence of scope indicators, Russell stipulates that the scope of the description is to be taken as the narrowest available.

In analyzing the scope-ambiguity problem in detail because it will turn out to have great importance later when we will consider the modal extensions of logic and specifically Quine's disagreements with Quantified Modal Logic. For the time however, our other purpose is also served because the scope ambiguity which exists for descriptions does not exist in the case of names, and in this way one can establish the first asymmetry between the two kinds of terms. To see this point consider the relations between (15),

(16), (4), and (7). (16) is the negation of (4) which implies (7). Statement (7) is the existence statement which follows trivially from the predication statement (4). By contraposition then the negation of (7) would imply (16): if, in other words, the $(\exists x)\phi x$ does not exist then (16) is true. It is equally clear that (15) in that case would be false (if there are no F's then there are no FG's).

Now, Russell claims that if there is an $(\exists x)\phi x$ (i.e., if (7) is true) then there is no difference between (15) and (16) as far as truth-value goes. This is obvious here because of the distributivity of the existential quantifier in a conjunction. If there is a $(\exists x)\phi x$ then (15) is true just in case ' Ψ ' is true of 'c' while (16) is true just in case ' Ψ ' is false of 'c'. According to Russell no ambiguity of scope exists for names, because the question of existence does not arise for them. Names and descriptions therefore are shown to be asymmetrical, on the basis of this consideration.

On the other hand, however, the statement that when $(\exists x)\phi x$ exists there is no distinction between (15) and (16) requires scrutiny. It can be raised to the status of a general rule to the effect that when descriptions do refer, then they act as names (logically proper names, that is), which appears as an interesting converse of the statement that if proper name (i.e. "Pegasus") does not denote then it is not a name but a description. This general rule however cannot be proven as a general theorem in the Principia.

Russell claims that it can be proven in each special case and suggests a general procedure. This too is understandable, because such a proof, since it would have to hold for every possible context, would either belong in the metalanguage or it would be based on induction.

With respect to the formal difficulties then Russell drew the limits of his solution and established thus its applicability: names and descriptions are not equivalent, but can be treated as such, if one is careful to avoid some well-signalled pitfalls. With respect to material difficulties, the situation is more complex. Let us concentrate on the name "Aristotle." Since we can meaningfully ask "Did Aristotle exist?", we conclude that it is not a logically proper name; it must then be treated as a description-in-disguise, but which description? Let us assume that the proper analysis of this description in disguise is "the teacher of Alexander." Yet, if "Aristotle" is always to be understood as a shorthand for "the teacher of Alexander", then the statement "Aristotle was not the teacher of Alexander" has to be analytically false, a contradiction, which it is not. The problem cannot be avoided by opting for another description because the same argument can be repeated for each description one cares to propose, including the seemingly "analytic" "the man called 'Aristotle'."

This material asymmetry cannot easily be regulated over, for it clearly involves appeal to meaning of terms. It is at this point that Russell's theory started to stumble. Wittgenstein,

The cluster theory both in its Wittgensteinian primitive version and in the version of Searle is not a radical departure from the Russell Theory.¹⁷ It falls squarely within the TTR for it agrees with Russell's theory more than it disagrees with it. Specifically the cluster theory accepts the validity of Russell's analysis of descriptions, accepts the general thesis that extension is determined by meaning, that proper names have sense, and that the best way to view the sense of a name is by some kind of logical-semantic equivalence with descriptions. The disagreement in fact consists of a modification of that last clause: Russell would insist on a one to one correspondence between name and description and would require a distinction between logically proper names and descriptions. Nevertheless the difference is significant for the cluster theorists cannot take proper names and descriptions as similar types of terms: an asymmetry has been established implicitly, for surely the descriptions have a unique sense. The statement "the teacher of Alexander taught Alexander" is analytic.

A more significant departure was initiated by P.F. Strawson in his seminal paper "On Referring." In that work Strawson questions both the problematic of Russell, as well as the general framework of his solution. Specifically, much of the logical impetus of the puzzles of Russell is lost by Strawson's questioning of the assumption that a singular categorical statement, if significant, has to have a definite truth value (one of the two 'T' or 'F'). This questioning affects not only the puzzles of non-existence but runs deep into the analysis of logic. Strawson questions the

value of a logic that has paradigmatic application to mathematics and science and not to ordinary language. Such an approach is bound to lose much of the Russell-Quine (and Frege) problem of objectivity of science, but that was already lost in the theories of later Wittgenstein. Strawson's problem of reference is not motivated by the problem of establishing what scientific discourse is about, but by the task of understanding linguistic usage.

Strawson agrees with the cluster theorists in rejecting the idea of a logically proper name, but his most significant step in the questioning of Russell's analysis of descriptions. How then is reference to be established either for names or for descriptions? Strawson argues that reference cannot be established in abstracto by logical analysis of terms. Instead one has to pay attention to the context in which a sentence is uttered. Reference is established in use, and not simply by virtue of the meaning of statements. "People refer, not words" is an accurate slogan-summary of the new standpoint. Even if one disagrees with this stand, one at least has to grant that Strawson has a legitimate criticism against Russell. Implicit in Russell's theory is the idea that unless the grammatical subject of a singular statement refers the whole sentence is meaningless.

While Strawson does distinguish the ascriptive from the referential aspect of the use of words, still he does agree with the TTR in taking meaning to be a vehicle for reference. His disagreement is that reference is not exhausted by meaning (i.e., meaning is

only a necessary condition for reference). Instead reference must be dealt by considering the context of the utterance of a sentence which involves a speaker-hearer situation. Had Strawson limited his claims to this appeal to context, his departure would have been considered as an obvious counterproductive suggestion. We all recognize the importance of pragmatics in semiotic, yet some abstraction is necessary if our aim is to obtain a general theory. One must however see Strawson's attempt as a suggestion for a radical departure. In the first place his argument has theoretical significance concerning the adequacy (or even the very possibility) of a rigorous and general theory of reference. Secondly, Strawson does attempt to provide the rudiments of a general theory of referring, but without appeal to rigorous logical analysis. His approach utilizes the second-order analytic tool of "presupposition": "A presupposes B" means "B has to be true in order for A to have a truth value, otherwise A is neither true nor false." In Strawson's theory there is a narrow and wide use of this tool. In the paper "On Referring", the substance of Russell's analysis (existence and uniqueness), are not presuppositions of reference, not analy-
santia of reference. In uttering a singular statement, it is presupposed that there is a unique object to which the speaker is referring by using the singular statement; if no such unique object exists then the sentence is neither true nor false, but it is not meaningless.

To see the second use of presupposition, one must examine the limitations of the first. Among the many limitations that Strawson's solution has, the most obvious one is that of impression or even circularity. While Russell analyzes reference either as ostension (in naming) or by proper appeal to existence and uniqueness, Strawson seems to presuppose reference ("the object the speaker is referring to"). How is reference achieved in a speaker-hearer situation? Here one needs to introduce a second use of 'presupposition', and in the process one sees the connections between the seminal paper "On Referring" and Strawson's subsequent work especially Individuals and The Bounds of Sense. According to Strawson, reference, which can occur in context of speaker-hearer situations, presupposes the existence of a unique spatiotemporal framework of individuals that fall into kinds. It is by virtue of this unique framework that identification and re-identification of objects is possible, and thus reference is established. This appears as an Aristotelian view, but only in content,¹⁸ not in argument. Aristotle's metaphysics is based on logical considerations while Strawson's is based on epistemological considerations. In this respect Strawson is a Kantian. The universal framework of individuals is a presupposition in the sense that it is the minimal ontological framework that has to be used if we are to accept the very possibility of any intelligible discourse. Given that we have the latter, according to Strawson (no relativist he), the de jure existence of the former is established. This is clearly a form of the transcendental

argument, that was introduced in philosophy by Kant, and, as we said, in the Introduction, led to relativism because of the distinction between the objects as they are in themselves and the objects as we know them. Strawson however in the Bounds of Science seeks to avoid this predicament by arguing against its presupposition. Kant was misled by an erroneous psychological model in epistemology.

Before launching into the criticism of Strawson's theory of reference, let us pause for a while to appreciate its significance, if only as a way of showing how important is the concept of reference in philosophy. Strawson's concept of presupposition represents a radical break with Twentieth Century philosophical tradition because it suggests a return to metaphysics as an independent discipline. Before Strawson, the Positivists questioned the legitimacy of metaphysics (as opposed to the non-metaphysical science), while Quine questioned the separability of metaphysics from science (no prior theory of substance, no independent standpoint). For Strawson there is an autonomous discipline of metaphysics as a discipline that investigates presuppositions of intelligibility and yields ontological results which are now legitimized by the above appeal to presuppositions. In this way he can distinguish between descriptive metaphysics and revisionary metaphysics (the latter to be avoided).

This rehabilitation of metaphysics must also be seen as a legitimation of the ordinary language approach to philosophy. By distinguishing metaphysics from science, and by establishing the

former as a study of presuppositions, Strawson agrees partly with later Wittgenstein who sees philosophy as separate from science. But while Wittgenstein (and the Positivists as well) see this separation as based on the idea that philosophy is not a doctrine (that can be true or false) but is rather an activity (that can be successful or unsuccessful, well or poorly done), Strawson provides a content for the activity, and thus explains it and provides a clear goal for it. By comparison, the conceptual clarification activity of the Positivists and the therapeutic activity of later Wittgenstein suffer from vagueness or foundational difficulty: the former requires a concept of analyticity, while the latter is completely tied down to concrete situations. In Strawson's account the analysis of ordinary language has as a clear goal the mapping of metaphysical presuppositions. Ordinary discourse is examined by philosophy as a vehicle for doing metaphysics. The success of this approach can also be illustrated by the work of G. Ryle on the problem of body and mind and Strawson's own work on the concept of person. I consider these works successful not as a clarification of linguistic usage alone, but as clarifications that indicate ontological distinctions. To say, for instance, that 'mind' functions not as a noun but as an adverb, is to at least caution the investigator about a likely category mistake. Category mistakes are not just grammatical mistakes, they are mistakes in ontology. Russell's criticisms, therefore [Russell: "Mr. Strawson on Referring"], while valid against some members of the ordinary

language philosophy, are not fully accurate when applied to Strawson, who after all is doing metaphysics.

Despite the novelty and the philosophical significance of Strawson's views on reference, there are serious drawbacks to his position. First and foremost both his critique of Russell and his fundamental idea of reference-in-use (speaker-hearer context), would lead us to abandon a general theory of reference, and Strawson has not made a good case for abandoning this project. I have argued this point in detail elsewhere [1975], but it is sufficient here to note that Russell's theory cannot be defeated by stating oddities of 'use', in the same way that the irregular fall of a leaf does not constitute a counter-example to Galileo's law of freely falling bodies. Some level of abstractness is allowed for all theories, especially if the theories happen to square with other sciences and mathematics. This is a general point, so let us make it specific. According to Russell, 'identity' is the key to reference; Strawson abandons this formal notion and opts for a weaker notion as the explanans of reference: "'identification' and 're-identification' in a speaker-hearer context." There is no doubt that the move results in loss of clarity, but is it a gain in accuracy? The answer here is not obvious, it is conditional on criteria. The criterion used by Strawson is that of agreement with intuition. Others would consider coherence with science a better guide to accuracy. But even if that is not so, still appeal to intuition is not a sure guide: I do not know intuitively which to choose

from "'The present King of France is bald' is false' and 'the present King of France is bald' is neither true nor false'.

Strawson's appeal to intuitions however is only a first step; the justification for that appeal, as I explained, rests on an appeal to Kantian metaphysics. In this specific case, identification of an object rests on the presupposition of a framework of individuals, and this framework of individuals is established as "the limiting features of any notion of experience that we can make intelligible to ourselves." It is an open question of how costly this metaphysical commitment to Kantianism is. For our purposes, we can point to some such costly commitments involved in the idea of uniqueness and universality of the referential framework.

Many apologists of Kant have argued that even if everything else in the First Critique fails, still Kant's theory agrees with our intuitions. This point is crucial when one recognizes that some of the presuppositions on which the transcendental argument was based turned out to be false because of later scientific advances. Consider for instance Kant's absolute belief in Euclidean Geometry and Newtonian Mechanics.¹⁹ Strawson is placed in the position of defending Kant against advances that question the universal application of the above two theories. To argue that Euclidean Geometry forms the limits of our intuitions, may very well be circular, and at any rate it is not helpful: are we to change our science according to Strawson? A way out would be to distinguish metaphysics from science and place Strawson's efforts in the former,

but then one has to explain the relations between the metaphysics of space (i.e., the description of our intuitions) and the formal account of space contained in the sciences. Is science intuitively unintelligible? Are the original non-scientific intuitions clear? How are we to make our metaphysical intuitions clear except by science or some other such formal method?

While the above remain open questions for anyone who would take Strawson's course, we can say with certainty that Strawson's theory of reference with its appeal to objecthood and ultimately to Kant will not do for our purposes, of countering Relativism, for two basic reasons. In the first place, any appeal to intuitions is subject to the basic relativistic argument. Secondly, the peculiar nature of the recent relativistic movement cannot allow for appeals to Kant. The basic motivation behind most of the recent relativistic theories was the attempt to understand advances in modern physics that are distinctly anti-Kantian. This is clear at least for Hanson and Feyerabend, and it applies *mutatis mutandis* for Toulmin and Kuhn. In fact we can construct an argument that the relativist can use to block Strawson's account of reference (applied to the dispute at hand): Strawson's account of reference presupposes a universal framework of individual objects (ultimately derivable by Kantian means). Leaving aside the question of "universality", which is clearly unsupportable theoretically, the content of such framework, if it is to be used as the ultimate framework of explanation, runs circular to the professed aim of our science. Any

science worth its salt (i.e., any explanatory science, any science about which there is metaphysical dispute) does not involve previous agreement as to the categories that are to be used in its explanations (Toulmin's point). Put in another way, as Hanson might put it, in the frontiers of science (i.e., particle physics) objecthood is the explanandum and any appeal to an ultimate framework of individual objects would turn our science circular.

The choice at this point is three-fold. One can, in the manner of Strawson, take objecthood as given, that is to say as part of metaphysics, and thus as a limiting presupposition of science. A second route would be to take objecthood as radically indeterminate, depending on arbitrary choice of whole frameworks - and even within one framework one can consider objecthood indeterminate, because there are many sets of objects which would fulfill the framework as a system of predicates.²⁰ The third alternative, realism, can be more clearly explained by considering the response of the first two on the subjects of identity and predication. Strawson takes identity as clearly derivative from objecthood (identification, re-identification of objects). The opposition (Quine and the Relativists) take identity narrowly as a formal relation, and its interpretation as subject to indeterminacy. The realist accepts the formal account of identity as minimal, but then extends the concept in application. He does not assume objecthood as the basis of identity, but rather takes some kinds of identities as the key to objecthood. All the identity relationships that he recognizes have necessity

as a characteristic, so he uses the concept of necessity, a metaphysical concept, as a way of distinguishing among predicates the ones that apply essentially to objects from the ones that do not do so. Some of his conclusions here are akin to Strawson's because Strawson, like the Realist, does draw an Aristotelian distinction between kinds of predicates, but the Realist's distinctions are based not on grammar but on science: it is the identifications that are shown by science to be necessary (i.e., to hold for all possible worlds) that are used as ways to understand objects. These identities are then used in definitions contrary to Strawson who clearly downplays definitions in "On Referring", as an element that led Russell astray. The definitions themselves can be of many kinds; not merely an appeal to genus (natural kind) and differentia but also atomistic reductions of the form "water is H_2O ". The points above will become clearer as we proceed to explain modern essentialism, but for the time let us note that Strawson's account of reference leaves us without a good account of objecthood, identification, or identity. Kripke's examinations of these concepts and their relations to reference are the natural step for any Strawsonian who is not satisfied by appeals to Kant.

The final assessment of Strawson's contribution to the problem of reference then is mixed. Apart from any evaluation one has to recognize that Strawson's contribution even though was not a radical departure from the TTR still provided the seeds for such a departure. The seminal point in Strawson's contribution was

the recognition of a double function of expressions: they can function referentially and they can also function ascriptively. This is certainly a departure from Frege, Russell, and Quine who seek to provide a theory that explains the first in terms of the second. Drawing such a sharp distinction certainly requires that an independent theory be provided for the referential function of words. Yet, as I explained, Strawson's so-called theory is unsatisfactory as a theory. Part of the problem arises from the fact that the distinction is not drawn sharply enough. Strawson is still subscribing to one fundamental TTR thesis that takes the ascriptive function as a necessary condition for the referential. The other part of the problem is due to the fact that what he proposes as the additional conditions, that would form together with the necessary conditions the jointly sufficient conditions for referring, are formally questionable (they do not make a 'theory') and more importantly, they do not even promise a theory. Strawson hurries to appeal to metaphysical presuppositions, before he even has time to propose a theory of referring.

It was left to Donnellan to exploit Strawson's break, and put his finger squarely on the 'referential' function of singular terms. Donnellan's achievement was to draw the sharpest possible distinction between the 'referential' and the 'predicative' (ascriptive) function of descriptions. He provided arguments that showed that the predicative use is not only not a sufficient condition for the referential function, but even not a necessary condition.

Reference can be established for a description in a speaker-hearer situation even though the predicative content of the description fails to apply to the object referred to. This is clearly a departure from the TTR, for it is now clear that extension for singular terms is by now clear that extension for singular terms is by no means determined through intension. The obvious question then would be how is reference determined if not by way of meaning or intension? Barring a Strawsonian appeal to Kantian universal frameworks, the only plausible theoretical way to explain reference (in a context) is causality and direct ostension. Donnellan did not provide a theory however. He simply isolated the 'referential' by citing a few fundamental observations about reference. It was left for Kripke to provide such a theory of reference, or rather to provide the rudiments for such a theory. But let us examine these developments in order starting with Donnellan.

As we explained earlier, one of the first problems that the TTR encountered was to reconcile the asymmetry of names and descriptions with the claim that all names are descriptions in disguise. Specifically, the requirement that proper names be meaningful implied, accordingly to the TTR, that some synonymy relation be established between a name and a description. But then no description could be found to carry the task, because in no case could we have an analytic statement of the form 'K is the C' where 'K' and 'C' are filled by a proper name and its corresponding description respectively. The statement "Aristotle is the teacher of Alexander"

is not an analytic statement, nor is its negation contradictory. The cluster theorists tried to amend this defect by replacing 'the C' with a cluster of descriptions loosely connected by inclusive disjunction, but the previous argument can be modified to apply here as well. One can consistently negate the attribution of the cluster to the proper name. If we introduce modalities, the argument can become even stronger in two ways. In the first place the argument becomes general: "Aristotle might not have had any of the characteristics or properties that our descriptions attribute to him", a perfectly consistent statement. On the other hand, the asymmetry between proper names and descriptions is now clear: compare the odd but consistent "the teacher of Alexander might not have taught Alexander" or "the Stageirite might not have been born in Stageira", with the paradoxical "Aristotle might not have been Aristotle."

All these difficulties of the TTR were based on a principle that I have already mentioned on numerous occasions, and which we encountered for the first time as a "Fregean Dogma." The thought behind the principle is so simple as to hardly require separate mention. Given that the key to understanding proper names (as well as other singular terms) is capturing their reference, and given that the use of such names is learned, it was thought as obvious that the only way that reference is established is by appeal to some characteristic which is uniquely true of the object to which the term refers. This unique characteristic could be expressed

as a description so that to give the identifying description was thought as the same with establishing reference to the object.

It is this principle, which he calls "the Principle of Identifying Descriptions", that Donnellan questions as a first step to a better account of reference. He starts by focusing his attention on definite descriptions and drawing a distinction in the way descriptions are used. Depending on the context, a description may have a referential or a predicative use. Consider for example the utterance

(17) "Smith's murderer is insane."

Depending on the context, it can mean:

(18) "Whoever killed Smith is insane"

which is a predicative use of "Smith's murderer" or it can mean

(19) "This man (say, Jones) who killed Smith is insane"

which is a referential use. Strictly speaking (19) cannot easily express what Donnellan has in mind and for a very good reason: reference is supposed to be established independently of predication! How to convey this extra-predicative reference by predicative means would almost be contradictory. Yet Donnellan gets his point across. We could amend the situation by a new version of (19) that is supposed to be identical in meaning to the original (19):

(19) This man-who-killed-Smith (= Jones) is insane.

This version is more satisfactory because by hyphenating the whole subject phrase, we have turned it into a proper name, lacking, that is, any significant internal logical structure. It functions

as an individual constant. It is also significant that to convey the referential (in print) we had to also resort to some form of identity. At any rate, (19) in the old or the new version, is clearly different from (18) from a referential standpoint, because in (19) reference is clearly presupposed as established, while no such presupposition exists for (18). This clearly matters even in a truth-value relevant way, since statement (19) will still be true even if it turns out that Smith in fact committed suicide, as long as Jones is insane. Similarly, if the murderer of Smith is found who happens to be other than Jones and he is not insane, statement (18) would clearly be false but (19) would still be true. Finally, if Jones is not insane, (19) is false and known to be so, while (18) may still be true.

This argument is a clear rejection of the Fregean Dogma and of the TTR, because it implies that the establishment of reference does not require that a description be uniquely true of an object. Instead, reference is somehow established independently of whether the object referred to fulfills or does not fulfill the predicative function contained in the description. To use another Donnellan example, I, as a speaker, can establish reference to an object (Jones) by using the expression "the man over there with the martini", while in fact Jones does not have a martini at hand. If the fulfillment of a description by an object is neither necessary nor a sufficient condition for reference to the object, then reference is clearly separated from sense, descriptions, or predications, and must be explained, or accounted for, in some other fashion.

But before we focus on this problem, let us simply note this distinction and examine its significance.

The distinction between predicative and referential use clearly explains away the paradox that the cluster theorists faced. The statement "the teacher of Alexander might not have taught Alexander" is not paradoxical or contradictory because the expression 'the teacher of Alexander' is used referentially, not predicatively. Even the seemingly logical contradiction "the teacher of Alexander is not the teacher of Alexander" is explained away by appeal to equivocation. It is not of the form ' $a \neq a$ ' but rather ' $a \neq b$ ', and in 'the $\neg A$ is not the A' with the first use referential and the second predicative (the two things different). That much should have been obvious since the old French satirical song verse "your daddy isn't your daddy, but your daddy doesn't know", in which the same expression 'your daddy' has referential, predicative, and again referential use respectively.

Donnellan's suggestion should also help us understand identity statements and thus solve Frege's puzzle. Not all identity statements have the same cognitive content. It depends largely on the use of the terms involved: sometimes the identity statement does establish the reference of a term, but only if the reference of the other term is already established (non-trivial identity). In other occasions an identity statement establishes the meaning of a term just in case both terms involved are used predicatively. Both of these cases should not be confused with the law ' $a = a$ ' which is a logical tautology.

Concerning the puzzle of the statements of non-existence, it is clear that the distinction between predicative and referential use would not be immediately applicable by way of providing a solution. Whether we analyze the statement 'The ϕ does not exist' as involving a referential or a predicative use of the expression 'the ϕ ', the problem of the truth and meaningfulness of the statement still remains, as long as truth is based on reference. What is needed to solve the non-existence puzzle is a complete theory of how reference takes place and why in some cases there is reference to be established while in others none can be found. Donnellan's early contributions simply told us that reference is not established by way of sense. It does not tell us how it is established. In a more recent paper ("Speaking of Nothing"), Donnellan tries to use Kripke's theory of reference to solve the puzzles of non-existence, but it is better to examine this solution after we have introduced Kripke's work.

For the purposes of this essay however, the most significant aspect of Donnellan's contribution is that his distinction can be used to defeat the main argument of the Relativists. As we argued extensively in Chapter 2, the strongest and most fundamental argument of the relativists is based on the non-categoricity of science as a formal system, and the use of this formal argument in turn is based on the idea that, at least, as far as the theoretical terms of science are concerned, their reference is system dependent. That claim in its turn was based on the more general claim that

all reference is established by way of meaning so that, simplifying the intermediate steps, we can say that the system relativity of meaning results in the system relativity of reference. Put in another way, if all terms (even the most paradigmatic cases of referring terms) acquire reference by way of description, then this should be the case a fortiori for theoretical terms; these terms moreover are only implicitly definable by the theory in which they figure. It follows that their reference is system relative. On the basis of this an in principle argument can be constructed to show that the reference of the theory as a whole is indeterminate by virtue of the non-categoricity of the formal system. In concrete terms then, we can say that theories do not and cannot share reference, even if they contain common terms, and despite the best intention of their proponents, because, by virtue of the system relativity of meaning, the term 'in' (say 'electron') refers to different things in different theories. In general there can be no way of showing that they could ever refer to the same objects.

Donnellan's observation undercuts one of the basic premises of this argument by showing reference to be independent from sense. It is possible for two different theories to use the same theoretical terms to refer to the same objects even though their claims about these objects are different, and even if what is claimed in both cases about these objects is, strictly speaking, false. Rutherford and Bohr, later in his career, could be referring to the same entity when they were talking about the electron in different ways. At

least there is no in principle reason that they could not, as the Relativistic Argument requires.

By isolating the referential function and by drawing a sharp distinction between the referential and the predicative, Donnellan took a lot of the wind from the relativistic argument about reference. One ought not however to overestimate the effectiveness of Donnellan's thesis in undermining the relativistic argument. Our work is by no means completed. Consider in the first place the form that this counterargument to relativism takes. It does not have the form: "it certainly is not so!" but rather of the "it-does-not-have-to-be-so" form. The reasons behind this conservative appraisal are the following. In the first place the thesis of Donnellan, although meant as a general approach to reference is still limited to definite descriptions. In the second place, Donnellan, by providing some counterexamples to Russell, did not absolutely dislodge the TTR; it might still be possible to explain away these counterexamples. Finally, at best Donnellan isolated (in a Biochemist's sense of the word) the referential. He has no theory of reference to speak of. Independently of Donnellan's thesis, and from a Relativistic standpoint, it would even be possible to accept Donnellan's observation and still argue plausibly that, while Donnellan's observations held true for individual terms, nevertheless the Relativistic argument was about theoretical terms which are general, and whose reference is still determined by way of intension, along the lines that the TTR is suggesting.

Passing from an appreciation of the significance of Donnellan's work to an evaluation of his conclusions, we should concentrate on the second point mentioned in the previous paragraph, leaving the other points for later treatment. Do Donnellan's counterexamples effectively dislodge the TTR? In this case, the strongest version of the TTR that we can take should include the Fregean Dogma, Russell's Theory of Descriptions (but not his theory of logically proper names - since Donnellan partly agrees with it), and Quine's extension of Russell's Theory.

If Russell's Theory is meant as providing a complete analysis of reference, that is to say an analysis of descriptions meant as providing the necessary and sufficient conditions for all referring, then Donnellan's counterexamples are effective, but only in a formalistic sort of way. According to Donnellan a description does not have to be uniquely true of an object to establish its reference; indeed it can be even false of the object, as the "man-with-the-martini" counterexample shows. Yet it is significant that some description was used, and moreover, a description believed to be true by both speaker and hearer. Consider for example if the speaker had tried to establish reference to the object by using the expression "the man over there who is less than seven feet tall" or "the man over there whose nose is not equivalent to the square root of 2." It follows then that while false descriptions may establish reference, still not all false descriptions will do (nor all true ones). One could imagine the response of a Russellian along one of the two

following lines. Either we replace "the description uniquely true of an object" with "the description believed by hearer and speaker to be uniquely true of the object," or we can take a more empiricist line of approach and claim that "man with martini at hand" be understood as "man with glass at hand containing liquid which may be a martini" (probably due to appearance of olive-looking object suspended inside the liquid content and so on...).

What these two responses amount to is an admission by the Russellian that while reference of a singular term is not exhausted by a description uniquely true of the object, still what Donnellan is proposing as a determinant of referring is a vague appeal to the pragmatics of the situation. He (the Russellian) then can insist that the pragmatic element that enters in referring be analyzed along the traditional lines. Furthermore, this does not have to be the case in all contexts (especially scientific contexts) but only the contexts where pragmatics are essential. For the purposes of science we still need a general theory of reference.

At this point we reach a theoretical standstill because on the one hand, Donnellan has produced a counterexample that does affect the TTR, yet in the absence of a theory the counterexample cannot be taken as a clearcut refutation of the TTR. The Russellian, on the other hand, has a well-entrenched theory and can limit the effectiveness of Donnellan's counterexample by limiting its application to pragmatics (but not to semantics).

Yet the Russellian ought to worry about the counterexample as indicative of a more significant point (and of more trouble to come). The deeper point can be illustrated by two kinds of considerations. In the first place, Donnellan's counterexamples are clearly applicable to the cluster-theory versions of the TTR (which are the weakest and thus the most difficult to disprove). This consideration is significant if examined along the following lines: proper names and singular terms in general are used but always with a subtle provision for falsity built into their use: one can still be wrong in his beliefs about the individual object. To allow for that possibility of falsity, one has to forgo an absolute requirement that a truth condition be fulfilled before falsity can be possible, because that would place the truth-condition outside the realm of the possibly false and the only candidates for such definitely true conditions are too uninformative to establish reference to a specific object. Yet despite the possibility that all identifying descriptions could be false of the object, still reference to the object must be established to even allow for the possibility of the descriptions being false of the object. This condition is obviously stronger when one considers modal contexts because in these contexts the descriptions do not have to identify an object or the same object ('the author of the Iliad' in some other possible world may denote Homer or some other person or many people or no person at all since there are possible worlds in which there is no Iliad).

This underlying requirement, that reference to an object be established prior to and independently of any truth-condition being fulfilled by the object, provides grounds for the rudiments of a theory. Donnellan gave us 'circumstantial evidence' that there is a purely referential aspect prior to and independent of any attributive aspect. Does his distinction between referential and predicative use pin down the referential adequately? Again, his observation of the distinction is too limited and lacks strong theoretical backing to be considered a genuine disproof of the TTR. Consider the two-fold analysis of (17) (i.e., (18) or (19)): there is no doubt that (17) is ambiguous and that it can be analyzed either along the line suggested by (18) or by (19). As my citing of the spicy French song showed, ordinary language does recognize the double nature of such definite descriptions. But, speaking from a formal standpoint, doesn't Russell's analysis capture both uses? This question deserves a more extensive treatment, so we should adopt a standpoint independent of both the TTR and Donnellan's rudimentary theory (the distinction) so that we can evaluate their claims to the explicandum in a more judicious way.

There is no doubt that singular terms (including proper names) are problematic from a logical point of view that is obvious both in ordinary discourse and in the formalization of this discourse by means of first-order functional logic (predicate calculus). This can be shown both in the French lyric example but also by the long struggle of logicians to tame these terms. Take an

ordinally singular statement,

(20) " Rostropovich is a cellist".

There are three ways of formalizing this sentence. The first is hardly an improvement over the sentence:

(21) 'Cr' ('C is true of r')

where 'C' stands for 'is-a-cellist' and 'r' stands for 'Rostropovich'.

This is not to say that it cannot be useful for logic in any way.

Consider, for example, a first premise stating that all cellists have two hands; by introducing Cr we would conclude by a valid argument form that "Rostropovich has two hands", if that is what we cared to establish. The reason I find 'Cr' inadequate is because there seems to be more logical structure to the initial statement than 'Cr' reveals. On the basis of the original statement one does not only conclude that there is a cellist, but can also make some other inferences that are significant. A second translation would be "there is something that is Rostropovich and is a cellist" or in logical notation

(22) ' $\exists x (Rx \ \& \ Cx)$ '

It is obvious however that this translation does not capture all the logical structure that is packed in the original (20). Consider the ordinary discourse inference based on (20)

(23) "I am Rostropovich."

(24) "So, you play the cello."

Clearly the conclusion (24) cannot be logically derived either from (21) or from (22). Since the inference (20), (23) - (24)

appears to have a Modus Ponens form, it is reasonable to analyze

(20) as having an A-form of categorical statement:

(25) $(x)(Rx \supset Cx)$

Yet (25) cannot be the one and only accurate translation of (20), because (25) would be true in case there was no Rostropovich, while (20) would either be false or truth-valuelss.

The above considerations indicate clearly that there is both a referential and a predicative function to singular terms. Russell's theory, captures all the logical aspects packed into (20) plus one more aspect not developed in (21), (22), or (25): the fact that 'R' is a predicate has additional structure that can be uncovered by identity (not just predicate calculus alone):

(26) $(\exists b)(Rx \equiv_x x = b : Cb)$.

If Russell's theory captures so many of the logical nuances of individual terms, why would one reject it from Donnellan's standpoint? The general answer to this is that, on the one hand, Russell's theory captures too much, while, on the other, it fails to capture some important aspect. It captures too much because, according to Donnellan's counterexample, no truth of (26) type form is required before reference is established. Even if the description strictly speaking fails to be true of the object referred to (and indeed is true of another object), still reference may be established. But in addition to the ways that we proposed, the Russellian can handle the counterexample with a question: how then is reference established in this case? Is there no identity involved at all?

Donnellan would reject any specific identity (because it may be false of the object, while reference still occurs), but I see no easy way of getting rid of identity altogether. Indeed some scrutinizing of identity might help us unpack the referential aspect that Donnellan has in mind.

There remains the question of whether Russell's theory captures the predicative aspect of descriptions that Donnellan isolates in the double rendering of (17). Again there would be disagreement here because the predicative aspect, that Russell's theory captures, is contained in (25) and is thus different from what Donnellan has in mind. Although (25) and (18) appear to be similar in form, Donnellan's 'predicative' applies to use not to logical forms. The difference can be easily illustrated if there were no Smith's murderer, (18) would be false according to Donnellan, while if there were no R, (25) would be true (vacuously so) according to predicate logic. It is not the aim of this dissertation to adjudicate on the matter whether ' $(x)(Fx \supset Gx)$ ' is an accurate translation of 'All F's are G's', or whether the person who advises "everything I have heard about this product is good" (even though he has heard nothing) is lying or speaking the truth. Instead what I want to point out is that the predicative aspect that Donnellan is talking about has clearly some of the aspects of (25), even if one looks at ordinary discourse alone.

What I have in mind here is that in ordinary discourse we normally indicate that we are engaged in predicative use (and not

referential) by suitable expressions. If our aim is to speak of 'whoever murdered Smith' by using 'Smith's murderer', we normally compensate in contexts like (17) by adding expressions of caution like "must be" as follows

(26) "Smith's murderer must be insane."

This is a normal usage which we find often in judgments "The owner of this house must be short, or paranoid, or a devout Christian, or low middle class, or whatever." If my observation is correct, then, in the first place, Donnellan is shown to be on thin ice, since in ordinary discourse situation we do draw a distinction between the two uses. Secondly, however, there is no in principle reason why we should not adopt Russell's claim to the predicative use. How are we to judge in this case, given so much that is at stake (i.e., the proper translation of ordinary discourse into logic)? A lot depends on the reading of the "must be" in (26) or in judgments like (26). I can think of four ways of analyzing (26) and one should develop them, so that even if one cannot choose between them at least one will be aware of the alternatives.

The first two ways have already been mentioned. The first is Donnellan's which would equate "must be" with "is", but would claim little by way of logical structure. At best it would be rendered by:

(27) $(\exists x)(Mx \ \& \ Ix)$

Yet (27) clearly cannot carry the weight of (26) because the 'I'

predicate is attributed to x by virtue of its also being an M . It is the peculiarities of the M that lead to the I .

The second way, Russell's has been adequately exposed by (25), and despite the paradoxes of implication, still it establishes stronger connections (conditional ones) between M and I in ways that I believe match to speaker's intentions.

The other two ways differ from the first in that they take the "must be" as a kind of semantical indicator and thus consider (26) not a judgment of its own but a meta-judgment or an elliptic judgment. Specifically (26) for them is the conclusion of a tacit argument. One would consider it a normal deductive argument of the form

$$(28) (x)(Kx \supset Ix). \quad , \quad Km \vdash Im$$

which is indeed similar to the memologico-deductive explanation form. The other would consider the tacit argument to be of inductive form and, specifically, abduction, or inference to the best explanation: given the facts of the murder, the best way to explain them is to assume that the murderer was insane. Put in a more formal way: the facts of the murder of Smith are highly improbable, yet they become more probable on the assumption that the murderer is insane.

The third way of interpreting has the blessings of conservatism and overcomes the difficulties of both the first (no connections between M and I) and of the second way (what if there is no murderer at all). Yet it also loses the predicative aspect of "Smith's

murderer" because it has to employ the predicate K which would spell out the specifics of the murder, while it takes "Smith's murderer" as an individual constant, i.e., a proper name. This surely does not help because, in the first place, this is what we sought to analyze. Secondly, and more importantly, the proper name here occupies referential position, otherwise the inference would not go through.

As a Realist I am partial to the last solution, because I find abduction to be the most significant type of scientific inference, which allows us to employ the content of explanations in our inferences. In the specific case for instance, I find that the evidence for the predicative use of "Smith's murderer is insane" will seek to analyze the murder and show the connections between this murder and insanity. At the same time however, for purely formal reasons (which I outlined by the example (20)-(25)) I find that some such appeal to Russell is required to allow for all the logical nuances of singular inference. The two positions are not irreconcilable. One can take Russell's account as minimal formally, and there "must be" as merely pointing to the ' $\supset$ ' connective, while the task of spelling out the 'why' of this connective can be allocated to the abductive account: a purely material task. I do not however wish to hid the difficulties involved here. The abductive account departs from the material conditional of Russell, and furthermore requires explication. Specifically we are no where near a clear understanding of , let alone obtaining criteria for, what counts as a better

explanation or a 'best' explanation. This, as a project, is probably one of the best motivations for being a Realist, but we are not close to a solution yet.

We are left therefore with four available solutions, each with strengths and weaknesses and with only our general sympathies as a guide. Nevertheless for our present, limited purposes we can say that Donnellan's account is not strictly speaking correct. If there is a distinction between the referential and the predicative, it is not clear that logical exposition will not capture it. Even ordinary discourse seeks to capture it by means that are a lot more permanent than simple appeal to different contexts; such a means is the replacement of "is" with "must be". Knowing the context surely does help, because knowing the exact sense of (26) type statement helps us decipher it. One can even do better and assign to the context certain permanent features that will help us analyze the particular piece of discourse. (Consider for example a statement like "the owner of this house must be a dwarf." One feature of the context might be the kind of evidence that one would be willing to cite in support of the statement's truth. The problem with the traditional conservative approach (the third) is that it cannot distinguish between the two following pieces of evidence (i) "...because every one in the village is a dwarf" or (ii) "...because the ceilings are only five feet high." Clearly (ii) is more appropriate evidence for the statement. Another closely related cut into the context would be to decide what would count as an effective

counter to the claim. Consider, for example, the counter "I am the owner of this house and I am certainly not a dwarf" as opposed to "This is not a house, it is a model of a house", or "According to your reasoning high ceilings would indicate that the owners were giants." The latter replies are more close to the spirit of the "must be" than the former one (even though the last one is clearly erroneous). Admittedly, such criteria are not sharp, but they indicate some important distinctions that will turn out to have significance later. Again consider the evidence in support of "Jones must be mad" and "Smith's murderer must be mad"; in the first case the evidence will have to mention other things besides Jones, while in the second case one can rest with the facts involved in Smith's murder (because the madness in the first case is not attributed to Jones by virtue of being Jones, while in the second case it is of the murderer-x-qua-murderer that we attribute madness).

I bring these points up here as a preamble of what is to follow. I want to show the intimate connection that exists between the distinctions referential-predicative and modalities. This in fact is the point that Kripke grasped, and which eluded Donnellan. As it stands however, Donnellan caught a glimpse of the distinction and produced an argument to show that the TTR was erroneous. Given the abstractness and generality of Russell's solution, Donnellan's argument could be dismissed or at the very least contained. To make it stand as crucial counter-instance a theory was required. Donnellan's suggestions do not amount to a theory because three

crucial aspects were left unexplained: (a) He did not give us a theory to explain why his distinction did not apply to proper names. (b) He did not give us a theory to explain the distinction between the referential and the attributive use of descriptions (too much appeal to context without interpretation of the salient features of the context). (c) Even though he pointed towards the referential he did not propose a theory as to how reference takes place.

Much of the importance of Kripke's contribution lies with the fact that his theory did account for the three items that Donnellan's explorations left unexplained. It would however be wrong to consider Kripke's theory as a simple response to Donnellan, for two reasons. Kripke captured the reasons behind the semantics of modal logic and because he recognized the significance of Donnellan's distinction for modality. And a closely related second point: while Donnellan abandoned identity (as a Russellian element in the analysis of reference to individual) since any specific identity was subject to his counterexample, Kripke paid a closer attention to identity in general and connected it with modalities.

3.3 Modality, Quantification, and Reference

Even by virtue of its name ("Causal Theory of Reference") the CTR appears as a formidable doctrine to examine. Given our insecure knowledge of the concepts of "cause", "theory" and "reference", rolling them into one promises that the ways of going wrong are not just squared, they are cubed. I shall introduce

the CTR by examining first the problems of modality, because, in my opinion, the only way to make proper sense of the CTR is by considering first the problems associated with the semantics of modal logic and especially its extension into quantification theory (QML). Even a cursory look at the very recent literature on the CTR, however, will show that my methodological decision is not (de facto) uncontested. Not only history, but ignorance of history repeats itself, and in the same way that the connections between Frege's problem of reference and his philosophy of science were lost, in our most recent times (where often half-baked articles are raised to the status of theories) the CTR is often discussed as an independent theory with hardly any mention of its genesis in modality. These approaches do not only miss the connections between the new theories and science, but they have hardly learned the lessons of the TTR. The way they avoid modality is by focusing on the deictic activity involved in naming. This could only lead in two fruitless directions: either back to Russell's Logically Proper Names (which as we argued were neither logical, nor names, nor, a fortiori, proper) or back to an error clearly recognized by the Cluster Theorists; as we argued in 3.2, ostension cannot on its own bypass the predicative aspect of the use of singular terms, only by isolating a characteristic of the object can the deictic activity activity succeed in establishing reference generally.

In addition to these negative considerations concerning the alternative, there are positive considerations in support of our

approach. The one already mentioned is the connection between modality and the philosophy of science which will be further developed in the following pages. Of equal importance are the considerations concerning identity. Donnellan's work dissociated the reference of descriptions from any particular identity statement, yet not from identity all-together. The problem associated with the semantics of QML bring forth the need for a reconsideration of identity on new, non-Fregean grounds. Indeed Kripke's work can be seen as an attempt to develop a new, stronger, theory of identity that bypasses the Fregean dogma, and in this sense, can serve as the basis for explaining the elusive "referential" aspect of use in separation from the predicative.

There were hints of the need to examine modalities already in the preceeding section. The earliest had to do with the strong modal version of the anti-cluster argument: Aristotle might not have been any of the things that our uniquely identifying descriptions ascribe to him. This argument and its variations were made explicitly. An implicit appeal to modality was already contained in the difficulties of drawing a sharp distinction between the referential and the predicative aspect of descriptions. The source of the difficulties was due to the fact that the distinction between what is uniquely true of an object and what uniquely identifies an object cannot be drawn in actuality. One needs to distinguish among characteristics of the objects all of which happen to be true, and the only way this can be done is by appeal not only to

what it true, but what might have been true or what must always be true. Of course Donnellan did draw his distinction but not in a systematic way. As we argued it would be easy for a convinced Russellian to dismiss Donnellan's observations as aberrations that belong solely within pragmatics, and of no systematic significance for semantics. The distinction however does have systematic import, as we will show, when we consider modal contexts.

Finally the most recent hint of modalities to come came at the end of the preceeding section in our attempt to understand the difference between "Jones must be insane" and "The writer of this letter must be insane." The surface-syntactical similarity and the strong "must be" lead one to translate both as of the general form ' $(x)(\phi x \supset \Psi x)$ ', yet there is a subtle difference between the latter and the former when we evaluate the strength of the connection between antecedent ϕ and consequent Ψ . We roughly spelled it out by saying that in the first case the connection is weaker because it is not by virtue of being 'Jones' that the x is insane, while in the latter insanity is attributed to x because x is the writer of the letter.

These subtle, but also logically vague, distinctions can only be adequately established, if at all, by modal logic. In fact, it was considerations like the above which led C.I. Lewis to the development of modal logic in this century. Specifically, the thinkers who felt that material implication contained paradoxes, believed that the ordinary use of conditional phrases appealed

to a stronger connection between antecedent and consequent which the extensional material implication could not capture. Put in another way, judging a conditional statement to be 'true' depended on more than the falsity of the antecedent or the truth of the consequent. The problem is where does this 'more' belong. One suggestion would be to say that it does not belong to logic as a theory of truth but only to "reasonableness." In other words, "crazy" conditional statements maybe 'unreasonable', yet the question of their truth is exhausted by their conditional form. The other suggestion would be to establish such a strong conditional for necessary implication. Such a conditional could not utilize any of the existing truth-values (and still be simple), so it had to be expressed by the introduction of modalities: "necessarily if p then q" which became in logical notation

$$(29) p \rightarrow q =_{df} \Box(p \supset q).$$

Concerning the truth values associated with the necessity operator ' $\Box$ ' (or 'L' in other notations), it is clear that it cannot be an extensionally defined operator. While the truth of "necessarily p" implies the truth of "p", the truth of "p" does not imply either the truth or the falsity of "necessarily p". With the prospect of getting involved in a morass of intensionality, was it a reasonable demand to develop a modal logic? There is a historical and a methodological answer to the problem.

Historically modalities were introduced by Aristotle in two ways. A formal way, contained in the De Interpretatione, and the

Prior Analytics, introduced modalities as they apply de dicto to whole statements, and have to do properly with connections between the logical truth of certain truth-functional constructions. The philosophic import of such considerations are evident in considering the error of deriving "necessarily p or necessarily not-p" from "necessarily: p or not-p" in defense of, say, fatalism. A more important kind of modality, which Aristotle did not thoroughly investigate in his logic, was the de re modality. De re modalities do not apply to whole sentences and thus cannot be taken as meta-talk or talk-about-talk (de dicto), but they are used to characterize properties as necessary, or contingent, or actual, or potential, or essential, or accidental. I do not intend to engage in Aristotelian scholarship here but simply to trace the fate of one of these concepts, which did or even does figure in science because of Aristotle. My aim is to find a way of deciding whether the return to modalities is a reasonable course of inquiry. Take for example the concept of potentiality or potency ("dynamis") which is still used widely in science and is clearly a disposition term.

The concept of potential was introduced originally by Aristotle as part of the distinction actuality-potentiality used in the context of explaining change. The distinction runs parallel to the other extension (covering such subsequent distinctions like competence and achievement, ability and activity, etc.). The fact that it was meant to be used in explaining developmental change placed on it two requirements that cannot be easily reconciled:

(i) Potential had to explain the very possibility of change, and in this way it was the seat of indeterminacy (pure potentiality).
 (ii) Potential had to explain the special course that the developmental change took in the various cases; in this way, even though Aristotle was not a determinist and allowed some wide parameters for change, still potential did have to become an element of determination. These contrary requirements placed on the concept will help to explain why it fell out of favor later in the history of thought.

From the 17th Century onward, potentiality suffered the fate of notions like teleology; banishment from science proper, partly because its dual nature was seen as non-explanatory: a kind of all purpose distinction applicable to anything and everything, and ultimately circular or speculative. Maybe we can understand the banishment better if we look into the parallel distinction between form and matter. Matter is replaced with mass and ultimately, the atomistic hypothesis tries to eliminate the dualism of matter-form by postulating the simple atom as the form of matter that can explain all other composite forms. There are of course revivals of the concept of potential later on (as in the work of Maxwell) but it is important to note here that the revivals occur at the first steps of a new sciences and always in the hope of eliminating "potential" later on. Furthermore, potential is always introduced by appeal to the actual: by what activities can we establish the existence and nature of potential (consider here the definition

of a magnetic field). Parallel to this banishment from science, there is a corresponding banishment from philosophy proper. Hume's work effectively removes modalities from the realm of the real world (Hume cannot see necessity or causality in the world).

There is no denying that this flight from the potential helped many sciences to develop, in the same way that the flight from teleology did. What potential was called to explain in the case of physiology or genetics can be explained by appeal to molecular structure. Compared to this reductionistic explanation (one actuality explaining the other), potential appears like a fudge factor used to replace our ignorance with a fancy term.

Of course potential was maintained in matters human, but a hardcore reductionist would claim that it is due to our relative ignorance. At the same time however we should note that potential even in human matters does not enjoy its past status. People have often opposed the measurements of such potentials as intelligence, on ontological grounds (partly due to the determination factor in "potential"). Also note in this context the opposition to Chomsky's concept of competence, which is meant to characterize the speaker's innate knowledge of the language (equally and genetically transmitted).

The preceeding brief history of one modal concept would lead to the conclusion that the reasonable inquirer should stay clear of modalities as counterproductive, or accept them provisionally only up until a better, extensional alternative is proposed. On

the other side of the argument however there are strong methodological reasons for looking into modality with a serious interest. Consider for example a whole class of relations which are used extensively in connected discourse and which are lost in extensional logic.

I have in mind here relations that are expressed by "because" and its relatives. While it is true that "because" is used in many contexts and signifies different things or connections in different contexts, still there is not even a treatment of it in simple cases. It can be claimed here however that 'because' cannot be dealt by any appeal to connectives, because it is a loose type of meta-connective signifying connections between premises and conclusion, or explanans and explanandum, or justificans and justificandum. This observation about the use of "because" is in my view correct, however it does not lead us out of our problem. Even if one managed to clarify in each case the specific usage of "because", still, except for the case of logical consequence, the other relations are problematic. Justification for instance has been shown by Gettier's paradox to be an imprecise notion, not susceptible to rules of logical inference alone, and possibly involving strongly intensional elements. In the case of explanation even more confusion is to be found, due to the many kinds of explanation (causal, functional, teleological, etc), together with the inadequacies of the D-N model which we have already mentioned.

Even if one dismisses functional and teleological explanations as merely classifications or as appeals to necessary conditions

[Scheffler, 1968], still there is a two-fold problem involved in explanation. The material problem surrounds the notion of 'cause'. The formal problem is itself two-fold: (a) the D-N model does not formally distinguish explanation and prediction, (b) the D-N model presupposes law-like universalizations, while it is formally unable to distinguish them from accidental universalizations. It is accepted that appeals to accidental universalizations do not constitute real explanations. Leaving aside the explanation-prediction symmetry problem, we can say that the other two problems are related.

Concerning the problem of nomic universalizations, the most convincing account is the one using counter-factual conditionals: nomic universalizations can support corresponding counterfactual conditional instances; accidental universalizations cannot. The problem with this criterion is the status of counterfactuals. Extensionally speaking they are trivially true, but extensionally they are not! The only way available to make some sense of them is by modal logic, because supposedly true counterfactuals establish strong connections between antecedent and consequent. Modal strict implication is the best candidate for the task.

Concerning causal statements even the most conservative approach that analyzes them by appeal to necessary or sufficient conditions (and thus by universalizations) faces not only the previous problem but also the problem of the singular causal ascriptions in the past. We utilize such causal statements not only in history, but in geology, biology, medicine, and we seem to know well what we are talking about. Again, even the most Humean, conservative

analysis of such statements [Danto, 1976] has to use counterfactuals as I argued in my paper "Danto on Causality."²¹

Both roads then lead to the need for an investigation of modalities. At least, with the aid of a logic of modalities, we can expect a better understanding of such matters as explanation and causality. Such an expectation is certainly rational, not only because the logic of counterfactuals will enable us to use counterfactuals with less embarrassment, but also because the ontological implications of such logic may help us uncover some ontological implications of our science, in a clear manner. Consider for example how modality de re might help us understand the distinctions between essence and accident, which certainly figures in nomic universalities. All in all then, the preliminary decision to explore modalities is certainly a rational one.

It is not within the scope of our inquiry to discuss the development of the different modal systems. Since our aim is to discuss theories of reference we will concentrate on two converging lines of inquiry: the general problem of the semantics of modal logic and the reference of singular terms in modal contexts. On the way we will introduce as much of modal logic as is required to make clear our thought on these two subjects. It should be noted at the start however that without a semantic account, the development of modal logic was extremely cumbersome and problematic. Semantics is not only a guide to the application of an axiomatic theory but also to its development, for it provides the notion

of validity that can be used metatheoretically in the development of the axiomatic systems of modal logic. One has to be able to decide given certain well-formed formulas, whether they are valid. This is needed as a way of investigating the consistency and completeness of the axiomatic system at hand. The situation was not hopeless. Frege, for example, managed to develop his axiomatic system successfully without a notion of validity. In the case of modal logic however, C.I. Lewis, Feys, von Wright, Brouwer, and the other modal logicians developed many axiomatic systems (the best known among them M, BW, S4, S5) which employed different axioms and were shown quickly to be deductively non-equivalent. The choice between them could not be based on intuition, for all agreed on formulas that were intuitively accessible (such as the laws of equipolence). On the other hand except for the question of independence, the consistency and completeness of the systems could not be decided by a purely syntactic approach for the reasons outlined above: the modal operators were known to be non-extensional. With these considerations as problematic background framework, let us proceed with our problem of the relations between reference and modality.

In discussing the problems surrounding the TTR we noted that one of its major weaknesses was the principle that names had to be considered as synonymous to some descriptions, that they were in fact descriptions-in-disguise. The significance of the principle is apparent: without it one would have to accept that names lack meanings. At the same time the weakness of the principle was equally

apparent: any asymmetry between names and descriptions would weaken the whole theory. In fact most of the objections that I outlined in the previous section sought, either directly or indirectly, to establish such asymmetries. I noted at the time that these asymmetries became more clearly pronounced when adapted to modal contexts. In the case of the referential vs. predicative use of descriptions, for instance, one could still maintain that there is no distinction since strictly speaking both uses have to be true of the object in question. In a modal context however 'the so-and-so' could easily refer to many different objects (or even to none) with no corresponding change in the predicative use.

In developing these non-modal versions of the criticisms we noted that Russell had already anticipated them and in the theorems of the Principia as well as in the discussion of the theorems he had provided for ways out of the difficulty or even for concessions to the opposition. Specifically he did draw a distinction between logically proper names (objects of acquaintance for which the question of existence could not be significantly raised) and other proper names (descriptions-in-disguise) and descriptions. In Russell's view, the asymmetry mattered formally only in the cases where there would be ambiguity of scope, and of those cases only in the case where the object that the description or the name (not logically proper name) purported to refer to did not exist. As I explained in detail, if the object in question did exist, then such ambiguity of scope did not matter and descriptions functioned logically in

exactly the same way as logically proper names. This claim is formally captured by theorems *14.18 and *14.28 of the Principia. The latter theorem, in particular, which reads

$$*14.28 \vdash E!(\lambda x)\phi x.\exists.(\lambda x)\phi x = (\lambda x)\phi x,$$

can be taken as legitimizing absolute substitutivity for descriptions as if they were names. In discussing the theorem, Russell points out its significance for thoroughgoing substitutivity of descriptions, yet a specific theorem to that effect, he recognizes, cannot be proven generally. Instead it must be proven for each specific case and he suggests that it could be done by employing theorems *14.242, *10.23, and *14.11 employed in the particular context. As I pointed out, the reason why the general theorem cannot be proven is that one will have to deal with all contexts, and in order to do this one would have to either go to second order, third order, etc. or utilize induction. The second route, of course, already goes into second order but in a least controversial way; it is clearly preferable.

Could one utilize the above procedure to deal with the modal variants of the asymmetry? Again Russell anticipated the problem: the above procedure is applicable only in the cases where the 'Ψ' - content in which the description '(\lambda x)\phi x' is embedded is truth-functional, i.e., extensional. As A. Smullyan notes, before one utilizes the general substitution theorem

$$*14.18 \vdash E!(\lambda x)\phi x.\supset: (x)\Psi x.\supset.\Psi(\lambda x)\phi x$$

one has to make sure that the 'Ψ____' context is extensional. This

provision is built into a theorem later in the chapter:

$$\begin{aligned} *14.3 \vdash \therefore p \equiv q. \supset_{pq} . f(p) \equiv f(q) : E! (\lambda x)(\phi x) : \supset : \\ f\{[(\lambda x)(\phi x)] \cdot \chi(\lambda x)(\phi x)\} . \equiv . [(\lambda x)(\phi x)] \cdot f\{\chi(\lambda x)(\phi x)\} \end{aligned}$$

The theorem comes at the foot of the *14.28 theorem as a limiting proof. Russell clearly recognizes its peculiar function as well as its peculiar nature: it is a second order theorem since, as the subscripts 'p, q' to the first material implication signal show, it quantifies over propositions. Given this peculiarity Russell decides:

In this proposition, however, the use of propositions as apparent variables involves an apparatus not required elsewhere, and we have therefore not used this proposition in subsequent proofs.

and later in the same place:

These propositions are immediate applications of the above [*14.3]. They are however independently proved because *14.3 introduces propositions as apparent variables, which we have not done elsewhere, and cannot do legitimately without the explicit introduction of the hierarchy of propositions with a reducibility - axiom such as *12.1

[1962, p. 185]

This wonderful theorem contains the limits of the TTR drawn from the inside, but with a view to the outside both in proof and in content, because, in order to establish the extensionality of the 'f ____' context in the antecedent, Russell has to quantify

over propositions and in this way move to a higher order. At the same time the limits of the application of the TTR are drawn "from the inside" since the extensionality condition is contained in the antecedent: it is a sufficient condition, not a necessary one. Put in another way, no provision is offered either way for cases in which non-extensional contexts are involved. It follows then that from a formal standpoint the TTR is adequate, unless one is to take a strong stand on the formal validity and general value of modal logic. Even if one admits that such substitutivity for descriptions does not hold in modal contexts, why not admit (by contraposition now) that the contexts in question are not extensional and leave it at that? And if we have a choice between the TTR and modalities, is it apparent that we must choose the latter over the former?

One could at this point take a strong step in support of the TTR and (in the spirit of Quine) argue that since modal logic violates sound theorems like *14.12 $\therefore E!(\lambda x)\phi x. \supset : \phi x \cdot \phi y. \supset_{xy} x = y$ then so much the worse for modal logic! There are two aspects to the modal failure of *14.12. In the first place, its failure indicates the intensionality of the modal idiom, since the theorem serves as the grounds for substitution. The reasons for its failure, on the other hand, is presummably the fact that although in the actual world the ' $(\lambda x)\phi x$ ' may exist, it does not follow that in other possible worlds the ' $(\lambda x)\phi x$ ' denotes a unique object, or any object for that matter.

At this stage appeal to "possible worlds" would only make the case worse for modalities. As with many intensional idioms, they could be considered as referentially opaque and thus unfit for proper quantification. Quantification without substitutivity is groundless. Yet even without a clear account of "possible worlds" it is clear that modalities cannot be dismissed easily and this is not due only to their general importance in matters of philosophy of science. Among intensional contexts all of which involve some referential opacity, the modal contexts are the least opaque. Barring expressions like "it is true that..." and its synonyms which are not opaque since they allow for disquotation, the modal contexts allow for some substitutivity, and are clearly less opaque than quotation or belief or indirect speech. Consider the difference between the two substitution inferences:

- A. 1. John believes that 2 is greater than 1
 - 2. 2 is the only even prime
 - 3. John believes that the only even prime is greater than 1
- B. 4. Necessarily 2 is greater than 1
 - 5. 2 is the only even prime
 - 6. Necessarily the only even prime is greater than 1.

The success of the second inference, as we will see later, cannot be taken as a general indication that modalities are free from opacity. It does however show that modalities cannot be eliminated as thoroughly opaque, nor can they be easily analyzed away as standing elliptically for beliefs which, in the speakers mind, justify the

statement that is called necessary. Had the latter been the case, we should expect a thorough referential opacity in the manner of inference A. In fact, the discriminating way in which modal contexts allow for substitution (call it "discriminative opacity" or "discriminative transparency") allows us to distinguish different ways in which identities can be established between different terms. Consider for instance the inference

- C. 1. Necessarily $(9 > 7)$
 - 2. 9 = the number of planets
 - 3. Necessarily the number of planets is greater than 7.

This inference which is similar to B, is indeed unsuccessful. The difference is due to the two characterizations. In the first case '2' is characterized as 'the only prime' in the second '9' is characterized as 'the number of planets'. Some obvious further examples will help us establish that modal contexts allow for substitutivity of names and some descriptions, while they disallow substitutivity of some other descriptions. The latter are the ones that are derived from contingent matters-of-fact truths (that could have been otherwise).

There are connections between the above observation and the distinction between referential and attributive use of descriptions but we shall examine them in due time. At this point we must return to the supporter of the TTR and his disputes against modalities. The traditionalist account here, represented by Quine, is to stand by the TTR as effective in all but the modal (and other intensional)

contexts, and to argue that even the minor advantage provided by discriminative opacity shows modal logic to be in serious conceptual trouble, either in its entirety or in its extension to quantification. Specifically, the charges are that quantified modal logic ("QML" from now) leads to Aristotelian essentialism and there are good philosophical arguments to show that essentialism is wrong or unfounded.

The non-traditionalist line, represented by Barcan-Marcus, Fitch, Smullyan, Kripke, and Parsons, among others, argues against the TTR and attempts to show that Quine's qualms with QML can be avoided. Concerning essentialism opinions diverge. Some maintain that QML is by no means committed to Aristotelian essentialism [Parsons, 1969], while others argue that essentialism is an intuitively clear and fundamentally correct theory [Kripke, 1972, 1973]. Having drawn roughly the two opposite sides on the issue we can proceed to show the problems associated with modal semantics.

Quine's opposition to modal logic is radical. He not only questions its logical correctness, but even its aims, its raison d'être, tracing them all the way to the modern rebirth of modalities. According to him, "modal logic was conceived in sin" because it rested on a use and mention confusion in the Principia. Lewis' troubles with material implication were based on the indiscriminate use of "implies" instead of "if...then..." connection in the Principia. What the modal logicians misunderstood, supposedly, was that the horseshoe sign of material implication and the connection

designated by "implies" belong to logically different categories. "Implies" must not be taken as a statement operator (making new statements out of other component statements) because it is a meta-predicate. It does not connect statements, instead it is a dyadic (or polyadic) predicate that is true of names of sentences. By introducing strict implication ' $\implies$ ', spelled out as $L(_\supset_\)$, Modal Logic introduced a semantical operator as if it were a truth functional connective. In Quine's view this amounts to turning validity into a connective.

If the above diagnosis is correct then the whole search for semantics of modalities is misconceived. It sought to find objective counterparts for modalities to be about, while none are to be found. While it is true that in ordinary discourse we talk of modalities, such as necessity or possibility or impossibility, and we even characterize certain properties or events as necessary or possible or impossible, one should not expect to find any connection or any property with the said characteristics. Necessity and the other modalities are semantic properties of sentences. In short, nothing is necessary; sentences sometimes follow necessarily from other sentences. Seen in this light, the best explicans of necessity is validity, and the best way to avoid confusion is to be reminded of the fact that 'necessary' normally is an elliptical locution designating a relation between sentences. By making clear the relata and the relation we can always eliminate the problematic

modal term; it disappears upon analysis like all good semantic terms.

This general view of Quine outlined in many of his writings [1953, 1961, 1962] is certainly in agreement with the traditional positivistic philosophy of science. Both Carnap and Nagel explain the necessity associated with laws of nature along lines similar to Quine's. [Nagel 1961, p. 52-56, Carnap, Gardner ed. 1966: p. 214, Carnap 1960]. Even Toulmin, who is not sympathetic to the traditionalist philosophy of science, still analyzed the necessity of natural law by appeal to the "must" involved in inference [1960, p. 92-94]. There is a clear tendency, derived either from the letter or the spirit of Quine's analysis, to analyze modalities as de dicto not de re, as semantic properties (about linguistic objects not other objects).

In accordance with his analysis of modality and with his general views on referential opacity and transparency, Quine in the article "Three Grades of Modal Involvement" outlines three ways of commitment to modalities and discusses their relative merits, with concrete suggestions at the end concerning the acceptability of such commitments. Quine is willing to accept necessity and the other modalities that are associated with it by the laws of equipolence, as long as they are considered as semantic predicates. This type of modality presents no problems (logical or ontological) as long as we keep clear the language-metalanguage distinction and place the sentences, to which these modal operators apply, within quotation marks, as

they should be. The new sentence belongs in the metalanguage and has to use the name of the sentences in question not the sentences (or statements) themselves.

There is nothing in principle wrong with this conservative strategy. If we can do with less, we should do with less, especially when the alternative sends us into the questionable metaphysical realms of possible worlds. It is important however that this conservative account of modality be capable of handling all ordinary discourse involving modalities, or at least as much of modal talk as is utilized in contexts that are of clear interest to us: nomic universality, counterfactuals, causality. Such de dicto (metatalk) modality, however, is insufficient to handle the task. The reason is that we need to speak of iterated modalities, that is to say of modalities that apply significantly to other modalities, or modalities in modal contexts. As I argued in my paper on Danto's causality theory, what we need is ways of speaking about what would have been the case, had some other condition not been obtained, or had some law of nature not been the case, or had some preventive agent operated instead. This talk is common in scientific discourse, and is taken as significant. If we are allowed some latitude of expression we can say that descriptions of the actual, now and in the past, do not exhaust what we mean by scientific knowledge; we need to know the possible and even the possibly possible. In Aristotelian talk: not only why the object is the way it is but also why it could not have been otherwise. Quine's conservative

approach cannot allow for iterated modalities since according to his analysis, it follows that what comes after the modal operator is to be enclosed in quotation marks or to be expressed by some such naming device. There can be no iterated modalities because the second modal operator will always have to be enclosed in quotation marks, and whatever is within quotes can have no logically significant structure; it is only a name. By the same token, it can have no logical relations with elements outside the quotation marks.

What is needed here is an account of modality that utilizes modal operators as statement operators. These operators generate statements out of other statements (of the same language however) in much the same way that logical connectives generate further statements by the use of formation rules. Such a modal system would contain rules of formation of such statements together with rules of transformation that can be used to establish implication relations between the various modal statements. With no clear sense of validity available, the only procedure available is then the axiomatic method. The system of Brouwer (BW) and the two systems of C.I. Lewis S4 and S5 are built on the initial system T of Feys (which contains no iterated modalities) by introducing precisely axioms of iterated modalities. When one considers these additional axioms, one is struck by their distance from intuitions;²² yet Prior was able to point to an ancient interpretation (of Diodoros

Kronos) which makes good sense by using temporal distinctions plus the locution 'always true' [1967].

Quine objects to the expansion of modalities in the way indicated above, because if modal operators are now taken as statement operators, the road back to explaining them away as semantic predicates is forever closed and with it the way to a conservative approach to modality. The argument is not difficult to furnish; connectives are syncategorematic terms, they cannot be eliminated except by replacement with other connectives, their ultimate definition is implicit. Modal connectives cannot be explained by paraphrase of statements into statements that contain only truth-functional connectives, because as I showed earlier modal operators are not truth-functional. Some modality therefore will always remain as primitive. In the case of BW, S4, and S5, moreover, the above conclusion is clear already in the additional axioms each of them employs. Consider for example the axiom of S5 ' $\Diamond p \supset \Box \Diamond p$ ' (roughly: if p is possible then it is necessarily so). Since we know that one of the intuitively clear modalities is that p does not imply necessarily p, we could not take the above schema substitutionally as an instance of ' $\phi \supset \Box \phi$ ' with ' ϕ ' standing for ' $\Diamond p$ '. Modalities in other words are here to stay according to S5.

Despite his general disagreement with this second level of modal involvement, Quine, in fairness, admits the existence of a logically redeeming feature to this extension of modalities. He shows by formal proof that for any statement 'p' nested inside

another statement 'F', if p is truth-functional then it continues to be so even in $F(p)$, just in case ' $F()$ ' is not referentially opaque [1953, in 1966, pp. 161-2]. This proof has immediate significance for the extension to iterated modalities. By a conservative reading, judgment on the value of the extension is withheld until it can be decided whether modalities are opaque or transparent and in what way. By a sympathetic-to-modality reading, we have already argued for the controlled, discriminative transparency of modal contexts. The extension, therefore, to iterated modalities is initially acceptable.

If the above proof is correct then what is wrong with the extension of Modal Logic to iterated modalities? The argument that it lacks clear semantics is weakened by the proof; at least as far as statements go, the assurance of truth functionality in modal contexts is as much as we need. There are however two arguments against iterated modalities here. The first argument is that, as we noted earlier, one has to examine modal contexts for transparency of reference and a non-circular theory is needed to distinguish the substitutable from the non-substitutable (inside the modal contexts) expressions. In the inferences B and C we appealed to intuitions to uncover two expressions that function differently, as far as reference goes, in the modal contexts. We said vaguely that the kind that failed substitutivity were descriptions derived from matter-of-fact truths. This is surely an unclear characterization. A theory of reference is needed here,

because appeal to intuitions is useless in cases where iterated modalities are involved.

The second argument, Quine's, is that if we allow the modal operators to operate as statement operators then there is no in principle reason why we should not turn them into sentence operators capable of standing before open and closed sentences alike. The argument is sound because if modal operators are taken as connectives, then there can be no justification of their restriction by any formation rule. At this point modalities are mixed with quantifiers which in Quine's view would be a grievous fault since we require of quantifiers to quantify into a modal statement. An even more serious step is already contained in the first: if we accept the construction ' $(\exists x)\Box K$ ', even if it is the innocent ' $(\exists x)\Box(x = x)$ ', then the road to essentialism is open, for, by substitution, we have now properties that are essentially attributed and others that are only contingently attributed and we do not have a clear idea of what these "essences" are.

I provided this detailed exposition of Quine's thought in order to show that Quine's opposition does not stem from his advertized blanket disagreement with "deviant" logics [1970, pp. 80-94], but rather from his opposition to Quantified Modal Logic. In his view acceptance of modalities leads to QLM and thus to essentialism. Since this is the basis of his disagreement, we have to investigate Quine's qualms with QML and essentialism. Scattered in his many writings on or around the topic of modality and intension,

the following five arguments can be taken as the strongest and cleanest items in his arsenal; any honest proponent of modalities must come to terms with them.

Argument I:

QML violates Leibniz's law of Indiscernibility of Identicals:

$$(x)(y) [(x = y) \supset (P) (Px \equiv Py)]$$

This can be shown by the following two unsuccessful inferences:

7. Necessarily ($9 > 7$)

8. $9 =$ the number of planets

9. Necessarily (the number of planets > 7).

Using Leibniz's law, and putting equals for equals, we reached from true premises a false conclusion. Again

10. Necessarily the Morning Star is the Morning Star

11. The Morning Star = the Evening Star

12. Necessarily the Morning Star is the Evening Star.

In both cases Leibniz's Law is violated because the two identicals do not have the same properties. There is a property which is true of the one side of the identity (necessary identity to the morning star) but false of the other.

Argument II:

The QML violates elementary quantification theory laws. There can be no adequate account of quantification theory without an adequate account of substitution, because without substitution

the EG and UI rules fail. In the case of QML we should be able to infer

$$(13) (\exists x)\Box(x > 7)$$

from (7). But to do so we must take (7) as saying something about an object, irrespective of the way it is described. In this case the object is the number 9, but the move to (13) is unwarranted because the predicate ' $\Box(- > 7)$ ' is true not of number 9 as an object simpliciter, but of number 9 only if described as a numeral, and false if described as 'the number of planets'.

Argument III:

The QML courts analyticity.

One way to respond to the previous argument is to admit that necessary statements or predicates do not denote objects but objects-as-described. In other words, necessity is not about objects but about concepts. It follows from this that there are necessary truths that hold not of 9 but of 'being number 9' as there are necessary truths that hold of 'being the number of planets'. While objects can generate confusion in substitution, no such confusion arises for concepts. There is no connection conceptually between 'being number 9' and 'being the number of planets'. The well-known connection is accidental and no part of the concepts involved. Consider the case in which another planet was discovered: there would be no change in either concept. This move, favored by Church and Carnap moves into full intensionality and is surely subject

to Quine's critique of analyticity. In fact, it is in cases like this where Quine's critique of analyticity makes most sense. On the other hand the benefits of this move for QML are questionable. In the first place without identity conditions for concepts, their use in quantification is dubious. Secondly if all necessary truths turn out to be analytic and vice versa, then why complicate an already confused picture? We could eliminate necessity altogether (and with it all the modal properties). Thirdly, the move towards analyticity would rob modal logic of one of its chief aims: to account for causality and nomic universality. Finally, if one bypasses the move towards analyticity and simply insists that some ways of describing an object maintain the truth of its modal properties through quantification then we fall into the hands of the next argument of Quine.

Argument IV:

QML leads to Aristotelian essentialism

Consider our previous example of the number 9 which is also the number of planets. The modal predicate ' $\Box(9 > 7)$ ' is true of the number 9, if the latter is described as the numeral 9 (presumably its proper name) or if it is described by some description like

$$(14) x = \sqrt{x} + \sqrt{x} + \sqrt{x} \neq \sqrt{x}$$

but it is not true of it if it is described by a description like

$$(15) x \text{ is the number of Planets.}$$

Given that both descriptions are true of 9 then this implies that of the various descriptions of an object some are privileged, (for example (14) above), while others are not so ((15) above), and moreover, that this distinction is not established by appeal to truth. This leads to essentialism for it is maintained here that of the various actual properties of an object some are essential to it while others are accidental. The trouble with essentialism is that it purports to establish a real distinction which has no correspondent in actuality. Instead Quine finds that modalities depend on the way objects are described, not on the way objects are. If we describe Smith as a 'mathematician', then he is essentially rational and accidentally biped; if we describe him as a 'cyclist' then he is essentially a biped and accidentally rational. Given that Smith is both a mathematician and a cyclist, what is his essence and what are his accidents? In other words, Quine explains away modalities as de dicto not de re.

Argument V:

QML leads to the conclusion that all identities are necessary. This is the clearest and most serious objection against the QML. One could conceivably bypass even the charges of essentialism, perhaps by appeal to possible worlds or what is conceivable or inconceivable. In this case however, we have an outright proof of a theorem that is considered false. The proof starts with Leibniz's Law in a first-order formulation (which is acceptable).

$$(15) (x)(y) (x = y \supset Fx \equiv Fy)$$

By substituting ' $\Box(x =)$ ' for 'F' we obtain

$$(16) (x)(y) [x = y \supset \Box(x = x) \equiv \Box(x = y)]$$

From the above schema we can drop ' $\Box(x = x)$ ' as true, because of the equivalence ' $p \equiv T. \equiv .p$ ', and because if anything is necessary, self-identity has to be it.

We therefore conclude that:

$$(17) (x)(y) [x = y \supset \Box(x = y)]$$

In other words all identities are necessary. (17) however is considered false, presumably because there are contingent identity statements. Take for example the identity 'The morning star = the evening star'. This identity is contingent because (i) it was discovered (by empirical means) and presumably logically necessary matters are not discoverable, and (ii) it is conceivable that it could be false: we may discover that in fact they are two different objects.

This argument is important not only from a logical point of view but from a philosophy-of-science standpoint as well. As we said earlier one of the main motivations behind the development of modal logic is the need to account for the necessity of natural laws. Quine's argument was adopted by philosophers of science like Carnap and Nagel in their treatments of necessity in nature. The necessity that we attribute to the laws of nature must be construed as a de dicto necessity, not as a de re one, because it always involves reference to what we know to be the case, not what

is the case. To attribute necessity to nature runs counter to the well-established truth of fallibilism. We may discover that our laws are false. Even scientific identifications like 'Water = H_2O ' cannot be considered as necessary because in the first place they were discovered and in the second place they may turn out to be false.

To the above criticisms of the QML, by Quine, we can add criticisms of the use of QML in causal contexts, which, as I mentioned, was the other main motivation for logically exploring modalities. Even though these criticisms are nothing new (they are adaptations of Quine's arguments) still they are significant for they bring out vividly the problem of transworld identification. The best example here is D. Føllesdal's adaptation of Argument III for causal contexts by substituting ' $\Box$ ' for the ' $\Box$ ' operator. Consider a causal necessity statement of the form

- (18) It is causally necessary that the man who drank from this well got poisoned.

Then consider the contingent identity

- (19) The man who drank from this well is the man who was born in p at t.

It follows by substitution from (18) and (19) that

- (20) $\Box$ (The man who was born in p at t got poisoned)
surely an unacceptable result leading to the equally unacceptable

- (21) $\Box x$ $\Box$ (x got poisoned).

Føllesdal concludes from this argument that substitution in causal uses of the QML is impossible because it is possible to describe an object in ways that are not causally equivalent. If on the other hand one insists on the substitutability of causally equivalent descriptions alone, then Quine's old argument (adapted by Davidson in note 1 of the first chapter) can be used to show that the restriction will lead to the obliteration of all modal distinctions. The proof shows that for any true sentence 'p' and for two causally equivalent descriptions, we can prove that ' $\Box$ p'. This proof (that can be found anthologized by Linsky [1971, pp. 54-55]) rests heavily on extensional logic.

Hintikka has tried to avoid the difficulty by stipulating that substitutivity of identicals be disallowed for causal contexts. The problem with this suggestion is that if the substitutivity of identicals is forbidden, then identity loses its sense: what could identity mean if not substitutivity? Føllesdal argues further, in the spirit of Quine, that the interpretation of quantifiers requires unrestricted substitutivity of identicals. Consider (21) above which is of the form ' $(\exists x) \Box Fx$ '. It stipulates that there is an object, call it 'K', of which it is true in all causally possible worlds that it is F. When we substitute identity for 'F', we reach the previous conclusion that there is no identity that is true in the actual world W_0 and false in other possible worlds $W_1, W_2, W_3, \dots$. The grounds supporting this claim is the claim that, by definition, the schema ' $(\exists x) \Box Fx$ ' talks about

an object that we can identify in all possible worlds. If any relations then, identity has to hold for all possible worlds. Identity and quantification go hand-in-hand. We are faced thus with our old problem, that identities, if true, must be necessary and a new problem, of how to identify objects across possible worlds.

There have been general as well as specific responses to the above arguments. Ruth Barcan-Marcus, against whom Quine's arguments were initially directed, responded primarily not to the specifics of the arguments but to the general presuppositions lying behind them. Her general approach was to attack the use of extensionality as a measuring stick by which all intensional logics are judged and found to be lacking. She notices first that there is no such thing as an unambiguous thesis of extensionality, even though there may be some core agreement among theorists. Instead of an unambiguous theory of extensionality, we have various principles of extensionality, some of them stronger, others weaker, some disallowing all forms of modalities, others allowing some such forms while excluding others.

Marcus's approach certainly deserves notice even apart from the problem of establishing modalities, because it affords us insights into tough logical and semantic notions. Her theory is that extensionality and the related notions of intensionality, substitutivity, equivalence, and depending on how one sees it, even identity, are not all-or-nothing kinds of properties but instead admit of degrees. This is not to say that they are vague predicates, but that they

are often used without explication and thus may yield results that are open to the charges of equivocation. The various principles of extensionality are derived from equating (by definition) identity with weaker equivalence relations. In this score, a theory is purely intensional if it disallows such a weakening definition of identity. At the one extreme then we have Marcus's QML systems which treat identity without defining it, but only characterizing it as implying necessity. As we move away from this position we can equate identity with strict equivalence, material equivalence, coextensiveness, indiscernability and so on. Each equation implies certain substitutability theorems and disallows others.

The above considerations can be used to attack Quine's arguments against modalities. Concerning, for instance, contingent identity statements, Marcus holds that there cannot be any, for "what does it mean to say that an object might not have been identical to itself?" [1962, p. 284] The error of Quine, according to Marcus, lies in mistaking certain equivalences for real identities. In accordance with the above, Marcus draws a distinction in ordinary discourse between the 'is' of identity and the 'is' of attribution, and a second distinction between proper names, which are purely referential (derived from 'tagging') and descriptions. The 'morning star - evening star' paradox is not a real paradox, because the crucial identity is not a real identity, and thus, it is not necessary. It must be explained away either as some form of equivalence, or as some kind of predication. Confusion often arises because of

the fact that some descriptions have become names, as in "the Stageirite", but in such cases identities are necessary, if they are true. Thus "Aristotle is the Stageirite" is not a statement informing us as to the birthplace of Aristotle but a statement identifying an object.

Concerning Quine's substitution paradox (the 'Number of Planets' argument), again Marcus' general thesis leads to an elegant solution. In general, some theorems of substitution hold for each theory; which one, depends on the equivalence used to explicate identity for that theory. The specific theorem of substitution that Quine uses ('if $x = y$, then $\text{---}x\text{---} \equiv \text{---}y\text{---}$ ') is not a substitution theorem for axiomatic QS4. Accordingly the statement "The number of planets is 9" cannot be taken as an identity statement in QS4, since it is not necessary, and thus the expression 'the number of planets' cannot be substituted for '9' in these modal contexts. On the other hand, the identity ' $9 = 5 + 4$ ' is necessary and thus allows for substitution in other modal contexts like ' $\Box(9 > 7)$ '.

There are two loose ends in the above theory. The first has to do with gratification and its relation to identity; the second has to do with essentialism. Marcus has something to say on both topics, but in both cases the response is insufficient, or insufficiently pursued. Concerning quantification she objects to Quine's strictly objectual account, which leads to Quine's well known criterion of ontological commitment. Instead she proposes a more

ontologically neutral account of quantification, leaving it up to identity to specify 'thinghood'. This is a serious step for it dissociates quantification and identity which were traditionally associated.

With respect to essentialism, she has little to say. One can surmise, from her general position and from her account of quantification, that she does not consider essentialism to be a problem. Her insistence that purely intensional contexts keep a strong unanalyzed identity relation, could only lead one to suppose that her idea of modality is ultimately a de dicto and not a de re one. On the other hand, given her understanding of quantification as non-objectual, the question of the real properties of objects would not arise at this stage. They should arise as soon as we ask about truth, and thus about reference, but she argues, meta-theoretically, that in QML truth must be defined not absolutely (by reference to objects) but relative to models.

In his "Reply to Professor Marcus" Quine exploits the above weaknesses and launches a general attack on her general position. He notes, at the start, that the same old "use and mention" confusion runs through her various formulations of identity. Specifically, the latitude of interpretations for the various substitution and equivalence principles that she employs, is not legitimate because the various interpretations do not belong to the same semantic level. By carefully distinguishing semantic levels, not only does the ambiguity vanish (or is greatly diminished) but also some of

the formulations are shown to be obviously wrong as they require that quantifiers outside the quotation marks, govern variables inside them. This in Quine's view, as well as in most logicians' views, is patent nonsense.

Concerning the substance of Marcus' claim, that identity, equivalence, intensionality, etc. are not fixed notions but admit of degrees, Quine counters by claiming that one can have a fixed unique identity relation defined by reflexivity and substitutivity. Specifically, "any Fxy is an identity relation ($x = y$) just in case $(x)Fxx$ and $(x)(y)(Fxy \supset x = y)$." The proof of this equivalence shows that any two functions which fulfill the above condition are coextensive. His conclusion then amounts to saying that once we have quantifiers, variables and truth functions the relation of identity is uniquely fixed.

One could counter the above claim by indicating that Marcus is not willing to accept a unique account of quantification. The comparison however between the objectual quantification of Quine and the substitutional quantification of Marcus, if this indeed is her account, is bound to be in favor of Quine and to present problems for QML. As has been often pointed out against substitutional quantification, there are truths about objects that are unnameable (the real numbers, for instance) and in such cases expressions cannot be substituted for variables. The philosophical import of this observation is that while substitutional quantification can preserve some of the properties of truth functional systems

(the T-F distinction) still it loses an important element that all accounts of Truth should have: reference. This violates the absolute account of the TTT but also makes quantification difficult to understand, since in logic quantification is the idiom for reference to objects.

I described the previous argument in some detail, not in order to show Quine winning the Quine-Marcus dispute but in order to make clear the obvious problems that all QML theorists have to face: a proper theory of semantics for QML. In fact, concerning the dispute one has to admit that Quine was unfair to the letter, if not to the spirit as well, of Marcus's position. She never claimed identity to be a vague notion; instead her definition of explicitly intensional systems requires a concept of identity that is as strong as possible, since it is not to be equated or explicated by any equivalence whatsoever. Quine's attack, on the other hand, isn't entirely unfair because this concept of identity, which is characterized as necessary, remains unexplained, while in extensional systems it is equated with other equivalences. One has the right to ask whether Marcus is talking about one concept or many, and her careful use of the term 'equated' cannot help because if it means '=df' then Quine has in hand a legitimate complaint. My opinion on the matter is that no real problem of interpretation exists so long as we keep in mind two factors. (1) The strong identity for QML is different from other explications of identity for other systems in the same way that the end of a spectrum is different

from any other point inside the spectrum. (2) The account is admittedly 'strange', yet the concept of identity is so fundamental and the attempts to grasp it so painstaking that even 'strange' accounts deserve a hearing, as long as there is an accompanying formalization.

Even with a liberal interpretation of the Marcus position (which grants strong identity for QML), Quine still has a good criterion that is derived from his initial disagreements with A. Church over intensions. In a 1943 paper on Whitehead's philosophy, Quine uses the already mentioned 'number of planets' argument to show that the modal predicate 'necessarily exceeds 7' is not true of the object itself (number 9) but only of the object-as-described-in-a-certain-way. Church countered the argument by insisting that it worked only for contingent specifications of objects, which are merely coextensive. Intensional objects on the other hand, have strict identity conditions and escape the argument of Quine because "being the number of planets" and "being the number 9" are not synonymous. In his reply to Marcus, Quine returns to Church's argument and offers a proof that if one accepts intensions, then there can be an infinity of coextensive specifications for every intension admitted. Given any ' ϕx ' and any true ' p ' that does not follow logically from ' ϕx ', then assuming that ' x ' is an intension, we can say that it is not specified by ' ϕx ' alone, but also by ' $\phi x \cdot p$ '. Since ' p ' was selected randomly, there is an infinity of such specifications [1962, p. 299].

This argument closes effectively the escape-through-intension for QML. What is left open is essentialism; one has to argue specifically that 9, the number not the numeral, independently of any specification, has the property of necessarily exceeding 7. But this idea, as we explained before is unacceptable to Quine and many others (Lewis, Carnap) because it draws a distinction among the characteristics that an object has, which is not based on considerations of what is true about the object. We are back then to our problem of understanding de re modalities.

The Quine-Marcus controversy shows clearly that the problem for QML is the lack of clear semantics. Of course, semantics are a problem for all theories not only QML. The difference however is that in other theories it is the theory-of-meaning part of semantics that is in trouble, while their theory of reference (at least for the extensional theories) is relatively clear. QML on the other hand, suffers from a lack of a theory of reference; that much was obvious in Quine's objections since they rested on identity, naming, denotation, description and truth. As long as Marcus' account left reference untouched Quine's criticism persist.

Kripke's contributions to ML are important precisely because he was able to collect all these important results of ML as well as the criticism of the TTR and to combine them with an original, formally adequate, semantical theory for ML and QML. The outcome was not only important for the justification of QML; it provides new insights into the nature of reference, identity, necessity,

as well as some surprising conclusions concerning the plausibility of essentialism as a viable philosophical position.

Before we examine Kripke's account, however, we have to examine two replies to Quine's specific arguments, those of Fitch and Smullyan because some of Kripke's insights are based on their theories. Fitch's and Smullyan's approach is significant because it, unlike the Marcus approach, is a direct response to Quine's argument. They claim that Quine's arguments are based on erroneous steps in the proof. Moreover, the results of Fitch and Smullyan point to a strong distinction between names and descriptions and thus bring us back to the criticism of TTR.

In his 1949 paper "The Problem of the Morning Star and the Evening Star", Fitch notices that Quine's well known argument does not lead to unequivocal conclusions. If one takes the expressions "The Morning Star" and "The Evening Star" as names then in accordance with ML, the conclusion

$$(22) \sim \Box (\text{The Morning Star} = \text{The Evening Star})$$

is false. If on the other hand they are to be taken as descriptions then the identity statement that leads to (1)

$$(23) \Box (\text{The Morning Star} = \text{The Morning Star})$$

is to be analyzed in accordance with the Principia as

$$(24) \Box (\exists b) [(Fx \exists_x (x = b)) \cdot (b = b)]$$

which is false since it implies the falsity

$$(25) \Box (\exists b) Fb$$

In this way the paradox appears to be defeated since no consistent reading of the expressions in question leads to taking both (22) and (23) as true.

The above counter argument can be defeated however once we notice that the transition between (23) and (24) is based on taking the scope of the description as falling within the scope of the modal indicators. If we reverse the scope and interpret (23) as

$$(26) [(1x)Fx] \Box ((1x)Fx = (1x)Fx)$$

which is interpreted by PM*14.01 as

$$(27) (\exists b) [(Fx \equiv_x (x = b)) \cdot \Box(b = b)]$$

which is clearly true, since there is, as a matter of fact, a morning star (and only one) and because self-identity is necessary. Thus, under this interpretation, Quine's result stands. Or does it?

If we use the same interpretation of (22) we obtain

$$(28) [(1x)Fx, (1x)Gx] (\sim \Box((1x)Gx = (1x)Fx))$$

which by *14.03 and *14.01 Theorems of the Principia yields

$$(29) (\exists b)((Fx \equiv_x (x = b))(\exists c)(Gx \equiv_x (x = c)) \cdot \sim \Box(c = b)).$$

which by Barcan Theorem 2.32 yields

$$(30) (\exists b)((Fx \equiv_x (x = b))(\exists c)(Gx \equiv_x (x = c)) \cdot \sim (c = b))$$

which is false, since it claims that the Morning Star is different from the Evening Star.

Fitch concludes from the above that Quine's objection cannot be maintained, except if one takes the scope of the descriptions differently for (22) and (23).

Smullyan in his 1948 paper "Modality and Description", had taken a similar line with respect to Quine's other argument concerning the substitution of '9' by 'The Number of Planets' in modal contexts. Smullyan draws initially a distinction between statement

$$(31) \text{ The so and so satisfies the condition that is necessary } \\ \text{that } Fx$$

and the statement

$$(32) \text{ It is necessary that the so-and-so satisfies the condition } \\ \text{that } Fx.$$

He then proceeds to show that Quine's argument violates a basic stipulation in the Principia that in the absence of specification the scope of a description is to be taken as the narrowest possible, and thus he is led to a conclusion of type (32) while he should be led to one of type (31). Such a conclusion presents no paradox since, as a matter of fact, the Number of Planets is 9 and 9 necessarily exceeds 7.

The arguments of Fitch and Smullyan then are based on the problem of scopes of descriptions. In support of Quine one could point to Principia *14.18 that stipulates that if the object denoted by the description does exist then it has all the universal properties attributed to objects names, and to Russell's claim that in such case the question of ambiguity of scope does not arise. As we explained it is here in modal contexts that the distinction between names and descriptions is made sharp. Even if the $(1x)Fx$

exists, scope ambiguities do matter as long as modal logic brings into consideration not only what is actual but what might have been the case.

We are brought back again to the need for a clear semantics for QML. After all statements of type (31) require cross reference between the actual world and possible worlds. That was already obvious in the awkward reading of (31): "The Number of Planets x is such that it satisfies the condition that x is necessarily greater than 7," which is different from the innocuous "The Number of Planets is 9 and 9 is necessarily greater than 7."

3.4 Essentialism and the Causal Theory of Reference

Seen against the background of problems, observations and theories that I outlined in the preceding sections, Kripke's seminal work Naming and Necessity appears as hardly innovative. The pieces of the puzzle were being assembled in different parts of the table; one simply had to stand back to see that they could all be put together with a few simple moves. This judgment, of course, is one of hindsight, yet it is true at the same time that very few new ideas or arguments were contained in Naming and Necessity. By 1910, Russell had drawn and had proved the limitations of his theory from the inside. Barcan-Marcus, Fitch and Smullyan in the fifties and sixties had replied to Quine, and, in the process, they had drawn attention to the distinction between names and

descriptions, as well as to the distinction between those descriptions that are substitutable in modal contexts and those that are not. Barcan-Marcus had accepted identities as necessary and had drawn a distinction between identity and predication. Donnellan had shown the Fregean Dogma to be erroneous, had established a strong distinction between the predicative and the referential use of descriptions, and had in this way indicated the need for a new approach to reference.

Kripke's main contribution was to put together all these fragments under the general framework of a new theory of reference, or rather of the beginnings of a new theory of reference. The task was by no means simple and it couldn't have been carried out by other thinkers, because what enabled Kripke to develop the CTR were developments in the semantics of Modal Logic for which he was responsible. This earlier work of his, which in my view is his most ingenious contribution, is basic for a proper understanding of the CTR. The fact that the Semantics of Modal Logic ("SML" from now on) are not often discussed in connection with the CTR is again due to the dissociation of reference and philosophy of science. This time however, there are good reasons for the separation. Kripke himself, because of choices that he made in developing the SML failed to adequately exploit certain of its implications for the CTR, and in this manner failed to establish both the CTR properly and its positive connections with Scientific Realism. This I take to be the main task of this section. Accordingly my

approach, as always, is partly appreciative and partly critical, as my aim is to establish better foundations for the CTR and ultimately Scientific Realism. I shall begin then by outlining the major steps that enabled Kripke to construct the CTR with a view towards showing in which way they rest on Kripke's contribution to the SML. I shall then proceed to develop the latter with a view towards discussing the concept of relative possibility which is essential for the CTR. Returning to the CTR, we will have the opportunity to explain its major theses and show the way in which it replaces the TTR. This move, which, as I announced at the start, has foundational implications for Scientific Realism, will be then examined from a critical standpoint, and I shall urge that certain modifications must be effected by a clearer consideration of modalities. The result of these modifications will be a better basis for the CTR and the beginnings of a new version of Scientific Realism.

Developing the CTR out of the fragmented problems, proofs, observations, and theories required a few bold steps. In the first place, one had to abandon completely the Fregean Dogma and thus be prepared for an entirely new approach to reference. Secondly, one had to answer Quine's Argument V, in a more satisfactory way than Marcus. Specifically, one had to explain how, at one and the same time, identity statements were, if true, necessary, and also why certain other identity statements appeared contingent. A third requirement was to explain the difference between names and descriptions, but in a way that would support Fitch's and

Smullyan's claims and their subsequent distinction between necessary and contingent identities between descriptions. To do all of the above, a new theory of reference was required, and Kripke was able to set up the foundations for it, by establishing a new distinction that cut across names and descriptions. This new distinction is the by now famous distinction between rigid and non-rigid designators, which cannot be drawn except on modal grounds. What is even more important is that the distinction has to employ the concept of possible worlds. Specifically, rigid designators are expressions (names and some descriptions) that refer to the same objects in all possible worlds. The problem, of course, is now two-fold. In the first place we must make sense of the problematic concept of possible world. The second problem is to explain identity across possible worlds: in what way is the object referred to the same in different worlds. Both problems are of general significance. Kripke's SML provides a fine formal account of possible worlds. Concerning the concrete intuitive interpretation of possible worlds and the problem of transworld identity one has to return to the essentialism of Naming and Necessity and the CTR. In the case of both problems, however, Kripke's novel idea is to utilize the relation of accessibility (of one world to the other), or relative possibility. This seminal idea, which was initially proposed by S. Kanger but was elaborated and made clear by Kripke, lies at the heart of the SML, and it should (with minor modifications) lie at the heart of the CTR as well.

As I explained earlier (3.3), the major problem in the development of Modal Logic was the absence of clear semantics. Without semantics, there could be no validity to be used as a guide to the development of axiomatic modal systems. The problem is especially acute in the most interesting modal systems BW, S4 and S5 which employed iterated modalities. One could show syntactically that they were not deductively equivalent, or that one contained the other, but in the absence of clear intuitions, there were difficulties in the choice among different systems, which are formally explainable by the consideration that without a concept of validity, no interesting metatheoretical proofs and results were available to guide both our choice and our application of the systems in question.

At this point appeal to 'truth in all possible worlds' as a definiens of validity would have at best yielded the well-known tautologies but not modal truths. Kripke used the idea but in a different way. Instead of providing semantics for ML by using a model, he first defined a model-structure. A model structure is an ordered triple $\langle H, K, R \rangle$ in which H is a member of K and R is a reflexive relation on the members of K . Intuitively K is the set of possible worlds and R is the relation of relative possibility (or accessibility) which exists between worlds. The relation is explained as follows: A world H_2 is possible relative to world H_1 (or accessible from H_1) just in case every statement that is true in H_2 is possible in H_1 . The relation is obviously reflexive

because every statement that is true in a world is a fortiori possible in it. The above model structure is designated for axiomatic system M which is the simplest. For the more advanced systems relation R is specified further. In the case of S4 it is also transitive, in the case of BW it is symmetric (in addition to being reflexive), and in the S5 it becomes a full equivalence relation. Accordingly, S5 contains all the other systems deductively, while M is contained in all the others.

Using the appropriate model structure for each system we can now provide semantic valuation for the formulas of the system by appeal to models. A model provides truth values for the atomic sentences in each world that is a member of K . Specifically it is a binary function $\phi(A, H)$ whose variables are atomic formulas and worlds and whose values are truth and falsity. The assignment of values to non-atomic wffs is done in the normal inductive way. Of interest to us is the modal valuation " $\phi(\Box A, H) = T$ if and only if $\phi(A, H') = T$ for every $H' \in K$ such that HRH' ." In plain words, A is necessary in world H just in case it is true in all worlds H' that are possible relative to (accessible from) H . On the basis of these valuations, validity is defined as truth in all models (for each model structure). Kripke was able to prove on the basis of this validity definition a completeness theorem that showed the valid formulas for each system to be provable in that system. A similar procedure was applied to the extensions of Modal Logic

into quantification, but there as I will explain the results are a little more controversial.

Of interest to us here is not only the metatheoretical rescue of ML but also the two related notions of relative possibility and of necessity in accordance with the model structures. The truth of modal statements is based not simply on assignment of truth values but rather on consideration of such assignments across possible worlds. There is a restriction however built into the conception of possible worlds. Not all possible worlds matter but only the worlds accessible to a world. The accessibility relation R may appear initially vague or even circular, but as it is spelled out by reflexivity, and transitivity, or symmetry, it becomes clear and is adequately spelled out by the axiomatic systems. The value of spelling out accessibility is ultimately, in my view, that it enables us to introduce time as an essential ingredient of our ontology. Of course there is not one single R relation, and eventually we have to make a choice. We have however obtained a handle over the ways in which R can vary: we even have alternative logics to describe each variation.

What we have gained then by moving from absolute possibility to relative possibility is a lot more structure. Relative possibility moreover contains absolute possibility as a limiting case. This latter case takes R as a full equivalence relation and provides semantics for $S5$. This semantic account of ML at the very least

provides a clear notion of possible worlds so that appeal to possible worlds can be less problematic for the CTR. In my view it provides a lot more but this is a contested point. The source of the difficulties is now not the possible worlds per se, but (a) the question which possible worlds, and (b) the problem of identity of objects across possible worlds. These last two problems arise in the CTR because of the key concept of rigid designators. It was thought that rigid designators require not only appeal to possible worlds but also a rather strong version of Aristotelian essentialism. In my view the SML do not only support essentialism but also provide a structure for explaining it more fully. This is clearly a point of contention. Kripke himself took the bull by the horns and in Naming and Necessity affirmed the truth of essentialism but he did not base it on the SML; he simply appealed to ordinary intuitions. Terence Parsons on the other hand [1969] argued, against Quine, that the QML is not committed to Aristotelian essentialism in any strong sense of "commitment." Parsons uses Kripke's semantics and proves that no purely essentialistic statement is a theorem for QML or for QML in conjunction with other statements that are themselves non-essential. The only assistance that QML offers to essentialism is by way of meaning; essentialistic sentences become more meaningful. But even in this case we could add to our QML the provision that no essentialistic statement be a theorem (by substitution) and this would not result in any appreciable loss let alone contradiction.

Parsons' results are limited by two considerations. In the first place he has to construct an adequate example of a purely essentialistic sentence. In his construction he limits himself to general statements, and avoids singular ones. Yet our essentialist problem is precisely generated by singular terms and their identity cross possible worlds. One may argue of course that singular terms are eliminable, in the manner outlined by Quine, in favor of general terms. To do so however, requires Russell's theory and as I have argued the theory fails for modal contexts. The failure is due both to the predicate generated out of the singular term and to the identity that the TTR employs. On the other hand, arguing from an informal standpoint, Kripke's theory of names treats many general terms as if they were singular terms (natural kind terms in particular).

The second source of error in my view is common to both Kripke, and derivatively, to Parsons. The specific model structure that they choose for QML is such that it shows one important formula, Barcan's Formula, to be not valid and thus not a theorem for QML. A different choice of model structure (Hughes and Cresswell) shows the Barcan Formula to be a theorem of QS5. Barcan's formula is not only an essentialistic modal statement but also, in my view affords a good account of essentialistic metaphysics. It can be used as an independent axiom with other modal systems (QS4, for instance) to spell out a good formal structure that is essentialistic,

and in my view, close to the spirit of the CTR. I will argue this point later, for now we must return to the CTR.

The fundamental concept of the CTR is that of a rigid (as opposed to non-rigid) designator. Rigid designators include names and some descriptions and refer to the same objects in all possible worlds. It goes without saying that the descriptions that are rigid are the ones that are based on necessary characteristics (essential characteristics) of objects. Rigid designators are important to the CTR because they are the primary vehicles of reference. This is a first step in the explanation of the purely referential aspect of linguistic use which Donnellan isolated. It also explains the distinction between referential and predicative use of descriptions. The latter is carried out by the non-rigid descriptions: this is why failure of predication does not imply failure of reference, which was the third of Donnellan's observations.

Barcan's points are equally vindicated. Rigid designators are substitutable in modal contexts. Kripke's theory explains this fact, since rigid designators refer to the same object in all relatively possible worlds (relative to the actual that is). It follows from this that Quine's Argument V can be handled: indeed all identity statements are necessary; they couldn't have been otherwise. There are two problems that arise at this point. In the first place how could one discover necessary identity statements? Furthermore aren't there informative identities? In answer to the second question Kripke would admit that there are informative

identities but they fall into two kinds: contingent "pseudoidentities" and necessary essentialist identities. The former are not real identities because they do not hold between rigid designators: they are either simple predications in which a predicate is attributed to a rigid designator, or more complex predications involving non-rigid terms alone. In the latter case there are two possibilities, one involving the A-categorical form as in "Smith's murderer must be insane", and the other involving reference to a specific subject, in which case the non-rigid designator establishes reference but not by virtue of its predicative content, as in "This-fellow-who-murdered-Smith is insane."

The above is now enough as a departure from the TTR and as a defeat of the Relativistic Argument. We need however more of a positive support for Scientific Realism. Let us consider then one serious objection to the above theses, which is also connected with the informativeness of identity. The objection is that rigid designators as explanations of reference involve us in circularity. Specifically, rigid designators are defined in terms of possible worlds on the one hand, while, on the other hand, the way these referents in the other possible worlds are identified is by again rigid designators (or essential properties); but how do we know these essential properties except by appeal to possible worlds?

Kripke counters the above claim by undercutting its presuppositions in a two-fold manner. In the first place he claims objects are not identified by their characteristics: this is the Fregean

Dogma that is shown (by Donnellan and others) to be false. Secondly, the objection rests on a mistaken view of what a possible world is. It is not to be thought of as a distant planet or a parallel universe containing objects which may or may not be counterparts of objects in our worlds. Possible worlds are not discovered or discoverable; they are stipulated on the basis of the actual world and specifically on the basis of the objects of this world: what would have happened to this object (say the hand-written copy of my dissertation) had things been different (had for instance the writer left for the South Seas five months ago).

Kripke's first claim has direct import for our problem. Without the Fregean Dogma the weight of reference falls not on predication but on proper names, whatever their form, as long as they are rigid designators. The problem for us and for Kripke is two-fold. In the first place, how can these identities that establish reference be necessary and informative? Secondly, given that proper names function minimally in science how is reference achieved for scientific terms? To the first question Kripke answered by drawing a distinction between the necessary and the a priori. The two categories do not coincide; the first is a property belonging to metaphysics, the second to epistemology. There can be necessary a posteriori statements. Scientific identifications for example belong to this category. Even if a liquid closely resembling water was found on another planet it would not be called water unless it had the same underlying structure. Again, in all possible worlds,

Nixon, if he existed, would be Nixon even if in many of these possible worlds he had died as an infant. On the other hand, the characterization "the U.S. President in 1970" is non-rigid, because it does not refer to Nixon in all possible worlds. Concerning Nixon there is synthetic necessary knowledge (concerning his natural parents, his DNA structure, etc.).

Kripke's claims have intuitive force, but the circularity is not yet removed from his account. It is a definitional circularity because rigid designators are defined in terms of possible worlds and possible worlds defined in terms of "these objects." The problem runs deep here and has caused misconceptions. Some thinkers (Donnellan, for example) reduce all reference ultimately to the deictic activity that connects the namer of the object to the object. Kripke, himself, in his most recent writing has been looking into identity as a primitive relation. Both answers are not helpful. The first takes us back to Searle's objection that even ostension has to be based on some characteristic of the object. The second road is not really a solution to the problem, especially as it limits us to traditional metaphysics without any strong connections to the sciences. Of course the results are not in yet; Kripke may find new things about identity (besides its being a primitive) and R. Nozick in his forthcoming book Philosophical Explanations [1978 ms] is exploring similar avenues concerning personal identity. In both cases however, the separation between

Metaphysics and Science takes its toll. There is a better road to a solution that is closer at hand.

There are two leads here connected to two central problems. There is first of all the problem of the role that proper names play in science. The second problem is why among the various characterizations of any object some are considered necessary while others are not, and why some of these necessary identities belong to science? Kripke's Naming and Necessity has no explicit answers for either question. We can however extrapolate them from the general theory of reference that he is proposing.

According to Kripke's version of the CTR, reference is determined not by any specific identity connecting a term with its meaning, but either by necessary (real) identities or by causal means. The so-called causal means are of two kinds, either direct naming or an appeal to this direct naming by "appropriate causal chains" connecting the present user to the original naming. In either case one has to ask how reference is established, and the answer is vague on many counts. In what way is reference causal? What are the "appropriate" causal chains? When is reference to an object successful and when is it unsuccessful? The answer to the last question is presumably the subject of the previous. In the case of successful reference the present user is appropriately connected to the referent; in the case of unsuccessful reference either there is a break in the causal chain or a non-rigid designator was introduced as a rigid one. This vague explanation may help eliminate

the present King of France, Santa Claus, Pegasus, etc. but still is problematic. Consider the case of Thurber's great aunt who is looking at the charred remains of a house and is informed "Electricity did this"; the statement is accurate, uttered by a properly informed person, etc. Yet she is now affraid of outlets because "electricity is creeping into the room." This category mistake must count as a failure of reference even though all the causal chains are in order connecting her present use to the original naming (after all she needs no criteria according to Kripke). To block this case the CTR theorists have to strengthen the "appropriateness" of the causal chains. Causal chains must not only exist but the user must intend to use the name, with the same reference in mind, as the previous link in the chain. This not only courts mentalism but also courts meanings back into the act of referring. The relativist is back to his old argument. The CTR theorist may still argue that the question "how do you know that user A and user B use the term *l* with same intention and intension?" is inappropriate yet he does owe us an account of sameness of intention or appropriateness of causal connection, because in our example Thurber's great aunt certainly intended to use the term with the same referent in mind as the user from whom she first learned it.

What the CTR theorists tried to capture by appeal to causality is this bedrock element in referring which, according to Donnellan, no description or predicate could capture. If there is always reference despite the failure of the TTR to capture it, it must

be due to factors capable of accounting for it. Two candidates presented themselves. The first is spatiotemporal identity: the idea that the object that we encounter again and again is the same object surely matters in reference. But this would cover (incompletely) only the objects of acquaintance and the original act of naming. The excess necessity in reference then is to be captured by causality. Admittedly the appeal to two especially problematic notions coupled with the appeal to mentalism will not satisfy the relativist, but against him the negative argument of Donnellan still stands. How is science connected with the above theory of reference?

In response to the first question concerning the place of naming in science, Kripke has provided us with an answer. Contrary to the TTR which analyzes the reference of singular terms by the model that it used to analyze general terms (i.e., fulfillment of intension), the CTR reverses the relation and considers some general terms, the natural kinds, as proper names. Their reference therefore is not to be determined by criteria that outline the term's intension but causally. By the term "electricity" we are referring to that same kind of thing, as the original namer who knew little about it. His relative knowledge or ignorance (or ours for that matter) does not figure in reference; it is the identity of the kind that matters and the causal connections of our linguistic community leading again and again either to the kind or to the original user of the name or to the original encounter.

The reintroduction of kinds to the ontological framework of reference, as rigid designators and as distinct from other predicates is certainly within the realistic spirit. There was already a strong connection between kinds and proper names as rigid designators: the essential properties true of the individual object across possible worlds include the 'kind' into which the object properly belongs. Also, the account of natural kind terms as proper names certainly agrees with the other realistic intuition, that theoretical terms in science are not terms of convention (i.e., one criterion terms like 'President of the U.S.') but rather terms meant to designate natural kinds and are used as such independently of how informed we are about them. This much is clear in our use of models in science; when we classify phenomenon under a kind we do not first collect all of its characteristics and match them against the characteristics of the kind, instead we do it on the basis of a few elements, expecting our phenomenon to possess the rest. What makes Kripke's account appealing is that even if the object does not exhibit these remaining characteristics, and if in fact it exhibits what M. Hesse would call negative analogies, still the phenomenon is not rejected automatically from the kind but instead a new category is generated for the phenomenon that retains much of the previous characteristics plus a new name.

This appeal to natural kind terms as referentially similar to proper names while it includes them into the realm of necessity, still does not remove either the circularity that we noted earlier

on, nor does it help us solve our second problem "why does science turn up rigid designators?" Kripke has very little to say on this matter. It is an open question whether he should. On the one hand he is advancing a controversial thesis that cannot rest on intuitions alone since these intuitions concern possible worlds. He is arguing for example that it is inconceivable that there would be a world in which an element which did not have 79 as its atomic number would be our gold. Even if intuitions support him on the matter, all that we are saying is that we define substances and we think of our definitions as necessary, calling some of their characteristics "essential." On the other hand the same question "why does science turn up rigid designators" must be read as asking for an examination of the intimate connections between science and necessity. Kripke is talking about language and reference, not about philosophy of science. Yet if our argument is correct, reference plays a key role in science that is connected to objecthood and thus objectivity. So the question must be asked.

How can we answer it? There are three steps to take. First we have to try to develop what Kripke would have answered, based on what he claims in Naming and Necessity. Depending on how satisfactory this answer will be, we will have to proceed to two subsequent considerations. In the first place one has to show, or at least to point to, aspects of the scientific method connected to essentialism. In the second place this specific kind of essentialism must be cleared out by appeal to the semantics of modal logic. In both cases our assessment is only preliminary.

In support of Kripke it should be said that he is well aware of the limitations of the CTR. He claims that it is not yet a theory of reference because, as we said, it does not eliminate reference from the explanans. What he claims for the CTR is that it presents a better picture of what takes place in reference, and this is certainly an improvement over the TTR, because, despite the latter's elegance and theory-status, it started from erroneous intuitions about referring. The CTR at least knows that what it has to account for is in fact part of the actual phenomenon of referring. This much is a repudiation of the Fregean Dogma.

According to the CTR then necessity is a fundamental component of all referring, and it applies to proper names and natural kinds alike. But what are the grounds for this necessity and what is the form that it takes? Kripke attributes necessity to proper names and descriptions of essential characteristics. The former category includes natural kinds the latter includes mathematical characterizations, scientific characterizations, and statements about origin. On what is the necessity of these entities based? Leaving aside, or searching under, ordinary intuitions about possible worlds, we can say that the only necessity that Kripke clearly admits is that associated with identity of individual objects in space and time. Indeed it is on the basis of this identity that the whole talk of possible worlds is to be understood. Possible worlds make sense only in terms of "what would have happened to these objects had things been different."

Two observations are in order here. In the first place, the departure from Russell's TTR is not as radical as was portrayed. While the CTR rejects any specific identification (between proper name and description) as the key to reference, still it accepts identity-in-general as that key. The difference is that whereas the TTR considered identity as perfectly definable by ordinary extensional means (what is uniquely true of an object defines its identity), the CTR requires that identity be defined strongly; it has to hold for modal contexts. Why so? Because Donnellan's observations defeat many of these actual world identities, but most importantly because the element of time must be brought into identity, and this is where modalities have to enter into the picture. The identities that the CTR is pointing to are meant to hold in-time and through time. This is a vague statement, because even the TTR is supposed to hold identities in time. After all, "the author of Waverly" or "the first President of the U.S." are fixed once and for all time. But what does this mean? It can be understood in two ways. One way is to take time as just another dimension in the time-space continuum and claim that all truths are 'timeless'. Another way is to focus on one such slice of the continuum, an instant world-state, and again evaluate all state-ments as true or false in that instant. Both ways would identify 'G. Washington' with the 'First President of the U.S.'. The latter way however would run into trouble, if the instant that was chosen was before 1776. But then there would be no 'first President of

the U.S.' and that world description would have to bear no judgment on the identity, or if it had to, it would correctly be false.

Kripke in my view finds both accounts inaccurate. The first, which is Quine's account, does not capture the distinction between things that are necessary (i.e., they are bound to either happen or hold for ever) and things that are contingent (that might be or might have been otherwise). The second account does not even bring time into the picture. Both approaches are contrary to intuition, and it is this intuition that generated the connections between reference and identity in the first place. Thus, an account of identity that does not include time essentially (not by means of the 'at time t_1 ' qualification that many 'rigorous' philosophers are so fond of) is bound to be inadequate in Kripke's mind.

How can one introduce time into our ontology. A second look at the identifications that were considered necessary will help. The two places where time figures in our picture of the world is causality and natural kinds. In the first case this is obvious. In the second case one has to examine what one knows when he knows a natural kind and in what way this involves time. Knowledge of natural kinds involves primarily not similarities and differences (as the empiricist thinks) but either one of two things, depending on the kind. In the case of the non organic world it involves permanence through time, that is to say, it involves structures which either remain the same or in case they change, the forces

that account for this change function in a permanent way across time. In the case of the organic world, to know a natural kind is to know its development across time. It is these elements that the term 'essential' seeks to capture. One can see then why Kripke takes reference to be not a function of any one identity but only of some identities that are necessary.

The second question is whether spatiotemporal identity of individual objects is ultimately all that we need by way of accounting for necessity. Kripke is ambivalent about this matter and on the whole his account is inadequate. On the one hand he attributes necessity to some descriptions of individual objects that are directly connected to their spatiotemporal identity. He thinks for example, that the molecular components of Nixon are an essential attribute of Nixon. Similarly for atomic numbers: 79 defines the natural kind gold across possible worlds. This would indicate a vindication for atomism on the ground of spatiotemporal identity of individual concrete objects, and it would even endorse a certain reductionism in science. It might also explain, in part, why science, our science, turns up rigid designators. One can even claim that Kripke's recent [1980] ventures point in this direction for he claims spatiotemporal identity to be a 'primitive' for our ontology. On the other hand, however, one has to take into account his other statements to the effect that identity across time is not necessarily reducible to identity of ultimate components.

Kripke then is not taking a stand as to the reductionism vs. emergentism issue, so his theory would not be a vindication of any theory of science that rested heavily on one or another of these theories. Yet by refusing to take a stand, he cannot offer an answer to the problem of why science turns up rigid designators. In the example of gold as the element which has atomic number of 79, Kripke claims that this is a necessary characterization because it is part of the nature of gold. This does not help us because it is simply an appeal to essentialism but without an explanation.

On the other hand I do not believe that taking spatiotemporal identity as primitive will help us, with or without a vague appeal to essentialism. Even though I consider identity as the key to reference and to have an objective account of the world: still, as I argued, developments in science change our specific identity statements. Identity of species was different before and after Darwin, identity of disease was different before and after Bernard, identity of a gas was different before and after Lavoisier etc. The only hold we have on identity is that it is a necessary relation. The understanding of identity therefore requires a clear understanding of essentialism, and not the other way round.

The return to primitive spatiotemporal identity is therefore a fruitless road that would merely lead us back to the history of philosophy. Identity is a crucial notion, as I have argued all along, but its explication requires that we investigate the

connections between essentialism and science. Once we choose this course of inquiry, two roads are open before us, which in my view summarize the Scientific Realism program. The first road leads to an investigation of the Scientific Method. In order to examine the connections between necessity and science, we have to have a clear understanding of the method of science, so that we can search for the specific parts of science that require an essentialist account. On this score a simple appeal to causality will not do. We need to know the particular ways in which causality enters into scientific research and explanation. The account should include not only our knowledge about causal mechanisms, but also the way causality enters into our scientific practices. It is important that in science we use causes in order to investigate causes: our investigation, as the recent advances in Quantum Mechanics have demonstrated, becomes involved in its subject as a part of it.

What does this proposal amount to philosophically? What can the philosopher do short of waiting for the scientist to carry out his investigation? The philosopher's task is two-fold. He has to investigate the method of science with an aim towards uncovering its ontological components. This task is already under way in the writings of R. Boyd which are, unfortunately, not yet published. In this search, the already existing methodology of science, developed by the Positivists, is a useful tool as a preliminary systematization that allows us to investigate further its ontological underpinnings. It is also to the credit of the

Positivists that they never sought to cover up their difficulties; if anything they brought attention to them by their tireless repeated efforts to overcome them. Consider for example Goodman's problem of projectible predicates. To the Realist this is one place where he can seek the introduction of time into the ontology of science.

Parallel to the realistic investigation of the method, and in a dialectical interaction with it, another inquiry is in order: the investigation of the history of science. The existing influential histories are often either "vehemently pre-Kuhnian" or, equally vehemently, Kuhnian. A realistic account will focus on the underlying ontological progress that is exhibited in the development of science, but this time it will not be just a historical interpretation, because these developments in ontology will have also a place in the methodology.

The proposals outlined above are programmatic, or they are works-in-progress of other realists. What we can do, from the standpoint of the philosophy of language, is to provide some clarification of problems of reference that will assist and establish the foundations for the previous task.

In our discussions of Henkin's way out of the non-categoricity argument (generated by the Löwenheim-Skolem Theorem), we claimed that identity is a key concept in science. This claim standing alone surely looks erroneous; what could we ever learn from a relation that an object has to itself and which no object fails to have. We then proceeded to argue that for the realist the

relation of identity is necessary on the one hand, and informative on the other, because advances in science affect our judgments of identity. The 'relativism' that this last claim invites is checked by the first characteristic: we have necessity as a guide to proper specifications of identity. The two characteristics together amount to essentialism, as Kripke has shown to us in Naming and Necessity. Not all identities are real identities; only the necessary ones are and they alone establish reference. Underlying this theory of reference is an ontological demand that objects be considered as objects-in-time. Essentialism is in my view such an ontology. To understand essentialism however is difficult. One way is to look into modal logic and its semantics, but this generates problems. Kripke ultimately attributes all necessity to identity through time, since the essentialistic possible worlds are stipulated on the basis of the objects of the actual world. In formal terms this concept could be captured by the idea of relative possibility, even though the formal semantics of QML do not imply essentialism.

We reach a circle at this point since possible worlds are established by reference to these actual objects of our world, while the identity of (and thus the reference to) these objects is based on essential properties. To break out of the circle we have first of all to argue that science establishes identities but in an essentialist manner (a la Kripke). The next two steps

are interconnected. In the first step we examine the connections between essentialism and science by using as a guide the previously argued claim that essentialism is central to science because as an ontology it is the only one that talks about objects-in-time. Our next consideration then is to examine the ways in which time figures in science. Among the concepts that are candidates as standing for objects-in-time two are prominent: natural kinds and causation. This is only the first step that allows us to merely take a glance at a realistic ontology.

The next step is more serious and affords a more clear view. Consider the obscurity of the essentialistic ontology. One of the basic tasks of the philosopher of language is to untangle essentialism by analysis. Before essentialism is admitted into the austere halls of the philosophy of science it has to clear (logically purify) its act. The modal axiomatic systems were always available; the question is which one is adequate for the task (as we have already decided on their completeness). This question cannot be answered by formal semantics alone but by an informal account of semantics; after all our aim is to use the modal system (which captures scientific essentialism) in science. We have available one concept that can be of instrumental value for our task: the concept of relative possibility or accessibility. The initial plausibility of the R relation as an explicans of essentialism is based on Kripke's idea that possible worlds are stipulated rela-

tive to the objects of our world. This informal idea however is not utilized properly in Naming and Necessity and has no correspondent in the "Semantical Considerations on Modal Logic." The trouble in the first case is that Kripke does not consider physical possibility as the key to his essentialism but only metaphysical possibility. In his view then, while objects are the determinants of possible worlds, laws of nature are not. He claims that in no possible world gold can have anything but 79 as atomic number (and still be gold) and he attributes it to the 'nature' of the substance. At the same time however he claims that it is conceivable that in these other possible worlds the laws of nature might be different. This difference implies a sharp separation between science and metaphysics because obviously the necessity of the first statement is due not to the laws of nature but to the laws of substance: i.e., the referential relation that establishes that this object here is gold, and it has to 'somehow' remain the same no matter how different the world might be.

In the second case the relation R is left purposefully unspecified. By specifying it as reflexive, or symmetric, or transitive we could obtain the appropriate semantics for M, BW, S4, and S5. But how are we to understand relative possibility? Intuitively the idea is appealing because it puts limits on the possible worlds and orders them in ways that match our experience of the world. Intuitively we want to say that the world in which Socrates is married to Xanthippe is not possible relative to the world W_1 in

which Socrates dies as an infant. It is in this sense that I take essentialism to be an ontology of objects-in-time. But in the formal semantics the accessibility relation R is not meant to capture physical possibility. What then is its formal meaning? The answer to this is simple: whatever meaning it has (and it has at least four) is completely spelled out by the four axiomatic systems; so one has to take his pick.

There are two problems here. The first has to do with non-quantified modalities generally, but by extension applies to quantified modalities generally, but by extension applies to quantified modalities as well. Consider R specified as a full equivalence: semantically this would provide for the S5-validity of modal statements. The specific axiom of S5 however states that:

$$S5A: \Diamond p \supset \Box \Diamond p$$

In other words, if something is possible, then it is necessarily possible. To justify such an axiom we could say that 'possible' means 'true in some (possible) world', therefore if p is possible (i.e., true in some world) then in all worlds it is possible (by virtue of being true in that one world); in other words, it is necessarily possible. But this move eliminates all the significance that relative possibility has and turns it into absolute possibility. It is clear that this is not the modality that the essentialist has in mind. To see this we have to apply the reflexive-symmetric-transitive R to our example with Socrates: the world W_0 which we considered as inaccessible to the world W_1 is

now accessible. S5 therefore is not the proper modal system for the Realist, and, by extension, QS5 is equally unsuited for his essentialism.

Concerning QS5 however there is one theorem which is provable only in QS5 but not in QS4 or in other systems. This is the famous, or infamous, Barcan Formula whose significance for the Realist will become apparent.

$$BF1: \Diamond(\exists x)Fx \supset (\exists x)\Diamond Fx$$

and its equivalent

$$BF2: (x)\Box Fx \supset \Box(x)Fx.$$

The Barcan Formula is not only essentialistic in form, but it was also considered false by many thinkers. Consider the following application of BF1:

"If it is possible that there is a man who is 15 feet tall then there is a man who is possibly 15 feet tall." To many people this statement is false because the consequent requires the existence of an object with the strange qualification of 'possibly 15 feet tall'. The significance of the formula for realism will be examined shortly. Here let us concentrate on its validity.

According to Kripke's semantics, the BF is not valid, and the proofs that show it to be a theorem of QS5 are erroneous. According to other writers [A. Prior in "Modality and Quantification in S5" JSL 21, 1956, p. 60-62], it is derivable, and according to the semantics of Hughes and Cresswell [1968, p. 170 ff] it is also valid. Both the validity of BF1 and its theoremhood depend

on the interpretation of quantifiers. Contrary to Kripke, Hughes and Cresswell require that the domains of objects that the models assign to the worlds, W_0 , W_1 that stand in the $W_0 R W_1$ relation, should be related by inclusion: domain D_0 should be a subset of domain D_1 . What this tells us is that the world W_1 which is accessible to world W_0 cannot contain less members than W_0 , instead it must contain all the members of W_0 .

What is captured by this requirements in my view is an interesting conception of time or rather objects-in-time. The world in which I do not exist, is not possible relative to our actual world. The statement to the contrary, i.e., "I would not have existed had my parents never met" is not describing a relative possibility now unless one accepts QS5. Otherwise it discusses the relation between the world before I existed and another world without me. To establish this one has to eliminate from R the property of symmetry.

With these considerations in mind let us return to the Barcan Formulas. The BF1 places on us the burden that if we are going to speak of possible truths then we have to indicate in this world how they can be possible. In other words, it is not enough to claim that it is possible there are centaurs, one has to accept the burden of showing the objects that could possibly be centaurs. This does not keep centaurs out of our ontology but it makes the burden of accepting such entities costlier. One should be circumspect as to the possibilities he is entertaining.

The equivalent formula BF2 is equally fascinating. It sets limits on the possible in the following way. If one accepts that all the objects of our world have a necessary property, then it follows that in all possible worlds all objects will have that property. If we admit, for example, that everything is necessarily material, then it follows that all possible worlds contain only material objects.

Both restrictions that the BF provides are important to essentialism as a Realist would conceive it. In fact, Kripke, who takes the Barcan Formula as non valid, later, in Naming and Necessity explains the relations of identity and reference along similar lines. He claims, for example, that even if we found the fossilized remains of a horned horse, still that horse would not be our unicorn, because the latter was from the start a fictitious creature, and no amount of similarity would turn two distinct objects into one. Indeed, he claims that this is one of the errors of the possible world thinkers who did not realize that such worlds are stipulations drawn on the basis of what might have happened to these objects of our world. The Realist, on the other hand, is even stricter in his conditions, because he seeks to capture what may happen and what must happen to the objects of this world. As to what might have happened, this belongs to history in its two senses: either to the past (history as res gestae) or to our accounts of the past (history as historia rerum gestarum).

It appears then that on the basis of the formal restrictions the Realist is interested in the essentialism that is contained in QS4 augmented with the inclusion of the Barcan Formula as a separate axiom. Hughes and Cresswell have shown that the BF is independent of the QS4 and that its addition to the QS4 does not result in equivalence to the QS5. The special axiom of S4 states that

S4A: $\Box p \supset \Box \Box p$

or in equivalent form

S4A': $\Diamond \Diamond p \supset \Diamond p$

Understood informally these axioms mean that if a statement is necessary for our world, then it is necessary for all possible worlds; and equivalently something is possibly possible only if it is possible.

It should not be of much surprise to us now to find that the essentialism that is appropriate for the Realist contains a strong restrictive clause about what is physically possible or what is conceivable. He believes in the necessity of the laws of nature.

It should not also be surprising that for Realism the relation of accessibility is reflexive and transitive but not symmetrical,

because as we said the aim of the Realist is not to describe objects timelessly but objects-in-time. Behind these formal requirements then the Realist begins to recognize the somber form of time.

FOOTNOTES

CHAPTER I

¹ This type of solution and the accompanying complexity is not to be found in Popper's Theory alone. M. Bunge, for instance in his Sense and Reference introduces constructs as the proper entities referred to in science. As far as I can see, this type of solution has its source ultimately in Plato but more recently in the famous "Semantic triangle." Much of modern philosophy, Quine for one, have spent much ink combating this ontological extravagance.

² Davidson's proof proceeds as follows:
Assume that (a) logically equivalent singular terms have the same reference, and that (b) a singular term does not change its reference if a contained singular term is replaced by another one, having the same reference. The 'fact that p' corresponds to 'the fact that q' just in case p and q are equivalent. Take p to be any true sentence. The statement that p corresponds supposedly to the fact that p. But we could substitute for the second 'p' the logically equivalent $\hat{x}(x = x \cdot p) = \hat{x}(x = x)$ which in turn is equivalent to $\hat{x}(x = x \cdot q) = \hat{x}(x = x)$ q being any true sentence this is equivalent to 'the fact that q'. This shows that any fact is equivalent to any other fact, or, more alarmingly, that
'the statement p corresponds to the fact that q' with p and q as any true sentences.

Adapted from Davidson [1969, p. 3; 1967, p. 752-3]

³ The author here is speaking from experience having spent three years in search of a suitable analysis of the concept of fact, that could be used as the basis of an epistemic ontology.

⁴ There is a good critique of the Popperian verisimilitude in the British Journal of Philosophy of Science, Vol. 25 (1974). In independent articles J.H. Harris, D.W. Miller, and P. Tichy have shown that Popper's verisimilitude criterion cannot compare two theories both of which are false. This removes effectively all the non-formal significance that verisimilitude would have for Realism.

⁵ It is sufficient here to mention that the author is more in agreement with Kripke's solution to the paradoxes [1975] for two reasons. In the first place he agrees with Kripke's critique of Tarski's solution: it is not intuitively clear that when we

discuss truth or falsity in ordinary contexts we are involved in shifts from language to metalanguage. Tarski's medicine is more symptom directed than cause directed. In the second place, I find Kripke's solution which employs 'groundedness' of statements more plausible and more interesting in an epistemological way.

⁶ A more detailed account of Kant's dealings with the problem of truth is contained in my 1974 paper "Kant and the Problem of Reference."

⁷ If one universalized the second order schema into the form: '(p)[T(p) $\equiv$ p]' then he would be robbing Tarski's Theory of the great advantage that it offers in allowing us to discuss metatheory without committing ourselves to questionable entities. Alternative formulations such as (p)('p' is true $\equiv$ p) quantify inside quotations and the (p) (it is true that p $\equiv$ p) quantify into intensional idioms. The net result is again to remove one of the essential advantages that the TTT has to offer.

⁸ I am indebted to J.C. Webb for this reading of the Condition W as well as for the phrase "Tarski's Challenge."

⁹ Two caveats are in order here. One should not dismiss the above explanations as formalistic ploys: they are sensitive to the ontology involved in the concepts in question since the important characteristics that are used in the explanation are related to identity. One must also resist the opposite temptation to argue that space, time and infinity are physical concepts about which further research is required: we are not discussing here the question of physical space or the question of the finitude or infinity of the physical universe. In a similar way, Tarski's Theory is not out to reveal the Truth.

¹⁰ Borgess tells us of the story of the perfect map that covered exactly the whole area of the mapped.

¹¹ By "different" modes of reference I wish not to lead to Goodman's different ways of referring. His claim is that different symbolic systems refer differently. My claim is that within any symbolic system there are many modes of referring corresponding to different types of things referred to.

¹² "On Pragmatism" 1976, unpublished.

¹³ I have dealt with this problem extensively in my unpublished paper "Literary and Psychological Narratives as Competing Explanations of Emotions." 1976.

¹⁴ I have developed the evidence in support of this hypothesis in a research paper on Biology entitled "On the Nature of Mitochondria and Chloroplasts."

¹⁵ R. Boyd [ms. pp. 13-40] develops this point in detail.

CHAPTER II

¹ Davidson in "Truth and Meaning" in J. Davis et al., ed. p. 1-3.

² Kargopoulos "Kant and the Problem of Reference" ms.

³ The point is developed in the Preface to Electricité et Optique and repeated in Poincaré's review of Maxwell [1905, p. 210-224].

⁴ The Way Things Are p. 195, and The Logic of Modern Physics, p. 48-49.

⁵ Kant's metaphysical proofs of the existence of a unique space or time consisted of claiming that we could not conceive either the plurality of space or time or the nonexistence of them.

CHAPTER III

¹ Searle's account of Chomsky in G. Harman ed. [1974], p. 25.

² Of course it is still possible to argue that even in this case mathematical induction, a formal procedure, mirrors the psychological procedure of counting. [Poincaré, Kant.] Is there anything more to this than the claim that one cannot think of the number 5 without thinking of other numbers as well.

3 I have in mind here the strong connections between the TTR and Wittgenstein's Tractatus.

4 By Quine in [1953] and [1960].

5 Davidson's Presidential Address entitled "On the very idea of a conceptual scheme", [1973].

6 According to this approach two expressions belong to the same category just in case they are intersubstitutable without loss of grammaticality.

7 But even here a good case can be made that number 1 is not an individual object but a concept.

8 The "Scott is the author of Waverly" puzzle is an identity puzzle on the one hand (similar to Frege's), and a referential opacity puzzle (similar to Quine's) on the other.

9 I leave aside here such sciences of the particular like History.

10 Even if this happens to be the empty set.

11 Russell, as we will see did not take this radical step, but Quine did on the basis of Russell's theory.

12 Kargopoulos: "Lorenzen's Constructive Ontology" [1975, ms].

13 Kargopoulos: "Quine and Strawson on Singular Terms and Predication" [1975, ms].

14 More precisely, the question whether an x is an electron-according-to-theory-T can only be answered by appeal to x's fulfilling the criteria outlined by T for being an electron.

15 Quine's example: "if anybody contributes, I will be surprised" is distinct from "if everybody contributes, I will be surprised." Normally 'everybody' and 'anybody' function similarly as in "anybody can do this" "everybody can do this."

16 Compare the tautological "The ABC is A" with the non-tautological "The A or B or C is A."

17 I am not referring here to the rest of Wittgenstein's Theory contained in the Investigations but only to the cluster theory, to the extent that the latter can be isolated from the former.

18 Strawson, in other words, agrees with Aristotle only as to the content of his ontology, not as to the grounds.

19 I will leave aside for now the more problematic concepts of causality and spatiotemporal identity that were challenged by quantum mechanics.

20 A variant of this approach is the pragmatic choice of ontologies illustrated in the external (to the framework) questions in Carnap's "Empiricism, Semantics, and Ontology."

21 In his recent book [1973] Danto tries to account for meaning of singular causal statements by analyzing the causal idiom as a semantic predicate of sorts. The result is Humean because 'a causes b' is analyzed in terms of a conjunction of the well known constant conjunction plus the 'semantic' fact that this is an instance of a law. I argued that even this carefully evasive approach requires counterfactuals.

22 Consider for instance the BW axiom ' $p \supset \Box \Diamond p$ ' (if p is true then it is necessary that it is possible that p).

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