

INFORMATION TO USERS

This material was produced from a microfilm copy of the original document. While the most advanced technological means to photograph and reproduce this document have been used, the quality is heavily dependent upon the quality of the original submitted.

The following explanation of techniques is provided to help you understand markings or patterns which may appear on this reproduction.

1. The sign or "target" for pages apparently lacking from the document photographed is "Missing Page(s)". If it was possible to obtain the missing page(s) or section, they are spliced into the film along with adjacent pages. This may have necessitated cutting thru an image and duplicating adjacent pages to insure you complete continuity.
2. When an image on the film is obliterated with a large round black mark, it is an indication that the photographer suspected that the copy may have moved during exposure and thus cause a blurred image. You will find a good image of the page in the adjacent frame.
3. When a map, drawing or chart, etc., was part of the material being photographed the photographer followed a definite method in "sectioning" the material. It is customary to begin photoing at the upper left hand corner of a large sheet and to continue photoing from left to right in equal sections with a small overlap. If necessary, sectioning is continued again — beginning below the first row and continuing on until complete.
4. The majority of users indicate that the textual content is of greatest value, however, a somewhat higher quality reproduction could be made from "photographs" if essential to the understanding of the dissertation. Silver prints of "photographs" may be ordered at additional charge by writing the Order Department, giving the catalog number, title, author and specific pages you wish reproduced.
5. PLEASE NOTE: Some pages may have indistinct print. Filmed as received.

Xerox University Microfilms

300 North Zeeb Road
Ann Arbor, Michigan 48106

76-21,194

WEINSTEIN, Mark Leonard, 1940-
THE METAMATHEMATICS OF REDUCTION:
A PERSPECTIVE ON ONTOLOGY.

City University of New York, Ph.D., 1976
Philosophy

Xerox University Microfilms, Ann Arbor, Michigan 48106

THE METAMATHEMATICS OF REDUCTION:

A PERSPECTIVE ON ONTOLOGY

by

MARK WEINSTEIN

A dissertation submitted to the Graduate Faculty in Philosophy in partial fulfillment of the requirements for the degree of Doctor of Philosophy, The City University of New York.

1976

(i)

This manuscript has been read and accepted for the Graduate Faculty in Philosophy in satisfaction of the dissertation requirement for the degree of Doctor of Philosophy.

5-17-76

date

Andrew Rosenberg

Chairman of Examining Committee

5-17-76

date

Marshall Cohen

Executive Officer

Arnold Koslow

Charles Landesman

Peter Caws

Supervisory Committee

The City University of New York

(ii)

ACKNOWLEDGEMENTS

Acknowledgement is due the publisher, D. Reidel Publishing Co. of Dordrecht-Holland for their kind permission allowing me to reprint material found in Appendices C and D. I also wish to acknowledge Professor Arnold Koslow for granting me access to his results reproduced here in Chapter I and Appendix B, and for affording me a model of clarity and style.

Acknowledgement is also due to my children, Rebecca and Jack, who have somehow managed to survive it all.

TABLE OF CONTENTS

Acknowledgements	ii
Introduction:	
The Mind/Body Identity Theory as a Claim in Meta-Science	1
CHAPTER I:	
A Partial Rehabilitation of the Deductive: Nomological Model of Explanation and the Relation Omer-explains	10
CHAPTER II:	
An Appraisal of the Position of the Event Ontologists	46
CHAPTER III:	
The Semantic Reduction Paradigms of Quine, Suppes, and Adams	76
CHAPTER IV:	
The Syntactic Reduction Paradigms of Eberle, Tarski, Nagel, and Kemeny & Oppenheim	130
CHAPTER V:	
The Views of Paul K. Feyerabend	175
CHAPTER VI:	
A Theory of Theories and An Analysis of Ontological Reinterpretation in Meta-Science	246

APPENDIX A	297
APPENDIX B	301
APPENDIX C	306
APPENDIX D	314
BIBLIOGRAPHY	323

INTRODUCTION

The collection of principles associated with the attitude of Logical Empiricism contains at its center the notion that matters of ontology are underdetermined by empirical consideration.¹ This view, espoused by the major thinkers within the movement, had its impetus in various characteristic lines of reasoning. Not least important among these was the Carnapian view that ontological positions are meta-linguistic. Within a language or language frame existence questions are empirical² and general ontological questions trivial. Ontological decisions per se, choice of language frame, although based in some way on empirical concerns, are inherently pragmatic; in some appropriate sense of 'pragmatic'. This view is reflected in a different sense in the Quineian position that matters of ontology are tied to the total organization of linguistic and empirical factors. Ontological decisions are made in order to maximize the utility of the total set of concepts, the nonlogical-conceptual net, cast upon reality, so to speak. No ontological positions are forced by empirical considerations, rather, ontological claims are a complex function of decisions

as to centrality or peripherality of empirical discoveries. To these views was added the qualification of E. Nagel that ontological decisions are inherently complex. His view is that to say that some entity is real (or realer) is in no sense to make an unequivocal assertion. Judgments of reality are far from of a piece. Different considerations result in quite different analyses of what 'real' means in different ontological claims.³

Given the centrality of these views in the development of Logical Empiricism, it is surprising that discussion of the Mind-Body Identity Theory (henceforth MBIT), one of the most serious of the ontological problems dealt with by philosophers close to the movement, reflects little realization of the inherent complexity of the ontological vis a vis the scientific. That the MBIT was maintained on scientific grounds, scientific knowledge standing in for the empirical as its most reliably constituted manifestation, is clear from the original statements of the position. We find in Place's classic article "The thesis that consciousness is a process in the brain is put forward as a reasonable scientific hypothesis."⁴ Smart, in his original exposition, maintains "...that science is increasingly giving us a viewpoint whereby organisms are able to be seen as physico-chemical mechanisms...that everything should be explicable in terms of physics...except the occurrences of sensations seems to me to be frankly unbelievable."⁵

It will be a consequence of this study that a failure to take into account the complexities of the issues involved in scientifically based ontological decisions has rendered much of the discussion of the MBIT quite beside the point. It will turn out that the irrelevance of much of what has been said on the main issue involved in the MBIT can be traced to the fact that a key notion, unexamined but presupposed by the discussions leads to areas far removed from the normal arena of dispute. I shall take the main issue of the MBIT to be whether or not the prima facie disparate language frames of the mental and the physical are inherently diverse in ontological commitment or whether they are, ontologically, of a piece. That is, do the facts of mentalistic and physicalistic discourse commit us to mental and physical entities here construed to be diverse; or, rather, is it more appropriate to construe these divergent modes of discourse as having for their subject matter a uniform ontology, usually although not necessarily, the physical.

It is a commonplace in discussions of the MBIT that a correspondence between the mental and the physical is assumed as the basis for the empirical warrantedness of the claim to the identity between the mental and the physical.

Feigl in his introduction to a collection of essays on the MBIT puts the matter most succinctly; "Scientifically the most plausible view to date is that of a one - one (or at least a one - many) correspondence of mental states to neurophysiological process-states. The discussions,

given this assumption, then stress either conceptual issues as evidence, a priori, against the identity or some meta-theoretic consideration, usually simplicity, as sufficient (nearly) for the identity. A second set of problems in these discussions accepts the framework just mentioned, and then argues for various analyses of 'identity' or for various proposals as to the appropriate relata for the identity relation.

Without going into detail, it seems that the Carnapian view should have ruled out the a priori as irrelevant to the issue. That is to say that, although given a language frame, it makes sense to ask a priori questions, i.e. attempt to clarify the conceptual as opposed to, say, the empirical aspects of the frame, if we are to choose between frames, unless we have some large including frame, a priori considerations, that is, conceptual considerations within frames, are not relevant to making choices.⁷

It seems, further, that the attitude associated earlier with Nagel should have pointed to the fact that ontological criteria, where existing, are diverse, far from uniform, and certainly do not fall under the rubric 'simplicity'. Moreover, given the diversity and the complexity of the nomological net, it seems hardly likely that a particular notion such as correspondence would suffice where numerous shades and grades of empirical realities concerning science and scientific languages are in question.

Given the assumption of correspondence as unproblematical

the emphasis on criteria for identity is, perhaps, understandable. If, however, we take as our working assumption that the notion of correspondence is itself difficult to make precise in any uniform way, the issue is not that of identity, but rather with the conditions under which identification is warranted. We therefore return to the basic insight of the Logical Empiricist claim of ontological underdetermination. The issue is upon what sorts of evidence, whether actual or merely projected, we may identify the referents of prima facie conceptually distinct terms. More generally, upon what sorts of scientific developments we ought to allow the identification of disparate scientific frames by means of a uniform semantics. It is not that, given some single relation, correspondence, and some ontological maxim, as Occam's razor, identity is 'implied'. Our problem is this: what various states of the sciences and what difficult-to-render pragmatic decisions, warrant our identifying prima facie semantically diverse frames.

As an example of the complexities involved, some states of science warrant identification, whereas strictly analogous concerns within different conceptual settings do not. So, for example, isomorphism between laws of phenomenological thermodynamics and statistical mechanics may warrant identification of the properties of heat and mean kinetic energy. Isomorphism between laws governing the flow of electricity in a wire and the flow of water in a pipe provides no such warrant. Obviously, if differences in ontological

yield are warranted by prima facie similar situations, some more analysis of the difference between the two "isomorphisms" is required. To classify all such situations under a single rubric, for example correspondence, is just to bring together what ought to be maintained distinct.

Although the analysis of 'correspondence' is not found within the writings on the MBIT, there are discussions in the Philosophy of Science that bear immediately on these concerns. The discussions on the reduction relation between theories in the sciences constitute a major contribution to our understanding of the empirical and logical content of 'correspondence'. Another obvious source of clarification is to be found in the meta-mathematical discussions of mappings; more of that below.

As one might expect from the writings of the Logical Empiricists mentioned;⁸ meta-theoretic notions such as reduction reflect the complexity of science. We do not find a single univocal analysis of 'reduction' but rather a fair number of differing attitudes and reduction paradigms employed. Given the recent attempts⁹ to sensitize philosophers of Science to the complexity of actual scientific practice, this diversity should be the norm rather than the exception.

Our major concern is with the warrant for decisions to identify the referents (assign uniform semantics) to scientific theories as a complex function of the plausible projection from de facto inter-theoretic relations to ideal

states. That is to say, what are the structural relations between theories and what are the dynamical relations between these structures that prompt our decision to unify ontology in the name of scientific advance. As we shall see, issues are more complex than a perusal of the literature on the MBIT would have us believe. The relations between theories are multiform; the ideals towards which they tend are similarly diverse. We shall consider the different paradigms embedded in the literature on reduction. These are a fair sample of possible reduction relations. Then, and only then shall we raise the issue as to what ontological decisions are warranted and by which paradigm.

Before we can turn to the various reduction paradigms a prior problem must be considered. The meta-theory of scientific practice within which the various reduction paradigms are offered is not without serious difficulty. Further there are arguments against the meta-scientific stance associated with the theorists of reduction which are found in the very discussion of the MBIT. Our first task is then to rehabilitate the standard meta-theory of scientific practice meeting the objections that bear on our problem. Once we deal with these issues we shall turn to the problem of mapping reduction paradigms onto ontological decisions.

FOOTNOTES

Introduction

¹This is not the original view of the earliest members of the movement; including even Carnap. The original position is the rather optimistic one that metaphysical issues can be disregarded as pseudo problems. The realization that metaphysical issues are cognitive in at least some respects resulted in the view that they are heuristic principles, vague programs for research, etc.

²Clearly if the frame is one of, say, pure mathematics, existence questions are answered in non-empirical ways, for example, computationally. Notice that the issues involved may include criteria such as simplicity, naturalness, consistency with prior formalism, etc. The distinction is classical and, hopefully, clear enough to continue.

³Classical presentations of the positions mentioned are R. Carnap, "Empiricism, Semantics, and Ontology," Meaning and Necessity (Chicago: University of Chicago Press, 1956); W. V. Quine, "Two Dogmas of Empiricism", From a Logical Point of View (New York: Harcourt, Brace and World Inc., 1961), Ch. 6.

⁴U. T. Place, "Is Consciousness a Brain Process," reprinted in C. V. Borst, 1956; The Mind/Brain Identity Theory, (London: MacMillan & Co. Ltd: 1970), p. 42.

⁵Smart (1959) "Sensations and Brain Processes," reprinted in Borst, The Mind/Brain Identity Theory, pp. 53-54. J. O'Connor, Modern Materialism: Readings on Mind-Body

Identity (New York: Harcourt, Brace and World, 1969), and Borst, The Mind/Brain Identity Theory, 1970, offer a decent survey of the classic moves in the MBIT. C.F. Presley, The Identity Theory of Mind (St. Lucia, Queensland: Univ. of Queensland Press, 1967) offers a characteristic line of development. The locus classicus for the meta-scientific analysis of the 'mind-body problem' is H. Feigl, "The 'Mental' and The 'Physical'," in Feigl, Scriven and Maxwell, Minnesota Studies in the Philosophy of Science (Minneapolis: University of Minneapolis, 1958) vol. 2.

⁶Borst, Mind/Brain Identity Theory, p. 31.

⁷There are alternative views as to the nature of the metaphysical, the a priori and related matters. I do not defend the attitude I take here. I merely apply it to the problem at hand "For this kind of doctrine admits no other demonstration."

⁸I abstain from jurisdictional disputes. Quine is not usually thought of as a Logical Empiricist. I use a broad classification in order to include thinkers whose attitudes and methods seem relevantly similar and relevantly similar to my own. See P. A. Schilpp, The Philosophy of Rudolf Carnap (La Salle, Illinois: Open Court, 1963), pp. 860 ff. and pp. 915 ff. for Carnap's views as to the deep similarities between Logical Empiricism, and especially Nagel and Quine.

CHAPTER 1

The main problems of the standard meta-theory of scientific practice relevant to our concerns are of two sorts. First there are the various trivializations associated with the classic presentation of the Deductive-Nomological (D-N) model of explanation.¹ Secondly, there is the rejection of the ontological base of this model. For the second sort, specifically, what is being alluded to is the move away from the construal of the languages of science as having for their domain concrete particulars in some standard sense, to the construal of these languages as having particular variables which range over events. These events are usually thought of as n-tuples of concrete particulars, properties and space-time coordinates. This latter issue deals most directly with the problems associated with the MBIT and eventually our focus will be squarely on it.

As we shall see, however, although variously motivated, these two prongs of the attack upon the D-N model have a common locus, which is what I will refer to as the 'ill management' of the syntactic apparatus of standard formal languages, which are construed for our purposes as first order. The phrase 'ill management' is carefully chosen to suggest the inherently pragmatic solution to the dilemmas

presented.²

The various syntactic trivializations of the D-N model have been catalogued and effectively dealt with by Omer,³ a perusal of which suffices for a synopsis of the various tortuous paths the syntactic notion of explanation has taken.⁴ In our opinion Omer has come up with a set of restrictions on the syntactic apparatus that meets the standard syntactic objections to the D-N model in the literature.

Omer's restrictions are as follows: we let "T", with or without super-scripts, represent a set of sentences, $\{t_1, \dots, t_n\}$ or alternatively, the conjunction of the sentences $t_1 \wedge \dots \wedge t_n$. Which we intend, will be clear from the context. T is the explanans. We let 'e' represent a particular sentence, the explanandum. Notice, no restriction is placed on 'e' although in various contexts it will be construed as an event description or even as a law-like sentence. We use 'T_c' to refer to the set whose members are well formed formulas of the longest sequence $t_{c_1}, \dots, t_{c_r}$ of truth functional components of T⁵ or alternatively for the conjunction of the various t_{c_i} 's. In addition we require that

$$\vdash T_c \equiv T$$

Given those conventions we say; T Omer-explains e, in symbols, $T \vdash_0 e$ if and only if (1) T_c implies e

- (2) T_c does not imply not-e
- (3) For some t_{c_i} , $t_{c_i} \in T_c$, t_{c_i} is a universal law.
- (4) For any t_{c_i} , $t_{c_i} \in T_c$, t_{c_i} is non-comparable with e,
i.e. neither $t_{c_i} \vdash e$ nor $e \vdash t_{c_i}$.
- (5) It is not possible to find⁶ sentences $s_1, \dots, s_r$
($r \geq 1$) such that for some $t_{c_1}, \dots, t_{c_n}$ ($n \geq 1$)
in T_c : (i) $t_{c_1}, \dots, t_{c_n} \vdash s_1 \wedge \dots \wedge s_r$
(ii) $s_1, \dots, s_r \not\vdash t_{c_1} \wedge \dots \wedge t_{c_n}$
(iii) on replacing $t_{c_1}, \dots, t_{c_n}$ in T_c by $s_1, \dots, s_r$
notated T_{c_s} , $T_{c_s} \vdash e$.

The five conditions just stated are of extreme importance in the work to follow. They will be alluded to by use of their indices.

Omer's five conditions are diversely based. The first three are standard in the literature. (1) being, according to most thinkers, stated as early as Aristotle and is the core notion that marks the relevance of logical analysis to discussions of scientific explanation. (2) in conjunction with (1) rules out the inconsistency of T. (3) is, again, standard and demands no discussion here.

Omer's condition (4) and (5) are based on certain

principles of assertive discourse after Grice and Quine.⁷

Most generally the principles alluded to can be stated as follows: "...one should not deliberately make a less informative statement on the topic of conversational exchange when one has just as good grounds for making a more informative statement."⁸ The qualification "on the topic of conversational exchange" is a necessary one, for although the requirement is for a maximum of information, the information maximized should be relevant. That is, the information should be minimally redundant.⁹

To speak about maximizing information or minimizing redundancy is to presuppose some notion of the information content of the sentence used to furnish this information. Following Carnap and Popper,¹⁰ Omer equates information content deductive consequence. So briefly, if $p \vdash q$ then the content of p is said to be greater than the content of q . If, in addition $q \vdash p$, then the content of p is identical with the content of q . (4) can then be seen as the demand that content be maximized. The intuition is as follows. Self-explanation seems to be the uninformative extreme for any deductively based notion of explanation. So we rule out the case where t_{c_i} implies e . If, contrarywise, e implies t_{c_i} then the content of e is greater than the content

of t_{c_i} , so to assert ' $t_{c_i} \supset e$ ' is to make a weaker statement than we are entitled to make, that is, ' e ' itself. Condition (5) on the other hand demands minimum redundancy, in the sense that the explanans is as close to the explanandum as possible. Notice further that since (1) and (2), the irrelevant maximal, inconsistencies are ruled out. We take notice of the fact that given (4) e does not imply T , otherwise e would imply every t_{c_i} . That is to say that T always has some redundant information relative to e "if we say that any information more than that contained in e is redundant."¹¹

Although (1)-(5) are, as Omer carefully shows, sufficient to meet the standard trivializations of the D-N model, (5) implies "... (that) ... the long anticipated and rejected Hempelian 'minimal law' must be the fate of the D-N model of scientific explanation."¹² That is to say, that an analysis of the conditions sufficient to meet obvious trivializations of the D-N model implies that D-N style explanations have properties undesirable for an adequate account of explanation. The properties are referred to, when instantiated in a law, as the 'minimal law'.

The minimal law has been characterized by Hempel in the following fashion: "If an explanation is of the form D-N, then the laws $T_1, \dots, T_r$ involved in the explanation

logically imply T^* which would by itself suffice to explain the explanandum event by reference to the particular conditions involved in the sentences $C_1, \dots, C_k$ "¹³ Schematically we then say, if $\{T_1, \dots, T_r, C_1, \dots, C_k\} \vdash e$, then $\{(x_1, \dots, x_k) (C_1 \wedge \dots \wedge C_k \supset e), C_1, \dots, C_k\} \vdash e$. where ' $(x_1, \dots, x_k) (C_1 \wedge \dots \wedge C_k \supset e)$ ' is the universal closure of the conjunction of the members of $C_1, \dots, C_k$ and where ' $(x_1, \dots, x_k) (C_1 \wedge \dots \wedge C_k \supset e)$ ' is the minimal law, T^* .¹⁴ Note that the minimal law is just the universalization of the claim that the initial conditions imply the explanandum's occurrence. Note further that what is called the 'minimal law' is not a law but a law scheme, that is to say the presentation of the logical form of a possible law.¹⁵

The rejection of explanations based on minimal laws is forced by the need for 'explanations based on theoretic principles'.¹⁶ Such explanations are required in order to broaden and deepen our understanding by virtue of permitting explanations that explain a wide range of empirical phenomena (laws) and by virtue of showing these laws to be a manifestation of a few basic phenomena ('breadth' and 'depth' respectively). Further, the minimal law, if the only available explanans, precludes the familiar situation in science where "one and the same phenomena may be deductively subsumed under different and logically incompatible laws and theories..."¹⁷ Thus if explanations are limited to minimal laws as explanans we can not account either theoretically or descriptively for the actual practice of scientists.

Omer never states why his analysis of explanation precludes all but minimal laws from being explanans statements. In fact it is just not clear that the notion of non-redundancy in Omer is related to the notion of specificity of explanation that is at the core of Hempel's minimal law. We

now define as follows; T minimally explains e , in symbols, $T \vdash_m e$ if and only if (1) $T \cup C_1, \dots, C_k \vdash e$. (2) T is of the form $(x_1, \dots, x_k) (C_1 \wedge \dots \wedge C_k \supset e)$. Note that the construction of T is thought to be parasitic upon the existence of a set of laws $T_1, \dots, T_r$ which imply T but are not implied by it.

Theorem I: $T \vdash_o e \Rightarrow T \vdash_m e$

We prove the theorem via the limitative contrapositive;

$$\sim (T \vdash_m e) \Rightarrow \sim (5)$$

Proof; Assume $\sim (T \vdash_m e)$ then there is an T^* such that $T \vdash T^*$ and $T^* \vdash_m e$. But that is to say that there is a deduction $D_1, \dots, D_n$ of T^* from T . By compactness¹⁸ we knew that only finitely many members of T are used in this deduction. Let us enumerate them and group them together into the set T^+ . That is, (1) we form the set,

$$T^+ = \{ t : t \in T \wedge (\exists x) (x = D_1 \vee \dots \vee D_n \wedge t = x) \}$$

(2) We form the set T_C as before,¹⁹

(3) We form the set T_C^* from the set T_C in a similar fashion,

(4) Lastly, we form the set,

$$T_{CT^*} = \{ T_C - T_C \cap T^+ \} \cup T_C^*.$$

The set T_{CT^*} , as in condition (5) of the definition of $\vdash_o$, is the result of replacing elements of T_C by elements of T_C^* . Notice that this construction is parasitic upon the existence of the deduction $D_1, \dots, D_n$.²⁰

If we permit, as before, symbols that name sets to equivocally name conjunctions of the members of the same sets, it is clear that $T^* \equiv T_C^*$, but $T^* \vdash e$, whence $T_C^* \vdash e$. But T_{CT^*} is a super-set of T_C^* , so by the rule that if $X \vdash a$ then $X \cup Y \vdash a$, $T_{CT^*} \vdash e$ as well.

But recalling the construction in (4) above it is clear that $T_C \vdash T_{CT^*}$, since $T_C \vdash T^*$. This last since T^* is the

minimal law, but since T^* is minimal we may assume without loss of generality that $T^* \not\models T_c$. But that is as much as saying that Omer's condition (5) is violated. ■

We now ponder as to whether, $T \vdash_m e \Rightarrow T \vdash_o e$. Not surprisingly, the answer here is negative; however, the reason turns out to force us to reconsider what Omer means by condition (5) on pain of incoherence. The negative result just alluded to follows from the application of a standard result of model theory to Omer's analysis. The result I have in mind is Craig's Interpolation Lemma.²¹ The theorem can be stated as follows: Assume $\vdash X \supset Y$, and let $Wr(A)$ be the set of predicates in a sentence, A , and let $W(A)$ be the union of the set of predicates and the set of individual terms in A , then if $Wr(X) \cap Wr(Y) = \emptyset$, there exists a sentence Z , such that $W(Z) \subseteq W(X) \cap W(Y)$, and $\vdash X \supset Z$ and $\vdash Z \supset Y$. If $Wr(X) \cap Wr(Y) = \emptyset$ and X is consistent then $\vdash Y$. Notice, an alternative statement that sharpens its application to contingent sentences is, either $\vdash \sim X$ or $\vdash Y$. Naturally, in any plausible application of Craig's Lemma to scientific explanations it is demanded that Y is not a theorem of logic and that X is consistent (alternatively that the negation of X not be a theorem). So in our applications we demand that

$$Wr(X) \cap Wr(Y) \neq \emptyset.$$

Theorem II: $T \vdash_m e \not\Rightarrow T \vdash_o e$ ²²

We show this by proving the limitative contrapositive,

$$T \vdash_m e \Rightarrow \sim(5)$$

Proof; Assume $T \vdash_m e$, whence $T \vdash e$, but then by Craig's Lemma there is an s such that $T \vdash s$ and $s \vdash e$. But since we can form T_{C_s} , the substitution of s into T as per Theorem I, (4) we know, (1) $T \vdash T_{e_s}$, since $T \vdash s$ and $T \vdash t_{C_i}$ for all $t_{C_i} \in T$; (2) $T \vdash o$ since $s \vdash e$.

But that is to say that Omer's condition (5) is violated. ■

Theorem II is, of course, disastrous for a prima facie plausible interpretation of (5). That is, the construal of 'it is not possible to find' in (5) as 'it is not logically possible...' For since by (1) $T \vdash e$, then (1) implies not (5). Whence Omer's criteria, are under this interpretation, incoherent.²³ Obviously, we must rethink our construal of 'it is not possible to find'. Our rethinking turns out to be a clarification of the pragmatic underpinnings of Omer's work. We notice that Craig's result is not constructive, that is, although we knew the sentence 'Z' exists, we don't guarantee that we can always find it. Since logical inconsistency is no part of the principles of 'assertive discourse' we demand that (5) does not entail the impossible task of constructing explanation schemata in violation of Craig's Lemma. Rather we demand that a theorist offering a possible explanans, T , reject T as inadequate when a counter-example of the sort (5) requires, is presented. This will be enough to handle all of the standard trivialization moves in the literature, and as we shall see, some arguments for the need to posit events as particulars in the domain of discourse of scientific

languages.

If we recall the description of the trivialization moves at issue, footnote 4 and Appendix A, what we have in mind can be rather easily motivated. What we want to show is that these, characteristically philosophical, excesses can be controlled in a principled fashion. We thus demand that a minimal construal be placed on explanation schemata that guarantees relevance and non-redundancy. This is the point of a counter in the name of (5). That is to say if a theorist offering an explanation scheme is presented with an objection of the sort (5) envisions, the theorist then moves from his original scheme to another to which the objection does not apply. As we shall see this is analagous to the rejection of superfluous elements in scientific explanations when a closer explanation is found. Further, it is analogous to the demand that theoretic explanations be applied to phenomena only when generalizations consistent with both the theory and the explanandum are available. Think, for example of standard arguments against Freud that claim the theoretic explanation is vacuous since no clear generalizations, confirmed as correct, are available.²⁴

We notice that Theorem I still goes through since T^* is constructible given that some non-minimal T explains e . The details of this construction have been already presented pp. 14-16 above. There is, however, one other problem with the counter presented in the name of (5). We want to make sure

that the counter that shows an explanation scheme, $T \vdash e$, to be trivial does not offer, in itself a trivial explanation. So we assume that, $T - T \wedge \{D_1, \dots, D_n\} \vee S \vdash e$, for some counter, S , satisfies (1)-(4). That is, $T^* = \{T - T \wedge \{D_1, \dots, D_n\} \vee S\}$ is consistent, universal (has a universal sentence as a member) and that for no $t^*_{c_i} \in T^*$, $e \vdash t^*_{c_i}$ or $t^*_{c_i} \vdash e$. We call such a counter an Omer-counter and assume throughout in all subsequent theorems that counters are Omer-counters.

Hempel hasn't denied that minimal laws are explanations, he merely affirms that explanations are not limited to minimal laws. That is not the case, however, with Omer who claims that (1)-(5) are necessary and sufficient.²⁶ Since we wish to use Omer's analysis to block potential trivializations of the D-N account of explanation, we must place some further limitative condition on the explanans. The problem is to weaken Omer's criteria to block the problems associated with the minimal law without robbing the analysis of the ability to stop certain trivial and redundant schemata from counting as appropriate D-N schemata.

In order to do this we must take a closer look at the problems associated with the minimal law. Given Theorem I these are problems for us as well. Hempel's reservations about minimal laws is, as mentioned earlier based on the inadequacy of minimal laws in the context of theoretic explanation.

In general an explanation based on theoretical principles will broaden and deepen our understanding...it will achieve an increase in breadth because the theory will usually cover a wider range

of occurrences then do the empirical laws...and a theoretical explanation deepens...(our understanding in that)...it reveals the different regularities...(to be)...manifestations of a few basic laws...²⁷

We further recall that Hempel requires that, "one and the same phenomena...may be deductively subsumed under different and logically incompatible laws and theories..."²⁸ We allude to these desiderata as breadth, depth and diversity, respectively.

We now characterize the desiderata as follows. Firstly we recall the definition of ' t_e ' in footnote 20. ' t_e ' satisfies Omer's criteria (1)-(4). We list the remaining desiderata as (6), (7) and (8), in consequence of the special role (5) will play. That is, (5), under my pragmatic interpretation requires an explanation to be formed in response to all claims of redundancy and irrelevancy by applying the construction in Theorem I(4), that is, all redundant elements of the proffered explanans have been systematically excluded from any role in the explanation.

We say that an explanation relation, t_e , is broad if and only if there are sentences e, e' such that:
 (6) $\langle T, e \rangle t_e$ and $\langle T, e' \rangle t_e$ and where e and e' are logically independent. The rationale behind the characterization is, briefly, that if e and e' cover diverse sets of phenomena ²⁹ T will cover these as well. Notice a fairly natural extension of (6) is to define a breadth measure in terms of the cardinality of the sets of tuples of the sort contained in t_e . To compare T and T' in respect of breadth we take

the cardinality of the set, $E = \{ e: \langle T, e \rangle \in t_e \}$ as opposed to the set $E' = \{ e': \langle T', e' \rangle \in t_e \}$, and where the members of E and E' are logically independent.

An obvious analogue for 'diverse' is easily constructed. We say that an explanation relation, t_e , is diverse if and only if there T and T' such that:

(7) $\langle T, e \rangle \in t_e$ and $\langle T', e' \rangle \in t_e$ and where T and T' are logically independent, (whence, perhaps, logically incomparable or even inconsistent, as well). A measure analogous to the one generated for breadth is readily constructible.

Depth is more of a problem. A simple notion like transitivity for t_e would be disastrous since t_e is anti-transitive, that is, if $T t_e S$ and $S t_e e$ then not $T t_e e$ by (5). This problem will be returned to shortly, but briefly, we will try to generate a relation $t_{e'}$ such that $T t_e S$ and $S t_e e$ implies that $T t_{e'} e$. The cogency of our view will be in the closeness of $t_{e'}$ to t_e especially in terms of the ability of $t_{e'}$ to block in a principled fashion the kinds of irrelevanta found in the literature on the D-N model of explanation.³¹

The key to our solution to the problems associated with minimal laws will be the pragmatic interpretation of (5). Under that interpretation for any pair $\langle T, e \rangle \in t_e$, for some explanation relation, t_e , there are only finitely many actual counters in the name of (5), although, of course there may be infinitely many potential (logically possible) counters.³² Given the finitude of the actual counters

offered we conjoin them and merely have to deal with one particular counter. That is to say, in the proofs to follow when we deal with counters we deal with, at most, one.

Understanding this much we now add to the characterization of an Omer-counter³³ the following. If S is an Omer-counter to the claim $T \vdash e$, S is the conjunction of all proffered counters presented in the name of (5). Further $T \vdash S$ and $T_{C_S} \vdash e$ where T_{C_S} is the substitution of S into T as in Theorem I, (4). We now show the following in preparation for our proofs that $\vdash$ is broad, diverse and lends itself to the construction of a relation, $\vdash'$, that is deep.

Lemma I: If $T \vdash e$ then there is a set T' such that $T' \vdash e$ and $Cn(T') \subseteq Cn(T)$.

Proof; By the definition of $\vdash$,³⁴ conditions (1) through (4) of Omer's construction are satisfied. So all we have to show is that (5) in its pragmatized version is satisfied by some subset T' of T . That is, we show that given an explanation of e by T , we can find a subset T' of T that explains e and is free from known redundancies and irrelevancies.

By the pragmatic version of (5) we are dealing with actual counters rather than with the class of logically possible counters. We are thus assured that the number of counters is finite. We form the list, $s_1, \dots, s_k$ of actual counters offered against the claim that $T \vdash e$.

We then construct T' according to the procedures outlined in Theorem I (1)-(4), i.e. $T' = \{T - T \cap T + \} \cup S_C$,

where T^+ , as before, is the set of members of T used in some deduction³⁵ of S_c from T (Notice for ease of exposition we say ' T ' rather than ' T_c ') and where S_c is the largest set of truth functional components of $s_1, \dots, s_k$.

But $s_1, \dots, s_k$ is the set of Omer counters so T' satisfies (5) in respect of e , i.e. $T' \vdash e$. But since $T \vdash S_c$ by construction $T' \subseteq T$. Note that if there are some counters then $T \cap T^+ \neq \emptyset$, whence $T' \subset T$ ■

In the theorems to follow we will alter the statement of depth, breadth and diversity in the name of clarity of presentation. In order to do this we will offer the notion of a theory T closed under Omer-explanation. That is to say we will speak of the set $E = \{ e: T \vdash e \}$. We will speak of T as the generator of the set $T \cup E$, just when the members of E are the Omer-consequences of T . The set $T \cup E$ will be characterized as an OES (Omer explanation set). This will enable us to state breadth as the condition that obtains when the subset E of an OES has, at least, two members, e and e' that are logically independent. For diversity we show that there are logically independent theories T and T' where T and T' generate OES's with identical E 's. For depth we show that if we have two OES's where there is some member in the product of the consequences of the first and the generator of the second we can pick out a favored set that links the two OES's in a way that controls the ability to 'pack' in irrelevancies or redundancies. OES's in the favored relation will be said to

form 'Omer-chains'.

Theorem III: If $T \cup E$ is an OES then there are sentences e and e' , logically independent, such that $e \in E$ and $e' \in E$.

Before we begin the proof, a word as to what is going on. Our problem is that we fear that t_0 , in being tailored to rule out redundancies, is so narrow an explanation relation that the significant yield of a theory T under t_0 has only one member. This is close to the intuition behind the minimality of the minimal law. That is, it seems as if the minimal law is so constructed as to rule out its being an explanans statement for all but one explanandum. What we do in the proof is show that given an explanation set, i.e. a generator, T , and its consequences, E , that is broad, we can construct subsets of T where each subset Omer explains one of a tuple of logically independent explananda. We then show that we can combine these subsets and their respective consequences in a way that guarantees the combined elements to be still in the Omer explanation relation.

Proof: We assume without argument that there is a consistent set T 's, such that $T \subseteq T'$ and where $Cn_e(T')$ is broad.

' $Cn_e(X)$ ' names the consequence closure of a set X under , in symbols $Cn_e(X) = \{e: X \vdash_e e\}$ ³⁶

(1) Firstly we partition T' into two subsets T^* and T^+ , making sure that $T^* \vdash_e e$ and $T^+ \vdash_e e'$. The existence of these subsets is, of course, guaranteed by Lemma I. We remark that this is equivalent to the claim that $T^* \cup \{e\}$ and $T^+ \cup \{e'\}$ are OES's. ³⁷

(2) We now work to show that we can construct a set T out of the elements of $T^* \cup T^+$, such that $T \cup \{e, e'\}$ is an OES, i.e. $T \models e$ and $T \models e'$. That is the theorem.

(3) It is clear that $T^* \cup T^+ \vdash e$ and $T^* \cup T^+ \vdash e'$. It is equally clear that it is not necessary that $T^* \cup T^+ \models e$ and $T^* \cup T^+ \models e'$. If we assume $T^* \cup T^+ \not\models e$ or $T^* \cup T^+ \not\models e'$ it must be because of some violation of restrictions (1)-(5).

(i) It is clear that (1) is not the problem by hypothesis.

(ii) It is equally obvious that (2) is not the problem since T is consistent whence so are all subsets of T , in particular $T^* \cup \{e\}$ and $T^+ \cup \{e'\}$. This is by the compactness property in respect of consistency.³⁸

(iii) Condition (3) of Omer's construction is similarly easy to guarantee once we see the construction in cases (iv) and (v) immediately following

(iv) Assume that Omer's condition (4) is violated, Given that $T^* \cup \{e\}$ and $T^+ \cup \{e'\}$ are OES's the (4) violation must be between some $t_{c_i}^* \in T^*$ (really T^*_c) and e' or between $t_{c_i}^+ \in T^+$ and e . Since condition (4) denies that either $e \vdash t_{c_i}$ or $t_{c_i} \vdash e$, we have two main sub-cases.

Let ' e^0 ' represent either e or e' and similarly for ' T^0 ', T^* and T^+ and ' $t_{c_i}^0$ ', $t_{c_i}^*$ and $t_{c_i}^+$. Notice;

$$\text{Cn}(T^*) \subseteq \text{Cn}(T^0)$$

$$\text{Cn}(T^+) \subseteq \text{Cn}(T^0), \text{ and}$$

$$\text{Cn}(T^0) \subseteq \text{Cn}(T').$$

(a) Assume $e^0 \vdash t_{c_i}^0$, we then form the set;

$$T^{\oplus} = \{T^{\circ} - \{t^{\circ}_{c_i}\} \cup \{e^{\circ}\}\}$$

for example, if the violation is that $e' \vdash t^*_{c_i}$ we form
 $\{T^* - \{t^*_{c_i}\} \cup \{e'\}\}$

Note that $T^{\oplus}$ is free of (4) violations. Note further that $Cn(T^{\oplus}) \subseteq Cn(T^{\circ})$ since $e^{\circ} \in Cn(T^{\circ})$ and by the construction of T^* .

(b) Assume $t^{\circ}_{c_i} \vdash e^{\circ}$, we then form the set;

$$T^{\oplus} = \{T^{\circ} - \{t^{\circ}_{c_i}\} \cup \{e^{\circ}\}\}$$

for example, if the violation is that $t^+_{c_i} \vdash e$ we form
 $\{T^+ - \{t^+_{c_i}\} \cup \{e\}\}$

Note, for the example offered, $e \in Cn(T^{\circ} \cup T^{\oplus})$ since $e \in Cn(T^*)$ and $Cn(T^*) \subseteq Cn(T^{\oplus})$. By enumerating the possibilities we see that the subset relation is general in respect of all cases. But that is to say; $Cn(T^{\oplus}) \subseteq Cn(T^{\circ}) \subseteq Cn(T')$.

We then form the set, $T\# = T^{\oplus} \cup T^{\oplus}$. But $\langle T\#, \{e, e'\} \rangle$ satisfies (4) and, of course $Cn(T\#) \subseteq Cn(T^{\circ})$ whence $Cn(T\#) \subseteq Cn(T')$.

The intuition behind the formalism is this; when in searching for a potential explanation we choose from the pair $\langle T\#, \{e, e'\} \rangle$, we refrain from choosing pairs that violate (4) in either respect. We still have, however, in $T\#$ all the sentences needed to make the explanatory force of $T\#$ compare favorable with that of T^* and T^+ , that is, the Omer explanans sets relative to e and e' . In particular our construction guarantees for sub-case (a) that $Cn(T\#)$ is $Cn(T^*) \cup Cn(T^+)$ since some $e^{\circ} \vdash t^{\circ}_{c_i}$ and e° is in $T\#$. Note, although $t^{\circ}_{c_i}$ is in $T\#$, $t^{\circ}_{c_i}$ is never used as an

explanans for e^0 . $t^0_{c_i}$ is there, however, to play whatever other role it may have relative to other explananda. For sub-case (b) our construction guarantees that, $Cn(T^\#) \subseteq Cn(T^*) \cup Cn(T^+)$ since some $t^0_{c_i}$ has been removed from either $Cn(T^*)$ or $Cn(T^+)$ by the construction of $T^\#$. But $e^0 \in Cn(T^\#)$ so the theorem goes through. Realize e^0 is either e or e' .

(v) We now have to construct a set, T , so that $\{T \cup \{e, e'\}\}$ is an OES and where $Cn(T) \subseteq Cn(T^\#)$, that is to say $Cn(T)$ is free from violations of Omer's conditions (1)-(4). Recall that $\{e, e'\} \subseteq Cn(T^\#)$. Assume $\langle T^\#, \{e, e'\} \rangle$ violates condition (5). If so, it must be because there is an Omer counter S , i.e. $T^0 \vdash S$ and $S \vdash e^0$. We then form, as before, the set $T = \{T^\# - T^\# \wedge \{D_1, \dots, D_n\} \cup \{S\}\}$, where $D_1, \dots, D_n$ is some proffered deduction of S from $T^\#$. But then T is free of violations of (5) relative to e and to e' . But since $T^0 \vdash S$, $Cn(T) \subseteq Cn(T^\#) \subseteq Cn(T^*) \cup Cn(T^+)$. $T^\#$ is (1)-(4) violation free by construction in respect of $\{e, e'\}$ but S is an Omer counter so no additional problems are raised. But that is to say $T \vdash e$ and $T \vdash e'$; which was that to be proven. ■

We now prove diversity for $\vdash$.

Theorem IV: If T and T' are logically independent there are two OES's $T^* \cup \{e\}$ and $T^+ \cup \{e\}$ where $Cn(T^*) \subseteq Cn(T)$ and $Cn(T^+) \subseteq Cn(T')$.

Proof: We assume without argument that there are T and T' , logically independent, such that $T \vdash e$ and $T' \vdash e$.³⁹ But

then by Lemma I there are sets T^* and T^+ where

$Cn(T^*) \subseteq Cn(T)$ and $(Cn)T^+ \subseteq Cn(T')$ where $T^* \vdash e$ and $T^+ \vdash e$.⁴⁰

Theorem V: If $T \vdash S$ and $S \vdash e$ then there is a relation, $\vdash$, such that $T' \vdash e$ where $T' \vdash S$ and $Cn(T') \subseteq Cn(T)$ and where the claim $T \vdash e$ is trivialization free except for S . That is to say, S is the only Omer counter to the claim $T \vdash e$.

Before we begin, a word about the motivation behind the construction. We want to replicate the notion of depth; that is to say, the transitivity of an explanation relation. The need, however, to define a relation that ruled out redundancies and irrelevancies forced us, as remarked before, to generate the relation $\vdash$ that is, in fact, anti-transitive.⁴¹ What we want to do is generate a relation where redundancies and irrelevancies can be controlled. We do this by building chains of Omer explanations each one of which is free of redundancies and irrelevancies. Such chains of Omer explanations will be referred to as "Omer chains". Omer chains are thought of, then, as sets of pairs in the relation $\vdash$.

Proof: (1) it is clear that $T \cup \{S\}$ and $S \cup \{e\}$ are OES's; it is equally clear that $T \cup \{e\}$ is not. We now show how to construct a set T' that is without significant trivializations viz a viz e . That is to say S is the only Omer counter to the claim that $T' \vdash e$.

We then stipulate that $\vdash$ is the least relation satisfying the construction to follow. We assume without argument that $\langle T, e \rangle$ satisfies Omer's condition (1)-(3).

(i) Assume that $\langle T, e \rangle$ violates (4), i.e. there is a $t_{ci} \in T$

such that either $t_{c_i} \vdash e$ or $e \vdash t_{c_i}$.

(a) Assume that $e \vdash t_{c_i}$. We then form the set

$$T^* = \{ T - \{t_{c_i}\} \cup \{e\} \}.$$

This is the construction from Theorem III, case (iv) (a).

And as before since $e \vdash t_{c_i}$, $Cn(T^*) = Cn(T)$, Whence

$$Cn(T^*) \subseteq Cn(T).$$

(b) Assume that $t_{c_i} \vdash e$, We then form T^* as immediately

above, case (i) (a) this theorem. Note that $Cn(T^*) \subset Cn(T)$

this since $t_{c_i} \in T$ and $t_{c_i} \notin T^*$. This raises the following

problem. Is it possible that $T^* \not\vdash S$, i.e. we lose S

from the consequence closure of the resultant set--and

this because t_{c_i} is part of a necessary condition for S .

We assume this for reductio. Take the conjunction of the

members of the largest sequence of truth functional compo-

nents of this necessary condition and call it ' $\dots \wedge t_{c_i} \wedge \dots$ '⁴²

Since this is equivalent to a necessary condition

for S , $S \vdash \dots \wedge t_{c_i} \wedge \dots$. But then $S \vdash t_{c_i}$. But since $t_{c_i} \in T$,

$T \not\vdash S$. This is absurd (i.e. condition (4) is violated

in respect of the pair $\langle T, S \rangle$ which is contrary to our as-

sumption that $T \vdash S$). But if our assumption is absurd it

is clear that $T^* \vdash S$.

(ii) Assume $\langle T^*, e \rangle$ violates (5), that is to say there is an

Omer-counter S' , such that $T^* \vdash S'$ and $S' \vdash e$;

(a) If $S = S'$ the theorem is proved since $T^* = T'$.

(b) If $S \neq S'$ we then form the set;

$$T' = \{ T^* - T^* \wedge \{D_1, \dots, D_n\} \cup \{S'\} \}$$

This is the construction from Theorem I (4). Now, since S'

is an Omer-counter $T^* \vdash S'$ since $T^* \vdash_e S'$. But recall,

$$Cn(T^*) \subseteq Cn(T) \text{ and } T^* \vdash S' \text{ so}$$

$$Cn(T') \subseteq Cn(T)$$

S' , however, is an Omer-counter, so we have no further violations for the pair $\langle T', e \rangle$. And, of course S' is the only Omer-counter, whence $\langle T', e \rangle$ satisfies (5).

(2) All that remains to show is that $T' \vdash S$.

Assume for reductio that $T' \not\vdash S$, but we know that $T^* \vdash S$, so the element discarded in forming T' must be part of a necessary condition for S , i.e. $T^* \wedge \{D_1, \dots, D_n\}$ is a necessary condition for S .

We then construct as follows;

Conjoin the members of $T^* \wedge \{D_1, \dots, D_n\}$ then as usual form the conjunction of the set whose members are the longest sequence of truth functional components of $T^* \wedge \{D_1, \dots, D_n\}$.

We call this long conjunction ' $\dots \wedge t_{c_i} \wedge \dots$ '. But

$\dots \wedge t_{c_i} \wedge \dots$ is a necessary condition for S , so $S \vdash \dots \wedge t_{c_i} \wedge \dots$, but then $S \vdash t_{c_i}$. In the case developed so far,

$T^* \wedge \{D_1, \dots, D_n\} \subseteq T^*$ and $T^* \subseteq T$ so t_{c_i} is one of the truth functional components of T , since we are dealing with

the longest such list; whence the pair $\langle T, S \rangle$ violates Omer's condition (4), that is $S \vdash t_{c_i}$. But then, of course $T \not\vdash S$ which is absurd.

In the foregoing we have defended the adequacy of a modified version of t_0 for theoretic explanation. That is that t_0 is both broad and diverse (whence t'_0 is as well) and there is an intuitively plausible relation, t'_0 , that permits

us to generate deep explanations (Omer-chains) that resist the use of redundant or irrelevant elements in explanations. How do we reconcile this with Theorem I. Recall that T Omer-explains e implies that T minimally explains e . The answer is that although we can use minimal laws as explanans statements, we can build non-trivializable explanation chains between these 'minimal' explanation. Further as the construction is wedded to counters presented against trivial explanations, there is no need to insist the process of demanding closer fitting explanations be continued until the minimal law is reached. The minimal law stands as a potential explanans statement but it is not the only one nor is it the ultimate one for it too will be explained in the light of theoretic advance. The core notion for such theoretic explanations is, of course, t_0' . What is important is not the task of insisting that scientists persist in finding ever closer explanations, it is rather that Philosophers desist from using the apparatus of first order theories in a promiscuous and misleading fashion.

As mentioned earlier the need to generate t_0' points to the equivocity of the work 'explain'. We remind the reader of the dictum of Sellars mentioned earlier; theories explain why phenomena obey the laws they do. In our terminology the t_0' relation between some T and some e , explains why there is an t_0 relation between some S and some e , that

is because there is an t_0 relation between T and S. The importance of this for showing how ontological commitment shifts under deep theoretic commitments will be the ultimate goal of this paper. This whole problem will be returned to later when we use Omer-chains to elucidate the notion of theories and laws themselves being explained, that is, when we turn to the notion of reduction.

As it stands now Theorems III-V are considered to justify the notion of explanation presented. We therefore feel free to rest with our syntactic plus pragmatic construction as an analysis of a non-trivial, essentially deductive, explanation relation. We think then, of this analysis of explanation as incorporating various proposals governing the well management of standard deductive apparatus in the arguments against the D-N model put forward by the 'event ontologists'. Later on, when we continue with our presentation of the various analyses of the reduction relation offered in the literature we will utilize, relative to the constructions presented, fairly standard notions in analysing the meta-theory of scientific practice.

FOOTNOTES

Chapter I

¹A recent statement of the D-N model by Hempel is as follows: "... (an) explanation may be conceived as a deductive argument of the form

$$\begin{array}{rcl} & C_1, C_2, \dots, C_k & \text{Explanans} \\ (D-N) & L_1, L_2, \dots, L_k & \\ \hline & E & \end{array}$$

Here $C_1, C_2, \dots, C_k$ are sentences describing the particular facts involved; $L_1, L_2, \dots, L_k$, are the general laws on which the explanation rests... The conclusion E of the argument is a sentence describing the explanandum phenomenon." Carl G. Hempel, Aspects of Scientific Explanation, New York: MacMillan, Inc., The Free Press, 1965.

²There are avenues of attack against the D-N model besides for those we have characterized in the text. So, one finds criticism of the D-N model on the grounds of its inadequacy in respect to some intuitive notion of causation. A classic argument of this sort is found in A. Pap, "Disposition Concepts and Extensional Logic," Feigl, Scriven and Maxwell, Minnesota Studies in the Philosophy of Science (Minneapolis, Minnesota: University of Minneapolis press 1958) vol. 2. We will deal with the attempt to offer a causal account in lieu of the more standard D-N model within the context of our discussion of events ontologies (Chapter II). Another area of concern for the D-N theoretician is the host of 'pragmatic' problems generated by the work of N. Goodman, Fact, Fiction and Forecast, Indianapolis, Inc.: The Bobbs Merrill Co., Inc., 1965. We will deal with these issues in an elliptical manner

throughout. Notice, we do not maintain this paper as a rehabilitation of the standard meta-theory against all arguments, actual or potential; rather we attempt to show, firstly, its durability, and secondly, and more importantly, its utility.

³I.A. Omer, "On the D-N Model of Scientific Explanation," Philosophy of Science 37:417-33.

⁴The kind of trivialization moves alluded to include the classic moves of R. Eberle, D. Kaplan, and R. Montague, "Hempel and Oppenheim on Explanation," Philosophy of Science, 28:168-71, and the responses of J. Kim, "On the Logical Conditions of Deductive Explanation," Philosophy of Science, 30:286-91. cf. Appendix A, for some examples. The way the problem has been dealt with in the arguments and counter-arguments alluded to is, roughly, as follows. Some simple, although authoritative, version of the D-N model is presented as a sufficient (nearly) condition for explanation. An example of an intuitively obvious explanation scheme is then offered. By 'explanation scheme' I mean the representation in some simple language, frequently first order predicate logic, of the logical form of a possible explanation in science. Explanation schemes are frequently offered with the intention of exhibiting some clear principle required to distinguish explanations from, for example, mere deductions. The theorist, in an attempt to discredit the presented explanation scheme then applies to it techniques gleaned from the study of formal languages. The

techniques used frequently preserve important logical properties, such as logical equivalence. Using these techniques a second explanation scheme is generated. This latter one usually being undesirable in some way. The D-N theorist then responds to this "trivialization move" by altering, usually tightening, his criteria for the adequacy of an explanation scheme. Omer's work is a fairly exhaustive account of the moves and counter-moves made during the 1960's. The conditions he generates are sufficiently rigorous to block a wide variety of such trivialization moves; certainly the most well known instances of the genre.

⁵Let 'T' name a set of sentences of first order logic. It is clear that all of the sentences in T can be decomposed into atomic sentences or universal sentences. Clearly the truth assignment to the sentences of the decomposition is sufficient to determine the truth assignment to the members of T, whence in a secondary sense, to T itself. We choose the longest sequence of sentences in this decomposition because we want to be able to rule out redundant self-explanations of the various t_{c_i} 's in T_c by themselves. T_c is thus 'spread out' so that we can talk of comparing the elements of T without fear that elements might be "hidden" in complex sentences.

⁶The words "It is not possible to find" are Omer's. The construal of this phrase turns out to be the key to the application of Omer's conditions to our issues.

⁷W.V. Quine, Methods of Logic (New York: Holt and Co.,

1950), p. 15 and P.F. Strawson "If and $\supset$ ", Off-print $\propto$ 19, (Cambridge: University of Cambridge), p. 7.

⁸Omer, "On the D-N Model of Scientific Explanation," p. 418.

⁹Ibid., p. 425.

¹⁰R. Carnap, Introduction to Semantics (Cambridge: Harvard University Press, 1946), p. 149.

¹¹Omer, "On the D-N Model of Scientific Explanation," p. 426.

¹²Ibid., p. 433.

¹³C.G. Hempel, Aspects of Scientific Explanation, (New York; Free Press, 1965), p. 346. We put 'T' for 'L' throughout. Although Hempel construes 'e' as an event description the problem is generalized to laws, minimally explained, and even theories minimally reduced. Chapter IV below.

¹⁴Ibid., p. 345.

¹⁵A. Koslow, personal communication, has argued that an alternative account of the minimal law is preferable. Since the statement of Koslow's analysis requires apparatus not yet generated we leave this until later (foot-note 17 below).

¹⁶Ibid., p. 345.

¹⁷Ibid., p. 347

¹⁸By 'compactness' we mean some finitary property of a relation or set. The particular notion of compactness property alluded to is found in R. H. Thomason, Symbolic

Logic (New York: MacMillan and Co., 1970), p. 66, M3.

¹⁹See pages 11-12 for details.

²⁰A. Koslow, personal communication, thinks that the need to relativize T^+ to the deduction $D_1, \dots, D_n$ restricts the generality of the proof. In a clear sense he is correct for we are working from a particular deduction of T^* from T and there might be others. I persist in this rather narrow fashion for, as we shall see, my interest is with counter examples offered against manifestly trivial or redundant explanation schemata. What my proof of Theorem I focuses on is the ability to deal with apparantly trivial explanation schemata by generating counter examples that show us the trivial and redundant elements of the explanans that can be discarded. The value of such a procedure is that it enables us to reject, in a principled fashion, various, intuitively undesirable explanation schemata.

Koslow offers the following generalized version of my result. We first define as follows; $T \vdash_m e$ if and only if there is no T such that, (1) $T \vdash T^*$

(2) $T^* \not\vdash T$

(3) $T^* \vdash_e e$; where $T \vdash_e e$ if

and only if

(1) $T_c \vdash e$

(2) $T_c \not\vdash \sim e$

(3) For some $t_{c_i} \in T_c$, t_{c_i} is a universal law.

(4) for all $t_{c_i} \in T_c$, neither $t_{c_i} \vdash e$ nor $e \vdash t_{c_i}$. That is, $T \vdash_e e$ if and only if Omer's

(1)-(4) are satisfied by the pair $\langle T, e \rangle$.

Theorem K-I If $T \not\vdash e$ then there is no T^* such that $T \vdash T^*$ (but not conversely) and $T^* \vdash e$.

Proof: Form T^*_C since $T^* \vdash e$ there are a finite number of $t^*_{c_1}, t^*_{c_2}, \dots, t^*_{c_n} \in T^*_C$ such that $t^*_{c_1}, \dots, t^*_{c_n} \vdash e$ (Compactness). Now since $T \vdash T^*$ each $t^*_{c_i}$ is a consequence of a finite number of t_{c_i} 's. We list them all for each $t^*_{c_1}, \dots, t^*_{c_n}$ in one list: $t_{c_1}, t_{c_2}, \dots, t_{c_k}$. Thus $t_{c_1}, t_{c_2}, \dots, t_{c_k} \vdash t^*_{c_1} \wedge \dots \wedge t^*_{c_n}$. Now since $T^* \not\vdash T$, there must be some t_w in T_C which does not follow from $t^*_{c_1}, \dots, t^*_{c_n}$ (otherwise $T^* \vdash T$).

Consider $t_w, t_{c_1}, \dots, t_{c_k}$.

(1) They are all in T_C .

(2) $t_w, t_{c_1}, \dots, t_{c_k} \vdash t^*_{c_1} \wedge \dots \wedge t^*_{c_n}$ (even without t_w)

(3) $t^*_{c_1}, \dots, t^*_{c_n} \not\vdash t_w \wedge t_{c_1} \wedge \dots \wedge t_{c_k}$ for it doesn't imply t_w .

(4) $t^*_{c_1}, \dots, t^*_{c_n} \vdash e$ (by hyp)

replace $t_w, t_{c_1}, \dots, t_{c_k}$ in T_C by $t^*_{c_1}, \dots, t^*_{c_n}$. Call this result $\bar{T}$

(5) $\bar{T} \vdash e$. This violates Omer's (5) for $T \not\vdash e$.

Corollary: $\vdash$ is not transitive.

Proof: Suppose $T \vdash T^*$ and $T^* \vdash \bar{T}$ (and not conversely) and $T \not\vdash \bar{T}$.

But then since $X \vdash a \Rightarrow X \vdash a$, we have $T \vdash T^*$ and $T^* \vdash \bar{T}$ and $T \vdash \bar{T}$.

But this contradicts Theorem K-I.

The relation between Theorem K-I and Theorem I is clear. Theorem K-I implies Theorem I. This since Given $\bar{T}$ we can always generate our set $T_{C_{T^*}}$. We do this by first using

$\bar{T}$ and standard techniques for the proof of the deduction theorem (cf. Thomason (1970), p. 68 ff.) to generate the appropriate deduction and then following (4) of Theorem I to generate T_{CT*} . Notice that $T^+ \subseteq T$. The fact that it is possible that $T^+ \subset \bar{T}$, is the core of the problem for my version. That is to say that there may be redundant or superfluous elements in $T - T^+$. We will then require another counter example and another round of the construction. Koslow handles all of this in one step. Realize, however, that all Koslow knows is that there are a finite number of t_{C_i} 's used in any deduction. The list $t_{C_1}, t_{C_2}, \dots, t_{C_k}$ is not, in general, constructible. Of course we know the list is there to be found (Compactness). What my proof does is outline a procedure for finding the list albeit piecemeal.

It turns out, however, that Koslow's construction leads to versions of my results that point up salient properties of the attitude that I take throughout this chapter. For this reason we will follow along with Koslow's versions of Theorems II-V in footnotes.

²¹A. Robinson, Introduction to Model Theory and the Meta-Mathematics of Algebra, (North Holland: 1965), pp. 116-17.

²²Koslow has an alternative proof given the interpretation of the issue offered in footnote above. So we have:
Theorem K-II: $T \vdash_m e \not\Rightarrow T \vdash_0 e$
Proof; Suppose $T \vdash_m e \Rightarrow T \vdash_0 e$ but then by Theorem K-I there is no T^* such that $T \vdash T^*$ (but not conversely) and $T^* \vdash e$. But Craig's Lemma provides such a T^* . On the other

hand $T \vdash_m e$ denies that there is such a T^* .

Koslow then adds, "Notice that in the statement of Theorem K-I it is not assumed that T^* is a law or theory" (loc. cit.) This becomes important for our attempt to escape the apparent dilemma that Craig's Lemma creates for t_0 . For if we assume that what (5) should have said is;

(5^k) It is not possible to find laws or theories, $s_1, s_2, \dots$, ...etc. then Craig's Lemma guarantees no such thing even for as weak an explanation relation as t_e as defined above (cf. footnote 20). As Koslow points out this is crucial if we construe the Hempelien minimal law as the 'weakest of those which explain' (loc. cit.). We will stay with the analysis that Hempel offers in Aspects and so t_e stays defined as in the text rather than as in footnote 20. Notice, that the most we can assert is that $T_{C_s} \vdash e$ rather than the stronger $T_{C_s} \vdash t_e e$. As we shall see I will opt for another tactic to free t_0 from the problem of the availability of an interpolation statement, s . My tactic is tailored to serve the use I put Omer's criteria to, i.e. I will take Omer as a necessary condition for a meaningful explanation relation. This will enable us to deny the cogency of prima facie trivializations of the D-N account of explanation as well as problems associated with Nagel's theory of reduction.

²³ It might be argued at this point that since it is possible that $e \vdash s$, for some interpolation sentence, s ,

(5) is trivially satisfied in some cases. A perusal of the standard proof of the Lemma (loc. cit.) shows us that the

result is general in respect of this case; whence there are instances of the violation of (5) guaranteed by Craig's Lemma.

²⁴p. Kline, Fact and Fantasy in Freudian Theory (London: Methuen & Co. Ltd, 1972) .

²⁵What we mean here is that the example offered to demonstrate the inappropriateness of a pair $\langle T, e \rangle$ as an explanation schema, S, be a plausible candidate for an explanans statement in respect of e. Recall, Craig does not guarantee that such an S exists. The best we get from Craig is the necessary existence of an interpolation statement, not a statement that satisfies, for example, (3). What we do require, however, is that if such an S exists, and it exists for the examples of trivialization arguments in the literature, that the pair $\langle T, e \rangle$ be given up and the pair $\langle S, e \rangle$ be taken instead. Notice that the statement here is not quite accurate. What we mean is precisely stated in the theorems to follow.

²⁶Omer, "On the D-N Model of Scientific Explanation," pp. 432-33.

²⁷Hempel, Aspects of Scientific Explanation, p. 345.

²⁸Ibid., p. 347. We note that in the quotation Hempel says 'deductively subsumed' rather than 'explained'. It is clear from the context that it is the latter that is intended. We also remark that in the discussion Hempel includes the deduction of approximations to laws rather than

laws themselves. We will omit this issue here returning to it when we consider the challenge to the D-N model associated with P. Feyerabend.

²⁹Recall that all we require of e is that it is a sentence, if e is an event description, as Hempel usually assumes it to be then e and e' describe different concrete events.

³⁰For anti-transitivity see Corollary Theorem K-I, footnote 20.

³¹Note here in passing that if we accept Sellars' distinction between a law explaining phenomena and a theory explaining why the law explains the phenomena there is a precedent for our having two explanation relation, similar although diverse. So if t_0 turns out to be a broad and diverse t_e , then if t_0' has a transitivity analogue the adequacy of our construction entails that 'explanation' is not univocal. See W. Sellars, "The Language of Theories," in Feigl and Maxwell, Current Issues in the Philosophy of Science (New York: Holt, Rinehart and Winston, 1961), for the position mentioned.

³²Of course, if our suggestion is to be pragmatically viable the number of actual counters must be small enough to be manageable. We assume that in actual practice the number of counters will be few in number and rarely brought forward. The point is that the examples from the literature that attempt to trivialize the notion of D-N explanation permit of obvious counters.

³³See pages 19-20.

³⁴See footnote 20.

³⁵Recall that there are alternative deductions, but since we are interested in getting rid of superfluous elements we take any deduction offered. The worst that can happen is that we sloppily omit elements of T that are not really needed. But realize that we get rid of all elements of T needed to deduce S_C . Realize further that by construction, $T' \vdash e$.

³⁶Note: to say that t_e is not broad is to say that either theoretic explanations, in general, are not broad, which seems absurd or that Omer's conditions (1)-(4) are not even a necessary condition for an appropriate explanation relation. Given the centrality of (1)-(4) in informed discussions on the topic this is highly unlikely.

³⁷ T^* and T^+ may even be identical but then the proof is immediate.

³⁸Thomason, Symbolic Logic, pp. 82-84, in particular D11, M32, M33 and M34.

³⁹The argument here is the analogue to the argument offered in footnote 36. If t_e is not diverse then either t_e is a mistaken analysis of a minimally adequate explanation relation or else diversity is not a desiderata of explanation.

⁴⁰Using the construction preceding Theorem K-I, footnote 20, Koslow proves analogues to both Theorem III and Theorem IV.

Theorem K-III: ' t_o ' is broad; Assume that for every e , e' such

that $T \vdash_0 e$ and $T \vdash_0 e'$, either $e \vdash e'$ or $e' \vdash e$. If $e \vdash e'$ then $T \vdash_0 e$, $e \vdash e'$ and $T \vdash_0 e'$ which contradicts Theorem K-I. Similarly if $e' \vdash e$.

Theorem K-IV: ' $\vdash_0$ ' is diverse; We suppose that T is not logically equivalent to T' . Assume that $T \vdash_0 e$ and $T' \vdash_0 e$. Then if $T \vdash T'$ (but not conversely) Theorem K-I implies a contradiction. If, on the other hand $T' \vdash T$ then again a contradiction follows by Theorem K-I. So, neither T nor T' implies the other. (loc. Cit.). We recall that both theorems require the ' t_m ' be defined as the closest possible explanans. But that seems too strong. We don't necessarily require minimal explanations, rather we require a device that enables us to pinpoint redundant and superfluous elements. The issue seems most clear if we focus on the addendum quoted after the Theorem K-IV. If we follow Koslow in his construction, then all potential explanans statements relative to some explanandum are logically independent. But that precludes our capturing a highly desirable property of explanations. We will shortly refer to the property we have in mind as 'depth'. Our relation ' $\vdash_0$ ' will enable us to build chains of explanations, mirroring the practice of explaining e.g. laws via theories. Koslow's theorems show his construction to be inadequate to that task (see Theorem V, page

⁴¹See footnote 20.

⁴²Here ' t_{c_i} ' stands for the conjunction of the members of the longest sequence of truth functional components of itself.

CHAPTER II

The point of the construction of the relation 'Omer-explains' was to generate a deductivist account of explanation that was narrow enough to exclude certain trivialization moves usually held to vitiate the deductivist's model while retaining sufficient breadth to accommodate the need for theoretical explanations. Within this account we have retained two essential components of the deductivist position. Firstly, we retain the view that logical consequence, with as many qualifications as is necessary, is central for the understanding of scientific explanation or more precisely, the relation of logical consequence is the core notion needed for understanding those complex relational structures, found upon rational reconstruction, whose members are laws and event descriptions. Secondly, we retain the view that, upon the rational reconstruction of scientific discourse, events and their relations will be mirrored by sentences and their meta-linguistic relations rather than be denoted by singular terms and related by relations in the object language itself.¹

These views, seemingly standard among a wide variety of otherwise divergent views, have been recently challenged by those whom I shall call the 'event ontologists'; notably Davidson and Kim.² These authors working from diverse sectors of the philosophic enterprise have offered an analysis of scientific discourse relevant to our concern, that challenges these most basic suppositions. In the contributions by Davidson and Kim, the problems discussed within the standard account, in terms of a deductivist model, are dealt with within the context of the generation of some notion of causation.

So we find, "Much of what philosophers have said of causes and causal relations is intelligible only on the assumption...that causes are individual events and causal relations hold between events."³ This is to be understood within the context of the view that rejects "'caused'...(as)...a more or less concealed sentential connective and that causes are fully expressed only by sentences."⁴ Kim, similarly, demands that we "...abandon the logico-descriptive method and start taking events seriously."⁵ This, Kim maintains, forces us to "define the causal relation directly for events without reliance on linguistic intermediaries."⁶

Before we begin examining the arguments of the authors mentioned, consider a few remarks of a clarificatory nature. We do not claim that all proponents of the standard model reject events as an ontologically significant category. Hempel uses locutions such as, "When an individual event b is said to have been caused by an individual event a."⁷ Hempel,

in fact, uses a variety of similar locutions. In his presentation of the D-N model in Aspects of Scientific Explanation, he says, "Here $c_1, c_2, \dots, c_k$ are sentences describing the particular facts involved...to explain...the explanandum phenomenon."8 The issue is thus, not the existence of events or even facts. Rather it is the following: are events (or facts), given a rational reconstruction of scientific discourse, to be taken as an ontologically irreducible category or are they better seen as complexes composed out of whatever the particulars in the domain of scientific discourse will turn out to be.

Notice in the preceding paragraph when we say 'event' we intend the term in the sense of Davidson and Kim, that is, we mean 'event' in the ordinary sense. An event in such a sense is composed of, among other things and in whatever sense, particulars. That these particulars might themselves be events is a possibility left open by both the standard account and the accounts of Davidson and Kim. But this is irrelevant to our concern and the concerns of Davidson and Kim. The apparatus of quantificational logic does not demand a special character for the particulars in the domain of discourse. What it does demand is the freedom from confusion that results when the various syntactic and semantic categories implicit in such languages are conflated. Our issue is thus most happily put as follows: are the complexes described by sentences or noun phrases to be treated, quantificationally, on a par with the elements that stand as the

values for individual terms. The ultimate analysis of the values for such individual terms, in substantive contexts, remains completely open.

What we now do is the following. We first meet the challenges presented by Davidson and Kim using the results of Chapter I. Secondly, we show that the constructive attempts of the event ontologists, where adequately specified, suffer from the standard problems of the deductivist model without principles of well management. We next show that a plausible attempt to fill in the semantics of the event ontologist's account leads back to the standard account or into incoherence. Lastly, we deal with one of the strong motivations for the event ontologists entire procedure, that is the apparent quantification over event particulars in ordinary language and in particular the problem of the 'polydacity of predicates'.⁹

A characteristic offering from Davidson¹⁰ argues against the view that:

(1) The fact that...caused it to be the case that...¹¹ constitutes a sentential connective in any plausible sense. Davidson's argument fails to join with our issue directly since in our favored meta-theory the phrase in (1) would be rendered, roughly, by membership in the consequence closure of some theory cum initial conditions. This is not, of course, to connect sentences but rather to express metalinguistically, a relation, between a sentence and a set. It will be interesting, however, to examine the construction as our first opportunity to use the analysis in Chapter I as a tool for

rejecting arguments brought forward against the standard account.

Davidson assumes, correctly, that no causal connective is truth functional and offers the following argument after Frege to show that given plausible principles of substitution something like the phrase in (1) is truth functional. With 'p' for 'there was a short circuit', 'q' for 'there was a fire' and 'r' for 'Nero fiddled', and assuming that ' $q \equiv r$ ' is true we get:

(2) the fact the p caused it to be the case that q.

(2.1) the fact that p caused it to be the case that $\hat{x}(x=x \wedge q) = \hat{x}(x=x)$

(2.2) the fact that p caused it to be the case that $\hat{x}(x=x \wedge r) = \hat{x}(x=x)$

(2.3) the fact that p caused it to be the case the r.

The moves from (2) to (2.1) is warranted by the general principle that for a conjunction ' $A \wedge B$ ' the value assigned to the conjunction is identical to the value assigned to 'B' just when the value assigned to 'A' is true. ' $\hat{x}(x=x) = \hat{x}(x=x)$ ' is, of course, tautological, whence true. The move from (2.2) to (2.3) is warranted by the same principle since we assume 'q' and 'r' are equivalent in truth value.

It has been shown that an array like (2)-(2.3) suffices to demonstrate the truth-functionality of the connectives, in the case at hand, 'caused it to be the case'.¹² If we are to accept the appropriate-ness of the array to the issue we then show, absurdly, that the connective in (1) is truth

functional. The argument is irrelevant, however, to our notion of explanation. What Davidson could be construed as showing is that the argument offered shows the inadequacy of a correlative explanation, i.e. the explanation of (2.3) on the basis of the array (2)-(2.2). The explanation is inadequate because it would not enable us to distinguish an explanation of some particular event from an explanation of any other event. But his argument is irrelevant since, in schematic form, we have:

S1. $q \equiv r$

S2. $p \supset q$

S3. $p \supset (\hat{x}) (x=x \wedge q) = (\hat{x}) (x=x)$

S4. $p \supset (\hat{x}) (x=x \wedge r) = (\hat{x}) (x=x)$

S5. $p \supset r$

and S1-S5 is in violation of Omer-5 since we can offer in its stead

S'1 $q \equiv r, p \supset q \vdash p \supset r$, and

S1, S2 $\vdash q \equiv r \wedge p \supset q$

Davidson fails to notice that his argument, [(2)-(3)], leaves the D-N model untouched since, as mentioned earlier, the standard meta-theory marks 'caused' not as a sentential connective nor as a 'more or less concealed' sentential connective but rather as the name of a metalinguistic relation. Whence, if Davidson presents his argument as a move against the standard account of explanation the argument is based on the confusion between the use of a sentence to generate a sentence with 'caused' as a connective and the use of the

name of a sentence to mention the fact that it is in some favored relation to a set of sentences. Before examining the basis for Davidson's account we will turn to a similar, although more relevant, construction of Kim's.

Kim, quoting Davidson favorably,¹⁴ offers a series of arguments against D-N constructions. Firstly, he attempts to show that if explanation (causation) is based on 'nomic subsumption', where, roughly, a law subsumes its instances, if a law subsumes any event described by a true sentence it subsumes all events described by true sentences; where events are the instances of laws and where sentences describe events. The argument offered is as follows:

(3) $(x)(Fx \supset Gx)$

(3.1) Fc

(3.2) Hb

(3.3) $H(Ix)(x=b \wedge Fc)$ ¹⁵

(3.4) Gc

The point of the (3.i)'s is that if, intuitively, $(x)(Fx \supset Gx)$ subsumes Fc and Gc then it is equally clear that $(x)(Fx \supset Gx)$ subsumes Hb and Gc where 'Hb' stands for any event description 'p' such that 'p' is true.

As is obvious, what we have here is a variant of Davidson's argument without the problem of confusing sentences and their names. Kim correctly expresses the argument as a pattern of inference rather than as a series of substitutions into sentential contexts. Presenting the argument as a pattern of inference, however, makes the procedure patently

irrelevant to the point at issue. For although logically valid, the argument is unreasonable as an example of an argument scheme useful in scientific contexts. This is simply because (3.2) and (3.3) are useless and irrelevant steps and are marked so by Omer-5.

Kim offers the following familiar objection against the D-N model:

(4) T

(4.1) $p \vee \sim T$

(4.2) p

thereby claiming to show that any event is a D-N consequence of any true statement with 'T' standing in for any arbitrary true statement. Such a schemata, to my knowledge, has never been offered by any D-N theorist, nor should such a bizarre array be countenanced since $p \vdash p \vee \sim T$ whence (4.1), (4.2) violates Omer-4.

Kim's last offering is:

(5) (x) (Cx $\supset$ Bx)

(5.1) Cb

(5.2) Rbc

where 'R' names a relation such that (x) (y) [(Bx $\wedge$ Rxy) $\supset$ By]

(5.3) Rc

In Kim's text the argument is presented as an instance with 'Cx' for 'x is a crow', 'Bx' for 'x is black', 'Rbc' for 'b has the same color as c'. I grant immediately that the interpreted argument is trivial. But the reasoning for distinguishing the schematic representation from the interpretation is just to mark the pattern as appropriate, although having,

at least one, trivial instance. It has never to my knowledge been maintained that the abstract presentation of the logic of a phenomena is necessarily as profound as the application of that logic to problems of immediate concern. To argue (5)-(5.3) as interpreted against the D-N model seems analogous to rejecting Principia Mathematica as an analysis of mathematical reasoning on the ground that some sections in it include arguments whose abstract form has trivial instances.

Part of the problem in understanding the motivation behind the offerings of Kim and Davidson is the relative lack of analysis of principles assumed in arguments offered. Kim in a 1970 paper¹⁶ states rather precisely, what seems to me to be the points at issue. Let 'D-N' name some set of conditions adequate for scientific explanation and let 'L' and 'C' name a law and a set of initial conditions respectively. Then if E and E' are statements describing events e and e', the principle behind the sort of arguments offered can be stated as:

(6) If $e=e'$ then $\langle L, C, E \rangle \in D-N$ iff $\langle L, C, E' \rangle \in D-N$ ¹⁷

(7) If S' is $S(a/b)$ then $a=b$ implies S and S' describe the same event for event descriptions S and S'.

(8) If $\vdash S \equiv S'$ then S, S' describe the same event.¹⁸

Kim then offers analogs to the arguments listed here as 2.n and 3.m.

Kim seems most dissatisfied with (7) and (8) and perhaps he is right. It seems plausible, however, to examine (6). Note at this juncture that Davidson assumes pretty much the

same principle in "Causal Relations." He says: "...but it seems to me that if it is Smith's fall that killed him and Smith weighs twelve stone, then Smith's fall was the fall of a man who weighed twelve stone whether we mention it or not."¹⁹ The issue, as I see it, is as follows: although in some sense it seems plausible to write as premises of an argument ' $e=e$ ', to assume that for any two descriptions of e and e' , E and E' , E and E' are equally appropriate as explanada is just to forget what the history of science has taught us. That is, that in order to explain event e , we must first encapsulate our knowledge of that event in a language that permits the kind of inferences or generalizations that de facto result in the explanatory structures that we are satisfied with. It is true, for example, that I can be referred to as the Instructor of Symbolic Logic at Hunter College: I can similarly be referred to as that person who has inhaled noxious substances into his lungs for some twenty years. To explain my imminent lung cancer under the first rubric is a task too foolhardy to even contemplate, to explain my demise under the second description is, unfortunately, all too easy.

The point is that (6) is false and therefore (7) and (8) misleading when applied in explanatory contexts. Although, in some sense, (7) and (8) may capture certain intuitions about events it is not clear that as far as well-managed scientific languages are concerned (7) and (8) permit the alteration of event descriptions under the full panoply

of devices symbolic languages afford. Now, although the last word on the well management of symbolic languages is not in, Omer's criteria seem a decent first attempt, at least in that they are sufficient to meet the most overtly artificial of the trivialization moves now familiar in the philosophic literature.

Without entering into greater detail than is necessary to make the point, it seems interesting to note that both Davidson's and Kim's constructions are vitiated by the very same problems the authors present as challenges to the D-N model. Davidson, in "Causal Relations," says "...on the proposed analysis one can show that the causal relation holds between any two events..."²⁰ and then trots out an old friend: "...imagine some description '(Ix)Fx' under which the existence of an event a may be shown sufficient for the existence of b. Then the existence of an arbitrary event c may be equally shown sufficient for the existence of b: just take as the description of c the following:

$(Iy)(y=c) \& (\exists! x)Fx.$ "

Davidson then sums up: "It seems unlikely that any simple and natural restriction on the form of allowable descriptions would meet this difficulty."²¹ We certainly agree with Davidson if he means that no "simple restriction" could be proposed a priori. But as we shall see, and already have seen in the pragmaticized version of Omer-5 it is not merely the a priori that is available to the methodologist. The methodologist has instances that can be, tentatively, taken

as exemplars of sound practice. And the full array of moves available in first order languages seems far in excess of what is required for the actual practice of science. In this light it is important to realize that ' $(\exists y)(y=c) \wedge (\exists! x)Fx$ ' is not a form of description that one would find in the writings of scientists, unless of course there was some relevance to the inclusion of ' $(\exists! x)Fx$ ' in the description of event c , perhaps there being the concurrence of both c and the entity having F necessary for some further causal process. But that is to deny the cogency of the entire procedure, for the counter intuitive aspect of the whole process is the 'arbitrariness' of the event c ; whence its tendency to be irrelevantly generalized. Of course, then the question from whence the restriction upon substitutions. The answer is worked out in practice; in the practice of the formalist who deepens our understanding of 'whatever' we do and the practitioner in the field who makes judgments as to the particulars. There is, of course, much more to the story, but we have said enough. The root error in the procedures under examination is the inappropriate conflation of the role of the logician to generate languages adequate to all procedures (as the story goes) and the role of the language user to come up with specific instances fit to some actual purpose. The logically possible forms of terms whether predicates in the sense of 'grue' (an intelligence test for philosophers) or individual terms as in the above do not point to the form of actual terms in use. And these forms are important. The methodologists

must not ignore the actual. The argument being brought out here is from the same source as the reinterpretation of Omer-5 and the argument against sentence (6) above. This argument is retained throughout in various guises and its cogency is to be judged by the clarity it brings to issues. Naturally, it is not impossible to give some formal statements of informal criteria like 'appropriateness or relevance'. Chapter I is intended as an example of such. But, as in Relevant Logic, to demand a formal analysis of informal issues is to court a system so narrow as to be useless (or almost so). The semantics for Relevant Logics show them applicable in models that are non-distributive; that marks them irrelevant to the majority of mathematics, distributivity being a property of almost all mathematical theories. But that is no excuse to pointedly use the possibility of irrelevance to confuse issues. In fact, such irrelevance is equally a tool as well, against the position that first uses it. We will continue to show this in our examination of the position brought forward by Kim.

Kim offers the following: An event, for the monadic case, is a concrete particular, whose name, written '[a,t,P]', reflects its relevant structure. That is, 'a' names an individual, the individual that is the locus of the event; 't' names a time (time interval) through which the event persists; and 'P' names a property, the property that constitutes the event (the event is thus best thought of as a having P at (during) t). An instance of the relevance of

the determining structure is Kim's analysis of causation where, without superfluous details, and minimally, e causes e' if e and e' are instances of generic events, events of that sort, such that the constitutive properties of kind K , where e and e' are events of kind K are constantly conjoined.²²

Once the relevance of transparency for event names is seen, event names unlike most names have essential meaning, it seems natural to ask if there are any criteria for distinguishing events named in terms of the structure of these very names. Kim maintains, for example, that e and e' are identical if $x=x'$, $t=t'$ and $P=P'$. So we ask: is $[x,t,P]$ identical with $[(\exists x)(x=a \wedge Rc),t,P]$ where ' Rc ' is any true sentence. If Kim responds by saying 'yes' we apply criteria (7) and (8) to show that all particulars that manifest P at the same time are the same event. If Kim responds 'no' we ask why that restriction is any less ad hoc than the restriction applied to normal event locutions such as those event locutions that are sentences with the particular named, as subject.

Kim feels the weight of a similar counter. So he says: "When there is a constant conjunction between F and G (generic properties of events) then, for any object you please, we can pick out a property H , such that that object has H and H is constantly conjoined with G ..."²³ The recipe for constructing H is as follows: "... e_1 is of kind F , e_2 is of kind G , and whenever an F -event occurs there occurs a corre-

sponding G-event. Let e_3 be any arbitrary event and let R be any relation such that $R(e_3, e_1)$. We explain 'H' to be true of any event e just in case $(\exists f)(R(e, f) \& (Ff))$. Then clearly e_3 belongs to generic event H , and H -events are constantly conjoined with G -events."²⁴ Of course, there is no argument for the necessity of generic events, i.e. kinds of events whose constitutive property is $(\exists f)(Ref \& Ff)$. Kim adds, "This plainly is a result we want to avoid."²⁵ Naturally, we want to avoid this for the same reason that we want to avoid the construction Kim himself presents against the D-N model. In fact they are just the same result in different syntactical guise appropriate to the area of discourse we want to confound.

Despite his careful and arduous development of an alternative to the D-N model, Kim finally asserts: "In any event, my aim here has been to outline a uniform and coherent ontological framework of events adequate for the formulation of Humean causation rather than to resolve substantive issues traditionally associated with the Humean approach."²⁶ Although Kim merely maintains his analysis as a "modest beginning" we must remind him the a framework for such "modest beginnings" was formulated some forty years prior by Braithwaite, Carnap, Hempel, et al. As is well known these attempts both answered and raised many questions, but for the most part were put forward within a meta-language that had the cogency and generality of the standard systems of first-order languages. These attempts, as alluded to in Chapter I,

were continuously challenged by theorists using first-order language in a peculiarly undisciplined way. In that way, much energy was diverted from more crucial issues. It is beholden on the methodologist to have the outline of a principled response to such prodigal attempts. And, of course, our Chapter I is just such an attempt. But why, may we ask of Davidson and Kim, is there a need for the new context. After all, the same old problems just keep on cropping up.

Not only are the construction offered by Davidson and Kim liable to the very objections they raise against the views of the D-N theorists, but there seem to be inherent difficulties in making sense of their inherently syntactic approach. By 'syntactic approach' here I mean the following: Despite their apparent interest in ontology, neither Davidson nor Kim attempt to assign a semantics to the syntactic devices they generate for naming or predicating to events. Notice, to give an interpretation of the symbolism in ordinary English is not to assign a semantics in the relevant sense--since the point of the symbolic device is to clarify the ontological import of the ordinary locution. Whence if the symbolic device is to afford clarification some analysis, other than the ordinary English must be offered. It is to the task of assigning a semantics for event locutions that we now turn.

Davidson maintains that events are particulars that enter into sentences by virtue of singular terms available

for quantification. An example of the use of such a singular term is:

(9) $(\exists x)$ Kicked (Shem, Shaun, x)²⁷

where (9) is read, 'there is an event, x , such that x is the kicking of Shaun by Shem'. Davidson then asserts, "...nothing now stands in the way of giving a standard theory of meaning for action sentences in the form of a Tarski-type truth definition (sic)."²⁸

Unfortunately, Davidson doesn't offer an example of such a 'definition' so we must fend for ourselves. In a 'Tarski-type truth definition' or more aptly, in a statement of the truth conditions for a sentence in accordance with Tarski's theory of truth, we associate with each of the extra-logical expression of a sentence an object or a set of objects. So, presumably, there is a function, f , mapping the extra-logical elements of (9) onto some domain where

$f('Shem') = \text{Shem}$

$f('Shaun') = \text{Shaun}$

$f('kicked') = \{ \langle x, y, z \rangle : \text{Kicked } x, y, z \}$

and where, since (9) is existentially quantified, there is some event e , where e is in the domain of interpretation of (9) under f and where $f('x') = e$.

We then say (9) is true if and only if

$\langle \text{Shem}, \text{Shaun}, e \rangle \in \{ \langle x, y, z \rangle : \text{Kicked } x, y, z \}$

If we are to make sense of our construction we must answer the following question; which event, e , is assigned to ' x '. That is, given that the assignment to 'Shem' and

'Shaun' is clear, what is the relationship of the event, *e*, to the particulars Shem and Shaun. Clearly, *e* is not a particular like Shem and Shaun. And equally clearly, if we want to ascertain which *e* to assign to (9) it must be the very *e* that includes Shem, Shaun and the described kick. The key word is the italicized 'includes'. So far that doesn't seem to present problems for it seems analogous to the standard:

(10) Ks, s'

where, say,

$g(K) = \{ \langle x, y \rangle : \text{Kicked } x, y \}$

$g(s) = \text{Shem}$

$g(s') = \text{Shaun}$

and where we say (10) is true if and only if

$\langle \text{Shem}, \text{Shaun} \rangle = \{ \langle x, y \rangle : \text{Kicked } x, y \}$

that is to say Shem and Shaun are included in the set of pairs $\langle x, y \rangle$ such that *x* kicked *y*.

Prima facie, the two italicized occurrences of verb forms of the word 'include' are equivocal in meaning. An event is not a set, but a concrete particular. So to be included in an event, whatever that means, is different from being the member of a set. So the first italicized occurrences of 'include' must mean something other than what the second italicized occurrences means. We ask: how are Shem and Shaun, not to mention the relation, kicking, included in *e*. Notice, the issue must be decided or the truth conditions are a charade.

Where Davidson remains uncommitted, Kim offers an analysis. "We think of an event as a concrete object (or n-tuple of objects) exemplifying a property (or n-adic relation) at a time."²⁹ The event, e , of Shem's kicking Shaun (at t) would then be

$$e = [\text{Shem}, \text{Shaun}, t, K']$$

and where ' K ' names the relation kicking. Notice K' is a binary relation, the relation of x 's kicking of y not the ternary relation, the event of x 's kicking of y as in (9). This is extremely important. We suppress the time variable, t , for perspicuity so $e = [\text{Shem}, \text{Shaun}, K']$ with K' binary. So if we have function, f , as before, we assign

$$f('e') = \langle \text{Shem}, \text{Shaun} \rangle$$

$$\text{just when } \langle \text{Shem}, \text{Shaun} \rangle \in \{ \langle x, y \rangle : \text{Kicking } x, y \}$$

With ' s ' for 'Shem' and ' S ' for 'Shaun' we now have: (9)

is true if and only if

$$\langle s, s', \langle s, s' \rangle \rangle \in \{ \langle \langle x, y \rangle, \langle x, y \rangle \rangle : K, x, y \langle x, y \rangle \wedge K' x, y \}$$

Recall, ' K ' is the ternary relation, the event of x kicking y , whereas ' K' ' is the binary relation of x kicking y .

Let us look more closely at the condition deemed necessary and sufficient for the truth of (9). In standard axiomatic set theory a triple, $\langle x, y, z \rangle$ is defined as a duple of a very special sort in particular $\langle x, y, z \rangle =_{df} \langle \langle x, y \rangle, z \rangle$ ³⁰

$$\text{So, } \langle s, s', \langle s, s' \rangle \rangle = \langle \langle s, s' \rangle, \langle s, s' \rangle \rangle$$

But that is terrible for the position at hand for a couple

$\langle x, y \rangle$ is defined as a particular sort of unordered set,

in particular

$$\langle x, y \rangle =_{df} \{ \{ s \} , \{ x, y \} \}$$

By substituting into the above and with parentheses and brackets for curly brackets we get

$$[(\langle s, s' \rangle) , (\langle s, s' \rangle , \langle s, s' \rangle)] ;$$

substituting further we get

$$((s) , (s, s')) , ((s) , (s, s') , (s) , (s, s'))$$

But since we know $\{x, x\} = \{x\}$ the above reduces to

$$\{ \{ s \} , \{ s, s' \} \} , \text{ but that is}$$

$$\langle s, s' \rangle .$$

Recall what reduces,

$$\langle s, s' \langle s, s' \rangle \rangle = \langle s, s' \rangle$$

So, either the truth conditions as stated are incoherent;

for we were to assign a triple to K, K being the ternary relation the event of x kicking y or if we accept the analysis we are forced to set $K=K'$.

But then with our identities that becomes,

$$\langle s, s' \rangle \in \{ \langle x, y \rangle : Kxy \wedge Kxy \} \text{ ., i.e.}$$

$$\langle s, s' \rangle \in \{ \langle x, y \rangle : Kxy \} .$$

This last, however, is the reduction of event locutions in the style of (9) to standard relations between ordinary particulars. That is to say, the attempt to characterize event locutions has gone nowhere for the analysis of this attempt in terms of a unified semantics in the style of a Tarski truth condition has resulted in event locutions reducing to standard locution; the extra place in the predicate reserved for events disappears. Recall in all fairness that the re-

duction of event locutions (predicates with an extra event place) to standard locutions is based on Kim's analysis of events, not Davidson's. But recall, as well, that Davidson doesn't have an analysis of event locutions, merely a relevantly uninterpreted formalism and the intuition that problems associated with event description in English can be handled by the simple device of increasing the number of places in chosen predicates by one. Our analysis has shown that the new syntactic element is either incoherent or indistinguishable from sentences where predicates fail to have extra event places.

Given the problems the 'direct ontological' approach generates, it seems appropriate to examine the deep motivation for the views of Davidson and Kim. Kim, self-avowedly, follows Davison's lead. Davidson, on the other hand, takes the impetus for his view from two sources. Firstly there is the rejection on the basis of the Fregian style trivialization proofs of the standard model of explanation. That is the path we have been following so far. Second is the analysis of sentences describing actions. The analogy between these two sorts is based on the fact that we have, in both areas, the apparent quantification over events.

Davidson offers the following: "... (the) 'it' of 'Jones did it slowly and deliberately...' seems to refer to some entity presumably an action, that is characterized in a number of ways."³¹ It is well known that apparent quantification in ordinary discourse is a poor indicator of deep onto-

logical commitment. Davidson, realizing this, bases his argument on the need to analyze certain plausible inferences involving expressions of the sort mentioned rather than the use of the referring expression 'it'. The inferences that Davidson offers for analysis have an interesting property. Davidson quotes Kenny³² approvingly as having first noticed this property and dubs it, following Kenny's lead, the problem of the polyadicity of predicates. Consider the following:

(11) Jones buttered the toast in the bathroom with a knife at midnight.

(11.1) Jones buttered the toast in the bathroom.

(11.2) Jones buttered the toast.

Davidson maintains his analysis of event locutions as a method (the method) for offering a syntactic account of the fact that (11) implies (11.1), which in turn implies (11.2). He asserts, "... (the) sentences contain a common syntactic element ('butters') which we intuitively recognize as relevant to the meaning relations of the sentences."³³ Using the analysis of event locutions offered earlier in respect of (9) we can construct the following Davidsonian translation for (11):

(11.3) $(\exists x) (\exists y) (\exists z) (B_{xyz} \wedge Tx \wedge Ky \wedge Lz \wedge Mz)$

where ' B_{x_1, x_2, x_3, x_4} ' is the quaternary predicate to be read 'the event x_4 of x_1 buttering x_2 with x_3 '; whence the matrix is to be read 'the event z of Jones buttering x with y ' an obviously, ' x is toast', ' y is a knife' and ' z occurred in the bathroom (lavatory)' and ' z occurred at midnight' for

the other matrices. Accepting this account we now have for (11.1) and (11.2) the following:

(11.4) $(\exists x) (\exists y) (\exists z) (Bjxyz \wedge Tx \wedge Lz)$

(11.5) $(\exists x) (\exists y) (\exists z) (Bjxyz \wedge Tx).$

And that, of course, affords us an analysis of the intuitive inference relations since (11.4) and (11.5) both follow from (11.3) by a proof in quantificational logic.

Davidson admits that if, for example, (11.2) could be construed as elliptical for:

(11.6) Jones buttered the toast (with a knife) somewhere and somewhen. The inferences go through without quantifying over events and without having to add an extra 'event place' to the standard predicate. And, of course, the inferences do go through in many plausible reconstructions of space-time locutions in fairly natural extensions of quantification theory without the added apparatus to take care of events as a syntactically distinct class of particulars.³⁴ Davidson, however, follows Kenny in rejecting an analysis of the sort that marks (11.6) as the full account of the elliptical (11.2) and does this on the grounds that "...we could never be sure how many standby positions to provide in each predicate."³⁵ Whence the label 'polyadicity of predicates'.

There seem to be two available replies to Davidson. First we can see, in a sense, that he is correct in asserting "...we cannot view verbs...as usually containing a large number of standby positions..."³⁶, if he means, in general and a priori. It is not clear, however, that we cannot decide

the issue a posteriori, and this last in the following sense. Although the analysis of event locutions is not a priori necessary, working from the success of physicalist explanations, we can say that all that is needed for descriptions of events in physicalist language is a function that maps sentences into quadruples of real numbers standing for Cartesian coordinates. Predicates like 'in the bathroom', 'at midnight' are then, presumably, definable in terms of such a scheme. Adverbial modifiers like 'slowly' are more difficult to handle and remain unresolved on Davidson's account as well. In an account such as the one just mentioned, 'slowly' would be analyzed in terms of difference between quadruples of coordinates.³⁷

Of course, Davidson's concern is with actions and it is not surprising, given the sorry state of Psychology that no adequate conceptual scheme, sufficient to localize actions, is a posteriori describable based on the actual practices of psychologists. Theorists of action must therefore "muddle through" the best they can. Davidson's scheme for quantifying over actions is thus a device, born in adversity, in lieu of an appropriate scientific analysis. The point is, however, that this is not true of events in general. If we are willing to deviate from the usual methodological stance in the spirit of paying due respect to actual practice there are few proposals for locating events, Newtonian vs. Relativistic space. Although it might be difficult to choose between them, we can, in principle, describe the logic of

such spaces and map our sentences describing events into topological descriptions of these spaces in the usual manner.

Similar direction is not forthcoming from the conceptual schemes we use to describe action, the Law, Psychology, etc. An analysis of how events, in some significant sense, are located within these latter disciplines furnishes a bewildering array of ad hoc procedures. But the very fluidity of the conceptual basis seems to preclude the availability of these disciplines for drawing conclusions at the level of ultimacy required for ontology. It is just in areas like Ethics that the surface grammar of utterance is least available as a source of data for ontological analysis.

The second reply to Davidson's argument meets his contention head on. Does (11) imply (11.2) rather than, say, (11.6)? Let us look more closely at (11.2). 'Jones buttered the toast with a knife', In the standard account of the truth conditions for (11.2) the sentence is true if, and only if,

$$\langle \text{Jones}, a, b \rangle \in \{ \langle x, y, z, \rangle : x \text{ buttered } y \text{ with } z \}$$

But, although stateable, the truth conditions for the sentence (11.2) are not adequate for the truth of the various statements (11.2) could be used to make. For (11.2) is an ephemeral statement.³⁸ That is to say (11.2) could be used to make various statements of divergent truth values. The truth conditions for (11.2) are thus inadequate to meet the fact that (11.2) could be false if made in respect of Jones

at midnight in the living room. We are then faced with the fact that there is a construal of (11.2) such that (11) is true and (11.2) is false. But that is just to say that (11) does not imply (11.2). Notice that if (11) is elliptical for (11.6) the counter-example breaks down and the implication goes through.

Granted the argument I present is not without possible rejoinder. But the point can still be made. If the move from (11) to (11.2) is the basis for the ontological commitment, it is not clear that the implications are clear enough to warrant the claim that they demand an implicit event language to mark them as valid. This last in conjunction with the earlier arguments brought forward against Davidson and Kim seems sufficient to free ourselves from the necessity of accepting the event ontologists' position in respect of the inadequacy of the standard meta-theory of scientific practice, that is to say D-N like construction couched in first order logic that mark deducibility as the fundamental analysis of scientific explanation.

Having dealt with the arguments against the standard meta-theory of scientific practice, relevant to our concerns, we now move to the analysis of the correspondence relation as explicated within the standard discussion of the reduction relation between scientific theories.

FOOTNOTES

Chapter II

¹If causes relate events, this is the standard view of Braithwaite, Scientific Explanation (London: Cambridge University Press, 1953). See N. Rescher, "Lawfulness As Mind Dependent," Essays in Honor of Carl G. Hempel (New York: Humanities Press, 1970) for a recent statement that traces out one extreme.

²D. Davidson, "Causal Relations," Journal of Philosophy, (1967) Volume 64, Number 21.

Idem, "The Individuation of Events," Essays in Honor of Carl G. Hempel, ed. Rescher (New York: Humanities Press, 1970)

J. Kim, "Events and Their Descriptions," Essays in Honor of Carl G. Hempel, ed. Rescher (New York: Humanities Press, 1970)

Idem, "Causation, Nomic Subsumption and the Concept of an Event," Journal of Philosophy, (1973) Volume 70, Number 8.

³D. Davidson, "Causal Relations," p. 702.

⁴Ibid., p. 695.

⁵Kim, "Causation, Nomic Subsumption...", p. 222.

⁶Ibid., p. 222.

⁷Carl G. Hempel, Aspects of Scientific Explanation (New York: MacMillan, Inc., 1965).

⁸Ibid., p. 336. Italics mine.

⁹It is not obvious what the relevance of an analysis of ordinary language is to scientific discourse. This problem will be dealt with in detail in Chapter V below. Suffice it

to say here that Davidson as well as others take ordinary language to be informative as a tool for the analysis of scientific languages.

¹⁰Davidson, "Causal Relations," pp. 694ff.

¹¹Sentence indices hold within chapters only.

¹²D. Follesdal, "Quantification into Causal Contexts," Boston Studies in the Philosophy of Science II, eds. Cohen and Wartofsky (New York: Humanities Press, 1965) or W.V. Quine, Word and Object (Cambridge: M.I.T. Press, 1960), pp. 197-98.

¹³Notice we, shamelessly, put ' $\supset$ ' for 'caused' not because 'cause' means ' $\supset$ ' but rather if 'p causes q' then surely 'p $\supset$ q'.

¹⁴Kim, "Causation, Nomic Subsumption...", p. 219, especially fn. 4.

¹⁵We use 'I' as the description operator in lieu of the inverted iota.. The easiest way to see the validity of the move to (3.3) is to follow the example of our analysis of the move from (2) to (2.1). The situations, although not identical, are relevantly similar.

¹⁶Kim, "Events and Their Descriptions."

¹⁷Ibid., p. 206.

¹⁸Ibid., pp. 207-8.

¹⁹Davidson, "Causal Relations," p. 692.

²⁰Ibid., p. 699.

²¹Ibid.

²²Kim, "Causation, Nomic Subsumption..."

²³Ibid., p. 231

²⁴Ibid., p. 227

²⁵Ibid.

²⁶Ibid., pp. 235-6.

²⁷Davidson, "The Logical Form of Action Sentences," The Logic of Decision and Action, ed. Rescher (Pittsburgh: University of Pittsburgh Press, 1966), p. 92.

²⁸Ibid., p. 92.

²⁹Kim, "Causation, Nomic Subsumption....," p. 222.

³⁰E. Mendelson, Introduction to Mathematical Logic (Princeton: D. Van Nostrand Co., Inc., 1964), p. 162.

³¹Davidson, "The Logical Form", p. 81.

³²A. Kenny, Action, Emotion and Will (London: Rutledge & Kegan, Paul, 1963), ch. 7.

³³Davidson, "The Logical Form....," p. 83.

³⁴See N. Rescher, Topics in Philosophical Logic (Holland, Netherlands: D. Reidel, 1968), Chapters 12 and 13 for examples of systems with fairly standard semantics sufficient to make the inferences from (11) to (11.6).

³⁵Davidson, "The Logical Form....," p. 83.

³⁶Ibid., p. 83.

³⁷This is an optimistic stance. What I have in mind here is having a standard rate, r , and a standard interval, I , so that a rate r' is deemed slow if it exceeds I . The rate would itself be defined in terms of a starting place and an ending place and a time interval. The whole issue is fraught with difficulties. 'Slow' is applied equivocally to

different phenomena, there is an unwarranted assumption of constancy of rate during the time interval, etc. Davidson (loc. cit.) sees the same problems and fails to indicate how the device of adding predicate places for events resolves the issue.

³⁸See Brody, Logic: Theoretical and Applied (New Jersey: Prentice Hall, Inc., 1973) for the distinction between 'sentence' and 'statement' as well as the analysis of 'ephemeral' as used here; especially Chapter 4. Briefly, an ephemeral sentence is one for which the truth value changes as a function of the context of utterance. A statement is a sentence free of contextual ambiguities, whence the appropriate bearer of a unique truth value.

CHAPTER III

To recall our introductory remarks, the point of the preceding was to rehabilitate, at least vis a vis relevant alternatives, the standard meta-theory of science. This was in order to apply powerful model theoretic techniques, available within that meta-theory to the notion of correspondence. As alluded to earlier, it is usual in the literature of the MBIT to assume some correspondence, usually 'one-one' between phenomenological terms and terms referring to sets of neurophysiological entities.

Within the context of the MBIT, the notion of 'one-one' correspondence is of essentially metaphoric content. If the claim is warranted by the relation of the mental and the physical in some scientific sense, some notion of the scientific meaning of these terms must be afforded. Similarly, the notion of 'one-one' correspondence must be analyzed within such a scientific framework as well. The following gives a rough sketch, consistent with the basic framework of Logical Empiricism, of such an analysis.

Terms of whatever sort may have their meaning explicated in terms of the syntactic structures that mirror their semantic relations. Such a syntactic structure will manifest

'deductive' relations between terms by virtue of sentences in which these terms appear. We call such a syntactic structure a theory and presume it axiomatizable (whence axiomatized). We note at the outset that such an axiomatization constitutes what is usually referred to as a partially interpreted calculus. The interpretation of such a calculus does not exhaust the possible meaning relations between terms. Whence the open-texturedness terms despite the rigor of the formulation.

Such a structure does, however, give meaning to the terms while permitting the enrichment of such meaning via the potential for change. Such change, governed in the main by empirical demands, constitutes the ever shifting movement from analytic to synthetic that now is realized to be at the very heart of scientific growth.¹ Notice, of course, that an axiomatization is best construed as a snapshot of some interval of scientific practice. So, to axiomatize a set of terms via a theory is to commit oneself to a temporally bounded segment of a developing conceptual process.

The notion that we will turn to is that of reduction. Reduction as a relation between theories is the best candidate for the elucidation of the correspondence relation between terms. The MBIT's are probably more concerned with what, in the literature on reduction, is called 'reduction of branches (fields)'. We will assume, at least for a time, that the notions of reduction of theories and the reduction of branches are equivalent, that is, that either some particular

theory, at a time, constitutes a branch or that the various theories, when conjoined, form a theory. Later these assumptions will be discarded as the various scientific options upon which identifications may be grounded are examined with an eye to the realities of science de facto. Before we can go further, a closer look at the details of the empirical basis for the MBIT seems in order.

The situation as envisioned by the MBIT's in question is, roughly, as follows. There is available a body of knowledge consisting of statements of facts and using mental terms. We may understand such statements as being about persons and using a characteristic vocabulary to refer to states of minds of persons. It might be noted that the body of knowledge envisioned is variously construed. Most frequently it is thought of as consisting of phenomenological reports. These reports, however, are usually deemed to be meaningful by virtue of their use in social contexts which fixes the meanings of the terms employed. The phenomenological basis for mental terms is frequently tied to the notion of the reporting use of mental terms. Statements of the sort described seem to bear a strong resemblance to what is usually thought of as the observational base of scientific theories. The limiting of mental terms to statements of an observational character, though frequent in the MBIT, is fairly obviously inadequate as a description of what constitutes the class of mental terms. Statements describing mental items may be highly theoretic in character, as is 'Oedipus complex'.

Further, as is well known, mental terms frequently refer to dispositions rather than to manifest states.

In any event, the immediate point in relation to the MBIT is that such a body of knowledge is seen, via various scientific advances, to correspond to knowledge stated in some other theoretic language--most usually the language of neurophysiology. The correspondence between the mental and the neurophysiological should not be confused with the correspondence between phenomenological terms and the complexes of behavioral contexts that constitute the social basis necessary to fix the usage in a manner sufficient for phenomenological reports to constitute a language in any significant sense. It is rather that within the contexts that afford the meaningfulness of our phenomenological reports the references of such reports are shown to be identifiable with certain central states of the organism--these central states being the subject matter of the science of neurophysiology. These neurophysiological states and properties are then through some favored relation, usually 'one-one' correspondence, identified, as a matter of contingent fact, with the corresponding reported phenomenological phenomena.

Recall once more that although the literature on the MBIT is severely limited as to the choice of examples of mental terms, it seems a fairly natural extension of the sorts of situations envisioned to include in the MBIT claim that such theoretic terms as 'paranoia' and such dispositional terms as 'contented' be tied to the corresponding

neurophysiological states.²

In the MBIT literature the correspondences is deemed massive. In fact, it is usually asserted that for any mental term there is a complex description in neurophysiological language such that the situation as described neurophysiologically can be identified with the 'referents' of the mental terms. Alternatively, we have theories where there is a subset of mental terms sufficient to generate the mentalistic aspect of all other mental terms and these former are the only ones for which a correspondence is required. As is obvious, these two approaches are equivalent as far as the ability to give physiological descriptions for mental terms is concerned. In the second case we need give only a physical 'interpretation' to these 'primitive' terms; the others being thus interpreted as 'defined' from the others. There are problems here that any more detailed account of the MBIT must take into consideration. We, however, will disregard these problems entirely.³ It is on the grounds of this projected massive correspondence between mental designata and neurophysiological states and some 'meta-physical' principle, usually simplicity, that the identification of these rather disparate items is deemed warranted.

As mentioned earlier, the notion of correspondence, although central to the problem as discussed, has been almost totally unexamined by the theorists in question. Even the more scientifically oriented theorists disregard this crucial area. We find Feigl⁴ speaking in the most amazing manner of

a correspondence based on the evidence of a neural self-scanner by virtue of which a person can correlate his phenomenological presences with wave patterns from his brain. Clearly, the consequences of such a procedure are far from clear if for no other reason than that the background information--that is how these 'wave patterns' and 'presences' fit within a theory sufficient to give some understanding of each separately--seems more crucial than any correspondence. To ape the Wittgensteinian in this regard, the correlation between these phenomena, even if highly attested to, could mean anything, even nothing. We note in passing that this is a real issue in the psychological sciences. Classic experiments with arousal drugs within emotionally charged situations are almost totally underdetermining of any serious conclusion and just because the facts about the operating of arousal drugs are so far removed in their theoretic underpinnings from anything even vaguely resembling the subtlety and complexity of the 'theory' (in this case ordinary usage) governing our understanding of the emotions that little of consequence can be learned.

The point of the preceding is to orient the discussion in a direction, hopefully, more productive of clarification. If the preceding remarks are indicative of a philosophical bias, the bias is based on the notion that terms take their meaning from the complex linguistic situation within which they operate. Thus, it seems foolhardy to talk of correlating terms, or even phenomena, without facing up to the fact

that these terms or these phenomena are embedded in a matrix, whence to speak significantly of them is to speak of them in relation to this matrix.

In the following, therefore, we will take not terms simpliciter, but rather relational structures within which terms are to be found as the object of inquiry. That is to say we will deal with theories. This then shifts the objects to be identified. We will not speak of identifying the referents of terms in some dubious isolation but rather will talk of 'identifying' the ontologies of theories, that is to say their models.⁵ We put 'identify' in scare quotes for the following two reasons. Firstly, we are not looking for a set of premises from which an identity conclusion follows. Rather we are looking for such situations as might warrant an identification. Secondly, the relations between 'ontologies' of theories, or more aptly, models of theories, will turn out to be far different than that of identity.

We will speak first, in a purely abstract fashion of the relations between theories and their models when such theories can be said to be in the reduction relation. We will, in this context, attempt to clarify issues, trace consequences and compare paradigms. This will afford us some idea of the potential for warranting an identification. We will reserve till last any attempt to apply this abstract analysis to the specifics of the MBIT or, as we should now say, the possibility of the ontological reduction of the psychological to the neurophysiological.

A word about what follows. We will examine a number of basic views on reduction. That these views are the basic ones can be seen from a perusal of the second generation of the literature on reduction.⁶ These basic views include the semantic reduction paradigms of Quine⁷ and Suppes,⁸ plus the applications in Adams⁹ and the syntactic paradigm of Nagel¹⁰ and Kemeny and Oppenheim,¹¹ plus the generalization of these views in Eberle.¹² Lastly we will turn our attention to the criticism from Feyerabend, especially Feyerabend's "Explanation, Reduction and Empiricism."¹³

The first reduction paradigm to be examined is that of Quine. Constructed as a hedge against the Pythagorean tendencies of the Lowenheim-Skolem theorem, Quine denies that "what justifies the reduction of one system of objects to another is preservation of relevant structure."¹⁴ This is in light of the fact that, given Lowenheim-Skolem, "any interpretable theory can...be modeled in the natural numbers."¹⁵ Whence, modeling does not imply reduction in any significant sense. We note that given the proof of Lowenheim-Skolem after Kleene¹⁶ not only can we prove that any interpretable theory has a model in a domain whose cardinality is that of the natural numbers but that we can "produce a numerical interpretation that will fit S in case S is consistent."¹⁷ That is to say, we can construct a numerical interpretation; actually produce the numerical predicates. The effectivity of the construction takes us far from the minimal sense of modeling found in standard proofs of Lowenheim-Skolem.¹⁸ But,

as we shall see, it doesn't take us far enough, at least if we want to reduce in some ontologically significant sense.

Quine's paradigm is the following:

The standard of reduction of a theory T_2 to a theory T_1 can be put as follows. we specify a function, not necessarily in the notation of T_2 or T_1 , which admits as arguments all objects in the universe of T_2 and takes values in the universe of T_1 . This is the proxy function. Then to each n -place predicate of T_2 , for each n , we effectively associate an open sentence of T_1 in n -free variables, in such a way that the predicate is fulfilled by an n -tuple of arguments of the proxy function always and only when the open sentence is fulfilled by the corresponding n -tuple of values.¹⁹

Quine asserts:

The proof of the Lowenheim-Skolem theorem is such as to enable us to give the predicates of the numerical model; but the standard of ontological reduction that we have reached requires more than that. Reduction of a theory T_2 to natural members-true reduction by our new standard, and not mere modeling, means determining a proxy function that actually assigns numbers to all of the objects to T_2 and maps the predicates of T_2 into open sentences of the numerical model. When this can be done, with preservation of truth values of closed sentences, we may speak of reduction to the natural numbers. But the Lowenheim-Skolem argument determines, in the general case, no proxy function. It does not determine which numbers are to go proxy for the respective objects of T_2 . Therein it falls short of our standard of ontological reduction.²⁰

Given that our concern is ontological reduction as well, it becomes important for us to check Quine's claim that the ontologically significant aspect of the paradigm is the proxy function. To do this we will be forced to replicate a version of Klenne's proof, after Quine.²¹ Prior to that task, we must characterize Quine's paradigm in a somewhat more perspicuous manner.

Quine's notions, as well as the views of the other authors to be considered are couched within standard first order theories. Given the centrality of technical considerations to what follows we will interpolate a number of basic definitions of terms employed. The analyses offered are standard throughout and they afford a 'benchmark' against which the substantive proposals of authors mentioned can be compared. The definitions offered are from Van Fraassen.²²

A theory T is minimally thought of as a Syntax Syn , which itself is an ordered quadruple $\langle P, V, L, S \rangle$ where,

P is a non-empty denumerable set of predicates of all degrees n , $n \geq 0$

V is a denumerable set of variables disjoint from P

L is the set of logical, $\{ \supset, \neg, (,) \}$ disjoint from P and V and where S is the least set s.t.

(a) if $P^n \in P$ and $\langle x_1, \dots, x_n \rangle \in V$, $P^n_{x_1}, \dots, x_n$ belongs to S

(b) if x is a variable and A and B belong to S , so do $(\neg A)$, $(A \supset B)$ and $(x)A$.

A model for theory T is a couple $M = \langle D, f \rangle$ where D is the domain a non-empty set, f is a function (the interpretation function of M) defined for each predicate of T s.t. if P^n is an n -ary predicate then $f(P^n) \in D^n$.

A mapping, d , of variables of T into the domain of M is called an assignment function.

We say d satisfies A in M , $M \models A(d)$ (alternatively A holds, or is true in M) just when $\models$ is the least relation²³ s.t.

- (a) $M \models_{P^n} p_{x_1}, \dots, x_n (d) \text{ iff } \langle d(x_1), \dots, d(x_n) \rangle \in f(P^n)$
- (b) $M \models (A \supset B) (d) \text{ iff } M \not\models A(d) \text{ or } M \models B(d)$
- (c) $M \models \sim A(d) \text{ iff } M \not\models A(d)$
- (d) $M \models (x_1)A(d) \text{ iff } M \models A(d')$ for all assignments d' which are like d except perhaps at $x_1 (d' =_{x_1} d)$

If there is a chosen binary predicate $I^2 \in P$, the identity predicate we add

- (e) $M \models x_1 I^2 x_2 (d) \text{ iff } d(x_1) = d(x_2)$

Given the fact that Quine speaks of 'the universe of' a given theory, it is a plausible interpretation of Quine to take him as tying, at least within the context of reduction, a theory to some particular favored model. This sort of view, the 'one model view' is a natural view in many contexts, especially within the formal sciences, where the historical priority of some standard interpretation takes conceptual precedence over some, usually recently generated, axiomatization. As we shall see, this kind of position leads us into problems if we are to deal with the natural sciences, especially the reevaluation of the interpretation of a theory within the context of reduction. We will take Quine as an example of such a 'one-model view' without claiming any privileged information as to Quine's intentions. Suffice it to say that the position is certainly attractive enough to warrant close examination and further there is no hint in Quine's text to render such an interpretation manifestly inappropriate.

We take for a theory T ,²⁴ $T = \langle \text{Syn}, M \rangle$ where Syn is a standard first order syntax²⁵ and where M is favored model $M = \langle D, f \rangle$ for domain D and interpretation function f .

We now offer the following formal analyses of Quine's paradigm. We say:

$T_1 R_Q T_2$ if and only if

(1) there is a function, p , the proxy function, such that $p(d_2) = d_1$ for all d_2 in D_2 and for some d_1 in D_1 .

(2) for all n -ary predicates P_2^n of T_2 for all n and for some expression E_1 of T_1

$\langle d_2^1, \dots, d_2^n \rangle \in f_2(P_2^n)$ if and only if

$\langle p(d_2^1), \dots, p(d_2^n) \rangle \in f_1(E_1)$.

In our later development we will distinguish the 'only if' of condition (2) and refer to it as the 'converse' of (2).

We now turn to Quine's version of Kleene's proof of Lowenheim-Skolem. The proof shows, to quote Quine, "

that every consistent set S of quantificational schemata (i.e. every set of wff's of the lower predicate calculus admitting of a true interpretation in some non-empty domain admits of a true numerical interpretation (i.e. an interpretation of predicate letters such that all schemata of S come out true when the variables of quantification are construed as ranging over the positive integers.²⁶

Again, to quote Quine, the proof shows how "given S to produce a numerical interpretation which will fit S in case S is consistent."²⁷

The procedure for proof is as follows:

- (1) We start with a consistent set of sentences, S , in prenex normal form with an initial universal quantifier and with all variables distinct. the existence of S for all consistent sets of sentences is guaranteed by standard results of quantification theory. The members of S are thought of as $\langle \mu_1 \phi_1 \rangle, \langle \mu_2 \phi_2 \rangle \dots$ where the μ_i are strings of quantifiers, initially universal and the ϕ_i are quantifier free expressions, the matrix of the expression. We assign positive integers in any random fashion to the universal quantifiers.
- (2) On the basis of the assignment to the universal quantifiers we assign integers to the existential quantifiers using exponential primes in such a fashion as to guarantee some particular integer for every existential quantifier.²⁸
- (3) (1) and (2) can be performed for all possible assignments to the universal quantifiers and there are infinitely many ways. We assume these assignments ordered and speak of the 1st, 2nd, ... 'substitution sequence', i.e., assignment.
- (4) Each substitution sequence then generates, for every sentence in S , a sentence that is the substitution of the integer assigned to the quantifier for the variable contained in the respective quantifier. (Note that there is some particular integer for each existential quantifier for every assignment to the universal quantifiers.) Let this new sentence be ψ_m^n for a sentence with m -matrices under the n th substitution sequence. It is clear that every ψ_m^n is a truth function of some member of the infinite list $\omega_1, \omega_2, \dots$ where the ω_i

on the list are the result of 'applying predicate letters to numerals'.

(5) Since for every finite r we can assign truth values to $w_1, w_2, \dots$ and decide whether $w_1, \dots, w_r$ falsifies $\psi_m^1, \dots, \psi_m^n$, we say that an assignment to $w_1, \dots, w_r$ is innocuous relative to $\psi_m^1, \dots, \psi_m^n$ if the assignment does not falsify $\psi_m^1, \dots, \psi_m^n$ for any m and n .

(6) The main task is to show that we can construct an interpretation, $\mathcal{I}$, 'that fits S if S is consistent'.²⁹ We show that the following is such an $\mathcal{I}$:

'for each r , w_r is to be true iff there is an innocuous assignment to $w_1, \dots, w_r$ or it is the earliest, if there are several, that assigns truth to w_r '.³⁰

(7) The proof then proceeds via the following:

(a) for each m and n $\psi_m^1, \dots, \psi_m^n$ is consistent (Lemma 5)

(b) for each r there is an innocuous assignment to $w_1, \dots, w_r$ (Lemma 6)

But then since $\mathcal{I}$ is such that for each r , w_r is true just when $w_1, \dots, w_r$ is innocuous and w_r is assigned true, makes ψ_m^n true for all m and n (Lemma 7); whence,

(8) Thesis: for each m , $\mathcal{I}$ makes ' $\mu_m \phi_m$ ' true.

Prf. $\mu_1, \dots, \mu_m$ has the form

$\ulcorner (\alpha_1), \dots, (\alpha_a) (\beta_1), \dots, (\beta_b) (\gamma_1), \dots, (\gamma_c) (\delta_1) \dots (\delta_d) \dots \urcorner$

Hence truth of ' $\mu_1, \dots, \mu_m (\phi_1, \dots, \phi_m)$ ' (in the universe of positive integers) amounts by the meaning of quantification to this: for each choice of numerical substitutions for

$\alpha_1, \dots, \alpha_a$, there are numerical substitutions for $\beta_1, \dots, \beta_b$

such that for each choice of numerical substitutions for $\gamma_1, \dots, \gamma_c$ there are (and so on) such that $\phi_1, \dots, \phi_m$ comes out true. But, by the definition of substitution sequence, some substitution sequence for $\mu_1, \dots, \mu_m$ realizes any desired substitution on $\alpha_1, \dots, \alpha_a$ followed by some on $\beta_1, \dots, \beta_b$ followed by any whatsoever on $\gamma_1, \dots, \gamma_c$ followed by etc. So $\ulcorner \mu_1, \dots, \mu_m(\phi_1, \dots, \phi_m) \urcorner$ will be true if $\phi_1, \dots, \phi_m$ comes out true under substitution according to every substitution sequence for $\mu_1, \dots, \mu_m$; hence, if $\psi_m^1, \psi_m^2, \dots$ are all true. But by Lemma 7, makes $\ulcorner \mu_1, \dots, \mu_m(\phi_1, \dots, \phi_m) \urcorner$ true. Therefore $\mathcal{I}$ makes $\mu_m \phi_m$ true.³¹

Let us recall what the point of the presentation of the proof was. Quine claimed that it is the existence of the proxy functions, p , assigning integers to the members of the domain that rendered the ontological sufficiency of the reduction paradigm. What we now show is that given the proof of Lowenheim-Skolem we can effectively associate, for any model, integers such that if an n -tuple of the model of the domain of the function is satisfied by some predicate in an associated theory, the integers as values of the proxy function will be satisfied by the constructed numerical predicate. What we will not be able to prove is just exactly the converse of condition (2) in the definition if $T_1 R_q T_2$. Notice since we consider Pythagoreanism as the absurd limit of ontological reduction the failure of the converse becomes impor-

tant. This will be our first clue to the generation of a reduction paradigm of ontological import. One that will focus on the persistence of a model of the reducing theory in the set of models of all theories reduced. The development of this notion will be slow in coming. It begins with the reduction paradigm of Eberle, Chapter IV below and is finally presented in Chapter VI.

Quine's claim that what is crucial is the assignment of integers to the members of the domain of the model is surprising in that the following is immediate.

Theorem I: If M is a model, $M = \langle D, f \rangle$ where D is at most denumerable when there is a function, p , such that $p(d_i) = i$ for some unique integer i and for all d_i in D .

Proof: Trivially from the well ordering principle and the claim that D is at most denumerable. ■

Scholia: For the reader unfamiliar with certain basic notions in meta-mathematics; the well ordering principle asserts that any set can be 'ordered' by a relation that is transitive and where symmetry between pairs in the relation implies identity of the relata. Further, every sub-set of the relation R has an 'R-least element', i.e., a element that is in R to every other member of that sub-set. An example of a well order is the less than relation on the positive integers.

By the denumerability of the set D we mean that D is no larger than any sub-set of the positive integers. Since D can be well ordered we can assign a unique rank to every mem-

ber of D . We then assign integers to ranks in consecutive order whence we guarantee the existence of the function, p .³²

We now show that given the proof of Lowenheim-Skolem we can guarantee a function in the stronger sense spelled out in the following.

Theorem II: For any model $M, M = \langle D, f \rangle$ there is an effective procedure for defining a function, p , such that:

- (a) $p(d_i) = i$ for all d_i in D and unique integer i , and
- (b) for all n -tuple, $\langle d_1, \dots, d_n \rangle$ of members of D if $\langle d_1, \dots, d_n \rangle \in f$ then $\langle p(d_1), \dots, p(d_n) \rangle \in g$ for some interpretation function g and some numerical model $N, N = \langle I, g \rangle$.

Notice: we use 'numerical model' in the minimal sense of Quine, i.e., the domain is the domain of positive integers.

Proof: (i) Prior to the details let us recall that our notion of model, $M = \langle D, f \rangle$ is equivalent to the notion

$M = \langle D, \langle R_1^1, R_2^1, \dots, R_1^2, R_2^2, \dots, R_1^j, R_2^j, \dots \rangle \rangle$ where the R_i^j are relations of all ranks, j . That is because the interpretation function, f , takes as arguments ordered n -tuple of D whence by the thesis of extensionality the values under f are properties and relations.

(ii) We take any model M , now construed in the latter sense ((i) above) and construct a set of sentences, S , from M by the simple device of letting the elements of M name themselves. This simplifies orthography and guarantees immediately that M satisfies S , $M \models S$. We now take S and construct $T = S \cup \text{Cn}(S)$, i.e., we close S under the consequence relation. Notice $M \models T$ as well.

(iii) We know that each element of D or alternatively each name d_i can occur in many sentences of T , in fact infinitely many. This is trivially obvious since for any sentence, p , p implies $\sim p$, implies $\sim \sim p$ etc. But given the Axiom of Choice we can choose from these sets of infinitely many sentences some particular sentence. The sentence chosen 'represents' the set in a crucial way. This will be outlined immediately. Let the sentence chosen for element d_i be $(\dots d_i \dots)$. The set of all such sentences is the choice set for the sentences in T . This set is guaranteed to have at most one member for all of the elements in D . Notice if some name, d_j , occurs in no sentence in S we add the sentence ' $d_j = d_j$ '. Notice ' $d_j = d_j$ ' is in T anyway since $\Vdash d_j = d_j$, whence $S \Vdash d_j = d_j$.

(iv) Following standard procedures³³ we can write a sentence equivalent to $(\dots d_i \dots)$. The procedure is as follows. We add to the set of unary relations of T the set of relational symbols $\{N_1, N_2, \dots\}$ where for each d_i , d_i is the only member of the set of names in T such that $N_i d_i$. Semantically, $f(N_i) = \{d_i\}$. The various N_i 's can be thought of, intuitively, as the predicate 'is the name of'. Notice; if such a distinguishing predicate, or better, definite description, is not available we would ponder why the various d_i 's were thought of as distinct, in the first place. We then take the 'representing' sentence from our choice set for each d_i and write the following: $(\exists! x_i)(\dots x_i \dots \wedge N_i x_i)$.

Where $(\dots x_i \dots)$ is just $(\dots d_i \dots)$, our representing sentence, with ' x_i ' in all and only those places where ' d_i ' occurred before, and ' x_i ' a variable used nowhere before.

Given what ' N_i ' means it is clear that we can assert

$$\vdash (\exists! x_i)(\dots x_i \dots \wedge N_i x_i) \equiv (\dots d_i \dots)$$

Again, using standard procedures, we spell out, explicitly whatever assumptions are needed to guarantee that ' $N_i d_i$ ' is a definite description of element d_i . The device Quine uses in such a situation is to add to the stock of sentences of S , ' $(\exists x_i) N_i x_i$ ' and ' $\sim (\exists x_i)(\exists x_j)(N_i x_i \wedge N_j x_j \wedge x_i \neq x_j)$ ' for each member x_i and each predicate N_i .³⁴ Notice if we can't add these sentences to S without retaining S 's character as a statement of the relations on M , we wonder why the various d_i 's were thought distinct at all. If we call the set S with these added elements S' it is clear that S implies S' . But T implies S and S implies $(\dots d_i \dots)$ for each sentence and for each d_i , whence given the equivalence at the top of the page, $(\exists! x_i)(\dots x_i \dots \wedge N_i x_i)$ is in T for each i .

(v) Given our set of sentences of the form

$(\exists! x_i)(\dots x_i \dots \wedge N_i x_i)$ we can now perform the following.

Firstly, we rewrite them in pre-nex normal form. Secondly, we apply step (2) of Quine's proof of Lowenheim-Skolem, above. This step guarantees some particular integer for x_i .

Notice that in pre-nex normal form our sentence looks,

roughly, like this $(x_j)(\exists x_i)((\dots x_i \dots) \wedge (\dots x_j \dots \supset x_j = x_i))$,

so ' x_i ' occurs in an existential quantifier, whence is

assigned some particular integer.

(vi) We then assign the integer, i , assigned to ' x_i ' to the member of D called ' d_i '.

(vii) Now if $N = \langle I, g \rangle$ is a numerical model in the sense of Quine, then I is a set of integers. But it is now obvious that by the proof of Lowenheim-Skolem presented above, if p is a function effectively defined by the procedure (i) through

(vi) then if $\langle d_1, \dots, d_n \rangle \in f$ then $\langle p(d_1), \dots, p(d_n) \rangle \in g$.

Scholia: Theorem I is no news to anybody, especially Quine; but Theorem I guarantees, at most, numbers assigned to objects. What we have shown in the stronger Theorem II is that we can find an assignment that assign numbers in such a way as to guarantee the satisfaction of some numerical predicate under the function whenever a relation in any arbitrary model is satisfied by an n -tuple of arguments for the function. That is to say that for whatever model we pick we can replicate its 'structure', the structure of its relations, in the integers. The key here is the Axiom of Choice, for that gave us some sentence that could stand in place of any element of the model. Whence we could 'plug in' Quine's proof of Lowenheim-Skolem. Notice that the predicates (relations) for N are not any particular numerical relations, e.g. arithmetic relations. The incredible generality of this construction points to the irrelevance of Quine's proof for more significant assignments of numbers to things, that is to say, measurements. This tack will be picked up shortly.

Our construction, so far, shows us that we can satisfy the conditions for a proxy function up to a point. What we

cannot guarantee, however, is the converse of (2) in the definition of $T_1 R_q T_2$, that is, we cannot be sure that if $\langle p(d_1), \dots, p(d_n) \rangle \in g$ then $\langle d_1, \dots, d_n \rangle \in f$. This is obvious once we realize that the proxy function, p , is merely a map from D into $\mathcal{I}$. The following construction satisfies that weak claim and demonstrates the independence of the converse of (2) from the other conditions. Let our domain, D , be $D = \{a, b\}$ where a and b are distinct objects. That is, a is not identical with b . Further let $p(a) = i$ and let $p(b) = i$ as well. Let R be the identity relation among individuals of the type in D and let S be the identity relation between integers. Clearly if $\langle a, b \rangle \in R$ then $\langle p(a), p(b) \rangle \in S$. But it is equally clear that $\langle p(a), p(b) \rangle \in S$ and not $\langle a, b \rangle \in R$. Notice, of course, that the counter-example fails if our proxy function, p , is 'one-one'.

Quine has asserted, "Goodman...[in The Structure of Appearance] ...called for an isomorphism thereby requiring one-to-one correspondence between the old objects and their proxies. I prefer to let different things have the same proxy. For instance, n is needed as proxy for both $n^\circ C$ and n -meters".³⁵ But there is a better reason for stopping our maps short of a one-one correspondence. And that is if the proxy function is one-one, given Theorem II above, any theory with a model of denumerable cardinality reduces under that interpretation, to the positive integers.

Theorem III: If $M = \langle D, f \rangle$ and $N = \langle I, g \rangle$ and if there is a function, p , where p is one-one from D into I then for all n -tuples in D , $\langle d_1, \dots, d_n \rangle \in f$; $\langle d_1, \dots, d_n \rangle \notin f$ if and only if $\langle p(d_1), \dots, p(d_n) \rangle \in g$.

Proof: (i) 'If then' is immediate from Theorem II. For 'only if'; recall since p is one-one, $p(d_i) = p(d_j)$ implies $d_i = d_j$.

(ii) Given that for all d_i , $\langle d_1, \dots, d_n \rangle \in f$ then $\langle p(d_1), \dots, p(d_n) \rangle \in g$ we assume for reductio that for some n -tuple of integers $\langle i_1, \dots, i_n \rangle$, where for each k , $i_k = p(d_k)$, $\langle i_1, \dots, i_n \rangle \in g$ and $\langle d_1, \dots, d_n \rangle \notin f$.

Recall: since p is one-one for every d_i such that $p(d_i) = i$, $p^{-1}(p(d_i)) = d_i$ for p -inverse, p^{-1} .

(iii) Now given that p is one-one, p is an isomorphism from D into I . That is to say that D is isomorphic to some subset of I , I' .³⁶ In fact, $I' \cup g'$, where $g' = \{ \langle d_1, \dots, d_n \rangle, \langle p(d_1), \dots, p(d_n) \rangle : \text{if } \langle d_1, \dots, d_n \rangle \in f \text{ then } \langle p(d_1), \dots, p(d_n) \rangle \in g \}$ is a submodel of N , N' .³⁷ But

since N' is a sub-model of N , then for all expressions, A $N' \models A$ implies $N \models A$. But I' and D are isomorphic; whence, in particular, for all n -tuples $\langle d_1, \dots, d_n \rangle$ in D , $\langle d_1, \dots, d_n \rangle \in f$ if and only if $\langle p(d_1), \dots, p(d_n) \rangle \in g'$.³⁸ But the isomorphism relation is symmetric.³⁹ But then there is a function, q , from I' into D such that $\langle i'_1, \dots, i'_n \rangle \in g'$ if, and only if, $\langle q(i'_1), \dots, q(i'_n) \rangle \in f$, for i'_k in I' . Notice that by construction q is an isomorphism onto D . But $(p^{-1} \circ p)$ is such a function, where 'o' is composition of functions.

(iv) But for all i'_k , $i'_k = p(d_k)$, but for all $q(i'_k)$, $q(i'_k) = d_k$. But then if we substitute these identities into the statement mapping I' into D , (iii) above, we derive if $p(d_1), \dots, p(d_n) \in g'$ then $d_1, \dots, d_n \in f$. But since N' is a submodel of N all n -tuples in g' are n -tuples in g . Whence we draw absurdly, if $\langle p(d_1), \dots, p(d_n) \rangle \in g$ then $\langle d_1, \dots, d_n \rangle \in f$. ■

Given Theorem III and given the fact that every consistent theory has a model of cardinality equal to the positive integers, we have the following choice. Either we admit that every consistent theory ontologically reduces, in the sense of Quine, to the integers or we have to qualify which interpretations count as crucial for ontological reduction. Notice all the fact of denumerable models demands is that all theories reduce under some interpretation to the integers. But this raises a problem for the theorist. How do we disqualify such models if we want to withstand the extreme view of ontological reduction to the integers. There seems two plausible alternatives. Firstly, we can restrict the 'numerical interpretation' into which theories are mapped. Secondly, we can demand a stronger condition than the mere mapping of some model or other. Notice, Quine's (or better the 'Quineian') view that only alludes to some particular model obscures this issue by making it difficult to raise. Nowhere in Quine's position is there any mention of the fact that the reduction of a set of sentences, i.e., the interpre-

tation of that set, in the numbers amounts to more than the mapping of some favored model via the proxy function. We will return to this problem when we examine Quine again (Theorem IV below). But first we will turn to the first alternative: to speak of the restriction of the class of 'numerical' interpretation' to some interesting class of arithmetic models. This brings into our purview the position of Suppes, whose analysis of measurement offers a natural extension to the position of Quine we have been examining. But first a word of orientation.

Theories can be related to other theories in a variety of ways and for a variety of purposes. One of the most important of these ways of relating theories is to claim that one theory can serve in lieu of the second. So, for example, Quine says that an ontological reduction, "would enable us to repudiate the old objects (objects of the reduced theory) for all purposes and recognize just the new ones (objects of the reducing theory)."⁴⁰ There are weaker views, most especially situations where we can merely reinterpret the old theories for some purposes as being about some other set of objects. But more of that later.

One of the weakest senses of 'reduction' is that of mere modeling. That is to say the demonstration that one theory can replicate the 'behavior' of another theory in certain limited ways. In this last sense we find numerical theories, theories whose domain of interpretation are numbers, as the most usual reducers. A simple example is the mathe-

mathematical modeling of some phenomena as in the construction of a computer model for some economic situation. In this latter sense, numbers replicate the relations between things, thereby enabling us to deduce consequences at a high degree of efficiency. This mirroring of the structure and operations on a set of non-numerical objects in a set of numbers is the basic notion behind that of measurement. But to measure is not to claim that the things measured are numbers but rather that they are numerables. Clearly the reduction if not ontological but merely shows the availability of a 'translation' of a theory into a highly versatile and manipulable language.

To say that objects are numerable in the sense of Theorem II is to make the weakest of that sort of claim. For any set speaks about numerables at least on some interpretation. We demand, therefore, more than mere numerability: We demand, for a significant assignment of numbers to things, measurability, that is to say the assignment of numbers to things in some significant and interesting fashion.

Suppes has generated a reduction paradigm that is closely related to the sense of 'measurable' used here. A closer examination of Suppes' theory of measurement seems, therefore, to be in order. This will close the gap between the weaker view and the views to be examined thereafter, while clarifying the distinction intended in the foregoing.

The very generality of Quine's sense of 'numerical interpretation' precludes its being immediately identifiable

with the notion of measurement. Recall: a 'numerical interpretation' is one for which "an interpretation of predicate letters such that the schemata of S (for arbitrary set, S) come out true when the variables of quantification are construed as ranging over the positive integers."⁴¹ A way to get some sense of the generality of the procedures involved is to recall section (8) of Quine's proof of Lowenheim-Skolem where all that is required is that some numbers in a favored relation to the numbers assigned to variables within universal quantifiers be found. Clearly the set of relations so constructed are guaranteed little relevance to more important and useful numerical relations, for example the relations found within arithmetic.

Suppes offers two natural restrictions on the forms numerical predicates can take if a 'numerical interpretation' is to constitute a theory of measurement. The stronger of the two restrictions, defining what he calls 'extensive quantities' are "those for which a 'natural interpretation' of addition and concatenation can...be specified."⁴² Notice the sort of restrictions Suppes presents.⁴³ A 'natural' operation is, among other things, one found useful in the procedures of mathematizing, one having usual and plausible properties. Suppes chooses for extensive quantities those for which "numbers can be assigned uniquely up to a similarity transformation (that is multiplication by a positive constant)."⁴⁴ The point here as presented is an instance of the deep issue involved throughout the preceding and subsequent chapters.

That is to say, how do we generate pragmatic principles sufficient to well manage the use of apparatus of a highly general sort. Apparatus of the sort envisioned has had as instances first order logics, Chapters I and II and here the apparatus of numerical theories. Notice the solution is the same throughout. The methodologist can resolve issues when sufficient attention is paid to the pragmatic restriction inherent in actual practice. The very generality of the apparatus of the formal discipline permits the extension of this practice in many ways. It is the practice ultimately that determines which extensions be taken. Notice that we do not require the practice to be fixed at any given historical level. The point of general apparatuses is to permit practice to change. But the mere possibility of such change should not be considered a license to confuse issues. This point will be made in a variety of contexts in the chapters to follow. It is just presented here as a reminder in passing.

Suppes⁴⁵ gives a plausible definition of what he considers a theory of measurement. We offer the following lengthy construction.

If $s = \langle m_1, \dots, m_n \rangle$ is an n -termed sequence of positive integers, then a relational system $\mathcal{U} = \langle A, R_1, \dots, R_n \rangle$ is of type s if for each $i = 1, \dots, n$ the relation R_i is an m_i -ary relation. Two relational systems are similar if there is a sequence $\underline{s}$ of positive integers such that they are both of type $\underline{s}$. Suppose the two relational systems $\mathcal{U} = \langle A, R_1, \dots, R_n \rangle$ and $\mathcal{B} = \langle B, S_1, \dots, S_n \rangle$ are of type $s = \langle m_1, \dots, m_n \rangle$. Then $\mathcal{B}$ is a homomorphic image of $\mathcal{U}$ if there is a function f from A onto B such that, for each $i = 1, \dots, n$ and

and for each sequence $\langle a_1, \dots, a_{m_i} \rangle$ of elements of A , $R_i(a_1, \dots, a_{m_i})$ if and only if $S_i(f(a_1), \dots, f(a_{m_i}))$.
 ... $\mathcal{U}$ is embeddable in $\mathcal{B}$ if some sub-system of $\mathcal{B}$ is a homomorphic image of $\mathcal{U}$.⁴⁶

Notice if f is one-one then $\mathcal{U}$ and $\mathcal{B}$ are isomorphic. Suppes specifies further: "A numerical relational system is simply a relational system whose domain of elements is the set $\mathbb{R}$ of all real numbers. A numerical assignment for a relational system $\mathcal{U}$ with respect to a numerical relational system $\mathcal{R}$ is a function which imbeds $\mathcal{U}$ in $\mathcal{R}$."⁴⁷ Notice: "A numerical assignment is not required to be one-one."⁴⁸

Given this much, Suppes then offers the following definition: "...a theory of measurement is a class $\mathcal{K}$ of relational systems closed under isomorphism for which there exists a finite sequence $\underline{s}$ of positive integers and a numerical relational system $\mathcal{R}$ of type $\underline{s}$ such that all relational systems in $\mathcal{K}$ are of type $\underline{s}$ and imbeddable in $\mathcal{R}$."⁴⁹ Notice $\mathcal{K}$ is closed under isomorphism since "it is reasonable that no special set of objects be preferred."⁵⁰ That is, it is the structure of the set of objects not some inherent property of the members that renders the system $\mathcal{R}$ applicable to them.

The intuition behind Suppes' construction seems clear. We start out with a numerical relational system with some certain structural properties perhaps satisfying some plausible set of axioms as in footnote 43. The theory of measurement is thus the set of objects that have their relations in the same structure. Where 'same structure' is analyzed

in the standard way, that is in terms of mappings of entities that preserve the satisfaction (inclusion) of n-tuples of elements in corresponding relations. This guarantees that the behavior of the elements in the members of K replicate the behavior of the members in $\mathcal{R}$, at least in so far as the relation under consideration (that's the point of the sequence $\underline{s}$) is concerned.

The following is clear from the analysis Suppes offers for the notion of measurement: If M is a model in the set K , as before then $MRq\mathcal{R}$, that is any model in a theory of measurement reduces in Quine's sense to the relational structure that constitutes the numerical aspect of the theory of measurement. This should be sufficient as it stands to render Quine's analysis too general to be of any use as a paradigm for ontological reduction. That is, if we want to preserve the distinction between things measured and the items we use to measure, i.e. numbers.

Suppes gives a simpler statement of his position:

Given an axiomatized theory of measurement of some empirical quantity, the mathematical task is to prove a representation theorem for models of the theory which establishes, roughly speaking, that any model is isomorphic to some numerical model of the theory. The existence of this isomorphism justifies the application of numbers to things. We cannot literally take a number in our hands and 'apply' it to a physical object, say. What we can do is show that the structure of the set of phenomena under certain empirical operations is the same as the structure of some set of numbers under certain arithmetical operations and relations ...The great significance of finding such an isomorphism of structures is that we may then use all our familiar computational methods, as applied to the arithmetical structure, to infer facts about the isomorphic empirical structure.⁵¹

Notice, to prove a representation theorem is just to find the set K in the analysis of measurement above.⁵²

Before we continue with Suppes' more general theory of reduction, notice that there is an asymmetry in the embedding of the models in K , that is that all of the models of the 'reduced theories', i.e., the models of the empirical quantities, are embedded in $\mathcal{N}$ but not conversely. This is a clear analogue to the problem we discussed that led to our Theorem II. We remark it, again, as a clue for coming up with an analysis of a reduction relation that is of ontological import. Clearly, in so far as Suppes' analysis is appropriate for measurement it is inappropriate for ontological reduction. The relation between models of the reducing theory and the theory reduced will be the key to our analysis in Chapter IV and VI to follow. That is to say, we will require that the theory that reduces furnish a model for the reduced theories. It will not be that models of the reduced theories are in some favored relation to the reducing theory. This is counter-intuitive in the context of ontological reduction, since such a reduction is invariable the grounds for repudiating models of the reduced theory. Rather it is that certain models of the reducing theory be available as a significant model for the theory reduced. As we shall see in the forthcoming a simple way to guarantee models of the reducing theory for the reduced is to demand that the reducing theory imply the reduced. This in one move guarantees the availability of models of the former to the latter. As

it turns out the situation is complicated in relevant cases by discrepancies in the vocabulary of theories in the reduction relation. Prior to our analysis of such 'deductivist' reduction paradigm let us return to Quine's view and see how it fares in relation to the insight that theories that reduce should furnish models for the theories reduced.

As mentioned earlier, we take the Quineian position as implicitly defining a theory as a pair, $\langle \text{Syn}, M \rangle$, that is as a syntax and some particular model. On such an interpretation Quine's reduction relation, R_Q , preserves consequences in a particularly limiting sense. This is spelled out in Theorem IV to follow. Theorem V shows just how limited this sense of consequence preservation turns out to be.

Before we begin our precisising of the consequences of Quine's view, a word about where we are heading. As mentioned earlier, one of the strongest of the inter-theoretic relations is when a theory, T_1 , can replace another theory, T_2 , within the domain of applicability of the latter theory. This may happen because a model or models of the former is accepted as the preferred model or models of the latter or when the actual formalism of the former is held superior as an explainer of relevant phenomena to the formalism of the latter. These notions, although distinguishable, are themselves related in interesting ways, as we shall see. The theories, T_2 , to be reduced are appropriate candidates for reduction, as opposed to being discarded, only if they have been successful as explanations or unifiers in some domain.

Clearly if T_1 is to be a superior substitute to T_2 the job T_2 has been capable of doing should not be read from the roles of jobs to be done. So we demand as a fundamental desiderata of any reduction relation that in so far as T_2 is successful (or true) T_1 be equally successful.

If then the point of reduction is to, in some sense, show the eliminability of T_2 we want to show that whenever T_2 yielded an expression, A , we could substitute a T_1 analogue for it. We now offer the following definition of 'analogue'. A sentence $a(A)$ of T_1 is an analogue of an expression A of T_2 if and only if

- (i) $T_1 R_Q T_2$
- (ii) If A is an atomic sentence (an expression free of all logical signs), $P_2 x_1, \dots, x_n$; $M_2 \Vdash P_2 x_1, \dots, x_n(d)$ iff $\langle d'(x_1), \dots, d'(x_n) \rangle \Vdash a(A)$, where $d'(x_i) = p \circ d(x_i)$ for proxy function, p , and where 'o' is composition of functions. Notice $\langle d'(x_1), \dots, d'(x_n) \rangle \subseteq D_1^n$ or alternatively $\langle d'(x_1), \dots, d'(x_n) \rangle$ is a member of $f_1(a(A))$.
- (ii) If $A \Vdash (x_1)B$ then $M_2 \Vdash A$ if and only if $d''(x_1) \Vdash a(A)$ where $d''(x_1) = p \circ d^*(x_1)$ for all assignment functions d^* of T_2 such that $d^* =_{x_1} d'$
- (iii) If $A = (B \supset C)$ then $M_2 \Vdash A$ if and only if $M_1 \not\Vdash a(B)$ or $M_1 \Vdash a(C)$
- (iv) If $A = \sim B$ then $M_2 \Vdash A$ if and only if $M_1 \not\Vdash a(B)$
- (v) If there is an identity predicate, I , and if $A \equiv x_1 I x_2$, $M_2 \Vdash x_1 I x_2$ if and only if $d'(s_1) = d'(x_2)$.

Given that much we now show the following:

Theorem IV: If $T_1 R_q T_2$ for all sentences A of T_2 , $M_2 \Vdash A$ if and only if $M_1 \Vdash a(A)$

Proof: Assume $M_2 \Vdash A$ and $M_1 \nVdash a(A)$ but then $M_2 \Vdash \sim A$ which is absurd. Conversely assume $M_1 \Vdash a(A)$ and $M_2 \nVdash A$ but then $M_2 \Vdash \sim A$ but then $M_1 \nVdash a(A)$ which is absurd. ■

Given the discussion immediately preceding the construction we can see the desirability of the result. Notice that the theorem proves the converse that was missing in the reduction of arbitrary theories to 'numerical interpretations'. That is to say that, unlike reduction to numerical theories, whenever a T_1 expression from a limited class (the class of T_2 analogues) is asserted we can move to a statement of T_2 . So to precise our intuition, if an expression of Quantum Mechanics is asserted, it being an analogue of some statement, in say, Chemistry, given the reduction, we can use the Chemical expression without fear of confuting Quantum Mechanics--at least in the limited sense to be discussed immediately.

Although Theorem IV is desirable it is quite limited in scope. That is to say, the 'equivalence' of A and a(A) is limited to the models that implicitly define the theories in question. That is, of course, because the notion of theory used was $T = \langle \text{Syn}, M \rangle$. Now this seems desirable in say, formal theories from which Quine has drawn his paradigm.⁵³ Given the historical development of the formal sciences, the intended model invariably pre-dates the formalism which

codifies practice. The same is not the case in the special sciences. Even if we deal with some special model and even if this model is deemed crucial in some Campbellian sense⁵⁴ it seems clear that in the special sciences a theory is not tied to some favored interpretation. This is, of course, only if we rule out instrumentalism as an account of scientific theories. But within the context of this discussion instrumentalism is irrelevant. And this, because we are interested in ontological reduction, that is the reduction of one theory to another that has ontological consequences. Instrumentalism renders a discussion of this sort superfluous since instrumentalism maintains that theories have no ontic force whatsoever, whence, a fortiori, switching from one theory to another can not force an ontological recommitment.

In the special sciences the situation is rather different. A body of data is organized via a set of laws, these latter being syntactic entities. The laws then, themselves being possibly organized via a theory. Whether that further theory exists or not, the syntactic statement of the data can be considered as a theory of the data in the technical sense explicated in Chapter I and earlier in this chapter. The theoretical account may then be tied to some other model, however what remains invariant in these circumstances is the codification of data via the laws (theory). Notice the notion of 'invariant' is itself relative in that new data or even new theoretic understanding may force us to reject the

data from which the theory is generated. However, we may disregard such a shift in this context.

In the standard account, and this is the account for whose adequacy we have been arguing throughout, it is this theory and its consequences that are the crucial notion for the analysis of explanation and confirmation. These latter are the central concern of any meta-theory of scientific practice. This view, which might be labeled as demanding the primacy of syntax as opposed to, say, interpretations of syntax, has at its core the possibility of the reinterpretability of these syntactic components. But this notion of the possibility of reinterpreting the syntax demands that the theory, thought of as a purely syntactic entity not be tied to one particular model. This is crucial to our concern since we will take ontological reduction as inherently involving the possibility of rejecting some model of a theory in light of the persistence of some other model drawn from some extremely fecund reducer. (This view is developed in Chapter VI below.) Notice Quine himself must admit that a theory is not inherently tied to some interpretation, if only because of the examples he takes as paradigmatic. So, if what we can decide is that, for example, numbers are no more than sets, our decision is only possible given that an axiomatization of, say, arithmetic permits of both a set theoretic as well as a Platonist interpretation.

If we take the consequence class of a theory as its most important element we should demand that T_1 preserve

the consequence relations found within the theory, T_2 , for which it is a reducer. It seems therefore that what is required is a strengthening of the sense of analogue relation as discussed prior to Theorem IV. That is to say, we take as a minimum desideratum for any theory in the reduction relation, T_1 , that it be capable of 'doing the job' that T_2 has done heretofore. Since the job done by theories is part and parcel of the consequences of that theory. We demand that T_1 have analogues for all of the T_2 consequences. That this is not true of Quine's reduction paradigm can be easily shown.

Theorem V: If $T_1 R_q T_2$ and $T_2 \models A$ then it is possible that $T_1 \not\models a(A)$. We demonstrate the theorem by presenting the following, artificial, yet appropriate example.

Let $T_1 = \langle \text{Syn}, M_1 \rangle$ and be axiomatized by the set of sentences $\{ (x) R_{xx} \}$.

Let T_2 be analogously defined and be axiomatized by the set $\{ (\exists! x) P_{xx} \}$.

Further, let $M_1 = \langle D_1, f_1 \rangle$ have $D_1 = \{1\}$ and f_1 be defined such that $f_1(R) = \{ \langle 1, 1 \rangle \}$. $M_2 = \langle D_2, f_2 \rangle$ has $D_2 = \{a\}$ and f_2 is defined such that $f_2(P) = \{ \langle a, a \rangle \}$. With proxy function p , defined as $p(a)=1$ it is clear that $\langle a, a \rangle = f_2(P)$ if and only if $\langle p(a), p(a) \rangle = f_1(R)$, that is to say $T_1 R_q T_2$.

It is also clear that if A is $(\exists! x) P_{xx}$ then $a(A)$ is $(\exists! x) R_{xx}$ and therefore $M_1 \models a(A)$ if and only if $M_2 \models A$

as is expected. However, and this is crucial, let

$M' = \langle D', f' \rangle$ where $D' = \{1, 2\}$ and where f' is defined so

$f'(R) = \{ \langle 1, 1 \rangle \}$ and $f'(P) = \{ \langle 2, 2 \rangle \}$.

that $f'(R) = \{ \langle 1, 1 \rangle, \langle 2, 2 \rangle \}$, M' is a model of T_1 .
But as is obvious $M_1 \not\models a(A)$ since $1 \neq 2$. ■

Although the construction is artificial the lesson behind the construction is not. Since we can construct M_1 and M' such that M' and M_1 are not isomorphic we can always find for any non-categorical theory, that is a theory all of whose models are not pair-wise isomorphic, models that fail to preserve analogues of the consequences of some reduced theory. And, of course, the theories that one would deal with in any significant application of reduction to the special sciences would not be categorical.

As we have been maintaining Quine's reduction paradigm or to recall our 'hedge' the Quineian one-model view, breaks down because of the tying of theories to some favored model. If we are to guarantee consequence preservation under reduction we must use a broader notion of theory. Suppes, whose analysis of measurement we had been analyzing in respect of the problem of reduction to highly general theories, has presented in rough outline an analysis of reduction that he feels is appropriate to the special sciences: "...the thesis that biology may be reduced to physics would be in many people's minds appropriately established if we could show that for any model of a biological theory it was possible to construct an isomorphic model within physics."⁵⁵

There are two new elements here. Firstly, we now talk about isomorphisms between 'any' model of the reduced theory and some favored model of the reducing. Notice, the model

from Physics is a model 'within' Physics, that is to say a model that is part and parcel of what Physics is to be taken to be. Quine, of course, had spoken about some particular model only. In this sense Suppes is a clear generalization of the Quineian procedure. Secondly, instead of some precise definition of the map from model to model we find the vague 'possible to construct'. This vagueness is replicated in other places as well.⁵⁶ "To show in a strong sense that Thermodynamics may be reduced to Statistical Mechanics we... show that any model of Thermodynamics we may find a model of Statistical Mechanics on the basis of which we may construct a model isomorphic to T."⁵⁷ Notice that here Suppes places no special condition on the model of Statistical Mechanics chosen. This is a crucial ambiguity in Suppes. We will, however, take Suppes in the limited sense found in the former quotation. That is to say, the model for the reducing theory must be special in some way, a model 'in' the theory.

Suppes is uncommitted as to how we are to go about constructing the relevant model, that is to say what limits are to be placed on the construction. He does, however, quote approvingly the result of Adams.⁵⁸ "An exact result of this character can be established for one branch of Physics in relation to another. An instance of this is Adams' (1959) result that for a suitable characterization of rigid body mechanics is isomorphic to a model within simple particle physics."⁵⁹

Before examining Adams' result with an eye to the flex-

ibility of the constructions possible. Let us characterize Suppes' paradigm in a more precise and conspicuous fashion. So we say $T_1 R_S T_2$ if and only if for any model M_2 of T_2 there is some model M^*_1 , special in some way, of T_1 such that M_2 is isomorphic to M^*_1 . M^*_1 is said to be a model in T_1 . We notice that M^*_1 may be chosen in a variety of different ways. For example, it might be the model envisioned by the original proponents of T_1 or, alternatively, it might be a model chosen by the proponents of T_2 in order to show the reduction possible.

We notice immediately that if M^*_1 is an arithmetic model the set of M_2 's with M^*_1 constitute a theory of measurement. We notice further that for each M_2 , $T_2 R_Q T_1$ with $T_1 = \langle \text{Syn}, M^*_1 \rangle$. This follows immediately since the isomorphism between the sets is sufficient to guarantee the proxy function's existence. Of course, finding the proxy function is another matter since short of very general theories as reducers, for example 'numerical interpretation' the finding of such a function is a matter of tremendous endeavor. "A satisfactory general definition of isomorphisms for two set-theoretical entities of any kind is difficult if not impossible to formulate. The standard mathematical practice is to formulate a separate definition for each general kind of ordered n-tuple."⁶⁰

Before leaving our general remarks about Suppes' view notice that Suppes' position places a limit as to how we can construe the model(s) in T_1 . So if we are to persist in

the intuition that the favored model(s) is one model in particular it follows immediately from the transitivity of isomorphism that if $T_1 R_s T_2$ then T_2 is categorical. That is to say that all models of T_2 are pair-wise isomorphic. The problem here is that it is well known that none save the most elementary of mathematical theories are categorical. From this we derive the following dilemma. Either the models in T_1 are more than a singleton or there are no significant Suppes reducers.

As remarked earlier the insight behind Suppes' view stems from the notion of a 'representation theorem', a notion that, as a perusal of the analysis of measurement above, seems natural within the formal sciences and especially within the notion of applying a general language, for the purposes of manipulation, to some more or less general apparatus. Again, to recall our earlier remarks. We said that in so far as an analysis is adequate to analyze measurement, to that extent was such an analysis irrelevant to what we have been calling 'ontological reduction'. And this was because of the simple distinction between being numerable and being a number. Presumably many things are the former without being the latter. This line merely casts suspicion on the relevance of Suppes' work for our concern.

A series of results by Koslow⁶¹ give us further grounds to wonder as to the appropriateness of Suppes' paradigm. It is almost truistic within the mainstream of methodologists

that explanation relations are inherently deductive. This, of course, is the posture retained here throughout. Similarly in theorists like Nagel, whose contribution will be examined in the following chapter, reduction relations, being a species of explanation, are fundamentally deductive as well. Koslow's result shows that "the contribution that Suppes-style reduction has to offer among theories deductively linked is nonexistent....(given an extremely 'plausible' assumption)...(or) meager." Koslow's position is prohibitively complex for the body of the text and we reproduce it in Appendix B. The result shows, with the weaker result first that either all 'chains' of reducing theories, that is theories such that $T_1 R T_2, T_2 R T_3, \dots, T_{n-1} R T_n$ reduce to one pair, with R as Suppes' reduction and if T_{j-1} implies T_j (Theorem 4, Appendix B) or to one theory (Theorem 7, Appendix B).

Although this result seems untoward in the extreme, we can rehabilitate what Suppes says if we take it as spelling out reduction in the sense appropriate to the application of numbers to things, that is to say as an explanation of measurement. We can do this without fear of the application of Koslow's results since there is no reason to suppose that the various numerical theories used as the basis for measurement imply the theories that they are the measure of. That is the interpretation we place on Suppes' work. Suppes' reduction paradigm is thus seen as a generalization of certain plausible ideas generated from the experience of mathe-

maticians in respect of showing the availability of some language to others. Suppes, himself, is not overly interested in reduction. The paradigm presented here is generated from a few, almost peripheral, remarks in Suppes' corpus. Suppes is clear, however, in his support of Adams' analysis found within the reduction of a pair of theories from Physics. So, if we are to look for a reduction analysis, in the style of Suppes, with manifest relevance for the natural sciences, it is to Adams that we must turn.

Adams asserts: "...a theory will be construed as an ordered pair of sets $T = \langle C, I \rangle$ s.t. C is the set of entities satisfying the axioms and I is the set of intended models."⁶² Reduction is then minimally analyzed⁶³ as "Condition A. Let $T_1 = \langle C_1, I_1 \rangle$ and $T_2 = \langle C_2, I_2 \rangle$ be two theories s.t. T_2 is reduced to T_1 by relation R , then for all i_2 in I_2 there exist i_1 in I_1 s.t. $i_2 R i_1$ "⁶⁴ and further, "Condition B: Let $T_1 = \langle C_1, I_1 \rangle$ and $T_2 = \langle C_2, I_2 \rangle$ be two theories s.t. T_2 is reduced to T_1 by relation R , then for all c_1 and c_2 if c_1 is in C_1 and $c_2 R_{c_1}$ then c_2 is in C_2 ."⁶⁵

Condition A of the paradigm is clearly an extension of Quine in just those cases where R is strong enough to be structure preserving in some relevant sense. In fact if R is R_q then $T_1 R_q T_2$ implies, trivially, that Condition A is satisfied. What Condition A allows for some M_2 at least is, after Suppes, that the mapping from T_2 to T_1 be more complex than Quine envisions. A look at accepted instances of reduction, e.g. Nagel's⁶⁶ account of the reduction of thermo-

dynamics to statistical mechanics show the complexity of the relation between entities of T_2 and entities of T_1 . In fact, even the most casual examination of accepted instances shows that a reduction paradigm that limited mappings of individuals to individuals in the sense of Quine would be non-instantial. Most importantly, for our major concern, the most recent work in, for instance, localization of brain functions, points away from the correlation between psychic events and a neurological condition defined in terms of simple neurons.⁶⁷

Once we realize that a proxy function after Quine, i.e., one mapping individuals onto individuals, is of dubious value we are faced with a serious problem. There just may not be predicates available in T_1 at any given time to yield the analogues of the desired sentences in T_2 . A simple example points this up. We take two theories with domains of equal cardinality--that is at least consistent with the claim that these models have a definable isomorphism between them, and show how, if the mapping is not individual to individual, we can easily make model expressions non-expressible in T_1 as originally stated. We return to our earlier definition of a theory and let $T_1 = \langle D_1, f_1 \rangle$ and similarly for T_2 . We set $D_2 = \{a, b, c, d\}$, $D_1 = \{f, g, h, i\}$ and set $f_1(R) = \{\langle f, i \rangle, \langle g, h \rangle\}$ and $f_2(P) = \{\langle a, b \rangle, \langle c, d \rangle\}$ where R and P are the only predicates of T_1 and T_2 respectively. We then define a 'proxy function', p , s.t. $p(a) = \langle f, g \rangle$, $p(b) = \langle h, i \rangle$, $p(c) = \langle f, h \rangle$, $p(d) = \langle g, i \rangle$. Now let T_2 be axiomatized by

the sentence ' $Pab \wedge Pcd$ ' and let T_1 be axiomatized by the sentence ' $Rfi \wedge Rgh$ '. Under the function, p , the analogue of our T_2 sentence is some predicate R' s.t.

$\langle p(a), p(b) \rangle \in f_1(R')$ and $\langle p(c), p(d) \rangle \in f_1(R')$, but that is, of course a predicate s.t. $\langle \langle f, g \rangle, \langle h, i \rangle \rangle \in f_1(R')$ and $\langle \langle f, h \rangle, \langle g, i \rangle \rangle \in f_1(R')$. But, there is just no predicate R' in T_1 ; of course, we can invent one, but the point is there is not one available.

Like our earlier example, this one is artificially transparent. It does, like our earlier one, teach us an important lesson of quite general import. Once we weaken the restrictions on the mapping, if we retain, informally, the notion of analogues for sentences of the reduced theory, it is not clear that for any chosen T_2 sentence there is available an analogue in the reducing theory. One of the values of, e.g., the reduction of thermodynamics to statistical mechanics is that it tells us a new thing about statistical mechanics. That is, it furnishes us with a guide as to how statistical mechanics is to be developed to account for theories already understood. That is, it tells us what new predicates to add to our original stock. If we relate this to the MBIT; to reduce, or attempt to reduce psychological theories, laws, etc., to neurophysiology is not merely, or perhaps even primarily to aid the psychologist, for in so far as he has laws or theories his account of the phenomena is, at least to some degree, adequate. It is rather to aid the neurophysiologist by informing him of the direction that

his discipline must go if it is to be meaningful to the world as already understood.

This last line raises a host of substantive issues, the details of which are, fortunately, irrelevant to the level of abstraction on which our discussion is. If we say that Psychology, in whatever sense, reduces to Neurophysiology, we are certainly not talking about Psychology and Neurophysiology today. Rather we are talking about disciplines to be constructed in light of the plausibility of the reductions envisioned. The problems here are multifarious. A metaphoric account of this is briefly: how close does the state of Neurophysiology as an effective reducer have to be to count as saying that Psychology reduces to Neurophysiology, which, after all, is a science de facto, defined by a characteristic set of predicates, etc. These issues are hardly susceptible to formalization and clearly, not enough has been written on any level. As we shall see--the most a formalist account yields is some analysis of the a priori conditions of reducibility; but more of this when we deal with Feyerabend later.⁶⁸

However complex the mapping, Adams is still working from the key intuition behind Quine's paradigm, i.e., the basic reduction is one between entities referred to via the theories reduced. In light of our earlier discussion this seems less than defensible. In fact, when pressed, Adams admits that although the core of Conditon A in his example is that "each rigid body is a set of particles," We are dealing

with "the rather vague notion of a particle...(and that) molecular theory lends support to the hypothesis that rigid bodies are composed of entities small enough to approximate the point particles required in the derivation of the laws of RBM (Rigid Body Mechanics)."69

But the vagueness of the notion of particle is just exactly what militates against the adequacy of the semantical account.⁷⁰ If our focus is on science in the sense of theoretical science and by this I mean any corpus of scientific literature that explains empirical generalizations by the positing of new entities we must admit that the point of such an enterprise is to discover just what the nature of its subject matter is. So, for example, any science including a molecular theory is a discipline whose domain is inherently not understood in terms of the concepts for which the molecular theory is posited. To generate even as moderate a theoretic stance as to claim that water, a liquid, is composed of molecules of hydrogen and oxygen, two gasses, is to necessarily preclude an adequate understanding of water in terms prior to this formulation. Naturally prior concepts are employed, even the pre-Socratics saw the possibility of applying old concepts to explain puzzling phenomena; think of rarefaction and condensation in Anaximenes. But old concepts must be applied to phenomenon in new and startling ways. The best example of this last claim is the development of atomic theory and the systematic reappraisal or even abandonment of the fundamental concepts as those concepts were applied in a

fashion adequate to the body of empirical data that atomic theory came to embrace. In a word: a scientific examination is not about a pre-determined set of entities--here science is unlike mathematics. Science is rather an ongoing process by which the nature of the entities mentioned by terms used, is discovered. In jargon, what a science gives us is a syntax and, hopefully, some expectations of what the syntax is about. Sometimes our expectations are satisfied in some natural way, other times not. The actual structure of the semantics into which the syntax is to be interpreted is, again disregarding instrumentalism, only to be discovered as a concomitant of a syntactic advance.

What the perspective alluded to here implies is the primacy of syntax. Condition B can then be seen as an attempt to add syntactic considerations to a basically semantic paradigm. Condition B guarantees that "...the laws of the secondary theory 'follow' from the laws of the primary theory."⁷¹ The scare quotes on 'follow' point to the truncated sense intended: a sense captured more explicitly by the notion of analogue used in conjunction with Quine. That is the sense of Theorem IV above. Adams tries to get at what he takes to be the crucial aspect, so: "...it must be possible to show that if the primary theory in the reduction is correct in that all of its intended models satisfy its axioms, then all of the intended models of the secondary theory satisfy its axioms--therefore the secondary theory is also correct."⁷²

This is a complex issue and will be delved into when we look at the paradigm of Eberle below. At this point we remark that Condition B is analogous to what might be called 'confirmation from above'. That is, the fact that T_1 in some sense implies T_2 , furnishes T_2 with whatever additional credence T_1 may have. To precise our intuition: there seems little question that the unificatory role of reducing theories is crucial just because it lends support to the theories reduced. It is just too much to imagine that Classic Chemistry is incorrect while allowing, under reduction, that it fits the all-pervasive structure of particle Physics.

If we agree that the need for indeterminacy of models is required for scientific advance, it is natural that we turn to an attempt to formulate a syntactic criteria for reduction. We now turn our attention to such essentially syntactic accounts.

FOOTNOTES

Chapter III

¹Such a claim has been carefully analyzed by P. Achinstein, The Concepts of Science (Baltimore: The Johns Hopkins Press, 1968). C.G. Hempel, Aspects of Scientific Explanation (New York: The Free Press, 1965), Section II, as well.

²Actual scientific advances includes all varieties of psycho-physical correspondence. So not only are there procedures to localize sensations but there are attempts to tie, e.g. schizophrenia to the presence of a brain chemical. This last is analagous to the tying, prior to the identifying, of a disease syndrome to the presence of a bacterium.

³To give some idea of what I have in mind here. The residual 'non-mental' aspect of mental terms is more frequently than not taken to be socio-cultural, as in the analysis of mental items as being inherently rule governed. How such sociological and normative consideration relate to neurophysiological theories and terms, for example, is a matter of great complexity and about which little has been said by the proponents of the MBIT.

⁴H. Feigl, "The 'Mental' and the 'Physical'," Minnesota Studies in the Philosophy of Science (Minneapolis: University of Minnesota Press, 1958), Vol. 2.

⁵The idea that models constitute ontologies is a simple generalization of the fundamental idea of Quine that "To be assumed as an entity is, purely and simply, to be

reckoned as the value of a variable." W. V.O. Quine, From A Logical Point of View (New York: Harper Torchbooks, 1953), p. 13.

⁶The second generation of literature on reduction include L. Sklar, "Types of Inter-Theoretic Reduction," British Journal for the Philosophy of Science, 1967, vol. 18, pp. 109-24; Glymour, C. "On Some Patterns of Reduction," in Philosophy of Science, 1970, vol. 37, pp. 340-53; K. Shaffner, "Approaches to Reduction," Philosophy of Science, 1967, vol. 34.

⁷W. V. Quine, "Ontological Reduction and the World of Number," Ways of Paradox (New York: Random House, 1966).

⁸Suppes, "A Comparison of the Meaning and Uses of Models in Mathematics and the Empirical Sciences," (1960) Studies in Methodology and Foundations of Science (New York, Humanities Press, 1969).

⁹E. Adams, "The Foundations of Rigid Body Mechanics and the Derivation of Its Laws From Those of Particle Mechanics," Axiomatic Method, ed. Henkin, Suppes, Tarski (Amsterdam: North Holland Co., 1959)

¹⁰E. Nagel, The Structure of Science (New York: Harcourt, Brace and World Inc., 1961).

¹¹Kemeny and Oppenheim, "On Reduction," Readings in the Philosophy of Science, B.A. Brody, ed. (New Jersey: Prentice Hall, 1969).

¹²K. Eberle, "Replacing One Theory by Another Under Preservation of a Given Feature," Philosophy of Science, 1971, vol. 38, pp. 486-501.

¹³P. K. Feyerabend, "Explanation, Reduction and Empiricism," eds. Feigl and Maxwell, Minnesota Studies in the Philosophy of

Science (Minneapolis: University of Minnesota Press, 1962)

vol. 3.

¹⁴Quine, "Ontological Reduction," p. 201.

¹⁵Ibid., pp. 201-202.

¹⁶S. C. Kleene, Introduction to Mathematical Logic

(Princeton: D. Van Nostrand Co., 1950), pp. 389-98; also p. 431.

¹⁷Quine, "Interpretation of Sets of Conditions," Selected Logic Papers (New York: Random House, 1966). p. 205. 'S' names a set of 'consistent...quantificational schemata. Ibid.

¹⁸E. Mendelson, Introduction to Mathematical Logic

(Princeton: D. Van Nostrand Co., 1964), pp. 64 ff.

¹⁹Quine, "Ontological Reduction," p. 205.

²⁰Ibid., p. 206. Italics mine.

²¹Quine, "Interpretations."

²²B. Van Fraassen, Formal Semantics and Logic (New York: MacMillan Co., 1971), pp. 101, 103.

²³By 'least relation' we mean the product of all relations satisfying the construction, similarly for 'least set'.

²⁴We understand the phrase 'with or without subscripts' appended to all defined expressions.

²⁵We follow standard procedure in marking all theories as first order.

²⁶Quine, "Interpretations," p. 205.

²⁷Ibid.

²⁸Ibid., p. 206. The recipe for the assignment is as follows; 'to the j th existential quantifier in the string, for each j , we assign (following Kleene) the number

$2^j \cdot 3^{a_1} \cdot 5^{a_2} \cdot \dots \cdot p_r^{a_r}$ where $a_1, a_2, \dots, a_r$ are the respective

numbers assigned to the preceding universal quantifiers and P_r is the r th prime number after 2'.

²⁹Ibid., p. 207.

³⁰Ibid.

³¹Ibid., pp. 209-210.

³²We recommend Mendelson, Introduction to Mathematical Logic, Intro. for a brief but lucid account of these and related issues.

³³Quine, Methods of Logic, 3rd Edition (New York: Holt, Rinehart & Winston, 1972), sec. 42.

³⁴Quine, Philosophy of Logic (New Jersey: Prentice Hall, 1970), p. 25.

³⁵Quine, Ways of Paradox, p. 207.

³⁶Mendelson, Introduction to Mathematical Logic, p. 8, p. 90ff.

³⁷See Van Fraassen, Formal Semantics and Logic, p. 121.

³⁸Mendelson, Introduction to Mathematical Logic, p. 90, Prop. 2.33.

³⁹Ibid., p. 91 Exercise 1.

⁴⁰Quine, "Ontological Reduction," p. 202.

⁴¹Quine, "Interpretations," p. 205.

⁴²P. Suppes, "A Set of Independent Axioms for Extensive Quantities," Studies in the Methodology, p. 36.

⁴³Definition: A system $K, Q, *$ will be said to be a system of extensive quantities if it satisfies the following seven axioms:

A1. If x, y , and z are in K , and if xQy and yQz , then xQz .

A11. If x and y are in K then $x*y$ is in K .

A111. If s, y , and z are in K , then $(x*y)*zQx*(y*z)$.

A1V. If s, y and x are in K and xQy then $X*zQz*y$.

Av. If x and y are in K and not xQy , then there is a z in K such that $xQy*z$ and $y*zQx$.

Avl. If x and y are in K then not $x*yQx$.

Avii. If x and y are in K and xQy , then there is a number such that $yQnx$. (op. cit. pp. 37-38) These axioms permit many plausible interpretations in the set of integers closed under standard operations. Suppes offers as an example 'any additive semi-group of positive numbers closed under subtraction of smaller numbers from larger numbers.' (op. cit., pp. 37-38.

⁴⁴Ibid.

⁴⁵Suppes, "Foundational Aspects of Theories of Measure-meant," written with Dana Scott, reprinted in Suppes, Studies in the Methodology.

⁴⁶Ibid.

⁴⁷Ibid.

⁴⁸Ibid.

⁴⁹Ibid.

⁵⁰Ibid.

⁵¹Suppes, Introduction to Logic, (Princeton: D. Van Nostrand Co., Inc. 1957), pp. 266 ff. for examples.

⁵²Ibid., p. 263.

⁵³Quine's examples are from the theory of measurement as well as the reduction of arithmetic to set theory.

⁵⁴M. Hesse, Models and Analogies in Science (Indiana: University of Notre Dame Press, 1966) for a discussion of the 'Campbellian' position that models are necessary if theories are to be explanatory as well as guiding the development of the theory in the future by dint of the 'neutral analogy'

between a theory's models.

⁵⁵Suppes, "A Comparison of Meaning and Uses," pl 18.

⁵⁶Idem, Introduction to Logic, p. 271.

⁵⁷Ibid., p. 271.

⁵⁸Ibid., p. 271. Suppes mentions Adams (1959) originally presented in 1955.

⁵⁹Ibid.

⁶⁰Ibid., p. 262.

⁶¹A. Koslow, personal communication (see Appendix B).

⁶²E. Adams, "The Foundations of Rigid Body," p. 259.

⁶³Ibid., p. 262.

⁶⁴Ibid.

⁶⁵Ibid.

⁶⁶Nagel, The Structure of Science, p. 342 ff.

⁶⁷The literature here is massive. A readable collection of relevant essays is Held and Richards, Perception: Mechanisms and Models (San Francisco: Freeman & Co., 1972). A minimal attempt to square philosophic theories to relevant fact is found in Mundle, Perception: Facts & Theories (London: Oxford Univ. Press, 1971).

⁶⁸The classic literature on reduction makes much of this: See Nagel, The Structure of Science, ch. 11 and Kemeny and Oppenheim. Unfortunately the issues was presented in terms of defining reduction at a time when the obvious problem is rather what developments count as precluding identifying a theory, in development, over time.

⁶⁹Adams, "The Foundations," p. 262, 264. By 'semantical account' we mean a reduction paradigm taking relations (mappings) between models as central.

⁷⁰Ibid., p. 261.

⁷¹Ibid.

CHAPTER IV

The reduction paradigm of Eberle¹ is based on the purely syntactic notion of a representing function.² We let ' $\phi\alpha$ ' denote the result of replacing all free occurrences of α in ϕ by free occurrences of β , (where possible), otherwise by itself:³ Where α, β , are variables and ϕ, ψ are expressions containing predicates, logical signs and perhaps, other variables.

"...f is a representing function just in case f is a function which maps formulas into formulas maps variables one to one into variables and for all appropriate formulas in its domain,
 (a) If ϕ is atomic then $f(\phi\alpha) = f(\phi)f(\alpha)$, for all variable α and β ,
 (b) $f(\sim\phi) = \sim f(\phi)$,
 (c) $f([\phi \rightarrow \psi]) = [f(\phi) \rightarrow f(\psi)]$

(g) $f(\forall\alpha\phi) = \forall f(\alpha)f(\phi)$,

Furthermore, f preserves identities if, in addition
 (i) $f([\alpha = \beta]) = [f(\alpha) = f(\beta)]$ "⁴

Eberle's reduction paradigm is as follows: $T_1 \text{Re} T_2$ if and only if f represents T_2 in T_1 with respect to consequence. That is to say any member of the consequence closure of T_2 is represented in T_1 . This last notion is a specific choice from among the more general "f represents T_2 in T_1 with respect to K just in case K is a function which assigns to any theory T a set of expressions of T and f is an effective representing function mapping the expressions of T_2 into the expressions of T_1 in such a way that for any expression E of T_2 , E (is in) $K(T_2)$ then $f(E)$ (is in) $D(T_1)$."⁵

Notice the more general notion permits a wide variety of other senses of 'reduction' so $K(T)$ can be, for example, the observational consequences of T , or some set of approximations to the members of T , that is sentences in some definable 'neighborhood'. This generality will be of interest when we deal with the paradigm of Kemeny and Oppenheim as well as face the challenge of Feyerabend in Chapter V. Notice in passing that Eberle's paradigm is defined for theories in the sense of our p.85 above, that is theories with no constants as singular terms. Note further that with f the identity mapping $T \rightarrow T$, this is analogous to the trivial self-explanation of an event e by itself that we worked against in Chapter I. We should therefore expect some further restriction on Re . This will be discussed later when we relate Eberle to the discussion of Omer-explains.

The salient characteristic of Re is that it permits of 'essential' preservation of model structure without limiting such structure preservation to one to one mappings of individuals. That is, it meets the need to have complex isomorphisms defined between models in the style of Suppes and Adams. This clearly, is a virtue and just how this flexibility of mapping is accomplished will be made clear in a moment. The intuition behind 'essential' structure preservation is the following. Whenever we have an appropriate set of elements, in the domain of desirable models of T_2 , in some relation there is a complex, but definable,

relation of entities in the models of T_1 . That is, the relations defined in the models of T_1 , will reflect the relations definable on some appropriately chosen set of models of the reduced theory T_2 . Much of this is clearly stated in the results, quoted from Eberle, to follow. The notion of 'chosen', 'desirable', etc. will also become clearer as we present the view, inherent in Eberle, that it is the models of the reducing theory that ultimately govern our choice of appropriate models for the reduced theory.

But first, let us examine the claim that representing functions permit arbitrarily complex mappings between elements in the domain of models for the theories in the reduction relation. Recall, we consider this a crucial desiderata for any reduction relation useful for the empirical sciences. That is to say, even the most casual examination of paradigmatic reduction instances points to complex relations, far from the simple, one-one mappings of individuals that the Quineian position envisions. Rather than attempting to generate the requisite intuition from the precise characterization of representation functions on page 126, such recursive definitions being notoriously difficult to comprehend, let us deal with a superset of the class of representing function, that is, the class of proper substitutions. Our ability to use the class of proper substitutions as a handy pictograph is based on the following: "...suppose that T_1 is obtained from T_2

by a uniform proper substitution on the predicates of T_2 then T_2 is reducible to T_1 ."6 'Uniform proper substitution' refers to the "result of properly substitution on the predicates of T_2 in such a fashion that a given predicate is replaced in all of its occurrences in all formulas of T_2 by instances (with respect to its variables) of the same formula."7 The notion of proper substitution is after Mates' analysis of 'substitution instance'. A substitution instance being the result of what Mates calls a 'legitimate substitution'. But first

...an auxiliary notion is needed, that of a stencil. A stencil is an expression that is either a sentence...or is obtainable from such a sentence by putting the counters '1' or '2' or '3' and where that sentence contains occurrences of individual constants. Thus the following are stencils;

Fa 1 2
F 1 2 1
(x) (Fx 1 a $\rightarrow$ (1y) Gy 2)b

but

Fx 1 2
Fa 1 3

and

are not. If a stencil contains no occurrences of '1', (i.e. if it is a sentence) it is a degree 0; if it contains an occurrence of '1' but not occurrence of '2' it is of degree 1....and so on. If σ is a stencil of degree n ($n > 0$) and $\delta_1, \dots, \delta_n$ are individual symbols then $(\delta_1, \dots, \delta_n)$ is the result of putting δ_1 for '1', δ_2 for '2', δ_3 for '3' and so on in σ .

Now let ϕ be a sentence....and let θ be an n -ary predicate....that occurs in ϕ . Further let σ be a stencil of degree n . To substitute σ for θ in ϕ we proceed as follows; for each occurrence of θ $\delta_1, \dots, \delta_n$ in ϕ , where $\delta_1, \dots, \delta_n$ are individual symbols, we put $\sigma(\delta_1, \dots, \delta_n)$. If in the substitution we have at all places obeyed the rule that no variable shall occur bound at any place in any substituted $\sigma(\delta_1, \dots, \delta_n)$ unless it already occurs bound at that place in σ (really at that place in the sentence from which σ is derived) then we say that the substitution is legitimate....

A sentence ψ is a substitution instance of a sentence ϕ iff ψ can be obtained from ϕ by a legitimate substitution.¹⁰

The flexibility of the notion is immediate given the following example: we can substitute the stencil

'G $\textcircled{1}$ $\wedge$ Ha $\textcircled{1}$ ' for 'F' in $(x) (\exists y) (Fx \supset (Gx \wedge Fy))$

thus obtaining,

$(x) (\exists y) (Gx \wedge Hax. \supset (Gx \wedge (Gy \wedge Hay)))$

Notice, what is relevant is that in the place of the unary predicate Fx we get the complex condition ' $Gx \wedge Fax$ '. Now clearly the model relations that could stand as an interpretation for this substitution far transcend the kind envisioned by Quine. To work from the example; we assign to the members of X , $x = \{x: x \in F\}$ the set of pairs

Y , $Y = \{ \langle x, y \rangle : x \in G \wedge \langle y, x \rangle \in H \wedge y = a \}$

We note that although the notion is flexible, the notion retains the crucial property alluded to in the following theorem "...if ϕ is a theorem and ψ is a substitution instance of ϕ then ψ is a theorem."¹¹ Whence, given familiar properties of $\vdash$; if $\phi_1, \dots, \phi_{n-1}$ then $\psi_1, \dots, \psi_{n-1} \vdash \psi_n$ where the ψ_i are substitution instances of the ϕ_i . Thus, as we have argued since Theorem IV, Chapter III, is crucial since we want to preserve deductive consequence under whatever our reduction paradigm turns out to be. We note further, within this context, that Re has the crucial property of consequence preservation since; "...if f is a representing function defined on all expression of a theory T , is ϕ a formula of T and ϕ is a theorem of logic, then $f(\phi)$ is a theorem of logic."¹²

Eberle carefully traces out the consequences of his notion. The main results of interest to us are the following. With f an effective representing function:

(a) (1) f reduces T_2 to T_1

(2) for any model of T_1 , M_1 , there is a model of T_2 , M_2 , such that for all sentences, s , of T_2 , $M_2 \models s$ if and only if $M_1 \models f(s)$.

(3) for any M_1 and assignment function on M_1 , d_1 , there is some M_2 and assignment function, d_2 , on M_2 such that for all formulas, a , $M_2 \models a(d_2)$ if and only if $M_1 \models f(a)(d_1)$; are all equivalent. That is to say (1), (2) and (3) are all equivalent. Notice the relations between models are just the reverse of what Suppes and Adams require. That is to say that rather than relating the set of models of T_2 to some chosen models(s) in T_1 we can find a model of T_2 for any model of M_1 we choose. We will argue that this is how things should be since the point of reduction is some ontologically significant sense is to focus on the construals of T_1 as crucial. But more of that later and especially when we generate our reduction paradigm in Chapter VI.

The reversal of the relations between the models of the reducing and reduced theories within the paradigms of Adams and Eberle make the following result rather surprising.

(b) If we define the reduction relation, R , in Adams' reduction paradigm, p. 117, above as : for all models M and M' , MRM' if and only if for all sentences, s , in T_2 , $M \models s$.

if and only if $M' \models f(s)$, with f a representing function as before, (a) and (b) of Adams' paradigm are equivalent to (A') and (B'): (A') for every intended model I_2 to T_2 there is an intended model I_1 of T_1 such that f reduces the truths of I_2 to the truths of I_1 , that is to say by (a) above, $I_2 \models s$ if and only if $I_1 \models f(s)$ for all s in T_2 . (B') f reduces T_2 to T_1 , that is to say $T_1 \text{Re} T_2$.

The last consequence of Eberle's paradigm of interest to us is:

(c) T_2 is interpretable in T_1 if and only if there is an f , such that f reduces T_2 to T_1 and f maps all s in $T_1 \cap T_2$ onto themselves. That is $f:s \rightarrow s$ for s in $T_1 \cap T_2$. ' T_2 is interpretable in T_1 ' here refers to the Tarskian notion

frequently called 'definitional extension' and recursively characterized as: " T_2 is interpretable in T_1 iff there is a theory T and a set D satisfying the following conditions:

(α) T is a common extension of T_1 and T_2 and every constant of T is a constant of T_1 and T_2 .

(β) D is a recursive set of sentences which are valid in T and which are possible definitions in T_1 of the non-logical constants of T_2 .

(γ) each non-logical constant of T_2 occurs in just one sentence of D .

(δ) every valid sentence of T is derivable (in T) from a set of sentences each of which is valid in T_1 or belongs to D .¹³

Let us begin our evaluation of Eberle's paradigm with

condition (c). If T_2 is interpretable in T_1 then there is a theory T^* such that both T_1 and T_2 are sub-theories of T^* . But that is to say that T^* implies both T_1 and T_2 . From this last, as is obvious, for any model, M^* , such that $M^* \models T^*$, $M^* \models T_1$ and $M^* \models T_2$. What this amounts to is that T^* alters none of the deductive structure of T_1 or T_2 , that is to say T_1 and T_2 remain intact as theories under their common extension T^* . Condition (c) thus sheds light on condition (a). The intuition here is as follows. Given the common extensibility of T_1 and T_2 we can define relations on the objects in the domains of the various M_1 's that are satisfied just when the various sentences, s , in T_2 express relations defined in the M_2 's. We are back to the notion of analogue as defined earlier, p. 107, above, but now the notion is not restricted to favored models of T_1 . We now can guarantee that for every possible interpretation of T_1 we can define our T_2 analogues. But what this shows is that T_1 on any possible future interpretation, that is to say on any interpretation that the future development of science might require, can generate the analogues for any sentence of T_2 .

This seems highly desirable if we work from clear paradigms. So, for example, if the reinterpretation of statistical mechanics in quantum rather than Newtonian terms is deemed desirable given scientific advance, this will not, nor should it, alter the reduction of phenomenological thermodynamics to mechanics. For the relation between heat

and the mean kinetic energy does not specify all of the properties of the molecules involved, merely those properties necessary for the deduction of the relevant laws. Just what the molecules are, that is, what the favored models of Elementary Particle Physics will turn out to be is irrelevant to the claim that whatever else, they have the complex property "that the probability of a molecule occupying an assigned phase cell is the same for all molecules and is equal to the probability of a molecule occupying any other phase cell..."¹⁴ Of course, if future advances deny that crucial property, that is if T_1 is altered so that a new theory T'_1 , possibly inconsistent with T_1 , is accepted, the reduction may be threatened. But that is because there will be no T^* that is a common extension of T'_1 and T_2 . As we shall see, our choice of which models are intended for T_2 will hinge upon there being models that are relatable to the intended models of T_1 . But more of this later.

This last line raises a host of issues that will be crucial when we discuss the position of Feyerabend, Chapter V below. But, prima facie, Eberle's paradigm seems consistent with standard practice, that is to say the re-interpretation of accepted laws and theories in the face of theoretical advance. The point at this juncture is to fix on the fact that the relations between models are just the opposite of what Suppes requires. We do not find an analogue within the set of M_1 's for any M_2 , rather we can

guarantee that any M_1 will serve as an analogue for some M_2 . Or, alternatively, for any M_1 we choose we can find some set of objects, that is to say some M_2 , that satisfies T_2 that is an analogue for M_1 . Here the difference seems of crucial importance, given that our concern is ontological. When we reduce, the ontological 'weight' is on the models of T_1 , since T_1 's ontology, whatever it turns out to be, is going to be favored. This to recall our earlier remarks presupposes a realist interpretation of theories. That is to say, our theories inform us as to why the world acts the way it does, or alternatively, why law like relations obtain, by telling us of what the world is really composed and what the relations obtaining between these 'theoretical' entities are.

One way to see the inadequacy of the paradigm that Suppes presents, if our purpose is ontological, is to realize that we don't need to find analogues for any T_2 interpretation, since one major concern of ontological reduction is, as Quine originally pointed out, to repudiate the need for the entities involved in T_2 interpretations. Certainly we don't want to be committed to replicate mistaken models of old theories at just that conceptual turn when the old theories are seen to be, not what they were thought to be, but rather something else.

Whatever air of paradox in respect of (a), vis a vis (b), remains, vanishes if we recall our earlier remarks in respect of Suppes. That is to say that Suppes'

paradigm is best construed as reduction in some other sense; that is the demonstration of the adequacy of an extremely general theory to a more specific one, for example, a theory of measurement to a physical theory. In such a case, we naturally require that the theory of measurement (T_1) apply no matter what the correct construal of the theory measured (T_2) turns out to be. This is just Suppes' claim that for any T_2 model, M_2 , M_2 be isomorphic to some favored model of T_1 , the model M_1 in T_1 . Notice that there are limits within the context of measurement as well. This seems one way to render the heart of the debate over complementarity in Particle Physics. Should a theory that denies the correctness of Classical Mechanics apply a theory of measurement that presupposes a classical interpretation for the operations performed by apparatus used in laboratory measurements.

Notice it is not that some 'theoretical reducer', a more basic theory, has shown the reduced theory to be incorrect. It is rather that theoretical advance within Particle Physics has demanded that values assigned to physical phenomena be limited as a function of Plank's constant. Whereas the numerical aspect of the theory of measurement, as classically construed, permits of assignment of values in a fashion that is dense, that is numerical operations are defined that take as values quantities that have no possible physical interpretation within Quantum Mechanics. In our terms, the favored model of Particle Physics is not

isomorphic to the numerical model that is the core of the Classical theory of measurement.

The split between the paradigms of Suppes and Eberle render (b) difficult to make sense of. After all, Adams seemed, at first sight, to be a simple generalization of Suppes' paradigm--yet we have argued that Suppes and Eberle are speaking of reduction in quite different senses. How then do we account for the equivalence stated in (b). The key is to realize that the equivalence is between R_2 and R_e plus (A'). But, of course, (A') is just that component that we must reject, whence the equivalence breaks down. Rejection, naturally, is based on the cogency of the discussion so far. Eberle takes a similar position:

"Apparently it could happen, for example, that an intended model of concatenation theory is so conceived that it interprets the language of that theory with reference to concrete non-repeatable inscriptions. If so, concatenation theory might well fail to be reducible (in Adams' sense) to number theory simply because the incorrect but intended model has no intended counterpart among the models appropriate to number theory. But surely this should not be taken to show that concatenation theory is not reducible to number theory. What we find unacceptable in this example is not the claim to reduction, but rather the demand that concatenation theory conform to the incorrect model."¹⁵

If we substitute 'inappropriate' for 'incorrect' in the last quoted sentence we get full generality.

We note, finally, that (B') expresses the reason for (B) in the Adams' paradigm. Recall, (B) in Adams' account was the demand that the correctness of T_1 imply the correctness of T_2 . (B') given (c) now explains why--it is just because

T_1 'implies' T_2 . The scare quotes on 'implies', naturally, bespeak the complex relations between T_1 , T_2 and T^* . We will return to the Tarskian version of Eberle's paradigm when we discuss Nagel, whose classic reduction paradigm is equivalent to Tarski, at least under its strongest and most usual version.

To recapitulate our discussion up to this point, our examination, so far, has shown two disparate senses of 'reduction'. The Quine, Suppes and part of Adams view that uses the notion of reduction to refer to the applicability of extremely general theories to other theories, no matter how construed. Secondly the Adams-Eberle view that presents a reduction relation that permits the interpretability of a basic theory in more concrete theories on the basis of the interpretation of the more basic theory in them.

As is obvious, the latter seems of more importance if we are to discuss reduction as it is applied to reduction instances in the empirical sciences. Further, there seems plausible warrant for this latter notion to have ontological import just in the sense that the former notion does not. This in the sense of Sellars' view, alluded to earlier, that a theory explains laws (or more concrete theories) by telling us what the laws (or theories) are really about--thereby explaining why the laws that hold, do hold.

In the preceding sections we have accepted as a working hypothesis the view that reduction relations of plausible

ontological significance preserve 'consequence' the scare quotes, again, in cognizance of the complications involved. So, specifically, we hold that $T_1 R T_2$ for some reduction relation, R , if at least the following holds: for all sentences, A , in the consequence closure of T_2 , T_1 implies $a(A)$ for some T_1 analogue of A , $a(A)$. This is a moderately strong requirement, stronger than some, e.g. Quines, cf. Theorem 5 above, but not quite as strong as others that are similar. The classic view of Nagel¹⁶ is a similar although somewhat stronger view. It will be worthwhile to compare Nagel's view to this minimal position if only to clarify the model relations in Nagel's views. This will be important for resolving certain outstanding problems standard in the literature on reduction which are relevant to our concerns.

Nagle's views are fairly complex and have been consistently misrepresented in the literature on reduction. For that reason we feel it best to present the view with a fair amount of detail. Throughout we will be dealing with what Nagel calls, 'inhomogeneous reduction': "In reduction of this type the secondary science (T_2) employs in its formulations of laws and theories a number of distinctive descriptive predicates that are not included in the basic theoretical terms or the rules of correspondence¹⁷ of the primary science (T_1)."¹⁸ It is obvious that if our interest is with the MBIT, the sciences to be related, e.g. Psychology and Neurophysiology, are inhomogeneous.

The core of Nagel's position on reduction has been stated in his earliest published views, so: "the objective of the reduction is to show that the laws or general principles of the secondary science are simple logical consequences of the assumptions of the primary science."¹⁹ In 1961, Nagel adds a significant alteration, an alteration that if noticed would have rendered a main line of criticism superfluous. "... a reduction is affected when the experimental law of the secondary science (and if it has an adequate theory as well) are shown to be logical consequences (inclusive of the coordinating definitions) of the primary science."²⁰

Nagel's notion of theory, or in his terms, science, are again, complex but worth repeating. A science, S, is a set of statements composed of the following subsets. "(a) the theoretical postulates of S, the theorems derivable from them and the coordinating definitions associated with theoretical notions and the postulates or theorems.

(b) the experimental laws of S

(c) the observation statements of S

(d) the borrowed laws of S"²² We follow Nagel then by considering a theory, T, $T = \langle P, C, E, O, L \rangle$ where $P \cup C$ are the elements of (a) and similarly for E, O, L and (b), (c), (d). The elements of L are normally considered to include elements of mathematics and logic but also include principles governing the experimental apparatus used in the testing of T. So, for example, the laws of optics are accepted

without serious question within the context of Biology although the adequacy of observations relevant to Biology e.g. microscopic observations, is dependent upon the correctness of the laws of optics. We note, naturally, that this assumption may be unwarranted in some cases and more importantly serious disputes about reducibility may hinge on the acceptance or rejection of such 'borrowed laws'. The classic example of such a problem is the acceptance, in the notion of Complementarity, of the laws of Classical Mechanics as governing the theory of the instruments employed in generating the data of Quantum Mechanics. We will at this point, at least, accept the first position and consider L as either an innocuous subset of P or perhaps, better, as meta-principles acting as rules of inference. The choice of these two options are logically equivalent, at least as far as T is concerned; although the latter is, perhaps, the better choice on the grounds of keeping relevant distinguishables distinct.

Nagel presents the following as a necessary condition for reduction. We say, $T_1 R_n T_2$ only if

- (1) $T^* \vdash T_2$ where $T^* = T_1 \cup B$ and where
- (2) the members of B, the b_i 's, are sentences of the form
 - (a) $(x_1, \dots, x_n) (S_i x_1, \dots, x_n \subseteq P_i x_1, \dots, x_n)$
 - (b) $(x_1, \dots, x_n) (S_i x_1, \dots, x_n \supset P_i x_1, \dots, x_n)$

and where there is a b_i in B for every predicate P_i of T_2 and for some condition S_i expressible in T_1 (every predicate in S_i is a predicate of T_1 , i.e., a predicate occurring in

the set of postulates of T_1) and where S_i has no variable free other than $x_1, \dots, x_n$.

(2) (a) and (b) are formal expressions of the following in Nagel. In order for the secondary science to follow from the primary science, when "the laws of the secondary science do contain some term 'A' that is absent from the theoretical assumptions of the primary science...Assumptions of some kind must be introduced which postulate relations between whatever is signified by 'A' and traits represented by theoretical terms already present in the primary science."²² Nagel gives a more detailed account in two places. Firstly in a footnote in Structure of Science Nagel asserts that "For every term 'A' in the secondary science but not in the primary science there is a theoretical term 'B' in the primary science such that A and B are linked by a biconditional ...(or) only a one way conditional: If B, then A,"²³ In his most recent statement he says; "...the bridge laws may specify the conditions, formulated in terms of the ideas and assumptions of the reducing theory under which the attribute (of the secondary theory) occurs."²⁴ We have formulated our condition in terms of the this most recent statement where a 'condition' rather than a single predicate is a sufficient or necessary and sufficient condition for the ascription of a predicate of the reduced theory. Notice it is not clear that reductions always go predicate by predicate. That is to say it is not clear that we can always isolate T_2 terms in the fashion required by (2) (a)

and (b). Since we will eventually focus on the more general analysis of Eberle where representing functions guarantee model relations of an appropriate sort we will leave this detail unexamined.

Nagel spends a great deal of time discussing the epistemological status of the members of B and finally opts for the view that they are "...physical hypotheses asserting that the occurrence of the state of affairs signified by a certain theoretical expression B (S_i) in the primary science (T_1) is a sufficient (or necessary and sufficient) condition for the state of affairs designated by A."²⁵ (where A is the predicate P_i of T_2 not occurring in T_1).²⁶

Nagel distinguishes between (1) and (2) calling the first the principle of derivability and the second the principle of connectivity. He argues that derivability implies connectability quoting "the familiar logical canon that, save for some essentially irrelevant exceptions, no term can appear in the conclusion of a formal demonstration unless the term also appears in the premises."²⁷ It is interesting to examine the 'irrelevant exceptions' mentioned. They are, for the propositional case, "If S_1 then S_1 or S_2 " and ' S_1 and (S_2 or not S_2)' is derivable from ' S_1 '. And for the quantificational case 'For any x, if x is a planet then x shines by reflected light!...(implies) 'If Mars is a planet then Mars shines by reflected light,'"²⁸

The interest of this problem for us is that it affords us, for the propositional case, with the opportunity of

bringing Omer-explains to bear on the problem. It is obvious that the propositional moves mentioned are in violation of Omer-(4). Since, with S_1 as T_C , the set of T_{C_i} 's is a singleton and therefore for some T_{C_i} , $T_{C_i} \vdash E$ with E as ' S_1 or S_2 ' or ' S_1 then S_2 or not S_2 '. So Omer turns out to be relevant again, albeit, fairly trivially. In a moment we will find Omer applicable in resolving a classic issue in the problem of reduction. The quantificational case merits a moment of pause. The claim here is that ' $(x) Ax$ ' implies ' Aa ' where ' a ' is new relative to the premise ' $(x) Ax$ '. This observation although quite correct, is misleading for ' a ' is not new relative to the theory which included ' $(x) Ax$ '. The argument in question is valid just because ' a ' names a member of the domain, so the element named by ' a ' is implicitly contained in the interpretation of the sentence ' $(x) Ax$ '; syntactically ' a ' occurs in the infinite expansion ' $(\dots \wedge a \wedge \dots)$ ' of ' $(x)Ax$ '.

Kemeny and Oppenheim (K & O)²⁹ have argued that (1) of Nagel's analysis of reduction is superfluous, since the condition of derivability is trivially satisfied by condition (2) if construed as they do, as (2a). The argument for this position is as follows. If we let T' be the result of substituting into every sentence of T_2 the condition S_i for the correlated predicate of T_2 , P_i , then it is clear that $T^* \cup T' \vdash T_2$. The point here is that if we have warrant for T_2 and B we have equal warrant for T' , whence T' is as cogent an assumption as is B . So if we accept as desirable $T^* \vdash T_2$, we ought to equally

accept $T^* \cup T' \vdash T_2$ which then guarantees, given B that $T' \cup B \vdash T_2$, i.e. we have an empirically (given Nagel's analysis of the warrant for B) indistinguishable alternative to (1) that is guaranteed immediately by (2a).

The move here is not without its problems given our acceptance of Omer-explains as the basic explanation relation for a well managed scientific language. This is, of course, since $\langle \{T^* \cup T'\}, T_2 \rangle$ violates Omer-(5), since $T' \cup B \vdash T_2$. This brings up what turns out to be an interesting question. What is the relation of T^* , T' and T_1 as potential explanans statements for T_2 ? The clue to the resolution of the problem is a closer look at the conditions. Nagel adds to (1) and (2) in his attempt to guarantee an adequate reduction paradigm. These are the so-called 'non-formal' conditions for reduction. We note, that it is traditional in the commentary on Nagel to mention these non-formal conditions and then disregard them completely. So we have:

(3) "that the theoretical assumptions of the primary science (T_1) be supported by empirical evidence."³⁰

(4) "the theory (T_1) must also be fertile in usable suggestions for developing the secondary science and must yield theorems referring to the latter's subject matter which augment and correct its currently accepted body of laws."²⁹

(5) "Apparantly independent experimental laws....are shown to involve a common invariant component represented by a

theoretical parameter which in turn becomes firmly tied to several kinds of experimental data."³²

The trouble with $T' \vee B$ as an explanans statement is clear. There is a strong presumption that T' is a merely ad hoc construction having no independent warrant as a theoretical exegesis. Recall, T' is just the translation of T_2 into the language of T_1 . That is to say, as before, T' is the result of substituting into every sentence of T_2 , the condition S_i for the correlated T_2 predicate P_i . But then T' does not give us the depth of understanding that reduction as a species of theoretic explanation ought to afford. In Nagel's terms T' although satisfying (3) if B does, has no evidence for it not tied to the evidence for T_2 and B , that is to say no independent evidence. More importantly, there is no reason to suppose that T' will satisfy (5)³³ In fact it is highly unlikely that T' will satisfy (5) since it is, as a theory, limited to just those elements of T_1 relevant to T_2 laws.

Notice before we continue that $T_1 \vee B$ implies T' or $T_1 \vee B$ fails to imply T_2 . But that is as much to say that T' is a necessary condition for the reduction of T_2 by $T_1 \vee B$. This is easily shown. If we assume $T_1 \vee B$ implies T_2 and recalling what the members of B are we see that for every sentence, S , in T_2 there is a sentence S' , where S' is $S(P_i/S_i)$, that is to say S' is S with condition S_i in all and only places where predicate P_i occurred before, such that $T_1 \vee B$ implies S' . But the set of S' 's

constitute an axiomatization of T' , whence $T_1 \cup B$ implies T' . If we recall our discussion of theoretic explanation in respect of the problem of depth of explanation, Chapter I and especially Theorem VI, it was argued that a relation t_o' be constructed to handle theoretic explanation. t_o' is a relation defined on a pair of Omer-explanations. So, in general, $T t_o' T'$; if there is a T^* such that $T t_o' T^*$ and $T^* t_o' T'$. The application of t_o' to our problem is clear. We say that $T^* = T_1 \cup B$ reduces T_2 , just when $T_1 t_o' T'$, and $T' \cup B t_o' T_2$; with T' , as before, the 'translation' of T_2 into the terminology of T_1 . This squares with Nagel's intuition. Derivability is a distinct condition from connectability, the latter being the existence of the set B , since it is just not generally true that T_1 implies T' if $T' \cup B$ implies T_2 .

Before we square our construction with Nagel's text a word about our application of t_o' to the context at hand. There is a difference between t_o' and the notion of reduction here brought forward, since for t_o' we have a middle term in common with both Omer-explanations, whereas in reduction we have for the first pair, $\langle T_1, T' \rangle$ and for the second $\langle T' \cup B, T_2 \rangle$. This is the result of our dealing with reduction instances where there is a disparate vocabulary in the reducing theory and the theory reduced. This is, as before, what Nagel calls 'inhomogeneous' reduction.³⁴ It is clear that for the MBIT inhomogeneous reductions are the only ones of interest.

Notice in the case of $\vdash_0$ we made no such assumption. We show that our condition is maximal in relation to Omer-(5) in other words: If $T' \cup B \vdash_0 T_2$, then it is not the case that there is a sentence S , such that $T' \cup B \vdash_0 S$ and $S \vdash_0 T_2$. The proof is immediate since by definition of $\vdash_0$ such an S would violate Omer-(5), p.12 Chapter I. Notice further that we can't collapse the conditions. We can't assume that T_1 implies $T' \cup B$ since then T_1 implies T_2 , which is contrary to the assumption that the reduction is inhomogeneous.

Nagel's response to K & Ø's argument that since " T' follows from accepted theories (T_2)... by means of further well established theories (the biconditionals) T' must be recognized as a well established theory"³⁵ was to focus on condition (2b) as a possible account of reduction that escaped the argument. He realized that he had disregarded the interesting question as to "whether a reduction is satisfactory when achieved by augmenting the theory of the primary science by a new postulate... which is empirically confirmed but may contribute next to nothing to the explanatory power of the initial theory."³⁶ Our demand that $T_1 \vdash_0 T'$ is thus in accord with Nagel's spirit, if not with his letter, for there is a clear sense in which the connection between T' and T_2 can now be seen to be non ad hoc.

If we recall, in our discussion of $\vdash_0$ we were concerned that our relation would degenerate into the vacuous

explanatory limit, the minimal law. Briefly, to review, the minimal law explained E, for some event description E, whenever a set of laws $L_1, \dots, L_k$ and initial conditions $C_1, \dots, C_j$ implied E. The minimal law is the relativization of $L_1, \dots, L_k$ to $C_1, \dots, C_j$ or if one wishes the universalization of the initial conditions as antecedent of a particular conditional $(x) (C_1x, \dots, C_jx \supset E)$. The construction of T' is strikingly similar. We construct T' by relativizing T_2 to T_1 although we relativize in relation to the boundary conditions for reduction, the bi's, rather than the initial conditions for some event explanation. That is we merely substitute the T_1 conditions into the T_2 sentences for the T_2 predicates. Thus just as ' $(x) (C_1x, \dots, C_jx \supset E)$ ' is tailor made to explain E, and, most probably, only E, then T' is tailor made to reduce T_2 under the theoretic boundary conditions encapsulated in B.

The problem with the minimal law was resolved by explaining it via an Omer explanation chain, using the relation t_0 . Similarly we resolve the dilemma posed by the vacuous reducer T' by realizing that T', although a necessary condition for reduction is far from sufficient. We require that the connection must run deep: T' itself must follow, in light of the assumption asserted in the boundary conditions, from the actual reducing theory T_1 . The sense of 'in light of' is just that it is the assumption B that informs the theoretician what T' to try to derive from T_1 . The success of the reduction in Nagel's

sense (5) is then the hope that T_1 , a useful theory in its own right will be a useful reducer. To recall the terminology of Chapter I, T_1 will be broad: it will do what T' could never hope to do, reduce theories other than T_2 .

Viewed within this perspective, the distinction between (2a) and (2b) is less than crucial. The only difference within this context is that under (2b) $T_2 \vee B \not\models T'$. T' may still follow from T_1 , and hopefully it should if we are to have a reduction; but most importantly it is still the case that $T' \vee B \vdash T_2$. The importance of (2a) vis a vis (2b) is, however, manifest if discussion of the inverse to the proxy function in Quine, pp. 105 ff. it is apparent that condition (2a) furnishes an ontological warrant that (2b) fails to provide. Recall, if the b_i 's are of the form $(x_1, \dots, x_n) (S_i x_1, \dots, x_n \equiv P_i x_1, \dots, x_n)$, we can quite generally assert that any set of conditions satisfying the condition expressed by ' S_i ' satisfies the condition expressed by ' P_i ' and conversely. Clearly if we want to warrant the claim that the very same entities are being referred to diversely by the two conditions, the converse is crucial. Again, evidence against the converse would be evidence against the identity of the entities concerned. If (2b) is known to be maximal, "that is to say we know that $(x_1, \dots, x_n) (S_i x_1, \dots, x_n \supset P_i x_1, \dots, x_n)$ and $\sim (x_1, \dots, x_n) (P_i x_1, \dots, x_n \supset S_i x_1, \dots, x_n)$ ", then the presence of the entities satisfying the T_2 condition is merely a necessary condition for the presence of the T_1 entities satisfying

the T_1 condition. Thus we can assert the distinctness of the appropriate entities of T_1 from those of T_2 . Notice although (2b) implies distinctness of entities, by the identity of indiscernibles, if known to be maximal, (2a) does not imply identity of entities, merely reciprocal and constant conjunction. In recall, our earliest remarks, (2a) does not imply identity although it certainly might be taken as evidence that warrants identification.

We notice that our discussion, in its reflection of the discussion immediately subsequent to the crucial theorem VI is consistent with the equivocality of 'explanation' or in this case 'reduction' or what we might call deep or micro-theoretic explanation. We had mentioned earlier that this equivocality is an analogue to the position of Sellar's that theoretic explanations differ from explanations of, say, instances by laws. Here that distinction is even more apparant. For to say $T' \cup B \vdash T_2$ is to say that at least relative to the evidence we have found a warranted empirical connection between entities of T_1 and entities of T_2 . To say, however that $T^* \vdash T_2$ is to say that this empirical warrant is based on deep relationships between the kinds of entities talked about in T_1 and those of T_2 .

We will, in light of our ontological concern use the Nagel analysis in the sense of (2a). We will, however, be content to speak in terms of (1), i.e. $T^* \vdash T_2$ although this is to be understood in the sense of its being an

implicate of the stronger claim $T^* \vdash T_2$; which fully analyzed is to be construed as $T_1 \vdash T'$ and $T' \vdash B \vdash T_2$. Our acceptance of Nagel's (2a) enables us to compare it favorably with the paradigm of Eberle and return to our talk of analogues. The flexibility that this vocabulary affords will enable us to generate versions of the paradigm of great diversity. Our point throughout will be to afford logical analyses of possible reduction instances without tying ourselves a priori to any particular scheme.

The key to our switch in terminology is the theorem of Eberle's quoted as (c), p.136.. Recall:

(c) T_2 is interpretable in T_1 iff there is a f , s.t.

f reduces T_2 to T_1 and f maps all $s \in T_1 \cap T_2$ onto themselves.]

Notice, in so far as T_1 and T_2 are heterogeneous $T_1 \cap T_2 = \emptyset$ except perhaps in terms of L_1 and L_2 , the borrowed laws of T_1 , T_2 respectively. 'Interpretability' is defined in

terms of a set D and a theory T so we have the following;

(α) T is a common extension of T_1 and T_2 and every constant of T is a constant of T_1 T_2 . Note $T_1 \subseteq T$ and $T_2 \subseteq T$.

(β) D is a recursive set of sentences which are valid in T and which are possible definitions in T_1 of the non-logical constants of T_2 .

(γ) each non-logical constant of T_2 occurs in just one sentence of D

(δ) Every valid sentence of T is derivable (in T) from a set of sentences each of which is contained in T_1 or belongs to D .

We take the notion of a 'possible definition' for some predicate 'P' to be a sentence of the form $(x_1, \dots, x_n)$

$(Px_1, \dots, x_n = \phi)^{39}$ where 'P' is n-ary and where ϕ has no occurrences of variables free besides $x_1, \dots, x_n$. The connection between the Tarskian notion and Nagel's paradigm is obvious. In fact, if we take Nagel to be (1) and (2a) by merely limiting the b_i in B to the set of non-logical constants of T_2 in such a way as to guarantee both at most and at least one b_i for every predicate P_i in T_2 we have,

(d) $T_1 R_n T_2$ implies T_2 in interpretable in T_1

That is, assuming that the predicates of T_2 are finite whence the set B is recursive. We accept these assumptions as warranted by the subject matter under discussion and accept

(d). Whence by (c) we have;

(e) $T_1 R_n T_2$ implies $T_1 R_e T_2$.

The converse does not hold since there is no guarantee that (3), (4) and (5), the informal requirements, hold for instances of interpretability. But it is clear that the notion of interpretability captures the 'formal' aspect of Nagel-reduction. We will then let Eberle's paradigm stand as surrogate for these formal aspects of Nagel's paradigm. Alternatively we could strengthen Eberle by adding conditions equivalent to (3)-(5) in just those instances where Eberle wants his paradigm to be of scientific significance. The issue here is too trivial to warrant further discussion.

The crucial dispute between Nagel and K & O is just

how much of T_2 should be a deductive consequence of T' . In our new terms how much of T_2 should be represented by the function f in T_1 . Or alternatively, for which sentences of T_2 ought we have analogues in T_1 . K & O working from, presumably, instrumentalist intuitions, say that only the observational sub-part of T_2 should be reduced to T_1 . Their paradigm would look like the following, $T_1 R_{K \& O} T_2$ only if $T_1 \cup C \vdash T'_2$, where T'_2 is the set of observational consequences O_2 of T_2 and C is the set of coordinating definitions of T_1 (or at least a set consistent with the set C_1 of coordinating definitions used by T_1 prior to the reduction.³⁷ It is clear that this is a definable restriction. With Eberle surrogate for Nagel all we do is limit f to the set $O(T_2)$, the set of observational sentences of T_2 .⁴¹

K & O have asserted their claim based on the intuition that "we must certainly require that the new theory should fulfill the role of the old one, i.e. that it can explain (or predict) all those facts that the old theory could handle."⁴² The observational consequences ('all statements of the language containing only observational terms')⁴³ supposedly, being all of the relevant facts. Notice in The Structure of Science, Nagel demanded that the laws follow, i.e. $T^* \vdash E_2$, but since $E_2 \vdash O_2$, $T^* \vdash O_2$ by transitivity.⁴⁴ He further had maintained that all of T_2 follow if T_2 is adequate. There seems to be no serious dispute for if E_2 is well confirmed and further if T_2 is adequate the failure to have T_1 analogues for the relevant sentences seems a

good argument against T_1 as an appropriate reducer. For if T_1 does not preserve analogues of these elements there seems to be an incoherence in maintaining the T_1 can handle everything T_2 can if part of what T_2 can 'handle' is the systematization of the relevant observational data encapsulated in E_2 and T_2 itself.

We feel that it is a groundless presumption, short of an epistemological argument often sought but never found, to limit the role of a theory to some set of 'facts' couched in observational terms--rather than the explanation of, e.g. laws whose statement requires theoretical vocabulary, K & O's position is even stranger given their admission that 'there has been a great deal of controversy as to just what the observational...terms are.'⁴⁵

In fact, the whole attempt to limit in a priori fashion what aspects of science are to be deemed crucial in the face of theoretic advance seems to be an incredible presumption on the part of the meta-theoretician. What seems to be required is a clear analysis of the logic of the various choices and possibly some analysis of the consequences of the choices. There seems no warrant for any preference short of non-existent philosophic arguments. As far as an empirical argument based on past instances is concerned, the paradigms are fragmentary and incomplete (and at least as far as K & O are concerned unmentioned). There is even a stronger argument against taking the observational consequences as crucial and exhaustive as the sum of the relevant

reducibles. K & O have admitted to "a certain oversimplification...We intentionally over look the fact that most observations involve a margin of error."⁴⁶ That is merely a covert way to admit that most observation statements are not even consequences of the theories involved. Rather what is demanded is various principles of 'closeness of fit' and these principles are not observational, but theoretical. In fact it is the demand that observations fit the experimental laws that acts as the rule for discarding observations as error laden. But even this is far from adequate as an analysis of actual practice (or even desirable practice as an induction from past success). Frequently, observational consequences are mapped onto some neighborhood of values or are mapped onto some equivalence class of statements. These choices are more often than not based on theoretic demands whether of T_2 or even of T_1 . How these procedures fail to vitiate scientific arguments on the grounds of circularity is an enormous problem one just being faced by serious methodologists. What is clear, if anything is, is that the ploy of maintaining some set of observational statements as sacrosanct is both descriptively inadequate and methodologically incoherent. Without continuing what by now is mere polemic, it seems clear that Eberle's paradigm is patently superior, if only because it enables the meta-theoretician to hold on to a degree of flexibility obviously required by the tenuous nature of the art.

As is implicit in the discussion so far, K & O have partitioned theories into essentially two distinct 'levels'--the observational and the theoretical, Nagel had offered an analysis that distinguished between five components that implied three distinct levels, the level of theory (the postulates) the experimental laws and the observational consequences. Suppes, a methodologist working closely with actual scientific practice and especially the theory of experimental measurement and design in the Psychological sciences, affords us a general picture that includes the possibility of any degree of complexity. "What I have attempted to argue is that a whole hierarchy of models stands between the model of the basic theory and the complete experimental experience. Moreover for each level of the hierarchy, there is a theory in its own right. Theory at one level is given empirical meaning making formal connection with theory at a lower level."⁴⁷

Following Suppes' suggestion we might think of a theory as partitioned into levels $t_1, \dots, t_n$ with t_n as the observational basis and with t_1 as the sentences at the highest level of abstraction. To get maximum generality we can think of a reduction relation R , or better a series of relations R', R'' etc. s.t. $t_1 R t_2$, $t_2 R' t_3, \dots, t_{n-1} R'' t_n$ with the R 's not necessarily distinct. All we demand then is that the more abstract theoretical levels 'preserve consequence' in the sense that we have been using ever since Eberle, i.e. if $t_i \vdash s$ then $t_{i-1} \vdash a(s)$

for some s analogue. This extremely general notion has all of the deductivist notions as a special consequence. We consider it a virtue that no account a priori of the relation R , or in the analogous jargon, the representing function that maps vocabulary onto vocabulary, is offered. It certainly is important for methodologists to analyze actual reduction instances. It is worth mentioning that Nagel's reduction example is one of the few actual analyses in the philosophic literature. All we hope to do here is open up the formal analysis sufficiently to give the possibility of including actual instances. Our offering although extremely general is not vacuous for something of importance is contained in the notion of an analogue. What that is, is that the model(s) of the higher levels t 's are shown adequate to the lower level t 's. The notion of the persistence of a model throughout the theoretical hierarchy will be the notion that furnishes us with an account of the way ontological considerations are motivated via theoretic explanation. This is developed in Chapter VI below.

There is another area of clarification required. Eberle had claimed that under the representing function the models were 'almost isomorphic'. This we should recall was the principle that the models enabled analogues to be formulated in a sense made clear in (a) and (b) p.135-36. Eberle's mapping, however was quite precise, precise enough to be equivalent to the Tarskian notion. And in the Tarskian

or equivalently the Nagelian paradigm there are exact analogues in T_1 for every T_2 sentence. Given the surfacing of the need to consider this as an oversimplification of actual practice, problems of goodness of fit etc., we can hope that more flexible mappings than the one defined by Eberle are definable. Eberle had mentioned that it is possible that the function, f , map into 'approximations' or what is more appropriately called 'neighborhoods'. This is highly desirable once we realize that it is not the problem of error in some perjorative sense, but the realization that science deals with abstractions--that is to say that the data on any given level may be altered in a variety of ways before it can be applied as data to a higher level. Again the point is not to set up requirements, a priori, for stringency, etc., but rather to furnish the abstract possibility of characterizing whatever turns out to be a desirable extension of actual practice. This is not the time to offer a lesson in the weakenings of 'isomorphisms' available within the theory of formal algebras. But we call the reader's attention to the work of Apostel⁴⁸ a small segment of which is reproduced in the Appendix C. So we should rightly substitute for 'isomorphism' in Eberle (a) and (b) some suitable weaker relation. Again the point is not to decide between, but to make the possibilities clear. In fact we should not be surprised if the various relations change over time given the history of science, improvement of experimental techniques, instrumentations, etc.

Our issue, given the more realistic view of the complexities of the structure of theories and their models is to comment on the ontological consequences of the various definable relations. If we take $K \& O$ as the extreme, i.e. if $T_2 = \{s_n^1, \dots, s_n^k\}$ then $T_1 R_{K \& O} T_2$ then $T_1 \vdash s_n^i$ or better $T_1 \vdash a(s_n^i)$ where s_n and $a(s_n)$ are not necessarily distinct, we can see that $R_{K \& O}$ affords clear ontological warrant, at least if we eschew instrumentalism. The reason for this is fairly obvious, since the only elements of T_2 retained are the observational, if we accept the Sellarian view that theories explain by virtue of telling us what our observation statements are really about⁴⁹ then with the discarding of all the rest of T_2 , and relative to the reduction in question the T_1 entities are the only ones available to be ontologically committed to. Of course, the very minimality of the reduction makes it plausible that a wide variety of T 's could equally serve. That is, there may be a large number of mutually inconsistent T 's s.t. $TR_{K \& O} T_2$. So although we have ontological warrant here it is by no means definitive.

As we require more of our reductions, with the other limit $T_1 R T_2$ or again $T_1 \vdash a(s)$ for all s in T_2 , we are clearly limiting the number of reducers available. But notice that we have a stronger warrant for the entities of T_1 . For T_1 and its ontology are shown to be, possibly, persistent throughout a large number of theoretic structures i.e. the various s_i 's that make up T_2 . The reduction thereby both stabilizes our acceptance of the relations

defined in T_2 , by showing them to be consequences of a persistent reducer and enables us, short of a belief in miracles, to hold that the reason for the persistence of T_1 and the correctness of T_2 its laws etc. is that T_2 is really about the entities that T_1 speaks of.

Of course as T_1 reduces other theories the ontological warrant of T_1 is strengthened. I claim that the analysis offered is patently obvious in the most general and pervasive instance of a reducer known to the history of science. That is the reduction of the various theories of Chemistry, Heat, parts of Biology, etc., to the theories of Particle Physics. Notice, Particle Physics is not a stable entity, but rather has a life history, and a life history determined to a considerable extent by the need for Particle Physics to be an effective reducer. Recall that Quantum Physics was formulated in order to account for the Balmer series for Hydrogen, that is, to be a reducer.

Quantum Physics, the most successful reducer in the history of science to date, was formulated in light of the ontological presumption that the properties of the light emission of chemical elements be explained in terms of the properties of physical micro-structure, adequate to the task. Notice, in respect of K & O, that the spectrum for light emission of hydrogen was by no means an observation statement, but already a complex theoretic analysis of the observational data.

Before we turn our attention to other matters, let us look at an example offered by Eberle to demonstrate the flexibility of his account. The analysis of the reduction of Galileo's Law of Free fall (G) to the Newtonian Law of Gravitation (N). This is a notorious example since it challenges any 'deductivist' account since (G) and (N) are 'inconsistent' in at least the sense that for (G) the speed of a body falling is constant, whereas for (N) it is the rate of fall that is constant, the speed is itself accelerating. Eberle takes the relevant laws as; "(G) there is a real number k , s.t. for every body b , $g(b, h(b)) = k$ where ' $g(b, h(b))$ ' reads the acceleration due to gravity on b at the height of b , $h(b)$. "(M) For every body b , $g(b, h(b)) = G \cdot M / [R + h(b)]^2$ " where G is the constant of gravity, M the mass of the earth and R the radius of the earth. According to Eberle's formulation (G) is not reducible to (N) "roughly because the gravitational acceleration, according to Galileo's law does not depend on the height $h(b)$ at which it is determined,"⁵⁰ Eberle then uses a technique of Tarski the notion of 'relative interpretability' to give a syntactic account of the reduction consistent with his views. Notice, as is well known there is a precise relation between (G), and (N) and one which approaches being deductive in a sense to be made clear immediately.

The definition of the relativization of a sentence ϕ to a predicate π not occurring in ϕ as in follows (where π is unary);
The relativization of ϕ to π is that sentence which results

from ϕ by replacing every subformula of ϕ of the form $\forall x \psi$ by $\forall x [\pi x \rightarrow \psi]$ and every subformula of the form $\exists x \phi$ by $\exists x [\pi x \wedge \phi]$ ". The relativization of (G) to π reads " $(G\pi)$ there is a real number k , s. t. πk and for every body, b , if πb then $g(b, h(b)) = k$." With the following definitions of " $(D) (\forall x) (\pi x \leftrightarrow x \text{ is a body} \rightarrow h(b) = 0)$ " By letting $k = G.M/R^2$ we can easily verify the following; (1) the conjunction of (N), (A) and (D) imply $(G\pi)$; hence (2), the theory consisting of (G) is relatively interpretable in the theory consisting of (N) and (A)."⁵¹ Eberle then asserts, "Recalling our example, we note that the limit according to Newton's law, of the magnitude of gravitational acceleration as the height $h(b)$ of a body b approaches zero, is just what it should be according to Galileo's law. Thus if the appropriate term in Newton's law is replaced by a term denoting a limit whose condition of convergence is just that expressed by the definiens in the definition of π , then Galileo's law (as it stands) is reducible to this alteration of Newton's theory by a function which represents Galileo's constant k by the limit term."⁵²

Eberle's account is of interest for it shows us an application of the principles discussed. In particular it shows how we can in relation to a predicate of T_2 (G) map the arguments for that predicate into an equivalence class of values for some predicate of T_1 (N). In this case map all values of $h(b)$ onto 0 where 0 is the measure given by an experimentally indistinguishable class of heights relative

to fairly primitive techniques for measuring acceleration. Eberle thinks his account is of general interest and asserts, "Generally the condition under which the predicted values of one theory converge to those of another seem special cases of ideal conditions."⁵³

Unhappily, no such general set of ideal conditions, or 'limits' is ascertainable at least not in the physical sciences. That there is no general set of limit conditions under which old theories reduce to new has been strenuously argued by Bunge⁵⁴ using counter examples for those cases where it was thought that some clear sense of limit applies. It is unwieldy to represent these examples in the body of the text but see Appendix D for a synopsis of the examples offered. A review of the examples offered shows that much of the diversity of 'limit' conditions is to be found in the more recent areas of Physics, an area where, understandably there is much instability and theoretic competition. But it is important to note the complexity of the subject matter when we are working from examples in the actual practice of the special sciences. For as we should recall, the point of this section was to demonstrate the complexity of the informed correlate for the intuitive but unexamined notion of correspondence as found in the writings of the MBIT. It certainly seems to be a reasonable conjecture that whatever complexities found in the physical sciences are far surpassed in the welter of theory and partial theory that goes under the name of Psychology. In

fact even if we limit ourselves to that relatively well ordered fragment of Psychology referred to as Experimental Psychology the complexity of the various theoretic choices, say, in terms of the stochastic models for data is overwhelming.⁵⁵

Whatever else we have discarded, we have maintained a basic commitment to a deductivist model if only in the extremely complex sense developed. This contention, that the reducing theory ought to include analogues of the statements of the reduced theory has been challenged on a basic epistemological level. Perhaps the complexities we have generated are, artificially, the result of the Procrustean tendencies inherent in the deductivist account. It is to this challenge that we turn our attention.

FOOTNOTES

Chapter IV

¹R. Eberle, "Replacing One Theory by Another Under Preservation of a Given Feature," Philosophy of Science, vol. 38.

²Ibid., p. 489. What are called 'representing functions' may be regarded as certain Boolean homomorphisms. More specifically, they are variable-substitution preserving homomorphisms in cylindric (or, alternatively, in polyadic) Boolean algebras whose universes are sets of expressions.

³This is recursively characterized as

(1) (a) $[\prod_{i=0}^{n-1} r_i]_{\beta} = [\delta_0, \dots, \delta_{n-1}]$ and

(b) $[r_0 = r_1]_{\beta} = [\delta_0 = \delta_1]$ where $\delta_i = r_i$ if $\alpha \neq r_i$ and $\delta_i = \beta$ if $\alpha = r_i$.

(2) $[\phi \rightarrow \psi]_{\beta} = [\phi_{\beta} \rightarrow \psi_{\beta}]$ and similarly for other connectives.

(3) If $\phi = \forall x \psi$, then (a) $\phi_{\beta} = \forall x [\psi_{\beta}]$ if α is free in ϕ and $\psi \neq \beta$, and (b) $\phi_{\beta} = \phi$ otherwise; and similarly for existential formulas; where $\prod$ is an n-place predicate and where x and δ are variables.

⁴Ibid., p. 489.

⁵Ibid., p. 489. Note: subscripts are reversed in Eberle's text; we however always use 'T₁' for the reducing theory and 'T₂' for the reduced.

⁶Ibid., p. 492. With subscripts reversed.

⁷Ibid., p. 492.

⁸B. Mates, Elementary Logic (New York: Oxford University Press, 1972).

⁹By 'sentence' we mean a formula in standard syntax with no variable free.

¹⁰Ibid., p. 179.

¹¹Ibid., p. 178.

¹²Eberle, "Replacing One Theory...", p. 490.

¹³A. Tarski, in collaboration with A. Mostowski and R.M. Robinson, Undecidable Theories (Amsterdam: North Holland Publishing Company, 1953), p. 21.

¹⁴E. Nagel, The Structure of Science (New York: Harcourt, Brace & World, 1961), p. 344.

¹⁵Eberle, "Replacing One Theory...", p. 494.

¹⁶The original statement of Nagel's views is in E. Nagel, "The Meaning of Reduction in the Natural Sciences," Science and Civilization, Stauffner (ed.) (Madison: University of Wisconsin Press, 1949). The mature statement is in Nagel, Structure of Science, Chapter 11. A recent clarificatory restatement is in E. Nagel, "Issues in the Logic of Reduction," Mind, Science and History, Kiefer and Munitz (ed.) (Albany: State University of New York Press, 1970).

¹⁷The rules of correspondence or 'coordinating definition' are "a set of rules that in effect assign an empirical content to the abstract calculus by relating it to the concrete materials of observation and experimentation". The abstract calculus "is the logical skeleton of the explanatory system...that implicitly defines the basic notions of the sys-

tem." Nagel, Structure of Science, p. 80.

¹⁸Ibid., p. 342.

¹⁹Stauffner, Science and Civilization, p.301

²⁰Idem, Structure of Science, p. 352.

²¹The borrowed laws are laws necessary for S that "may not fall within the acknowledged province of S and may not be explained by any theory of S". Ibid., p. 348.

²²Ibid., pp. 353-54.

²³Ibid., fn. 5, p. 355.

²⁴Nagel, "Issues in the Logic...", p. 126.

²⁵Idem, Structure of Science, p. 354.

²⁶Ibid., Chapter 6, sec 3. Nagel also considers as plausible the view that the status of the members of B is like that of the coordinating definitions. The definitions in question would be stipulations whose adequacy is judged by the pragmatic efficacy of the reduction as a whole. He does, however, consider "these alternative views and supporting reasons...not necessarily incompatible". (p. 356, op.cit.) His argument for the lack of incompatibility of views of these sorts is found for the general case in Chapter 6, sec. 3.

²⁷Ibid., pp. 352-53.

²⁸Ibid., p. 353.

²⁹Kemeny and Oppenheim, "On Reduction," B. Brody, Readings in the Philosophy of Science (Englewood Cliffs: Prentice Hall, 1969).

³⁰Nagel, Structure of Science, p. 358.

³¹Ibid., p. 360.

³²Ibid., p. 361.

³³(4) raises a host of interesting issues since of (4) $T^* \not\equiv T_2$, but rather some corrected version of T_2 . We will return to this, unnoticed aspect of Nagel when we discuss the challenge to Nagel from Feyerabend.

³⁴Nagel, "Issues in the Logic...", pp. 122 ff.

³⁵Kemeny and Oppenheim, "On Reduction," p. 310, with 'T' for 'T' and vice versa.

³⁶Nagel, Structure of Science, p. 358.

³⁷Quine, "Ontological Reduction".

³⁸Tarski, Undecidable Theories.

³⁹Tarski gives an examples: "A possible definition of $\triangleright$ in T is any sentence of the form $\wedge x \wedge y (x \triangleright y \leftrightarrow \phi)$ where ϕ is a formula of T (x and y being any two variables s.t. no variable different from both of them occurs free in ϕ)." (op. cit. p. 20)

⁴⁰Kemeny and Oppenheim, "On Reduction," p. 318, fn. 11. See also definition 6, p. 314. The crucial aspect of K & O's claim is state in, 'Definition 3. $\text{Red}(T_1 T_2, D)$ if:...

(2) any part of O explainable by means of T_2 is explainable by T_1 ...' (op. cit. p. 313).

Where ' O ' names the observational data of T_2 in the strong sense of any body of observational data consistent with T_2 . (op. cit., p. 314, fn. 11; see also Definition 6, p. 314)

⁴¹See Eberle.

⁴²Kemeny and Oppenheim, "On Reduction," p. 307.

⁴³Ibid., p. 308.

⁴⁴See page 144, *supra* for the notation

⁴⁵Kemeny and Oppenheim, "On Reduction," p. 308.

⁴⁶*Ibid.*, p. 309.

⁴⁷P. Suppes, "Models of Data," Studies in the Methodology and Foundations of Science (New York: Humanities Press, 1969), p. 34.

⁴⁸L. Apostel, "Toward the Formal Study of Models in the Non-Formal Sciences," The Concept and the Role of the Model in the Mathematical and Natural Sciences (Dordrecht, Holland: D. Reidel Publishing Company, 1961)

⁴⁹W. Sellars, "The Language of Theories," Current Issues in the Philosophy of Science, Feigl and Maxwell eds. (New York: Holt, Rinehart and Winston, 1961).

⁵⁰Eberle, "Replacing One Theory...", p. 496.

⁵¹*Ibid.*, p. 496. (A) is the sentence 'No bodies are real numbers' common to the theories that include (G) and (N).

⁵²*Ibid.*, p. 498.

⁵³*Ibid.*

⁵⁴M. Bunge, "Problems Concerning Inter-Theory Relations," Induction, Physics and Ethics, Weingartner and Zecha (eds.) (Dordrecht, Holland: D. Reidel Publishing Company, 1970)

⁵⁵For an example of the complexities see selections 16 and 18 in Suppes, "Studies in the Methodology...". See also almost any selection in Readings in Mathematical Psychology, Luce, Bush and Galanter (eds.) (New York: John Wiley and Sons, 1963), vol. 1.

CHAPTER V

At the end of the last chapter we had remarked that the problem of positing a univocal relation between 'levels of theories' has an obvious analogy with the diversity of connections between 'theories themselves'.¹ In fact, given the diversity of such connections, the attempt to subsume these diverse connections under one heading, e.g., deducibility seems highly artificial. This diversity, presented in the literature via highly persuasive examples has brought into question the whole 'technical' attitude in the Philosophy of Science--especially in respect of the tendencies of writers in that tradition to argue for a cumulative picture of scientific growth.² Clearly, if we require, as we have, that theories in the reduction relation have some fairly straightforward sense of analogue definable between them, we are espousing some version, albeit a weak one, of the view that focuses on the similarities, and even the desirability, of these similarities between theories.

One of the leading spokesmen against the view that science is inherently cumulative is P. Feyerabend. Feyerabend's concern is methodological. He maintains that his views should be taken as grounding the "principle of proli-

feration: Invent and elaborate theories which are inconsistent with the accepted point of view even if the latter should happen to be highly confirmed and generally accepted."³ It is not clear what the principle of proliferation comes to since inconsistency is hardly definitive of relevant choices. As we have seen in the example of the relation between Galileo's Law of Free Fall and the Law of Gravitation, two theories can be 'inconsistent' although clear analogues of each other.⁴ That is, even inconsistent theories can be seen as part of the cumulative process constituting the development of science.

We now turn to the views of Feyerabend. Owing to the complexity of the issues and the responses, we utilize the device of number indices that show analytical priority. Hopefully this makes the position easier to follow. In any event it serves as a useful index for cross referencing.

0. I have deep sympathy with the positions I see Paul Feyerabend maintaining; most especially in the need for total freedom of expression and conceptualization.⁵ As a corollary of the aforementioned, I concur with the view that the special sciences are often presented in an incredibly pompous and misleading fashion and that both scientists and methodologists frequently misdescribe the coherence and facticity of the productions of theoreticians.⁶ Further, this misrepresentation, whether intentional or merely institutional, is a major factor in the repression of peoples

and in the destruction of the critical faculties of thinkers; whether students or professors.⁷

Moreover I agree that, even in some ultimate sense, totally divergent points of view may be equally correct although mutually inconsistent or even incommensurable, in some deep and profound sense of these three turgid terms.⁸ I further am willing to maintain that aesthetic criteria are necessarily involved in all human choices and especially in the choices that govern our acceptance or rejection of scientific proposals; that rationality is a much overrated faculty and that the word 'rationality' is frequently used as an empirically empty but politically useful tool.⁹

Lastly I agree that a fundamentally historical perspective is a necessary condition for making sense of philosophy and especially those philosophic aspects of science that are the legitimate concern of methodologists and that the need for the historical perspective reflects the ultimately dialectical nature of conceptual development.¹⁰

The task I set myself in the following is to work around these areas of profound agreement and to criticize the narrowness, the misrepresentations and especially the unwillingness to extend standards of liberalism to opponents, within the works of Feyerabend himself.

1. F(eyerabend) meets our issue at what is, seemingly, a crucial point. Earlier we had presented the view that ' $p \in C_n(T)$ ' for some event description ' p ' and theory ' T '

is at the center of any discussion of explanation in the special sciences. Further, after a careful development we centered on a notion of reduction with syntactic relations at its core.

1.1 F, taking Nagel's position as his object of concern denies the cogency of the deductivist view. He denies

(A) reduction or explanation is (or should be) by derivation.

(B) the meaning of (observational) terms are invariant with respect to both reduction and explanation.¹¹

1.11 F denies that $T_1 \vdash T_2$ is an adequate account of reduction on the grounds that for de facto reduction instances T_1 is inconsistent with T_2 . He further argues that the weaker reduction paradigm, $T_1 \cup B \vdash T_2$ is inadequate as well, on the grounds that the sentences in B are either inconsistent with T_1 or T_2 or both.¹²

1.2 F takes 1.1 (A) as implying (B) and quotes Nagel accordingly: "It is of the utmost importance that the expressions peculiar to a science will possess meanings that are fixed by its own usage and are therefore intelligible in terms of its own rules of usage, whether or not the science has been or will be explained in terms of some other discipline."¹³

1.21 The implication between 1.1 (A) and (B) is fairly simple to see. If the terms in the sentences in B (in the

case $T_1 \cup B \vdash T_2$) do not occur in the same sense as their occurrences in the sentences of T_1 and T_2 , the derivation is fallacious; committing the standard fallacy of equivocation.

1.211 Notice the statement is an oversimplification. In terms of the five-fold distinction Nagel maintains for theoretic terms (see p. 144 above) what we require is that for all terms S_2 in P_2 there is a b in B such that S_2 is coordinated with some P_1 condition. Recall for $T_2 = \langle P_2, C_2, E_2, O_2, L_2 \rangle$. P_2 is the set of theoretical postulates for T_2 . Nagel, characteristically, assumes uniformity for the terms in the remaining aspects of T_2 in the reducing theory T_1 . Clearly the definition can be contracted or expanded to handle special cases. Or perhaps reductions may even be blocked because of the lack of coordinating conditions between aspects of T_2 other than P_2 . For example, an argument against the MBIT asserts that even if characteristically mental predicates could be defined in terms of neurophysiological conditions, the underlying grammar of the mental is so different from that of the physical, intentional for the former, extensional for the latter, that psychological theories could never be captured in Physicalist terms. See 6.311 below, for further discussion.

1.22 Given F's denial of 1.1(B) and the relation just mentioned, F's denial of 1.1(A) is required even without independent grounds. As we shall see, the denial of meaning

invariance is the fundamental position that characterized F's attitude and this most relevantly for observational sentences.¹⁴

1.221 This raises issues of deep methodological concern, for in the standard view it is the availability of sets of observation sentences, logically independent of the theories for which they stand as potential confirmers that account for the empirical (as opposed to the metaphysical) nature of scientific theories. Further, it is this body of observation sentences that stand as the ground for decisions as between inconsistent but adequate alternative theories (explanations).

1.3 F's rejection of the deductivist paradigm is based on,
(a) historical instances of de facto reduction instances
(b) methodological principles seen as normative in respect of these instances.

1.4 The position that F offers in lieu of the deductivist theory is most cogently referred to as 'the replacement' theory of reduction.

What does happen (in reduction) is, rather, a complete replacement of the ontology (and perhaps even the formalism) of T' by the ontology (and the formalism of T as a corresponding change in the meanings of the descriptive elements in the formalism of T' (provided these elements and this formalism are still used).¹⁵

Altogether, the use of T will necessitate the elimination both of the conceptual apparatus of T' and the laws of T'. The conceptual apparatus will have to be eliminated because it involves principles...which are inconsistent with the principles of T and the laws will have to be eliminated because they are inconsistent with what follows from T...This being the case the demand for explanation and reduction (in the sense of Nagel) clearly cannot arise...For such a demand would imply the demand to derive from correct premises what is false and to incorporate what is incommensurable.¹⁶

1.41 We take notice at this juncture that F's account is claimed to be most suited to "such comprehensive structures of thought as Aristotle's theory of motion, the impetus theory, Newton's celestial mechanics, Maxwell's electrodynamics, the theory of relativity and the quantum theory."¹⁷

1.42 The replacement view of theories has one immediate problem. If we take the standard instances of de facto reduction, e.g. Newtonian Mechanics and the Galilean Law of Free Fall, the reduced theory is still both quoted and used despite the fact of reduction. This is evidence against F's claim that the reduced theory is replaced.

1.421 If F claims that $T_1 RT_2$ implies some change in meaning of T_2 after the reduction, then he needs some criteria for the identity of theories, at least relative to such change. That is what is called 'Galileo's Law of Free Fall' both prior to and posterior to the purported reduction to Newtonian Mechanics? Note, if such criteria are not forth-

coming, we cannot even state the reduction of some theory T_1 to T_2 on pain of committing the fallacy of equivocation.

1.422 We note that the Hegelian tendencies espoused by F might afford some solution¹⁸ to the vexing problem of conceptual change. Unfortunately, quoting obscure passages from famous thinkers does not furnish a plausible alternative to careful analysis. Despite the centrality of these problems to F's views, his attitude towards such clarification is less than favorable. "As far as I am concerned even the most detailed conversation about meanings belongs in the gossip column and have no place in the theory of knowledge."¹⁹

1.423 We remark that the example of Galileo's Law of Free Fall may be ill-suited to our purpose, since, presumably, the issue is with global theories of the sort mentioned in

1.41 We choose the example because it is obvious and is one which can be handled by the deductivist account (cf. Chapter IV supra). The same argument could easily be made for more global examples, e.g. Special Relativity and Newtonian Mechanics.

1.4231 The importance of 'global theories' for F's position is based on "the influence upon our thinking of a comprehensive scientific theory...(that)...affects our general beliefs and expectations and thereby our experience and our conceptions about reality."²⁰ This is tied, in Feyerabend,

to the "Kantian" position of the "pervasive character of theoretical assumptions" from which it follows that "what is regarded as 'nature' at a particular time is our own product in the sense that all the features ascribed to it have been first invented by us and then used for bringing order into our surroundings."²¹

This deep issue has many connections with the discussion to follow. For example, the 'principle of proliferation' (see p. 175 supra) is required to counter the pervasive character of such comprehensive theoretical structures; so that "it is still possible to specify facts that are inconsistent with it."²² A more technical statement of this position is found in the 'connectedness' of strong alternative theories (2.6i's below). The pervasiveness of such global theories extends to the observation language itself.²³

We note, finally that Appendix D gives scientific reasons why the mapping of the sort envisioned in respect of Galileo and Newton may not exist if we include in the class of global theories such items as General Relativity Newtonian Mechanics and Quantum Mechanics.

1.43 We note that if methodology is not to present examples only relevant to 'bacteria free environments',²⁴ the fact that Newtonian Mechanics is actually retained despite its reduction (replacement) by Special Relativity demands some account.

2. F attacks 1.1(A) based on the fact that actual pairs in the reduction relation are inconsistent, when derivability is clearly impossible.

2.1 The deductivist response to this is clear.

...the reduced laws are either derivable from the explanatory framework, or are good approximations to the laws derivable from the latter.

(ii) Moreover, it is pertinent to note that in actual scientific practice the derivation of laws from theories usually involves simplifications and approximations of various kinds so that even the laws which are allegedly entailed by a theory are in general only approximations to what is strictly entailed by it.²⁵

2.11 The view of Suppes mentioned briefly at the end of Chapter IV substantiates this position. A theory 'attaches to reality' only through a hierarchy of models each of which satisfies some goodness of fit relation that varies from science to science and from situation to situation.

2.2 F denies the cogency of the move in 2.1 since, "it eliminates the very procedure of criticism of theories"²⁶ held to be central to scientific progress, i.e. falsification.

2.21 F's objections is clearly too strong. As long as our use of approximations is well managed, as long as we do not relinquish the role of falsifiability for regressive purposes the use of approximations is harmless. That is to say, as long as the purpose of approximations is not to make our

theories less responsive to the data basis upon which they are constructed. Notice, we make no claim as to the epistemological status of the data basis, rather we merely require that it have the logical role of being a data basis. How close our data fits our theories varies widely. This is agreed on all hands.²⁷ What is required is that scientists eventually reject theories where the tendency towards weaker and weaker goodness of fit requirements seems more apparent than the tightening of such relations. But even this might be too stringent a demand. Ad hoc adaptations, including weakening the relations between models might be methodologically desirable in a given historical period. This is F's own view.²⁸ The interesting psychological aspect of F's work is his unwillingness to permit his critics the same license he permits himself.

2.22 What seems most bizarre about F's position is his total lack of sympathy for formal analysis.²⁹ Clearly, if any distinction between 'progressive' as opposed to 'regressive' weakenings of ideal types (e.g., deductive relationships) is to be made, some description of the actual details is required. For this there seems no candidate for a meta-language with the flexibility and precision of standard meta-mathematics.

2.23 There is another possible interpretation of F's position. That is the view that all concrete methodological proposals are equally inappropriate.³⁰ But that leads to

the following: Either science is completely arbitrary or it is the job of methodologists to tease out those criteria that legitimize scientific development. The question is which meta-methodological paradigm affords the greatest possibility of fruitful development. If as F maintains, conceptual analysis à la Hegel is to do the job that deductivist models have heretofore done, we await some examples. The languages of the deductivists are, for the most part, less than a century old. The deductivist program has been fraught with oversimplifications and obscurantist disputes. "Still it would be unwise to bring the process to a standstill in the very beginning by the remark that the new ideas are undeveloped, general and metaphysical. It takes time to build a good theory."³¹ Including, of course, the theories of methodologists. The deductivist paradigm can not be judged except in some mature form. The question is as to whether logical empiricism is itself a degenerating research program.³² Work of methodologists like Suppes or Eberle points in the appropriate direction. Moreover, on F's own principles, we must judge the theoretical paradigm against relevant alternatives. F derides the logician for furnishing "inadequate instruments that make nonsense of the complex, delicate and often self-inconsistent theories (the scientist) uses."³³ Works such as Rescher's Hypothetical Reasoning,³⁴ in which an attempt to generate a principled account of the methods of reasoning from inconsistent sets as premises, are available to be compared to relevant alternatives. Where

are the alternatives?

2.231 There seems to be evidence that the logical empiricist program is progressing. Even as meager a contribution as Chapter I above shows the possibility of resolving outstanding but debilitating problems. Techniques are constantly being applied to new domains. Radical new proposals within the framework are being offered.³⁵ Dead ends are being dismissed. Notice the lack of discussions of 'grue', the problem has been turned into the fruitful enterprise of discussing criteria for entrenchment. In fact, the move towards the acceptance of pragmatic criteria seems universal and salutary. Even the 'old guard' is being moved. (See Carnap's response to a pragmatization of the notion of probability by Putnam.)³⁶

2.232 We will increasingly make use of the terminology of Lakatos³⁷ where an attempt to discuss scientific objects of a 'large size' relative to the standard analytic model, is begun. Lakatos speaks of complex combinations of theories and practices referred to collectively as 'research programs'. It is by virtue of a comparative analysis of component parts of research programs as well as a comparative analysis of competing research programs that such terms as 'regressive' and 'progressive' are given content. This view has been criticized by F and Kuhn³⁸ on the grounds that it has no content. The development in the 4.m's is an

attempt to begin to show the possibility of giving an account of 'research program', 'progressive' and 'regressive' using standard meta-mathematical techniques.

2.233 We note as an aside that when F is pressed to discuss intertheoretic relations he chooses meta-mathematics as his favored meta-language (or at least alludes to formalist procedures).³⁹

2.24 Before we leave this area, one more comment. F claims that for the deductivist the only plausible methodological principle is 'anything goes'.⁴⁰ That is, that the deductivists' demand for principles has to be so qualified that the only principle is whatever principle he can come up with. This seems incorrect. Once we accept the 'dialectical' aspect of conceptual development, the logician's tools are required. For how else are we to decide whether the relation between e.g. a theory and its models is being altered in a progressive rather than in a regressive manner. As we shall develop later, to evaluate a research program is, among other things, to make comments about the movement in the direction of closeness of fit of models. The logicians need not respond with dismay when his favorite (deductive) relation is not instantiated. His reaction should be tempered with the realization that deductive relations are the limit to which approximations tend.

2.3 F takes 1.1 (B) as sufficient to render the deductivist account superfluous (1.21, 1.22 supra). The main problem is then to deal with the incommensurability of pairs in de facto reduction relations. For Feyerabend 'incommensurability' is a term for theories, such as Classical Mechanics and Special Relativity,⁴¹ that although reduction instances are differing in their conceptual basis. F rests heavily on his notion making it central to his argument in "Explanation, Reduction and Empiricism" and "Problems of Empiricism." The concept of incommensurability, although widely used by F, is only minimally analyzed by him. There is only one place in his corpus where it is defined at all.⁴² The 2.5i's below is one attempt to give F's notion some clear logical content.

2.31 Before we begin, as a general statement of position, we admit that there are pairs of theories in the reduction relation that are incommensurables; what we argue is that there is some possibility of comparing incommensurables and that this possibility is based on standard meta-mathematical techniques. (F seems to agree, 2.232 supra.)

2.311 We further grant that for our purposes, the MBIT relevant reducers are, at least prima facie, incommensurable. We take as given that say, Freudian theory and the Theory of Normal Neurons are incommensurable and that the latter is a potential reducer of the former.

2.4 F uses the term 'incommensurable' as a descriptive label for many pairs quoted in the literature on reduction.⁴⁴ As mentioned, F rarely offers an account of the notion. Fortunately such an account does occur.⁴⁵ "Two theories will be called incommensurable if their main descriptive terms depend on mutually inconsistent principles."⁴⁶

2.41 A perusal of F's examples show that by 'main descriptive terms' he means extra-logical expression of all types. So, "...incommensurable theories may not possess any comparable consequences observational or otherwise"⁴⁷ or "complete replacement of statements (and perhaps even descriptions) in a certain domain. More especially, it may replace natural interpretations by others."⁴⁸ 'Natural interpretations' is intended as the label for the most elementary interpretations of sensory phenomena.⁴⁹ Other examples show how strongly he interprets the consequences of incommensurability. So, "the new conceptual scheme (relativity theory) does not only deny the existence of classical states of affairs--it does not even permit us to formulate statements expressing such states of affairs (there is no arrangement in the Minkowski diagram that corresponds to the classical situation)."⁵⁰ With respect to impetus theory and Newtonian mechanics he states, "...the main concepts of the former can neither be defined on the basis of the primitive descriptive terms of the latter nor related to them via a correct empirical statement..."⁵¹ "...And this is true

because such empirical statements are inconsistent with another part of the explanans (reducing theory)."⁵²

2.412 We note that F has maintained that "theories can be interpreted in different ways. They will be incommensurable in some interpretations and commensurable on others."⁵³ We note this observation because it motivates our formal analysis below.

2.5 The problem with F's proposals is that they rely on examples which are only minimally analyzed. Moreover, the only clear statement in his corpus (2.4 supra) seems to demand that incommensurable theories are inconsistent as well, at least in the sense of having 'inconsistent principles'.

2.51 Many authors including Nagel⁵⁴ have commented on the apparent incoherence of F's views. For if either principles or laws of T are inconsistent with principles or laws of T', the formalism of T and T' must be logically comparable in at least the minimal sense that for some principles P, P' or laws L, L' and some sentences S, S'; $P, L, S \models T$ and $P', L', S' \models T'$ ' $S \wedge S'$ ' must be well formed and $V(S \wedge S')$ must be well defined for some admissible valuation V of T and T'.

2.511 We use the truth values 't', 'f' and later 'u' as shorthand for the satisfiability, unsatisfiability and undecidability, respectively, of a model in a theory and define a value, v, from the theory to the model in the stan-

dard sense (cf. Chapter IV supra). We will use the term 'admissible valuation' in proofs which follow and all we will require is the minimal claim that "a set X of sentences of a language L is satisfied by an admissible valuation L iff $v(A)=t$ for every member A of X "⁵⁵ and that v assign at most one of the pair $\langle t, f \rangle$ to A for all sentences A of L .

2.52 If we take the formalism of a theory to include the wff's of the theory and if the ontology of a theory means, at least, some elements in the range of admissible valuations of the theory, i.e., some model of the theory, then for inconsistency of theories to be ascertainable there must be a sharing of the formalism and the ontology in the sense so painstakingly denied (2.41). For just at S, S' and at $V(S)$ and $V(S')$ there must be some element in the formalism of $T \wedge T'$ and in the intersection of the ranges of the admissible valuations. But clearly if the intersection of the two formalisms and of the two ontologies is non-empty, then there is a clear sense in which relatedness is manifest. Or to use F 's favored expression, in which replacement is not complete.

2.521 There is a sense of deep incompatibility that is at the heart of F 's position. Recall F 's claim that "not a single descriptive of T (the reduced theory) can be incorporated into T' (the reducing theory)" that for incommensurable pairs, "the meanings of all descriptive terms of

the two theories, primitive as well as defined terms will be different."⁵⁶ F takes these views to the extreme. So we find "the conceptual disparity...(between Classical Mechanics and Special Relativity)...infects even the most 'ordinary' situations: and the relativistic concept of a certain shape, such as a table, or of a certain temporal sequence, such as my saying 'yes' will differ from the corresponding classical concept..."⁵⁷ We remark that F accepted the criticism⁵⁸ encapsulated in 2.52 and asserts that "...it indicates that the use of incommensurable theories for the purposes of criticism must be based on methods which do not depend on the comparison of statements with identical constituents."⁵⁹ We will return to F's analysis when we have a clearer formal statement.

2.522 It is not clear that 'replacement' is an inappropriate label for the relation between theories that do in fact share formalisms and ontologies. What is clear is that in so far as they are deemed inconsistent they include non-replaced formal and ontological elements.

2.53 The notion of incommensurability connotes non-comparability; but inconsistent pairs are, in fact, comparable in at least the minimal sense outlined above (2.52). What then can F mean by "Incommensurable"? There are clues, firstly the claim that all sentences including the most remote (the observational elements of the formalism) are invested with theoretic significance and secondly, the examples of incom-

mensurables offered.

2.531 One characteristic example offered is that of Impetus Theory and Classical Mechanics.⁶⁰ The incommensurability of the two is a function of the occurrence in the former of the following principle: "motion is a process arising from the continuous action of a source of motion of a 'motor' and a 'thing moving'".⁶¹ The presumption is, then, that no element of Classical Mechanics can be equated with impetus, since the fundamental principle of Impetus Theory is not a principle or even a consequence of Classical Mechanics. In fact it is rightly argued that the concept of 'motor' and even the concept of 'cause' implicit in Impetus Theory is alien to its analogues in Classical Mechanics.

2.54 The first task is to give an account of the claim that all elements of the theory are qualified by the principle quoted. (2.5311). A plausible construal of this claim is that, in some sense, the principle quoted is a presupposition of Impetus Theory. A word about where we are heading. It is clear that the principle quoted, P, is, at least, a consequence of the theory in question, T. The question is what more, if anything it is. The intuition F seems to working from is the crucial role of P vis a vis T. This in the sense that it is related to every consequence of T. A plausible strengthening of the notion of consequence is that P is part of the generator of the consequence class of T, that is, that P is a member of a small set of T sufficient

to yield as deductive consequence all the rest of T . This marks P as a postulate of T in the familiar logical empiricist sense. This is the sense of, say, Nagel. But it is just that sense that F seems to want to deny. That is to say F wants a role for P stronger than that of being a postulate. For although the postulates of a theory are sufficient for the theory they are not necessary for it. The mere sufficiency of the postulates is what leaves open the possibility of the independence of the consequences of T from the particular postulate set. The quotations taken from F show clearly that this is far from his intention. What our construction will therefore focus on is the necessity of P for T . As we shall see when we develop our notion further there are indicators that our construction satisfies F 's intuitions.

2.541 We offer the following explication of 'incommensurable': T, T' are incommensurable if for some non-trivial presupposition P' of T' ; $v(P')=f$ and $v(T)=t$, for some admissible valuation v in the set of valuations V of L , a language in which T, T' are expressed. We say further: T, T' are incommensurable under v if v is a valuation as described above.

2.5411 By 'non-trivial presupposition' we mean a presupposition other than a theorem of logic; what is frequently referred to as a 'proper axiom'.

2.5412 Notice that 'incommensurable' is not symmetric on this interpretation. The reason for this is as follows: T may be incommensurable with T' since T fails to meet some deep principle of T'. T, however, being a lower order theory (perhaps an empirical generalization) may not have any deep principles for T' to meet. So, for example, some statement about the human emotions may be incommensurable with neurophysiology since all neurophysiological entities are spatially localized whereas there are no criteria for localizing an emotional state. It may not be a principle however, of whatever 'emotion theory' we have, that emotions are not localizable. The theory may be sufficiently indeterminate in its deep principles so that the issue of localization, in some sense comparable to the localization of neurophysiological entities, may just not arise. Whence we can offer a neurophysiological account for the emotional state without running afoul of any deep principles of the psychological theory involved. Notice we are not claiming that the analysis above is correct or even universally accepted. For the Cartesian, for example, it was a principle that mental states are not localizable. But we certainly want to leave open the possibility that such a state of affairs be the case. See Shaffer⁶² for a position similar to the one we describe.

2.542 It is important to note that we are using a weak variant of the plausible definition. Stronger variants

include: '...for some valuation, v^* , special in some way' (i.e. where $v^*(T)$ is intended in some special sense) or '...for every valuation v ...' The motivation here is (2.412) but there will be other reasons manifest as we continue.

2.543 It is clear that incommensurability does not imply inconsistency. For assuming that a theory implies its pre-suppositions, in the case envisioned, $T' \Vdash P'$, if T' is incomplete in the sense of not necessarily $T' \Vdash A$ or $T' \nVdash A$, for all sentences A expressible in T' , and it is a warranted assumption that any scientific theory is incomplete in that sense, $T \nVdash P'$ does not imply $T \Vdash \neg P'$. Whence T, T' are not necessarily, if incommensurable, inconsistent. We define 'X implies Y' (in symbols $X \Vdash Y$) if and only if for every admissible valuation, v , if $v(X)=t$, then $v(Y)=t$. Notice if $\Vdash Y$ then for every $v, v(Y)=t$. These definitions although standard are not the only ones available. We choose them in order to motivate our analysis of presupposition in terms of a minimally tri-valent language, 2.5543ff. See 2.55541 for an alternative we do not take.

2.5431 We remark that there are notions of presuppositions that differ from the implicative one we offer. So, for example, Bunge offers an essentially pragmatic notion.

To say that a theory A is based on a theory B means that A presupposes B , i.e. that B belongs to the background of A . And a theory A presupposes another theory B just in case the following conditions are met:

(a) B is a necessary condition for the meaning

or the verisimilitude of A, because A contains concepts that are elucidated in B, or statements that are justified in B, and
(b) B is not questioned while A is being built, worked out and criticized, tested or applied-- i.e., B is taken for granted pro tempora as far as A is concerned.⁶³

(b), the 'methodological side' of the notion of presuppositions is close to Nagel's sense of borrowed laws (cf. Chapter IV above) and seems relatively clear. (a) seems harder to make sense of, since Bunge requires that the 'concept of presupposition is to be kept distinct from that of entailment'.⁶⁴ We wonder what the sense of necessary condition in (a) is if we must refrain from an analysis in terms of implication. In any event our analysis of presupposition, to follow, is chosen because it illuminates the Feyerabendian thesis of incommensurability. We make no claim as to the universal acceptance of such an analysis nor for its universal applicability to contexts in which the word 'presupposes' occurs.

2.55 What still remains unclear in the definition of 'incommensurable' offered is the analysis of 'presupposition'.

There seems to be two alternatives. So, for a set X of sentences and a sentence B, X presupposes B if

(a) for every $A \in X$, $A \Vdash B$, or

(b) for every $A \in X$, $A \Vdash B$ and $\sim A \Vdash B$.

(a) is weak standard usage; the American College Dictionary defines 'presupposes' as "to assume beforehand..to imply as an antecedent condition."⁶⁵

(b) follows Van Fraassen op cit, who after Strawson⁶⁶ argues that: "A presupposes B for sentences A, B iff $V(A)=f$ then $V(B)=t$ and if $V(A)=t$ then $V(B)=t$!"⁶⁷ That is the truth of B is a necessary condition of the assignment of either 't' or 'f' to A. This latter seems a natural extension of the claim that the truth of B is a necessary condition for the meaningfulness of A, at least in the clear sense that the former is implied by the latter. Notice our claim is very strong. We do not merely demand that X imply B, but that every member of X imply B. We do this in an attempt to capture the characteristically strong claims of F, for examples see 2.521.

2.551 We assume a language, L in which T, T' are expressed. Further we assume 'choice negation' for L.⁶⁸ That is for every A in L there is a sentence $\neg A$ in L s.t. $v(A)=t$ (resp f) iff $v(\neg A)=f$ (resp t).

We do not assume that L is bivalent, that is for all A in L and v on L v maps A into the set $\{t, f\}$. The reason for this, as we shall see, is that on 2.55 (b) L is minimally tri-valent, i.e. $v(A) \in \{t, f, u\}$, Lemma I, 2.552. The assumption of choice negation for ' $\neg$ ' is governed by the need for a plausible three-valued matrix for the logical signs of L. Non-bivalence will be a consequence of the analysis of presupposition as well as the Feyerabendian claim that scientific theories have non-trivial presuppositions (2.5411).

2.552 If L has choice negation, then on 2.55(b) the presuppositional relation has the following semantical import: A presupposes B iff $\{A\} \Vdash B$, $\{\neg A\} \Vdash B$.⁶⁹

2.553 We do demand that L, although permitting presuppositional relations, permit classical deductive relations as well. Given L as a presuppositional language to be defined below, it follows that: if $X \Vdash A$ then $A \in \text{Cn}(X)$ for any definable consequence relation on L, where X is a set of sentences of L. More specifically of $\text{CNL}(X)$ is the class of classical and non-classical consequence relations definable on L then $X \Vdash A$ then $A \in \text{CNL}(X)$, i.e., $\text{CNL}(X)$ is the smallest set of sentences Y s.t. X contains Y and X is closed under C and N, classical and non-classical necessitation. (cf. 2.554(c), (d) below).

2.554 We take from Van Fraassen the following definition of a presuppositional Language, L:

"L = $\langle \text{Syn}, V, N, C, V^* \rangle$ where

(a) Syn is the syntax of L

(b) V is the set of classical valuations on L

(c) N is a non-classical valuations on L

(c) N is a non-classical consequence relation on L from sets of Syn to sets of Syn.

(d) C is the classical consequence relation on L from sets of Syn to sets of Syn.

(e) V^* is the set of V-super valuations [2.5541 below] induced by a V-N saturated set of sentences of Syn, i.e. a set of sentences of Syn closed under N for valuations in V."⁷⁰

A theory is thus a subset of Syn closed under C and N for valuations in V whence for valuations in V^* as well. This last is clear from the following.

2.5541 "If X is any subset of Syn a V-supervaluation of Syn induced by X is a valuation, x , of Syn s.t.

- (a) $s(A)=T$ iff $v(A)=T$ for all $v \in V$ that satisfy X.
- (b) $s(A)=F$ iff $v(A)=F$ for all $v \in V$ that satisfy X
- (c) $s(A)$ is undefined otherwise."⁷¹

Notice that since V^* is a set of supervaluations the theorems of L include all theorems of logic (i.e. L is at least argument sound and complete). (2.553)

This is a consequence of the following theorem.

"Let L and L^* have the same syntax and be s.t. the admissible valuations for L^* are the supervaluations for L; the $X \Vdash A$ in L^* iff $X \Vdash A$ in L for all sentences A and sets of sentences X of L."⁷²

Notice the theorem speaks of 'supervaluations for L' simpliciter, rather than supervaluation 'induced by X': the former is merely a generalization of the latter, whence the theorem holds for all sets X.

2.5542 A word about what's going on. The presuppositional languages reflect the non-standard necessitation relation inherent in the view that for presupposition B of X, A implies B for all A in X. This is a 'non-standard' necessitation relation since there is no demand that B be provable; whereas for the classical necessitation relation if

for all A, A implies B, then B is provable. The proof of this remark is clear. If we assume that for all A, A implies B then certainly for A as $C \vee \sim C$, A implies B. But for all v, $v(C \vee \sim C)$ is t whence for all v, $v(B)=t$, i.e. B is a theorem whence provable by completeness. The key to the availability of these special cases of 'non-classical' necessitation is the existence of the supervaluation that marks B true in all and only those cases where the valuation marks all of X true. If we think of a presupposition that goes true in all and only those cases where the presupposing set goes true the intuition is clear.

2.55421 Bunge⁷³ criticizes Van Fraassen's account of presupposition on the grounds that "the meaning ingredient is not taken care of." We feel, on the contrary, that the notion of meaning is captured in at least the sense relevant for formal semantics. Since, if we think of the meaning of an expression as its use, a plausible formal surrogate for the use of a symbol is the set of valuations under which the interpretation of the symbol is defined. We especially feel a strong analogy between Van Fraassen's construction and the notion of 'implicit definition' where the "basic terms of the theory are 'implicitly defined' by the postulates of the theory."⁷⁴ If the postulates of the theory are those sentences that go true in all and only those models that the theory goes true in, the analogy is complete.

2.54422 The point of these elaborate formal notions is crucial. The results alluded to show that presuppositional languages are not so different from non-presuppositional languages. At least in the crucial sense that standard deductive techniques apply within the,. The incommensurability of the theories T, T' subsets of Syn, will be a function of the non-classical nature of intertheoretic relations.

2.55423 For the reader immersed in details: the same argument is being made throughout. The techniques of standard meta-mathematical analysis, the utilization of the clarificational techniques of logic are still available to the methodologist. So far we have met the challenges of the obscuring use of first order logic (Chapter I), the tendency to reconstrue these techniques through ill-management (Chapter II), and now, finally, the challenge that the nature of scientific theories and theory change render logic irrelevant.

2.5543 We define $\underline{V} = \bigcup V^*$ of admissible valuations on L and for all $v \in \underline{V}$ and sentences $A \in L$; $v(A) \in \{t, f, u\}$
We follow Suppes⁷⁵ in defining the following three-valued matrices.

A	$\sim A$	$\wedge$	t	f	u
t	f	t	t	f	u
f	t	f	f	f	u
u	u	u	u	u	u

2.55431 The set of valuations $\underline{V}$, is minimal in the sense that all we require of $\underline{V}$ is that the range include at least the values 't' 'f' 'u'. For a richer set of valuations we can think of 'u' as shorthand for some valuations other than 't' or 'f'.⁷⁶

2.5544 It is obvious from the matrix for ' $\sim$ ' that ' $\sim$ ' is choice negation. We define: $(A \supset B) =_{df} \sim (A \wedge \sim B)$, from which it follows immediately that $V(A \supset B) = u$ iff at least one of the A or B take 'u' as values.

2.4434 We define: A is undecidable relative to v iff $v(A) = u$ for A in L and v in $\underline{V}$. Further A is undecidable in respect of X relative to v for any set of X of Syn iff $v(A^*) = u$ and $V(X) = t$ where A^* either A or $\sim A$. Notice that it follows immediately from the matrix for ' $\sim$ ' that A is undecidable at v (relative to X) iff $\sim A$ is undecidable at v (relative to X).

2.555 Given this much apparatus we return to our analysis of incommensurability (2.542) and the options for the analysis of presupposition 2.55 (a) and (b).

2.5551 Theorem I: If T and T' are incommensurable under v then either v is not an admissible valuation for the set $T \cup \{S'\}$ or $T \cup \{S'\}$ is unsatisfiable under v , for all sentences S' in T'. We take the sense of 'presupposition' to be as in 2.55 (a).

Proof: Since T and T' are incommensurable with 'presupposition' as analyzed in 2.55(a) for some presupposition P' of T' whenever $v(T)=t$ then $v(P')=f$. But since P' is a presupposition of T' for every S' in T ; $S' \Vdash P'$, i.e., for every v' s.t. $v'(S')=t$, $v'(P')=t$ as well. Now take any one of the sets $T \cup \{S'\}$ for any S' in T' . Assume that there is a v^* s.t. v^* satisfies $T \cup \{S'\}$, but then assume v^* is admissible then $v^*(T)=t$ and $v^*(\{S'\})=t$, but then v^* agrees with v at T and v^* agrees with v' at S ; but then v^* must assign both t and f to P' . But that implies v^* is not admissible contrary to our assumption.

2.55511 The point of the theorem is clear; in so far as an interpretation marks theories as incommensurable, to that extent is their simultaneous satisfiability impossible. And that in the deep sense that no sentence of T' can be incorporated, on that interpretation, into T . But that is as much to say that in so far as T is both true and incommensurable with T' under v , T can neither explain nor reduce T' in the standard deductivist sense.

2.55512 We notice that $T \cup \{S'\}$ is not inconsistent, but rather that it is uninterpretable in so far as it is incommensurable. The relation between this weaker notion and inconsistency is clear.

Theorem II: If T and T' are incommensurable under all v then $T \cup \{S'\}$ is inconsistent for all S' in T' .

Proof: Assume $T \cup \{S'\}$ is consistent but then there is a v s.t. $v(T \cup \{S'\}) = t$ where v is admissible. But this is assured by Theorem I.

2.55513 Notice that the inconsistency here is deep, i.e. T and T' are not merely inconsistent in the sense that their conjunction is unsatisfiable, but rather T is inconsistent with every sentence of T' . It is the depth of the inconsistency that marks our analysis as adequate to our purpose. But more of that when we look at F 's account of the choice between incommensurable theories. (2.6ff)

2.5552 The situation is different if we analyze 'presupposition' in the sense of 2.55(b). For ease of exposition we will prove the next theorems in a general way. Recall in 2.541 and 2.542 we spelled out the various options for an analysis of incommensurability in terms of either some v , some special v^* , or all v . We will prove the theorems in terms of 'undecidable' rather than 'undecidable relative to v '. The theorems then apply as altered to all interpretations of 'incommensurable'.

Lemma I: Let L be a language in which T and T' are expressed. Let T and T' be incommensurable in sense 2.55(b), then L is trivalent at least.

Proof: Since T and T' are incommensurable for all sentences S' in T' : $\{S'\} \Vdash P'$ and $\{\sim S'\} \Vdash P'$, for some non-trivial presupposition P' of T' . But then for all v if $v(S') = t$ then $v(P') = t$ and similarly for v and $\sim S'$.

We also know that for some v at least $v(T)=t$ and $v(P')=f$. Call this last valuation v^* . Question: What does v^* assign to S' for any S' in T' ? v^* cannot assign t or else $v^*(P')=t$ which is absurd, similarly v^* cannot assign f . But then v^* must assign some value other than t or f , i.e., L is trivalent at least.

Theorem III: If T and T' are incommensurable 2.55(b) then for all sentences S' in T' , S' is undecidable relative to T (2.5545).

Proof: Recall by assumption we have a principle P' of T' and for all S' in T' , $\{S'\} \Vdash P'$ and $\{\sim S'\} \Vdash T'$. But by incommensurability there is a v , at least, s.t. $v(T)=t$ and $v(P')=f$. But then by Lemma I $v(S')=u$, whence by the matrix for ' $\sim$ ' (2.5543) $v(\sim S')=u$. But then S' is undecidable relative to T . But the proof is general in respect of all S' in T' .

2.55521 We recall in passing that we define $\Vdash A$ as for all v , $v(A)=t$. That is to say A is a theorem of logic. If A is merely a presupposition of a set X for all valuations, s , such that $s(X)=t$, $s(A)=t$, that is to say $s(A)=t$ for all supervaluations, s . This motivates our use of the term 'undecidable' as the correlate for the value ' u '. What we effectively rule out is the following. In a 'classical' necessitation relation if for no v , $v(A)=f$ then for all v , $v(A)=t$, i.e., $\Vdash A$. In our presuppositional language it is possible that for no v , $v(A)=f$ but yet for some

$v, v(A) \neq t$ since for some v , $v(A)=u$. We certainly don't want to call such an A provable whence our definition of 'implies'. With this possibility excluded there seems no reason to refrain from the use of 'undecidable' as our interpretation for 'u'.

2.5553 Prior to generating some immediate corollaries, we define:

X and Y are not inconsistent iff $v(X \supset Y \supset A)=u$ for any arbitrary sentence A and for some v . The intuitive plausibility of the definition is apparent. If we define X and Y are inconsistent iff $X, Y \Vdash A$ for any arbitrary sentence A then by the result of Van Fraassen that guarantees classical yield for presuppositional languages (2.553ff) and the deduction theorem we have $\vdash X \supset Y \supset A$, i.e. for all v , $v(X \supset Y \supset A)=t$. But then the pair X, Y is consistent iff for some v' , $v'(X \supset Y \supset A)=f$. If we then take not inconsistent as neither consistent nor inconsistent by the interpretation of 'u' throughout we have for some v^* , $v^*(X \supset Y \supset A)=u$ for a not inconsistent pair X, Y .

2.55531 Theorem IV: If T and T' are incommensurable and if T and T' are each consistent then T and T' are not inconsistent.

Proof: If T and T' are incommensurable then for all S' in T' and for some v , at least, $v(S')=u$ just when $v(T)=t$ (Theorem III). But by the assumption of consistency for each of the T and T' there is such a v . Construing T' as

as the conjunction of the sentences S' in it, it follows from the matrix for ' $\wedge$ ' (2.5543) the $v(T')=u$. But by the definition if ' $\supset$ ' (2.5543) $v(T \supset T' \supset A)=u$ for any A , i.e., T and T' are not inconsistent.

2.5532 The point of Theorem IV is that if we analyze pre-supposition in the strong sense (b) the inconsistency of T and T' is undecidable and this in the clear sense that we cannot assign either t or f to conjunction of T and T' or to T and any of the members of T' .

2.5554 Claim: If T and T' are incommensurable then deductive relations between T and T' are undecidable to the extent that they are incommensurable. The way to make this precise is to see that for any v under which T and T' are incommensurable

(i) $v(T \supset T')=u$

(ii) $v(T' \supset T)=u$

(III) $v(T \wedge B \supset T')=u$

(iv) $v(T' \wedge B \supset T)=u$, where B is a set of sentences correlating terms of T and terms of T' . The claim is immediate from the commutativity of ' $\wedge$ ' the definition of ' $\supset$ ' and the preceding theorems.

2.55541 We consider the foregoing as a sign of the appropriateness of the formal analysis offered to the intentions of Feyerabend (2.41). That is to say, we have given an analysis of 'incommensurable' that shows that elements of

incommensurable theories can not be deductively related, nor reduced, nor shown inconsistent.

2.6 The issue we have been trying to clarify is given its most detailed abstract development in Feyerabend's "Reply to Criticism." In that work, given the problems between inconsistency and incommensurability in the definition offered in "Problems of Empiricism" and the concomitant alternative analysis (our 2.55(a) and (b)), F opts for another account. He generates the notion of 'strong alternative theories' (s.a.t.) as a surrogate for inconsistency and then tries to show how incommensurables can be compared.

2.61 According to F two theories T, T' are s.a.t.s's when,

(i) T' "contains assertions over and above the assertion (the prediction) that leads to the contradiction."

(ii) "The additional assertions must be connected with the contradiction more intimately than by mere conjunction."

(iii) "There ought to be some independent reason to accept T' "

(iv) T' must account for T 's success."⁷⁷

(ii) is analyzed by Feyerabend in terms of 'regular functions', i.e. functions that diverge (coincide) everywhere if they diverge (coincide) at all. The point is that the inconsistency goes deep, i.e. is a product of theoretic principles and shows throughout the theory.

2.611 We call the reader's attention to condition (i)

and (ii). F presents these without comment or analysis. .

We consider our analysis, here 2.55(a), as an account of the 'metaphoric' 'intimate connection' between the additional assertions and the contradiction. We consider this a further sign of the adequacy of our account to F's intentions.

2.62 F's analysis of s.a.t.'s raises a problem for the standard logical empiricist view, for if we incorporate the notion of s.a.t.'s into what F calls a 'model for progress', that is an account of appropriate scientific change we can not retain the cumulative attitude towards science which sees contradictions as to be adjusted. In fact it is clear that insofar as T and T' are s.a.t.'s

- (i) T cannot be subsumed under T'
- (ii) T cannot be explained on the basis of T'.
- (iii) T cannot be reduced to T'⁷⁸

And this, of course, because T' being a critic of T is inconsistent with it.

2.621 F realizes that there are responses to this claim that inconsistency is not incompatible with a cumulative approach. 'Layer cake model', sometimes in flavors,⁷⁹ is F's terms for the view of Nagel, Hempel and Carnap. "Now it is of course always possible to preserve the layer model and the philosophy on which it rests. This could be done either by replacing strict rules of derivation with approximate rules or by replacing T with some other theory $\bar{T}$ which does follow from T'.

"The first suggestion is unacceptable; it eliminates the very same procedure of criticism by theories which has brought about the transition from T to T'...The second suggestion is nothing but a concealed admission of defeat. For what is so attractive about a philosophical point of view concerning the relation between theories which can be upheld only of real theories which have been discussed in the scientific literature are replaced by emasculated caricatures!"⁸⁰

2.622 These seem odd comments for F to make. Earlier he had taken as basic assumptions:

"(1) that no theory ever agrees (outside the domain of computed error with the available evidence...

(2) Theories agree with the facts (as opposed to the observations) only to a certain extent..."⁸¹

But of course if (1) and (2) are correct then unless all theories are always refuted the rules of derivation are either by approximation or some 'play' in the relation of a theory to inconsistencies must be allowed. The issue for us is not deducibility, but the fact of analogues in the reducing theory for aspects of the reduced. And of course, deductive relations between theories guarantee this in a particularly clear way (cf. Chapters III and IV). These analogues when not as 'tight' as is enabled by deductive relations obviously have to satisfy some criteria for 'goodness of fit', etc. Given the flexibility of scientific

practice, the methodologist is best engaged in looking at the various relations between theories in de facto reduction relations. The job is to generate from these instances criteria that account for the functioning of science rather than the inappropriate machinations of particular theorists. Clearly in a task such as that there is room for disagreement and equally clearly past paradigms can be inappropriately applied to problems of interest (see F's argument against the Quantum Physicists for misreading Newton's methodological principles).⁸²

Recall that the underlying concern in F's work is to leave open the possibility of a radically different micro-physics in the face of the 'conservatism' found in the Copenhagen Interpretation.⁸³ But whatever the issue of concern, little is to be accomplished by demanding a level of stringency for polemical purposes far beyond what is required for an analysis of actual practice. F's second move is similarly bizarre given his attitudes in his other writings. For given his fundamental attitude towards reduction, it is just the replacement of T by some other theory, say $\bar{T}$, that we should expect when T is reduced to T'. Recall that in our analysis in Chapter IV we said that, minimally, if T_1 reduces T_2 then for every m_1 , $m_1 \Vdash a(T_2)$. Now $a(T_2)$ is not necessarily identical with T_2 , although that is the limit to which instances can tend. All we need in actual instances is a relation close enough so that we can make judgments and give some coherent analysis. Whether $a(T_2)$ is an 'emasculated caricature' remains to be seen, and certainly may differ from case to case. What must be judged is the performance of

$a(T_2)$ in all these areas that T_2 has been shown to be adequate. And this cannot be decided a priori nor can the demand for relations of a particular degree of 'closeness of fit' between T_2 and $a(T_2)$ be correlated, a priori, with successful reduction. If $(T_2)=a(T_2)$ then our problem is solved. Of course, if all F is asserting is that in actual reduction instance $T_2 \neq a(T_2)$ then there is nothing to disagree about. Whether this a 'concealed admission of defeat' depends on the stringency in the notion of analogue and actual cases--but this is an area in which the paucity of actual instances examined is telling. Suffice it to say that the notions brought forward in the preceding Chapters claim to afford a meta-linguistic flexibility while retaining precision and clarity far in excess of what F associates with the standard deductivist view; while retaining the basic insight of the 'layer cake model', i.e. that the relations between theories in a model for progress, or rather for a model that maps the progression of science are basically subsumptive, and that the reducing theory, captures in some scientifically appropriate way, the content of the reduced.

2.63 As we have seen, inconsistent theories are not the only critics available. The deep issues are to be found within the realm of the semantically incommensurable (2.55(b)) and these, as we have seen are not inconsistent:

Feyerabend sees the problem as well.

Now one of the most frequent objections about admitting incommensurable theories into the class of strong alternatives of a given theory T is that they cannot contradict T and therefore cannot be used to criticize T...The remark is entirely correct, the use of incommensurable theories for the purposes of criticism must be based on methods which do not depend on the comparison of statements with identical constituents.⁸⁴

2.631 F claims comparison between incommensurables is possible in the following ways: "Firstly we may...compare two sets of infinite elements with respect to certain structural properties and inquire whether or not an isomorphism can be established..." The example given is a Euclidian plane and an arbitrarily bent surface with a Riemannian metric in relation to Newtonian Mechanics and Special Relativity. "Secondly both theories may be able to reproduce the 'local grammar' of sentences which are directly connected with observation results...Thirdly we may construct a model of T within T' and consider its fate."⁸⁵

It should be admitted on all hands that the suggestions offered are plausible methods of comparing theoretic structures; but that is not the issue. The issue is, rather, what is the relation between such methods of comparison and incommensurable theories. In this context F has taken a very strong stand on incommensurables. So we find: "Not a single primitive descriptive term of T can be incorporated into T'...the meaning of all descriptive terms of the

two theories, primitives as well as defined terms will be different..."⁸⁶

2.632 The problem here is the definition of 'can'. If he means that for any n-ary predicate P of T any possible definition of the form $(x_1, \dots, x_n) (P \dots \equiv \phi)$ for open sentence ϕ of T', is inconsistent then by obvious definitions and by our earlier theorems T and T' are not incommensurable in sense 2.55(b). As he must mean that any definition of the form above is ill-formed or undecidable in truth value. Let us look more closely at this later tactic.

2.6321 F's basic arguments are epistemological, as he frequently points out. So let us divide theory T into two sets T_t and T_e where all that is being maintained is that T_e is evidence for T_t. T_e may of course be observational but nothing follows about the semantic status of the members of T_e and that is enough not to beg the question against F. We require that T_t 'imply' T_e; again we can take 'imply' in as weak a sense as is consistent with good practice although we like it best when 'imply' means what it ought to, i.e. logical implication this giving us the best understood explanation and confirmation relations, and the clearest model relations. The incommensurability of T and T' is thus the fact that no term in T_t of T_e can be defined in terms of either T_t' or T_e'.

2.6322 The most interesting proposal, given the epistemological discussion to follow is the second. As Nagel has pointed out if the theorist can reproduce the local grammar of incommensurable theories in the sense that one can

describe the observational and predicative statements an investigator utters as responses to the situation which 'prompt' the utterances, and to compare the order of these responses with the order of the physical situations that prompt them, so as to ascertain the agreements of disagreements between the two orders...the secondary statements (they are clearly observation statements) of investigators cannot have meaning dependent on the theory being tested and must be invariant to alternative theories.⁸⁷

2.63221 Nagel's point is a good one. If we substitute 'meta-theoretic' for 'observation' in the first parenthesis above we get full generality. The point then is that to compare two incommensurable theories in a meaningful way presupposes some neutral ground (whether observational or not). Can we make this intuition precise?

2.63222 Nagel claims that the descriptions of the investigator's responses (or for matter the automatic output of a measuring device)⁸⁸ is neutral vis a vis the alternatives. Take the following situation. T and T' are incommensurable and are analyzed as in 2.6321. T implies Se, Se in Te and T' implies Se', Se' in T;. We know from 2.554 that Se and Se' are mutually undecidable in truth value. We have a measuring device, x, that reports (possibly) Se, and one, y, that can report Se'. The devices, whether human or

otherwise, have been fed the theory and are capable of making the appropriate response if the situation warrants. Notice: x and y can even be the same device, z, but if so, z must keep T and T' separate, in fact they cannot be connected at all, as we have seen. F claims that human beings are devices of type z, i.e. they can learn incommensurable theories and languages.⁸⁹

We, in the meta-language say 'A' just when x emits Se and 'B' when y emits Se'. Note, we don't use 'Se' or 'Se' ' we mention them using our sentences 'A' and 'B'; and similarly for ' $\sim$ A', ' $\sim$ B'. This last remark is sufficient to guarantee the logical independence of our sentence from the theories in question. Recall, we want our report to constitute a fair test of T and T'. But there is a clear sense in which it can't be if 'A' and 'B' are theory laden vis a vis T of T'. Intuitively ' $A \wedge \sim B$ ' should go against T', whence if we are comparing alternatives, give us strong warrant for T. But if e.g. both 'A' and 'B' presuppose T' then ' $T \supset (A \wedge \sim B)$ ' is undecidable given 2.554. Now it is not clear that some theory of evidentiary support could not be generated in which ' $A \wedge \sim B$ ' is evidence for T despite the fact that ' $T \supset (A \wedge \sim B)$ ' is undecidable. But short of such a result the relevant intuitions seem to deny the cogency of the claim. But the situation is perfectly general for all cases, whence if our sentences 'A' or 'B; presuppose either of the theories in question we cannot have a fair test of the outcome of the test procedure.

2.623 The point of this analysis is clear. If we want to compare incommensurables so as to decide between them as theoretic postures we require a language within which the comparison is made which is neutral between them. That is, if we want a fair test. Nagel says, "if secondary statements have this sort of neutrality, it is not evident why only such observations statements have this privileged status."⁹⁰ The response is easy to make. Any statement can have this privileged status as long as it is not a statement in the theories in question. As long as it is not an implicate of a theory with presuppositions of the sort envisioned in our analysis of 'incommensurable'. F's point is that all observations are (or should be) parts of such global theories. "The description of every fact is dependent on some theory."⁹¹ (See 1.3 for 'should be'.) Now clearly this does not entail which theory what statement should be in and this permits the possibility of test neutral vis a vis particular alternatives.

2.63231 Issues get complex fairly quickly, for if 'A' and 'B' (2.63222) presuppose some principle P*, what is the relation between P*, P P', especially if they all are principles of the sorts of theories F envisions; i.e., theories that are 'global', have something to say about all situations. This raises the whole question of possible tests for competing global theories all over again. Clearly a lot more has to be said.⁹²

2.6324 Neutrality of the observation base is easily guaranteed if we assume that observation sentences have a certain openness vis a vis theoretic commitment. So, if some sentence Se could be incorporated into either T or T' because it implies neither P nor P' nor any other basic principle, Se might be sufficiently neutral to stand as a judge between them.

2.63241 This last view is what F calls the 'hole theory' of language.⁹³ The hole theory maintains that certain languages maybe ordinary English or some more precise Observationalese, is uncommitted as to relevant theoretic possibilities. F gives a number of examples to show us the implicit theoretic commitment of even seemingly innocuous ordinary terms.⁹⁴ He quotes the Oxford English Dictionary as follows, "'solid; is defined as...'free from empty spaces, cavities, interstices, etc; having the interiors completely filled up and in...of material substance; of a dense and massive consistency; composed of particles which are firmly and continuously coherent."⁹⁵ The claim then is that ordinary English analyses as ordinary a term as 'solid' within a theoretic perspective. Notice if the definition is theoretical it is wrong, for solids are not completely filled up, at least if we mean an absence of space within the interior of the elementary particles making up the solid. The definition is thus either inadequate or the words are not being used in any proper theoretical sense. Eddington's

two tables need a different vocabulary to describe them.

'Solid' in the ordinary sense translates into a smaller ratio of mass to space than e.g. liquids or gases. In a device like a dictionary it is not always clear which vocabulary is being used for a dictionary attempts to give many uses.

Common sense and the rejection of interpretations that entails that the dictionary is making simple errors is a good guide in these matters.

2.53242 Notice: all the hole theorists maintain is that some elements of some vocabularies can be free from theoretic commitment, not that they should be. This latter claim will be looked at more closely when we examine the epistemological basis of F's views (the 3.m's below). Notice the hole theory follows immediately from examples like 'liquid sunshine'; metaphors of this sort are only intelligible because no theoretic implications are permitted. To attempt to analyze such examples as if they were instances of some new and bizarre theory would be to miss the point completely.

2.633 Given the result (2.63222) we look at (1) and (2) of 2.631. Again we are comparing T and T' but not in terms of local grammar, but rather in terms of various model theoretic structures. (1) is to compare certain (presumable favored models) for 'isomorphism', (3) is the attempt to interpret T in (some model of) T'. In both cases the point is the same, that is to see how much of T(T') we have

to give up to make it compatible with $T'(T)$. I use both examples for it is not clear, if there are incompatibilities, what aspects of which theories have to be changed. So, for example, if some psychological theory is deeply incompatible with some deep neurophysiological principle it is not clear what gets changed. Think of the alteration in our view of the electrical aspects of brain physiology given the inability to make a rigorous localization view of memory storage coherent with the facts.

2.6331 The availability of means of comparison of the sort (1), (3) is all we require in any event. The issue, as we have maintained all along, is to plot relations between elements that seem amenable to formal analysis, i.e. theories and their models. Once we admit, if we do, that some pairs are incommensurable the job gets more complex but that does not seem to imply the impossibility of the task. But more of that later when we generate our formal paradigm.

2.6332 What the analysis of 2.63222 yields is that the incommensurability of T and T' need not render questionable the language (theory) in which the comparison is made. That is, the language in which the comparison is made not be in any prejudicial relation to the theories under consideration and this in the strong sense that the language in which the comparison is made need not presuppose any of the theories in which the test results are construed.

2.7 The point of this last sentence is to clarify the sense of the key concept of theory dependence as used by F. Nothing F says seems to render the fundamental attitudes of Logical Empiricism in that respect inapplicable. Although the notion of theory dependence can be incorporated into the logical empiricist posture there are deep epistemological divergences between F and our favored views. It is to these that we now turn.

3. The analysis of incommensurability we have afforded has equated meaning with theoretical presupposition. This particular account of meaning, F's, differs from the logical empiricist account characterized by the view that the meaning of terms is tied to the observational basis. So, F quotes Carnap: "...there is no independent interpretation for Lt (the theoretic terms of some theory T, in a language L). The system T is itself an uninterpreted postulate system. The terms of (Lt) obtain only an indirect and incomplete interpretation by the fact that some of them are connected by correspondence rule with observational terms and the remaining terms of (Lt) are connected with the first ones by the postulates of T."⁹⁶

3.01 This view is linked with the view of Nagel quoted above, (1, 2). Recall: "It is of the utmost importance that the expressions peculiar to a science will possess meanings that are fixed by its own procedures, and are therefore intelligible in terms of its own rules of usage, whether or not

the science has been or will be reduced to some other discipline."⁹⁷

3.1 The issues here are complex and involve deep philosophical concerns. The first, and most important concern, in the general empiricist attitude that requires scientific theories to be tied to observations, The second is the reconstrual of these observations in the light of scientific advance. The third is the need to have some ground on the basis of which to judge competing reconstruals (theories of a particular sort).

3.2 There is a further issue here. That is the cumulative picture of scientific practice alluded to earlier, and this in light of certain very real issues in the Philosophy of Physics. A careful reading of the Feyerabendian corpus points to the problem of Quantum Mechanics and especially the dispute between, say, Bohr and Heisenberg on the one hand and Bohm and Einstein on the other. So, e.g., Bohr is quoted as follows, "However far the new phenomena found on the micro level transcend the scope of classical physical explanation the account of all evidence must be explained in classical terms."⁹⁸ This is clearly analagous to Nagel 3.1 above.

F presents the following as a generalization of the above views. "Only such theories are admissibly (for explanation and reduction) in a domain which either contain the theories already used in the domain or are at least consis-

tent with them."⁹⁹ This is a particularly informative version of 1.1 (A) and B and points to the methodological arguments against them and it.

3.31 The deep issue behind it all is F's 'principle of proliferation' as a superior methodological principle to the one quoted in 3.3 So we have, "Invent and elaborate theories which are inconsistent with the accepted point of view, even if the accepted point of view should be confirmed and generally accepted."¹⁰⁰ In more general terms the demand is that we should proceed counter-inductively.

3.311 Before we continue we might ponder the content of such a demand, after all short of some spelling out of the Hegelian notion that there is some favored direction in which inconsistencies develop, to demand that we proceed by contradicting accepted views leaves the direction of development completely unspecified. And this can be seen completely independently of the fact that contradictions imply all propositions."¹⁰¹

3.312 The position envisioned is anti-inductivist in a strong sense. F quotes Einstein as maintaining that "Physical theories are 'free creations' that can only occasionally be related to observation."¹⁰² This seems particularly odd if only because F maintains, "At all times people have found it important to make observations. Even the most obtuse myth is formulated in a fashion that pro-

vides detailed observations and explanations of a great variety of phenomena and brings coherence into otherwise unintelligible occurrences."¹⁰³

3.3121 Now F has maintained that what he means by 'theory' includes 'myths, political ideas and religious systems' and perhaps if we include such a diverse set as the object of our concern we might find F's principles warranted. But if in fact all of these kinds of structures have observational elements it is not clear how F is to distinguish myths, etc., from scientific theories; these last, after all being the object of concern here. Notice it is not at all clear how empiricists are saddled with the view that observational consequences are the sole deciding factor in distinguishing science from other things. Those empiricists who tried to maintain this position never did so for very long.

3.3122 F in response to the empiricism of Bohr asks, "...does it not perhaps mean that those who thought that it was the emphasis upon observation which lead to their discoveries misplaced the element that distinguishes them from their predecessors."¹⁰⁵

Now if what is maintained in 3.312 is correct and we assume it is then clearly it is not observation simpliciter that distinguishes science from, e.g. myth. It seems that the implicit assumption of the logical empiricists has always been that it is the relation between observation and

theory that accounts for the special status of science. So we find (for the inductivists) that it is the specificity of the relation between observation and theory that is the issue (there is a reason why Carnap looks for a confirmation function) or we find (for the falsificationist) it is the riskiness of the consequences of theories that is the principle of demarcation.¹⁰⁶

3.3123 Without going into an elaborate discussion of the preceding section, we can see that it is not observation, but rather the relation between theory and observation that is the issue. But that requires that some statement, even if only in principle, of the nature of this relations must be forthcoming on pain of our intuition that science is different than other cultural artifacts remaining totally empty. We must deny F's claim that statements of criteria, i.e. statements that render the relation between theory and observation precise are impossible because such matters "defy rational analysis," that such statements are "mere verbal ornaments."¹⁰⁷ Now it is clear that the analysis of 'rational' connections between theory and observation is not complete and is fraught with deep problems. But there are a variety of reasons for this. My view is that among the more important reasons for the problematic nature of methodological output is the size of the objects heretofore considered by methodologists in the logical empiricist tradition. Hempel, for example, did Philosophy of Science

as if theories are sentences of the form ' $(x)(Fx \supset Gx)$ '.¹⁰⁸ Further this inappropriate choice of objects was determined by the fact that the logical techniques used by these methodologists were best suited to objects of that size. But these are not the only logical techniques available and so no longer do we seem forced to deal with scientific objects on that level. The tools available to the technically oriented methodologists far transcend the object language of first order theories. And it is increasingly to such more complex formal apparatus that methodologists are turning (should turn).

In any event, the issue is whether there is a subject matter for the methodologist to be interested in, i.e. is there a phenomena called 'Science'. The contention implicit here is that there is and what distinguishes a science from non-science is the relation between theory and observation (actually the various relations between the component parts that make up the complexes called theories). Later we will try to afford a slightly more detailed analysis of such a view. But first let us return to F's critique of what he calls 'radical empiricism'.

3.31231 One last word before we continue. F maintains "...a single step on the road away from metaphysics could bring reason back into empiricism. This step consists in dropping the idea of certainty or of directness or of the weaker idea of support."¹⁰⁹ We will assuredly accept the

inappropriateness of the first two italicized words; for the empiricist methodologist to drop the last, i.e. 'support' is to give the game away.

3.4 By a 'radical empiricism' F intends a methodological position maintaining 3.3.¹¹⁰ But this most especially in the sense that the data base for a theory (whether observations or lower order empirical regularities) remains invariant under explanation. (3.1 above.)

3.41 The main argument against radical empiricism comes from the demand for scientific realism. "A realist... wants to give a unified account both of observable and unobservable matters and he will use the most abstract terms of whatever theory he is contemplating for that purpose."¹¹¹

3.411 Recall: we demand realism as well for it is only on the realist interpretation of scientific theories that the interpretation of the MBIT we have been using, i.e. the MBIT as a claim in meta-science, is a coherent view. That is to say the reinterpretation of the mental as the physical, warranted by the advance of theories like neurophysiology, only yields the ontological conclusion if higher order theories are deemed sufficient for ontologically relevant reinterpretations of lower order aspects of science. This last seems to be the basic core of the realist attitude in science; theories are the ground for that interpretation of what is, as to what it is.¹¹²

3.4111 We cannot enter into the complexities of realism at this time, i.e. what features of what theories constitute adequate grounds for ontological reinterpretation. But see Nagel, Structure of Science,¹¹³ for a minimal analysis. An interesting argument that realism is a necessary condition for the meaningfulness of standard scientific test procedures is Boyd.¹¹⁴

3.42 The notion of reinterpretation of observation statements obviously denies the view that observation statements have their meaning fixed independently of theoretic commitment. F maintains that "observation statements, then are not semantically different from other contingent statements...the difference (between observation statements and more theoretical statements) is provided by the psychological and physiological or physical circumstances of their production."¹¹⁵

3.421 We may agree with F immediately if all he is maintaining is that observational statements, like other statements in science, are contingent. In fact F's real intentions are best seen when he denies the certainty of the observational statements of science on the grounds that this would result in their status as synthetic a priori, anathema to empiricists, on the grounds that all statements of science must be open to criticism.¹¹⁶ We mark the distinction between the certainty of observational statements and their stability.

3.422 A remark of F's can help to clarify this.

Taken by themselves the indications of instruments do not mean anything unless we possess a theory which teaches us what situations we are to expect in the world and which guarantees that there exists a reliable correlation between the indications of instruments and such a particular situation...Nobody would dream that the meanings of observation statements as obtained by the use of measuring instruments remains invariant with respect to the change and progress of knowledge. Yet, precisely this is done when the measuring instruments are human beings...¹¹⁷

3.423 As F correctly maintains, such a view is based on "the (very old) belief that (a) some states of mind (sensations or abstract ideas) can be known with certainty; that (b) it is exactly this knowledge that constitutes the foundation of whatever assertions we make about the world."¹¹⁸

The connection between (a) and (b) and meaning invariance (for the observation statements of human beings) is as follows: "If it is indeed the case that statements about, say, sensations are irrevocable once produced, then the same applies to the descriptive terms contained in them; their meaning is uniquely and irrevocably determined by the structure of the statements in which they occur as well as the circumstances which lead to the certain productions of these statements."¹¹⁹

3.43 Arguments against (a) and (b) abound. These occur on two levels, philosophical and scientific. For the first the most cogent are those of Wittgenstein that demonstrate that the occurrence of mental states is insufficient to

determine the applicability of a predicate. Scientific arguments are of a more roundabout nature, but include the inability of the program of German Introspectionist Psychology to come up with even a minimum regularity in the descriptions of mental states and this even for carefully trained observers in carefully constructed experimental circumstances. The problem of regularity is crucial here. If we focus on inner states, the hallmark of scientific objectivity, i.e. the uniformity of investigators' responses to controlled circumstance is not to be found.

3.44 The arguments against (b) are similarly well known. Even as atomistic an empiricist as Hume requires some middle to go from the experiences to the informative statements made on the basis of them (the so-called problem of causation). What I mean by that is the following: If an assertion like 'I am hungry' is to be informative the statement must have some consequence class associated with it, but that pre-supposes regularities, but these transcend any set of possible sensations, whence some additional faculty is required.

From a scientific point of view, the position is even more clearly argued. Given, say, the notion of the retinal image as an incoherent but roughly uniform field of electrical stimulation we find an image (the visual field) with a high degree of discontinuity but with high coherence. This requires that some process of synthesization be interpolated between the retinal image and the experience (visual

field); but that leaves room for error--whence certainty goes out the window. There is nothing wrong with the causal theory of perception: it is just inadequate to yield the special semantical status for sensation reports that lies at the basis of radical empiricism.

3.441 That sensation, in the sense of stimulation of the sensory organs is insufficient for knowledge, is most cogently argued by Psychologists like Kohler¹²⁰ who point up the incoherence (topological) between the experience and some plausible account of the stimulated state of sense organs. The relation between some image and some stimulus is thus causal, whence contingent. That in itself rules out the possibility that the image be necessarily connected with the stimulus, and of course the stimulus (neural) is all we have to connect the image with the object. That is, all we have except for a theoretic account of the relation. But theories are liable to change, whence are not certain either. The situation is the same for all sentences referring to mental states; in so far as they are informative, i.e. have causal consequences, they are merely contingently related to these regularities. In so far as they are mere reports, or better, mere expressions of mental states, they are (perhaps) incorrigible but totally uninformative--having no consequence whatsoever.

3.5 Once we realize that no, a priori, certain basis for empirical knowledge is available, the objectivity of science

becomes problematical. How is it possible for scientific theories to be based on facts if the surrogate for facts, observations, turn out to be responses whose production in general is governed by a host of variables many of which can only be known by the future advance of science that waits them for their imprimatur.

Further if these responses themselves are biased by prior, perhaps inadequate, notions as to what the relation between facts and observations are, how can observations be relied upon in crucial instances to judge between the implicit framework and some potential critic?

3.51 One of the most valuable aspects of the Feyerabendian corpus is that he tries, by careful examples, to show the answers to the above questions. This is not the place to examine the historical accuracy of his views. What we do realize is that on epistemological grounds something like his account is required or there is no corroborating our intuition that sensory observation, although fallible, is a good indicator of objective states of affairs.

3.512 The heart of F's solution is the notion of a natural interpretation, i.e. the placing of a construal upon observations that reflects some fundamental attitude of the organism towards objects, that is the elements of 'direct experience'.¹²¹ The issue, although quite detailed is clear in principle. Certain natural interpretations are extended

throughout a new domain enabling a reinterpretation of basic perceptual data. The naturalness of the interpretation giving the new analysis a wedge into its application in an area and in a manner that was never conceived of before.

I find the basic epistemological account as well as the examples alluded to sufficiently compelling to accept F's attitude towards the inherent corrigibility of any item of the perceptual basis. The question is, for us, given F's position, how do we as heir to the logical empiricist tradition analyze the relation of theory and observation. That is, can a formal model of scientific theories be generated that accepts the epistemology of F without having to accept what, by logical empiricist standards, is an obscurantist methodology. It is to this problem that we now turn.

FOOTNOTES

Chapter V

¹We mark the artificiality of the distinction with quotation marks. Whether some structure is a theory to be related to another theory rather than two levels of the same theory is a function of diverse historical or methodological perspectives and may certainly vary from case to case.

²A classic statement of the view that the history of science is a cumulative process, P. Oppenheim and H. Putnam, "Unity of Science as a Working Hypothesis," H. Feigl, M. Scriven, G. Maxwell(eds.), Minnesota Studies in the Philosophy of Science (Minneapolis: University of Minnesota Press, 1958), vol. 2.

³P. Feyerabend, "Reply to Criticism," R. Cohen and M. Wartofsky, Boston Studies in the Philosophy of Science (New York: Humanities Press, 1965), vol. 2.

⁴"While Galileo's law asserts that the acceleration of a free falling body near the earth's surface is constant, Newtonian theory entails that the acceleration is not constant but varies with the distance of the falling body from the earth's center of mass. Accordingly, even though the Newtonian conclusion may be 'experimentally indistinguishable' from Galileo's law, the latter is in fact 'inconsistent' with Newtonian theory'." Nagel, Structure of Science, p. 120.

⁵P. Feyerabend, "How to be a Good Empiricist: A Plea for Tolerance in Matters Epistemological," B. Baumrin, (ed.) Philosophy of Science: The Delaware Seminar (New York:

Interscience Publisher, 1963); "Against Method: Outline of an Anarchistic Theory of Knowledge," Minnesota Studies in the Philosophy of Science, M. Radner and S. Winokur, eds.

Minneapolis: University of Minnesota Press, 1970, vol. 4, sec. 4ff., especially p. 187; "Problems of Empiricism," Beyond the Edge of Certainty, R. Colodny, ed. (New Jersey: Prentice Hall, 1965); "Consolation for the Specialist," Criticism and the Growth of Knowledge, I. Lakatos and A. Musgrave, eds. (Cambridge: Cambridge University Press, 1970).

⁶Idem, "How To Be a Good Empiricist...", "Against Method...", sec. (1)-(4); "Consolation for the Specialist"; "Problems of Microphysics," Frontiers of Science and Philosophy, R. Colodny, ed. (Pittsburgh: University of Pittsburgh Press, 1962); "Complementarity," Proceedings of the Aristotelian Society, Supplementary Volume, 32. (1958); "Classical Empiricism," The Methodological Heritage of Newton, R. Butz and J. Davis, eds. (Toronto: University of Toronto Press, 1969); "Problems of Empiricism, Part 2," The Nature and Function of Scientific Theories, R. Colodny, ed. (Pittsburgh: University of Pittsburgh Press, 1969); "Niels Bohr's Interpretation of the Quantum Theory," Current Issues in the Philosophy of Science, H. Feigl and G. Maxwell, eds. (New York: Holt, Rinehart and Winston, 1961)

⁷Idem, "Explanation, Reduction and Empiricism," Minnesota Studies in the Philosophy of Science, H. Feigl and G. Maxwell, eds. (Minneapolis: University of Minnesota Press, 1962), 3:71; "Reply to Criticism," pp. 178 ff.;

"Against Method...", fn. 13, 27, 31, 41, 52, 218; "Consolation for the..." sections 3 and 7; "Problems of Empiricism," sec. 7.

⁸Idem, "Explanation, Reduction..."; "Problem of Empiricism"; "Against Method"; "Philosophy of Science: A Subject With a Great Past," Minnesota Studies in the Philosophy of Science, (Minneapolis: University of Minnesota Press, 1970); vol. 5, "Consolation for the Specialist."

⁹Idem, "Against Method," Preface, sec. 14, fn. 13, 28, 33, 41, 42, 219, 220 and 221; "On the Improvement of the Science and the Arts and the Possible Identity of the Two," Boston Studies in the Philosophy of Science, R. Cohen and M. Wartofsky, eds. (Dordrecht-Holland: Reidel Publishing Company, 1968) vol. 3; "Consolation," sec. 7, 9.

¹⁰Idem, "Against Method"; "Problems of Empiricism," Part 2; "Philosophy of Science"; "Problem of Empiricism"; "Reply to Criticism"; "Consolation for the ...".

¹¹Idem, "Explanation, Reduction," p. 43.; "Reply to Criticism," sec. 3.

¹²Idem, "Explanation," pp. 46ff. p. 74; "Reply," pp. 168ff., pp. 216-17; "Against..."fn. 137, 197, 205.

¹³Idem, "Reply," p. 164, quoted from Nagel, "The Meaning of Reduction..." p. 301. See also Nagel, The Structure of Science, pp. 345, 349, 352.

¹⁴Idem, "Explanation," sec. 7; "Reply," sec. 6-8; "Against Method," sec 6-13; "Problem," Part 2, sec. 5, 12, 13, 14; "Problem," sec. 5-8.

¹⁵Idem, "Explanation," p. 29.

¹⁶Ibid., p. 52.

¹⁷Ibid., p. 28; Also Feyerabend, "Reply," p. 171;
"Problems," pp. 231 ff.; "Against," sec 6, 13.

¹⁸Idem, "Against," sec. 3.

¹⁹Idem, "Problems," p. 230.

²⁰Idem, "Explanation," p. 29.

²¹Ibid.

²²Ibid., p. 30.

²³See 2.6322 for F's arguments and Nagel's counter.
See also Nagel, "Issues in the Logic," for a detailed
critique of F's position.

²⁴Feyerabend, "Against," p. 42.

²⁵Nagel, "Issues in the Logic," p. 121.

²⁶Feyerabend, "Problem," p. 229.

²⁷See 2.622 and Nagel's comments on 2.1 above.

²⁸Feyerabend, "Against," sec. 9; I. Lakatos, "Falsification and the Methodology of Scientific Research Programmes," Criticism and the Growth of Knowledge, I. Lakatos and A. Musgrave, eds. (Cambridge: Cambridge University Press, 1971), pp. 112 ff.

²⁹Feyerabend, "Explanation," p. 42, 92; "Against," p. 41, 20 ff., fn. 21; "Consolation."

³⁰Idem, "Against," p. 42, 77ff.

³¹Idem, "Problems," p. 150.

³²For 'research program' see Lakatos, "Falsification," but the meaning should be clear enough on the ordinary understanding of the term.

³³Feyerabend, "Against," p. 99.

³⁴N. Rescher, Hypothetical Reasoning (Amsterdam: North Holland Publishing Co., 1970).

³⁵W. Rozeboom, "The Crisis in Philosophical Semantics," Minnesota Studies in the Philosophy of Science, M. Radner and S. Winokur, eds. vol. 4.

³⁶R. Carnap, "Hilary Putnam on Degree of Confirmation and Induction Logic," The Philosophy of Rudolf Carnap, P. A. Schilpp, ed. (La Salle, Illinois: Open Court, 1963) p.989

³⁷Lakatos, "Falsification,"; "History of Science and its Rational Reconstruction," Boston Studies in the Philosophy of Science, R. Buck, and R. Cohen, eds. (Dordrecht-Holland: D. Reidel Publishing Co., 1970), vol. 8.

³⁸Idem, "Falsification," fn. 11.

³⁹Feyerabend, "Problems," p. 233.

⁴⁰Idem, "Consolation," sec. 9.

⁴¹Idem, "Against," sec. 13.

⁴²Idem, "Problems," fn. 19, p. 227.

⁴³References to the above include W.S. McCulloch and N. A. Pitts, "A Logical Calculus of the Ideas Imminent in Nervous Activity," Bulletin of Mathematical Biophysics, No. 5, 1943 and C. Brenner, An Elementary Textbook of Psychoanalysis (New York: Doubleday, Anchor, 1955). For Freud's own views on the possibility of reduction See R. Fancher, Psychoanalytic Psychology (New York: Norton and Co., 1973). Notice as far as the last is concerned that after the failure of 'Project for a Scientific Psychology,' Freud dismisses

the neurophysiological model and "turned his attention to the construction of a 'psychological' model of the mind." (Fancher, p. 98.)

⁴⁴Feyerabend, "Reduction," sec. 5, 7; "Problems," sec. 4; sec. 8. 9; "Against," sec. 13.

⁴⁵Idem, "Problems," fn. 19.

⁴⁶Ibid., p. 227.

⁴⁷Feyerabend, "Explanation," p. 94.

⁴⁸Idem, "Against," p. 84.

⁴⁹Idem, "Problems," sec. 4; "Problems," part 2, sec 12,

⁵⁰Idem, "Against," p. 83.

⁵¹Idem, "Explanation," p. 74.

⁵²Idem, "Problems," p. 168-69.

⁵³Idem, "Against," p. 84.

⁵⁴Nagel, "Issues in the Logic of Reductive Explanation," Mind, Science and History.

⁵⁵B. Van Fraassen, Formal Semantics and Logic (New York: MacMillan, 1970), p. 31.

⁵⁶Feyerabend, "Reply," p. 231.

⁵⁷Idem, "Consolation," p. 222. See also, "Against," sec. 13.

⁵⁸Idem, "Problems," pp. 231-32.

⁵⁹Ibid., p. 232.

⁶⁰Feyerabend, "Explanation," sec. 5.

⁶¹Ibid., p. 54.

⁶²J. Shaffer, Philosophy of Mind (New Jersey: Prentice-Hall, 1968), pp. 48-49.

⁶³M. Bunge, "Problems Concerning Intertheory Relations," Induction, Physics and Ethics, P. Weingartner and G. Zecha, eds. (Dordrecht-Holland: D. Reidel Publishing Co., 1970) p.300.

⁶⁴Ibid., p. 301.

⁶⁵American College Dictionary (New York: Random House, 1957), p. 959.

⁶⁶P. Strawson, Introduction to Logical Theory (London: Methuen, 1952).

⁶⁷Van Fraassen, Formal Semantics, p. 154.

⁶⁸Ibid., p. 70.

⁶⁹Ibid., pp. 56 ff.

⁷¹Ibid., p. 156.

⁷²Ibid., p. 96.

⁷³Bunge, "Problems, Concerning Intertheory Relations," p. 315.

⁷⁴Nagel, Structure of Science, p. 91.

⁷⁵P. Suppes, "Measurement, Empirical Meaningfulness and Three-Valued Logic," Studies in the Methodology and Foundations of Science, P. Suppes, ed. (New York: Humanities Press, 1969).

⁷⁶Suppes' problem in the monograph cited is similar to our own. In the name of leaving some work undone we leave the comparison to the reader.

⁷⁷Feyerabend, "Reply," p. 226.

⁷⁸Ibid., p. 228.

⁷⁹Feyerabend, "Consolation," fn. 5.

⁸⁰Idem., "Reply," p. 229.

⁸¹Ibid., p. 224.

⁸²Idem., "Classical Empericism."

⁸³Idem, "Complementarity"; "Problems of Microphysics";
"Niels Bohr's Interpretation,,,"

⁸⁴Idem, "Reply," pp. 231-32.

⁸⁵Ibid., pp. 232-33.

⁸⁶Ibid., p. 231.

⁸⁷Nagel, "Issues in the Logic," pp. 130-31.

⁸⁸See Feyerabend, "Against," Appendix

⁸⁹Idem, "Against," pp. 85ff.; "Problems," fn. 151.

⁹⁰Nagel, "Issue," p. 131.

⁹¹Feyerabend, "Problems," p. 175.

⁹²See A. Koslow, "The Law of Inertia: Some Remarks on
Its Structure and Significance," Philosophy, Science and
Method, S. Morgenbesser, I. Suppes, M. White, eds. (New York:
St. Martin's Press, 1970) for the possibility that truly
basic principles are less susceptible to alternatives than
we think. See Feyerabend's claim that if T and T' are general
enough 'A' and 'B' have to be interpreted in one or the
other; although some decision between them can still be made.
(Feyerabend, "Against," fn. 197, p. 204.)

⁹³Feyerabend, "Problems," p. 256.

⁹⁴Ibid., sec. 5; "Against," fn. 15.

⁹⁵Idem, "Problems," p. 257.

⁹⁶R. Carnap, "The Methodological Character of Theoretic
Concepts," Minnesota Studies in the Philosophy of Science,
H. Feigl and M. Scriven, eds. (Minneapolis: University of
Minnesota Press, 1956), vol. 1.

⁹⁷Nagel, "The Meaning of Reduction in the Natural Sciences," Science and Civilization, p. 301. See also his Structure of Science, p. 345 and 352. The close connection Nagel and Carnap in these issues is best seen in The Structure of Science, section 3, chapter 5.

⁹⁸Feyerabend, "Explanation," p. 43, quoted from N. Bohr, "Discussions with Einstein," Einstein: Philosopher Scientist, P.A.Schilpp (Evanston, Illinois: Library of Living Philosophers, 1948).

⁹⁹Feyerabend, "Explanation," p. 44.

¹⁰⁰Idem, "Against," p. 26.

¹⁰¹See Feyerabend, "Against," fn. 21 for F's concern over the supposed vacuity of lessons learned from the study of formal languages. In fact the only methodological principle that can be gathered from a proposal like F's is 'anything goes'. See his "Consolation," section 9, for F's attempt to turn this charge against his critics.

¹⁰²Idem, "Problem," p. 161.

¹⁰³Ibid., p. 146.

¹⁰⁴Feyerabend, "Reply," p. 252.

¹⁰⁵Idem, "Problem," p. 147.

¹⁰⁶See for example, K. Popper, Conjectures and Refutations (New York: Harper Torchbooks, 1963), ch. 1.

¹⁰⁷Feyerabend, "Consolation," p. 79.

¹⁰⁸Note, this is an overstatement. Hempel analyzed sentences of more complex forms, for example, $(x)(\exists y)Rxy$. But see Hempel's classic "Studies in the Logic of Confirma-

ation," and "Studies in the Logic of Explanation,"
Aspects of Scientific Explanation (New York: Free Press,
1965) for instances of the genre.

¹⁰⁹Feyerabend, "Problem," p. 148.

¹¹⁰Ibid., pp. 147-49 include a fuller statement.

¹¹¹Feyerabend, "Against," pp. 83-83.

¹¹²W. Sellars, Science, Perception and Reality (London:
Rutledge and Keagen Paul, 1963).

¹¹³Nagel, Structure of Science, pp. 141 ff.

¹¹⁴R. Boyd, "Realism, Underdetermination and a Causal
Theory of Evidence," Nous, 1973, vol. 7.

¹¹⁵Feyerabend, "Problem," p. 152.

¹¹⁶Idem, "Against," pp. 34 ff.

¹¹⁷Idem, "Explanation," pp. 36-37.

¹¹⁸Ibid., p. 38.

¹¹⁹Ibid.

¹²⁰W. Kohler, Gestalt Psychology (New York: New American
Library, 1947).

¹²¹Ibid.

¹²²Cf. especially the analysis of relative motion in
Feyerabend's "Problems...Part 2" and the motion of the
earth in "Problems," ; the analysis of non-isotropic space
in his article, "Reply."

CHAPTER VI

4.0 Prefatory remarks.

Whatever the problems in the logical empiricist philosophy of science and here I mean, especially, thinkers like Hempel, Braithwaite, Carnap and those who retained their style (notice, style is what is crucial here, for example the use of formal languages is particularly relevant) the entities chosen for examination were, relatively, non-controversial. That is to say, that although the analysis of, for example, law-like generalizations was hotly debated there was no serious question that such objects existed.

During the later development of these problems, methodologists, in an attempt to grapple with the facts of scientific practice, were forced to deal with objects of an increasingly complex sort; objects whose existence could no longer be determined in some facile manner. Law-like generalizations can be instanced by the mere presentation of textbooks in the special sciences. Naturally, as the objects under discussion become more obtuse the properties of these objects became harder and harder to clearly present. This, oddly, helped to clarify issues. So, for example, problems of the sort 'grue', proliferated during the 1950s', for, among other reasons, the availability of the statement of these problems within

the, then standard, first order analyses of laws and predicates, became increasingly rare as methodologists became cognizant of the artificiality of the constructions that permitted their generation.

The reaction against such formulations and the problems engendered by them resulted in a new breed of methodologists, who, working towards a level of descriptive adequacy based on an analysis of actual historical practice, denied the adequacy of a formal approach in understanding scientific artifacts. Certainly, as these new style methodologists might maintain 'clarity is not enough', especially if the increase in clarity was a harbinger of the increasing irrelevance of formalist construction to actual scientific practice.

An obvious example of a historically minded methodologist who eschews the simple objects of 'logician's' philosophy of science for a more life-like segment is Lakatos¹ who takes as the object of his scrutiny, "...a research program with a conventionally accepted... 'hard core' (of spatiotemporally universal theories) and with a 'positive heuristic' which defined problems, outlines the construction of a belt of auxiliary hypotheses, foresees anomalies and turns them victoriously into examples all according to a preconceived plan."²

Lakatos considers such an object minimally large to accommodate the need for, especially, comparative theory evaluation. This comparative approach is also maintained by him as crucial for the analysis of such fundamental

issues as confirmation, falsification and the 'demarcation' between science and 'meta-physics'. Given this essentially poly-theoretic view, Lakatos defines two crucial notions. "A research programme is said to be progressing as long as its theoretical growth anticipates its empirical growth, that is, as long as it keeps predicting novel facts with success ('progressive problem shift')' it is stagnating if its theoretical growth lags behind its empirical growth, that is, as long as it gives only post hoc explanations either of chance discoveries or of facts anticipated by, and discovered in, a rival programme ('degenerating problem shift').³

The problem with Lakatos' presentation is that, although based on a wealth of historical example, it furnishes little on the level of both generality and precision that has come to be expected from methodologists working within the logical empiricist tradition. Lakatos had been criticized on that score by theorists working within the historical perspective. So Kuhn says: "(Lakatos) must specify criteria which can be used...to distinguish a degenerative from a progressive research programme...Otherwise he has told us nothing at all." Feyerabend concludes that Lakatos' standards are mere 'verbal ornaments'.⁴

What we will attempt in the following is to bridge the gap between the detail and cogency of a formalist approach and the breadth of perspective found most usually within historically oriented theorists. We will generate the notion of a scientific structure, , a theory in a broad sense.

Hopefully within such an expanded view some clarity of logical content can be afforded to concepts heretofore discussed within the historical perspective. That is not to say that we are offering a formalization of 'research programme'. We will not discuss many interesting and important aspects of Lakatos' approach, specifically 'ad hoc' hypothesis and the 'protective belt'. Rather we are presenting a sketch that, hopefully, is the beginning of a genre. That is we hope to show that precision can be afforded by a formalist analysis even if we take the objects of our scrutiny as large relative to the standard practice of formalist methodologists. We disclaim any pretension to exhaustiveness, more is left out of our analysis than is put in. The following is only a first step in the attempt to formalize the crucial notions inherent in scientific practice.

Naturally, we will focus on the elements that our discussion so far has generated. So we will focus especially on the models of a theory and the theories' relation to other theories. Within this context we mention a crucial omission: we will not discuss the relation of a theory to its theory of measurement. This is because we will eventually want to apply our analysis to reduction in some ontologically significant sense. This last, of course, being then applied to the focus of our discussion, the MBIT.

A word about the object of our discussion, $\mathcal{T}$. We do not claim that $\mathcal{T}$ exists in any concrete sense. That is to say $\mathcal{T}$ may not be found in any textbook nor found in the

consciousness of particular methodologist. Rather $\mathcal{M}$ is the object of rational reconstruction. It is the object that the metatheoretic view of science generates as the beginning of the analysis of the logic of theory use in the special sciences. Despite the obscurity of the object of our perusal, the presentation is of value because of the perspective it permits. The criteria for adequacy for an artifact like the following is the criteria suitable to any 'metaphysical' construction. That is to say the following is to be judged in light of its potential to draw together in a unified way a host of problems thereby premitting a resolution by furnishing a perspective through which phenomena can be dealt. We accept as a hallmark of the efficacy of our approach the clarity of the application of our construction to the MBIT.

We take this opportunity to express our indebtedness to Suppes, Hanson and Shimony. To Suppes for his insistence that the meta-mathematical approach is adequate to the notion of model in the empirical sciences. To Hanson for his presentation of the complexity of that notion and to Shimony for his lesson as to the application of probabilities to the elements of science.

4.1 We define a scientific structure, $\mathcal{M}$, as follows:

$\mathcal{M} = \langle T, \mathcal{F}, \mathcal{R} \rangle$ with or without indices, primes, asterisks, etc., where

(a) T is the syntax of $\mathcal{M}$, that is a set of sentences that constitute the linguistic statement of $\mathcal{M}$. These sentences are closed under some appropriate consequence relation, so

T might be set equal to $CnO(T)$ where $CnO(T) = \{s:T \Vdash s \vee T \Vdash s\}$. We say 'might be' since we don't assume our discussion in Chapter I to be the last word in the management of scientific languages.

(b) $\mathcal{F}$ is a field of sets of functions F , such that for all F in $\mathcal{F}$ and f in F , $f(T) = m$ for some model theoretic structure, m where either

(i) $m \Vdash T$, or

(ii) m is a near isomorph to n , and $n \Vdash T$. By 'near isomorph' we mean that m is in some appropriate approximation to the isomorphism relation. The notion of approximation used here is parasitic on the availability of natural approximation relations as outlined in Appendix C. By 'appropriate' we mean a level of nearness consistent with the practices of the science in question. This notion is a posteriori and we certainly refrain from any attempt to generate a measure a priori. Our inability to define such a measure, even if suspect will not interfere with our construction since we will be interested in the history of such approximation relations every time. We therefore refrain from any absolute measure.

(iii) For sets X and Y , if X and Y are in $\mathcal{F}$ so is $X \cup Y$.

That is to say $\mathcal{F}$ is closed under set theoretic union. The reason for this is, as we shall see, that the crucial notion throughout will be a choice from $\mathcal{F}$ in terms of the syntax, T , and some subset of the field $\mathcal{F}$. We put all of the unions in the field since we don't want to put a priori limits

on the choices available.

- (c) $\mathcal{R}$ is a field of set of functions R , such that for all R in $\mathcal{R}$ and every r in R there is some theory T' and r represents T in T' in respect of some subset of T , $k(T)$. We close $\mathcal{R}$ under set theoretic union as well. Notice if $k(T)$ is $Cn(T)$ the $T' \text{Re} T$. We will speak of the subset $k(T)$ in relation to $Cn(T)$ (as proxy for $CnO(T)$) as an indicator of the appropriateness of T .

4.11 Before we begin a more detailed examination of the elements of $\mathcal{J}$, a word about the governing intuition. A theory in the sense of $\mathcal{J}$ is thought of first as a syntax, T . We add to this a class of possible models of T and a set of possible reducers. What we will be interested in, however, is what we call a realization of, $\mathcal{J}$ that is to say, a triple $\langle T, F, R \rangle$ where F and R are a choice from $\mathcal{F}$ and $\mathcal{R}$, respectively. $\mathcal{J}$ is thus a potential item that is realized when a commitment to a member of the set of possible models and the set of possible reducers is made. Of interest to us throughout is the history of these realizations.

We claim that the adequacy of $\mathcal{J}$ as a scientific structure is a complex function of the various elements in $\mathcal{J}$, as well as the life history of $\mathcal{J}$ in the sense of the history of the successive realizations of $\mathcal{J}$. We present our construction as normative in respect of practice. That is to say although theorists and meta-theorists do not necessarily deal with structure as large as $\mathcal{J}$, it is in terms of structures like $\mathcal{J}$ that fundamental meta-theoretic

concepts confirmation, explanation and especially ontological commitment are to be ultimately analyzed. We will focus almost exclusively on the last mentioned item.

4.12 The first problem of significance is our policy on the members of $\mathcal{T}$ and $\mathcal{R}$. We want to distinguish the various realizations of $\mathcal{T}$, $\langle T, F, R \rangle$. That is to say, we want to know at a particular time, which realization is to count as what $\mathcal{T}$ comes to at that time. In other words, we need a decision procedure for picking out realizations of $\mathcal{T}$ from $\mathcal{T}$ itself. We tie our notion of realization to choices from $\mathcal{T}$ and $\mathcal{R}$ actually made by experts in the field. That is to say what counts as a realization is what is presented as a realization by the workers in the domain of inquiry. We will discuss this policy shortly but first we turn to other matters.

Speaking of realizations of $\mathcal{T}$ in this broad sense gives us tremendous flexibility. Given such a broad perspective we can talk of a theory in the syntactic sense, T , as reconstructed. This seems highly desirable since it seems clear that such syntactic items are frequently reconstructed given advances of instrumentation alteration of attitudes to subsidiary theories and so on. Further, we can distinguish between the foregoing and a radically different sense of the reconstrual of our syntax T . That is to say we can speak of reconstrual in terms of the availability of new higher level theories, that is, reconstrual in terms of

the choice from $\mathcal{Q}$. As an example of the difference: To say that molecular theory is about rigid bodies as well as gases is to choose a wider set from $\mathcal{T}$; this is different than saying that molecular theory is about quarks rather than about electrons viewed as the minimal particle, this last being a choice from $\mathcal{Q}$. This distinction, the distinction between a model in the sense $f(T)$ and a model in the sense $f'(r(T))$ where $r(T) = T'$, is the crucial distinction necessary for a realist view of ontological commitment via theoretic advance.

Notice, what is common among the various realizations of $\mathcal{T}$ is the syntax T . We note this as an instance of our policy drawn from the analysis of reduction in Chapters III and IV; that is the primacy of syntax.

4.121 The models in the range of f 's in F will constitute a diverse set of structures, in fact could include any representation of the syntax. We quote Hanson⁵ with approval as an indicator as the diversity of such a set of elements. We also follow Hanson in permitting various m 's to be proposed prior to the formulation of the syntax. That is to say although we take syntax as central, in point of fact models often pre-date syntax, the latter frequently being an abstraction from a variety of diverse phenomena. The 'algebra' to use Hanson's phrase, "is the structure of the physical process." The various models are representations of both the algebra and the physical process (itself a model) "in

virtue of the fact that they have the same structure as the process itself. They are structural pictures of the dynamical reality."⁶ This view is akin to the position of Suppes whom we follow in maintaining "that the concept of model used by mathematical logicians is the basic and fundamental concept of model needed for exact statement in any branch of empirical science. To agree with this thesis it is not necessary to rule out or deplore variant uses or variant concepts of model now abroad in the empirical sciences...What I have tried to claim is that in an exact statement of the theory or in the exact analysis of data the notion of model in the sense of logicians provides the appropriate intellectual tool for making the analysis both precise and clear."⁷

4.13 We remark on the fact that our policy on $\mathbb{F}$ and $\mathbb{R}$ is an instance of our policy in respect of explanation (Chapter I) and our policy in respect of individual terms and predicates (Chapter I) and our policy in respect of individual terms and predicates (Chapter II) as well as the implicit policy of the authors quoted in Chapters III and IV. In fact our policy seems, if anything, a gloss on the whole rational reconstructionist basis of classical logical empiricism. That is to say, what is reconstructed, hopefully rationally, is scientific practice de facto. The use of actual choices made by specialists in the field as a guide in determining the members of scientific structures need demand no

slavish allegiance to the writings of particular scientists. Feyerabend's fears, in this regard, seem a bit exaggerated. The fact that, for example, Nagel quotes correctly from Chemistry texts in discussing the ideal gas laws is no sign of an all-encompassing acceptance of, say, methodological principles espoused by those same authors. Certainly the point of rational reconstruction is to, ultimately, criticize the aspects of science that go against sound methodological principles. But equally clearly these principles can only be drawn from an extension of sound practice. But this practice can, and frequently does change. And such change is in part the result of the positions drawn from a meta-theoretical analysis of that very same practice.

4.131 Shimony, whose work we will utilize in applying a probabilistic perspective to our construction, offers a number of arguments to substantiate the position that the work of experts in the field constitutes the basis upon which the meta-theoretician builds his structures. He takes the dilemma of scientific inference to be an instance of what he calls a "Copernican epistemology". Borrowing a metaphor from Heraclitus he sees epistemology separating into two problems. "the way up: to show how it is that human beings, whose experience is extremely restricted in space and time and is conditioned by psychological and physiological peculiarities can obtain an understanding of the universe beyond them...the way down: to understand in

terms of fundamental principles how there can be entities such as ourselves in nature capable of the kind of experience and endowed with the kind of faculties which makes natural knowledge possible."

The core of Shimony's response to this dilemma is his willingness to assign prior probabilities to 'seriously proposed hypotheses'. Shimony's problem thus meets ours. Shimony's text is a careful presentation of the various arguments that can be brought forward for the demand that the actual in respect of the behavior of experts in the field, has a conceptual precedence for the meta-theoretician. The key is that a "partial explanation of the success of scientific inquiry...consists in showing that nature is favorable to inquiry." The job is then to show how standard scientific practices are a response to this favorable 'nature'.¹⁰

The problem with any 'social'¹¹ theory of scientific method, that is one that bases decisions on the de-facto judgments of specialists are the "opposing dangers of being too strict and too loose. If the only persons who are to be so respected belong to some delimited group, such as a definite profession, then there is a danger of "blocking the road to inquiry" for such groups are subject to stagnation, parochialism and obeisance to authority. On the other hand, if the conjectures of everyone are to be taken seriously, won't a conscientious investigator find himself overwhelmed by capricious and crankish hypotheses?"¹²

Shimony then goes on to offer an analysis of 'ways out' of the dilemma. These include free access to publication, the maintenance of professional standards, the appeal of intellectual freshness, the dedication of individuals who although poorly trained managed to conquer the "citadels of entrenched scientific opinion." Suffice it to say that although not free of difficulties the appeal to the actual can be defended in a principled fashion. Further, and most importantly, the history of science does demonstrate the development of natural knowledge to the extent that we may 'vindicate the Platonic assumption that truth exercises a gentle but persuasive force upon the minds of men.'¹³

4.132 Naturally our approach, what we might call meta-theoretical 'realism' will find few opponents once it is divorced from the strawman of subservience to practice to the exclusion of criticism. What remains to be dealt with is the argument against the usefulness of formalistic procedures. Here I have no argument short of the intrinsic worth of a clear spelling out of the logic of an phenomena. We certainly feel it a virtue that the formalist procedure permits scientific structures to be handled in ways found useful in mathematics, that our approach permits of the application, as we shall see, of the apparatus of probability theory, and hopefully permits of clarification of a long standing philosophical problem.

4.2 We now return to the technical development of the

notions presented in 4.1 The notions that we define will all be relative to a realization of $\mathcal{T}$, that is to a choice from $\mathcal{T}$ and $\mathcal{R}$, or as before, a triple $\langle T, F, R \rangle$. In our discussion we will omit the qualification 'under a realization' when we speak of T . So for example in the following the model chain C is a chain drawn from the set F relative to the realization in which F is a member. Similarly in 4.3 when we say ' T is model progressive under f ' we should say ' T is model progressive under f relative to a realization $\langle T, F, R \rangle$ for f in F '.

(a) We define a model chain, C , for T as an ordered n -tuple $\langle m_1, \dots, m_n \rangle$ such that for each m_i in the chain m_i is $\langle d_i, g_i \rangle$ for domain d_i and assignment function g_i and where for each d_i, d_j in any m_i, m_j $d_i = d_j$; and where for each $i > j$, m_i is a later (in time) realization of T than m_j .

(b) Let M be any model of T making sure that $f(T) = M$ for some f in F and that $M \models T$, we then say that C is a progressive model chain if

- (i) for every m_i in C m_i is isomorphic to M
- (ii) for most pairs, m_i, m_j , in C , where $i > j$, m_i is a nearer isomorph to M than m_j .

4.21 We say 'most' in (b,ii) since we want to include the possibility of anomolous tests without denying that a chain is progressive. What we whould want here is a fairly complex statistic since, intuitively, order in the chain

should be important, anomalous 'jumps' should be discounted, etc. Recall what we are offering is the merest of sketches.

4.22 The intuition behind the construction is clear. A series of models (a model chain) is progressive if the property of being a model of T is universal in the chain or if the models in the chain are getting 'better' in respect of some particular model of T . This last seems to be the more realistic demand and is intended as a precise analogue of the improvement of 'goodness' of fit relations of models every time. We would deem Approximation II and II, Appendix I as a plausible candidate for the notion 'nearer isomorph'.

4.22.1 We do not feel that it is possible to be precise about the time intervals involved. We do not require, for example, that they be evenly spaced or even decided prior to fact. When a theory is to be crucially tested is dependent on many variables. These variables might include factors like improvement of instrumentation, or the realization that some new set of objects is identical to the domain for the models in question. We would certainly want to guard against making the intervals so long as to deny significant testing or so short as to require tests daily. That is the intuition behind the standard claim in the literature that the mere bulk of confirmation does not increase confirmability of the theory in question.

•••

4.3 Let T' be a subtheory of T in the sense that T' is the restriction of the relational symbols of T to some subset of these. Let f be a member of F . Let $\langle T'_1, \dots, T'_n \rangle$ be an ordered n -tuple such that for each i, j ; $i > j$ T'_i is modeled later than T'_j . We say that T is model progressive under f , if

- (a) T' is identical to T for all indices,
- (b) the ordered n -tuple $\langle T'_1, \dots, T'_n \rangle$ is well ordered by the subset relation.

4.31 The intuition here is clear. A theory is model progressive under f just when f models all subsets of the theory or when f , over time, models more of the theory than before. This has a clear relation to Approximation I, Appendix C. Notice the requirement is more stringent here than for a progressive model chain. The intuition is that although for the models of a theory, here thought of as, perhaps, some experimental realization, we might permit some experiments to go counter to our progressive expectation, however, to find relations in a theory not being satisfied at all should require us to seriously rethink the theory. Notice further we don't require all relations to be always satisfied since many aspects of the theory may not permit of realization since auxillary hypothesis might still be unknown, instrumentation might have to be developed, etc. But once we know how to model a relation we certainly don't want it disappearing from under our noses.

4.32 Let $\langle C_1, \dots, C_n \rangle$ be a well ordering of the progressive

model chains of $\mathcal{J}$, such that for all i, j $i > j$ C_i is a later model chain than C_j , $\mathcal{J}$ is model chain progressive if the n -tuple $\langle C_1, \dots, C_n \rangle$ is well ordered by the sub set relation.

4.321 The intuition here is that if the set of model chains for $\mathcal{J}$ is growing larger over time then $\mathcal{J}$ is model chain progressive.

4.4 So far we have limited our discussion to properties of models and theories in particular realizations. There is another interesting perspective on theories in our broad sense. That is inter theoretic relations holding between items in our broad theories. These will be developed further when we talk about the sets $\mathcal{Q}$. First let us look at a fairly natural notion in respect of the sets $\mathcal{W}$ in various $\mathcal{J}$'s. Let $\langle \mathcal{J}, \mathcal{J}', \mathcal{J}'' \dots \rangle$ be an ordering of all theories seriously proposed at a time. Further let

$$\langle \langle T, F, R, \rangle, \langle T', F', R' \rangle, \langle T'', F'', R'' \rangle, \dots, \rangle$$

be their respective realizations at a time. We say that a set of models M , $M = \langle m, m', m'' \dots \rangle$ is a strongly persistent model set if

(a) $M = \langle m = \langle d, f \rangle, m' = \langle d, f' \rangle, m'' = \langle d, f'' \rangle \dots \rangle$ and f is in F , f' is in F' , f'' is in F'' etc.

or

(b) M is a strongly persistent model set in a ordered subset of $\langle \mathcal{J}, \mathcal{J}', \mathcal{J}'' \dots \rangle$ and the set of ordered subsets of such that M is strongly persistent in them is well ordered

by the sub set relation in time.

4.41 The intuition here is that M is a strongly persistent set of models if the domain of the models of the m 's in M is a domain found either in all theories accepted at a time or is increasingly found in the theories accepted over time.

4.42 Before we turn our attention from the models of $\mathcal{M}$, the value for T under the choices from $\mathcal{M}$, a word about the minimality of our construction. We have generated three, hopefully, natural indicators of the adequacy of $\mathcal{M}$ in respect of its models. We make no claim to the exhaustiveness of these indicators but hope that more fruitful indicators be precisely defined and that all of these be capable of even greater precision and elegance.

4.43 Notice, although we eschew instrumentalism, that is demand that T be modeled in all of its parts, both observational and theoretical an instrumentalist position easily replicates our construction. This by the simple device of restricting the arguments under the members of F to some observational sub part of T . An alternative way of analyzing instrumentalist predelections is to focus on the fact that sets whose domain is medium-sized objects are strongly persistent in tremendous numbers of theories. Even greater specification is afforded if we focus on the persistence of models whose domains are experimental devices of a standard sort. An argument for realism as opposed to instrumentalism would try to show that this strongly persistent set did not afford the grounds for ontological commitment.

That is not to say, however, that the strongly persistent sets are without ontological efficacy. But more of that below.

4.5 We now turn our attention to the members of $\mathcal{R}$. Recall the members of $\mathcal{R}$ represent T in some theory T' in respect of a subset of T , $k(T)$. Let $\langle k_1(T), \dots, k_n(T) \rangle$ be an n -tuple of representations over time, that is if $i > j$, then $k_i(T)$ is a representation of T in T' at a time later than $k_j(T)$. We say that $\mathcal{J}$ is reduction progressive if

- (a) $k(T)$ is identical to $C_n(T)$ for all indices
- (b) the n -tuple is well ordered by the sub set relation.

4.51 A word about what's going on here. If we recall our discussion of Nagel above, Nagel construes the bi-conditionals that 'define' the relations in T in terms of the T' condition as synthetic, that is to say they are empirical claims about the coextensiveness of sets of entities satisfying the relations of the reduced theory and the conditions of the reducing theory. This is analogous to the existence of the map, the representing function, from T into T' . Clearly this map may be more or less articulated, that is to say we may know more or less about the empirical conditions that define the map from T to T' . That is the point of (b) above: a theory is reduction progressive if we are finding more about the conditions under which elements of T can be construed (mapped into) elements of T' .

4.52 Our condition for reduction progressive is closely analogous to our definition (4.3) of model progressive.

We abstain from generating an analogue for 'nearer isomorph' 4.2bii since approximation relations of the sort envisioned being most natural in terms of set theoretical entities seem barely definable in terms of syntactic entities like theories. We would welcome an analogue to 4.2bii here. We note that Eberle mentions the possibility of mapping theories into neighborhoods,¹⁴ but fails to define that crucial concept in the generality needed for non-mathematical syntaxes. We also refer to reader to Apostel¹⁵ for some attempt to explicate approximation relations between syntaxes. We note further that the example put forward at the end of Chapter IV above might be construed as an example of a syntactic approximation but here the mathematical nature of the sentences mapped permit of an approximation that may not be generalizable in non-mathematical contexts.

4.53 The results of Eberle's labeled (a), p. 125 above enables us to relate the aspects of $\mathcal{J}$ encapsulated in the set F with the aspects found in R . In particular; for any model m_1 of the reducing theory, T_1 , there is some model m_2 , of the reduced theory, T_2 , such that for all sentences s in T_2 , m_2 semantically implies s if and only if m_1 semantically implies $r(s)$, where ' $r(s)$ ' is the sentence of T_1 that is the value under r for s , for r in R . We use this fact to make the following demand. If m_1 is a model such that f_1 is in F_1 and $f_1(T_1) = m_1$ then we add to the set F_2 for theory $\mathcal{J}_2$, the function f_2 where $f_2(T_2) = m_2$. In other

words we add to the set of models of T_2 the models picked out by the representing function on T_2 in respect of the models in the set F_1 in $\mathcal{M}_1$.

4.53.1 The intuition behind this move is clear. If we demand of reductions ontological significance, we reflect this demand by permitting reducing theories to point up appropriate models for the reduced theory. We remark on the close analogy between this demand and the principle (P) Koslow demands in Appendix A page 302. So if T_2 is in our sense a 'sub-theory' of some extension of T_2 , T_1 , that is the reducing theory, we demand that models included in the set of proposed models of T_1 be included (added) to the set of models of T_2 .

We call a model m_2 as defined in (4.43) a reduction model for T_2 and, following the intuition of Sellars, maintain such a reduction model for T_2 to be ontologically significant. In fact we can distinguish for any theory T a subset of the set F , call it O , where the models defined as values under the f 's in O are the reduction models for T . We call the set O the Ontic set of T . That is to say O is the set that governs the ontological reinterpretation of T in respect of the set of representing function R for T .

4.6 We now turn to the task of generating a measure for the value of reductions. There seem to be two plausible measures on the value of a reduction. Firstly the depth of the reduction and secondly the breadth of the reduction.

As we shall see we use 'breadth' and 'depth' with clear analogy to the usage of these terms in Chapter I. We deal first with the notion of depth.

(a) We call an n -tuple of theories $\langle T_1, \dots, T_n \rangle$ a deeper reduction chain than an j -tuple $\langle T'_1, \dots, T'_j \rangle$ if for all i, j there is an r_i in R_i such that r_i represents T_i in $T_i + 1$ and similiary for T' and further T_k is identical to T'_k for all $1 \leq k \leq j$ and $n > j$. Intuitively a n -tuple of theories ordered by the reduction relation is a deeper reduction chain then another if the first chain is a super-set of the second chain under the functions r_i .

(i) We call a theory reduction-chain progressive if T is a member of a series of reduction chain each one of which, over time, is a deeper reduction chain then the preceeding ones.

4.61 We remark that there is no demand that the various r 's that order the reduction be identical. Further we make no assumption that the theories so ordered are in reduction progressive $\mathcal{T}$'s. Making these various notions independent permits of a wide variety of weightings as we shall see when we apply our notions to the MBIT Below.

4.611 We remark that our use of the notion of 'chain' is rendered plausible by the following result of Eberle's, "If f represents T_1 in T_2 with respect to K and g represents T_2 in T_3 with respect to K , then their relative product f/g represents T_1 in T_3 with respect to K ."¹⁶ This gives us a notion of 'transitivity' that warrants our use of the term 'chain'.

4.62 In order to generate a notion of breadth we first define the notion of a branching reducer. So, T is a branching reducer if there is a pair (at least) T' and T^* such that there is some r' and r^* in R' and R^* respectively such that r' represents T' in T r^* represents T^* in T and neither T' is represented in T^* nor conversely. A reduction branch, B , of T is a reduction chain, a n -tuple as in 4.61. We say that a branching reducer T is a progressively branching reducer if the n -tuple of reduction branches $\langle B_1, \dots, B_n \rangle$ at some time t is a super set of the set of reduction branches $\langle B_1, \dots, B_k \rangle$ at some earlier time. (4.32) gives the precise statement of the condition by an obvious analogy.

4.621 We direct the reader to 4.221 for our policy on time intervals used throughout.

4.63 If we recall our notion of a strongly persistent model set (4.4, 4.41) the ontic set of models seems a likely candidate for inclusion in the set of strongly persistent model sets. That is especially if the theory from which the model is initially drawn, that is the theory defined, originally, in terms of the domain common to the members of the model set, is a progressively branching reducer and if its reduction chains are in themselves progressive. One way to analyze the depth of commitment to instrumentalism or realism vis a vis the other is the focusing of the meta-theorist on the strongly persistent nature of the set of models of experimental apparatus, for the former, or the

persistence of the ontic set for the latter. Notice, it is no part of our view that realism is the more correct view. Rather we accept realism as the only posture within which the MBIT is coherent.

We say that a set of models O is a most highly favored ontic set if

(a) O is the set of intended model of a theory standing at the head of a progressive model chain. Notice O is thus the ontic set for all of the theories in the chain.

(b) The members of the chain are themselves reduction progressive.

(c) O is the set of intended models of a theory that is a progressively branching reducer.

4.631 This definition is an idealization. We may not require that (1)-(3) are all satisfied at any time. All we will require is that the evidence points to the fact that it is highly probable that (1)-(3) will be satisfied in some set O .

4.7 What we have been doing up till now is attempting to furnish, in terms of the apparatus generated in earlier chapters, various intuitive notions of adequacy for theories in the sense ¶ . Before we can put them to use we must first tie these notions to what we call the 'degree of commitment' for the elements of a scientific structure . For this we return to the work of Shimony.¹⁷

4.71 Shimony offers the following account of what he calls the 'tempered personalist concept of probability'.

Suppose that at the beginning of an investigation a person X has a body i of information and assumptions, and suppose that S is a set of propositions closed under truth-functional operations and containing all seriously processed hypotheses of which X is aware on the matter under investigation and all propositions regarding the possible outcomes of an envisaged set of observations. Then a tempered personalist function over S for X with the body i of information and assumptions is a map assigning to every pair (q, r) of members of S (such that r is consistent with i) a real number which is approximately equal to X's subjective degree of commitment to q upon supposition that r is his total evidence supplementary to i , subject to the conditions: (1) the map satisfies the standard axioms of probability, and (2) the prior probability (that is, the number assigned when r is tautological) of each seriously proposed hypothesis must be sufficiently high to allow the possibility that it will be preferred to all rival, seriously proposed hypotheses as a result of the envisaged observations.¹⁸

We note that condition (1) guarantees that the probabilities are Bayesian since the standard axioms:

- (i) $0 \leq p(h/e) \leq 1$,
- (ii) $P(h/e) = 1$ if e entails h ,
- (iii) $P(h \vee h'/e) = P(h/e) + P(h'/e)$, if e entails $\neg(h \& h')$,
- (iv) $P(h \& h'/e) = P(h/e) \times P(h'/h \& e) \times P(h/e \& h')$

where $P(h/e)$ is a probability function (the probability of h upon e);

imply Bayes Theorem:

$$P(h/e \& a) = \frac{P(h/a) \times P(e/h \& a)}{P(e/a)}$$

What Bayes' theorem asserts is that the posterior probability $P(h/e \& a)$ can be evaluated in terms of the prior probability $P(h/a)$ and the likelihood $P(e/h \& a)$. Where h is the hypothesis

under consideration, e is the evidence for h and a is a body of whatever auxiliary assumptions are required. This guarantees that condition(2), above, is satisfied. Or to use Shimony's phrase, the concept of probability is 'sensitive to truth'; that is to say, given a body of evidence the a posteriori probability of a pair of competing hypothesis will diverge as the evidence increases.

4.711 We note with apology our inability to replicate the careful arguments Shimony puts forward for his view. We offer, instead, the following polemical statement from Shimony's text. Needless to say we recommend, without reservation, Shimony's article and suggest a careful reading for clarifications and further argumentation.

...Bayes theorem...asserts that in a certain sense X is constrained to learn from experience. For if the ratio $P_X(e/a \& h) / P_X(e/a)$ (X 's appraisal of the likelihood given the evidence) is either much greater or much less than 1, then the posterior probability differs sharply from the prior probability: and even though this ratio is a matter of subjective belief, once e is specified, the evidence e actually obtained is not under X 's control, and to this extent the posterior probability is shaped by experience. The personalists claim that in this way the system of personal probabilities is adequate for guiding our practical expectations and for checking the theoretical hypotheses of natural science. They say, furthermore, that all attempts to evade the subjective basis of induction are deceptive, and they cite in particular the subjective character of choices of significance levels and of confidence intervals which are made by statisticians committed to an 'objectivist concept of probability (that is, probability in the sense of relative frequency). The critics of personalism are parodied as saying: Take away the sand, we shall build on the void...¹⁹

4.72 Our use of Shimony's work is as follows. The set of elements of S , the seriously proposed hypothesis will include hypotheses in respect of the various components of and including those properties of these elements elaborated above. That is to say an investigator, X , will be making judgments in respect of evidence, as to whether a realization of $\mathcal{M}$ is progressive, whether the models $f(T)$ form a progressive model chain and so on. This will enable an investigator to assign a weighting in respect of an experimental outcome. Depending upon the sort of hypothesis envisioned the posterior probability of $\mathcal{M}$ might greatly vary. That is some experiments might afford an extremely large increase in the posterior probability of a theory, whereas the same experiment performed in a different context might alter the probability of $\mathcal{M}$ hardly at all. So, for example, if an experiment showed T to be reduction chain progressive, the posterior probability assigned to T might be much higher than if a similar experiment showed T to be merely model progressive. This since, as a reducer, the potential warrant from an additionally large class of experiments, the experiments confirming the reduced theories, would lend credence to T far in excess of what its own experiments could afford.

We consider the highly 'non-uniform' potential for confirmation to be desirable in that it reflects the actual decisions of working scientists. The point of the so-called 'crucial experiments' is that not all experiments count the same. We consider the possibility of widely different

posterior probability on the basis of similar experiments, although within different conceptual contexts, to be a hallmark of the adequacy of our view. This especially in the sense that it is not merely a theory in relation to its evidence that furnishes the posterior probability but the theory and its evidence in the context of a class of possible other evidence and possible reducers. The serious task for the methodologist accepting my framework is to make precise the details of the various weightings of a theory relative to evidence and sensitive to the large frame within which theories occur. Needless to say that takes us far afield from the area of discussion that this thesis is a comment on and will be postponed until some later date.

4.72:1 To give the reader some idea of the use of our conceptual frame we furnish the following. Nagel asserts that the procedures of the reduced science remain invariant under reduction.²⁰ and argues for this position on the grounds that the abrogation of such invariance would redefine the reduced theory in a way that would make the reduction analogous to the 'fallacy of four terms'. Our discussion of Feyerabend, as well as the position outlined in this chapter preclude such an argument from being made. The terms in the statement (syntax) of $\mathcal{M}$ permit a wide range of alteration since we permit models to be freely admitted or excluded from the values of R' in $\mathcal{R}$. But that is to say that no interpretation can be necessarily favored. Any model can be rejected given, for example, an overwhelmingly more powerful

reduction model furnished by some reducer progressive in many more senses than the reduced theory itself.

An attitude like Nagel's seems best construed as the assignments of an overwhelmingly high probability for some favored model (or models) from ⁷. This could be motivated in a number of different ways. The model in question might itself be part of a highly persistent model set either because it is persistent in a wide class of theories not linked by reduction relations, perhaps being a model whose domain includes standard experimental apparatus or because the model in question is persistent in a class of theories all linked via a set of reduction relations. That is the model is persistent in a deep or progressive model chain.

The debate over the principle of Complementarity seems easily analyzed in our terms as well. The theory within which the theory of evidence for Quantum Mechanics (QM) is found is by far the most successful reducer of all times. Classical Mechanics (CM) has had progressive model chains almost from its initiation and until fairly recently; this despite anomalies, the anomalies being less than devastating. For despite the fact that no experimental (observational) model furnishes n-tuples satisfying all of CM, that is we didn't get (4.3,a) CM has been model progressive. More importantly CM was a reducer for which the theories reduced, theories in reduction chains for which CM was the nth theory, were extremely reduction progressive. Moreover CM was included in progressive reduction chains and was the most effective

branching reducer of all times, that is it was a progressively branching reducer, unparalleled until the advent of QM itself. QM on the other hand, although an effective reducer in its own right had an absolute limit to the sense in which it could be model progressive, that is, on pain of inconsistency, no particle or wave model could be adequate to the entire formalism. Furthermore the indeterminacy results characteristic of the slit experiments seemed to place an upper bound on the progressiveness of certain crucial model chains. More importantly problems of mathematical calculation prohibited the model chains for many models from being progressive beyond a point, and here almost any branch of Physics dealing with ordinary objects would serve as an example. Think of familiar comments about the inability to do fluid dynamics of rigid body mechanics in quantum terms if what we mean by these disciplines is their application to engineering problems. But that was as much to say that QM could not be model progressive in a host of models it inherited from CM and where the progressiveness of CM in this respect was the cornerstone of its application to many points of practical interest. The Copenhagen Interpretation in our terms thus seems to be the assignment of a higher probability to the models of CM as strongly persistent at the expense of the fragmentary reduction models offered by QM while placing QM at the head of the reduction chains at which CM had stood, thereby furnishing credence to CM in terms of the progressive reduction branches in which it stood.

What our model seems to afford is the ability to be fragmentary in our judgements of scientific structures. This seems a highly desirable pose if we want to replicate the situation now and seemingly continuing where scientists use a wealth of different theories, models and fragments of both in dealing with aspects of similar situations. It is this mosaic of elements that the methodologist desiring some degree of descriptive adequacy must attempt to replicate. Of course, this position is now familiar from theorists like Feyerabend and Lakatos. What we would hope to show is that a technical development is possible that replicates the complexity of their careful and inspiring descriptions of practice.

4.8 Before we leave this area of discussion let us relate our results to the characteristic Feyerabendian notion of incommensurable. Notice a pair T and T' are incommensurable if they are either deeply inconsistent in the sense made precise in (2.55a) and (2.551) or they are mutually indefinable as specified in (2.55b), (2.554) and (2.632). But then T and T' have no sentence in common if each is self-consistent (for 2.55a) and since (2.55b) implies (2.55a) generally for incommensurable pairs).

4.81 In order to understand the point of this claim define the following. $T' \text{ Rw } T$ (T' weak reduces T) if for some non-empty subset, $k(T)$ of T there is a function, r , such that r represents $k(T)$ in T' . (Notice, if $T' \text{ Re } T$ then $T' \text{ Rw } T$).

By the result of Eberle labeled (c); T_2 is interpretable in T_1 if and only if there is an f that reduces T_2 to T_1 and f maps all s in $T_1 \cap T_2$ onto themselves and the definition of 'interpretable' (p 136), it follows that for all s in $k(T)$ and for all predicates P in s there is a possible definition of the form $(x_1, \dots, x_n) (p_{x_1} \dots, x_n = \phi)$ where ϕ is an open sentence of T with at most $s_1, \dots, s_n$ free. (cf. pp. 156-157). But now we can prove the remark at the end of 4.8.

4.82 If T and T' are consistent and incommensurable then not $T;RwT$.

Proof: We show the theorem first for incommensurable in sense (2.55a). Assume $T'RwT$ for some sentence s at least. But then there is a possible definition of the predicate(s) in s . Call such a possible definition d . But then $T' \cup d \vdash k(T)$ but then by the deduction theorem $T' \wedge d \supset k(T)$ i.e. for all admissible valuations v , $v(T' \wedge d \supset k(T)) = t$. But by (2.554) if T and T' are incommensurable under some v , $v(T' \wedge d \supset k(T)) = u$. That is, since T and T' are incommensurable T and T' under some v (2.541). But if T and T' are incommensurable then T' and $k(T)$ are incommensurable as well since for all s in T (2.551) whence in particular for $k(T)$.

4.821 For 2.55(a) T and T' are deeply inconsistent in the sense of 2.551, that is to say, for all sentences S' of T' , $T \cup \{S'\}$ is unsatisfiable or $(T \cup \{S'\})$ is not an admissible valuation. But then for all possible definitions, d , either d is unsatisfiable or $v(d)$ is not admissible. But

this denies condition (β) of Tarski's definition (page 136 "D is a recursive set of sentences which are valid in T and which are possible definitions in T_1 , of the non-logical constants of T." Recall T_1 is the reducing theory, T_2 is the reduced and T is the common extension of T_1 and T . But, of course, our d is not valid in T since it is either non-satisfiable or demands an inadmissible valuation.

4.822 Given the result above it follows that for no r in R for 'R and T in $\mathcal{J}$ is there a T' such that $r(T) = T'$ and T is incommensurable with T' . That places a limit on the size of $\mathcal{J}$, that is to say $\mathcal{J}$ is not so large as to include incommensurable pairs.

5. Before we turn to the ultimate object of our concern, the MBIT, Let us look once more at a basic Feyerabendian contention - but this time within the context of an area of manifest relevance. As discussed in the preceding chapter, Feyerabend maintains as the basic methodological maxim, the Principle of Proliferation: Invent and elaborate theories which are inconsistent with the accepted point of view. As we saw in the development of his ideas this principle was developed to include theories even more radically disparate than inconsistent pairs, that is to say theories based on deeply inconsistent or divergent principles; that is to say incommensurables. The grounds for those views were essentially epistemological, Feyerabend maintaining that "experimental evidence does not consist in facts pure and simple

but the facts analyzed, modeled and manufactured according to some theory."²¹ Whence the need for the comparison afforded by strong alternatives since "there exist facts that cannot be unearthed except with the help of alternatives to the theory to be tested and that become unavailable as soon as alternatives are excluded."²²

5.1 We now pose the following question; is the principle of proliferation manifested in the sciences in any clear fashion; furthermore if it is, are we willing to accept such a manifestation as an indicator of the methodological appropriateness of the area concerned? The question becomes vital to our interest as soon as we realize that the science of Psychology is a blatant example of a polytheoretic discipline. Psychology as an area discipline includes the most incredibly diverse set of theoretic postures and methodological attitudes. To take an example, in the field of Personality Theory, the most 'mental' of the Psychological sub areas, a standard textbook,²³ one whose authors manifest a strong logical empiricist bias in their Introductory Chapter, no less than fourteen different theories are presented. These theories range from the 'physicalist', Stimulus-Response Theory', the peripheral behaviorism of Skinner, and the highly statistical Factor Theory of Cattell to the characteristically mental frameworks of Jung, Freud, Murray, Rogers and the 'philosophical' frameworks loosely referred to as 'Existentialist'. Pervin²⁴ in a more sophisticated

instance of the genre lists five basic kinds of personality theory, each of them insulated from the others by a variety of deeply divergent methodological (for example, Cattell and Freud) epistemological (Rogers and Skinner) and ontological postures. Boring²⁵ in his classic assessment of the development of modern Psychology lists four radically divergent approaches, Gestalt, Behavioristics, Brain Function and Dynamic Psychology, as the basic divisions within modern Psychology. Brunswik²⁸ lists ten basic attitudes in Psychological research.

5.2 It is hard to know what Feyerabend's response to the example of an area satisfying his maxim would be. Feyerabend hardly ever mentions Psychology at all and in one of his longest disquisitions on the MBIT takes mentalism to be defined by the epistemological criteria of certainty,²⁷ despite the fact that certainty was not a condition for the mental in even the most radical of the mentalists, that is the 19th century Introspectionists. Introspectionist experimenters had to be carefully trained not to confuse the pure with the derived perceptions (cf. Kohler for an interesting picture of the lengths the introspectionist experimental designers went to exclude apparant but irrelevant items).²⁸ Moreover by the early 19th century the problem of the personal equation ruled out any simple minded reliance on the evidence of introspection.²⁹ More importantly the hallmark for a correct introspection was

not epistemological but consistency with the naive neurological view that governed their work³⁰ (see, Kohler). Wundt's major work is entitled 'Physiological Psychology'.

5.3 There seems little point in belaboring the obvious. Most commentary on the MBIT whether pro or con is made in blissful disregard of the sciences and this despite the fact that the MBIT grows out of the unity of science view current in early logical empiricism. We now turn our attention to what a meta-scientific picture of the MBIT would look like.

6. What we will do is the following; firstly, we shall describe the meta scientific picture that would warrant a shift in ontology. Secondly, in light of this picture we sketch, briefly, the warrant for accepting (denying) the MBIT.

6.1 Before we begin let us recall (4.53)-(4.532) where the crucial notion of a reduction model was put forward. In light of those remarks we put forward the following: If O is the set of reduction models for a theory T , that is to say O is a subset of F generated by some T' in the set of values of some r in R , for $\langle T, F, R \rangle \in \mathcal{M}$, then if T' is a most highly valued reducer then the reinterpretation of T in terms of O is warranted.

6.11 We now work towards the notion of a most highly valued reducer. We had generated a few cogent dimensions along

which a scientific structure $\mathcal{T}$ could be evaluated. We now compile them as follows. We call $\mathcal{T}$ progressive if

- (a) $\mathcal{T}$ is model chain progressive (4.23).
- (b) $\mathcal{T}$ is model progressive (4.3).
- (c) $\mathcal{T}$ is reduction progressive (4.5)

6.12 We call T a progressive reducer if

- (d) T is reduction chain progressive.
- (e) T is a progressive branching reducer.

6.13 We say T is a highly valued reducer if T is the syntax of $\mathcal{T}$ and $\mathcal{T}$ is progressive and T is a progressive reducer.

6.131 T is a most highly valued reducer if T is a maximally progressive reducer, that is to say T is the n th member of a reduction chain such that for all T_i , $i < n$, T_i is a highly valued reducer.

6.1311 Of course, if T is a most highly valued reducer it is not reduction progressive. This caveat should be understood throughout.

6.132 Notice if T is a highly progressive reducer then the set of intended models of T , form an ontic set in respect of every T' that T reduces and where this ontic set is a most highly favored ontic set (4.63)

6.14 We notice that all of the T_i 's in a reduction chain are commensurable by the discussion in the (4.7 _{i} 's). Notice that the T_i 's can be partially inconsistent, that is they

can have subsets that are pair-wise inconsistent, but not all subsets or they are incommensurable in sense 2.55a. Such a partial inconsistency merely places a limit on the degree to which the reduced theory can be reduction progressive. That is to say T can be represented in T' up to the inconsistency. This is most easily seen in terms of the relation between representing functions and the notion of interpretability or, alternatively reduction in Nagel's sense. (pp. 156-157).

6.2 As we see it the leading intuition behind the MBIT is the claim that the area roughly referred to as Physiological Psychology is a progressive area. Granted it is only by a leap of the imagination that such an area could be called a **theory** in anything approximating the logicians sense, but if we can accept a piece of out and out science fiction, we might call some projected organization of that amorphous area, in terms of a clear syntactic statement, a progressive theory, that is to say, such a syntactic statement, if available, would be embedded in a progressive scientific structure. We note in passing that the structure is closer to being actual than the syntax. There are a wealth of models of syntactic fragments of such a theory plus a myriad of connections with, in general, Bio-Chemistry. Now the progressive nature of Physiological Psychology would hardly, by itself, warrant the MBIT in its usual, physicalistic, sense. For such an area might itself

be held irreducible to a purely physical basis. Think of the vitalistic ideas that permeated 19th century Experimental Psychology.³¹ The second pillar upon which the MBIT stands is the fact that Biochemistry, now shorn of vitalistic accretions, is a progressive (or even highly valued) reducer and that Physiological Psychology is reduction progressive within a reduction chain at which Biochemistry stands as the nth reducer.

6.21 Notice we stop our reduction chain at Biochemistry. It is not clear that the reduction of Biochemistry to, say, Physics is progressive. The enormity of the translation of Biochemistry into Quantum terms may seriously preclude the chain from getting deeper.

6.22 This intuition places Biochemistry in the role of a most highly valued reducer and so the intuition is warranted in terms of the criteria set forward. Notice if Biochemistry itself is reduction progressive the reduction is even more warranted in that the chain is even deeper than before.

6.23 Given this much it is clear why the neurophysiological model is deemed most appropriate for Psychological theories. It is, of course, because the entities in neurophysiological theories (disregarding purely formalist constructions like these of McCulloch³²) are biochemical entities. Relative to the subject matter at hand, that is, persons, biochemical entities constitute the domain of the

most highly favored ontic set.

6.3 As we have briefly sketched in the foregoing, it is the embedding of the progressive Physiological Psychology in a progressive reduction chain, for a progressively branching reducer, Bio-Chemistry, that seems to be the meta-scientific content of the so called 'one-one correspondence' between the mental and the physical so often found in the literature of the MBIT. But what are the arguments against this pose. Here the position contra is most often based on the incommensurability of the more purely psychological theories with Physiological Psychology. Recall our brief remarks in the 5.i's Physiological Psychology is an alternative to the many mentalistic varieties of theories found within the general research area of human behavioristics. It is a common experience for students in Introductory Psychology courses to be feted with information about neurons, synapses, etc. for the first few weeks of the course. The remainder of the term then turns to other matters, matters of molar behavior, Psychological testing, dynamical theories, etc. The neurophysiological posture serves to orient the student 'metaphysically' - little if anything is done with it for the remainder of the course where the focus is squarely on persons.

6.31 There are a number of grounds upon which the incommensurability of Psychology in the sense of the science of the behavior of persons, with Neurophysiology is based.

we will merely allude to these arguments, since the informal nature of their presentation precluded serious elaboration of the sort that would be consistent with the goals and attitudes of this presentation. Hopefully it will be sufficient to get the point across.

6.311 A persuasive argument with a quasi-formalist core is the claim that the description of properties of persons, especially their actions and their cognitive states, requires a syntax that is intentional. The presumption being that the descriptions of bodies, of whatever sort is only describable in extensional languages. The simplest way to characterize an intensional syntax is in terms of the opacity of substitution into special contexts. So, briefly, a context that permits counter-instances to the following extensionally valid argument is a context within an intensional syntax. If our context is '.....' the argument reads ...x..., $x = y \vdash$...y... The most usual examples of such intensional contexts use 'mental' operators like 'believes that'. Chisholm³⁵ offers a classic account. The intuition here is that since no extensional analysis of such a context is possible (note the enormity of the assumption) no reduction of such intrinsically intentional areas, like Psychology, in extensional areas like Neurophysiology, is possible

5.312 The argument, although persuasive, at first sight is peculiar for a number of reasons. The most telling of

these is that the mere fact that no logical analysis of intensional languages in extensional terms exists seems a poor reason to assume that none could be forthcoming. The most that such an argument might show is that there is a low probability, given the lack of ingenuity of logicians, for such an analysis to be offered. But even this is seemingly too strong. The very youth of the formal disciplines seems a good counter to such a claim. Further intentionality in the sense outlined seems to be based on some sense of content ascription where by 'content ascription' we mean some state S not only being a property of an organism O, but being a property for O, that is to say O takes S into consideration when moving into other states. Here formal advances are occurring at a rapid rate. Although we must in fairness mention that the ascription of content is usually analyzed within the context of machines, not persons. But this should not dishearten the physicalist, for unless the question is begged against him it remains to be seen whether persons are, in fact, machines.³⁴

6.32 Another seemingly persuasive argument against the reducibility of Psychology to Neurophysiology comes from the analysis of mental terms that points up the necessity of molar behavior within a context for the application of these terms to the behavior of persons. These sorts of arguments are frequently found within the Neo-Wittgensteinian school. See for example Malcolm's Problems of Mind,³⁵

in which he argues that no neurophysiological condition could be sufficient for the ascription of many mental predicates to persons since the ascription demands the fulfillment of certain contextual, usually normative, conditions. This position also owes much to the work of Ryle.³⁶

6.321 This argument is interesting and even, probably correct, but the most it shows is that neurophysiology has limits beyond which it cannot be model progressive, that is to say that there will be predicates of Psychology, here thought of as a model of neurophysiology, that will never be reduced.. Alternatively we can use this argument to show the limits for Psychology in terms of its being reduction progressive in respect of Neurophysiology. But notice this does not deny a physicalist ontology since the contexts and even norms might themselves, in principle as they say, be given a physicalist interpretation. Given the almost manifest absurdity of that last piece of science fiction we rest with the prior analysis. We will return to this argument in a bit.

6.33 A third kind of argument denies the reducibility on the grounds of the immediacy of the mental predicates (we could substitute 'privacy' or 'certainty' to get a host of others). These mental predicates are supposedly known by some faculty dubbed 'direct awareness' and are opposed to the mediate nature of our knowledge of, say, brain states. So, for example, although I am immediately aware that I am hungry I can only know through some chain

of inference that my brain is in the correlated state. Some arguments of this genre are at a high level of sophistication.³⁷

6.331 These arguments seem the least persuasive; since, short of the most radical sort of operationalism the fact that there are different modes of access with even radically different logical properties does not deny that the same item is the object of the various modes of access. We remark on Feyerabend's argument in "Problems of Empiricism" to the effect that all the foregoing shows is that if the MBIT is right we have a more direct access to the states of the brain than we thought. In fact we recommend this statement of Feyerabend's position as well as his contributions to Borst³⁸ As an adequate counter to the whole genre of arguments. We recall the 3.4i's above where the core of Feyerabend's response is given within the context of the necessity of some privileged status for the observation base upon which theories are built.

6.34 This brief sketch of the arguments against the MBIT could certainly be expanded. However we feel that such an expansion is unnecessary for even if all of the foregoing arguments are sound little of ontological consequence follows. This last remark must be elaborated.

6.431 Let us assume that Psychology is irreducible, because of its incommensurability, to Neurophysiology or to

put it another way, that Physiological Psychology will never explain in an adequate fashion the behavior of persons. From that it hardly follows that Psychology in some mental sense is progressive in its own right. In fact if Neurophysiology is a reducer of Psychology, at least in part, and it surely is, then all the argument shows is that Psychology is not reduction progressive in respect of Neurophysiology. But that is damaging for Psychology as mental science, for with the abatement of the theological basis for our mental sciences Psychology seems to be a science that, without Neurophysiology, sits in an extremely shallow reduction chain, one deep, or if we accept Psychodynamic theories or some recent moves in Psycholinguistics, two deep, at most. To take the person as the ontic basis for Psychology is just to admit ontological defeat, for with such a Psychology we have little competition for the ontological depth furnished Physiological Psychology by Bio-Chemistry. In fact, in such a Psychology no account of the real nature of persons is forthcoming, that is to say that Psychology doesn't reduce.

6.3411 To clarify a point made in passing. Up until the Renaissance, at least, Psychology, that is to say the study of persons, was embedded in a complex metaphysical structure that might be called 'soul theory'. Human beings had their behavior explained in terms of a special substance whose fundamental properties were analysed according to a theological model. Although soul theory was not a micro-

reducer in the standard sense, i.e. God was not the small components out of which human beings were composed, soul theory afforded the kind of theoretic underpinning that micro-reducers furnish to contemporary theories. That is to say that the properties of human beings, their penchant to evil, their base nature, their free will and so on were explained by virtue of their relation to the deity and his plans and purposes. Given the lack of commitment to such a theological base in contemporary Psychology the problem was what was to replace soul theory as the deep explanation for the Psychological generalizations that were increasingly to be found. The MBIT is an attempt to offer such a substitute.

6.342 The conclusion to be drawn from such a state of affairs seems to be a radical limit to our ability to understand persons. But this is clearly not to deny that persons are extremely complex physical organisms for it is just to deny that we know, with any depth, what persons are at all. That is to say that we know what persons are except on the level of empirical generalization. But the observation level generalization tells us merely what things do not what they are. As early as Wundt it was realized that what was needed in Psychology was observational generalizations rather than metaphysics. If we think of the basis of Psychology within what we have called 'soul theory', we find the mind being a vehicle for ontological commitment at a remarkably high level of concern. To quote Boring "Wundt ruled out the metaphysics, which Fechner, with his dominant philosophical interest, had not

avoided and which Lotze in his 'physiology of the soul' had cultivated. German psychology has always been metaphysical. Wundt, almost a philosopher, began the anti-metaphysical tradition which still persists."³⁹ We now have a clear surrogate for the anti-metaphysical nature of much of Psychology. If we take as our fundamental insight the position of Sellars referred to throughout, Psychology is a theory without an ontology, just in so far as Psychology is a theory without any reducers.

6.3421 We do of course grant that Psycho-dynamic theories, like Freuds afford some sense of theoretic depth to Psychological generalizations. But here, as we commented before, the depth is very shallow. Freud himself, of course, thought that his Psychodynamics would be reduced in terms of Neurophysiology. To quote Fancher, Freud's "ambition was to write a 'Psychology for Neurologists' whose purpose would be to see how the theory of mental functioning takes shape if quantitative considerations, a sort of economics of nerve force, are introduced into it!"⁴⁰ Freud's strongest influence where Helmholtz and Brucke. The state of neurophysiology at that period forced Freud to turn to 'psychology'. But nothing he generated, certainly not 'psychic energy' could be substituted for the depth of explanation such a Neurophysiological account could have offered. Or would still if one were available.

6.35 The upshot of this extremely brief reminder is as

follows. We may never be able to show that persons are neurological complexes that is if the arguments in 6.311-6.331 are sound. But we may never be able to do this if what we mean is something like showing how what persons do has analogous within neurophysiology and this without the basis of these standard arguments against the MBIT. But nothing said so far implies, or even suggests that persons are reliant for what they do on some irreducible mental stuff. The failure to reduce seems to be without ontological consequences. The epistemological and methodological consequences are, however, enormous. That is to say there might be systems whose complex and novel properties cannot be understood in any deep way; that about some subject matters we must be satisfied with mere generalizations rules of thumb, fragments of procedures etc. It would not surprise me if Persons turned out to be such unfathomable entities - but so is the brain, and so too is an array of dynamical particles with many members and so is subterranean turbulence within the earth's crust.

FOOTNOTES

Chapter VI

¹I. Lakatos, "Falsification and the Methodology of Research Programmes," Criticism and the Growth of Knowledge, I. Lakatos and A. Musgrave, eds. (Cambridge: Cambridge University Press, 1970). Idem, "History of Science and Its Rational Reconstruction," Boston Studies in the Philosophy of Science, R. Buck and R. Cohen, eds. (Dordrecht-Holland: D. Reidel Publishing Co., 1971), vol. 8.

²Idem, "History of Science," p. 99.

³Ibid., p. 100.

⁴T. Kuhn, "Reflections on My Critics," Criticism and the Growth of Knowledge, p. 239; P. Feyerabend, "Consolation for the Specialist," Criticism, p. 215.

⁵N. Hanson, "A Picture Theory of Theory Meaning," R. Colodny, The Nature and Function of Scientific Theories (Pittsburgh: The University of Pittsburgh Press, 1970).

⁶Ibid., p. 272.

⁷P. Suppes, "A Comparison of the Meaning and Uses of Models in Mathematics and the Empirical Sciences," Studies in the Methodology and Foundations of Science (New York: Humanities Press, 1969), p. 17.

⁸A. Shimony, "Scientific Inference," in Colodny, The Nature and Function.

⁹Ibid., p. 139.

¹⁰See Ibid. pp. 140-49 for the careful presentation of the argument.

¹¹Shimony quotes Peirce as follows. "He who would not sacrifice his own soul to save the whole world is, as it seems to me, illogical in all his inferences, collectively. Logic is based on the social principle." Ibid., p. 103.

¹²Ibid., p. 110.

¹³Ibid., p. 111.

¹⁴R. Eberle, "Replacing One Theory by Another Under Preservation of a Given Feature," Philosophy of Science, 1971, vol. 38, p. 490.

¹⁵L. Apostel, "Towards the Formal Study of Models in the Non-Formal Sciences," The Concept and the Role of the Model in Mathematics and the Natural and Social Sciences (Dordrecht-Holland: D. Reidel Publishing Co., 1961), sec. C, pp. 23ff.

¹⁶Eberle, "Replacing One Theory," p. 491.

¹⁷Shimony, "Scientific Inference."

¹⁸Ibid., p. 101.

¹⁹Ibid., pp. 91-92.

²⁰E. Nagel, Structure of Science (New York: Harcourt, Brace and World, 1961), pp. 345, 349, 352.

²¹Feyerabend, "Explanation, Reduction and Empiricism," Minnesota Studies, Feigl and Maxwell, eds., vol. 3, p. 51.

²²Idem, "Problem of Empiricism," Beyond the Edge, Colodny, ed. p. 175.

²³C. Hall and G. Lindzey, Theories of Personality (New York: John Wiley and Sons, Inc., 1970), 2nd Edition.

²⁴L. Pervin, Personality: Theory Assessment and Research (New York: John Wiley and Sons, Inc., 1970).

²⁵E. Boring, A History of Experimental Psychology, (New York: Appleton Century Crofts, Inc., 1950) 2nd Edition.

²⁶E. Brunswik, The Conceptual Framework of Psychology (Chicago: University of Chicago Press, 1952).

²⁷Feyerabend, "Problems," sec. 10-13.

²⁸W. Kohler, Gestalt Psychology (New York: New American Library, 1947) esp. Chapter 2.

²⁹Boring, A History of Experimental Psychology, Ch. 8.

³⁰Kohler, Gestalt, Ch. 2.

³¹Boring, A History of, Ch. 14-16.

³²W. McCulloch and W. Pitts, "A Logical Calculus of the Ideas Imminent in Nervous Activity," Bulletin of Mathematical Biophysics, 1943, 5.

³³R. Chisholm, "Brentano on Descriptive Psychology and the Intentional," Introduction to the Philosophy of Mind, H. Morick, ed. (Glenview, Illinois: Scott, Foreman and Company, 1970).

³⁴We recommend D. Dennett, Content and Consciousness (London: Routledge and Kegan Paul, 1969) to the reader for a careful account of these and related matters. See also F. Crosson and K. Sayre, eds. Philosophy and Cybernetics (Notre Dame, Indiana: University of Notre Dame Press, 1967).

³⁵N. Malcolm, Problems of Mind, (New York: Harper Torchbooks, 1971).

³⁶G. Ryle, The Concept of Mind (London: The Hutchinson Publishing Group, 1949).

³⁷An interesting and sophisticated argument having to do with the irreconcilability of identity conditions for mental and physical states is found in J. Thomson, "The Identity Thesis," Philosophy, Science and Method, S. Morgenbesser, P. Suppes and M. White, eds. (New York: St. Martin's Press, 1969).

³⁸C. Borst, The Mind/Brain Identity Theory (London: MacMillan, 1970).

³⁹Boring, "A History," p. 332.

⁴⁰R. Fancher, Psychoanalytic Psychology (New York: W.W. Norton and Co., Inc., 1973).

APPENDIX A:
TRIVIALIZATION INSTANCES AGAINST THE
D-N MODEL FROM I. A. OMER¹

If we take the D-N model of explanation to be minimally specified as T, C implies E , where T is some set of sentences universal in form, including some 'law' or 'law-like generalization' and C a set of initial conditions; jointly the explanans, and E the explanandum, the following constitute inappropriate explanation schemata consistent with the model as minimally specified.

(a) $(x)Px, Pa \supset R(ab)$ implies Rab . This is held to be undesirable since ' $Pa \supset Rab$ ' is equivalent to ' $\sim Pa \vee Rab$ ' whence it can only be known to be true by verifying ' Rab ' itself. The example is from the classic statement of the D-N model in Hempel and Oppenheim, "Studies in the Logic of Explanation."² Ackerman³ makes a similar claim blocking, $(x) Mx, Ma \supset E$ implies, E , in similar grounds.

(b) Given $(x) (Fx \supset Gx)$, Fa implies Ga , as paradigmatically appropriate, $(x) (Fx \supset Gx), Fa \vee Sa, Fa \vee \sim Sa$ implies Ga , is deemed inappropriate since the explanans is irrelevantly weakened.

(c) Maximizing content for the explanans forces the following: $(x) (Fx \supset Gx), (Ha \vee Fa), (Ea \vee \sim Fa)$ implies $Ha \vee Ka$, to be stated as $(x) (Fx \supset Gx), Ha \vee (Fa \wedge \sim Ga)$ implies $(Ha \vee Ka)$. But

this gives us clear reason to reject the latter since we can't claim ' $(x)(Fx \supset Gx)$ ' while pretending ignorance towards ' $(Fa \wedge \sim Ga)$ '. This example is from Kim.⁴ He offers much the same example in a later work⁵ reproduced in this text, Chapter II, p. 53.

(d) $(x)(Fx \supset Gx)$, Fa implies $Ga \vee Ha$, is rejected on the grounds of 'redundancy' that is to say less content in the explanandum than is appropriate. For example, $(x)(Fx \supset (Gx \vee Hx))$, Fa would be an appropriate explanans for the explanandum ' $Ga \vee Ha$ '. Similarly, $(x)(Fx \supset Gx)$, $(x)(Gx \supset Hx)$, Fa implies Ha is deemed undesirable in favor of, $(x)(Fx \supset Hx)$, Fa implies Ha .

(e) A slightly more complex example from Ackerman⁶ is as follows. Suppose that in a scientific language L , Hx and Ex are primitive predicates and that some explanation like the following is a... (D^*N) ...explanation, $(x)(Hx \supset Ex)$, Ha (implies) Ea . Later developments might result in the discovery that Hx and Ex could be defined as...
 $Rx \wedge Sx$ and $Sx \wedge Tx$ respectively...(this generates)...
 $(x)(Rx \wedge Sx \supset Sx \wedge Tx)$, $Ra \wedge Sa$ (implies) $Sa \wedge Ta$. This explanation would be ruled out since $Ra \wedge Sa$ (implies) Sa . But notice that means that a more detailed specification of an event may make it inappropriately explained.

Omer's criteria listed in Chapter I above are generated to afford a principled account of criteria sufficient to rule out examples like (a)-(e) while retaining the para-

digmatic, $(x)(Fx \supset Gx)$, Fa implies Ga . The examples cited have all been initially compiled by Omer in the work cited.

FOOTNOTES

Appendix A

¹I. A. Omer, "On the D-N Model of Scientific Explanation," Philosophy of Science, vol. 37.

²C. Hempel, "Studies in the Logic of Explanation," Philosophy of Science, vol. 15, 1948.

³R. Ackermann, "Deductive Scientific Explanation," Philosophy of Science, vol. 32, 1965.

⁴J. Kim, "On the Logical Conditions of Deductive Explanation," Philosophy of Science, vol. 30, 1963.

⁵Idem,

⁶Ackermann, "Deductive..."

APPENDIX B:
 NOTES ON SUPPES REDUCTION
 FROM THE WORK OF A. KOSLOW

In the following we let T, T^* , etc. be theories and we say $TRst'$ if and only if every model of T' is isomorphic to a model in T . We define $S(T)$ if and only if M is a model of T , then M is isomorphic to a model in T (Note $S(T)$ if and only if $TRsT$).

Theorem 1: If $T^* \vdash T$ and T^*RsT then $S(T)$

Proof: Suppose that M is a model of T^* , then M is a model of T also. Since there is a Suppes-type reduction of T by T^* , M is isomorphic to M' a model in T^* , i.e. $S(T^*)$ ■

This shows that if a theory T^* implies T and is a Suppes-type reducer then T^* must be 'full', i.e. $S(T^*)$.

Theorem 2: If $T^* \vdash T$ and $S(T)$ then $TRsT^*$

Proof: Let M be a model of T^* then it is a model of T , then it is isomorphic to M' a model in T (since $S(T)$).

This says that a 'full theory T reduces (Suppes-style) any theory which deductively implies it.... ■

Theorem 4: Suppose that we had a series of theories $T_1, T_2, T_3, \dots$ such that

(1) $T_1 \vdash T_2 \vdash T_3 \vdash \dots \vdash T_n$ and

(2) $T_1 \text{Rs} T_2, T_2 \text{Rs} T_3, \dots, T_{n-1} \text{Rs} T_n$, then each T_i is 'equivalent' to T_{i+1} , i.e. $T_i \text{Rs} T_{i+1}$ and $T_{i+1} \text{Rs} T_i$.

Proof: By Theorem 1 and (1) we obtain

$a_1: T_1 \text{Rs} T_2$ implies $S(T_1)$

$a_2: T_2 \text{Rs} T_3$ implies $S(T_2)$

$a_{n-1}: T_{n-1} \text{Rs} T_n$ implies $S(T_{n-1})$

Thus we have $S(T_1), S(T_2) \dots S(T_{n-1})$.

By Theorem 2 and (1) we obtain

$b_1: S(T_2)$ implies $T_2 \text{Rs} T_1$

$b_2: S(T_3)$ implies $T_3 \text{Rs} T_2$

$b_{n-1}: S(T_{n-1})$ implies $T_{n-1} \text{Rs} T_{n-2}$

$b_n: S(T_n)$ implies $T_n \text{Rs} T_{n-1}$

Comparing, we find using $S(T_1), \dots, S(T_{n-1})$ and the b_i 's that $T_2 \text{Rs} T_1, T_2 \text{Rs} T_2, \dots, T_{n-1} \text{Rs} T_{n-2}$, i.e. all the theories $T_1, \dots, T_{n-1}$ are 'equivalent' that is mutually reducing. To paraphrase Koslow's conclusion: If the theories in the chain are deductively linked then Suppes' reduction implies that the maximal chain of deductively linked theories is two members deep.

Pursuing the thought that models in T are members of a special class of models of T , we might require the following principle:

(P) If T_2 is a subclass of the theory T_1 then any model which is a model in T_1 is a model in T_2 .

The idea behind (P) is plausible if we think in terms of sets

of intended models. If C is a class of intended models of T_1 and T_2 is a subclass of T_1 , then surely those models which are the standard ones for T_1 are included among those which are standard for T_2 (we repeat, a subclass of T_1)...

Theorem 6: If $T^* \vdash T$ and $T^* RST$, then $S(T)$

Proof: Let M be a model of T . Then M is isomorphic to M' a model in T^* (since $T^* RST$). By (P) M' is a model in T so that $S(T)$.

Notice that this is the mate to Theorem 1 above. Thus together we have:

If $T^* \vdash T$ and $T^* RST$, then $S(T^*)$ and $S(T)$. This leads to another theorem.

Theorem 7: Given (P) and theories $T_1, T_2, \dots, T_n$ such that

- (1) $T_1 \vdash T_2 \vdash T_3 \vdash \dots \vdash T_{n-1} \vdash T_n$, and
- (2) $T_1 RST_2, T_2 RST_3, \dots, T_{n-1} RST_n$, then all the theories $T_1, \dots, T_n$ are Suppes-style equivalent, i.e. mutually reducing, that is $T_i RST_{i-1}$ and $T_{i-1} RST_i$.

Proof: By analyzing the proof of Theorem 4 above we see that all the $T_1, \dots, T_{n-1}$ are Suppes-style equivalent. Our concern is to show that T_n is also equivalent to T_{n-1} by showing that $T_n RST_{n-1}$. By hypothesis $T_{n-1} RST_n$. Further $T_{n-1} \vdash T_n$, therefore by Theorem 6, $S(T_n)$. But from our proof of Theorem 4 we have b_n , i.e. $S(T_n)$ implies

$T_n RST_{n-1}$, therefore $T_n RST_{n-1}$.

Theorem 7 with the aid of (P) extends Theorem 4 so that if Suppes reduce a chain of deductively connected theories then no nonequivalent reduction is ever possible... By Theorem 7, $T_1 R_s T_2$ and $T_2 R_s T_1$, i.e. Suppes-style reduction never gets underway (for deductively linked theories).

Note to the reader: Koslow's results were originally brought forward against the claim of Shaffner¹ that T_1 Nagel reduces T_2 implies $T_i R_s T_2$, where Nagel-reduction is analyzed as deductive consequence. Koslow's proofs thus show the unhappy consequences of Shaffner's claim. As we see in Chapter IV, Nagel's views are not adequately captured by the simple relation of deductive consequence.

FOOTNOTE

Appendix B

¹K. Schaffner, "Approaches to Reduction," Philosophy of Science, 1967, vol. 34.

APPENDIX C:
WEAKENINGS OF THE ISOMORPHISM RELATION
APPROPRIATE FOR USE IN THE
NON-FORMAL SCIENCES, FROM L. APOSTEL¹

Our purpose in this section will be to define several approximate or strengthened forms of isomorphism. To reach this aim let us first realise what is included in the notion of isomorphism.

Two sets D_1 and D_2 are isomorphic with respect to relations R and S , defined respectively on D_1 and D_2 at least, if the following situation occurs: there exists a mapping function F such that to each member of D_1 there corresponds one and only one member of D_2 under F , and if moreover whenever members of D_1 stand in the relation R , their F -images stand in the relation S , and inversely, then we say that the two domains are isomorphic under the two relations R and S . Two sets will be completely isomorphic if they are isomorphic under all their relations. Two relations will be completely isomorphic if they are isomorphic on all their domains. Two sets will be called isomorphic with reference to a class K of relations if the relations under which they are isomorphic under a set K of relations if the mapping relations that correlate the relations on the two sets have to belong to a given class K .

A well-known and much-used generalisation of isomorphism is homomorphism. Here the correlator has not to be one-one but is allowed to be many-one. As for the rest, the relations remain the same.

We wish to consider approximative isomorphisms and homomorphisms and take some steps towards ordering or even measuring these approximations.

We give some possible forms of approximation.

Approximation I: The correlator may be such that this function does not map the whole of the domain of R on S , nor the whole of the domain of S on R . We then have an approximate direct or inverse correlator. A correlator A will be a closer approximation than a correlator B if the class of members of the field of R that is not mapped upon the field of S by A is a proper subset of the class of members of the field of R not mapped upon the field of S by B . If we possess a measure on our sets we can define one approximation to a correlator as better than the other one, if the measure of the non-mapped set is smaller in the first case.

Approximation II: The correspondence may be such that not always when R exists in D_1 between some elements, S exists in D_2 between the images of those elements. The correspondence C_3 is a closer approximation than the correspondence C_2 if for C , the n -uples where the images have not the corresponding relations are a proper subset of the set of n -uples which for C_2 do not have the corresponding relations.

We shall say that an App. II-neighbourhood-relation iso-

...

morphism exists if for all cases in which the correspondence does not hold a relation lying in the neighbourhood (provided such a concept is defined) of the relation that should occur, holds. We shall say that an App. II-neighbourhood-element isomorphism exists if for all cases in which the correspondence does not hold, some elements in the neighbourhood of the image elements present the desired relation.

We wish to stress that approximation-neighbourhood isomorphisms of type I (both of relation and element kind) can be defined in the same way. If the definition of neighbourhood for one approximation is a refinement of the definition of neighbourhood for another approximation, then the first is a closer neighbourhood element or relation approximation than the second.

Approximation III: An App. III-isomorphism is both an App. I and an App. II isomorphism.

Approximation IV: In all preceding approximations we have considered the set D and the relation R to be classical sets or relations, defined everywhere, and precise everywhere. Let us now introduce for sets and for relations indetermination domains; i.e. elements for which it is not decidable if they belong to a set or not and couples for which it is not decidable if they belong to a relation or not. $D'1$ is a closer approximation to $D1$ than $D''1$: if the determination domain of $D'1$ has a larger intersection with $D1$ than that of $D''1$ (if no measure is available: if the first intersection includes the second). $D'1$ is moreover a tighter approximation to $D1$ than $D''1$: if the indetermination do-

main of the first is included in the indetermination domain of the second. Finally D_1 is a more adequate approximation to D_1 than D_1' if the intersection of the indetermination domain of D_1 with D_1 is included as a proper part in the intersection of the indetermination domain of D_1' with D_1 . It is important to understand that a closer, a tighter and a more adequate approximation to the same set are by no means always identical. Moreover, we have supposed here the set D_1 to be exact. It is obvious that we should also consider approximations to inexact sets.

A relation can have an indetermination region for its domain, and for its co-domain. If we want to consider isomorphism between approximate sets and approximate relations, we meet first the problem of the intersection of the indetermination region for relations and for sets. Either one of both can be zero, or both can exist; if they both exist they can intersect (even be included in each other) or be disjunct from each other. It is obvious that the case in which they coincide will be the easiest one for our purpose.

Let us say that if we have a set with an indetermination domain, and on this set a relation R with again an indetermination domain, then there is exact isomorphism between both if there is a correlator mapping members of the kernel of D_1 on members of the kernel of D_2 , members of the indetermination domain of D_1 on members of the indetermination domain of D_2 , and such that if R holds between elements of D_1 , S hold between elements of D_2 and if a n -uple is in D in

the indetermination domain of R, the images of it are in D2 in the indetermination domain of S. This strict isomorphism immediately gives birth to a series of approximation: we may call alpha IV approximation the case where the correlator maps elements of the kernel of D1 on elements of the indetermination domain of D2 (or inversely), beta IV where elements of indetermination domains are mapped on elements of kernels, gamma IV where elements of indetermination domains are not mapped (and thus in a sense mapped on complements), delta IV approximation where n-uples in the kernel of R are mapped on N-uples in the indetermination domain of S.

We shall call one isomorphism closer or tighter or more adequate if a closer, tighter or more adequate approximate to both D1 and R is correlated with a closer, tighter or more adequate correlate to both D2 and S. This definition again gives us the occasion to develop a sequence of isomorphisms: we can map namely a closer approximation to D1 on a non-closer approximation to D2, or we can have the approximations to D1 and D2 closer but not the approximations to R and S, or we can have all closer but only some tighter, and so forth.

Approximation V isomorphism: let us be aware of the fact that in approximation IV both correlator and correspondence were exact relations. The great multiplicity of cases already encountered was due to the indetermination of D, R and S and not to that of correlator or correspondence. It is now the place to mention that we can combine any form of approximation

IV with approximations I, II or III. Here the situation becomes extremely complex and we can only stress this feature. Let it also be stressed that even in approximation I, II and III, the correlator and the correspondence had no indetermination domains. The two feature we are thus liberalizing here, in V, are radically independent: we consider imprecise correlators and correspondences, and we consider moreover partial correlators and correspondences. It is perhaps best that we distinguish approximation V, alpha (subspecies: I, II and III), approximation V beta (subspecies: all subspecies of IV) and approximation gamma (both alpha and beta).

Approximation VI: Let it now be stated that in all the previous cases, even if we depart very strongly from the simple classical picture, we have been presupposing that a system can be described as a relational structure with given clear-cut relations and elements, the system having perhaps indetermination domains and the relations also, but these entities themselves being given and the wholes being built up out of these parts. It seems clear that this is a radical dependence on a type of logical atomism that geometry precisely tries to overcome through the construction of a geometry without points). An algebra without elements seems as urgent a desideratum if we want to develop the model theory for natural systems. Let $\underline{n}$ systems be given. We do not presuppose the concept of element or of relation, but we define them with respect to these systems. An element is

the smallest system that systems can have in common (as always in these topics, we presuppose the part-whole relationship and some of its properties), and a relation is a minimal system of sequences that systems can have in common. The definition of what is to be called element or relation should thus be a function of the intersection of systems.

FOOTNOTES

Appendix C

¹C. Apostel, "Toward the Formal Study of Models in the Non-Formal Sciences," The Concept and the Role of the Model in the Mathematical and Natural Sciences (Dordrecht-Holland: D. Reidel Co., 1961).

APPENDIX D:
REMARKS OF BUNGE ON THE COMPLEXITIES
OF INTER-THEORY RELATIONS

The philosophers interested in reduction seem to take it for granted that science teems with successful reductions: that thermodynamics has been completely reduced to statistical mechanics; that rigid body mechanics has been reduced to particle mechanics; that classical mechanics has been reduced to quantum mechanics; that every relativistic theory has at least one and at most one nonrelativistic limit--and so on.

The textbook paradigm of theory reduction is, of course, the alleged reduction of thermodynamics to statistical mechanics. This is usually accomplished, or rather attempted, by enriching the basic equations of classical point mechanics (wrongly supposed to account for the behavior of atoms and molecules) with stochastic hypotheses concerning chaotic initial conditons--or, rather, about the irrelevance of the precise initial state. It would be surprising if this trick were to work in general, for one knows that atoms and molecules are not structureless point masses but rather enormously complex quantum-mechanical systems glued by fields, which are nonmechanical entities.

As a matter of fact the trick does not work in general: in fact, only elementary kinetic theory--which ignores the

2nd law of thermodynamics--and some thermodynamic formulas have been obtained in this way. Thermodynamics as a whole, and particularly the 2nd law, which is its most distinctive feature, has not been reduced to particle mechanics--nor, for that matter, have been fluid dynamics, the mechanics of deformable bodies, and other branches of continuum physics. The reduction of thermodynamics is not a fact but a program. A good but unfulfilled program.

It is usually believed that every relativistic theory has exactly one non-relativistic limit, so that if the latter is taken, all second order and higher order 'effects' are lost but on the other hand the bulk of the facts, the first order 'effects', are kept. We shall presently show that, while some relativistic theories have no nonrelativistic 'limit', others have more than one, so that the belief under scrutiny is false.

Maxwell's electromagnetic theory for empty space is a relativistic theory--moreover it was so avant la lettre--and one that has no nonrelativistic limit. In fact, the basic equations of this theory contain no mechanical velocity v , whence there is no point in taking the limit of the functions involved for $v \rightarrow c$. And, as for taking their limit for c approaching infinity, it makes no sense either for it leaves us with the subtheory of static fields, wiping out the peculiarity of electromagnetism--electromagnetic induction. In short, there is no nonrelativistic approximation of Maxwell's electromagnetic theory: there are only the nonrela-

tivistic subtheories of electrostatics and magnetostatics, and the nonrelativistic approximations of electrodynamics (which is a different story). This simple metatheoretical result is important because it explodes the myths (a) that relativity is just a matter of higher order 'effects' (a refinement necessary only for high energy phenomena), and (b) that every relativistic theory has a nonrelativistic 'limit' that covers essentially the same ground.

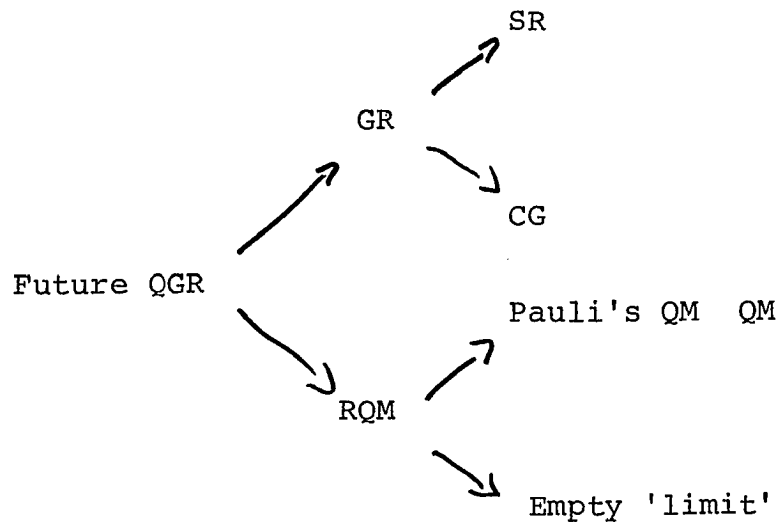
As to the existence of multiple nonrelativistic limits, the simplest case is the one of general relativity, or GR for short, GR goes over into SR for vanishing gravitation (equivalently: for flat space), but it goes over into the classical theory CG of gravitation (of Newton and Poisson) for weak static fields and slow motions. (Actually there is a third limit, namely for a vanishing matter tensor. In this case spacetime can still be Riemannian, and no previous physical theory is obtained, for there are neither matter nor electromagnetic fields left. But this case seems to be without a physical interest, both because it corresponds to no real situation and because it agrees with no previous physical theory: it is a factually empty limit.

It is of some interest to note that the CG limit of GR is not obtained by letting c go to infinity in all formulas. In fact, one of the specializations made for obtaining this classical (or rather semiclassical) limit is that all the coefficients of the matter tensor are set equal to zero except the 00 component, which is set equal to $m_0 c^2$. The existence of two different and nonempty limits of GR is also of inter-

est in that it vindicates Einstein's claim (disputed by Fock) that GR is a generalization of SR; but Fock, too, is thereby shown to be partly right in claiming that GR is a generalization of CG. As long as the one-limit tenet is kept, either Einstein or Fock will be regarded as possessing the whole truth concerning the nature of GR. Finally, if a quantum theory of gravitation were successful, it would presumably have at least two different limits: GR for either $\hbar \rightarrow 0$ or $T_{\mu\nu} = \langle T_{\mu\nu}^{QM} \rangle$, and relativistic QM for vanishing gravitation.

As to Dirac's quantum theory of the electron, there are two ways of obtaining a nonrelativistic 'limit' of it. One is the standard procedure of neglecting all the operators whose eigenvalues (or whose averages) are of the second order in v/c or higher; the other is to keep these operators while dropping the 'small' components of the state spinor, i.e. those which are of the order of v/c times the 'large' spinor components. Not surprisingly, two entirely different 'limits' are obtained: the first procedure yields essentially Pauli's nonrelativistic theory of the spinning particle, while the second procedure leads to an equation containing a spin-orbit term absent from the former. This second limit appears to be factually empty. Which refutes one more popular tenet, namely that every 'limit' of a given theory covers a subset of facts of the former. As to the second 'limit' (Pauli's theory), it reduces to Schrodinger's theory upon dropping the spin operator.

The situation so far is summarized as follows:



No arrows have been placed after SR and QM because the relations of these general theories to the more special ones they are supposed to subsume are so far not well understood. In particular, it is not known how to obtain the whole of classical mechanics (i.e. continuum mechanics) from QM, even though every textbook, hence almost every philosopher of science, takes this reduction to be a fait accompli.

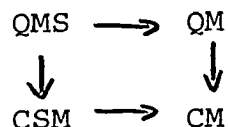
The situation is even more complicated in quantum theory. In this case one can make the following comparisons: (a) quantum-theoretical eigenvalues vs. possible classical values; (b) quantum-theoretical averages vs. possible classical values; (c) quantum-theoretical operators vs. classical dynamical variables. The first two comparisons are not as easy to make as is usually believed. To begin with, which classical theory is to be taken: classical particle mechanics, classical continuum mechanics, classical electrodynamics, or

what? Then, which limits should one take? Should one set Planck's constant equal to zero--and then lose the spin, which has a classical partner? Or should one take very large masses--which is false for a one-body microsystem? Or, finally, should one take the large quantum number approximation--which makes sense for bound states (discrete spectra) only? As to the dynamical variables themselves, all one gets is some analogies which are heuristically fertile but not much more than this. The quantum-classical comparison, in short, is far from being a simple affair.

One of the difficulties with the comparison is that the infinitely dimensional Hilbert space, representing the states of the system, has no classical limit. In this respect QM is much more radically new than any other nonclassical theory. (Only the phase of the state vector of a system does look classical, in that its equation of evolution is similar to a classical Hamilton-Jacobi equation. But then the latter need not concern a mechanical system.) If one focuses on the state vector while forgetting the operators, he will tend to interpret QM as a field or wave theory, whereas if he focuses on the dynamical variables he will tend to interpret it as a quaint theory of odd particles. But, clearly, these are just partial classical analogs: the theory as a whole fails to have a classical analog.

Moreover, QM and CM were not built to cope with the same problems: the former was not framed to pose and answer questions of dynamics, such as the trajectory of an

electron in a slit system. The task before the builders of QM was essentially to account for the very existence, the structure, and the spectra of atoms. The rest--a peculiar dynamics, molecular theory, and nuclear theory--came as a bonus. Consequently the founding fathers of QM did not enlarge mechanics, the science of motion. The new theory was called mechanics probably due to the mistaken beliefs (a) that any Hamiltonian theory is mechanical and (b) that the fundamental theory must be a sort of mechanics--rather than, say, a field theory. And yet, the foundations of QM are often discussed in the light of (imaginary) experiments concerning the motion of 'particles' through slit systems...the reduction diagram of the quantum theories of matter, basic quantum mechanics QM and quantum-mechanical statistics QMS, are often said to look like this:



where 'CSM' and 'CM' stand for classical statistical mechanics and classical mechanics respectively. Unfortunately no one seems to have proved that such relations do obtain. To begin with, no rigorous proof of the reduction of CSM to CM is available. Nor is there any proof that QM does go over into CM. The only available proofs concern a few isolated statements, such as Ehrenfest's theorems and some formulas involving total quantum numbers. But this falls short of a systematic proof for the whole theory. More-

over, although AM is usually compared to classical particle mechanics...it seems obvious that it should rather be compared to continuum mechanics, both because of the occurrence of boundary conditons and because, in the relativistic quantum theories, stress tensors can be defined. Also, unlike the quantum field theories and unlike CM, QM presupposes and employs Maxwell's classical electromagnetic theory. Hence it could not possibly go into CM in any of the 'classical limits' discussed above, unless the further restriction to null fields were made--in which case the very existence of bodies could not be accounted for. Finally, it is possible to argue that QM is a limit of CM enriched with certain stochastic assumptions concerning e.g. a random force exerted on the system by the environment. In short, we know very little about the QM-CM relations. And it is a mistake to pretend that we understand them, for this prevents any serious investigation of the matter.

FOOTNOTE

Appendix D

¹Mario Bunge, "Problems Concerning Intertheory Relations," Induction, Physics, and Ethics. P. Weingartner and G. Zecha, eds. (Dordrecht-Holland: D. Reidel Publishing Company, 1970).

Selected Bibliography

- Achinstein, P., The Concepts of Science. Baltimore: The John Hopkins Press, 1969.
- Adams, E. ., "The Foundations of Rigid Body Mechanics and the Derivation of Its Laws From Those of Particle Mechanics," Axiomatic Method. Henkin, Suppes and Tarski (eds).
- American College Dictionary, New York: Random House, 1957.
- Apostel, L. "Toward the Formal Study of Models in the Formal and Non-Formal Sciences," The Concept of the Model.
- Baumrin, B. (ed) Philosophy of Science: The Delaware Seminar. Inter-Science Publishers, 1963.
- Bohr, N. "Discussions With Einstein," Einstein: Philosopher Scientist. P. A. Schilpp (ed).
- Borst, C. V. The Mind/Brain Identity Theory. London: Macmillan and Co., Ltd. 1970.
- Braithwaite, R. B. Scientific Explanation. London: Cambridge University Press, 1953.
- Brenner, C. An Elementary Textbook of Psychoanalysis. New York: Doubleday Anchor, 1955.
- Brody, B.A. Logic: Theoretical and Applied. Englewood Cliffs: Prentice Hall, 1973.
- Buck, R. and Cohen, R. (eds) Boston Studies in the Philosophy of Science. Dordrecht-Holland. D. Reidel Publishing Company, 1970. Volume VIII.

- Bunge, M. "Problems Concerning Inter-theory Relations," Introduction, Physics and Ethics, Weingartner and Zecca (eds)
- Butz, R. and Davis, J. (eds) The Methodological Heritage of Newton. Toronto: University of Toronto Press, 1966.
- Carnap, R. "Empiricism, Semantics and Ontology," Meaning and Necessity. Chicago: University of Chicago Press, 1969.
- _____. "Hilary Putnam on Degree of Confirmation and Inductive Logic," The Philosophy of Rudolf Carnap. Schilpp (ed).
- _____. "The Methodological Character of Theoretical Concepts," Minnesota Studies in the Philosophy of Science, Volume I. Feigl and Scriven (eds).
- _____. Introduction to Semantics. Cambridge: Harvard University Press, 1946.
- Chisholm, R. "Brentano on Descriptive Psychology and the Intentional," Introduction to the Philosophy of Mind, Morick (ed).
- Cohen, R. and Wartofsky M. (eds) Boston Studies in the Philosophy of Science. New York: Humanities Press, 1965. Volume II.
- Colodny, R. (ed), The Nature and Function of Scientific Theories. Pittsburgh: The University of Pittsburgh Press, 1969.
- _____. Beyond the Edge of Certainty. Englewood Cliffs: Prentice Hall, 1965.
- _____. Frontiers of Science and Philosophy. Pittsburgh University of Pittsburgh Press, 1962.
- The Concept and the Role of the Model in the Mathematical and Natural Sciences. Dordrecht- Holland: D. Reidel Publishing Co., 1961.

Crosson, F. and Sayre, K. (eds) Philosophy and Cybernetics.

Notre Dame: University of Notre Dame Press, 1967.

Davidson, D. "The Individuation of Events," Essays in Honor of C. G. Hempel, Rescher (ed).

_____ "The Logical Form of Action Sentences," Essays in Honor of C. G. Hempel. Rescher (ed).

_____ "Causal Relations," Journal of Philosophy, Volume 64, Number 21, 1967.

Dennett, D. Content and Consciousness. London: Routledge and Kegan Paul, 1969.

Eberle, R. "Replacing One Theory by Another Under Preservation of a Given Feature," Philosophy of Science, Volume 38, 1971.

Eberle, R., Kaplan, D. and Montague, R. "Hempel and Oppenheim on Explanation," Philosophy of Science, Volume 28, 1969.

Fancher, R. Psychoanalytic Psychology. New York: Norton and Company, 1973.

Feyerabend, P. "Against Method: Outline of an Anarchistic Theory of Knowledge," Minnesota Studies In the Philosophy of Science, Volume IV, Radner and Winokur (eds).

_____ "Consolation for the Specialist," Criticism and the Growth of Knowledge, Lakatos and Musgrave (eds).

_____ "Philosophy of Science: A Subject With a Great Past," Minnesota Studies in the Philosophy of Science, Volume V, Stuewer (ed).

_____ "On the Improvement of the Sciences and the Arts and the Possible Identity of the Two," Boston Studies in

the Philosophy of Science. Volume II, Cohen and Wartofsky (eds).

_____ "Problems of Empiricism, Part II," The Nature and Function of Scientific Theories. Colodny (ed).

_____ "Classical Empiricism," Methodological Heritage of Newton. Butz (ed).

_____ "Problems of Empiricism," Beyond the Edge of Certainty. Colodny (ed).

_____ "Reply to Criticism," Boston Studies in the Philosophy of Science. Volume II, Cohen and Wartofsky (eds).

_____ "How to be a Good Empiricist: A Plea for Tolerance in Matters Epistemological," Philosophy of Science: The Delaware Seminar. Baumpin (ed).

_____ "Explanation, Reduction and Empiricism," Minnesota Studies in the Philosophy of Science. Volume III, Feigl and Maxwell (eds).

_____ "Problems of Microphysics," Frontiers of Science and Philosophy. Colodny (ed).

_____ "Niels Bohr's Interpretation of the Quantum Theory," Current Issues in the Philosophy of Science, Feigl and Maxwell (eds).

_____ "Complementarity," Proceedings of the Aristotelian Society. Supplementary Volume 32, 1958.

Feigl, H. "The 'Mental' and the 'Physical'," Minnesota Studies in the Philosophy of Science. Volume II, Feigl, Scriven and Maxwell (eds).

Feigl, H. and Maxwell, C. Minnesota Studies in the Philosophy of Science. New York: Holt Rinehart and Winston, 1961.

Feigl, H. and Scriven, M. Minnesota Studies in the Philosophy of Science. Minneapolis, University of Minnesota Press, 1958. Volume I.

- Feigl, H., Scriven, M. and Maxwell G. Minnesota Studies in the Philosophy of Science. Minneapolis: University of Minnesota Press, 1958. Volume II.
- Follesdal, D. "Quantification Into Causal Contents," Boston Studies in the Philosophy of Science, Volume II, Cohen and Wartofsky (eds).
- Goodman, N. Fact Fiction and Forecast. Indianapolis: The Bobbs Merrill Company Inc., 1965.
- Glymour, C. "On Some Patterns of Reduction," Philosophy of Science. Volume 37, 1970.
- Hanson, N. Patterns of Discovery. Cambridge, Cambridge University Press, 1969.
- _____ "A Picture Theory of Theory Meaning," The Nature and Function of Scientific Theories . Colodny (ed).
- Held, R. and Richards, S. (eds) Perception: Mechanicisms and Models. San Francisco: Freeman and Co, 1972.
- Hempel, C.G. Aspects of Scientific Explanation. New York: MacMillan Inc., The Free Press, 1965.
- _____ "Studies in the Logic of Confirmation," Aspects of Scientific Explanation.
- _____ "Studies in the Logic of Explanation , " Aspects of Scientific Explanation.
- Henkin, L. Suppes, P. and Tarski, A. (eds) Axiomatic Method. Amsterdam: North Holland, 1959.
- Hesse, M. Models and Analogies in Science. Indiana: University of Notre Dame Press, 1961.
- Kemeny, P. and Oppenheim, R. "On Reduction," Readings in the Philosophy of Science, Brody (ed).
- Kieter, H. and Munitz, M. (eds) Mind Science and History. Albany: State University of New York, 1970.
- Kim, J. "Causation, Nomic Subsumption and the Concept of

- an Event," Journal of Philosophy. 70:8, 1973.
- _____ "Events and Their Descriptions," Essays in Honor
in C.G. Hempel. Rescher (ed).
- _____ "On the Logical Conditions of Deductive Explanations,"
Philosophy of Science, 30, 1963.
- Kleene, S. Introduction to Mathematical Logic. Princeton:
Van Nostrand and Co., 1950.
- Kline, P. Fact and Fantasy in Freudian Theory. London:
Methuen and Co., Ltd., 1972.
- Kohler, W. Gestalt Psychology. New York: New American
Library, 1947.
- Koslow, A. "The Law of Inertia: Some Remarks on its Struc-
ture and Significance," Philosophy Science and Methods.
Morgenbesser, Suppes and White (eds).
- Kuhn, T. "Reflections on My Critics," Criticism and the
Growth of Knowledge. Lakatos and Musgrave (eds).
- Lakatos, I. "Falsification and the Methodology of Research
Programmes," Criticism and the Growth of Knowledge,
Lakatos and Musgrave (eds).
- _____ "History of Science and its Rational Reconstruction,"
Boston Studies on the Philosophy of Science. Buch and
Cohen (eds).
- Lakatos, I. and Musgrave, A. Criticism and the Growth of
Knowledge. Cambridge: Oxford University Press, 1970.
- Lindzey, G. and Hall, C. Theories of Personality. New York:
John Wiley and Sons, 1957.
- Luce, R., Bush, R., and Galanter G. (eds) Readings in
Mathematical Psychology. New York: John Wiley and

- Sons, 1963. Volume I.
- Malcolm, N. Problems of Mind. New York: Harper Torch Books, 1971.
- Mates, B. Elementary Logic. New York: Oxford University Press, 1972.
- McCulloch, W. and Pitts, N. "A Logical Logical Calculus of the Ideas Imminant in Nervous Activity," Bulletin of Mathematical Biophysics. 5, 1943.
- Mendelson, E. Introduction to Mathematical Logic.
- Morgenbesser, S., Suppes, P. and White, M. (eds). Philosophy, Science and Method. New York: St. Martin's Press, 1970.
- Morick, H. Introduction to the Philosophy of Mind. Glenview, Illinois: Scott Freeman and Company, 1971.
- Mundle, C. Perception: Facts and Theories. London: Oxford University Press, 1971.
- Nagel E. The Structure of Science. New York: Harcourt Brace and World Inc., 1961.
- _____ "Issues in the Logic of Reductive Explanation," Mind Science and History. Kiefer and Munitz (eds).
- _____ "Theory and Observation," Observation and Theory. Nagel, Bromberger and Grunbaum (eds).
- _____ "The Meaning of Reduction in the Natural Sciences," Science and Civilization. Stauffner (ed)
- Nagel, E., Bromberger, S., Grunbaum, A. (eds) Observation and Theory in Science. Baltimore: The Johns Hopkins Press, 1969.

- O'Connor, J. Modern Materialism: Readings in Mind-Body Identity. New York: Harcourt Bruce and World, 1969.
- Omer, I.A. "On the D-N Model of Scientific Explanation," Philosophy of Science. Volume 37, 1970.
- Oppenheim, P. and Putnam, H. "Unity of Science as a Working Hypothesis," Minnesota Studies in the Philosophy of Science. Volume II. Feigl, Scriven and Maxwell (eds).
- Pap, A. "Disposition Concepts and Extensional Logic," Minnesota Studies in the Philosophy of Science. Volume II, Feigl, Scriven and Maxwell (eds).
- Pervin, L. Personality: Theory Assessment and Research. New York: John Wiley and Sons, 1970.
- Place, U. "Is Consciousness a Brain Process," The Mind/Brain Identity Theory. Borst (ed).
- Popper, K. Conjectures and Refutations. New York: Harper Torchbooks, 1963.
- Quine, W.V. Methods of Logic. New York: Holt Rinehart and Winston, 1970. 3rd Edition
- _____. Philosophy of Logic. New Jersey: Prentice Hall, 1970.
- _____. Ways of Paradox. New York: Random House, 1966.
- _____. Selected Logic Papers. New York: Random House, 1966.
- _____. "Ontological Reduction and the World of Numbers," Ways of Paradox.
- _____. "Interpretation of Sets of Conditions," Selected Logic Papers.
- _____. From a Logical Point of View. New York: Harper Torchbooks, 1953.

- Radner, M. and Winokur, S. (eds) Minnesota Studies in the Philosophy of Science. Minneapolis: University of Minnesota Press, 1970. Volume IV.
- Rescher, N. Hypothetical Reasoning Amsterdam: North Holland Publishing Company, 1970.
- _____Essays in Honor of C.G. Hempel. New York: Humanities Press, 1970.
- _____"Lawfulness as Mind Dependent," Essays in Honor of C.G. Hempel.
- _____Topics in Philosophical Logic. Holland, D. Riedel Publishing Company, 1968.
- _____"The Logic of Decision and Action. Pittsburgh: University of Pittsburgh Press, 1962.
- Robinson, A. Introduction to Model Theory and the Meta-Mathematics of Algebra. North Holland, 1965.
- Rozeboom, W. "The Crisis of Philosophical Semantics," Minnesota Studies In the Philosophy of Science. Volume IV, Radner and Winokur (eds).
- Ryle, G. The Concept of Mind. London: The Hutchinson Publishing Company, 1949.
- Schilpp, P. The Philosophy of Rudolf Carnap. LaSalle, Illinois: Open Court, 1963.
- _____Einstein: Philosopher Scientist. Evanston, Illinois: Library of Living Philosophy, 1948.
- Sellars, W. Science, Perception and Reality. London: Routledge and Kegan Paul, 1963.
- _____"The Language of Theories," Current Issues In the Philosophy of Science, Feigl and Maxwell (eds)

- Shaffer, J. Philosophy of Mind. Englewood Cliffs:
Prentice Hall, 1968.
- Shaffner, K. "Approaches to Reduction," Philosophy of Science, Volume 34, 1967.
- Shimony, A. "Scientific Inference," The Structure and Function of Scientific Theories, Colodny (ed).
- Sklar, L. "Approaches to Reduction," Philosophy of Science Volume 18, 1967.
- Smart, J.J.C. "Sensations and Brain Processes," The Mind/Brain Identity Theory. Borst (ed).
- Stauffner, R. (ed) Science and Civilization. Madison: University of Wisconsin Press, 1947.
- Strawson, P.F. Introduction to Logical Theory. London: Methuen, 1952.
- _____ "If and $\supset$ " Offprint α 19. Cambridge: University of Cambridge Press.
- Stuewer, R. (ed) Minnesota Studies in the Philosophy of Science. Minneapolis: University of Minnesota Press, 1970. Volume V.
- Suppes, P. Studies in Methodology and Foundations of Science. New York: Humanities Press, 1969.
- _____ "A Comparison of the Meaning and Uses of Models in Mathematic 's and the Empirical Sciences," Studies in Methodology.
- _____ "A Set of Independent Axiom's for Extensive Quantities," Studies in Methodology.
- _____ "Measurement Empirical Meaningfullness and Three Valued Logic," Studies in Methodology.

_____ "Models of Data," Studies in Methodology.

_____ Introduction to Logic. Princeton: D. Van Nostrand
Company, 1957.

Suppes, P. and Scott, D. "Foundational Aspects of Theories
of Measurement," Studies in Methodology.

Tarski, A. in collaboration with Mostowski, A. and Robinson,
R. Undecidable Theories. Amsterdam: North Holland, 1953.

Thomason, R. Symbolic Logic. New York: MacMillan and Co.,
1970.

Thomson, J. "The Identity Theory," Philosophy, Science and
Method.

Van Fraassen, B. Formal Semantics and Logic. New York:
MacMillan Co., 1971.

Weingartner, P. and Zecha, G. (eds) Induction, Physics and
Ethics. Dordrecht-Holland: D. Reidel Publishing Co.,
1970.