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ABSTRACT

This report attempts to determine to what extent the thesis that observation is theory-dependent holds in the area of verbal learning theory. This area was chosen because: (1) a philosophical criticism of verbal learning theory will contribute to the difficult task of investigating the border area between philosophy and psychology; and (2) no one theory has yet emerged as pre-eminent so that the difficulties of handling competing theories are not existent. While no "theory" has yet been produced three general positions have emerged: gestaltism, functional associationism, and transformational linguistics. Discussed are: (1) some of the grounds for suspecting that the theory-dependency thesis holds for verbal learning theory; (2) that theoretical terms must be ultimately definable in terms of operations describable in some theory-neutral observation language; (3) some of the criticisms of operationalism to the more general question of whether there can be a theory-neutral observation language at all; and (4) details of the controversy between Skinner for the associationists and Chomsky for the transformational linguists. An appendix includes a paper which explains the philosophical underpinnings of the theory-dependency thesis as presented by Wittgenstein. (Author/CJ)

THE LOGICAL EFFECTS OF THEORY ON OBSERVATIONAL CATEGORIES AND METHODOLOGY IN VERBAL LEARNING THEORY

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Observational Categories in Verbal Learning Theory

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June 20, 1969

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I. INTRODUCTION

Ever since scientific research was placed on a firm empirical footing by the work of Bacon, Galileo, Newton, Mill and others, it has been generally assumed that the laws and theories of any science must arise directly out of observation and experimentation. Based on this assumption a great deal of work in philosophy and methodology of science has gone into making explicit what might well be called the "logic" of scientific discovery. This work reached its zenith in logical positivism where an attempt was made once and for all to base the rules of discovery and theoretical meaningfulness on a scientifically neutral observation language. It was assumed that an unproblematic observation language did exist, i.e., a language which was not itself theoretically determined but was agreed on by all, and which could be used to state the observable facts once and for all. The problem was then conceived as how to relate this neutral language to the highly abstract theoretical language, thereby showing the relation of theory to experience and the precise logical role of experience, formulated in the observation language, in providing an empirical content for the theories.

The great clarity which the positivists were able to achieve in this attempt has led to a recognition of the prob-

lems and inadequacies of this approach. Recently a number of philosophers and scientists have come to abandon the earlier model of empirical science as arising solely from careful observation and collection of data. These men, led by such as Thomas Kuhn, Willard Van Orman Quine and N. R. Hanson, have begun to argue that scientific theories are radically underdetermined by experience and that although scientific theories must have empirical content, in that they must be testable by experience, they do not and cannot arise solely out of experience. It has been argued that what even counts as relevant data is essentially theory dependent. That is, two scientists may look at the "same" thing and, because of different theoretical perspectives, may literally not <u>see</u> the same object. What is relevant data for one theory may be totally ignored by another theory and may not even be capable of being observed.

Such a conception of science is both radical and disturbing. If it is true, it raises some profound foundational and methodological questions. It deserves to be investigated in all its ramifications and implications. One of the possible implications of such a conception of science is the extent to which the methodology of science is affected by the inability even to see certain data from certain theoretical perspectives. On the one hand, it might be argued that the essential limitation of a general theoretical perspective would, of course, be manifested in differing methodologies

and experimental results, which, because of the differing perspectives, could not be said to contradict each other but rather to talk "through" each other. On the other hand, it could also be plausibly maintained that although such a relativity of theoretical perspective and hence relativity of observation may be important on a very high level of theory construction and evaluation, nevertheless on the levels of methodology or of experimentation such a relativity is not present or can be safely ignored for practical purposes.

This report attempts to explore some of these implications and to determine to what extent the thesis that observation is theory-dependent holds in a particular area of scientific inquiry. The case chosen for examination here is verbal learning theory at its present state of development.

Why verbal learning theory? The reasons are two: First, a philosophical criticism of verbal learning theory will, if nothing else, contribute to the lengthy and difficult task of investigating the border area between philosophy and psychology. Significant philosophical questions have been raised, notably by Wittgenstein, which point to an intimate connection betweeen the fundamental structure of language and how we acquire our ability to use language, but the nature of this connection remains problematic. Verbal learning theory as an empirical inquiry may shed some light on this philosophical issue.

On the other hand, an inquiry into verbal learning theory from a philosophical point of view may reveal conceptual problems which may in turn lead to resolution of some of the theoretical difficulties that plague the field. Thus both philosophy and psychology stand to gain from such an inquiry.

The second reason is more directly relevant to the enterprise of confirming or disconfirming the theory-dependency thesis. Heretofore, discussion of the thesis has been based primarily on examples of competing theories drawn from the history of science; e.g., the Ptolemaic system of astronomy vs. the Copernican, the phlogiston vs. the kinetic theories of heat, Newtonian vs. relativistic mechanics, etc. While such examples may be highly suggestive, the danger is present (especially if the theory-dependency thesis is true) that our current set of scientific theories and our interpretations of the history of science may distort our conclusions as to what the influences were of an outdated theory on those who held it. We do not today accept the phlogiston theory of heat and hence have difficulty determining how heat phenomena were seen by adherents of that theory. Our historical perspective prevents, or at least makes extremely difficult, any attempt to "see" from a discarded point of view. Such difficulties can be avoided, however, by considering a current issue in science, where no one theory has

yet emerged as pre-eminent. The investigator must, of course, not yet have committed himself to one of the competing theories.

Verbal learning theory in its present state of development is extremely well-suited to this type of investigation. A substantial amount of experimental work has been done in this field; and although nothing that has been dignified with the name of "theory" has as yet been produced, three general positions have emerged with regard to learning theory: gestaltism, functional associationism, and transformational linguistics. These positions can be examined with respect to their various observational categories and with respect to their polemical parts vis à vis each other. The results should show what influences there may be on observation and methodology due to the differences in basic point of view within the field.

Chapter II of this report, "Why Has Learning Theory Failed to Teach Us How to Learn?", sketches some of the grounds for suspecting that the theory-dependency thesis does in fact hold for verbal learning theory.

In Chapter III, "A Dogma of Operationalism", I have taken a critical look at the operationalist thesis, that theoretical terms must be ultimately definable in terms of operations describable in some theory-neutral observation language. This thesis is central to the positivistic view of science, and in particular to the associationist position regarding

verbal learning. As will be seen, there are many difficulties with operationalism, one of which is that it spawns a theory-dependent methodology in a way quite contrary to the wishes of its advocates.

The following chapter, "Seeing and Seeing As", carries through some of the criticisms of operationalism to the more general question of whether there can be a theory-neutral observation language at all. Here I outline the philosophical grounds, based primarily on Wittgenstein's discussion of this problem in Part II of his Philosophical Investigations, for holding the theory-dependency thesis.

Finally in Chapter V, "Chomsky and Skinner: A Partial Case Study", I have taken up in some detail the controversy currently raging between the prime advocates of two of the major positions in learning theory: Skinner for the associationists and Chomsky for the transformational linguists. There is evidence here, I believe, for the claim that their positions do indeed influence their methodologies, observations and polemics.

I have added as an appendix the paper "Science and Metaphysics: A Wittgensteinian Interpretation" which explains in greater detail the philosophical underpinnings of the theory-dependency thesis as they were presented by Wittgenstein.

II. WHY HAS LEARNING THEORY FAILED TO TEACH US HOW TO LEARN?*

Why, despite the almost universally held belief that psychology and especially learning theory are the foundation sciences of education, have these "foundations" given such minimal support and assistance to actual day to day educational practice? The answer which I propose to this tired old question is that, paradoxical as it may sound, learning theorists in psychology and practical educators are, for the most part, talking about two entirely different things.

I think it is abundantly evident that psychology, with the possible exception of psychometrics, has contributed little, if anything, to education. At any rate it is clear that learning theory, at once hailed as the best developed of the fields of psychology and at the same time the one field from which the most could reasonably be expected for educational purposes, has contributed next to nothing. For even Ernest Hilgard, one of the most respected learning theorists, and one who is interested in the problems of relating basic research in psychology to educational practice, clearly recognizes the paucity of contribution learning theory has made. In both the 1964 NSSE yearbook,[1] of which he is the editor, and in the third edition of his own

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[1] E. R. Hilgard (editor), Theories of Learning and Instruction, Yearbook LXIII, (Chicago: National Society for The Study of Education, 1964).

* This chapter has been published in the 1968 Proceedings of the Philosophy of Education Society.

widely read book on learning theory,[2] Hilgard concludes with an apologetic for the seeming irrelevance of learning theory to education.

It will be instructive to see the kind of reasons Hilgard advances for this lack of relation in order better to compare them with the answer I am proposing. His reasons for the lack of relation are essentially two. On the one hand is the general problem of development and application of theory which is common to all applied disciplines. On the other hand Hilgard believes that educators have generally not adequately specified the tasks and the criteria of success for these tasks for basic theory to be of much use. And, of course, these two answers are quite commonly accepted by psychologists and educators alike.

Without denying the importance of these factors, what I wish to do is to point out that these problems of development and application and task analysis logically presuppose that the facts of learning are the same for the different learning theorists and for the educator. As Hilgard says, "all the theorists accept all of the facts"[3]. That such a presupposition is indeed present is easy to see. We could scarcely begin to concern ourselves with development and application of theoretical results to concrete situations unless the facts of the concrete situations are of the same

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[2] E. R. Hilgard and G. H. Bower, Theories of Learning, 3rd edition, (New York: Appleton-Century-Crofts, 1966).

[3] Ibid., p. 9.

nature as the facts of laboratory-based theory. Nor would a more precise specification of tasks help in applying theory to practice unless the object domain of the task is the same as that of the theory.

For that matter, the supposition that all the theorists accept all the facts is not a very surprising one. It is a fairly common piece of scientific folklore and just a simple restatement of the generally accepted principle that we can always draw a sharp and clear distinction between an observation language which reports the facts of our environment and a theoretical language which interprets those facts. Thus the presupposition is that there is a neutral data language upon which all agree and differing theoretical languages to interpret the data and over which there can be disagreement.[4]

And yet, there has recently arisen a serious challenge, offered by such men as N. R. Hanson,[5] W. V. O. Quine,[6] Stephen Toulmin,[7] and, perhaps best known of all, T. S. Kuhn[8] to such "obvious" presuppositions. These men have

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[4] Ibid., p. 9.

[5] N. R. Hanson, Patterns of Discovery (London: Cambridge University Press, 1958).

[6] W. V. O. Quine, Word and Object (New York: John Wiley and Sons, 1960).

[7] Stephen Toulmin, Foresight and Understanding (New York: Harper Torch Books, 1961).

[8] T. S. Kuhn, The Structure of Scientific Revolutions (Chicago: The University of Chicago Press, 1962).

begun to argue that scientific theories are radically underdetermined by experience, and that although scientific theories must have empirical content -- be testable by experience -- they do not and cannot arise solely out of experience. It has been argued that what even counts as experience is essentially theory dependent. That is, two scientists may look at the "same" thing and, because of different theoretical perspectives, may literally not see the same object. What is relevant for one theory may be totally ignored by another theory and may be logically incapable of being observed.

It should be emphasized at this point what a truly radical conception this is. It might easily be supposed that all that is being claimed here is that any science in fact focuses on certain features of experience to describe and ignores others. For example, classical physics, it has often been said, owes much of its success to having concerned itself with just the right physical properties, position and momentum, ignoring color, taste, etc. If this is the sort of thing being claimed, then why all the fuss.

But this would be to miss the point entirely. For the "focussing" conception of science indicated above logically presupposes a kind of neutral experiential base upon which one may focus, now here, now there. Correlatively, a neutral observation language is also presupposed within which one could in principle describe all the physical properties of

situations and events, leaving it to the scientific theory to pick out those features which are to be covered by the theory. The non-favored features are still "there"; they are simply not deemed relevant.

However, it is the position of the view under consideration that no such neutral observation language exists nor can experience be described independently of theory. A radical view indeed.

Psychologists are not unaware of the problems of being constrained in their observations by the use of certain favored approaches and methodologies. For example, Underwood[9] has noted the unimaginativeness of many verbal learning experiments which seem often to return to the basic techniques of paired-associate experiments. However, most psychologists tend to treat such problems of constraint as problems in the psychology of methodology, assuming that with proper care and imagination they can be overcome. Without in the least attempting to minimize the psychological part of this problem, I want to be as clear as possible in suggesting that there may well be a logical and conceptual problem as well. In other words it may be the case that all the care and imagination in the world may be unable to help an experimenter see a certain result if such results are not countenanced by the theory he explicitly or implicitly espouses.

If such a theory-dependency thesis of observation is indeed true, then it can easily be seen, at least in outline,

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9 B. J. Underwood, "The Representativeness of Rote Verbal Learning" in A. W. Melton (editor), Categories of Human Learning (New York: Academic Press, 1964).

how this might give weight to my content on that the major reason learning theory has been of such little help to education is that learning theorists and educators are generally talking about two different things. For most learning theorists, given the general pervasiveness of at least a methodological behaviorism, will see more or less mechanical stimuli and responses; whereas, most educators, given the teleological concepts of ordinary language, see goals and actions as purposive. Such a conception immediately shows the extent to which Hilgard was correct in asserting that a better task analysis is often a good way of bridging the gap between theory and practice. For if the task description can be given an S-R twist it would be easier to make the application. On the other hand, if the general results of learning theory are cast in teleological form, the application would again be easier.

Let me then pursue the theory-dependency thesis a bit further. An extreme form of the thesis would present us with a most radical kind of Whorfianism. For if each of us sees only what the theory we have enables us to see, and it is furthermore granted that everyone's conceptual scheme differs at least slightly from everyone else's, and finally, that our conceptual schemes are, in some sense, our theories of the world, then no one ever sees precisely what anyone else sees and a rigorous notion of inter-subjective confirmation or justification of some one theory is logically out of the question. Such an extreme view often seems to be implied

by some of the things Kuhn says.

I do not think that such an extreme view is correct. For one thing it faces all the difficulties which any radical skepticism faces along with some of its own which I shall briefly mention. First of all, if this kind of theory-dependency thesis is even intelligible at all it will be intelligible <u>on its own grounds</u> only in terms of some theory which determines observational categories sufficient for us to see the intelligibility of the theory-dependency thesis. It seems obvious that such an all-embracing meta-theory is nothing more nor less than philosophy and thus that philosophical argumentation is appropriate to the theory-dependency thesis. For if the thesis actually asserts that it itself is outside the realm of any justification, even a philosophical justification, then quite clearly we can have no justification for accepting it, and yet equally clearly the thesis is capable of being argued about.

Second, even if we were to grant the extreme Whorfian version of the theory as a metaphyscial possibility, we could not on epistemological grounds ever assert or deny this possibility. For as Quine has so adequately pointed out[10] there is no way of deciding on the basis of the empirical evidence between someone's looking at the world radically differently and a mistake in translation. To make sense of the differences in conceptualization we do find, we must assume a tremendously large core of common conceptualization as a background.

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[10] <u>Op. cit.</u>

Having concluded this much, however, we are still left with a reasonably strong version of the thesis. And this version states that there may be logically incompatible observational categories which are, nevertheless, philosophically basic and hence incapable of being decided between on empirical grounds, although philosophical argumentation would be appropriate. There is also a weaker thesis which states that within a single philosophically basic observational category, it is possible to have differing empirical specifications of what falls under that category.

What I would now like to do is to illustrate both the strong and the weak theses with reference to some of the changes which have occurred in the definition of a stimulus as learning theorists have moved from conditioning theory to discrimination learning to conceptual behavior.

Historically, hard-line behaviorists began by taking the definition of a stimulus to be in terms of physical events of some sort or other impinging directly on the organism, e.g. light waves hitting photo-receptors, auditory nerves being stimulated, and what have you. And indeed such a definition works well for typical conditioning experiments where it is fairly easy to determine what change in the carefully controlled laboratory environment will count as a stimulus, and also fairly easy to generalize on the stimulus.

However, once one enters the field of discrimination learning, the subject must not only be conditioned to some

stimulus, he must also learn in some manner what is to count as a stimulus. This involves problems of attention, focussing, stimulus patterning, and stimulus generalization which do not seem to occur at all in classical conditioning experiments. This is not the time to enter into a detailed discussion of the experimental results of discrimination learning. Nor will I discuss whether or not these results can be accomodated within classical conditioning theory by means of some sort of selection and retention of repeated total stimuli defined in physical terms.[11] It will be sufficient for my purposes to note that discrimination learning results have prompted many psychologists to retreat from the kind of hard-line definitional behaviorism exemplified, for example, by Hull to a methodological behaviorism. A "methodological behaviorism," as I shall use the term, allows the introduction of any number of "mentalistic" intermediaries, or representations, or cues, as long as the introduction of such cues can be shown to have genuine explanatory power within the theory and as long as there is some observational test of such cues, no matter how indirect. Even Skinner verbally subscribes only to a methodological behaviorism, although he combines this with a further belief that on his system very few, if any, such mentalistic cues need to be introduced.

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[11] However see, Charles Taylor, The Explanation of Behavior (London: Routledge and Kegan Paul, 1964) for a sustained attack on the possibility that a simple extension of classical conditioning principles can account for the results of discrimination learning.

When one moves to the area of concept formation, the problems become even more acute. In discrimination learning single stimuli need to be discriminated one from another, whereas in concept formation whole classes of stiumli need to be discriminated from other classes. To see the problems involved in attempting to carry over the definition of a stimulus in physical terms as specified in conditioning theory to the physical definition of the class of stimuli which call forth a given concept one need only reflect on the incredibly wide physical dissimilarities involved in all the physical objects which fall under the concept of a chair. The possibility of remaining within the bounds of a physical definition of the stimuli seems remote indeed.

As a result, more and more psychologists have tended to introject into the organism larger and larger parts of the environment to which the organism is supposed to be responding in discrimination and concept learning. And this is, of course, to come closer to the position which many philosophers and gestalt psychologists have long urged; namely, that an organism responds to what it <u>believes</u> the environment to be and not to what the environment actually is.

And yet, as has been pointed out by Kendler,[12] this whole process of a change in the definition of a stimulus from conditioning to discrimination to concept formation can

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[12] H. H. Kendler, "Concept of the Concept", in A. W. Melton (editor), <u>Categories of Human Learning</u> (New York: Academic Press, 1964).

still be considered to fall under a theoretical stimulus-response associationism. Thus despite the change in definition of the stimulus (and usually corresponding changes in the definition of a response), we still have the notion that any behavioral event can be analyzed in terms of an environmental feature (stimulus), some components of total behavior (response), and the association between the two.

In the sense, then, in which human behavior is considered analyzable in an S-R kind of way, we have an illustration of the weak sense of the theory-dependency thesis. For it will be recalled that the weak version of this thesis claimed that there might be differences in empirical specification of a single philosophically basic observational category. Thus we have the philosophical category of an S-R analysis of human behavior and differing empirical specifications of this observational category ranging from physical definitions to cues internal to the organism. If the basic philosophical category is indeed of the S-R variety, then the criteria for deciding on the empirical specification of this category in different situations are, broadly speaking, empirical in nature. That is, we must await the results of the psychologists' investigations to tell us which ones are correct.

Nevertheless, it is still easy to see how, even under the weak version of the theory-dependency thesis, it might be difficult to translate the results of learning theory into educational practice. For it seems obvious enough that the

practicing educator observes the educational process largely in terms which define the stimulus as internal cues; whereas the most reliable, if limited, results in learning theory come from seeing stimuli in terms of physical events -- two widely different conceptions.

But now what if the basic philosophical category of a stimulus-response analysis of human behavior is wholly rejected? That is, what happens if the notion of a human action is actually unanalyzable in such terms and is either itself a basic philosophical observational category or at least cannot be analyzed in the causal terms of the S-R conception? Charles Taylor[13] has recently argued the latter while Richard Taylor[14] has argued the former. That is, both have argued on philosophical grounds that human action is essentially teleological in character in such a strong sense that the S-R conception sketched above is wholly inapplicable. What we now have is an illustration of the strong version of the theory-dependency thesis. For the claim by the two Taylors is that no matter how stimuli are defined they cannot, logically cannot, be used as an observational category for human action. And the reason is that human action belongs to a philosophical category different than that embodied in an S-R conception. Note, too, that the criteria for deciding between an S-R conception and a broadly teleological conception of human action are philosophical in character and hence must be decided on

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13 Op. cit.

14 Richard Taylor, Action and Purpose (Englewood Cliffs, New Jersey: Prentice-Hall, 1966).

philosophical grounds.

Without deciding if ordinary language analyses actually yield the metaphysical results for them, one can grant that the analyses of our ordinary use of action terminology are indeed teleological as claimed by the two Taylors. But if this is granted, and it is further granted that practicing educators largely make use of ordinary language in describing the educational process, then it will follow that the theory embodied in ordinary language renders it logically impossible to observe human action in the educational process in the categories in which learning theorists state their results. And hence it is logically impossible, as long as ordinary terms are used as the basic philosophical category for the observation of human action, that learning theory as presently constituted could be of any relevance to education. For the basic philosophical categories of the two ways of looking at the world are incompatible and it will require a philosophical argument to settle the issue between them.

In conclusion let me make just a few comments on this analysis. First, the framework I have offered gives prima facie promise of providing an explanation of how it is that learning theory has contributed what it has. Under my view one ought to be able to predict that principles of conditioning theory are most applicable in areas where our ordinary language concepts are not teleological, and least successful where such ordinary concepts are teleological; indeed, a

glance at Hilgard's summary of just these items reveals a *prima facie* confirmation.[15]

Second, my own opinion is that the two Taylors are wrong in asserting that the teleological character of human action is such as to render it inexplicable in an extended S-R framework. This is essentially the philosophical controversy over whether reasons or intentions or motives can be causes. Such a complex issue cannot be entered into now. However, as I have urged, the solution to this question must necessarily be a philosophical one.

Third, given this framework, the isomorphism which has been noted by Suppes and Atkinson[16] between the recent mathematical S-R learning theories and certain cognitive theories is easily understood. The formal isomorphism could be proved because both fell within the broad formal framework of an S-R conception of human action although they may have differed in empirical specification of stimulus and response. A cognitive theory falling under a different basic philosophical conception could probably not be proven isomorphic.

Fourth, I have not argued directly for the theory-dependency thesis, but have rather assumed it to be in broad outline correct. It has seemed to me that such a view has been ably argued by others and has not been conclusively refuted. Thus, I be-

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15 *Op. cit.*, p. 562-564.

16 P. Suppes and R. C. Atkinson, *Markov Learning Models for Multiperson Interactions* (Stanford University Press, 1960).

lieve it deserves to have some of its implications traced out in detail, and I consider the framework it provides for understanding the problems I have sketched in this paper to be a kind of indirect argument for the theory-dependency thesis.

Finally, despite the sweeping topics I have considered and the sketchy treatment I have offered of them I believe I have made it at least plausible that there may be philosophical reasons for the seeming irrelevance of learning theory to education. I hope I have also been able to indicate the vast amount of work which remains to be done by philosophers of psychology and philosophers of education in this area.

III. A DOGMA OF OPERATIONALISM

By operationalism, I mean that methodological precept which states that every term introduced into a scientific context must have a definite testing operation associated with it as its criterion of application. Interpreted in a certain way, this requirement is a relatively unobjectionable way of stating one of the most important necessary conditions of any adequate empirical theory, viz. that the theory be testable by appeal to experience. It is a long-standing truism of the philosophy of science that any adequate scientific theory be capable of being disconfirmed by experience. For without this requirement there seems to be no way in which a theory could explain empirical phenomena in the sense in which a theory gives reasons for expecting this phenomena rather than that.

The testing criteria for the application of a scientific term are usually referred to as "operational definitions," although, as will be seen, when actually used as definitions these criteria have some rather surprising consequences. In particular suppose the logical form of operational definitions is

$$(1) \quad Tx \equiv (Op(x) \supset R(x))$$

where this reads: x has the theoretical property, T, if and only if performing operation, Op, implies result, R, follows. By logical considerations alone, the theoretical term applies

whenever the testing operation is not being performed. Furthermore, even if it were possible to rule out such a result by some sort of general stipulation, it would remain the case that the theoretical term applies only when the testing operation is actually being carried out. We are thereby barred from postulating underlying theoretical entities which cause the operations to give the results they do (a desirable prohibition in the minds of many committed to operational definitions). We are also barred from attributing the theoretical term in those cases in which we would like to say that if we had carried out the test operation, (although we did not), we would have observed the results in the definition. Such prohibitions are unacceptable in most standard cases.

For reasons such as these, the form usually taken as exemplifying operational definitions is rather one of the following.

(2) $Op(x) \supset (Tx \equiv R(x))$

(3) $Op(x) \supset (R(x) \supset Tx)$

Such a form makes explicit the dependence of the application of the theoretical terms on the context provided by the performance of the operation. Unfortunately, this also renders the theoretical terms ineliminable, thus violating one of the intuitive criteria of adequacy for a definition.[1] Further-

[1] See Benson Mates, Elementary Logic (New York, Oxford University Press, 1965) pp. 187-193.

more, it usually (depending on the rest of the theory) violates the other criteria for definitions, viz. that they be non-creative. A definition is creative if it is possible with the aid of the definition to prove within the theory a formula which does not contain the defined term and which is not provable without the definition. Intuitively, a creative "definition" adds more structure to a theory and as such is somewhat misleadingly called a "definition" at all.

But although these considerations of operational definitions are somewhat embarrassing linguistically, they do not in any way count against the general thrust of the program of operationalism. However, there is worse to come. It has long been recognized that a strict adherence to the operationalist maxim is not possible. The stricture to operationally define <u>every</u> term leads to an infinite regress. We must simply assume that somewhere along the line, some terms are clear enough not to need defining. The examples I gave

$$Op(x) \text{ and } R(x)$$

serve this role. They are themselves assumed to be clear enough not to need defining.

However, even granted an arbitrary stopping point, there remain serious problems. Suppose, for example, that one gives an operational definition of temperature using the notion of a mercury thermometer as the test operation. Suppose further that another operational definition of temperature

is given using an alcohol thermometer. Under strict operationalist principles we would have to say that we had here two separate concepts, mercury temperature and alcohol temperature, and that any consistency of results in their overlapping ranges of application would be due to an empirical law connecting the two concepts of temperature. In short we would not have one temperature concept with two means of measuring it. Rather we would have two concepts which happen to be empirically connected. To translate into terms more familiar to social scientists, we would have to show empirically the convergent validity of the two temperature concepts rather than assume we had distinguished two overlapping measures of the same trait.[2] This way of looking at the matter does not seem at all plausible to me in the case of the temperature concepts and yet it is precisely the counsel given us by a strict adherence to operationalism.

But perhaps even this can be swallowed by operationalism. Perhaps the overlap between alcohol temperature and mercury temperature just is an empirical fact. But now what about the laws linking electrical resistance to temperature which allow the construction of resistance thermometers? Is this another empirical correlation? If so, how was its discovery ever motivated? What about gas thermometers which rely on the

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2 See, for example, D. T. Campbell and D. W. Fiske, "Convergent and Discriminant Validation by the Multitrait-Multimethod Matrix," Psychological Bulletin, 56, March, 1959, 81-105, and L. J. Cronbach and P. E. Meehl, "Construct Validity in Psychological Tests," Minnesota Studies in the Philosophy of Science, Vol. I, (Minneapolis, University of Minnesota, 1956) for further discussion of types of validity.

volume variation of a gas at constant pressure with temperature variation? Surely the natural explanation is that we have one concept which is linked by different laws to different operational applications, some of which might have been wholly unanticipated had we not treated the situation as involving just one concept to be captured. In short, a strict adherence to operationalism seems to lead to a proliferation of terms -- alcohol temperature, mercury temperature, resistance temperature, and gas temperature -- and a proliferation of empirical laws connecting them. I cannot think of anything better designed to inhibit and render impossible the simplifying and systematizing effect that theory is meant to produce. In short, operationalism tends to be anti-theoretical.

There is yet another indication of this anti-theoretical bias. Under strict operationalist principles, there is little possibility of changing or modifying operational criteria for some of the concepts in a theory. Consider, for example, the standard meter. If the operational definition of visual length makes use of some operation of comparison with the standard meter and further this operation defines length, then it becomes well-nigh impossible to see how one might ever come to see that the length of the bar might vary with temperature or the stress placed on it. We might never come to believe that to be exact we should modify the operational

definition to include a standard temperature and a standard method of support. Under strict operationalism it would be the most extreme kind of accident if anyone were ever to hit upon the possibility that the reliability of using the standard meter could be vitiated by the different temperatures on different occasions of its application. Remember, under strict operationalism, length just is the testing operation with the standard meter. There is no other concept of length which could be used as a standard against which to measure possible mistakes in the application of the operational definition. In such a situation the temperature dependence could only be explained by an indefinite number of laws relating an indefinite number of distinct length-at-a-temperature operational concepts. Finally, the likelihood that such an experimental situation could be discovered seems exceedingly remote.

Contrast this case with the picture one gets if one treats length as a unitary concept with varying operational definitions of its application. We then conceptualize length as a property underlying the operations used to ascertain it. We may, noting the dependence of volume of gases on temperature, believe some similar dimensional change might occur for solids. We could check to see if the underlying property of length is temperature dependent. We can understand, using other operational definitions of length, how we could find the standard definition to be in error or dependent on certain

facts. Surely this latter picture is the one we usually associate with science rather than the former.

And yet, if we are to believe what some operationalists tell us, they would have us actively pursue the former policy with the only control being to run constant correlation checks and factor analyses to try to keep the concepts from so proliferating.[3] That this mechanical method is not really sufficient is illustrated by a priority principle for the admission of concepts given by the same operationalist.

> "If the same response measure is used in the defining operations of two phenomena, and if the stimulus manipulations cannot be clearly differentiated, the phenomenon which can be demonstrated (hence defined) in a situation where by its literary conception the other would not occur, the first phenomenon takes precedent."[4]

But what is the "literary conception" other than a mini-theory surrounding the postulation of an underlying process, which postulation accounts for varying manifestations (operational definitions) of the occurrence of the process.

Let me briefly mention a series of psychological examples similar to the temperature effect on length. Rosenthal's massive studies showing the effect of experimenter expectation are most noteworthy in this regard.[5] Crudely the situation is this: One establishes the reliability of a certain operational definition, e.g. under a certain treatment, a certain response is reliably elicited.

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[3] B. J. Underwood, Psychological Research (New York, Appleton-Century Crofts, 1957), Ch. III.

[4] Ibid., p. 78.

[5] R. Rosenthal, Experimenter Effects in Behavioral Research (New York, Appleton-Century-Crofts, 1966).

Under a strict operationalism, if the treatment is applied, then the response is the phenomenon studied (Cf. Under a comparison with a standard meter, the length is the proportion of standard meter covered.) To show that the response (length) might be an effect of something else besides the actually specified treatment (measuring), one would have to suspect that a similar response might be obtained from or partially due to experimenter expectation (temperature variation). Note that one cannot say in all literality that the response to experimenter expectation and the response to the treatment are the same, for the operations are different, and different operations define different concepts. Even using 'similar' is at least misleading, for the responses could only be similar with respect to some method of identification which does not figure in the operational definition. For by the very concept of an operational definition, if the test is performed, then if the results obtain, that is the theoretical term or "the theoretical term is defined." Thus the relation, improbably discoverable, between the treatment-defined term and the experimenter expectation-defined term is a contingent empirical law. (Cf. the relation between length-at-temperature-t_1 and length-at-temperature-t_2 is an experimental law. We cannot say there is a relation between length simpliciter and temperature simpliciter.)

But contrast such an extremely artificial situation with one that says there is a single theoretical concept (length) being defined under the two treatments such that possibly both aspects of the operation, treatment and experimenter expectation (Cf.: both the proportion covered and temperature) affect the theoretical term (length). (I recognize the disanalogy created by the fact that for many treatments, experimenter expectation may serve as the sole causally operative factor whereas temperature merely affects rather slightly the proportion of standard meter covered. I think this disanalogy can be safely ignored for my purposes.)

But all of the foregoing is not new. It is explicit or implicit in much of the literature critical of operational definitions.[6] And while I think it serves to render a strict adherence to operationalism most implausible as a description of what scientists do or as a methodological precept for what they ought to do, nevertheless these arguments have not shown any fundamental incoherence in operationalism. Underlying processes or properties with varying manifestations corresponding to varying operationalizations can logically be denied (however implausible the denial may be). The phenomena

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[6] For example, see Carl Hempel, Philosophy of Natural Science (Englewood Cliffs, Prentice-Hall, 1966) for a critique from a philosopher of science, and Don E. Dulany, "Awareness, Rules and Propositional Controls", in T. R. Dixon and D. L. Horton (eds.), Verbal Behavior and General Behavior Theory (Englewood Cliffs, N. J., Prentice-Hall, 1968) for the view of a psychologist.

can be accounted for in terms of an indefinite multitude of empirical laws relating an indefinite multitude of operationally defined concepts. It is still open for someone to claim that what I have called a single underlying concept is actually justified by the multitudinous extremely well-founded empirical correlations which do exist among the operational parts of the single concept. It is further open to the operational social scientist to claim that whatever can be said for assuming underlying properties like length in the physical sciences, these considerations do not apply mutatis mutandis to the social sciences. Thus, it might be argued, a more rigid operationalism is needed in the social sciences to counteract the ever-present danger of a too-easy mentalism which would rob the social sciences of the empirical import guaranteed by operationalism.

What I want to do now is undercut this kind of defense to the traditional charges sketched above, by exposing a dogma of operationalism underlying this defense. The dogma is this: there is one, favored, observation language in terms of which we can be logically assured of reaching unambiguous agreement on our operational definitions. By an observation language I simply mean a language containing the terms in which the operational definition is formulated. The phrases, 'is an experimental treatment' and 'is a case of placing a meter alongside an object to be measured' are observation terms relative to operational definitions I

previously used as examples. My plan of attack is as follows. First I want to show that the logical role of the term to be operationally defined must be different from the logical role of the observation terms which figure in the definition. Second, I want to argue that there seems to be no way of independently specifying a set of terms which can play the logical role of observation terms for all possible operational definitions.

From these two features it will follow that what is treated as observable is relative to a certain background theory which is accepted for the purposes at hand. And this in turn will imply that notions like reliability, validity, convergent validity, discriminant validity and what have you are realtive to the background theory. Thus, the choice of observation language cannot be made *a priori*, but is itself subject to empirical-theoretical investigation. This means that cognitivism, or, for example, the principle of *verstehen* interpreted as a request for a certain kind of observation language, cannot be dismissed by simple *a priori* appeal to operationalist doctrines. In short, one of the apparent classical advantages of the operationalist will be seen to be illusory. The operationalist is fond of remarking that he postulates no hidden entities or processes. Everything is open to observation and all hypothetical constructs are tied explicitly to observation. But with the relativity

of observation thesis, this remark is seen in its true light: It registers a decision to use a certain set of observation categories. Even worse, the justification of the decision is usually taken to be obvious or else methodological in the sense of a priori. However, if I am right, the choice of observation categories is itself open to empirical investigation.

But it is past time to deliver on these promises. My first claim is that the logical role of the term to be operationally defined has a different logical status than the terms used in the definition. This difference can be brought out as follows: When an operational definition is first proposed, the term being defined has no status whatsoever except as a name for the particular operation and particular results of the first such test ever performed. In order to promote the term to the name of a class of operations and results we must, as the operationalists are fond of pointing out, establish the reliability of the operation. That is, we must show experimentally that the same operations yield the same kinds of results.

But how could we fail to show the reliability of a proposed operational definition? Presumably, only by showing that the same operations do not always or usually, yield the same results. But how in turn could we be assured that this was the case? It seems that we could know this only if we have some sort of criteria for the application of the kind of operation-term we have in mind and some sort of criteria

for the application of the kinds of result-term we have in mind. But what are these criteria? Are they also implicit operational definitions? They might be, but then the same problem of specifying the criteria for the application of their observation terms would be raised all over again. Thus we must either admit a vicious infinite regress of operational definitions, or else we must somehow independently of present considerations find some way of stopping the regress.

Let me assume for now that such a way has been found at some level or other. I shall discuss the "ultimacy" of this postulated level below. At any rate on this level one of the requirements for the criteria of application of its terms is that the observational terms refer to a process or property underlying these criteria. It is then by means of the persisting manifestations of these criteria that we identify the particular as a particular of the kind in question. That this is so follows from the obvious fact that without this feature we could never use common names or class names or property words at all; only proper names. To attribute any kind of classification to a particular is, <u>ipso facto</u>, to abstract from some of its particular properties. Put another way Leibniz' law says that for two purported particulars to be <u>two</u> particulars of a certain kind is for them to share the same underlying properties. These properties then are the defining characteristics of the kind in question. In order to classify at all, the terms of the classificatory

kinds necessarily refer to some underlying property in terms of which two or more particulars are particulars of that kind. The particulars differ in respect to properties not essential to the defining class, e.g., spatio-temporal or mass properties.

What does this rather abstract argument mean in a concrete case? It means at the very least that before we can sensibly talk of investigating the reliability of, say, the operational definition of intelligence as measured by the Stanford-Binet test, we must know when two events in the world <u>are</u> events of a particular subject's answering a Standord-Binet question, when two events <u>are</u> events of two or more subjects' taking a Stanford-Binet test and so on. For if we could not assume for the purpose at hand that we could unambiguously make such determinations, we could not even begin to establish the reliability of the test. In short the very necessity to classify, to apply the observational terms in the operational definition to more than one object requires that the properties or processes which form the basis of classification <u>underlie</u> particular instances of classification. Thus the logical role played by the observation terms in an operational definition is to function as unambiguous terms referring to persisting properties or processes. It is only against the background of such an assumption of persistence that the theoretical term to be operationally defined can be judged to designate a

reliable connection or not. Put yet another way -- to demonstrate a reliable empirical connection demands that there be logical criteria of identification of the concepts which are candidates for the empirical connection.

But this requirement that the observation terms designate some process underlying differing occasions of use of the term need not prove an embarrassment to operationalism if some observation language could be independently specified which would serve as the ultimate grounding for all operational definitions. For a variety of reasons, no such neutral, unique observation language seems to exist which logically guarantees unambiguous criteria of application.

Without going into the details of the arguments, one can mention two major considerations which point toward the conclusion that there is no neutral observation language. On the one hand there is the work in perception by gestalt psychologists which tends to suggest precisely what I have argued above, viz., that while it may be possible for a particular purpose to assign the role of logical criteria of identification to some one or more observation terms, no one seems to serve for <u>all</u> purposes. Closely allied with this point is the work of N. R. Hanson.[7] His brilliant phenomenological description of what an advocate of a heliocentric universe sees in watching a sunrise and what an advocate of a

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[7] N. R. Hanson, <u>Patterns of Discovery</u> (Cambridge, University Press, 1958).

geocentric universe sees watching the "same" sunrise strongly suggests that no basic observation language exists. The crucial weakness in such a theory is to specify in what sense the content of the two perceptions described above is nevertheless the "same." This problem will be treated in the next chapter.

The second major line of argumentation has already been hinted at by the above discussion of the nature of the classificatory activity itself. Since there seems to be no limit on the varieties of classificatory types it seems that every classification criteria must be indefinite in some respects. If an attempt is made to tighten up the classification to its logical extreme, one seems to lose precisely the notion of classification and is left with simply proper-naming. Thus, the history of the use of sense date as some sort of neutral perceptual given illustrates this point nicely. If the notion of a sense-datum is coherent at all, it seems to be in application to a particular unclassifiable experience. As in operational definitions, complete specificity is purchasable only at the price of being unable to say anything about the specificable item.

But now if in giving an operational definition we must assign the logical role of observation terms to some of our concepts and yet these are not concepts which can lay claim to being primitive or ultimate in this regard, then on what basis can we choose an observation language? It seems to

me that the answer is that those concepts or operational definitions are most reliable which are best connected with other concepts in a well-established theory (common sense?). Of course, for other purposes these concepts might be operationally defined.

The dogma of operationalism then is quite simple. It is the belief that some a priori reason can be given for choosing one observation language over another. This dogma manifests itself in behavioristic psychology in the disdain with which mentalist theories are held. But if I am correct, since both mentalist theories and behaviorist theories must presuppose some kind of underlying processes or properties to serve as observational base, and since no a priori reasons can be given for such a choice, then the choice of observation languages can only be settled by a long drawn-out empirical investigation of both kinds of theories to see which is in the long run the most theoretically fruitful and justifiable. To point to just one crucial area -- there is no particular reason, apart from elaboration in the respective theories, why human action is not just as observable as mere human movement. I think such a claim is a defensible part of what Max Weber means when he stresses the notion of verstehen.[8] To observe human behavior with verstehen is to see it as action and not mere movement from which action

[8] Max Weber, Methodology of the Social Sciences (Glencoe, Ill., Free Press, 1949).

must be inferred. Whether an observation language of movements will ultimately prove more successful than one of action, simply cannot be said at present.

And now let me rather sketchily illustrate this relativity of operational definitions to observation languages by examining how the notions of reliability, convergent validity, and discriminant validation can change into one another depending on the background observation language.

As I am using these terms, 'reliabilitly' roughly means replicability of a single method of measuring (defining) a single trait. 'Convergent validity' roughly means the ability to determine a single trait by different methods of measurement. (If the reader does not wish to beg any behaviorist questions, he can use 'validity' simpliciter to indicate the constant conjunction of two different individually reliable measurements (operational definitions). Such a formulation need not imply that there is or is not any "underlying" trait.) 'Discriminant validity' will mean for me a reliable distinctiveness between two measures or two groups of measures. This concept becomes important when there is some non-experimental reason to believe that the measures might be convergently valid. For example, if we suppose we have independent measures of 'creativity' and 'problem solving ability' and we think these are both parts of a larger con-

cept of intelligence, it become important to see if we can indeed discriminate these two measures.[9]

One example of the relativity of reliability and validity is already implicit in the preceding discussion of the possibility of an infinite regress of operational definitions. Suppose one investigator frames an operational definition of some trait and wishes to test its reliability. The definition might symbolically look like:

$$(3) \quad Op(x) \supset (R(x) \supset Tx)$$

As I have point out, Op(x) and R(x) will be observation terms for him. Testing for reliability will involve seeing if the situation described by (3) actually obtains. But for a second investigator who believes, e.g., that Op(x) must be operationally defined, the same experiments may be considered as testing the convergent validity of varying operational definitions of Op(x).

Suppose, for example that someone operationally defines $Op_F(x)$ where intuitively this is Op(x) performed by a female experimenter. Analogously $Op_M(x)$ is Op(x) performed by a male experimenter. Consider various tests of

$$(4) \quad Op_F(x) \supset (R(x) \supset Tx)$$

$$(5) \quad Op_M(x) \supset (R(x) \supset Tx)$$

where $$(6) \quad Op_1(x) \supset (R_1(x) \supset Op_F(x))$$

and $$(7) \quad Op_2(x) \supset (R_2(x) \supset Op_M(x))$$

are the (implicit or explicit) operational definitions of

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9 See Campbell and Fiske, "Convergent and Discriminant Validation," and Cronbach and Meehl, "Construct Validity".

$Op_M(x)$ and $Op_F(x)$. That is, the level of observation language has shifted.

The first experimenter will make no distinction between (4) and (5) and replicability will mean reliability of (3) to him. On the other hand the same replicability will mean convergent validity to the second experimenter. The hypothesized situation would show, that in this case experimenter bias according to sex would seem not to be present.[10]

On the other hand, suppose that (4) or (5) is <u>not</u> reliable. Then neither will (3) be reliable but the first experimenter will likely go no farther unless he reconceptualizes the problem. The second experimenter, however, can take the <u>same</u> experimental results which show unreliability of (3) as indicating a discriminant validity. Whether he does this or not will depend on his understanding of $R(x)$, $R_F(x)$, and $R_M(x)$ and the relations he expects among them.

There are, no doubt, other schemata showing the relativity of reliability and validity to each other and hence to the chosen observation language. However, enough has been said here to indicate the scope of the dependence on the chosen observation language. In chapter V of this report, an attempt will be made to fill in the schemata with concrete details taken from one current issue in verbal learning theory.

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[10] Rosenthal, <u>op. cit.</u>

The above schematic discussion of the relativity of validity and reliability to observation language confirms my earlier general thesis that there is no a priori privileged observation language. The observational categories seem to be theory dependent. For the question of reliability versus validity seemed to depend on the level of operational definition which assigned to different terms the role of observation terms. In turn this assignment seemed to be dictated by the implicit hypotheses or theory the experimenter had in mind.

Let me now turn to a final related point. I also claimed in my general discussion that although there was a relativity of observation language to theory, nevertheless, within a particular theory and a particular investigation, the logical role of the observation terms was different from that of the terms introduced by the operational definitions. I want now to sketch that possibility.

Refer again to the example outlined above. Suppose that someone does take the view of the second experimenter, the one who, working with (4) and (5), wants to show the convergent validity of $Op_F(x)$ and $Op_M(x)$. The point is that he must presume that there is a process or property underlying the observational terms in the operational definitions, (6) and (7) defining these terms. That is, for him, $Op_1(x)$, $Op_2(x)$, $R_1(x)$, and $R_2(x)$ must be recognizable from time to

time, from place to place, from situation to situation, from experimenter to experimenter, etc. Classifying such terms as observation terms in effect assigns them the role of already validated persisting traits relative to their theory. To carry out the present investigations he presumes the solution of other possible reliability or validational problems. (Whether we take the presumed solutions to be solutions of reliability or validational problems is relative to our theoretical view of the situation. This follows directly from the earlier discussion of the relativity of reliability and validity.)

Consider the situation of the first experimenter, the one who is using (3) to test for reliability. In effect he is presupposing the solution to the validational problem facing the second experimenter. For his reliability tests will yield unconfounded results only on the supposition that experimenter sex makes no difference. He must presuppose that the experimenter's having administered the operation regardless of the sex of the experimenter is a property which underlies the specific instances of that operation. (The specific instances may, of course, have experimenter sex differentiations.)

The important theoretical point to emphasize is that there must be some terms playing the role of observation terms and that these terms refer to an "underlying" property. The choice of which entities are to count as theoretical or

inferred and which as observational cannot, therefore, be made by some a priori consideration of whether, e.g. an intention "underlies" a movement classified as an action. For behaviorist theories, intentions will be inferred entities, if they are used at all. But this does not absolve the behaviorist of his own commitment to another, albeit different, set of "underlying" processes. The question then is not one of doing away with underlying processes; all theories have them somewhere or other. The question is rather one of choosing the most scientifically fruitful set of underlying properties. But this in turn is an empirical question concerning the most profitable choice of observation language. But now given my abandonment of any philosophical or epistemological reasons for choosing one observation language as "ultimate," a set of problems arise. Can we justify particular observation languages for particular purposes? How? Can any such justification retain any sense of the "objectivity" of science? That is, can we objectively decide between two theories consistently with the claim that each theory determines its own observational categories? What could serve, then, as the objective criterion for decision? These questions along with a concrete verbal learning theory illustration will be taken up in the following chapters.

IV. SEEING AND SEEING AS

As the arguments of the preceding chapter have shown, there are some serious difficulties involved in the positivist conception of the role of observations in scientific theory. On the positivist account of observation there is something about observation and about the language used to describe observation which suits it for performing the function of checking or verifying scientific theories. At least a necessary condition of the correctness of scientific theories is that they fit the facts of the world, and the facts of the world are somehow supposed to be directly revealed to observation and represented in the observation language.

One of the major prongs of the attack on such a positivist notion has centered on just this notion of a separately specifiable observation language. Phenomenological description has joined hands with psychological experimentation, along with the failure of the phenomenalist program, to cast doubt on the very possibility of a determinate observational base or neutral observation language. It has been urged that observation, far from providing an independent base against which theory can be checked, is itself theory laden. In some sense observational categories are theory determined. I shall call the upholders of such a position on observation, relativists.

Despite the rather widespread acceptance of some such doctrine of the relativity of observational categories, this kind of view faces serious internal difficulties. While I shall not be able to discuss all of these difficulties, I want to concentrate on two of the most severe. First the view that observational categories are theory relative seems, in its extreme form, to be self-defeating. One cannot appeal to the (neutral) facts of observation to establish the thesis that all observation is theory relative. For the theory in which such a thesis is itself proclaimed, ex hypothesis, determines its own observational and evaluative categories and hence has no more claim to absolute truth than any other.

Second, on the extreme version of such a thesis it seems impossible for scientists ever to disagree, or even more importantly ever to agree. For a plausible case can be made out that every scientist in some small sense has a different theory, and hence different observational categories. Thus there will be nothing in common about which scientists can agree or disagree. Their positions, appearances to the contrary notwithstanding, will simply pass each other by.

I will call the phenomenon pointed to by these two lines of response to the relativists, the objectivity of observation. The hard question then which must somehow be faced by the relativists is whether or not the objectivity of observation is an illusion. If it is, they must tell us why it has been so widespread and on what "objective" basis we are

to judge it the ubiquitous illusion it is. If it is not an illusion, the relativists must tell us how this is possible consistent with the very convincing arguments they offer to show the theory dependency of observational categories. It will be the thesis of this paper that Wittgenstein's discussion of seeing and seeing as offers the relativist the latter sort of way out. That is, I believe Wittgenstein's discussion of seeing and seeing as provides a way of preserving the objectivity of observation in a way consistent with the theoretical loading of observational categories.

Wittgenstein introduces his discussion of seeing and seeing as with reference to a standard perspectival drawing of a parallelepiped. The contrast is to be drawn between the illustration and the various contexts in which this figure might appear. It might appear in a text-book, Wittgenstein says, as an illustration of "... a glass cube, there an inverted open box, there a wire frame of that shape, there three boards forming a solid angle." (p. 193)[1] The distinction then is the perceptual one between seeing the illustration and seeing it as, e.g. a wire frame.

Given this way of introducing the distinction, it would be only too natural to conclude that the perceptual distinc-

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1 L. Wittgenstein, Philosophical Investigations, 3rd edition, (New York, Macmillan, 1968). In the following I shall use the by now standard practice of referring to the Investigations by a number simpliciter to indicate the section number in Part I and by page numbers for Part II.

tion between seeing and seeing as is to be understood as the distinction between seeing and seeing with an interpretation. Somehow, in seeing as we build the context of the text book directly into the perceptual experience itself. This is, I think the position taken by N. R. Hanson.[2] And yet this view, suggestive as it is, breaks down at just the crucial place.

Consider Hanson's description of the two ancient astronomers, the geocentrist and the heliocentrist, on the hillside watching the "same" sunset.[3] One sees, according to Hanson, the sun falling, the other sees the horizon rising. Yet, if Hanson is to avoid the problems of the relativist alluded to earlier, he must somehow specify what sense is to be given to the claim that the two see the "same" sunset.

Wittgenstein sees this point very clearly and for this very reason rejects the seeing-plus-an-interpretation analysis for seeing as. His reason is that the interpretation is like an indirect description which will only make sense on the supposition that there could be a direct description. (p. 194). By implication this is what cannot be done. In other words a direct reference to the experience of seeing would provide the analysis of "same" experience which is wanting here, yet Wittgenstein appears to believe that no such description can be given. The failure of the pheonomenalist program also

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[2] N. R. Hanson, Patterns of Discovery (Cambridge University Press, 1958).

[3] Ibid., p. 14.

testifies to this conclusion.

At this point Wittgenstein introduces a crucial qualification. He distinguishes between the continuous seeing of an aspect and the dawning of an aspect. (p. 194). He then effectively limits seeing as to the dawning of an aspect. The reason for this is that it makes no sense to speak of e.g. seeing a knife as a knife (p. 195). It just is a knife and one just sees it. A similar point is made concerning the continuous seeing of the rabbit aspect of the duck-rabbit.[4] One does not in such a case see the duck-rabbit as a rabbit, although someone else might say truly of me that I am seeing the picture as a rabbit. This limitation of seeing as to the dawning of an aspect seems to me effectively to negate any suggestion that Wittgenstein's thesis in this section is that all seeing is seeing as.[5] Put in another way, if all seeing were seeing as, one would still be faced with the problems of specifying what is common to two different occasions of seeing as. But this problem of specifying an analysis of seeing which can serve as the basis of several alternative cases of seeing as is just the same problem as that of specifying a neutral observation language. In short the thesis that all seeing is seeing as

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4 The picture of the duck-rabbit on p. 194 of the Investigations is a figure which can be seen as a duck facing left or as a rabbit facing up.

5 Insofar as I understand him, G. N. A. Vesey seems to be holding such a mistaken impression in his article, "Seeing and Seeing As," Proceedings of the Aristotelian Society, LVI, 1955-56.

is just one form of the relativists' thesis. Further Wittgenstein's rejection of the thesis that all seeing is seeing as seems based on precisely the same sorts of considerations as I have earlier called the objectivity of observation. Observation can be theory loaded and seeing can be seeing as only if we can give some sort of specification of two theories having the same observational category or two people seeing the "same" thing as two different things.

In the presence of a change of aspect, Wittgenstein toys with the idea that there is in fact nothing similar that is seen. (p. 196, p. 199, p. 212). For we report the dawning of an aspect in ways very similar to new perceptions. Yet this alternative, essentially the radical relativist alternative, is rejected by Wittgenstein. It is rejected because there are some obvious things which do not change in the dawning of an aspect. For example, if an exact copy is made before and after the dawning of the aspect, no change is shown (p. 196). Nor will it do to speak of a different sense-datum in a change of aspect, at least if this is meant to be the distinguishing characteristic. For such a private object in addition to suffering all the other disabilities of private objects that Wittgenstein so often pointed out, simply will not serve the purpose here. It will not serve to account for the difference in a changing aspect because it is precisely constructed to serve as that

which is perceptually the same. Thus if I saw the duck-rabbit as a duck and then saw it as a rabbit, it is just the non-visual aspects of the situation which have changed. "Now the only possible expression of our experience is what before perhaps seemed, or even was, a useless specification when once we had the copy." (p. 196). What we do now is place the duck-rabbit along side non-ambiguous pictures of rabbits, saying, "This is what I now see!" (p. 197). "'Seeing as . . .' is not part of perception. And for that reason it is like seeing and again not like." (p. 197). "Hence the flashing of an aspect on us seems half visual experience, half thought." (p. 197).

Thus Wittgenstein has seemingly rejected the view that something different is seen in seeing as. He has also told us that there are some non-visual cognitive aspects to seeing as. And yet the half thought, half visual experience cannot be so easily separated into their respective halves. Is it ". . . an amalgam of the two as I should almost like to say?" (p. 197). At this point, Wittgenstein gives phenomenological description after phenomenological description of various examples of seeing, all of which, seem to share the feature of amalgamated thought and visual experience. He mentions, among others, delayed recognition (p. 197), seeing a smile with and without understanding it, (p. 198), the limitations of our perceptual abilities when perfectly

familiar objects appear in strange ways (p. 198, p. 200), finding a figure in a puzzle figure (p. 199), and seeing an animal transfixed by an arrow, part of which is hidden (p. 203).

And the theme played over and over in discussing these examples is the relativity of the visual-cognitive distinction to the purposes at hand. " . . . /I/s the copy of the figure an incomplete description of my visual experience? No -- But the circumstances decide whether, and what, more detailed specifications are necessary." (p. 199). "There are, for example, styles of painting which do not convey anything to me in this immediate way, but do to other people. I think custom and upbringing have a hand in this." (p. 201). "When I see the picture of a galloping horse -- do I merely know that this is the kind of movement meant? Is it superstition to think I see the horse galloping in the picture?" (p. 202). "But this isn't seeing!" -- "But this is seeing! -- It must be possible to give both remarks a conceptual justification." (p. 203). "Is it a genuine visual experience?" The question is: in what sense is it one?" (p. 204).

The dawning of an aspect, i.e., seeing as, is an amalgam of perception and thinking. What are we to make of this? Wittgenstein asks, "How would the following account do: 'What I can see something as, is that it can be a picture of?' What this means is: the aspects in a change of aspects are those

ones which the figure might sometimes have permanently in a picture." (p. 201). I think this remark is meant to point out that there is a perceptual core to the seeing as experience. It is closely related to the perceptual core which makes something a picture of something.

And yet this core seems not to be fixed. It is dependent on the knowledge of the perceiver. "'Now he's seeing it like this,' 'now like that' would only be said of someone capable of making certain applications of the figure quite freely. The substratum of this experience is the mastery of a technique." (p. 208). We must have learned and know certain things in order to have certain kinds of perceptual experiences. Wittgenstein grants that this sounds strange, but reminds us that seeing as is not quite the same as seeing simpliciter. However, he suggests that there may be no reason to deny that while some people can only see X as Y, others might simply see Y. Speaking of the non-visual content of seeing as, Wittgenstein says, "Might I not for all that have a purely visual concept of a hesitant posture, or a timid face?" (p. 209).

But now what is this non-visual content in seeing as? A necessary condition for it is that we have mastered a technique; we must know something. ". . . /W/hat I perceive in the dawning of an aspect is not a property of the object, but an internal relation between it and other objects . . ."

The echo of a thought in sight "--one would like to say." (p. 212). But this means that Wittgenstein wants to connect seeing an aspect with the meaning of the concept(s) employed. For what else <u>are</u> internal relations but the conceptual connections of the object with other objects referred to in our conceptual scheme. This close connection with meaning is further amplified by Wittgenstein when he analogizes seeing an aspect to experiencing the meaning of a word. (p. 214). And he goes on to argue in great detail that although a particular kind of mental experience identifiable in terms of mental criteria <u>may</u> accompany the notion of meaning -- that is we may, sometimes or even usually, <u>experience</u> the meaning of a word, this is not essential to grasping the meaning. What we need to grasp the meaning of a word is to grasp the use of the language games in which the word figures. It is no accident, I believe, that Wittgenstein concludes Part II. section xi, the seeing, seeing as section, with another discussion of meaning and language games.

Wittgenstein's discussion of someone who is aspect-blind (pp. 213-214) also brings out this connection with language games. Someone who is aspect blind seems to be unable to play a certain language game or set of language games. "Aspect-blindness will be akin to the lack of a 'musical ear.'" (p. 214).

But now it is clear what the mastery of a technique has to do with seeing an aspect. Seeing X as Y is placing X in the context of the language game surrounding Y's. If we have not been trained into the Y-language game, or alternatively, we cannot be so trained, i.e. we lack a "musical ear" for that game, then we will be Y-aspect blind. Seeing something as something else involves placing the object into a set of internal relations with other objects. But Wittgenstein's way of marking off sets of internal relations is by means of the concept of language games. The internal relations surrounding any concept are determined by looking at the language games into which that concept enters. We see now that this general rule holds for perceptual concepts as well. Only now we must concentrate a bit more heavily on perceptual features of the language game, e.g. what pictures I place an object with, what I bring if asked to get something like the object, etc.

But now if seeing as is to be explained by the placing of the object seen into a new language game with a new set of internal relations, a question crucial for the relativist still remains. What is that which is common to the various occurrences of seeing as? What, in other words is the analysis of seeing? Here I think one can get a hint from Wittgenstein's use of ambiguous figures to illustrate his discussion. Consider the duck-rabbit. Suppose someone

sees the figure as a rabbit. He may describe his experience solely as seeing a rabbit. (p. 194-195). Then someone may point out the duck-aspect. He may then see it as a duck. He has found a new language game home for the figure. But notice now that his situation is perfectly symmetrical. A first sees a rabbit, then sees it as a duck. B may first see a duck and then see it as a rabbit. If A's seeing the picture as a duck involved his placing the figure in the duck-language game, then surely B's original seeing of the duck must have involved his also placing it in the duck-language game. The difference between seeing and seeing as is not that the former is solely perceptual while the latter is also cognitive. Rather all perception involves cognition and thought. Seeing as marks the transition between two areas of thought marked off by two different language games. In short, Wittgenstein does hold the relativist thesis that observation is theory laden.

He also notes quite clearly the problem of the objectivity of observation. "And now look at all that can be meant by "description of what is seen." -- "But this just is what is called description of what is seen. There is not one genuine proper case of such description." (p. 200). That is, no linguistic description will serve as the neutral objective way of specifying what is seen. "'You can think now of this now of this as you look at it, can regard it now as this now as this, and then you will see it now this way,

now <u>this</u>.' -- What way? There is no further qualification." (p. 200). There is no kind of language game which is somehow ultimately privileged and in terms of which we can see things in a basic way. There are no basic aspects, no sense data, no perceptual given.

Can anything then be said about the objectivity of observation? Consider another ambiguous figure used by Wittgenstein -- the double cross, an octagon with alternating segments in white and black thus forming a white and black cross intermingled. (p. 207). The aspects thus are a white cross on a black background or a black cross on a white background. Let me now quote Wittgenstein's description at some length.

> "Those two aspects of the double cross (I shall call them the aspects A) might be reported simply by pointing alternately to an isolated white and an isolated black cross.
>
> "One could quite well imagine this as a primitive reaction in a child even before it could talk.
>
> "(Thus in reporting the aspects A we point to a part of the double cross. -- The duck and rabbit aspects could not be described in an analogous way.)
>
> "You only 'see the duck and rabbit aspects' if you are already conversant with the shapes of those two animals. There is no analogous condition for seeing the aspects A." (p. 207).

There is a distinction between the two ambiguous figures. One, the duck-rabbit, presupposes a set of language games already in use. The other, the double cross, is connected with the initial learning of a language game. It is primitive.

> "Here we are in enormous danger of wanting to make fine distinctions. -- It is the same when one tries to define the concept of a material object in terms of 'what is really seen'. -- What we have rather to do is to accept the everyday language-game, and to note false accounts of the matter as false. The primitive language-game which children are taught needs no justification; attempts at justification need to be rejected." (p. 200).

Some language games are just given. We must accept them. Justification, at least the standard kind of justification, is out of place. "What has to be accepted, the given, is -- so one could say -- forms of life." (p. 226). I shall, however, return briefly to the notion of justification below.

What we have now is a distinction between the initial, primitive learning of a language game and learning some new connections in already existing language games. This is not a new notion. Wittgenstein has used it before in commenting on the ostensive definition picture of language put forth by Augustine. (32). He implicitly criticizes Augustine for not

taking into account the primitive learning of language games. Augustine's account depends on our already having a language. One finds this distinction again in section 201. Wittgenstein asserts that to block an infinite regress of rules, grasping the rules in terms of an interpretation, which in turn seem to require rules for their interpretation, etc., ". . . there is a way of grasping a rule which is not an interpretation, but which is exhibited in what we call "obeying the rule" and "going against it" in actual cases." (201) "/I/f a person has not yet got the concepts, I shall teach him to use the words by means of examples and by practice -- and when I do this I do not communicate less to him than I know myself." (208)

Thus even though they are in no sense absolute or even necessarily true, the primitive perceptual language games into which we are trained give some hope of yet providing an account of the objectivity of observation consistent with the relativist position. But further we can also see why there are limits to what we can see something as. (p. 208). There is simply a physical and evolutionary limit on what language games it is possible to be trained into. "/O/ur pupil's capacity to learn may come to an end." (143). He may be untrainable in certain areas.

This empirical limitation on the number and kinds of language games helps explain several other more puzzling limitations that Wittgenstein places on seeing and seeing as. He notes (p. 213) that seeing as is subject to the will.

If one ignores aspect-blindness, which has already been seen to be connected to our being trainable into the requisite language-games (recall the musical ear), then we can see things as other things provided simply that we have mastered the technique and wish to do so. On the other hand, the order, "Now see this leaf green" (p. 213), makes no sense because given the empirical conditions on our environment and the kinds of organisms we are, there are no alternatives to seeing the leaf green and hence no sense to be given to the order to see it that way. Seeing green leaves and seeing in general are as they are because of the evolutionary constraints which have operated. On the other hand, I feel sure that Wittgenstein would admit that it might be possible for someone to will to see both of the words in those tests given for color-blindness where ordinarily one sees one word if one is color blind and another word if one is not. The analogue to the order to see the leaf green would make sense here. But it is just because of the widespread existence of such a primitive language game as seeing green leaves that gives objectivity a hold. Those perceptual language games which cluster around seeing simpliciter and which are hence <u>not</u> subject to our will seem to provide a way out of the radical subjectivism which threatens the relativists' position on observation. These simple seeing language games are pervasive and evolutionarily justified. They can serve as the objective basis even though they are not totally stable

and subject to some exceptions, e.g. the color-blind person.

The fact that the primitive seeing language games are empirically conditioned and hence not subject to the will, also explains why we cannot see a knife as a knife (p. 195) nor a conventional picture of a lion as a lion (p. 206). For again seeing as marks the transition between language games and as such presupposes the ability to play the two alternative language games. But if knives and conventional lion pictures are limited to single primitive language games then seeing as will be inappropriate.

The distinction between primitive language games into which we can only be trained and which have no justification and the language games which presuppose such a primitive base provides the foundation for a relativistic analysis of the objectivity of observation. The primitive language games, seeing simpliciter, provide the objective base and the seeing as language games play the role of various theories tried out against the observational base. Thus one can understand a la Hanson,[6] why the physicist sees the cathode ray tube differently from the student and yet both see the same thing. The similarity consists of the similar primitive "glass-tube" games. The difference consists of the fact that the physicist has also been trained into the

[6] Op. cit.

physics language game.[7]

And yet it is important to notice that there is nothing a priori or absolute about the observational base. It can and does change. As our view of the world becomes more sophisticated, so also do the primitive language games into which we train our children. We may even discard some in favor of others depending on our needs and environment. The many varieties of snow perceived by the eskimoes is one well-known example of this phenomenon.

I want now, however, to return ever so briefly to the "justification" of the primitive language games we play. According to Wittgenstein justification is, strictly speaking, out of place here. "our mistake is to look for an explanation where we ought to look at what happens as a proto-phenomenon. That is, where we ought to have said: this language game is played." (654) It is obvious that I take this remark to refer only to what I have called the primitive language games. It is worth noting that most philosophers have taken this remark to refer to all language games. This mistake has been almost as pernicious as the positivists'

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7 There are several abstract philosophical questions connected with my treatment of seeing and seeing as in terms of language games. For one thing, one wants to know just a bit more precisely what a language game is. Second, the conceptual relations which constitute identity conditions for a language are often thought to be peculiarly philosophical. Yet it is at least hinted at by my treatment that there is no sharp line to be drawn between science and philosophy. This is nearly heresy in philosophy. Finally just what the relation is between science and philosophy needs to be spelled out in some detail on the relativist model of observation. The line a more complete answer to these questions might take have been adumbrated by Wilfrid Sellars, "Some Reflections on Language Games," Science, Perception, and Reality (New York, The Humanities Press, 1966).

trying to reduce everything to one favored language game. I do not think that Wittgenstein himself ever really advances beyond the observation that certain language games just are played. However, if nothing more is said, it can be asked, Ought we to train our children into these primitive language games? Why or why not?

What I want to suggest here is that a kind of evolutionary argument can be given for accepting, tentatively, the primitive language games we do have.[8] For the observational categories contained in these games, together with the "theory," i.e., the various possibilities of seeing as built on the primitive games, have stood the test of time. If we attempt to train people into language games which do not fit reality, these games are weeded out by the fact that anyone who can play them will not survive. But even so it must be admitted that it is certainly possible that the beliefs humans have about the world, while adaptive, do not acutally "correspond" to reality. I think this is correct. The quest for certainty of some kind, any kind, in philosophy has been and continues to be a fruitless one. However the

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[8] Wittgenstein's only hint at an evolutionary justification for primitive language games occurs when he talks about the role of natural history. E.g. "What we are supplying are really remarks on the natural history of human beings; we are not contributing curiosities, however, but observations which no one has doubted, but which have escaped remark only because they are always before our eyes." (415). For a fuller treatment of this kind of evolutionary justification, see D. T. Campbell, "Methodological Suggestions From a Comparative Psychology of Knowledge Processes," Inquiry, 2, 1959. 152-182, and by the same author, "Evolutionary Epistemology," in P. A. Schilpp (Ed.) The Philosophy of Karl R. Popper, in The Library of Living Philosophers (La Salle, Ill., The Open Court Press, (volume in press)).

role of observation contains a kind of certainty, although it is presumptive certainty. What is necessarily true is that for each investigation some set or other of observational categories must be adopted and for the duration of that investigation, it is presumed that no radical errors accrue to the particular set of observational categories chosen. Any formal theory of error analysis clearly makes just such a presumption that no radical error is present in the set of observational categories taken as a whole. They are assumed to be appropriate as a whole and this presumptive appropriateness is as close to certainty as one can get.

Finally the only ultimate test of a set of primitive language games along with the theoretical superstructure which accompanies them is how well they enable us to cope with our environment in the broadest sense. The choice between two theories if they are broad enough, may be a choice between two ways of looking at the world. If not so broad, the places where the two theories may depend on significantly different primitive language games needs to be carefully analyzed. For it would be there that the most heat and the least light would be likely to be generated. The behaviorist versus linguist controversy in verbal behavior illustrates this nicely.

In summary then, seeing as marks the transition between playable language games. Seeing and seeing as require master-

ing language games. Thus a whole set of behaviors are connected with perceptual concepts. Observation is cognitive. Nevertheless, one must distinguish between primitive language games which form the basis for seeing simpliciter and more sophisticated ones which presuppose the primitive ones as playing the role of observational base. What games play this role is empirically conditioned and can change. The only "justification" for playing the primitive language games is an evolutionary one. I think this view does consistently maintain a relativist theory of observation while holding out some hope for a solution to the problem of the objectivity of observation.

V. CHOMSKY AND SKINNER: A PARTIAL CASE STUDY

I come now to the application of the preceding philosophical results to a concrete problem in verbal learning theory. For the purposes of illustrating the thesis of the theory dependency of observation I have chosen the long-standing controversy between a behavioral analysis of verbal learning as exemplified by Skinner[1] and a transformational linguistics approach as exemplified by Chomsky.[2]

Before I begin this task, however, let me briefly review the conceptual tools I have developed in the foregoing. In the first place, I have argued that a radical version of the thesis of theory dependency of observational categories faces fatal internal flaws. Nevertheless, I suggested that a more moderate version may yet be true and helpful in disentangling current methodological controversies. Next I undertook a review of operational definitions as used by social scientists. I found that operational definitions are precisely the point at which theory is conceived to come into contact with observation. I further found that the methodological canon of operationalism is often misconstrued as an a priori justification of a favored set of observational categories. I showed that no unique observational base seems to exist, that the justification of any proposed base is

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1 B. F. Skinner, Verbal Behavior (New York, Appleton-Century-Crofts, 1957).

2 Noam Chomsky, "Review of Skinner's Verbal Behavior", Language, 35, 1959, 26-58.

broadly empirical, and that the logical role of observation, that of providing a presumptive base of underlying properties, must be played in any methodology, behaviorist or cognitivist. I then turned to the problem of specifying some sort of justification for a choice of observational categories. I found generally that observation concepts are tied to the myriad things that people do with the concepts. That is, to see X is to be able to play the X language game. Observation is theory dependent. The only justification for the perceptual concepts we have seems to be at bottom a pragmatic-evolutionary justification. Nevertheless, there seems to be an important distinction between two types of perceptual concepts. One of these we can only be trained into using -- in a straightforward conditioning kind of way. The other kind we can learn if we already possess a language which includes some perceptual concepts of the former type. I argued further that there is no a priori justification for supposing that any given concept is essentially tied to what I shall now call the conditioned group of perceptual concepts as opposed to what I shall now call the language-dependent group of concepts. Whether a given concept fits in one or the other is seen to be a pragmatic evolutionary matter. Such a view also explains the importance placed on language by so many contemporary philosophers and psychologists. There seems to be a functionally definable group of perceptual concepts which is language dependent, where a language here is understood in

the sense of a set of language-games, -- a form of life into which we have been trained. However, there also exists the conditioned group of perceptual concepts which constitute the base for the language-dependent group. Focussing on this group helps to explain why so many philosophers and psychologists concentrate on the role of simple conditioning principles in explaining language. Finally the possibility of shifting the membership of concepts in these two groups helps explain why linguistically oriented researchers and conditioning oriented researchers seem just to pass each other by.

It would be wise here also to point out the close parallel between the two groups of concepts, conditioning and language dependent, on the one hand, and the two logical roles played by different concepts in the discussion of operational definitions. In the latter discussion I distinguished the logical role of observation concept from the role of concept to be operationally defined. There too was this relativity of membership in one or the other. The concept to be defined parallels the language dependent concept in the present discussion. Both are dependent on the presumption of an observational base. The observational concept parallels the conditioned perceptual concept in the sense that both play a kind of ultimate, although presumptive, explanatory role. The problem of a choice of concepts to play the role of observational base in operational definitions is paralleled by

the problem of which concepts are empirically capable of merely being conditioned without presupposing a language.

I think the central questions of this chapter can now be posed. Are there, empirically, any perceptual concepts which can best be thought of as simply conditioned? Are these the ones that Skinner studies or does he over-extend himself? Are there, empirically, any perceptual concepts which can best be thought of as language-dependent? Are these the ones Chomsky studies or does he over-extend himself? How do these two men's criticisms of each other relate to these problems?

Let me introduce this discussion by means of a brief reference to a recent paper defending Skinnerian types of behaviorism from Chomskyite types of attack.[3] I choose this paper not because I particularly agree or disagree with its defense of Skinnerian behaviorism, but rather because the author illustrates so beautifully the typical misunderstanding of the theory-dependency thesis of observation. Despite an apparent awareness (p. 200, footnote 3) of the problems of interpretation in perception the author bases most of his arguments on the old positivist assumption

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[3] William M. Wiest, "Some Recent Criticism of Behaviorism and Learning Theory," Psychological Bulletin, 67, 1967, 214-225. The papers to which Wiest is specifically responding are Chomsky, op. cit. and Louis Breger and James L. McGaugh, "Critique and Reformulation of "Learning-Theory" Approaches to Psycotherapy and Neurosis," Psychological Bulletin, 63 1965, 338-358.

of an independently specifiable observation language. (Wiest prefers 'public stimuli'). He even denies that the recent criticisms of positivist philosophy of science affect his case (p. 214), and yet he unwittingly proceeds to base most of his argument on just those principles. One can only conclude that either the challenge to positivist philosophy of science has been seriously misunderstood or else the mere assertion that an error has not been made does not, after all preclude actually having made the error.

Let me now illustrate these charges. Wiest bases one of his major arguments against the critics of Skinner on the allegation that these critics do not properly distinguish observation and inference. For example, Breger and McGaugh state (p. 349)

> "The point we wish to make here is that disagreement between the behaviorist and the psychodynamic viewpoints seems to rest on a very real difference at the purely descriptive or observational level. The behaviorist looks at a neurotic and sees specific symptoms and anxiety. The psychodynamicist looks at the same individual and sees a complex intra and inter personal mode of functioning which may or may not contain certain observable fears or certain behavioral symptoms such as compulsive motor acts."

This passage appears to me to be quite specific that the difference is in the observation -- what is seen -- it-

self. In other words Breger and McGaugh appreciate the theory-dependency thesis of observation, believe it to be true, and are attempting to cast their point in those terms. And yet Wiest's criticism of this is just an ignoratio elenchi. He says "Clearly, the authors were using the terms see and look as equivalent to believe, infer, and interpret, rather than as a description of what is usually meant by scientific observation." (p. 218). Clearly, this is just what Breger and McGaugh did not mean if I am at all correct in attributing to them a theory-dependency thesis of observation.

In other words, Wiest attacks Skinnerian critics for trying to foist off on Skinner the necessity for accounting for their (the critics') theories. But, clearly, as Wiest claims, all that any theory must account for is the facts, the publicly observable facts. Wiest is apparently completely oblivious of the fact that his opponents are claiming that the very notion of an unproblematic delineation of what would constitute scientific observation in general is itself here in question. So long as the influence of theory on the acceptable categories of what constitutes an observable fact is not appreciated, protagonists will continue to hurl charge and counter-charge at each other without ever making contact. Chomsky will say (p. 54) that Skinner does not account for the fact that the child acquires grammar. Wiest will claim that this is inference and not fact (p. 220) and since whether or not it is "fact" is very probably theory

dependent in some sense, both are right and both may be wrong. What is needed here, as was indicated above, is some way of evaluating the various categorial schemes for observing facts to determine if there may not be some overlap and hence some basis for getting a discussion going. The problem is that of trying to give some sort of analysis of the objectivity of observation (see Chapter IV above). The unsupported assumption that there is an a priori way of settling on a public observation language is precisely what I have attempted to call into question in the preceding and this is precisely what Wiest ignores.

There is another aspect of Wiest's paper which illustrates what I called above, "the dogma of operationalism". It will be recalled that I argued in Chapter III that every operational definition must presuppose some kind of "underlying" processes or properties and that no a priori reasons could be given for such a choice. The behaviorist is here no better off than the cognitivist. And yet Wiest's criticisms of the cognitivists' choice of conceptual apparatus show him to be totally unaware of his own necessary and yet unsupported commitment. He dislikes the "ill-defined" terms favored by the cognitivists (p. 223) e.g. ideas, wants, schemata, intentions, plans, meanings, programs, and strategies, and criticizes these on the grounds that ". . . it moves them [the cognitivists] away from rather than closer to the empirical data. To get back to the data . . ., we

must now rephrase the empirical questions: What scientific observations define "figuring out," "wanting," and "deciding", and under what conditions do these events occur," (p. 223). It is perfectly plain that Wiest does not believe we can ever observe anyone wanting something. That someone wants something must always be an inference. Wiest is calling here for operational definitions in terms of his own favored categories which he honors by calling "scientifically observable."

And yet, as I have argued, the general behavior of mankind is to be taken as somehow the only pragmatic-evolutionary basis on which to determine an "objective" set of observational categories. Thus, far from its being obvious that typical behaviorist observational categories constitute obvious "scientific" (clearly a merely honorific title here) observation, the burden of proof is on those few who play the behaviorist language games to justify their somewhat bizarre choice of observational categories. Such a justification is seldom forthcoming. Instead we usually get some such claim as the above to the effect that wants, intentions, etc. are inferred and not observed. However, the whole evolutionarily edited body of common sense argues that we do sometimes observe people <u>wanting</u> things and <u>doing</u> things (a form of intentional behavior.) It is just absurd to suppose that I <u>always</u> or even usually infer from a student's arm movement that he has raised his arm and wants to ask a question. I simply observe this in most instances. Nor will it

do to point out that I may be mistaken. Of course I might, but then so might the behaviorist in thinking he sees an arm movement. And unless we are once more tempted down that old dead-end phenomenalist path, we must simply reject this whole familiar behaviorist line of argument.

But it is important to point out here that in rejecting this particular line of behaviorist argument I am not thereby rejecting the behaviorist position. It may indeed be true that our common-sensical descriptions of human behavior need to be refined and replaced by a behaviorist-like observation language. I am simply concerned that two things be kept straight. First, if we are to adopt a behaviorist set of observational categories, it will not be due to any a priori considerations concerning the inferred or hypothetical status of common sense categories. What makes an entity observed or inferred is the role it plays in the accepted background theory. (Cf. Ch. III). Second, only broadly based empirical considerations can ultimately determine whether the common sensical observational base is to be preferred to the behaviorist proposal. The former at least has the advantage of having survived a good long evolutionary weeding out process. This consideration is surely sufficient to put the burden of proof on those (the behaviorists) who would introduce a change.

Nor, for that matter, are the behaviorists the only ones

who fail to grasp the significance of the theory-dependency thesis of observation. It is one thing to say that we can observe verbal behavior as a particular kind of intentional action. It is quite another to suppose that it is an observable fact that children construct (in some sense) the grammar of the language they learn. And yet Chomsky does occasionally seem to give the impression that such a construction is indeed observed -- at least in the sense that this is one of those basic facts sanctioned by all theories. To the extent that he does do this he too seems insensitive to the theory-dependency thesis.

I turn now to a more direct consideration of the controversies between transformational linguists and Skinnerian behaviorists. It will not be my intention in the following to give even a partial explication of these two general positions. Indeed, I shall assume on the part of the reader acquaintance with at least some of the notions involved. My purpose here is not primarily to evaluate either of these two positions. It is rather to see if some of the claims and counter-claims can be better appreciated in the light of the theory-dependency thesis I have developed in the foregoing.

One of Chomsky's major attacks on Skinner has centered on the emptiness of Skinner's statement of one of the basic behaviorist laws of learning, the so-called Law of Effect.

It is worth quoting Chomsky here at some length.

> In Behavior of Organisms, "the operation of reinforcement is defined as the presentation of a certain kind of stimulus in a temporal relation with either a stimulus or response. A reinforcing stimulus is defined as such by its power to produce the resulting change [in strength]. There is no circularity about this: some stimuli are found to produce the change, others not, and these are classified as reinforcing and non-reinforcing accordingly." (62) This is a perfectly appropriate definition for the study of schedules of reinforcement. It is perfectly useless, however, in the discussion of real-life behavior, unless we can somehow characterize the stimuli which are reinforcing (and the situation and conditions under which they are reinforcing). Consider first of all the status of the basic principle that Skinner calls the "law of conditioning" (law of effect). It reads: "if the occurrence of an operant is followed by presence of a reinforcing stimulus the strength is increased" (Behavior of Organisms, 21). As reinforcement was defined this law becomes a tautology.[4]

Chomsky rejects Skinner's statement of the law of effect as being circular (tautologous). Yet Skinner, obviously

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[4] Op. cit., p. 36.

aware that such a charge is in the offing, explicitly says it is not circular. What is going on here? Consider the criterion Chomsky uses to condemn the law of effect. He requires that such an empirical law have separable criteria of identification for its relata. This is, of course, perfectly acceptable scientific methodology, for without it we can scarcely claim to have an empirical law with observational implications. Nevertheless, it is important to point out here that the independence required is not absolute but is rather a function of the descriptions used for the two relata. Suppose for example, that 'a caused b' is a true singular causal statement -- a paradigm case of an empirical statement. But, obviously, 'a= the cause of b', and by substitution we get 'the cause of b caused b' an analytic statement in which the description of the cause is <u>not</u> independent of the description of the effect. Yet one is under no compulsion to assert that the statement is not empirical.[5] The principle is generalizable. Just because the particular descriptions Skinner may offer are not independent does not mean that there are no other descriptions which might not be offered which would be independent.

This point is connected with the general discussion I gave earlier concerning operational definitions. It will be recalled that I showed that one man's reliability may be

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[5] This example is due to Donald Davidson, "Actions, Reasons, and Causes," <u>The Journal of Philosophy</u>, <u>LX</u>, 1963, pp. 685-700.

another man's validity. Skinner can very well take such a tack here. He may wish to claim that some specific instance of the law of effect really just is a reliably identifiable phenomenon. Chomsky on the other hand is demanding that the theory treat this as a validity problem. Yet since, as I have argued, there are no a priori reasons for supposing the one or the other, this is a clear case of Chomsky's having foisted his own observational categories, determined by his own theory, on his opponent. In other words, Skinner can very well accept the charge that his law of effect is tautologous and yet claim that it is not circular in any damaging way. This is because to say it is tautologous is merely to say the attributes thus identified are taken as defining attributes. Skinner would not need to deny that for other purposes, e.g. physiological, biological, linguistic, it may be useful to demand that his (Skinner's) observation categories, be further specified, i.e., operationally defined.

Indeed, such a defense would fit very nicely with the theory-dependency thesis I have been propounding. If there is no ultimate observation language, one can even make sense of Skinner's claim that "some stimuli are found to produce the change, others not, and they are classified as reinforcing and non-reinforcing accordingly." If one grants that different observational categories can be formed, then it makes sense to search for new ones. On one level of descrip-

tion it appears there are separable descriptions of varying stimuli. One finds a reliable connection between two, perhaps, and uses this as an operational definition of an observable in a new category of observables. There may well be other reasons for making such a decision. The circularity then noted in the law of effect by Chomsky is simply the decision to use a different set of descriptions as definitive of the observational categories in question.

But what is needed now is some sort of indication that Skinner does indeed hold such a position.[6] I think we can get a clue that Skinner does indeed, and non-damagingly, hold a kind of tautological law of effect from a consideration of that feature of his position which sets his version of operant conditioning off from other reinforcement theorists. I refer, of course, to his rejection of the drive-reduction account of reinforcement. Skinner is probably on good empirical ground in such a rejection, for an account of reinforcement in terms of drive-reduction faces serious problems in the face of the fairly widely accepted phenomenon of latent learning. Very briefly what this amounts to is that there is a range of phenomena e.g. curiosity which do not

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6 James E. McClellan, "B. F. Skinner's Philosophy of Human Nature: A Snypathetic Criticism," B. Paul Komisar and C. B. J. MacMillan, Psychological Concepts in Education (Chicago, Rand McNally, 1967). I believe McClellan argues that Skinner does hold such a position as outlined above, viz., that where others see two events, Skinner sees but one.

seem to depend on the customary physiological kinds of drives. Thus if a drive-reduction view is to be upheld, drives must be postulated which are not apparently identifiable separately from the behavior they are said to cause. In such a case the charge of circularity is in fact, well-placed. For the drive-reduction theorist does seem to have to use both the drive and the behavior in his system. But Skinner's notion of operant conditioning is precisely devised to obviate the need for finding some sort of drive. Rather the behavior naturally emitted, operant behavior, is shaped by the contingencies of reinforcement. I am certainly not claiming here that Skinner is very clear about this notion. I think he is not. But in at least one place he analogizes operants to a kind of blind variation of behavior and reinforcement to the fact that the behavior is not edited out by the environment.[7] In short he suggests an evolutionary model for the law of effect. But this means, crudely, something like the following. Suppose a bit of behavior, B_1, is emitted as an operant. Suppose it is "reinforced" in the environment, i.e., at least not edited out; it may even actually reduce a physiological or other drive. Suppose later B_2 is emitted which contains B_1 as a nested part. Suppose B_2 is also "reinforced". Now continue

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[7] B. F. Skinner, "Operant Behavior," in Werner K. Honig (editor), Operant Behavior: Areas of Research and Application (New York, Appleton-Century Crofts, 1966) pp. 12-32.

the process. At the next stage B_2 is a part of the organism's behavior repertoire, but notice that it is so because of the history of reinforcement. That is, in the hypothesized situation B_2 is what it is partly as a result of being a continuation of B_1, a formerly reinforced operant. When one thinks of the truly remarkable ways Skinner has actually arranged the contingencies of reinforcement to bring about the startling animal behavior he has, one can see, I think, that the identification of the animal's behavior depends on the training it received. The animal's behavior is an observable, yet its criteria of identification depend on training (shaping) in terms of discriminable stimuli connected to the schedules of reinforcement. The crucial point is that for most behavior which has not been intentionally shaped as in Skinner's animal behavior, we will not know the history of shaping. We will only observe the terminal performance and our categories of observation will generally be in terms of the terminal performance. The description we thus give of the terminal behavior may well be such as to render it impossible separately to identify the actual reinforcing events. This is not to say that the reinforcing events are not separately identifiable. Rather it is to say that they are lost in the unknown history of how the behavior came about. Put another way the observational categories we need to identify separately the rein-

forcement may well be physiological or neurological in character. The ordinary psychological categories are, as Chomsky alleges circular.

But if Skinner does hold such a view, we would expect him to urge that the identifying conditions for any kind of behavior must ideally involve the history of reinforcement. And indeed this seems to be precisely the case. For in discussing Chomsky's criticisms he distinguishes between two pieces of otherwise identical verbal behavior on the basis of the history of reinforcement.[8] Thus the actual causal sequence is for Skinner, a part of the criteria of identification of any observable category of behavior. For this reason, Chomsky's claim that the notion of reinforcement is empty, while possibly justified as against drive-reduction theorists is an _ignoratio elenchi_ when aimed at Skinner. It is an _a priori_ attempt to foist off Chomsky's categories of observation on Skinner who explicitly rejects them.

On the other hand, there is a sense in which Chomsky's criticism is justified. Let me get at this in the following way: In Chapter IV and in the Appendix I distinguish two different kinds of language games forming the basis of perception. The primitive language games were those into which one could only be trained and the existing set of them received an evolutionary justification. I believe that it is

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[8] _Ibid._, p. 29.

the training into such language games which Skinner's notion of operant behavior fits best. If one recalls Wittgenstein's remarks concerning how at the basic level I can only give the student practice and training and then compares this with Skinner's descriptions of shaping behavior, the analogies will be, I believe, quite striking.

The point is, however, that there were two different ways of learning a new language game (= coming to learn a new set of behaviors). The one was training or shaping while the other, radically different, depended on the prior existence of an already existing set of language games. This was the case of attempting to get someone to see something as something else where the original seeing was already mastered. This was the case of Augustine teaching someone a language presupposing he already had a language, only a different one. I think Chomsky's major criticisms are directed at this point. He is claiming, I think, that Skinner is trying to make all language learning fit the shaping model, whereas clearly most verbal behavior does not do so.

This point can be brought out in another way. One can grant Skinner his insistence on including the history of reinforcement in the identifying criteria for various behavioral concepts, but it does not follow from this that there are not other ways of conceptualizing the behavior as well. Indeed, since the actual history of reinforcement is

obscured in most of our ordinary categorizations of behavior, it would be an incredible piece of luck if our ordinary language categories matched the categories determined by reinforcement history in any way. What Skinner fails to notice is that these ordinary language categories have a "life of their own" in the sense that these categories can be and are used in teaching languages in the second sense noted above.

Once one grants the possibility that the concepts used as observational categories in ordinary language may combine bits and pieces of behavior which might belong to several different categories under operant behavior theory, one begins to get a feel for the reason so many investigators speak of the rule-governed nature of language. And it is but a short step from the rule-governed nature of language to that most elusive of linguistic categories -- meaning. What I am saying here is that Skinner does not appreciate the sense of rules in which Chomsky speaks of the necessity for the child to internalize the rules of grammar. Skinner seems to believe that someone's producing a sentence in a rule-governed way can proceed only through a conscious application of the rule.[9] He thus criticizes Chomsky on the grounds that a dog who has learned to catch a ball must have constructed the relevant part of the science of mechanics.

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[9] Ibid., p. 29.

This is simply to misconstrue Chomsky's sense of 'rule'. Chomsky's sense of rule is more open-ended than the laws constituting the science of mechanics and is generative as, for example, recursive functions are generative. In a certain sense which is easy to point to and yet hard to explain, rules are prescriptive whereas laws of mechanics are descriptive. The deduction rule of modus ponens is a good example.

Yet another way of getting at the distinction between shaping categories and what I will call ordinary language categories is afforded by the controversy in psychology over the question "what is learned?" This controversy is sometimes presented as a dispute between those who would assert that some sort of central underlying process is acquired as opposed to those who believe that various pieces of observable behavior are acquired. This is, however, misleading. For at least some of the controversy involves the choice of an appropriate set of observational categories without necessarily involving "underlying" processes or "hypothetical constructs" or "intervening variables" at all. It was pointed out by Campbell in 1954 that the acceptance of the position that all we have to go on in constructing our psychological theories are the responses of the organism does not thereby commit one to supposing that no central states can be legitimately inferred.[10] Nor does this imply

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10 Donald T. Campbell, "Operational Delineation of "What Is Learned" via the Transposition Experiment," Psychological view, 61, 1954, 167-174.

there is only one way of observationally categorizing the behavior which must serve as our grounding. In short, what is learned may go considerably beyond any simply categorization or combination or observed behaviors.

Consider one of the experiments discussed by Campbell. In this experiment a conditional finger movement was obtained through pairing a shock and a tone. The shock could be removed by an extensor movement of the finger. What was learned? An extensor finger movement or withdrawal of the finger? The question is open to experimental investigation. Turn the hand over and repeat the experiment. Now an extensor movement does not remove the shock, but finger withdrawal will. 90% of the subjects withdrew their fingers, 10% continued the extensor movement. The shaping hypothesis does very well in accounting for the 10%. It is not nearly so obvious how it will fit the 90%. The crucial question is, of course, what was shaped? It is the analogue of this question in verbal behavior and the seeming concommitant lack of answer that leads Chomsky to charge that Skinner's extension of operant analysis to verbal behavior is either false or a less precise metaphor than even ordinary language.[11]

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11 Charles Taylor, The Explanation of Behavior (New York, Humanities Press, 1964) also contains sustained attack on the ability of simple S-R mechanisms to account for complex behavior even in animals. Taylor's major strategy is to show the ad hoc character of the explanations introduced to handle these kinds of embarassments, to note the experimental im-

What is learned here is an action, not a "mere" movement from which one infers an action. It may be well to remind the reader here that in this case, at least, it is not necessary to consider the action as some kind of hypothetical construct or referring to some "underlying" property. (Cf. Chapter III.) There is no a priori reason why we cannot be said to observe directly someone withdrawing his finger.

The vagueness sometimes attributed to action categories can now be seen to have a perfectly plausible source. Recall the 90% who learned to withdraw their finger. Further discriminations are possible. Some may, according to their reinforcement histories actually have had muscle group transfer. Others may have had a more central part of the nervous system conditioned. Others may even have consciously adopted a plan to move their finger when the tone sounded. In short varying numbers of operant categories (those categories which include the particular reinforcement history) may be confined in one action category. If one restricts one-

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plications of these ad hoc explanatory principles, and to point out that where such experiments have been carried out the ad hoc principles fail in the same kind of way. Since any theory can, logically, be saved by adding epicycle on epicycle, only a thorough critique of the kind outlined above can ever discredit a theory. And even then, as Kuhn has pointed out, the discrediting will be effective only when a powerful alternative theory is available. See Thomas Kuhn, The Structure of Scientific Revolutions (Chicago, University of Chicago, 1962.)

self to typical operant analysis categories, of course, action categories will appear vague. On the other hand there is nothing at all vague about the action category per se.

Let me take another example. Consider two baseball pitchers, one left-handed, one right-handed. Suppose further that one has learned the game wholly by playing, reinforced only be natural contingencies; whereas the other has had the benefit of sustained coaching. Now suppose in a game both of them pick a runner off first base. It is hard for me to imagine two more different reinforcement histories and muscular movements and yet in terms of action category, these two men have quite obviously done the same thing.

Let me put the point in yet one more way. Skinner complains that we too often ignore the history of reinforcement which brings a particular terminal schedule of reinforcement into control.[12] In my terms he is complaining that our ordinary observational categories may mix up appropriate operant categories, as for example, in the baseball case above. Yet the fact is that we do use such categories, that they do have some survival value, that people do learn new pieces of behavior on the basis of ordinary categorizations, and that these categories do ignore the history of reinforcement.

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12 "Operational Behavior," p. 19.

It would be a most extraordinary piece of luck if, in spite of all of this, we could give a plausible operational analysis of the behavior based on ordinary action categories. This is not to say that such is impossible, only highly unlikely. Nor is this to say that operant analysis is not ultimately basic and terribly important. Indeed Skinner has probably shown us that a good deal more of our behavior can be given an operant analysis than we would ever have dreamed. However, the difficulties besetting an operant analysis of "higher" behavior, as Chomsky has pointed out, seen insuperable.

In short, I am suggesting that the usefulness of categorizations based on the results of behavior, where these results describe various organism-environment relations, has led, over the period of evolution, to the emergence of another method of learning than shaping. It is the method of learning which depends on already possessing a set of language games (Cf. Ch. IV). and it is the method of learning which has led Chomsky to posit the "unconscious construction" of grammars by the children who perceive them. If we recall Wittgenstein's problem concerning the infinite regress of rule, interpretation, rule, etc. we can see that given a standard interpretation we can describe ways of learning verbal behavior which make use of our presupposed ability to grasp the rule as standardly interpreted. Whereas

Skinner's shaping notion may help us understand how to stop Wittgenstein's regress, Chomsky's notion of grammars will help us be more precise in saying what actually goes on in most language teaching and learning, given an evolutionarily determined standard interpretation.

Let me now turn just briefly to one of Chomsky's most controversial points -- the claim that the facts demand that there be in each child a set of innate ideas containing a kind of universal grammar which can then be actualized in any one or more of the extant natural languages. One of the major criticisms of this position is that it names a problem without in the slightest explaining it.[13] Put in Wittgenstein's terms Chomsky ignores the regress problem and arbitrarily stops it with the metaphor of innate ideas. This names the problem but does not solve it. Such criticisms commit a similar sort of _ignoratio elenchi_ as Chomsky's charge of emptiness does _vis à vis_ Skinner. I have outlined this fallacy above in detail. Once again given the grammatical observational categories of the linguists it is simply beside the point to note that these categories are not those of the behaviorist.

Yet there is here, too, a grain of truth in the criticism. A fairly complete analysis of linguistic behavior will require the innate ideas metaphor to be cashed. It is

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13 Cf. Wiest, _op. cit._, Skinner, _op. cit._

legitimate to ask how the regress of rule, interpretation, rule, etc. is to be ended. I am not sure that Chomsky would deny this, but it is important to note anyway. The problem is that the behaviorists noting that the regress must be stopped attempt to stop it essentially within the lifetime and learning experiences of a single individual. Everything is to be explained in terms of individual shaping. The broadened possibilities of species learning and inheritance offered by evolution are seldom considered. Yet, in some respects, if the categories of things which can be shaped are considerably broadened, and the facts of evolution taken into account, the notion of shaping may yet be useful. We might well allow such things as judgments, perceptions, theories, etc. to be shaped by reinforcement contingencies.[14] Of course such categories almost surely do not contain the reinforcement history as part of their criteria of identification. This is obviously tied up with the "what is learned?" problem.

Sigmund Koch has summarized the preceding discussion very well. He says ". . . /D/efinition, at bottom, is a <u>perceptual training process</u> and that everything that we know

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[14] See, for example, Don Campbell, "Methodological Suggestions From a Comparative Psychology of Knowledge Processes," <u>Inquiry</u>, <u>2</u>, 1959, 152-182 and by the same author, "Evolutionary Epistemology" in P. A. Schilpp (ed.) <u>The Philosophy of Karl R. Popper</u>, in The Library of Living Philosophers (La Salle, Ill. Open Court, in press).

about the conditions of perceptual training and learning must apply to the analysis of definition."[15] The defining characteristics of observational categories depend on what we perceive and what we perceive depends on the observational categories. Skinner seems to emphasize categories which must include history of reinforcement so that the way observable behavior has been shaped stands out. This is no doubt one process of learning and corresponds to learning the primitive language games discussed in the Wittgensteinian analysis. Such a learning model will almost surely be most successful in explaining basic perceptual and discriminatory processes and least successful in explaining verbal learning. Chomsky on the other hand seems to emphasize categories which ignore history of reinforcement and instead are defined in terms of the outcomes of behavior vis à vis organism and environment. This model corresponds to the learning of new language games given some already played language games in the Wittgensteinian analysis. It would be expected to be most successful in explaining verbal learning and least successful in explaining the categories which ground verbal learning.

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15 Sigmund Koch, "Psychology and Emerging Conceptions of Knowledge as Unitary," in T.W. Wann (ed.), Behaviorism and Phenomenology (Chicago, Chicago University, 1964) p. 26.

In conclusion, then, I want to indicate three broad areas of empirical research where the two models may be in conflict and where current work in terms of one model may well be supplemented by work in terms of the other. First there is the distinction between first and second language learning. Where it not for evolution, one could predict that Skinner's model would most closely fit first language learning and Chomsky's second language learning. However, it looks very much as if Chomsky's ideas are more appropriate here. However, in the area of language readiness, e.g. discriminations, etc. one might look a bit more closely at the possibility that some of these abilities may be shaped. In particular this might be true if one allows a bit more breadth to the categories of what can be shaped.

As far as second language learning is concerned, if it is done bilingually, this is almost a paradigm case of Chomsy's model. If, however, someone who does speak a language is simply thrown into the milieu of a new language, some interesting comparisons should be available with first language learners. The former will probably exemplify Chomsky's model. Any deviations of his behavior while learning the rudiments from the behavior of a first language learner may well point to areas where operant analysis is appropriate.

The second area of investigation concerns certain possible distinctions within first language learning. From the

above analysis, one would suspect that certain concepts, the primitive observational ones, are most easily learned by a kind of broadened notion of shaping. It would be interesting to see which these are and whether they are invariant over cultures, learning techniques, etc. It would also be interesting to see if one could shape the more complex observables. A map of the ways in which the various perceptual categories are learnable would contribute to philosophical discussions in epistemology and philosophy of science. For if there are no philosophically justifiable basic observational categories, we can surely learn something about the most general features of the world from studying the psychologically and evolutionarily most basic categories.

Finally, if observation, categorization, and theorizing are all part of one continuum and react through evolution with one another and the shaping, editing environment, then cross-cultural studies of perception should be most useful. Again, taking the figurative intersection of the most basic categories across cultures across time, and across theories may well be the best and, indeed, the only way of answering that question of first philosophy -- What most generally, is there?

APPENDIX: SCIENCE AND METAPHYSICS: A WITTGENSTEINIAN INTERPRETATION

I

It is not an uncommon occurrence for teachers of philosophy to be asked at some point in introductory courses what metaphysics is. Nor is it uncommon, I think, for such teachers of philosophy to respond with some sort of paraphrase of the Aristotelian answer to this question; "Metaphysics," the eager student is told, "is the science of Being as such."[1] (Interestingly, the capital letter somehow manages to appear even in the verbal reply.)

But now, if the student is not wholly silenced, and especially if he is at all scientifically sophisticated, he may have a further query. How, then, is metaphysics different from physics? For surely physicists study the fundamental building blocks of nature and is this not the same as the study of Being as such? As late as 1965 one answer to this further query consisted in pointing out to the student that metaphysics is broader than physics.[2] The student was told that although physics is perfectly general with respect to its domain, if we wanted to find out, for example, about biological features of being, we would have to turn to biologists.

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[1] Aristotle, Metaphysics, Book IV, Ch. 1, 1003^{a}-18ff.

[2] D. F. Pears (editor), The Nature of Metaphysics (New York, St. Martin's Press, 1965), pp. 4-5.

Of course, this answer, as the skeptical student of today quickly perceives, will not do. In the first place it does not take into account the incredible advances in molecular biology which have occurred in the last few years. It seems today, as never before, that in some clear sense physics will be able to achieve the generality apparently required. And, of even more importance, even if all of science cannot somehow be unified, this result would allow the distinction between science and metaphysics to degenerate into a merely accidental feature rather than the essential distinction being sought.

No, if the philosopher is going to distinguish metaphysics conceived as the study of Being as such from contemporary science, he is going to have to bring up his big guns. And at least three of these big guns usually have to do with the following points:

1) The objects and, hence, the methodologies of science and metaphysics are fundamentally different. The former deals with empirical truth while the latter deals with conceptual truth.
2) Even granted a completely unified science, philosophy is a different order subject, dealing as it does with problems and conflicts which arise within and between science and common sense.[3]

[3] I realize that some philosophers, e.g. Gilbert Ryle, believe that a sharp distinction needs to be drawn between science and common sense. This conception seems nothing less than incredible to me. Surely nothing could be more

3) In any event, the tradition of separating science and metaphysics places the burden of proof on those who would assert their continuity. In other words, an argument, and a philosophical one at that, must be offered asserting the continuity of science and metaphysics.

After these points, suitably embellished, are made, the philosophy teacher can be confident of having silenced, if not convinced, all but the most obstreperous student. And yet the doubt lingers on. Can metaphysics <u>really</u> be distinguished from science? Many students (along with some unregenerate philosophers) continue to wonder. It will be the purpose of what follows to suggest that the scientifically minded metaphysician can find arguments in Wittgenstein's <u>Philosophical Investigations</u> which, if they are indeed sound, would serve to rebut the three major points advanced above.

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obvious than that science broadly conceived, is nothing more than a sophisticated extension of common sense investigation. However to argue this point in detail here would take me too far afield. I can only hope that it will be apparent that nothing I shall say in the sequel hangs on my ability to show the continuity of scientific and common sense investigation. The reason for this is that those who would draw the line between science and common sense do so in order to attach philosophy and especially metaphysics, to the problems arising from conflicts in common sense. But it must be obvious that even if such a move is legitimate, it does not suffice to distinguish science from metaphysics. Both would be separate from common sense, but not necessarily separate from each other.

Let me quickly enter some caveats to such an "unphilosophical" assumption. First, I am not claiming that the above three points constitute the sole means of drawing a distinction between science and metaphysics. Thus, even if I am wholly successful, I will not have shown that science _is_ identical or continuous with metaphysics. Nevertheless, these three points are quite crucial to all the attempts to draw the distinction which I have seen. So, if I can cast some doubt on them, I think a great deal will have been accomplished.

Second, I do _not_ claim that Wittgenstein himself (nor especially any of his disciples) ever used the arguments of the _Investigations_ to draw the kinds of conclusions I am trying to draw. However, in the next section I will outline a strategy of attack on the three above-mentioned principles which is such that I believe Wittgenstein argued for each of the major points of this strategy. In the following three sections, I will argue that Wittgenstein clearly does hold the positions outlined in the strategy.

Third, I grant that Wittgenstein explicitly argued against the kind of scientific metaphysics I am here defending. In the last section I shall examine this argument and try to show that it is neither convincing nor central to Wittgenstein's position. I will also argue that the central Wittgensteinian features which _do_ support my claim

help explain why Wittgenstein and his followers have never taken the final step I am urging be taken.

Finally, I will not have the space actually to defend in detail the Wittgensteinian arguments which I am claiming support scientific metaphysics. However, I believe that these arguments are among those most widely accepted and most influential in contemporary philosophy. Hence if I am successful, I will at least have presented the following dilemma: If philosophers accept Wittgenstein's arguments, they ought to be more patient with the student who cannot easily disentangle science and metaphysics. On the other hand, if they believe such a distinction is obvious they should relook at the Wittgensteinian arguments they all seem to accept.

II

I turn first to the strategy of attack. The first point was that empirical and conceptual truths must be distinguished -- assigning science to the study of the former and metaphysics to the study of the latter. This point will be attacked in two parts. First, I will deny that conceptual truths, whatever their analysis, are absolute and eternal and to be known only through some specialized philosophical method, i.e. metaphysics. This would move philosophy towards science, and will be explored in the next section.

Second, I will deny the applicability of the popular caricature of an empirical question's being decidable one way or the other by appeal to neutral experience (or a neutral observation language.) I will argue that there is no fundamentally clear distinction between theory and observation and hence no univocal way of deciding empirical questions by appeal to experience, i.e. the observable. This would cast doubt on the scientific method as something which could be clearly and distinctly specified. The particular way in which theory conditions observation is more nearly like the way philosophical method is often described, i.e. as providing a new way of looking at things. These two points would move science towards philosophy and will be discussed in Section IV.

As regards the claim that metaphysics is a different order subject than science or common sense, I challenge this by admitting hierarchical levels of investigation but denying that anything peculiarly metaphysical results from this hierarchy. In other words, I will deny that any other levels than appear in science need be invoked to explain common sensical and scientific conflicts. An investigation of this point will be undertaken in Section V.

Finally, the above investigation would constitute a "philosophical" argument in favor of the scientific metaphysics position -- albeit, not a conclusive one. For if

one can show that it is highly unlikely that we can clearly separate the empirical from the conceptual, ipso facto, we will be unable clearly to separate the scientific from the metaphysical. If one can show that the conflicts encountered in science and common sense need no more of an explanation than what is given by science and common sense, then a different order of metaphysical explanations will not appear necessary -- and, of course, the need for such metaphysical explanations has seldom been "obvious."

III

What then is a "conceptual" truth? It is presumably a truth concerning the relations of concepts, but this is rather vague. It is vague in two senses. First of all, it is very unclear just what a concept is, and a fortiori unclear as to just what sorts of relations these entities can enter in. But, second, it is also unclear as to whether the entity called a conceptual truth is like any other empirical truth, only limited to concepts, or whether it is significantly different in kind. In other words is a conceptual truth true because of relations similar to those which make "Copper conducts electricity" true -- the only difference being in the domain of discourse? Or are the relations which make a conceptual truth true somehow different?

To put the point in another way, a conceptual truth is a necessary truth and, at least *prima facie*, necessary truths have two separate aspects of philosophical interest -- their truth and their necessity. I am not denying that some theories of necessary truth may actually fuse these two aspects. However, if such is the case, such a fusion must be made explicit. For it seems to me that all too many discussions of necessary truth are completely vitiated by not keeping these two aspects distinct.

But now consider the two most popular theories of necessary truth -- the realist and the conventionalist. If we observe the above distinction between necessity and truth, we can represent the possibilities by the following matrix:

	Realist	Conventionalist
Necessity	X	X
Truth	X	X

In short, one could, *prima facie*, combine a conventionalist account of truth with a realist account of necessity. Such a position would attribute the truth to some set of conventions or meaning postulates or what have you, while attributing the necessity of the truth to some real connection in the world. I do not know of any philosopher who has held such a position, but it does seem *prima facie* possible.

Of more importance, however, is the result of adding on to the above matrix the requirement that the account of

truth given must satisfy a Tarski-like criterion. Most philosophers today seem to accept some version or other of Tarski's truth criterion, the particular one depending on their view of the appropriate truth bearer -- sentence, utterance, etc. Thus, exploring the consequences of accepting a Tarski-like criterion seems most promising. In fact, what happens in such a case is that the conventionalist account of truth is thereby ruled out. That this is so has been argued in several places.[4] Bascially the argument is this: According to Tarski,

> "Copper is copper" is true if and only if copper is copper.

Thus the truth condition is given by the words to the right of "if and only if". But to assert that this condition is some sort of convention to use words in a certain way is simply to confuse use with mention. For "copper is copper" is used, not mentioned, in its occurrence to the right of the

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4 Henry Veatch, Two Logics (Evanston, Northwestern University, 1969), Ch. V.
Roderick Chisholm, Theory of Knowledge (Englewood Cliffs, N. J., Prentice-Hall, 1966), pp. 82-84.
Gilbert Harman, "Quine on Meaning and Existence, I", Review of Metaphysics XXI, Sept., 1967, pp. 130-131, summarizes Quinean arguments against a conventionalist theory of truth. These arguments depend implicitly on accepting a Tarski-like criterion.

"if and only if" in Tarski's criterion. As such these words are about copper in the world. Furthermore the sentence, "copper is copper", will not be true if the condition is not satisfied, that is, if copper is not copper. Notice too that such a commitment to the Tarski criterion renders irrelevant the remark that it is conventional that we use the sign-design, 'copper', as we do. Of course that is conventional, but it does not touch the point that whatever the sign-design, it is used, not mentioned, in its occurrence to the right of the 'if and only if'.[5] In short, commitment to a version of the Tarski criterion is, in the absence of special pleading, commitment to a realist account of truth.

Notice finally that to say, "Yes, but 'copper is copper' will be true no matter what," is not to object to the above argument. It is at best to call attention to the necessity feature of necessary truth. In other words it still remains to be seen which account of necessity, not truth, is correct, the realist or the conventionalist. The realist account of necessity attributes the necessity of conceptual truths to "real" necessary connections in the world. The conventionalist account of necessity attributes the necessity of conceptual truths to our conventions governing the use of language.

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[5] I have, for simplicity's sake ignored the necessary relativization of the truth-predicate to a language. Nothing in the above argument hangs on this relativization.

Let me sharpen the issue by considering the following criterion of the necessary character of a necessary truth: The denial of a necessary truth is self-contradictory. Following customary practice I distinguish between those necessary truths which are explicit logical truths, e.g. 'A male sibling is a male,' from those which are not, e.g., 'A brother is a male sibling.' Let me consider the explicit logical truths first. The customary syntactical sense which can be given to the notion of contradiction in the case of explicit logical truths has led many philosophers to consider this class of necessary truths to be unobjectionable, albeit philosophically trivial. I do not want to dispute this standard view. I do, however, want to point out that as soon as the syntactical characterization of contradictoriness is associated with the standard <u>semantical</u> characterization in terms of interpretations, satisfaction, etc., there is a sense in which explicit logical truths are little better off with regard to the clarity of the notion of necessity involved, than are the so-called implicit necessary truths. For, lacking a semantics, it appears simply arbitrary to rule that 'P & -P' cannot be asserted. But once the semantics is added as, for example, by means of the notion of interpretations of a formal calculus, the justification for the non-assertibility of 'P & -P' usually falls back on the notion of the inconceivability of the situation thus described. But to make 'conceivability' a logical as opposed

to a psychological notion, we seem driven back to the notion of conceptual truth we were trying to analyze.[6]

In other words there is a sense of necessity very similar to the inconceivability-of-the-denial which seems to be presupposed even by the explication of the contradictoriness or inconsistency of the denial of explicit logical truths. Someone may simply never utter the syntactic form of, e.g. the principle of noncontradiction, nor anything equivalent to it. The sense in which we feel they nevertheless must accept the principle is precisely the presupposed sense I am talking about.

The use of the inconceivability-of-the-denial criterion is even more obvious in the case of implicit necessary truths. The denial of "All brothers are male siblings" is "there are brothers who are not male siblings." Such a statement is not syntactically self-contradictory as it stands. Still following fairly standard procedure, one might argue that it can be "reduced" to an explicit logical truth with its attendant notion of the syntactic self-contradiction of the denial by substituting synonyms for synonyms. Of course, as Quine has pointed out, such a move only serves to push back the problem.[7] For in explicating synonymy we tend to fall back

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6 See Benson Mates, Elementary Logic (New York, Oxford University Press, 1965), 5-7, for a discussion of the circularity involved, although from a different starting point.

7 W. V. O. Quine, "Two Dogmas of Empiricism," From a Logical Point of View, (Cambridge, Harvard University Press, 1961).

on some such notion as inconceivability-of-the-denial. Alternatively, we might appeal directly to the inconceivability-of-the-denial criterion in attempting to explain why the implicit necessary truth is necessary.

The purpose of rehearsing the above well-known moves is to remind one of the fact that sooner or later all explications of necessary truth are linked up with a common criterion. In my presentation I have chosen to link these with the "inconceivability-of-the-denial" criterion. No doubt other linkages could be made. The point is that once we reach the favored basic criterion, we can investigate the various theories of necessity with respect to the account each gives of this criterion -- in this case what it is to be able to conceive or fail to be able to conceive of a particular situation. Note that unless the analysis is carefully given, it will simply end up begging the question. For it is all too easy to analyze "inconceivable" in a way which makes use of precisely the modal notion, or one of its logical correlates, for which we are trying to give an account. On the other hand it is crucial to be able to give an account of "conceivability"; for almost all philosophical controversies turn on the acceptance or rejection of a putative conceptual truth and hence on the conceivability of its denial.

Thus, the realist account of necessity postulates necessary properties <u>in the world</u> to ground conceptual truths.

P will then be a necessary truth just in case -P is inconceivable. -P will be inconceivable just in case it asserts that some real connection in the world does not hold. Unfortunately the claim is thus far ambiguous. Is the claim of a "real" connection simply a restatement of the fact that a connection holds which does indeed hold? If so, the realist will have to admit that all truths are necessary truths or none are. If not, he will have to tell us what 'real' means here, a task Hume so effectively argued was impossible, or he will have to change substantially the locus of necessity to something like, e.g., generality. He might then say that real connections are just connections, but we can distinguish necessary from contingent in virtue of the greater geneality of the former relative to a particular purpose. This would render the distinction one of degree, not of kind. (As will become apparent below I believe Wittgenstein to hold a modification of this kind of position). However, no historical realists that I know of wish to take this way, as it seems to amount to giving up the explanatory value of the concept of necessity as a peculiarly philosophical term. It would in fact be to give up the attempt to distinguish philosophy from science in terms of the <u>kinds</u> of truth with which each deals. It is for reasons such as these that I believe Hume rejects the realist account of necessity.

Let me now turn to the conventionalist account. His interpretation of the inconceivability-of-the-denial criterion will be as follows: A statement is inconceivable if it is ungrammatical in a certain sense. The sense is that it simply does not follow the rules for correct use of the concepts involved. The problem for the conventionalist is in giving a non-trivial account of 'correct'. A non-trivial syntactical account of the rules of grammar can be given, but this is, as I have shown, dependent for its intelligibility on being able ultimately to give a non-trivial semantic account. A non-trivial semantic account of 'correct' can be given in terms of the empirical facts of actual linguistic usage, but this move, like the similar one made by the realist, is to give up the attempt to use the conceptual-empirical distinction to buttress the metaphysics-science distinction. In short, the conventionalist seems able to purchase a clear sense of necessity only at the price of making the necessity completely arbitrary. But this move founders on the rock of the clearly non-arbitrary character of necessary truths.

It is my belief that Wittgenstein proposes a view of conceptual truth which neatly avoids the problems of both the conventionalist and realist positions while at the same time maintaining their strengths. In doing this, I believe he shows that the empirical-conceptual distinction cannot be drawn absolutely, once and for all, but is rather relative to a set of language games which themselves are not absolute,

being in turn dependent on a form of life.

Wittgenstein's rejection of the realist position on necessity is well-known. I will mention just two indications of this rejection. The first is textual. In the famous section 66 of the Investigations in which he introduces the notion of family resemblances in criticizing the search for the "essence" of games, he says ". . . look and see whether there is anything common to all."[8] And of course there is not. In fact, the admonition to look and see is applied throughout the work to 'meaning,' 'understanding,' etc. and in no case is an essence to be found. But if anything is a conceptual truth, it is an essential predication and Wittgenstein, having looked, found nothing in the world to justify this necessity, no real connections.

This also indicates the philosophical reason for Wittgenstein's rejection of the realist account of necessity. If we accept his "meaning is use" doctrine and the claim that "Essence is expressed by grammar" (371), we can see that Wittgenstein will apparently have to locate the source of necessity in language.

Considerations such as the above have led many philosophers to attribute a conventionalist account of necessity to Wittgenstein. And indeed there are many places where he gives just

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8 L. Wittgenstein, Philosophical Investigations 3rd edition (New York, Macmillan, 1968). In the following I shall use the by now standard practice of referring to the Philosophical Investigations by a number simpliciter to indicate the section number in Part I and by a page number for references in Part II.

such an impression (e.g. 116, 199, 492, p. 185). What traditionally looks like a conceptual truth, Wittgenstein often claims is a remark on the "grammar" of the concept involved. In short it looks as if Wittgenstein is claiming that to accept a statement as necessarily true is to note the grammar of its employment in language which in turn is to recognize that ". . . this language like any other is founded on convention." (355)

Despite this evidence several writers have recently denied that Wittgenstein does in fact hold such a standard conventionalism.[9] He says, "When I obey a rule, I do not choose. I obey the rule blindly" (219). And in response to his interlocuter's asking if he is a conventionalist he says, ". . . they [human beings] agree in the language they use. That is not agreement in opinions but in form of life." (241)

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9 Michael Dummett, "Wittgenstein's Philosophy of Mathematics," The Philosophical Review, LxVIII (1959), 324-348; and Barry Stroud, "Wittgenstein and Logical Necessity," The Philosophical Review LXXIV (1965) 504-518. Indeed, it will be obvious in what follows that I am heavily indebted to Stroud's treatment of this point. However, I think I can advance additional arguments for the conclusion that Wittgenstein accepts a compromise between realism and conventionalism as well as put this conclusion to a use which Stroud perhaps did not foresee (and would perhaps reject). Both Dummett and Stroud base most of their arguments on passages in the Remarks on the Foundations of Mathematics. I think the point that Wittgenstein rejects standard conventionalism can also be made with reference to the Investigations.

But of even more importance than these textual indications, which can perhaps be given alternative interpretations, is a philosophical reason. If Wittgenstein were a standard conventionalist, that is, if for him necessity is grounded in our agreement to use language in a certain way, then the following triad (185) should be inconsistent:

A. A person understands some linguistic formula, i.e. he has adopted and assented to the conventionally formulable or formulated semantical rules governing the meaning of that formula.

B. He performs an action which we would ordinarily say constituted a mistake in applying the rule, e.g. he writes down . . . 998, 1000, 1004, 1008 . . . at the order "+2."

C. He does <u>not</u> acknowledge that he has made a mistake, i.e. he says that's what he thought the linguistic formula meant. He believes he has acted perfectly rationally.

The reason this triad should be inconsistent on the conventionalist view is that it is part of the rules of use of 'understand' that B or C or both fails to hold. It might even be urged that the man could not really have "adopted and assented to the conventionally formulated semantical rules" and yet fail to acknowledge his mistake. For "adopted and assented" just <u>mean</u> that he must acknowledge such a mis-

take. And yet, it seems terribly obvious that this reply is nothing but a piece of linguistic legislation. The beauty of Wittgenstein's description of the case lies in its pointing out that however odd the situation is, it is not obviously a linguistic oddity. It is rather more like the "one in which a person naturally reacted to the gesture of pointing with the hand by looking in the direction of the line from fingertip to wrist, not from wrist to fingertip." (185)

That Wittgenstein meant to assert the consistency and comprehensibility of the above triad can be brought out in yet another way. Suppose we think of the formula, '+2' as a syntactic string in an uninterpreted calculus. Then the meaning of this sign-design is conventionally fixed by the interpretation we give to this sign-design. A man will "understand" '+2' just in case he gives it this conventional interpretation. Looked at in this way, the conventionalist position seems almost inevitable. Once the interpretation has been made, i.e., the conventions established, the triad must be inconsistent. How can Wittgenstein believe that it is consistent?

The answer lies, I believe, in Wittgenstein's doctrine that rules (conventions) can always be variously interpreted (85).[10] Notice that this cuts much deeper than the somewhat

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10 See Wilfrid Sellars, Science, Perception and Reality (New York, Humanities Press, 1963) Ch. 11, for a similar discussion of the crucial notion of the variability of rule-interpretation.

trivial observation that the sign-design '+2' can be given different interpretations. It rather means that the sense of '+2' depends not only on the interpretation, but also on the way the interpretation is given.[11] Indeed if one is to take seriously Wittgenstein's remarks concerning the role of what seems "natural" in the situation described by the triad, we must go even further. We must assert, in effect, that the background natural language, which is and must be assumed even to state the notions of calculus and interpretation, is itself a set of rules which can be variously interpreted. Thus the seeming inconsistency in the triad, arises from a too-narrow conventionalist view of language. As long as we are assuming that we can assign a canonical role to some language (ideal or ordinary) in the sense that what the rules of that language say is "natural", then the triad will appear inconsistent. But if we see that the canonical nature of any language is relative to what the users of that language find it natural to do, we can see that the triad is consistent for the man who finds it natural to go in that way.

The conventionalist appeal to rules for the correct use of terms is thus not ultimately explanatory, for we want to know of two people who react in different ways to an order which one is right? Which one is following the rule, the

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11 See Benson Mates, op. cit., Chapter 5, for a discussion of the difficulty of giving a canonical form for interpretations of a formal calculus.

convention? (206). But if rules can be variously interpreted then the standard conventionalist appeal to rules as the source of necessity fails. For the rules, even if they are rules of the metalanguage, do not carry their own interpretation on their face. (86).

Faced with this apparent rejection of the standard conventionalist position by Wittgenstein, Dummett attributes to him a radical conventionalism.[12] This radical conventionalism is such that it must constantly and at each instance be renewed. In other words, even in a mathematical proof, the necessity of each step consists in our deciding at that particular time to treat that very statement as unassailable. If rules can always be variously interpreted, then in any particular case they must be given an application then and there.[13]

Although such a move would serve to block the problem raised for standard conventionalism by the ever present possibility that rules may be variously interpreted, it is quite clearly not the route that Wittgenstein takes. For in discussing the language game of 185 mentioned above, he says "It would almost be more correct to say, not that an intui-

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12 Op. Cit.

13 It also seems to me that Chihara interprets Wittgenstein in a similar way. His idea that, for Wittgenstein, different people may have different concepts of the "same" notion (same formula) seems to me comparable to the requirement constantly to interpret the formula in each individual case. See Charles S. Chihara, "Mathematical Discovery and Concept Formation," The Philosophical Review LXXII (1963) 17-34.

tion was needed at every stage, but that a new decision was needed at every stage." (186) (My emphasis.)

But if Wittgenstein rejects the interpretation Dummett wishes to foist on him, what is his answer to the constantly present possibility of variable interpretation or rules? I think Stroud's notion that the key is to be found in Wittgenstein's appeal to forms of life and natural history is the answer.[14] Let me begin by quoting Wittgenstein at some length.

> 201. This was our paradox: no course of action could be determined by a rule, because every course of action can be made out to accord with the rule. The answer was: if everything can be made out to accord with the rule, then it can also be made out to conflict with it. And so there would be neither accord nor conflict here.
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> It can be seen that there is a misunderstanding here from the mere fact that in the course of our argument we give one interpretation after another; as if each one contented us at least for a moment, until we thought of yet another standing behind it. What this shows is that there is a way of grasping a rule which is <u>not</u> an <u>interpretation</u>, but which is exhibited in what we call obeying the rule and going against it in actual cases.

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[14] <u>Op. cit.</u>

I take it that Dummett's proposal to substitute a constantly recurring decision as to application in the place of an appeal to a standard -- in the place of being in accord or in conflict -- is the position of the first paragraph. This is clearly rejected by Wittgenstein. In its place, he suggests that there is a way of grasping the rule which is not an interpretation but which is exhibited in actual cases.

But what is this way by means of which Wittgenstein hopes to block the infinite regress of rule, interpretation, rule, etc.? He says "The common behavior of mankind is the system of reference by means of which we interpret an unknown language." (206). "What has to be accepted, the given, is -- so one could say -- forms of life." (p. 226). But what is a "form of life"? And in what sense is it basic? Wittgenstein gives us the answer in section 208 and amplifies and defends it in the following sections through 241.

To see what a "form of life" is, one needs first to distinguish it rather sharply from one sense of language-game.[15]

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[15] For a detailed defense of this necessity, see the excellent article by J. F. M. Hunter, "Forms of Life" in Wittgenstein's Philosophical Investigations," American Philosophical Quarterly, 5 Oct. 1968, 233-243. I also believe that the adumbrated account of "form of life" which I offer above is similar to Hunter's own "organic account". "...[T]he sense I am suggesting for it [form of life] is more like "something typical of a living being": typical in the sense of being very broadly in the same class as the growth or nutrition of living organisms, or as the organic complexity which enables them to propel themselves about, or to react in complicated ways to their environment." p. 235.

The distinction I wish to draw is the one hinted at by Wittgenstein in 23. There he contrasts the multiplicity of language games with the larger activity or form of life. The speaking or using of language, the playing of the language-games is part of the form of life. It is this contrast between the total activity and behavior of a human organism on the one hand, and certain games, language games, with end-points, sets of rules, etc. on the other to which I wish to draw attention.

Put in terms more familiar to logicians, a form of life is a metalanguage in use, while a language game ranges from an uninterpreted formal calculus through an explicitly formulated and interpreted calculus as an object of study to a part of the metalanguage itself either conceived as an object or as a part of the metalanguage in use. It is the distinction between the active use of the metalanguage as a form of life on the one hand and any part of that language considered in a non-active way as an object of study on the other that is here crucial.

But this way of stating the problem is misleading. For when we think as logicians, we constantly feel that we can equally make the metalanguage an object of study. We can make its implicit rules explicit by giving them an interpretation in the same old way. What Wittgenstein does for us here is to point out that such a procedure is not ultimately explanatory. To avoid an infinite regress of the interpretation of the rules of a language we must, as noted above

(201), stop the regress by an exhibition of actual performance. It is this function that a form of life performs. Wittgenstein has simply pointed out to us that we need something like the notion of a metalanguage <u>in use</u> to make sense of the results we can obtain using the metalanguage -- object language distinction. For any investigation, there is an assumed, non-investigable, language in use which enables us to frame out investigations. But now how can this form of life, this metalanguage in use, serve as an explanation of the grasping of rules which is not an interpretation but which is exhibited in the actual cases?

The answer is really quite simple. It consists of two parts. In the first case, if we already know a language, then we can appeal to that language and our understanding of it to explain how a particular rule is to be followed. In other words <u>within</u> an established language game as played, as a part of a form of life, we can always use the already established rules and conventions in use to explain a new rule or convention. It is this possibility which Wittgenstein believes misleads those who would base their theory of language on ostensive definitions. "And now, I think, we can say: Augustine describes the learning of human language as if the child came into a strange country and did not understand the language of the country; that is, as if it already had a language, only not this one. Or again: as if the child could already <u>think</u> only not yet speak. And 'think' would here mean some-

thing like 'talk to itself'." (p. 15-16). Their mistake is in believing that what one can do assuming a language game as played, a part of a form of life, one can also do in general, without such an assumption.

But in the absence of a form of life, we do not use ostensive definition, we do not explain a formula by another forumla. Rather we train people into our form of life, into the general behavior of mankind. (86, 143). ". . . /I/f a person has not yet got the concepts, I shall teach him to use the words by means of examples and by practice -- and when I do this I do not communicate less to him than I know myself." (208) The qualification is crucial. There is nothing over and above the practice and examples; nothing in the situation, nothing in me. I train the student into my (our) form of life. And "our pupil's capacity to learn may come to an end." (143). He may be untrainable in certain areas.

But what is the force of this possibility? "I am not saying: if such-and-such facts of nature were different people would have different concepts (in the sense of a hypothesis). But: if anyone believes that certain concepts are absolutely the correct ones, and that having different ones would mean not realizing something that we realize -- then let him imagine certain very general facts of nature to be different from what we are used to, and the formation

of concepts different from the usual ones will become intelligible to him." (p. 230) To understand this passage, one needs, I think, to pay attention to yet another distinction in Wittgenstein's use of language-games. The distinction is between an interpreted language game which it is physically possible to play and one which it is not physically possible to play. We cannot train our pupil into the latter, either because of some fact about him or some general facts of nature which render that interpreted language game unplayable. The above passage is hinting at the contingent conditioning of our concepts and the very general empirical grounding of these concepts. People may be untrainable in just the sense that they may be organisms which it is impossible to train. And this "impossibility" is not easily classifiable as either contingent or conceptual despite its ultimate dependence on the very general facts of nature.

Let me sketch just one example of this ambiguity. Imagine a being like Hitler, although even more evil and untrainable morally. Is such an organism a thoroughly evil person or is our tendency to use the phrases "beast," "animal", not so metaphorical after all? I take it that Wittgenstein is, in the above passage pointing out to use the interplay between the conceptual truths expressing our categories in use and the very general facts of nature on which these categories depend.

But now let me return to the question of the status of necessity in Wittgenstein. His proposal is really two-fold. Assuming a form of life as given, we can make sense of the necessity of a proposed conceptual truth by an appeal to the conventions and rules governing the way the language games which are a part of this form of life are actually played. Most modern ordinary language analysts are thus right in looking at "what we would say if."[16] And, of course, it is no accident that almost one and all, these philosophers hold some sort of linguistic or conventionalist view of necessity. A person has the "right" concept or is following a rule "correctly" if he is behaving in accordance with the form of life into which he has been trained. Further, since most of us have been trained into that form of life, we can, in a certain sense, perceive its rules without undertaking an empirical investigation. In particular we can do this for the very general rules which, ex hypothesis, would be assumed as part of the form of life which gives structure to the empirical investigation. Thus in a certain sense the conceptual-empirical distinction can be drawn within an actually played language game. The necessary truths are those which are treated as necessary within this actually played game.

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16 E. G. Stanley Cavell, "Must We Mean What We Say?" in V. C. Chappell (ed.), Ordinary Language (Englewood Cliffs, N. J., Prentice-Hall, 1964).

However, as critics of conventionalism and ordinary language analysis have pointed out, there is the danger of taking these parochial (to an actually played game) results as somehow the "absolutely correct ones."[17] It is at this point that Wittgenstein's admonition to consider the very general facts of nature as being slightly different comes into play. When we cannot assume a given form of life or language game as played, then the search for necessity is meaningless <u>except</u> in the sense that these most general features of the world almost assuredly have conditioned the range of languge games which it is physically possible for us to play. After all, evolution assures us that those pupils whose capacity to learn (to be trained into a form of life) fairly quickly and fairly often comes to an end are not likely to survive. Thus the realist too is right in his contention that it is the features of the world which are responsible for the necessity of necessary truths. These features need not themselves be necessary. Rather it is through the screening function that these features play with respect to the physical possibility that certain forms of life will survive that they are responsible for necessity.

Furthermore, it is, I think, an open question as to whether or not there are any identity criteria for forms of life. Thus, I think it is simply undecidable at present whether

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17 See, for example, Benson Mates, "On the Verification of Statements About Ordinary Language," in Chappell, <u>Ibid.</u>

the general features of the world have already so limited the language games we can successfully play that we can justifiably say there is only one form of life or not. At any rate there is a serious epistemological question concerning the supposition of more than one possible form of life. For if I am correct, it is a given form of life which would allow us to understand this possibility in the first place by providing the background against which we would test our concepts concerning an alternative form of life. Thus in the largest sense, we are, epistemologically, locked into the form of life we have. However, it may be very useful not to construe 'form of life' quite so broadly. If we don't, we may be able to make sense, for example, of the current controversies surrounding scientism and humanism as actually misguided. These two movements might be different forms of life and hence irreconcilable by anything but an evolutionary process.

Interestingly, this account shows how conventialism and realism feed on one another's errors. As conventionalists we ought to be able to describe alternative language games from the ones we play, and if we try, they sound very much like Wittgenstein's woodsellers[18] or like the student who goes on . . . 998, 1000, 1004, 1008, . . . Yet we are not comfortable with such examples for we have been trained into

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18 Remarks on the Foundations of Mathematics, I., 149.

a form of life such that we cannot actually conceive such situations. (How many readers felt more than a little uneasy with my claim that the triad in 185 was clearly consistent?) We cannot conceive the situation because it is expressed in our language games which do not allow for such aberrations. So we are tempted to say that such conventionalistic accounts beg the question by presupposing the very notion of absolute consistency which it was part of our job to explain.

Then we are tempted by a realist account, but try as we might, we are unable to find the necessity in the world in any but an empirical sense. Yet we properly feel that the role of necessary truths must be played or else we will not even be able to make sense of an empirical investigation. And around we go again. It should be clear now, I think, that the circle can be stopped by taking a modified conventionalist position on actually existing language games and a modified realist position on the evolution of language games as a form of life. The genetic fallacy is then only a fallacy with the existing games. It is not a fallacy with respect to the evolution of language games.

To summarize this section, Wittgenstein has a theory of conceptual truth which renders the conceptual-empirical dis-

tinction relative to a presupposed language game actually being played. In this respect we can distinguish within the form of life into which we have all been trained between science and metaphysics. Within the game of philosophy and science as now played there are clear conceptual parts and clear empirical parts and a continuum in between. But a line between the two, while it can be drawn for particular purposes within a language game, cannot be drawn absolutely. In this latter respect, the respect in which science and philosophy are both instruments for coping with the world, there will be no hard and fast line between them. I conclude then, that insofar as a clear distinction between empirical and conceptual truths is presupposed by the attempt to separate metaphysics from science, this attempt can in a sense succeed, but in a larger sense will fail.

IV

It is a popular view of science that its job is to apply varying theories, uninterpreted calculi, to the neutral unproblematic data of observation. The true theory would then be the one which best "fit" this neutral data. (Specifying just what constitutes this "fit" turns out to be rather technically complex but this is irrelevant here.) Perception then would be primarily concerned with establishing what is the case, with assembling the data preparatory to theorizing.

Whatever preceptual errors we might make would be explained with reference to some kind of "correct" and theoretically neutral perception.

But if, as I have argued in the preceding section, Wittgenstein rejects the possibility of drawing a line between the conceptual and the empirical, then this indeterminacy should be reflected in perception as well. Notice, too, that the rejection of the distinction between conceptual and empirical is a radical rejection. There is no line to be drawn. It is not just that the line is somehow hazy. The latter, weaker, position would be compatible with the standard view of perception sketched above. Thus phenomena such as "seeing things that aren't there" or "reading something into the situation," or "failing to notice that aspect," are, on the standard view, all to be explained as failures or errors of perception. On the more radical view, there remains room for some of these "perceptual" errors to be explained as "conceptual" errors.

The radical rejection of the empirical-conceptual distinction involves, I think, the realization that there are unmistakably cognitive elements in perception (as well as perceptual elements in cognition.) One would therefore, be led to expect Wittgenstein to reject the standard view of perception. One would also expect him to urge that there is

something cognitive in all perception. Yet because of the empirical limits imposed by our being trainable into only a few forms of life and probably trained into only one, one would also expect Wittgenstein to set some limits to the cognitive interpretations which can be imposed on perception. I think that these predictions are precisely borne out in his discussion of 'seeing' and 'seeing as'.

Wittgenstein introduces his discussion of 'seeing' and 'seeing as' with reference to a typical schematic box-figure (p. 193).[19] He asks us to consider the contrast between some simple bare-bones kind of seeing of the illustration on the one hand and what we see the illustration <u>as</u> in the context of its appearance. Now we see the illustration as a box, now as a wire frame, now as a solid angle made of boards. The question then is, just what is this phenomenon of "seeing as" and how is it related to just plain ordinary seeing?

The suggestion which perhaps most naturally appears is that 'seeing as' is to be analyzed as seeing with an interpretation. Such a view would accord nicely with the standard scientific view of observation. Wittgenstein, however,

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19 I do not consider Wittgenstein's more famous example of a duck-rabbit (an ambiguous picture); for , although I think the particular example is immaterial, someone might easily accuse me of picking a special case. It is for this reason I have chosen the box-figure. Actually I think any object whatsoever will do, e.g., the cathode ray tube used by N. R. Hanson, <u>Patterns of Discovery</u> (Cambridge University Press) 1965, p. 15.

rejects this view for a very interesting reason. If such an analysis were correct, one ought to be able to specify the seeing simpliciter which forms the basis for the then-added interpretation. (p. 194). But this is just what we cannot do. We cannot describe the perceptual content of seeing in any ultimate non-interpretive terms. The failure of the phenomenalists' search for a neutral sense-datum language underscores this point.

But what then is the proper analysis of 'seeing as'? And what is its relation to seeing? We must, Wittgenstein tells us, "distinguish between the 'continuous seeing' of an aspect and the 'dawning' of an aspect." (p. 194). 'Seeing as' will refer only to the latter. (pp. 195, 206). Once the distinction is drawn, we can see that ". . . the flashing of an aspect on us seems half visual experience, half thought." (p. 197) . . . "an amalgam of the two, as I should almost like to say. The question is: why does on want to say this?" (p. 197).

This is indeed the question! Why is seeing as an amalgam of seeing and thinking? It is not seeing some object and then adding an interpretation. Wittgenstein has already rejected that possibility. (See also, p. 200). I seem to see something different each time for, "to interpret is to think, to do something; seeing is a state." (p. 212). But what kind of a state? ". . . What I perceive in the dawning of

an aspect is not a property of the object, but an internal relation between it and other objects." (p. 212).

An internal relation between it and other objects! But now we are back on familiar ground. For "internal relations" are conceptual truths concerning the categorization of various objects. They are generic identity criteria. And if what I have urged in the preceding section is correct, these conceptual truths are such as a result of our having been trained in a language game. I there took as a paradigm case of necessary truths, the criteria for the application of a concept. The question there was how one could tell that someone had the concept "right". In order to stop a possible infinite regress of rules and interpretations as an explanation of the correctness of a concept, Wittgenstein urged that a form of life be taken as the given, as a way of grasping a rule which was exhibited in actual practice. In turn, the form of life was taken to be a very general language-game <u>as played</u>. But, of course, a good part of a language game as played will be connected with actual perceptual experiences -- at least with the perceptual parts of the language. Thus getting a perceptual concept, X, "right" and perceiving x's are all wrapped up with each other.

But now we must recall the two <u>different</u> ways in which a concept can be gotten "right." It can be interpreted in

terms of an already given language game as played, or else we can simply be trained into its use. Seeing as when contrasted with seeing simpliciter fits the former characterization. When we see X as Y, we place the perceived object originally categorized as an X into the language game which is the necessary condition for its being categorized as a Y. Seeing as marks the transition from one language game to another. It marks the taking of an object associated with one set of internal relations and placing it in another set. But of even more importance, this view tells us what it is to be an object in a set of internal relations in the first place. It involves at its most basic level, having been trained into a language game.

But as there are empirical limits to the language games which can be played, so there are limits on the ways in which any object can be seen as something else. We can only see something as something else if we can put it into different patterns -- if we can put the duck-rabbit into a picture with ducks or a picture with rabbits. It makes no sense, Wittgenstein says, to say we see a knife as a knife simply because knives function almost exclusively in one language game (p. 195). Yet even here one could no doubt invent a language game in which it would make sense to say of someone that he is seeing a knife as a knife. (Perhaps a culture

which used knives in a way in which we would describe as phallic symbols would serve as an example. For this culture they would not be symbols. One can also note the psychological experiments designed so that the subjects are required to use (see) objects, e.g. hammers, in new ways, e.g as plumb bobs.)

This limitation on seeing as, e.g. our inability to see a knife as a knife, does not show that seeing as is seeing plus interpretation -- as, I think, has been wrongly supposed by many. Seeing is just as much an "interpretation" as is seeing as. More precisely neither is an interpretation. Rather seeing X is part of an actually played language game with X into which one has been trained and which is largely independent of other language games. Seeing as is the ability (propensity might be better) to play more than one game with the object. This is why an ambiguous figure like the duck-rabbit illustrates seeing as so beautifully. It precisely straddles two language games.

These remarks also illustrate why "The substratum of this experience [seeing as] is the mastery of a technique." (p. 208). One must have been trained into the alternate language game, must have mastered its techniques, in order to see some object as having the internal relations of that game. Within a language game we can, perhaps, dismiss as nonsensical the claim that someone can see the schematic

cube as a paper box or a tin box (p. 208). So far, I have only discussed the limits on seeing as which are connected with whether or not someone has in fact been trained into the appropriate language game. There are also the limits imposed by the very general facts of nature on what language games can be played. In fact if one reflects on the evolution of language games, and one thinks about the many language games we do not play because it was not physically possible to train human organisms into them, we can get a sense of how to analyze seeing simpliciter. If we figuratively take the intersection of all the actually played language games to get the most general features (this result would be very like a form of life), then the very general perceptive categories which result could serve as the categories of seeing simpliciter.

I would have no objection to this as long as it is seen that even here we have only the very general empirical conditions of training together with the internal relations of these general language games into which we have been trained. That is, even seeing simpliciter partakes of the central features of seeing as, viz. the training into and accepting and using of a set of conceptual connections. When we reach a set of language games general enough to constitute a form of life, when, that is, we are no longer

within a language game (or games), then we must simply fall back on evolution and natural history for an account of the "correct" use of a concept. If we have been trained into the mastery of a technique, we see X's where X's are categorized by their internal relations to other objects and events, in this game. We are accustomed to treat X's as they are treated in this game. (p. 198). This disposition can often be a very complex one. When X's also are categorized as Y's in another game, then we can see X's as Y's provided we have been trained in the alternate game as well. Once more the crucial distinctions are relative to an empirically conditioned form of life. But one can also see that perceptual experience, scientific data in the standard sense, is itself relative to a language game as played. Thus at least a part of the justification of scientific theories rests on the way in which they help us see the world. A part of the justification depends on the conceptual adequacy of the observational categories used. And a determination of this conceptual adequacy has traditionally been a part of philosophy. The preceding section stressed how empirical conditions are relevant to deciding on necessary truths. This section has stressed how conceptual conditions are relevant to the empirical data. In short, there is a continuum between science and philosophy.

Distinctions relative to an actually-played language game can be made, but, unfortunately, these distinctions are usually taken as absolute -- absolute in the sense that misses the important fact that the language games from which both science and philosophy arise are empirically conditioned and the product of evolutionary selection. Science and philosophy can be distinguished only as to the relative positions of concentration each assumes on the continuum of human enquiry into the world.

V

I turn now to the claim that philosophy is a different order enterprise from science. Of course, this claim cannot mean merely that philosophy is more general in the sense that it is a metalanguage and science and common sense are its object languages. This may indeed be true, but a metalanguage -- object language distinction is necessary in any number of "empirical" disciplines -- lexicography, linguistics, grammar -- to name just a few. For that matter Wittgenstein claims that just because philosophy deals with 'philosophy' it does not follow that there must be a second order philosophy (121). I take this to be a textual reason for denying that Wittgenstein believes the object language -- metalanguage distinction to be sufficient to distinguish philosophy from science.

No, we must look elsewhere. It seems to me that Wittgenstein himself can easily be interpreted as suggesting a clear distinction in kind between philosophy and science. This distinction is, I suggest, the distinction between description and explanation. The philosopher can only describe, the scientist explains. "Philosophy simply puts everything before us, and neither explains nor deduces anything. --Since everything lies open to view there is nothing to explain." (126). "Philosophy may in no way interfere with the actual use of language; it can in the end only describe it." (124)[20] "We must do away with all explanation and description alone must take its place." (109). "The problems are solved, not by giving new information, but by arranging what we have always known" (109).

Of course, even if we wholly accept such a descriptive role for philosophy, that will not by itself be sufficient to distinguish science from metaphysics. The reason for this is that if the preceding view of science is correct, there will be a descriptive part to science as well. It will consist of clearing up conceptual confusions in the "foundations" of sciences. Given the continuum picture of science I have presented, such a descriptive investigation into the foundations of any science will be separable from that science itself only relative to a particular language game

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20 I shall grant for the sequel that scientific explanations do disturb the use of language. However I do not think that this is actually true in any important respect.

as played. It will not be separable from science in the larger sense of the form of life which has evolved. Wittgenstein, I think, makes precisely this point in II.xiv." (p. 232).

Let us see, however, whether philosophy really can eschew explanation. Paul Feyerabend had taken this insistence on the solely descriptive role of philosophy and combined it with the characteristically Wittgensteinian meaning is use doctrine in a most illuminating way.[21] Feyerabend argues that from these two doctrines -- meaning is use and philosophy is description -- it follows that it should be meaningless, not just false, to assert that there are any philosophical theories in the sense in which these might be explanatory. And indeed this seems to fit well into the general gestalt of Wittgenstein's works.

However, there are two problems with this view, both of which Feyerabend recognizes. This first is textual. Wittgenstein says "If one tried to advance theses in philosophy, it would never be possible to debate them, because everyone would agree to them." (128) But if Feyerabend's interpretation were correct, Wittgenstein ought not have allowed the theses to be true, even though trivial. Rather he should have condemned them as meaningless. Feyerabend simply notes this

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21 Paul Feyerabend, "Wittgenstein's Philosophical Investigations," The Philosophical Review, LXIV (1955), pp. 449. D. Gruender, "Wittgenstein on Explanation and Description", Journal of Philosophy, LIX (1962) pp. 523-530 maintains a view almost identical to Feyerabend's.

inconsistency and lets it pass.

Closely allied with this point is a philosophical one. If meaning is use and philosophy is description, then Wittgenstein's celebrated attack on "essentialism" must be wholly out of place. It would be consistent for Wittgenstein to condemn essentialists for having <u>believed</u> they were explaining. That would have resulted from their misconception of the role of philosophy. But once that misconception is removed and the obvious fact of the traditional use of that kind of philosophical language is noted, an inconsistency arises. On Feyerabend's interpretation Wittgenstein could not argue that this language is wrong; he should rather claim it to be meaningless. Yet quite clearly Wittgenstein's criticisms of essentialism go beyond what Feyerabend's interpretation allows him. Feyerabend here simply suggests Wittgenstein is wrong and is actually in the philosophical tradition of explaining after all.

But now one faces a dilemma. If one agrees with Feyerabend that philosophy does explain, we seem to be driven back to the distinction between science and philosophy as resting on the types of truths, empirical or conceptual, with which each deals in its own characteristic way. On the other hand we can allow that these traditional philosophical pronouncements and criticisms are indeed meaningless, yet somehow are indispensable for leading us to "higher" truths. I have

argued in the preceding sections that the former horn is incapable of distinguishing science and metaphysics. Similarly, the second horn, in addition to its somewhat mystical unpalatibility, simply begs the question as to a differentia between science and metaphysics. The "higher truths" will be "higher", i.e. metaphysical, only if they are independently separable from scientific truths.

But I think there is another way out which can avoid this dilemma altogether and will furthermore render Wittgenstein's position as so far presented totally consistent. Let me return to the epigrammatic section 128. "If one tried to advance theses in philosophy, it would never be possible to debate them, because everyone would agree to them." How are we to interpret this? Consider the following salient points:

a. Wittgenstein criticizes any theory of meaning which relies upon the word's being constantly accompanied by any object or event -- mental or physical.

b. Wittgenstein rejects the search for hidden essences as explanatory principles. Everything must be open to view.

c. Meaning is due to roles in language games as played which in turn are manifestations of parts of a form of life which in turn must be accepted. We can only be trained into a form of life.

d. The mistakes of other philosophers are not stupid (340).

These points suggest very strongly to me the following interpretation of the crucial passage: The philosopher is never to advance theses in philosophy in the sense of postulating ontological entities like, e.g., particularity, in order to explain any problems. The reason for this is that when we look at our ordinary language games we find no use for such a notion. It is not ordinarily used, it would have to be hidden if it is there at all, and when we look and see, we do not find it. Nevertheless generations of philosophers and reflective people who have read philosophers no doubt constitute a traditional language game into which a number of people have been trained. So the mistakes made are not stupid and, e.g., 'particularity,' must have <u>some</u> meaning since it is used in this actually played game. Only its meaning does not point to a different order enterprise from that of common sense or refined common sense -- science. If someone claimed, as a thesis, that there are particulars or that every accident is the accident of some substance, the thesis would be trivial. Who could deny it? These are just remarks which lay open to us a clear view of how we use our language. They are to be interpreted as saying this is how the game is played. But in such a role they <u>explain</u> nothing. They serve to refocus our concern, for example, on what kinds of things are particulars. But that concern, of course, is shared by atomic physics. The philosopher errs only in believing something special is needed to explain

or ground these "grammatical reminders." We only need to get a clear view of the actual working of our language in order to see that most of our problems disappear. We can stop doing philosophy. And those problems which do not disappear can, perhaps, be theoretically handled, but in a way not different in kind from scientific problems.

If some such view as this is adopted, the inconsistencies noted by Feyerabend vanish, and it is possible to see how Wittgenstein can legitimately criticize other philosophers for being wrong and not just speaking nonsense. There is a meaning to their words within the language games as philosophers play them. But these meanings are not hidden or pretentious. They are not independent of any language game into which we might be trained. Just as the duck in the duck-rabbit is not hidden and is not independent of our having been trained to recognize ducks, we must simply rearrange materials that have always been there. And it is important to note that these materials are simply the way the world contingently happens to be in its most general features.

> "What we are supplying are really remarks on the natural history of human beings; we are not contributing curiosities however, but observations which no one has doubted, but which have escaped remark only because they are always before our eyes." (415)

Natural history which is always before our eyes! From this we can surely get generality as a mark of philosophy,

but a unified science also possesses the same generality. From this we can surely get a concern by philosophy with the highest levels of theorizing, with the rearranging of materials already present. But my remarks on seeing and seeing as were meant precisely to push at least a part of science in that direction as well. In short, I do not think that any of the ways of distinguishing science from philosophy which seem to depend on a different "order" of inquiry succeed. (The one exception, noted at the beginning of this section, is the metalanguage -- object language distinction. I am perfectly willing to grant a meta-status to philosophy. But I deny that this secures a special place for philosophy as opposed to unified science.)

VI

And yet, scholars of Wittgenstein will have become more and more outraged with my treatment of him. Such a misuse (I did not claim it to be a totally faithful interpretation) of Wittgenstein's position is absurd. Let Wittgenstein himself retort.

> If the formation of concepts can be explained by facts of nature, should we not be interested, not in grammar, but rather in that in nature which is the basis of grammar? -- Our interest certainly includes the correspondence between concepts and very general facts of nature. (Such facts as mostly do not strike

> us because of their generality.) But our interest does not fall back upon these possible causes of the formation of concepts; we are not doing natural science; nor yet natural history -- since we can also invent fictitious natural history for our purposes. (p. 230).

Surely in the end (the literal end of the book) there can be no more decisive rejection of the position I have here attempted to argue. There is even an argument against the position I have tried to sketch. We can invent natural history for our purposes (Cf. Husserl's use of free variation).[22] So how could the actual course of events make any difference?

The answer is, I think, implicit in what I have already said. There is a most crucial ambiguity in 'natural history' as used immediately above and 'natural history' as previously used in section 415. It is this: When Wittgenstein denies philosophy is doing natural history, he means natural history as conceived within a language game as played. I have already conceded that a distinction between science and metaphysics can be drawn relative to a given language game. On the other hand in section 415 'natural history' is being used in its supra-language game sense. That is, in the sense in which the most general features of experience condition the language games we can play. Yet despite the contingency

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22 Edmund Husserl, Ideas (translated by W. R. Royce Gibson), (New York, Collier, 1962).

of these features they are so general and so obvious that they both enable us to speak of conceptual truths relative to a language game while at the same time freeing us from having to carry out an empirical investigation as that is popularly caricatured. (It has been a major purpose of section IV to attempt to remove just that caricature.) But if it is now asked how invented natural history would do as well as real, I can reply that the invented language games serve their purpose precisely in pointing up what these most general features of actually played language games are. "The work of the philosopher consists in assembling reminders for a particular purpose" (127).

It should also be recalled in this connection that invented language games will serve their purpose and remind us of the pervasive features of our experience only if they do not differ too much from the language games which express our present form of life. For the form of life determines intelligibility generally, and a fortiori, determines the intelligibility of the invented history. (Cf. section III). But now one can see why Wittgenstein and his followers did not see the implication of his work.

It is far too easy to slip unnoticed from questions concerning the correct interpretation of a rule within a language game as played, wherein we simply assume the larger form of life which provides the background, to questions con-

cerning the background assumptions themselves. The formal distinction between the logical role played by the background assumptions versus the logical role played by particular analyses within the scope of these assumptions gives the illusion of being able to determine in some absolute sense the material content of this distinction. As long as the inquiry stays fairly close to the most general features of the language game as played, the analyses will, in fact, have fairly clear referential implications. We will be able to say something about the most general features of the world. But the reason for this is precisely that the world is, through evolution, empirically responsible for the fact that these games are played in the first place. On the other hand, it is a parochially contingent fact that philosophers are trained into the particular language games they play. Further, philosophers are generally not trained into modern scientific language games. Hence it would be all too easy for a philosopher with the usual philosophical training, but lacking, e.g. scientific training, to take a general feature of his set of language games as somehow much more pervasive than it is.

In fact one may well wonder why philosophers make as few mistakes as they do. I suspect the reason is that throughout history there has been a close enough connection

between science and philosophy so that philosophy does in fact incorporate in its language games a good deal of science. On the other hand, scientists, have tended to hold down their mistakes by actually doing philosophy, as is, I think, the case in the foundations of the natural sciences, or else by borrowing heavily from philosophers of science, as is, I think, the case in the social sciences where an austere positivism is still painfully evident. Because of this interaction, most results of science, of natural history, will not be relevant to our philosophizing. But <u>some</u> will, and some of our philosophizing will be relevant to science.

In addition, I think that this view gives me a handle for diagnosing why so many arguments in contemporary analytic philosophy seem just to pass each other by. A will claim X to be a conceptual truth because he cannot conceive -X. (Translation: A is working within a particular language game and has at least thus far been unable to train himself or be trained into another. Possibly the contingent facts in his preclude his to being trained. In A's game X is a conceptual truth.) B claims X is not a conceptual truth (and possibly even false); for here is a counter-example in which -X is perfectly intelligible. (Translation: B is inviting A to consider a new language game which allows the conceivability of -X. This may or may not be a language game which is precluded by natural history.) We may not know who to believe.

But quite clearly science -- natural history -- may be and often is, relevant. We may need only to point out to A (or B) what most people have known a long time. We may have to train A (or B) into a current scientific language game of which they were unaware and which reflects the requisite natural history clearly enough to settle the dispute. Or we may even have to await the development of science to see if A's (or B's) presupposed language games actually are ones we can successfully play. What is one man's conceptual truth is another's empirical falsehood and the only way to settle the issue is by considering all the language games humans play and can play. The charge of parochialism leveled at the more extreme advocates of ordinary language analysis is often justified.

On the other hand, the various competing theories in science serve the same function as invented language games. We can "invent" theories and models (of subatomic physics?) as long as these models are somewhat constrained by our actual experience. Their primary justification for acceptance is not their one-to-one correlation with observation but their fruitfulness in making our original problems clear. We can now see things differently.[23]

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[23] I do not here have the space to develop in detail this view of science. However, it is a more and more common view and it is expressed with great power by N. R. Hanson, Patterns of Discovery (Cambridge University Press, 1958).

There is another way of viewing the activities of analytically-minded philosophers, both contemporary and traditional. They are attempting to formulate identity conditions for language games and forms of life. I have already urged that all concepts, including perceptual ones (Sec. IV), are inextricably entwined with the very general features of how one behaves in the world, so conceptual analysis will include the actual "use" of language games. This is the kernel of truth in Wittgenstein's "meaning is use" doctrine. This doctrine has been sadly perverted into the "What one would say if" doctrine. At any rate, the results of such conceptual investigations should be looked on as normative recommendations as to how most fruitfully one should organize language games to cope with reality. But these normative recommendations are constrained by exactly the same kinds of empirical conditions as are foundational scientific theories. The justification for their acceptance is likewise the same in principle as for scientific theories. We do not look for a one-one correspondence with "philosophical" facts or intuitions, but rather we ask how well the philosophical analyses enables us to see our problems more clearly and how fruitful it is for further <u>human</u> inquiry.

Wittgenstein's mistake was then a simple one. It was one which he himself diagnosed as incredibly easy to fall

into. It was the mistake of being held captive by a picture generated by a language game into which one had been trained. In Wittgenstein's case it was the picture of science concerning itself with detailed laboratory experiments. It was the picture of science as clearly empirical as opposed to conceptual.[24] And the pernicious thing about this picture is that a sense within a language game as played can be given to it. Yet in the larger sense, science in its conceptual aspects seems not separable from philosophy after all. "Philosophical Investigations: conceptual investigations. The essential thing about metaphysics: it obliterates the distinction between factual and conceptual investigations."[25]

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24 If one looks at all the places Wittgenstein draws the distinction between science and philosophy -- e.g. 37, 81, 89, 109, 124, 203, 392; p. 212, p. 230 -- one can see, I think, just this picture operating.

25 L. Wittgenstein, Zettel (Berkeley, U. of Calif., 1967), p. 82e, §458.