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A COMPARATIVE ANALYSIS OF BEHAVIORISTIC AND TRANSFORMATIONAL PARADIGMS.

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A COMPARATIVE ANALYSIS OF BEHAVIORISTIC AND
TRANSFORMATIONAL PARADIGMS

DISSERTATION
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the Degree Doctor of Philosophy in the Graduate
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CHAPTER I

INTRODUCTION

Statement of the Problem

The purposes of this dissertation are twofold. Of primary concern will be the attempt to analyze the nature of the relationship between methodological restrictions placed upon inquiry by particular theoretical paradigms in the behavioral sciences and the character of knowledge proposed. A second and related purpose of the present investigation is to examine the feasibility of an alternative paradigm to that of mechanistic behaviorism which appears to predominate in contemporary sociology.

To these ends the dissertation will examine in detail two different, and indeed in many ways opposing, social science paradigms in order to ascertain the degree to which the non-empirical assumptions concerning (1) the "proper" nature of inquiry, (2) the nature of the phenomenon observed, and (3) the assumed relationship between observer and observed, affect the type of "knowledge" which such paradigms generate. A key question appears to be to what extent is contemporary knowledge in the social sciences non-cumulative in nature as a result of the reliance on mutually exclusive meta-theories to organize inquiry?
The two paradigms selected for comparison are the now dominant behavioristic model which orients a substantial amount of inquiry in sociology, psychology and political science and one of its challengers, a counter model which is constructed from the assumptions utilized by comparative ethologists in biology and transformative-generative grammarians in linguistics.

This study will attempt to make explicit the substance of the assumptions that orient inquiry within each paradigm and will attempt to demonstrate the consequences of such orientations for the content of the acquired knowledge each offers. Primary emphasis will be placed upon those assumptions that are incorporated into the method of verification, giving the methods of one paradigm a substantive content that significantly differs from that of the method utilized by adherents of the other paradigm.

**Significance of the Problem**

The relationship between theory and methodology is a concern that has generated a substantial amount of discussion in the social sciences. The work of Robert K. Merton\(^1\) perhaps represents the dominant viewpoint among sociologists regarding the problematic aspects of the relationship. His concern with the problem emanates from his

and others' dissatisfactions with the assumed state of discontinuity between sociological theory and sociological research. Merton sees this state as resulting from both the abstractness of theory and the concreteness of empirical research. Given this definition of the problem he offers the logical solution in his "theories of the middle-range."

Merton's work rates close examination because it characterizes prevailing views in the field and also because it has been the focal point of dissent. In a recent re-statement of his position he attempts to deal with a number of dissenting opinions, but the main thrust of his argument remains unaltered.

This paper will deal with what it takes to be the problematic aspects of the theory-method relationship in a rather different way. In part this difference in emphasis results from a different view of the nature of methodology. Merton attempts to clearly distinguish theory from method. The distinction he makes is a common one in the social sciences and rests on the differentiation of theory as empirical content and methodology as a logic system. Merton states that "we should distinguish clearly between sociological theory, which has for its subject matter certain aspects and results of the interaction of men and is therefore substantive, and methodology, or the logic of

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scientific procedure." To Merton there is actually no such thing as a sociological methodology since the method of sociological inquiry is a logic common to all the sciences, a view which, though common, is not held by all sociologists.

A contrasting view of methodology is offered by the recent work of Sjoberg and Nett. Theirs is the view that instruments and procedures used in scientific inquiry presuppose substantive theories. This view emphasizes the degree to which techniques used, as well as the results obtained, are based upon specific assumptions of a substantive nature. While Merton notes this characteristic of methodology, he fails to examine the implications of this observation. His failure to do so is rather directly related to his more general view of the scientific enterprise. If one accepts the substantive assumptions embodied in a methodology as the only possible assumptions or as the correct assumptions then there is no problem. Merton implies that there is and perhaps always has been this homogeneity of method both in the social and physical sciences. In many ways his work is also a denial of the existence of theoretical diversity at least in sociology. Explicit

3Ibid., p. 140.


5Merton, op. cit., p. 141.
confirmation of this viewpoint is given when he states with Maoist sagacity that at present "Sociological orientations that are not contradictory are regarded as if they were."\textsuperscript{6} Given this conviction on Merton's part, it is not surprising that he sees nothing particularly interesting about the fact that methodology is always loaded with particular substantive and hence theoretical assumptions.

For lack of a better word these theoretical models, which embody substantive assumptions which place restrictions upon what would otherwise be unorganized inquiry, will be called paradigms. This term is adopted from Thomas Kuhn's\textsuperscript{7} usage which generally corresponds to the idea intended here.

A certain amount of paradigmatic contrast is required to clearly assess the importance of assumptions like those mentioned above. Such contrast is found in the history of science at those times when important changes are being made in the instrumental and conceptual frameworks that orient a specific scientific endeavor. In his analysis of such events, The Structure of Scientific Revolutions, Thomas S. Kuhn offers a suggestive view of the relationship between theory and data.

\textsuperscript{6}Ibid., p. 54.

In discussing the relevance of his findings for the philosophy of science, Kuhn compares the prevailing philosophical views on the proper bases of rejection of one paradigm in favor of acceptance of another alternate paradigm. He states that, "Few philosophers still seek absolute criterion for the verification of scientific theories. Noting that no theory can ever be exposed to all possible relevant tests, they ask not whether a theory has been verified but rather about its probability in light of the evidence that actually exists."\(^8\)

This insistence on comparing theories, which characterizes the probabilistic verification school in the philosophy of science, is what actually takes place historically. One school asks that a given scientific theory be compared with all others that might be imagined to fit the same collection of observed data. Others require the construction in the imagination of all the tests a given scientific theory might conceivably be asked to pass. All have recourse to pure or neutral observation languages, of which none have been constructed that satisfy the criterion of neutrality to everyone's satisfaction.

A different popular approach is that of Karl Popper. Popper denies the existence of any verification procedures at all. He relies on falsification, which he sees as

\[^8\textit{Ibid.}, \ p. \ 144.\]
necessitating the rejection of an established theory. Since incompleteness and imperfection is the usual state of theory, then Popper would apparently have one reject all theories at all times. If, however, only severe failure to fit justifies rejection, Popper is in the same position as the others—forced to talk in terms of the degree of falsification and improbability as criterion of the adequacy of competing theories.

While it makes sense to ask which theory fits the facts better, Kuhn concludes from his study of the history of several scientific revolutions that:

This formulation, however, makes the task of choosing between paradigms look both easier and more familiar than it is. If there were but one set of scientific problems, one world within which to work on them, and one set of standards for their solution, paradigm competition might be settled more or less routinely by some process like counting the number of problems solved by each. But in fact these conditions are never met completely. The proponents for competing paradigms are always at least slightly at cross-purposes. Neither side will grant all the non-empirical assumptions that the other needs to make his case. Like Proust and Berthollet arguing about the composition of chemical compounds, they are bound partly to talk through each other. Though each may hope to convert the other to his way of seeing his science and its problems, neither may hope to prove his case. The competition between paradigms is not the sort of battle that can be resolved by proofs.9

Kuhn's work is directly relevant to the topic under discussion here because it illustrates the primacy of theoretical paradigms in shaping the facts and in the

9Ibid., pp. 146-147.
discovery of them. The apparent neutrality of facts is a consequence of the particular way in which the history of science is reconstructed in texts, according to Kuhn. The cumulative appearance of scientific knowledge which is the result of this misconception renders scientific revolutions invisible.

Kuhn's work is all the more important in this context because, while he is dealing with the history of the physical sciences, he presents a different image of scientific development than is generally held by social scientists who wish to emulate the methods of the physical sciences.

The current state of sociological knowledge also indicates that a thorough and self-conscious analysis of our present paradigms are in order. The existence of contradictory evidence and inconclusive evidence for even middle-range propositions is all too frequently the case in contemporary sociology. For example, in the studies of the relationship between social class and mental illness, the conclusion is almost inevitably reached that more evidence is needed before any conclusive statement can be made. The same situation can be found in studies of both suicide and crime, social phenomena for which it can scarcely be said that "evidence is lacking."

It is the opinion of this author that part of the reason for this present state is that we have failed to closely examine the assumptions that govern the process of
inquiry and have instead placed our faith in what we take to be a magical verification procedure that will somehow yield clarity and truth.\textsuperscript{10} One among many indications of the extent to which we view methodology as a substantively neutral magical device is that while sociologists feel that there is a certain amount of theoretical diversity in sociology, the same methods and techniques of verification are often purported to be equally useful in researching hypotheses emanating from different theoretical frameworks. The choices among methods are frequently portrayed as being dictated by constraints of a non-theoretical nature.

One might characterize this view as a naive methodology. It by no means represents a total of the stances taken. Sjoberg and Nett, among others, explicitly state that at the most general level the scientific endeavor is not neutral but that certain assumptions underlie the application of the scientific method. They list the following assumptions:

\begin{enumerate}
\item that there exists a definite order of recurrence of events,
\item that knowledge is superior to ignorance,
\item that a communication tie passed upon sense impressions, exists between the scientist and "external reality" (the so-called "empirical assumption),
\item that there are cause-and-effect relationships within the physical and social orders.
\end{enumerate}

Moreover, there are certain "observer" assumptions.

\textsuperscript{10}\textsuperscript{10}R. Krohn, "Method is our Magic," Sociologies, I (Winter, 1966), pp. 18-23.

\textsuperscript{11}\textsuperscript{11}Sjoberg and Nett, \textit{op. cit.}, pp. 23-24.
But what of the other assumptions embodied in specific endeavors in the social sciences? Sjoberg and Nett acknowledge that in the social sciences not only do the logical form and the content of the generalizations differ from one theoretical framework to another but also the assumptions regarding the scientific method and the nature of the data. There are a number of possible consequences of this diversity of assumptions. The possibility that assumptions are incompatible to such an extent that the methods implied by these assumptions lead to contradictory "findings" has been emphasized in the previous discussion. There is also a possibility that theoretical orientations in the social sciences share a broad base of substantive assumptions and that the methods employed do not preclude the testing of those assumptions on which they differ but only preclude refutation of the shared assumptions. Thus decisions between frameworks can still be made on the basis of which better fits the facts.

Sjoberg and Nett seem to espouse this latter view in their assertion that some of the assumptions held by sociologists seem to be amenable to empirical test. This gives the impression that while certain substantive assumptions are not testable, given present methods, others are.

Two other trends in contemporary sociology that indicate that a clarification of the nature of theoretical paradigms is in order are (1) the attempts to construct a
theory by combining propositions and (2) attempts to integrate different theoretical frameworks. The first concern is exemplified by both Zetterberg and Berger, Zelditch, and Anderson. Both of these efforts rest on the implicit assumption that was discussed previously. That is, they assume that the diverse theories of the social sciences do share a set of common assumptions. By using the addition method of Zetterberg without focusing upon the possibility that one is mixing apples and oranges the goal of constructing an ordered set of propositions is very likely to be short-circuited. The same is true of integration attempts with the most usual result being that the "new" theory subordinates minor aspects of one framework to major aspects of another theory. The intent here is not to depreciate such efforts but to explore the possibility of their being placed upon a more solid base.

There remains a final more subjective reason why an endeavor to clarify the impact of theoretical paradigms on the character of knowledge about human phenomena seems important. In the past decade or so there has been a spate of works in the social sciences that have expressed a wide range of misgivings and dissatisfactions with the now


predominant theoretical and methodological models. Accom­
panying these works is a general search for alternative
paradigms that would employ more satisfactory models. By
examining the underlying assumptions of one paradigm that
wed its theoretical substance to its methods and contrast­
ing these with the assumptions of a counter paradigm, the
strengths and shortcomings of both may become more apparent.

Since many methodologists and sociologists in general
place their trust in methodological procedures alone for
determining the probable truth or falsity of a given para­
digm it seems worthwhile to attempt to assess the degree to
which their trust is well founded. Their view of method as
a neutral device free from substantive assumptions is
challenged by the work of T. S. Kuhn. In addition,
sociologists themselves are becoming increasingly cognizant
of the intimate relationship between particular theoretical
assumptions and the methods employed by those holding such
assumptions. In the following chapter a method of examining
this relationship and making it explicit by contrasting one
paradigm, its logical structure, its assumptions and
methodological restrictions and its content, with the logical
structure, assumptions, restrictions on inquiry and content
of a counter-paradigm, will be presented and discussed.

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14Kuhn, op. cit., Chapter X.
CHAPTER II

MODE OF ANALYSIS

Selection of the Case

The examination of a paradigm in order to elucidate the possible substantive content incorporated in its method would illustrate Kuhn's general thesis regarding the paradigm-related nature of scientific verification. But this exercise in and of itself would reveal nothing problematic, especially to proponents of the paradigm. In effect the whole of the existing empirical support for the paradigm validates making the assumptions of the paradigm and their use or incorporation in methods. The "evidence" demonstrates that this orientation is a fruitful way of viewing things. The problematic aspect is only brought out by opposing views with contrary "evidence," that cannot be explained or predicted using the original paradigm. At present there is much debate of this kind in the social sciences. The consequence of some of this debate is the revelation of gaps in knowledge that lead to the extension and modification of one paradigm.

Other debates have a larger consequence, that of casting doubt on the basic validity of the complete
paradigm. It is the sort of debate that would be most likely to bring out the basic assumptions and findings of one paradigm and counterpose them to different sets of findings which are presented as being "more justified" on some basis or another. Often these are presented without acknowledging the importance of the fact that contrary assumptions are necessary to discover this second set of findings. Thus one can be left with the possibly mistaken impression that the counter approach is somehow more free of assumptions about content and is simply verified by a scientifically neutral method. By ignoring these assumptions the author can dismiss opposing views on the basis that they are ideologically based, as Catton for example dismisses the anti-reductionism arguments of a number of sociologists.¹

The glorification of the supposedly assumption-free, neutral method is particularly endemic to those social scientists that wish to emphasize the "scientific" aspect of their work. Their methods are presented as a near equivalent to pointer readings in the physical sciences. To estimate the extent to which this claim is valid, a paradigm whose proponents made such a claim, behavioristic psychology, was chosen for analysis.

B. F. Skinner's behavioristic psychology is one of the leading paradigms that bases its claims upon its purity of method and for this reason is a good one to use to assess the degree to which these claims are in order.

Skinner's paradigm was selected from among others for a number of other reasons also. For one, it represents a view that is thought to be somewhat peripheral to the author's area of vested interest, sociology. Some psychic distance is desirable in order to prevent one from getting caught up in the paradigm's assumptions to the degree that alternatives seem untenable. Yet the Skinnerian model is not so far distant from sociology that it is irrelevant to concerns in that field. A Skinnerian model underlies and provides the basis for much of the work in all of the social sciences with the possible exception of anthropology.2

The behavioristic paradigm is not a straw man either. It can muster a tremendous amount of research findings that support it. It is in no way speculative and untested. To clarify its impact as a meta-theory on the method used and evidence obtained, the behavioristic paradigm will be contrasted with an alternative paradigm that is concerned with roughly the "same" phenomena.

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The examination of alternative models is a useful analytic tool in a number of ways. It, by contrast, makes evident the extent of the assumptions that both paradigms make. By posing alternative views, it robs such assumptions of their misleading self-evidentness. In addition, the counter-paradigm offers a systematic alternative rather than attempting to reformulate and clarify only certain aspects of the original paradigm.

**Choice of the counter-paradigm**

Obvious sources of an alternative paradigm to behaviorism were the writings critical of the Skinnerian position. In researching the bases of criticism of Skinner it became apparent that there were several vaguely defined "schools" of thought within psychology. The first is that of the behaviorists, neo-behaviorists and neo-neo-behaviorists. On the importance of the differences between the varieties of behaviorism, there is some disagreement.\(^3\) It is the impression of the author that none of these varieties form enough difference on sufficiently broad grounds to constitute a radically reformulated new paradigm or to forge credible links with one of the other paradigms.

In researching the major criticisms of behaviorism it became apparent that much of the empirical foundation of

such critiques was developed largely in the works of men who are the major proponents of a mainly European school of animal psychology, ethology. Upon closer inspection, this mode of inquiry that ethologists have developed appeared to have drastically different assumptions about a whole range of questions from whether the aim of science is control or preduction and understanding, to whether the nature of man is active or reactive.

A third approach that is essentially different from either of the aforementioned two, is that of phenomenology, which is perhaps best represented by Carl Rogers, Combs and Snygg and a number of social psychologists. This school of thought places the prime focus of analysis upon the phenomenological field of the individual—viewed primarily in the ontogenetic sense. This phenomenological field is viewed as the organized basis of the individual's behavior. Because of the scope of analysis of this type of framework, it is specifically limited (at present) to the behavior of man, it has not gained as yet the status of a general theory and is often viewed by its adherents as a supplementary theory that enhances the understanding of man's behavior.

This framework almost exclusively emphasizes the discontinuity between the natural world and the way in

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which the phenomenological world of man is ordered. However, the particular 'common-sense' context is held by some to have arisen from particular behavioristic contexts to begin with, hence the modification S-M (meaning)-R—a meaning mediated model.

On the other hand, much of the phenomenological literature neither takes a position within the behavioristic framework nor for that matter is it fitted into any other framework in any clear manner. In a sense, since the key components of this framework involve the almost total separation between man and the physical universe, the man and his historical and phylogenetic past, the separateness of the theory from other more general theories is not incomprehensible. This lack of generality alone makes the phenomenological paradigm a rather poor choice for a comparison of paradigms because it simply lacks sufficient points of comparison.

Barring the phenomenological approach then, the primary critique of the dominant behavioristic framework has been launched from within the ethological framework. By and large more conscious elaboration of the phenomenological-behavioristic differences has been produced in recent years. However, there have been very few attempts to systematically delineate and describe the differences.

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between behaviorism and comparative ethology. Yet, Noam Chomsky, a critic of S-R theory, subscribes to an ethological approach and incorporates into his framework of transformational linguistics a markedly similar methodological stance and a similar set of assumptions. Unlike comparative ethology, Chomsky applies the framework to what he sees as a specifically human phenomenon, language. In this way, ethology and transformational linguistics together form an alternative paradigm to behaviorism in the study of both human and sub-human phenomena. Thus, while comparative ethology has not been systematically presented as an alternative paradigm to behaviorism, there are some indications that the incongruity between the two is grasped by some of its proponents.

In this dissertation comparative ethology and transformational linguistics will be contrasted and compared to behaviorism with regard to the degree of incongruence on the types of questions and the permissible answers that the two paradigms permit.

There are essentially two probable ways of going about such an analysis. Either, one can examine the method of acquiring evidence in order to discover the questions which such a method can legitimately answer, or one can examine the effect of theoretical assumptions upon the range

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of questions raised, methods used, and answers obtained, within the particular framework. This study will take the latter tack, although to do so would seem to immediately reject the behavioral mode of analysis. A partial legitimization for taking this route, however, can be found by taking Skinner's own work as evidence.7 In a case study of his own work Skinner describes his behavior in an attempt to show the irrelevance of the philosophy of science to the actual research situation.

According to his account he was waiting around with nothing to do in a laboratory during the war. He picked up a rat, placed the rodent in the palm of his hand, and pulled its tail. He found that the rat pulled back in the opposite direction. In a few short steps Skinner moves from this to the construction of elaborate instruments to record and measure this pulling. Now Skinner uses this example to demonstrate the lack of any preconceived method, hypothesis, theory, etc., in his work. Yet this example bears a close relationship to his later theoretical outlook.

Due to his previous training Skinner was curious about the behavior (not activity) of animals. Even ruling out physiological study, a number of alternative modes of action appear to have been possible in this case but were neither followed nor consciously ruled out by Skinner. Different

observations could have been made, for example: why pull the rat's tail? Isn't this obviously an external stimulus? Why did Skinner not follow a different procedure, that of simply letting the rat sit on his hand and observing what he did? Of course, if the rat sniffed and blinked and moved its feet, his behavior would not fit into any common-sense S-R framework, for it seems a bit Procrustean to label palm-beneath-feet as the stimulus controlling this behavior. Yet it is certainly a possibility.

It appears that some occultation of an orientation as to what is most relevant and most fruitful, is demonstrated by Skinner in this account, and that randomly emitted behavior on the part of an observer does not really describe the beginning of research sequences.

The writer is aware that questions like those raised above are generally outside the behavioristic framework (i.e., explanation of non-behavior) and that there is no attempt here to analyze Skinner's framework from within his own paradigm. However, these questions are not designed as a refutation in the usual sense. Yet questions such as those raised above are precisely the type that can be legitimately included using the counter-paradigm.

The purpose in raising questions of this type then represents more than a critique of a certain paradigm on any basis imaginable. The purpose is to use the counter-paradigm as an organized basis from which to argue for the
efficacy and plausibility of an alternate logically consistent view of the phenomena in question. For unless propositions that are contrary to those of one paradigm can themselves be organized into an interrelated framework, they do not represent an alternative so much as they merely point to gaps in knowledge which, at the present stage of the game, are not particularly damaging.

In summary, the interrelationships between the assumptions and methodological restrictions placed upon inquiry by a particular theoretical paradigm, Skinnerian behaviorism, will be contrasted with their counterparts in the paradigm used in ethology and transformational linguistics. The resultant effect of these assumptions and restrictions on the different "findings" of each paradigm will then be examined. In the following chapters the degree to which contrary explanations of the "same" phenomena can be viewed as the result of these differing rules of evidence will be made explicit. The paradigms will be sketched out to familiarize the reader with each and then analyzed in detail.
CHAPTER III
PARADIGM AND COUNTER-PARADIGM
A SYNOPTIS

In order to provide a clear orientation to later analysis, this Chapter will briefly describe the main components of the theoretical paradigms to be compared in the next Chapter. In this section particular attention will be given to the conceptual framework, the problems to which the paradigm addresses itself, and the quasi-empirical assumptions incorporated within the paradigm. In addition, the logical structure and the content of the paradigms will be outlined. Emphasis will be given to components which are particularly important in their effects upon methods of verification and which ultimately lead to dissimilar content in ways that will be analyzed in the next Chapter.

The Behavioristic Paradigm

First we will proceed with a brief description of the Skinnerian paradigm. Since this is the more familiar of the two to the behavioral scientists in general, a somewhat less detailed outline will suffice. The outline is drawn almost exclusively from the works of B. F. Skinner because he is a prolific exponent of behaviorism, and one who presents the
behavioristic point of view with regard to a broad spectrum of phenomena.

Every paradigm sets for itself a limited set of problems and demarcates the range of phenomena to be explained. For Skinner the primary problem is the control and prediction of the behavior of organisms. Behavior is defined as the observable movement of end organs. A further refinement in terms of explanatory priorities can be seen in the emphasis upon behavior that has some direct consequence upon the external environment of the organism, upon either objects or other organisms. Consequently behavior that is relevant to the paradigm is often described in terms of its effects upon the environment¹ (e.g., movement of a rat is described as bar pressing as opposed to a description of the muscular action that brings about the bar pressing). Such detailing of the orientation may seem trivial but the description of the counter-paradigm will show that the priorities set by behaviorism are not necessarily the only possible ones.

Stated very succinctly, Skinner's thesis is that behavior is the result of two factors (1) present stimulation and (2) the history of reinforcement, particularly the

frequency, arrangement, and withholding of reinforcing stimuli. The empirical support for this theory is derived from laboratory experiments with pigeons and rats.

A typical demonstration involves an organism which has been reduced to 80 per cent of its normal weight. The organism is placed into the restricted environment of a box-like apparatus that is fitted with a feeding magazine and has a recording device attached to it to measure a particular kind of response. Successive changes in the conditions under which the response will be reinforced makes possible the shaping of the response in many different ways. For example, a pellet of food is given only when a light flashes and when the rat presses a bar. Then the rat only presses the bar when the light flashes. Correspondingly the apparatus can be so arranged that only bar pressing of a certain duration is reinforced, thus variations in response can be obtained. Also, only responses to discs of a certain color or size can be reinforced which brings a response under the control of different stimuli. The history of previous deprivation (i.e., hours without water) can be varied but this seems to affect only the rate of response during extinction (the time during which the organism emits the response without reinforcement). By continual and careful rearrangement of contingencies surrounding the

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responses quite complex responses such as walking in figure eights, can be obtained from an organism.

With the exception of reflex action all behavior is apparently a product of such processes of reinforcement. Also these same basic principles of reinforcement and extinction apply not only to rats and pigeons but to human organisms. True there may be some quantitative differences between species but the same processes of learning apply. It is in this sense that Skinner asserts that there is remarkably little difference in behavior between species.

Before delving more deeply into the kinds of assumptions which orient the research of behaviorists, some examination of the conceptual framework is in order.

The concept response in the Skinnerian framework is further refined to distinguish between respondents, which are purely reflex responses elicited by particular stimuli (such as a dog salivating upon the appearance of food), and operants which are classes of responses for which no specific preceding stimulus can be found, and which are, therefore, defined in terms of the similarity of their consequences. All stimulating events which increase the frequency of the response, or in Skinnerian terms strengthens the operant, are termed "reinforcement" and all resulting changes "conditioning." The difference between conditioned reflexes and operant conditioning are that, in Pavlovian experiments, "a reinforcer is paired with a stimulus;
whereas an operant behavior it is contingent upon a response."³ Operants are emitted responses that have no single stimulus which invariably precedes them. "Behavior of this sort may come under the control of stimuli, but the relation is not that of elicitation."⁴

Although there are problems with the sorts of distinctions made here between instinctual action and conditioned behavior the concept of operant or response has other limitations. In laboratory conditions the unit of behavior is defined by the recording apparatus. In the situations to which Skinner extrapolates his findings units display no such boundedness nor does Skinner give us any criterion for determining such boundaries. This problem will become particularly evident when we discuss the counter-paradigm of transformational linguistics. One has the impression that units are seen from this framework as arbitrary anyway, although at times Skinner seems to indicate differently.⁵ Larger units are seen as simply being built from smaller ones, all being perfectly reducible and perfectly divisible into smaller units yet capable of being analyzed in combination or extrapolated from findings concerning smaller units that combine to yield the larger

⁴Ibid., p. 64.
⁵Ibid., p. 116.
entities. The matter is one of convenience as Skinner implies in the following discussion of social laws:

It is always an individual who behaves, and he behaves with the same body and according to the same processes as in a nonsocial situation. . . . The individual behavior explains the group phenomenon. . . . [However] another level of description may also be valid and may well be more convenient.6

By and large we can say that units of behavior called responses are determined by the apparatus in the laboratory and/or taken from the conventional terminology in discussing human phenomenon.

There are similar ambiguities with the concept stimulus. Skinner designates three different types of stimuli, eliciting, discriminative and reinforcing. The term eliciting stimulus is used to refer to those stimuli that elicit reflex behavior or what Skinner sees as innate behavior. Discriminative stimuli are those whose connection with a response is the result of reinforcement. Reinforcing stimuli are those stimuli the presentation of which alters the frequency of response.

Actually, stimuli which in the past were made discriminative also change the probability of response. The accurate classification of stimuli in terms of these categories requires a complete knowledge of the individual's history of reinforcement.

6Ibid., p. 298.
Reinforcing stimuli are perhaps the most powerful components of the Skinnerian paradigm. Their presentation accounts for most of the changes in the frequency of response or what is more commonly called learning. The nature of these stimuli is difficult to determine independently of a specific experimental situation. For Skinner biological advantage\(^7\) apparently accounts for the reinforcing power of such things as food and water, given appropriate states of deprivation. But other things are sometimes reinforcing without such advantages resulting. The explanation that Skinner offers for these is that they have been paired with more than one primary reinforcer.\(^8\)

However, in discussing the reinforcing effects of mere performance and of stimuli like tinkling bells, Skinner hints that almost anything can be reinforcing. The only clear definition comes from the experiment. "We observe the frequency of a selected response, then make an event contingent upon it and observe any change in frequency. If there is a change, we classify the event as reinforcing to the organism under the existing conditions."\(^9\) What these conditions are other than the laboratory situation itself, one should say, are unknown.

\(^7\)Ibid., p. 83.
\(^8\)Ibid., p. 77.
\(^9\)Ibid., p. 73.
Up to this point we have emphasized mostly the content, orienting questions and conceptual framework of behaviorism. Another more implicit kind of component of the theory is the logical structure that emerges from the paradigm and becomes a model for organizing knowledge. The assumption of the primacy of experience and the external environment leads to description in terms of the correlation of behavior to external stimuli. Also the relationships are particularly limited to quantitative ones such as the more one event is repeated the more a bit of behavior is repeated. One could perhaps best characterize this type of inquiry as the replication of uniformity. We will return again to this point in the comparison of paradigms to attempt to clarify it. It is one of those differences between paradigms that is illustrated best by example.

Since much of the controversy centering around behaviorism focuses upon its image of man, this brief description of behaviorism will be summarized by outlining this image. There is a tendency to describe statements on the nature of man as non-empirical assumptions. The position that this paper hopes to demonstrate is that while it is true that these may be viewed as assumptions orienting research, a situation arises in which the conception of man and other organisms emerges from the research. Thus it is

inaccurate to relegate these conceptions to a speculative status or to regard them as peripheral to scientific endeavors.

The image of man that emerges from the behavioristic paradigm is that of a reactive organism behaving in response to external stimuli. Insofar as it is not innate, behavior is shaped by external events that the organism has experienced in the past. The past refers almost exclusively to the ontogenetic history of the individual. The intrinsic nature of the organism is of infinitesimal importance and without content, with the exception of a few innate reflexes. The "natural" or non-problematic state of the organism is that of rest.

The principles describing behavior follow directly or indirectly utilitarian ends. This utilitarianism extends generally from rats collecting food pellets to students collecting grades and scientists collecting facts. This narrow utilitarianism upon which the individual's behavior is based is linked ultimately to a more general utilitarianism, often to survival value for the species.

The organism is viewed as void of content, contributing nothing to behavior. Since the organism contributes nothing, all organisms are alike. Differences are assigned to differences in experience, of a quantitative nature, not differences in capacity of a qualitative nature.
Whatever appears to be species-specific is viewed as secondary, yielding a basic equilitarianism between and among species as a central tenet of the paradigm. This conception of the organism with its emphasis upon environmentalism, utilitarianism and equalitarianism forms the recognizable core of behaviorism.

Ethology and Generative Grammar

The combination of two theoretical frameworks, ethology and generative grammar, into a single paradigm in order to contrast it with behaviorism requires some justification. On the surface they are quite dissimilar especially if viewed from a content point of view. Ethology is the study of animal behavior but has not been applied to mammals very extensively. Generative grammar is a paradigm in linguistics dealing with language as a species-specific behavior. It has some overlap with linguistics in the study of animal signal systems and the nature of cognitive processes in animals. This is noted because there is a tendency in the literature to interpret Chomsky's critiques as a reaffirmation of the validity of the man-nature dichotomy. It is this, in a way, but it is also more than this.

In this section ethology and generative grammar will be described briefly. The framework of comparative ethology will be described first. As with the presentation of behaviorism, a high degree of selectivity will be utilized for the sake of brevity. Since the emphasis in ethology is
upon diversity in the activity of organisms, much more
detailed content will be deleted in favor of emphasis upon
the organizing theoretical framework.

Again we will start with the organizing problems that
c characterize the ethological paradigm. As do the behaviorists, the ethologists propose an objective study of the
behavior of organisms. But whereas the behaviorists ask
"How this Behavior?" the ethologist asks "What Behavior?". The attempt is made to analyze the pattern of internal organ-
ization in this diverse behavior itself. The diverse behavior is categorized largely in terms of the movement itself rather than in terms of the consequences for the environment. This is done for the obvious reason that ethologists see a primary source of the organization of behavior to inhere in the neuro-physiological-skeletal-
muscular structure of the animal. Sometimes, the designa-
tion comparative ethology is used which denotes the emphasis that this view places upon the co-evolution of both behavior and physiological make-up. The emphasis in research is upon the observation and description of the behavior of different species of animals. Observations are usually made of the animal in its natural habitat insofar as this is feasible. There is a general de-emphasis upon conditioned behavior insofar as any conditioning is not a product of natural contingencies. Deliberate manipulations of the organism by the observer are made at times to check the validity and reliability of field observations.
For the conceptual framework of ethology we have drawn heavily upon Tinbergen's *The Study of Instinct*. In these works he attempts to synthesize the work of Thorpe, Lorenz, and others in order to present a coherent statement.

Ethology views the organism as a highly complex interdependent system. This system is considered as one that is always potentially active. Tinbergen states:

> It is of great importance for the understanding of instinctive behavior as a whole to realize that the various instincts are not independent of each other. We have rejected the reflex hypothesis of behavior and we have seen that each instinctive mechanism is constantly present, that is to say, prepared to come into action. Such a system can only work because blocking mechanisms prevent the animal from performing continuous chaotic movements.

These blocking mechanisms, themselves internal, are released by specific stimuli in the environment and by apparently internal stimuli provided by the animal's own prior performance. Tinbergen states in this regard:

> We have seen that the causal factors controlling innate behavior are of two kinds, viz., internal and external. In most cases both kinds exert an influence and they supplement each other. Usually the internal factors do not themselves evoke the overt response; they merely determine the threshold of the response to the sensory stimuli. Therefore, the internal factors like hormones, internal stimuli, and intrinsic impulses determine what the psychologist calls the motivation; and I will call them motivational factors. As


\[12\] Ibid., p. 111.
we have seen, it is highly probable that in many cases external stimuli may also raise the motivation, and some of them therefore also belong among the motivational factors.\textsuperscript{13}

The external factors related to motivation that Tinbergen refers to are things like temperature and length of the day. These are a requirement for the receptivity to more specific stimuli that initiate reproductive behavior in certain fish.

These factors, internal and external, provide a boundedness for units of innate behavior. These segments are typically combined in a rather rigid pattern to form a complex surrounding an activity that serves a biological end, such as reproduction, eating, sleeping, etc. The combination is given partially by the biological end and partially by the temporal sequence. In addition the behavior pattern is thought to depend on a common neurophysiological center. For some instincts there is not good corroborative evidence for this hypothesis but only a lack of negative evidence at present.

Perhaps the most important generalization of the ethologists concerns the internal organization of behavior. This particular component can be seen as a partial negation of the reflex theory. That is, without this hierarchal notion of the organization of external behavior much of innate behavior could be seen as a series of unconditioned responses triggered by specific stimuli in the environment.

\textsuperscript{13}\textit{Ibid.}, p. 122.
Tinbergen proposes instead of a simple input-output system an interdependent system that is hierarchically organized.

Thus the release of instinctive behavior is provided by specific stimuli that prevent chaotic movement. This alone will not suffice. It is possible to envision a situation where several specific releasing stimuli would present themselves at once to the organism. The natural environment offers many such situations. But as a rule, an animal does not do "two things at a time." Thus the relationship between internal centers of activity must be one of inhibition. Intensive nest-building, for instance, renders the male stickleback much less susceptible than usual to stimuli normally releasing fighting and vice-versa. This principle also seems to hold at higher levels, an animal in which the sexual motivation is strong is much less than normally susceptible to stimuli that normally release flight or eating. Within these activities, however, there also exist hierarchical neural structures which are characterized by coordinated and configurational type of organization.\(^1\)

This framework all has a consequence of giving content to the organism that circumscribes the pattern of behavior. Thus there is really not an equivalency or equality of stimuli at any one given time, although the organism is capable of responding to many stimuli at other times.

\(^1\)Ibid., Chapter V.
Here again in the study of the hierarchical organization of behavior there is a constant cross-checking of physiological and behavioral evidence. Since some of the physiological evidence requires that the organism's characteristic structure be segmented and artificially manipulated, there is a constant need for careful observation of whole, normal organisms in conjunction with physiological research. Also the method of checking the generalizability of findings is to work from an individual member of a species to that species in its natural habitat and then to other closely related species and finally, if possible, to species which are remotely related (morphologically and physiologically).

Up until now we have primarily discussed instinctive behavior but not all of the behavior of animals can be so explained, although instincts certainly keep lower animals on the track and are of prime importance in patterning biologically required behavior. The ethologists make a conceptual distinction between appetitive behavior and a consummatory act. This distinction separates behavior as a whole into two components of entirely different character. The consummatory act is a relatively simple chain of responses and is self-exhausting, their performance seeming to 'satisfy' the animal. As a rule the consummatory act is rigid and therefore an easily recognized form of movement.

As opposed to the consummatory act appetitive behavior is variable and plastic and in a way purposive. This
type of behavior may be a simple introduction to consummatory action such as in the case of a frog catching his prey; the preparatory turning toward the prey is continued and repeated until the prey is within range and in the median plane which gives this turning action a purposive structure. However, appetitive behavior is likely to be very complex, exhibiting adaptiveness and a complex integration of a conglomerate of many types of movements of different orders from simple patterns of locomotion to conditioned reactions.

Lorenz pointed out that appetitive behavior is purposive; that is, it ends only when a goal is obtained, while consummatory actions are self-exhausting. Tinbergen describes this purposiveness thus:

The end of purposive behavior is not the attainment of an object or a situation itself, but the performance of the consummatory action, which is attained as a consequence of the animal's arrival at an external situation, the sign stimuli releasing the consummatory act.\(^{15}\)

This in essence is the ethologist's view of the organism, an integrated set of blocked potentials for action which are released by specific stimuli in the environment. Thus Tinbergen states:

Even psychologists who have watched hundreds of rats running a maze rarely realize that, strictly speaking, it is not the litter or the food that the animal is striving towards, but the performance itself of the maternal activities or eating.\(^{16}\)

\(^{15}\)Ibid., p. 106.

\(^{16}\)Ibid.
To an ethologist a pigeon pecking a disc is engaging in appetitive behavior which is made rigid by the severe restriction of the experimental environment. The description provided by the cumulative recorder specifically ignores all other activities of the animal which may occur.

This view presents a different picture of the role of internal processes of organisms. Skinner at times grants a certain energy function to the organism's innate responses but views conditioning as elaborating and channeling these contentless forces. The ethologists see this as part of the function of internal motivational factors but see consummatory acts as "capping" various kinds of conditioned and generally flexible activities. Otherwise following a Skinnerian outline the rat should go on eating forever, doing little else. One might say that this is trivial—that inherent in Skinner's use of deprivation and reinforcers is some view of drive reduction or of consequences of behavior on external objects feeding back into the biological organism and changing subsequent behavior. Tinbergen is somewhat doubtful of the "effects" of external consequences upon animal behavior. In a number of cases the consequences are downright harmful to the individual (they raise the likelihood of attack by predators) yet the behavior does not change.\textsuperscript{17} It appears to Tinbergen that the more

\textsuperscript{17}Ibid., p. 153.
correct view for appetitive behavior is that its end is dictated by performance of consummatory actions. In most cases the attainment of objects is coterminous with initiation of performance but even when it is not, there is little or no change in the behavior of most lower animals (fish, birds, insects).

Yet, in many cases, behavior patterns are modified through experience. But what is learned is highly specific to the species and to the performance of particular instinctive activities. This is apparently due to the fact that the animal's sensory capacities are closely related to these activities. "The animal's own world is not only dependent on what its sense organs can or cannot receive. Its sensory world is still more restricted; it is composed of sign stimuli, at least as long as we are dealing with innate responses. This implies that the animal's perceptual world is constantly changing and depends on the particular instinctive activity that is brought into play."\(^{18}\)

This changing nature of the animal's sensory capacities results in 'innate dispositions to learn' which vary greatly in content between species and within the behavioral repertoire of an individual.

To illustrate this variability in learning ability and its dependence upon particular instinctive activities we will use Tinbergen's work on the herring gull. Herring

\(^{18}\) Ibid., p. 37.
gulls engage in a number of innate activities in the care of their young. They are also able to recognize their own offspring after a short period of time, although they are unable to make such distinctions during the first few days after their birth. It appears that the adults learn this. The ability of the herring gull to learn its own eggs is, by contrast, amazingly poor, although to the human observer, the eggs seem more varied than the chicks do. Many other examples seem to indicate that an animal's ability to learn to make discriminations between size, shape, color, type of movement is more or less localized and specific to particular behavior patterns.

That some human behavior cannot be viewed this way is granted by the ethologists. But much of the behaviorists' experimental evidence is from rats and birds and may even not fit rats and birds.

There remains a host of other important phenomena that ethologists have studied which promote more understanding of this framework. For a short and excellent statement the reader is referred to Chapter V of Tinbergen.

Before leaving the ethological framework, a few comments on the more general "purpose" of the knowledge sought are in order. While psychology is interested primarily in causal connections in the ontogeny of the individual, ethology is firmly planted in biology and emphasizes the

\[19 \text{Ibid.}, \text{pp. 145-146.}\]
view of the individual behavior as a juncture of ontogenetic and phylogenetic influences. This forces them into a study of the evolution of behavior since the view that ontogency recapitulates phylogency is now seen as an oversimplification.

The consequence of this evolutionary view is that species are expected to be behaviorally different as they are morphologically and physiologically different. Even learned behavior is held to be constrained by innate character. The term ethology itself denotes such a conception. For this reason some would say that the findings of ethology have little consequence for the study of human phenomena. Although the aim of this paper is not to judge the validity of such a view a few comments should be noted. For one thing the findings of ethology would caution against generalizing to another species on the basis of evidence from a given species. They also demonstrate the fruitfulness of historical versus ahistorical analysis of organisms. But one might contend that the human history that is of primary importance is history based on symbols not evolutionary changes. This whole area is generally ignored by ethologists and its existence denied or given no content by Skinner.

One exception to this general ignoring of history by ethologists is Von Bertalanffy. For Von Bertalanffy man's ability to generate symbol systems such as language and
myths, switches the focus of analysis from phylogenetic evolution to history as the frame of reference from which to explain man's behavior. History, based on symbols, enables an acceleration of happenings as compared to any evolutionary time schedule. Also these systems allow for some autonomy in human phenomena enabling development along principles unique to the system rather than via principles eternally fixed to the biological processes of its creators.  

Before summarizing the view of man that inheres in this paradigm, another area—transformational-generative linguistics—will be described. While it is rooted firmly in ethology it has more to explicitly say about what most behavioral scientists see as a phenomena that is of great importance to humans—namely language.

Transformational-generative grammar

The recent successes of transformational models in linguistics have made possible its rise as one of the most challenging counter-paradigms to the behavioristic model. It is not a new paradigm—many of its assumptions can be traced to early rationalist philosophers, however, its successful application to human phenomenon has lent it new credence. The proponents of this paradigm contend that the

methodological restrictions placed upon admissible "evidence" by the behaviorists unnecessarily limit the scope and type of knowledge generated by behavioristic methods.

On many important points transformational linguists—particularly their chief spokesman Noam Chomsky—share assumptions with the comparative ethologists. However, a complete understanding of this paradigm's particular application to human phenomena requires a description of its application to generative grammar.

The primary point of contention between the two paradigms—behaviorism and ethology—centers around the question of what phenomena are to be explained. What is to be the focus of inquiry? For the behaviorist obviously the answer is the behavior of an individual.

It appears that this assumption is so widely held that the designation "behavioral science" was chosen by the Society for a Unified Science to apply to fields of study in the biological, psychological and social sciences. Although the term was ostensibly chosen for other reasons, non-behaviorists saw it as an attempt to drum out as non-scientists those who did not adhere to the behavioristic ideology.\(^\text{21}\) It would probably be accurate to say, however, that the majority accept the behaviorism label and its assumptions unquestioningly.

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For linguists such as Chomsky behavior is only secondarily important. They see the structure of language as only one of the factors operating in the actual production of verbal behavior, although it may be an exceedingly important one. The phenomenon that they wish to explain is that of linguistic competence, the species specific knowledge of his language that is seen to underlie the normal human's language performance. That behavior does not directly reflect such a competence is taken as irrelevant at least to linguistic studies since many other non-linguistic factors can be presumed to be involved.

A perfectly explicit grammar (that is one that does not rely upon the intelligence of the understanding reader) is called a generative grammar and is a description of the ideal speaker-hearer's competence. Now traditional grammars have focused upon explicit lists of exceptions and irregularities rather than upon universal aspects concerning the regular and productive syntactic process. It is the combination of these two that can give a full account of the speaker-hearer's competence. In the words of Chomsky, the task of the linguist is described thus:

The person who has acquired knowledge of a language has internalized a system of rules that relate sound and meaning in a particular way. The linguist constructing a grammar of a language is in effect proposing a hypothesis concerning this internalized system. The linguist's hypothesis, if presented with sufficient explicitness and precision, will have certain empirical
consequences with regard to the form of utterances and their interpretations by the native speaker. Evidently, knowledge of language—the internalized system of rules—is only one of the many factors that determine how an utterance will be used or understood in a particular situation. The linguist who is trying to determine what constitutes knowledge of a language—to construct a correct grammar—is studying one fundamental factor that is involved in performance, but not the only one. This idealization must be kept in mind when one is considering the problem of confirmation of grammars on the basis of empirical evidence. There is no reason why one should not also study the interaction of several factors involved in complex mental acts and underlying actual performance, but such a study is not likely to proceed very far unless the separate factors are themselves fairly well understood.

In a good sense, the grammar proposed by the linguist is an explanatory theory; it suggests an explanation for the fact that (under the idealization mentioned) a speaker of the language in question will perceive, interpret, form or use an utterance in certain ways and not in other ways. One can also search for explanatory theories of a deeper sort. The native speaker has acquired a grammar on the basis of very restricted and degenerate evidence; the grammar has empirical consequences that extend far beyond the evidence. At one level, the phenomena with which the grammar deals are explained by the rules of the grammar itself and the interaction of these rules. At a deeper level, these same phenomena are explained by the principles that determine the selection of the grammar on the basis of the restricted and degenerate evidence available to the person who has acquired knowledge of the language, who has constructed for himself this particular grammar. The principles that determine the form of grammar and that select a grammar of the appropriate form on the basis of certain data constitute a subject that might, following a traditional usage, be termed "universal grammar." The study of universal grammar, so understood, is a study of the nature of human intellectual capacities. It tries to formulate the necessary and sufficient conditions that a system must meet to qualify as a potential human language, conditions that are not accidentally true of the existing human language, but that are rather rooted in the human "language capacity," and thus constitute the innate
organization that determines what counts as linguistic experience and what knowledge of language arises on the basis of this experience.\footnote{Chomsky, \textit{Language and Mind}, pp. 23, 24.}

It is extremely difficult to do justice to the highly complex system of transformational linguistics in a short description. For an adequate representation of the theory the reader is referred to Chomsky's works, preferably his second chapter in \textit{Language and Mind} which outlines in a simplified version what is involved.

For the purposes of this paper it is possible to describe the theory as one which explains the relation of the surface structure (any string of sounds or phrases in a particular language) to the underlying deep structure (also a system of categories and/or phrases but more abstract) by a set of operations which are called transformations. Thus a set of transformations explains how a human organism could transform a finite system of categories constituting the deep structure into an infinite set of surface structures.

Ultimately, at the highest level of abstraction, it is assumed that the categories and transformational processes are universal and also innate. Correspondingly at another level the syntactic rules of a language are thought to interpenetrate the semantic rules of a specific language. The boundaries between the two, the syntactic and
the semantic component, are empirical questions at present. The main goal, of course, is to outline the grammatical transformations that are structure-dependent, that is those that apply to a string of words by virtue of the organization of these words into phrases. It is, of course, entirely possible to think of other organizing principles that are independent of phrase structure—say, for example, ordering words according to length—but no human language is based upon such operations. This leads one to think that in some way the language-learner has some prior knowledge of these structure dependent rules. The innateness of these principles that Chomsky postulates is based on a number of observations. He has observed that every normal human being learns a language despite wide variation in both intelligence and in individual experiences. Furthermore, knowledge of the language is based upon the limited and often degenerate data of experience available to the child. To insist, as the behaviorists do, that a simple process of "stimulus generalization" or reinforcement can account for all of this seems to deny the existence of complex organizing principles in human phenomenon.\(^{23}\) In fact much of the work in sociolinguistics\(^{24}\) follows a similar line of argument but a transformational linguist would counter that

\(^{23}\)An assumption that is obviously contradicted by biology.

\(^{24}\)Particularly Sapir.
sociolinguistic studies focus upon structure-independent processes and that these in no way account for language competence.

With regard to content of the theoretical paradigm it is possible to note that breadth of the differences between this and the behaviorist paradigm. Whereas the behaviorist is an environmentalist and sees phenomena such as verbal performance as based rather directly upon experience in the ontogenetic sense of the word, the transformational linguist, like the comparative ethologist, devotes his initial concern to that which is independent of all experience. That the postulation of such an a priori is inherently metaphysical seems obvious only to the behaviorists, whose claim of the primacy of experience is at present a non-empirical assumption to the ethologists and others of their ilk. In one view man reacts to experience, in the other man organizes experience. For to state that the universal structure of language is prior to the experience of any individual does not mean to Chomsky that it does not need to be explained itself.

In fact, the processes by which the human mind achieved its present stage of complexity and its particular form of innate organization are a total mystery, as much so as the analogous questions about the physical or mental organization

of any other complex organism. It is perfectly safe to attribute this development to "natural selection," so long as we realize that there is no substance to this assertion, that it amounts to nothing more than a belief that there is some naturalistic explanation of these phenomena.25

Thus an a priori merely indicates the incompleteness of explanation in the developmental chain.

In addition to not accepting the primacy of experience, the ethologists and linguists also reject the equalitarian assumption of the behaviorists either as a fruitful theoretical ploy or as a description of reality. Particular attention is paid to species specific phenomena with as yet little work on the relationships between levels of organization. Chomsky sees language competence as a species specific phenomenon and does not feel that approaches to language via similarities to it in animal signal systems is likely to reveal relevant evolutionary processes that led to this distinctly human phenomena.

What role does Chomsky then relegate to experience in language acquisition? Obviously there must be a requirement that the child at least hear a native speaker to learn a particular language. Chomsky's view differs from behavioristic views in that he does not think language can be seen as being constructed by the child on the basis of external stimuli from speakers and the reward of and for oral performance.

26Chomsky, op. cit., p. 83.
In summarizing this description of ethology and transformational linguistics we will point to some similarities in the way the organism is viewed by both disciplines. The organism is seen as one which transforms experience or stimuli according to a prescribed set of rules that inhere in the nature of the organism. Although these rules have a physiological basis, they cannot be viewed as resulting in the organism selecting from experience upon a directly instrumental or functional basis. In addition, Chomsky offers a view of human language which dictates that the human by the transformation of experience is capable of generating a performance that goes beyond experience.

The most important consequence of these disparate paradigms, the behavioristic and what for simplicity's sake we shall call the transformational, is that the assumptions contained in the paradigms become incorporated into the methods of verification. While one might call such assumptions non-empirical, in that they are less amenable to direct empirical analysis, they become empirically warranted hypotheses by virtue of their role in shaping 'evidence.' The following chapter will contrast how this phenomenon is accomplished in the two paradigms, and how once accomplished, it results in incompatible conclusions.
CHAPTER IV

ASSUMPTION, SUBSTANCE, AND METHOD: COMPARISON
OF TWO PARADIGMS

Introduction

Before delving into the substance of the methodological restrictions contained in the paradigms described in the previous chapter the steps of the main argument will be retraced. This essay has phrased the initial problem as a question of the degree to which the incorporation of theoretical substance into the method of verification or into the organization of "evidence" precludes consideration of different types of evidence. Given the present state of multiple paradigms in the social sciences, to what extent is this situation generative of non-cumulative evidence? Can this situation be viewed as having consequences similar to those of historically changing paradigms in the physical sciences as outlined by Kuhn?¹

The analysis will be arranged in the following manner. First, the behavioristic methods of studying animals will be compared to the ethological approach. In each case

¹Kuhn, op. cit.
the concomitant effect of the methodological tenets on the shape of the evidence will be demonstrated and related to the types of premises that the evidence supports. In addition, the question of the degree to which the methods substantively incorporate these premises initially and preclude the inclusion of evidence that would support other premises will be addressed. In effect the analysis will proceed first from method to evidence to 'verified' premises, and then from premises to method to evidence.

Secondly, the behavioristic methods for the study of verbal behavior will be compared to the methods used by the transformational linguists in the study of language. The effect of these principles of what constitutes permissible operations in the gathering of evidence, and how restrictions preclude other types of evidence, will be assessed. Again, as in the comparison of ethology and behaviorism, the premises that the facts, which result from these methods, support will be described. Also, the possibility that these assumptions or premises were partially included in the original methods will be discussed. In this comparison the type of evidence to be included from Skinner will be different from the type used from Chomsky. This is necessary because while Chomsky has published a number of studies of various aspects of language, Skinner himself has done little research in verbal behavior, although he has published a book and some articles on the subject. The studies of others
that he cites (which are remarkably small in number), and the method of supporting his hypotheses about verbal behavior that his works seem to imply will be used as evidence of how he would proceed with such a study.

This may be a dubious technique. Particularly it will not appeal to those empiricists who think that with proper control truth will out. However, I think it can be demonstrated that it is highly dubious that using a Skinnerian method one would come out with findings or conclusions such as Chomsky reaches.

The two examples of the transformational paradigm will be contrasted separately with the behavioristic paradigm. This is done primarily to keep the analysis clear and explicit. There are differences between the ethologists and the linguists as there are some differences in the ways behaviorists deal with behavior of lower animals and what they are willing to extrapolate to human beings. In the summary the similarities between the ethologists and the transformational linguists will be drawn together from the preceding sections and generally contrasted with the behaviorists.

**Ethology and Behaviorism: A Comparison**

B. F. Skinner has outlined in his many works principles to guide decisions to be made in the experimental analysis of behavior, in order that observations be properly controlled and recorded. "We begin by choosing an organism--
one which we hope will be representative but which is first merely convenient." I think that we can legitimately dismiss the comment about representativeness. The question is representative of what? A convenient organism in most of Skinner's work has been either a rat or a pigeon. The next step is to select a bit of behavior that can be easily identified. This is required because the study will focus upon changes in the frequency of one type of behavior. Oftentimes the bit of behavior chosen, for example, bar pressing, is one which is easily recorded by a mechanical apparatus. "The movement of the bar is usually clear-cut, and it may be made to operate an apparatus by closing an electric circuit." The apparatus should be designed to record the number of responses and the time interval, in order that the rate of response can be determined. The cumulative recorder used by Skinner has the added advantage in that the rate of response can be estimated from the slope of the line and does not have to be exactly calculated for each experimental session.

Skinner emphasizes the advantages of choosing a segment of behavior that can be easily and mechanically recorded. This almost necessitates that the behavior have some immediate consequence upon the environment. This fits in

3Ibid., p. 72.
with Skinner's definition of a scientific analysis of behavior as a functional analysis, that is, an analysis of behavior in terms of its consequences. Given that only responses that have consequences for the external environment are usually recorded, the range of possible relationships is limited to those between the organism's behavior and the external result.

This relationship is further specified in the experimental situation. Skinner notes that the pigeon's response of pecking a disc has certain natural consequences. It may stimulate the bird tactually and aurally, and these consequences may be reinforcing. "We study the effect more expediently, however, by arranging an arbitrary consequence which is clearly reinforcing."\(^4\) Since the analysis is to proceed in terms of the frequency of response, "The behavior should not require much time, and it should leave the organism ready to respond again."\(^5\)

Before continuing with the kinds of methodological operations outlined by Skinner, let us turn to the methods of studying animal behavior used by representatives of the counter-paradigm, the ethologists. Instead of relying heavily upon the laboratory situation, the ethologists suggest that the initial observations of a species are carried out best in the animals' natural habitat. This habitat may

\(^4\)Ibid., p. 103.
\(^5\)Ibid., p. 72.
vary widely for a given species, so some care must be taken to note the general features of the environment. Care also must be taken to make the observer's presence as unobtrusive as possible. Animals are also observed in captivity, in zoological parks, aquariums, and the like, and checked against observations made of them in the wild.

Some differences in the types of evidence undoubtedly are due to these different conditions under which observation takes place. But to a certain extent the choice of observational situation is guided by prior premises about the behavior itself. The ethologists presume that the behavior they study is innate and attempt to infer from their observations the underlying internal organization which accounts for the external aspects. The initial problem in this framework is to describe the range of behavior. Then questions concerning the effect of the environment and the underlying physiological basis of behavior can be investigated. Skinner's interest, on the other hand, is in human behavior and how an individual acquires a behavioral repertoire.

The methodological restrictions that Skinner imposes upon the study of animal behavior preclude the inclusion of many of the facts that support the ethologists conclusions. Skinner characteristically studies animals in isolation.

6N. Tinbergen, Social Behaviour in Animals (New York: John Wiley, 1953), Chapter IX.
from other animals, even the experimenter. This preference precludes the emission of many types of behavior. Tinbergen\textsuperscript{7} described several kinds of behavior that are unlikely to be released by stimuli other than those which are presented by members of the opposite sex (or, in other cases, the same sex) of the organism's species. In this case, individualism as a methodological tenet prevents the attainment of the adequate stimulus environment for the release of certain kinds of behavior in many animals. As Von Bertalanffy notes, to show that the rat's behavior can be controlled in a certain restricted environment is different from demonstrating that this accounts for any very significant amount of the rat's total behavior. Lorenz's work, if accurate, certainly offers a different picture of the rat, \textit{en masse}.\textsuperscript{8} The gentle, docile, rat of the laboratory, in his natural habitat, is organized into superfamilies, who on the basis of nest odor, launch attacks on other families to the point of near annihilation.

To a large extent the apparatus determines the type of behavior that can be studied by Skinner. The selection of infinitely repeatable segment excludes from consideration the recording of long sequences of behavior reported by the ethologists. These sequences are often composed of move-

\textsuperscript{7}Ibid., pp. 112-113.

ments, which as segments (walking, pecking, etc.) are much the same. The differences are in a way in which they are organized in relation to other movements in the sequence or configuration. It is the relationship to the environment of parts or all of the sequence as a unit that is studied by the ethologists. By organizing inquiry in this way the ethologists are able to demonstrate the way in which even conditioned behavior in animals is closely connected to innate patterns. While Skinner's apparatus rules out organization of an internal nature, the ethologists build it into their basic categories.

The requirement that a given segment of behavior be immediately repeatable rules out the inclusion of any type of self-exhausting performance appearing in a Skinnerian analysis. The process of extinction, the diminution of the frequency of behavior, is brought about by withdrawing the reinforcing consequence. This leads to an interesting question. If in the natural situation extreme deprivation and minute pieces of food are not the rule, what accounts for the cessation of one type of behavior and initiation of another type? The ethologists answer this by pointing out that certain types of performances are self-exhausting, or consummatory. The behaviorist's evidence indicates that a reward and the withdrawal of them accounts for the maintenance and cessation of behavior, if not for the initial response. The ethologists argue that animals have
an innate structure that severely limits the opportunism
that Skinner's evidence implies.

Another requirement of the experimental situation is
that it be arranged so that as few stimuli as possible are
present. Generally, this is accomplished by placing the
animal in a small box which provides little possibility of
'contaminating' stimuli entering into the situation. One
or two stimuli are presented, one type of reward is offered.
This results in evidence that supports the empty organism
view. The only capacity that the organism can demonstrate
is the capacity to be reinforced by certain stimuli. The
possibility of an internal organization of this capability
even, is ruled out by the requirements that only one rein-
forcing stimulus be introduced, and that the response be
repeatable after the reinforcement. Actually, all of the
concepts are defined in terms of the outcome of the experi-
ment. If a stimulus increases the frequency of response
(which means that the organism must respond again and
again) the experiment is conclusive. Otherwise, one must
find another apparatus, bit of behavior, reinforcing
stimulus, etc.

Let us now compare this situation outlined above with
the methods used by the ethologists. First, we must remem-
ber that the ethologist does not confine his evidence to
externally observable behavior and events, but whenever
possible, uses neurological and other physiological
evidence also. But this difference does not totally account for the differences between the types of conclusions drawn by the Skinnerians and those drawn by the ethologists, since evidence of the physiological type related to any of the number of hypotheses offered is not at present available. Thus, to a large extent the facts that support the ethologists' generalizations are the result of inferences from the externally observable datum of behavior. In studying behavior, the ethologist seems to first note what kinds of movements are going on and in what sequence. Once he has assured himself that he has seen a characteristic pattern, he attempts to find out what, other than the previous performances, are triggering all or specific segments of the pattern. Often it is not readily apparent what aspect of the total situation is crucial in releasing the behavior and what is irrelevant. In this situation, dummy objects, with only one or two aspects of the whole object that appears to release the behavior, are introduced. For example, a male robin will often feign an attack upon another male that enters his territory during the mating season. Using several different types of dummies, it was discovered that only the red breast of the entering male is responsible for the behavior being released.\footnote{Tinbergen, \textit{The Study of Instinct}, pp. 28-29.} This highly specific selection of stimuli which seems to characterize both innate and conditioned behavior demonstrates that in
many instances something in addition to an S-R bond is required to account for the behavior.

The result of this type of approach is that a sequence of behavior is seen as the result of a host of factors. Some of these factors are external stimuli, others, the organism's own performance. In addition, more indirect motivational factors, such as temperature and hormone levels, contribute to the initiation of a pattern. Similarly, the effects of innate activities upon learning seems to be much more complicated than the S-R theorists' explanations would indicate. Whereas, from a S-R point of view, one might think that stimuli which always appear with innate releasing stimuli, would by a process of reinforcement, become paired with it, this is seldom the case. Yet, in specific instances there seems to be fairly conclusive evidence that learning of this sort is taking place.10

Another technique which is both based on the premise that the organism's internal system organizes its responses to selected segments of the environment and which leads to evidence of the nature of this internal structure, is the presentation of multiple stimuli that the organism is known to be capable of responding to in some situations. In some cases, the natural environment affords many such cases, making observation easy; in other cases, the observer must

10 Ibid., pp. 116-117.
take a more active role and arrange the situation himself. This type of experimentation has led to the discovery of some underlying principles governing different levels of behavior. In addition, it has provided some insight into the nature of so-called 'displacement activities.'

The consequences of starting with a particular method go beyond the initial findings. These findings imply still further questions. In *Science and Human Behavior*, Skinner raises a number of questions that would appear to be consistent with and follow from the findings of his experiments. Skinner suggests that certain questions concerning what are called by others, drives, can be more easily answered when stated in terms of deprivation and satiation. One such question is "Which drive is the strongest?"¹¹ Tinbergen seems to say that this question is generally irrelevant. He hypothesizes that the interrelationship between instinctual centers is such that when one is activated the receptivity to stimuli that release other activities is lowered.¹²

Skinner also suggests that in studying satiation, time be taken as the main variable. This presents a neat quantifiable variable but time per se (or by the clock) is

seldom in and of itself the relevant factor.\textsuperscript{13} This is not to deny its correlation with external and internal events which could make it a useful indicator under some circumstances.

Some decisions of course have to be made to allow research to proceed even in an atheoretical approach—if there is such a thing. We often de-emphasize the importance of starting points. The division of labor among disciplines studying human phenomena is spoken of, in our more tolerant moods, as being merely different paths to the same goal. The starting point in the circumstance reported above seems to generate tentative conclusions along with an orientation to further research. Thus, depending on whether one starts with field observation or a carefully controlled experimental design, certain new questions seem relevant. Perhaps the paths are presently parallel and not destined to intersect. We shall have to go further to see if these consequences are isolated or fairly general.

In the previous section we have been examining two alternative paradigms and the way in which they deal with sub-human behavior. We have attempted to demonstrate that by adopting different methods of acquiring evidence the proponents of each of the paradigms lessen the likelihood (and in most cases rule out the possibility completely) of

\textsuperscript{13}Tinbergen cites a number of examples where the apparent periodicity is given by the organization of seasons and the rate of internal happenings.
their reaching similar conclusions. Further, the different modes of inquiry make the discovery of evidence by researchers of one paradigm that would be suitable to the requirements of the other paradigm very incidental. Thus the different methods lead to the accumulation of preponderantly different types of knowledge.

Yet, the behaviorists and the ethologists study animal behavior for essentially two dissimilar purposes. The ethologist seeks to find out about the principles that describe and explain the behavior of the species that he observes; the behaviorist studies sub-human animals in order to learn something about the principles that explain human behavior. This fact alone explains why behaviorists focus upon how their experimental subjects learn, and ignore the innate aspects of animal behavior.

In the following section we will compare transformational linguistics and its methods in the study of language to those of the behaviorists in the study of verbal behavior, to ascertain whether or not the same process occurs. The transformational linguists incorporate into their study many of the same premises about the role of the organism in the learning process as do the ethologists and similarly refuse to accept the methodological restrictions laid down by the behaviorists.
Behaviorism and Transformational Linguistics

Let us begin by noting some of the ways in which B. F. Skinner deals with the subject of language performance in his book *Verbal Behavior* and then compare it with the way in which N. Chomsky deals with language competence and performance.

Skinner writes,

> In all verbal behavior under stimulus control there are three important events to be taken into account: a stimulus, a response; and a reinforcement.\(^{14}\)

Thus he places the analysis of verbal behavior solidly within his general theoretical framework. In line with his overall theory, Skinner also suggests a functional analysis of verbal behavior. The starting point for any functional analyses is the classification of the variety of behavior according to its consequences. If a verbal unit can be shown to have the same consequence, differences in the form of sound or behavior are of little import for a causal analysis. The overall assumption is that directly or indirectly a person's verbal performance is controlled by external events. The possible functional relations in verbal behavior as outlined by Skinner are as follows:

> In a mand, a given form of response which characteristically produces a given reinforcement varies in strength with the state of deprivation or aversive stimulation appropriate to that

reinforcement. No prior stimulus determines the specific form of response.\textsuperscript{15}

An example: If a child's mother characteristically gives him water when he says "water," then the child is likely to say "water" when he has been without water for an hour and even more likely if the length of time increases. There seems little doubt that this could be brought about under controlled laboratory conditions and it seems likely that the rate of response during extinction would be somewhat related to the amount of water given during the experiment.

In echoic, textual and intraverbal behavior, the response is determined by a prior verbal stimulus—auditory in the first case, written or printed in the second, and both in the third. Control is concentrated in the stimulus by generalizing the reinforcement.\textsuperscript{16}

This last sentence should be taken to mean that the control is there by virtue of the fact that development of the response is useful to the developer and everybody else. Skinner has a decided tendency to view any usual consequence as positively reinforcing. The echoic operant is a sound stimulus which generates a response that point by point corresponds to the stimulus. The textual operant does likewise with written stimuli. It seems clear that Skinner could not really mean this. It is obvious that parents and teachers settle for responses that do not exactly duplicate

\textsuperscript{15}Ibid., p. 185.

\textsuperscript{16}Ibid.
their own writing style or certain aspects of their speech quality. Intraverbal operants are under diffuse controls and do not correspond to the sound or look of the stimulus. Responses to "Two plus Two" and "How are you?" are examples.

The tact is the most important of verbal operants. Generally by this term Skinner means naming behavior. Tacts are operants in which "a response of given form is evoked (or at least strengthened) by a particular object or event or property of an object or event. These objects or events are non-verbal.17 Much of his analysis is convincing in a commonsense sort of way. But one must assume, to make these explanations plausible, that one has to have experienced the physical stimulus to make the same response as others make.

Chomsky's example of "There's a homicidal maniac in the next room"18 points to a not isolated instance in which one's previous history of contact with the physical object (homicidal maniac) has little to do with one's response.

Up to now Skinner cites the advantage of his analysis as one of not having to take the speaker into account as anything but a locus. The speaker is viewed as an interested bystander under the control of variables in the environment and his own history. In a class of operants called autoclitics Skinner attempts to analyze such phenomena as order, design, editing, and selecting as terms for

17 Ibid., p. 81.
behavior which is evoked by or acts upon other behavior of the speaker. Autoclitics function to describe, qualify or otherwise comment upon verbal behavior and thus clarify or alter its effect upon the listener. In general, grammar and syntax are to be seen as autoclitic processes.\(^{19}\)

Skinner's views of ordering of verbal behavior reveal how he attacks the problems of grammar and syntax. The ordering and grouping of responses also have several functions. In the first place, speech sounds are ordered in the patterning of responses. Apart from the spectra of single speech sounds, the only dimension of verbal behavior is temporal, and order is therefore an important property. Tip and pit are different responses, as are lookout and outlook. Secondly, verbal responses may be ordered to correspond to the order of the relevant stimuli. . . . Thirdly, order may arise from the order of verbal stimulation in the behavior of the speaker. A "train of thought" in free association follows the order in which verbal stimuli evoke other verbal responses. In the recitation of a long passage the order is due to similar intraverbal linkage. Fourthly, order may be traced to the relative strength of responses in the current repertoire of the speaker. Other things being equal, the strong response occurs first. Lastly, we have to note rhetorical order. In the response Him I despise the position of him may be in part a function of relative strength, but the rhetorical pattern has been designed for a special effect upon the listener.\(^{20}\)

The statement, "other things being equal," surely must refer to the syntactic structure of the language. What one can do with the order to obtain an effect upon the listener depends upon the rules of the language which one is

\(^{19}\)Skinner, Verbal Behavior, pp. 313-315.

\(^{20}\)Ibid., p. 333.
speaking. In Latin, the order of adjectives and objects can hardly be said to be due to the different order of relevant stimuli or different strengths of a response, unless the order itself is taken as the indication of both.

Chomsky's work in language is a reaction to what he sees as inadequacies in previous approaches. These non-transformational approaches in linguistics imply models of acquisition that are not far removed from paradigms of learning in contemporary psychology.

Specifically, Chomsky maintains that the assumptions that both behavioristic psychology and non-transformational linguistics make about what the organism brings to the learning task are incorrect. The model of concept formation that these paradigms offer is based primarily on some notion of matching or similarity or possession of a common property from some fixed set of available properties. Skinner's work on verbal behavior, if it is to describe any significant aspect of linguistic performance must invoke some notion of generalization from a reinforced event to similar events.

Chomsky maintains that transformational models (which he feels more adequately describe a language) could not possibly fit this "learning theory" model of acquisition. He believes that there is ample evidence to support attributing far more content to the capacity of the organism.

Before examining with the acquisition model which relates directly to learning we must deal with Chomsky's theory to see by what criteria he claims the theory fulfills the minimal requirement of descriptive adequacy. Only if his claim that his model is a more adequate descriptive model is warranted does the demand for a different model of acquisition have any legitimacy.  

Chomsky feels that modern linguistic theories have largely been concerned with strictly observational adequacy. This is not to say that this is only the recording of all utterances observed. Even at this level some criterion of relevance is employed. Generally what has been done is to note what is said by speakers of the language. In accounting for the lexicon one would note for example N →/pik/, in effect we get a taxonomy. At the level of syntax then one would note the sentences that are uttered and leave it at that because to describe a sentence as a unit one would have to assign a structural description (i.e., a noun would have to be described as a direct object, subject, etc.—in terms of its relationship to other forms in the sentence).

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22This point is made by Hockett, however, in this case as in all cases it is difficult to separate "evidence" that relates to a acquisition model without it having relevance for description and vice versa.

Though this approach might appear exhausting, the result would be a kind of knowledge, and it could be made consistent in its criterion of observational relevance. Though generally much less clear about what his response units are, this is what Skinner tries to do, classify verbal behavior in terms of its correlation with external stimuli (either the behavior of others, attributes of objects, one's past behavior, or, sometimes, combinations of these).

Chomsky feels that these approaches which imply a set of habits as the basis of language cannot account for the actual and possible understanding and performance of native speakers. To account for new sentences some notion of similarity to old sentences must be evoked. Even Skinner has to do this if he is to account for any significant aspect of language performance. Even more so in the behavior of the adult reinforcing the performance of the child, we must assume that in order to do this the parent must perceive some similarities to his own performance in order to respond. Chomsky maintains that what this framework cannot tell us is—what similarities, in what contexts?

On what kinds of evidence does Chomsky base this conclusion? For clarity's sake here we will restrict our examples to the syntactic level of a language rather than the phonological level (which is less familiar to non-linguists) or the semantic level (which is theoretically less integrated into transformational linguistic theory).
Basically Chomsky is proposing a model that generates only grammatically correct sentences of a language by a process of generating them from top to bottom not first to last, or left to right. This is made possible through the use of transformational rules that specify how and in what context (within a sentence) the top elements (very abstract) can be changed to yield a surface structure. To illustrate we will use one of the examples Chomsky uses, \(^{24}\) "A wise man is honest." The sentence can be characterized at a deeper level as containing the proposition, "A man is wise," imbedded within the sentence, "A man is honest."

Expressed in tree form, from deep structure to surface structure it would look like this.

Notice that in 1. the relationships "subject of" and "predicate of" hold for the sentence that directly dominates the NP (Noun Phrase) and VP (Verb Phrase). This shows how the deep structure plays a role in determining the meaning of the sentence on the surface.

How are the deep and surface structures related? In the example given we can transform 1, the deep structure, into 2, the surface structure, by performing such operations as the following ones.
3. a. assign the marker $wh$ to the most deeply embedded NP "man"
b. replace the NP so marked by "who"
c. delete "who is"
d. invert "man" and "wise."

What is the value of all this? To quote Chomsky:

Even this artificially simple example serves to illustrate some properties that appear to be general. An infinite class of deep structures much like / can be generated by very simple rules that express a few rudimentary grammatical functions, if we assign to these rules a recursive property— in particular, one that allows them to imbed structures of the form $[s....]_s$ within other structures. Grammatical transformations will then iterate to form, ultimately, a surface structure that may be quite remote from the underlying deep structure. The deep structure may be highly abstract; it may have no close point-by-point correlation to the phonetic realization.

Knowledge of the language involves an implicit mastery of these processes. To grasp the meaning of this think of the underlying deep structure of a class like 1, applying the finite rules, given a recursive property, to yield, the phonetic realization: "The thoughtful, good, prudent, clever, moral, . . . , man is honest."

To choose one example makes the whole scheme a bit arbitrary, which it is not. But this is not the place to outline a language like English from beginning to end. Suffice it to say that transformational grammars are tentatively accepted as correct because they solve a number of problems that could not be solved previously. For the sake of adding credence to the imputed value of the example above

\[25^\text{Ibid., p. 27.}\]
we will describe some of these solutions and give references to them.

One previously unsolved problem is that the surface structure of a sentence, in itself, generally gives very little indication of the meaning of the sentence. For example, the two sentences below have quite ambiguous meanings which are not indicated by the surface.

"I disapprove of John's drinking,"

"I disapprove of John's cooking."
The same is true of yet another kind of sentence,

"Starving children can be dangerous."

An explanation that uses transformations can account for this ambiguity by showing that there are two different underlying deep structures thus accounting for two meanings of one surface form.

Certain deletions that are permissible in English when new sentences are formed by conjunction (connecting with an "and") are also explained through the transformational rules. 27

In brief we can say that the transformational linguists have gathered a good deal of evidence to support their thesis. For a more detailed description and discussion of the evidence, see J. Katz and P. Postal, An Integrated Theory of Linguistic Descriptions (Cambridge, Mass.:

26 Ibid., pp. 27-32.

27 Chomsky, Syntactic Structures, p. 36.

A number of other external considerations pertaining to language acquisition and understanding support the plausibility of the model also. Some of these have been mentioned before but we will restate them here more systematically. First, there is the fact that human beings seem to be capable of taking incomplete, and in many other ways degenerate, evidence from their own listening experiences and generating "new," yet acceptable, utterances. That is, they do not merely imitate, then expand by trial and error, and their capacity for generating seems limitless on the surface. For example, in one book it is difficult to find many sentences that are identical. Also, increasing the number of books compared does not seem to increase the proportion of repeated to unrepeated sentences. With this being the case any behavioristic study of frequency or increase in frequency with reinforcement is senseless, since the probability of any given sentence is near zero. And since any concept of idea or the like is anathema to the behaviorists, they would most certainly have to use utter-
ances as the datum. Some behaviorists have argued that it cannot be that every response is reinforced but instead that it is a class of responses. Thus the class of acceptable sentences constitutes a response class like a set of bar-presses. However, until the condition that defines membership in this class is established the concept is empty. If the condition that defines membership involves the idea of those generated by a grammar, then we are back to the transformational view.

We can add to this, that although our examples relate to syntax it seems that a model which also incorporates phonology, the phonetic representation, and semantics, the rules governing the selection of referents, into one processual model, and relates one to another, would parallel the steps of the language learner. There seems to be some indications that an integrated theory based on a system of transformational operations could describe and explain this learning phenomenon.

Let us again attempt to make explicit the assumptions that are incorporated into the paradigm. First there are


2Chomsky, Current Issues in Linguistic Theory.

the decisions as to what evidence is relevant. Verbal performance is not what the model can explain. Performance is probably very dependent upon the condition of the speaker. A model of the phonological performance of the speaker would have to include a host of these conditions, i.e., food in the mouth, a bad cold, drugs in the biological system, etc. Yet there is no reason to believe that just because at the time we observe a speaker and he is eating that he has lost his ability to form phonologically correct utterances. Thus Chomsky uses the concept linguistic competence. This is a sort of covering law model that predicts the range of possible sentences in a given language or languages.

Another component of the paradigm is the specification of interrelationships between what we might call levels, phonological, syntactic, and semantic, as a set of finite rules. But notice what type of relationships these rules posit. They do not posit a point by point correlation between levels. Instead they state that the relationships is a transforming one. That is, certain items can be deleted at certain levels, inverted, conflated, substituted, refined, etc. Some rules contain options in certain contexts. The overall result is that the arbitrary appearance of surface structure can be explained.

Now the positing of these types of relationships requires some defense by Chomsky in terms of how a speaker comes to do this and yet be fairly unconscious of what he's doing. To answer this question Chomsky invokes an innate
mental apparatus. His critics have shown a great unwillingness to accept this assumption. Since there is very little neurophysiological evidence one way or the other, the innateness hypothesis often appears as a "What else could it be?" type of explanation.\(^1\)

In both paradigms we have some processes taking place that reveal the basis of some of the mutual exclusiveness of the two paradigms. Both of these paradigms offer some explanation of how human "knowledge" is acquired. Skinner uses an inductive method and logic to show how it is that induction is the basis of human knowledge acquisition. A general class of responses (knowledge could be considered as a response) is built up through a process of carefully reinforcing contingencies. In effect the content of the theory and the meta-theory are the same.

With transformational linguistics also we find that the method of constructing the evidence is justified by the evidence. If the behaviorist's method is the only possible way of arriving at valid knowledge then the transformationalists' theory is wrong. Chomsky quite explicitly recognizes the implications of what he says for the restrictions of the correlational method of the behaviorists.\(^2\) He is

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\(^2\)Chomsky, *Language and Mind*, p. 79.
also quick to cite philosophers of science like C. S. Pierce who have developed notions similar to his own.

Insofar as language acquisition can be said to be similar to knowledge acquisition the content of Chomsky's system supports the logical relationships it employs, because they are the same. Given the enormous disparity between experience and knowledge that abounds in all cultures and the difficulty that behavioristic approaches have in explaining this fact, why not accept the merging of the data and the theory? It seems that Hockett brushes upon this type of point when he brings up some types of relationships that linguists have had little to say about.

In the model of knowledge acquisition that Chomsky's work implies, the experiential world that the language learner is dealing with consists of words. But the way in which one can negate and transform "evidence" that consists of sentences surely differs from what is required to negate other experiences. The selection of a hypothesis about words and sentences might justify a different method in the study of this phenomenon but not be equally applicable to the methods of verifying hypotheses about other experiences.

As Hockett says:

One cannot begin a sentence with a house. If one wants to say something about the house one must use, in a sentence, a symbol that refers to that house. But one can begin a sentence with a word, even when one's intention is to say something
about that word rather than about what it refers to.33

This point is made not in the defense of behaviorist methodological restrictions but to raise some questions about the generality of the transformational model in dealing with some of the problems psychologists have set for themselves. By extension of the transformational model they would become pseudo-problems, although there seems no imminent danger of this happening. Within the current academic division of labor perhaps two different paradigms with two different perspectives on inquiry can maintain themselves, if they are in separate departments, without ever coming to grips with the challenge each presents the other.

Along with the current trend in Western thinking of imputing great importance to experience in explaining human phenomena there is another trend that places great emphasis upon history or change. Both of the above paradigms imply different things about the nature of history. For Skinner historical change like evolutionary change is a matter of random slippage. In the replication of reinforcing contingencies, including both physical and behavioral consequences, there are random changes that feedback into the organism and produce "new" responses.

33Hockett, The State of the Art, p. 100.
For Chomsky a mark of ultimate success of this theory would be to show no basic change in linguistic systems at an abstract level. For ultimately he hopes to demonstrate through the study of all known natural languages that they have a universal deep structure. The form and content of the deep structure can then be called a species specific characteristic of human beings. It is part of human nature. The apparently changing surface structures of language then become but infinite instances of the same deep structure and a limiting and limited set of rules. Fundamentally there is no change.

Discussion

In the preceding sections the types of methods employed in the investigation of animal behavior by the ethologists were compared with those used by B. F. Skinner, a proponent of behaviorism. Then behavioristic methods of ordering evidence concerning a specifically human phenomenon, language, were compared to those used by the transformational linguists. In both cases the differences between the methods of the two are sufficient to make it extremely unlikely that any similar conclusions would be reached using these methods. Indeed each paradigm's proponents have gathered evidence that substantially supports conclusions that are almost diametrically opposed to the conclusions of adherents of the counter-paradigm, with regard to both human and sub-human behavior.
These two distinct methods of inquiry are related to other considerations of a more abstract nature that surround inquiry. I think that we can correctly label these more general considerations assumptions because although they are incorporated into the methods of inquiry, they are derived from views about man, his position in nature and history, and values concerning the goals of knowledge that go well beyond the present factual basis of either paradigm.

It is the agreement on these types of considerations that accounts for the affinity between the ethologists and the transformational linguists. While the ethologists are marshalling evidence concerning the innate organization of animal behavior, Chomsky maintains that it is of a type that will enable an eventual higher synthesis with the type of knowledge that he seeks. The preference for a functional or non-functional type of analysis is tied in with assumptions concerning the nature of man and other organisms and the goals of knowledge.

Skinner argues that a scientific analysis of human behavior is a functional analysis. Although the term functional has other connotations, Skinner uses it rather straightforwardly to mean an analysis of behavior in terms of its consequences. Such an analysis guides the construction of evidence that demonstrates that external consequences feedback into the organism as positive or negative
reinforcing stimuli and alter the course of subsequent behavior. This process, "which the human organism shares with other species, alter(s) behavior so that it achieves a safer and more useful interchange with a particular environment."

The onward and upward progression of behavior implied in such characterizations seems a bit strained to say the least. The truth value of this is open to question on a number of counts in regard to language. For example, in speculating about the origin of language Skinner says, "Plausible advantages are not, as such, an explanation of the origin and maintenance of verbal behavior, but they point to the reinforcing contingencies which are." He continues to point out that verbal behavior allows man to extend the range of his co-operation with others, a reinforcing consequence that accounts for the maintenance of the behavior. One could as easily say that without verbal behavior the range of possible deception of others would not be so great. In fact the signal systems of animals are more truly informative. Human language can be used to inform or mislead, but this does not seem to have extinguished verbal behavior.

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But the problem here is not so much whether this view is right or wrong but the way in which it orients inquiry. Much of Skinner's non-experimental work is filled with the chasing down of possible justifications, in terms of survival criteria, of consequences of behavior. In the long run, it seems to show only that anything can be viewed as having instrumental consequences. Even the consequences of a child hearing a bell tinkle when he shakes is reinforcing because; "Any organism which is reinforced by its success is manipulating nature, regardless of the momentary consequences, will be in a favored position when important consequences follow." Performance itself is reinforcing. In fact, by not emitting verbal behavior (by keeping quiet) we can reinforce someone. Or, if nothing else can be found, Skinner appeals to the possibility or expectation of future consequences (although they may be objectively nonexistent) as reinforcing.

Such views make Skinner's claim that behavior is maintained through reinforcing contingencies seem empty. Insofar as men do anything, it must be reinforcing to do so. This results because the concept reinforcing stimuli lacks independent identifiability.

37 Skinner, Science and Human Behavior, p. 78.
38 Skinner, Verbal Behavior, p. 199.
39 Ibid., p. 206.
There is undoubtedly much evidence that both human and non-human organisms show a certain amount of outer-directedness, although for humans the degree of this outer (or other) direction is linked to particular types of societies. Given such evidence it seems legitimate to focus upon the ontogeny of the individual in a scientific analysis. Inevitably, this focus seems to present an image of the organism as opportunistic creatures oriented in their behavior toward securing individual and collective survival. Within the behavioristic paradigm it is unnecessary to posit that there is any conscious purposiveness to the direction of behavior; the processes of reinforcement and extinction are sufficient to account for it.

The ethologists and followers of Chomsky operate with a view of man and other organisms as being inner-directed. That is, they seek to determine what limits the impact of experience whether it be the experience of the individual or the phylogenetic experience of the species. It seems highly probable that much of the appeal of Chomsky's innateness hypothesis is because it seems closer to the human essence. If one hypothesizes that man inherits an ability to negate and transform experience in structured ways, as Chomsky does, or that animals negate certain perceptual content because of their innate organization, then environmental determinism is partially refuted.
Tinbergen, in his conceptual distinction between appetitive and consummatory behavior seems to conceive only of performance ends in the ontogeny of the individual. But he is unable to maintain his assumption steadfastly given the sampling limitations in his particular area of analysis, animal behavior. Since the species he is studying are surviving, survival is an empirical fact. But does the process of evolution actually only show adaptations that serve survival ends? (even for the species?) Does it not show change and extinction as well? Von Bertalanffy follows this line of thinking but wanders into a teleological evolution of mind concept.

Another ethologist, Lorenz, states that,

Adaptation of the a priori to the real world has no more originated from "experience" than adaptation of the fin of the fish to the properties of water. Just as the form of the fin is given a priori, prior to any individual negotiation of the young fish with the water, and just as it is this form that makes possible this negotiation, so it is also the case with our forms of perception and categories in their relationship to our negotiation with the real external world through experience.  

It is not clear from his work though exactly how the evolutionary process is viewed by Lorenz.

All that is clear is that the idea of random successive adaptations to random changes in the environment is rejected by ethologists like Von Bertalanffy.  

40 Lorenz, op. cit., p. 81.

41 Von Bertalanffy, op. cit.
process that seems to be more than coincidence Chomsky says that "natural selection" is an explanation without content.\(^{42}\) This assertion seems to deny the applicability of respected evidence that supports what might be called the "outer-directed" theory of evolution.

Perhaps we are extending these statements too far and creating certitude where there is but a tendency. However, that the assumptions of infinite systems, or the tendencies toward such assumptions, seem to be what unhinge the ethologists and the transformational linguists from a functional analysis and it seemingly inevitable components, utilitarianism and instrumentalism.

Hockett, an unreconstructed structural linguist (structural linguists being more likely to accept behavioral methodological restrictions as a starting point), presents a thorough critique of Chomsky's assertion that languages make infinite use of finite means and that any theory of language must adequately account for this fact.\(^{43}\) Hockett, quite correctly, I think, apprehends that this "theorem" is crucial, not incidental. Other well-defined and infinite systems exist, some systems of logic and mathematics and some games, such as baseball. Hockett argues that languages

\(^{42}\)Chomsky, Language and Mind, p. 83.

are not infinite systems because they are not well-defined and therefore supposed alogrithmic characteristics are irrelevant.\textsuperscript{44}

If one accepts Chomsky's assertion it means that ends (possibly even in the limited sense of having completed a process) are not built into language systems.\textsuperscript{45} One consequence of this is that this linguistic ability enables men to perhaps say what they don't "want" to say, and state relationships between words that they have not experienced, nor ever will, as anything but words.\textsuperscript{46}

The dispute rages on in linguistics. One can cite instances of the infinite generations in language. For example, childhood verses like "the House that Jack Built" could be extended for infinity in the same way that one could calculate numbers for infinity. On the other hand, humans do not do either because of the constraints of human existence.

Returning to Skinner, there is another interesting discussion that he partakes in which relates to what I have

\textsuperscript{44}\emph{Ibid.}, pp. 104-113.

\textsuperscript{45}Chomsky, \textit{Language and Mind}, p. 80. Chomsky's comments on works concerned with epistemological development in children demonstrate his remarkable attraction to explanations based upon elaboration of innate basic concepts. "If this analysis is correct, then what we are observing is not a succession of stages of intellectual development, in Piaget's sense, but rather slow progress in bringing heuristic techniques into line with general concepts that have always been present."

\textsuperscript{46}Thus "false" consciousness becomes an objective phenomenon with an objective source.
termed the infinity assumption. In a reply to Carl Roger's statement "We might then value: man as a process of becoming...", Skinner retorts "Man as a process of becoming --what?"^7

Let us digress again into another type of assumption that is intertwined with the instrumental-infinite assumption and leads into still another.

The behaviorist seems to operate within an implicit assumption that if there is no difference it makes no difference and empirically verify it by attempting to show the converse. Questions concerning human nature particularly are a waste of time, although the "nature" of animals is similarly ignored. This assumption and its incorporation into the experimental method by choosing variables that show few species restrictions in animals and through extrapolation to "analogous" human situations yield an empirical equalitarianism.

Chomsky's theory, on the other hand, deals with the essential sameness of humans and seeks to make it problematic and inquire into the content of human faculties.

I have yet to find a statement in Chomsky or Katz or Postal or Fodor concerning the possible uses of their knowledge. Chomsky implicitly hints that he seeks to destroy the scientific legitimacy of behaviorism and its

^7Skinner, Cumulative Record, pp. 32 and 34.
concomitant technique of control (from some points of view, an excellent use).

By contrast, Skinner in his work notes that "Purely formal analysis of grammar and syntax . . . are of little interest here, where no form of verbal behavior is significant apart from its controlling variables." Since behavior is ultimately controlled by its consequences for Skinner, I think he accurately judges the import of grammar for his framework. The consequences that might follow from this knowledge are not the type that yield controlling variables.

The difference seems to revolve around a content-control goal of knowledge. Chomsky states that unless we first find out what it is that is acquired we can only obtain a trivial knowledge about how it is acquired. But this proposal is based upon the view that the organism negates, conflates, and transforms experience according to a set procedure. The organism obviously must have some experience to negotiate but this empirical evidence can be incomplete. That is, in the area of language:

It is easy to show that the new events that we accept and understand as sentences are not related to those with which we are familiar by any simple notion of formal (or semantic or statistical) similarity or identity of grammatical frame. Talk of generalization in this case is entirely pointless and empty.

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49 Chomsky, "Review of *Verbal Behavior*," p. 56.
The benefit of grammatical study is that:

If we know, from grammatical study, what choices are available to him and what conditions of compatibility the choices must meet, we can proceed meaningfully to investigate the factors that lead him to make one or another choice.\textsuperscript{50}

If we do not proceed on the basis of this knowledge we will meet with only limited success.

In addition to the lack of interest in control and manipulation in the practical sense, it is not used as a criterion of valid knowledge either. Instead, Chomsky focuses upon prediction and explanation. The adequacy of a proposed theory of a grammar is based on the probability of the occurrence of sentences generated by that theory as in question.

Those who subscribe to the opposite paradigm see all of this as too complicated. Even if an organism does transform his experiences in rather complex ways, how much impact does this have? Human life is first and foremost a response to external conditions. As Hockett says, Chomsky's view puts one in the position of the boy who gets interested in butterflies; when his mother brought him a thick book on the subject; it told him more than he wanted to know.\textsuperscript{51}

This choice of what we want to know is a real problem because, in part, it determines one's choice of criterion of truth. Skinner states that the goals of inquiry are the

\textsuperscript{50}Ibid., p. 57.

\textsuperscript{51}Hockett, \textit{op. cit.}
prediction and control of human behavior. However, the
goal of prediction is dropped rather quickly. He states
that even if it can be shown that some aspects of behavior
can be predicted on the basis of genetic constitution this
knowledge is of little use. Such factors cannot be
manipulated in either experimental analysis or practical
control. Throughout Skinner's work we find a near obsession
with the development of a technology for controlling human
behavior. Certainly his utopian novel, Walden Two, can be
seen as the social benefits that could be reaped from
adhering to the goals he sets for psychology.

In fact, Skinner's selection of reinforcing stimuli
for the experimental situation shows as much if not more
concern with practical control as experimental manipulabil-
ity. Skinner enumerates a number of primary reinforcers
such as food, water, and sexual contact, as well as escape
from injurious conditions. Presumably the key to the
reinforcing power of these stimuli lies in their connection
to the well-being of the organism. Since oxygen fulfills
these requirements one wonders why it is never used or dis-
cussed as a primary reinforcer? Both the oxygen content or
the carbon dioxide level in the air seem to be controlable
and manipulable in an experimental situation. If either


stimulus proves to be reinforcing, as some evidence indicates it is, I am not sure what one would extrapolate from it to explain human behavior. However, the whole question is overlooked in the behavioristic literature. This observation seems to illustrate a tendency in Skinner's work to rely heavily upon the present culturally defined reward (or deprivation!) system as a source of reinforcing stimuli. To do so certainly facilitates the goal of the scientific study of human behavior that Skinner proposes; to enable us "to make optimal use of the control we possess."55

There is a certain logic to choosing manipulation and control as primary criterion for the adequacy of knowledge. As Sjoberg and Nett state much of the success of modern science has been, not that the knowledge made predictions possible, but that scientists were successful in the manipulation of events to produce predictable results. Just as the practitioners of the physical sciences demonstrate the adequacy of their control through the creation of a tremendous technological apparatus, the knowledge of behavioral sciences could be validated by the creation of a technology for controlling human behavior.


Summary

This discussion and analysis, will be summarized by focusing on how the two paradigms deal with three aspects in different ways. These aspects are equalitarianism vs. hierarchy, environmentalism vs. the a priori, instrumentalism vs. endlessness.

Behaviorism incorporates into its methodology an equalitarian assumption. By rigidly controlling the experimental variable and not allowing contamination by an interaction of other stimuli, they allow for no internal selectivity on the part of the organism. Only those responses which have a known history of reinforcement (by definition the ones the experimenter can definitely say he has brought about) are considered admissible behavior. The recording apparatus is devised so that it cannot record different movements, only the consequence on an object. Thus the descriptions of response patterns show little difference from species to species because the methods of obtaining them are partially designed to mitigate against inclusion of any species-specific phenomenon. Then the conclusions are extrapolated to "analogous" phenomenon where satisfactory control is impossible, on the assumption that the processes are basically the same.

In the other paradigm hierarchy is the basic tenet. Combining neurological evidence and field observation comparative ethology concludes that the organism is internally
organized in a hierarchical pattern that results in changing receptivity to stimuli and an inhibiting relationship between responses at some levels and relationship of coordination at other levels. To supplement this view of lower animals transformational linguistics proposes a model of man's intellectual capacity that is hierarchically arranged from abstract deep structure to concrete surface structure. It posits that man generates his performance from top to bottom (deep structure to surface structure) in the process transforming his experience.

Each paradigm adopts a logic in keeping with its assumptions. Behaviorism restricts itself to point by point correlation. The other paradigm adopts an abductive model that allows for systematic reconceptualizing. And in their explanation of human knowledge each empirically legitimates their choice of logical system by concluding that this is the basis of the human knowledge acquisition process.

Behaviorism attempts to explain performance totally without recourse to characteristics of the organism. The responses of the organism are reactions to stimuli in the environment. In part the equalitarian assumption emanates from this assumption and is itself refined by it. All differences are due to different experiences. What is not different is not problematic. This assumption is incorporated into the criterion of valid knowledge. Only those factors which are externally manipulated can be incorporated in the
empirical demonstration if the resultant knowledge is to be useful (i.e., increase our control of behavior).

Comparative sociology and transformational linguistics attempt to search for evidence that indicates what it is that the organism brings to experience. Before we can see how conditioning shapes behavior we have to determine what is innate. Then, we may proceed to the interrelation-ship between the two. For the transformationalists again it is first "essence" then experience. Both ethologists and transformational grammarians by focusing upon the parameters of the possible, assign much more relevance to exotic evidence, explaining the negative instance, etc., than do behaviorists. Behaviorists on the other hand focus upon what is usual at present, at least, outside the experimental situation. Given the lack of sufficient control or knowledge of all variables in the outside world, much of the apparent unexplained variance can be reasonably assigned to this difference of observational vantage point.

Behaviorists offer a functional explanation which in their case implies that the control of the environment over behavior lies in its instrumental effects upon humans. In the laboratory the utilitarian consequence is supplied. In the analogies to the outside world they are diligently persued.

Although recognizing the realm of necessity in the ontogeny of the individual, the comparative ethologist is
dubious about objects as consequences as opposed to a performance as consequence. This is the end built into the innate pattern for the animal, but performance usually implies object attainment also. However, biology severely limits the opportunism of animals. The transformational linguists also offer a tentative innateness to the source of man's capacity to transcend consequences and create his own along a different dimension. Both results are accomplished by looking not at external ends or consequences but at internal structure and organization of the phenomenon.
CHAPTER V

CONCLUSIONS AND IMPLICATIONS

The initial question raised by this dissertation concerned the consequences of paradigm-imposed restrictions on methods of analysis for the type of knowledge acquired. We argued that cumulative knowledge in any given area was dependent upon the domination of a single paradigm. The existence of multiple paradigms in the study of man results therefore in multiple bodies of facts that each contribute to the development of a different paradigm.

Because they have failed to understand the essential nature of paradigms, sociologists have long adopted the view that though assumptions concerning the nature of man, the relationship of observer to observed, the goal of knowledge, etc., may differ from one theoretical orientation to another, there exists a basic unity of method which allows each orientation to contribute to the overall knowledge. The result has been that we have come to place a blind faith in method, seeing it as a neutral technique that organizes the data and points toward valid knowledge. At the same time the theoretical diversity that exists is viewed rather calmly as a temporary condition that will be taken care of
once the facts are in. At that time some reformulation will be order, but until then we must concentrate on refining our methods.

The pervasiveness of this view is not total, but it is general enough to warrant thorough examination. This dissertation has attempted to demonstrate just how it is that two different paradigms are able to generate noncumulative knowledge. We have examined two paradigms and compared their assumptions, logic systems and criteria of valid and admissible evidence. Each paradigm in its separate way incorporates into its methods of obtaining data, substantive assumptions that at the least increase the probability of their confirming their expectations. In addition, the implied next steps, once certain solutions have been fairly well established, indicate that the direction of further research for each paradigm continues on a separate path.

According to Kuhn, if we examine the history of the physical science this is what we find to be the course of "normal" science. During normal times the research in a given area is directed by a paradigm in a very un-selfconscious way. That is the practitioners seldom have need to articulate the premises upon which they are operating. Kuhn describes this phase of the history of science as one of the solution of puzzles.1

1Kuhn, op. cit., pp. 35-42.
In many ways this description does not fit the situation in most of the social sciences because a single paradigm seldom is in a position to dominate even a single area of study without being attacked by a significant minority position that fails to endorse all or a significant aspect of the paradigm. This may be the reason that we found, with the possible exception of ethology, frequent defense of some fairly basic premises.

With differences of the nature that were summarized in the conclusion of Chapter IV it becomes somewhat dubious as to whether a decision between the two paradigms will be made on the basis of which better fits the facts. For each paradigm there are phenomenon which cannot be explained by it on the terms dictated by the other paradigm. In addition each paradigm has set forth a list of problems that would seem to be of primary interest and for the two paradigms these differ greatly, in fact they are about as nearly reversed as they could be. One aspect that precludes revision of either paradigm is that neither takes the others facts to be admissible evidence of what the paradigm's adherent say it is. For example, Chomsky states that unless the behaviorists' concept of response class includes some notion of being a class by virtue of its generation by a given grammar, it is an empty concept with regard to verbal performance. On the other hand, for the behaviorists to accept the transformationalists assertion that a person
'knows' the rules of a grammar would require that a person verbalize these rules. In effect each would transform any evidence of the other before it would be relevant to its own paradigm.

It might be tempting to conclude that paradigms stand in the way of discovery. Such a conclusion, however, is at best only partially true. In the case of transformational linguistics it was the very failures of the previous "structural" paradigm that led to the search for an alternative. Only because the adherents of structural linguistics dogmatically and thoroughly attempted to extend their approach to its limits could these limits be conclusively demonstrated and recognized. This does not necessarily mean that the limits were recognized by those who were doing the "demonstrating." Indeed many of the proponents of the behavioristic paradigm remain unconvinced, a fact which may even force the transformationalists to redouble their efforts. Thus while refusal to reject a paradigm in the face of failures impedes discovery for some, the discoveries of others depend upon someone's tenacity in attempting all solutions that are possible within the former paradigm. Then the requirement for an alternative can be ascertained by others.

The present inquiry arose directly out of certain problematic aspects of the assumed relationship between theory and method confronting present day sociology. The
last decade has witnessed a certain critical stance toward the theory-method nexus, a stance which has focused its attention primarily upon the specific image of man which currently pervades American sociology. The image of man under attack is that of man as object, of man as devoid of content, man as receptacle, man as tabula rasa, or whatever label one wishes to place upon this conception of homo sapiens.

This image of man has not only assumed a position of theoretical importance in the behavioral sciences, but has become part of the conventional wisdom of American society. In a society that values scientific modes of inquiry and evidence, the predominant behavioral science paradigms play a major role in the assessment of various means of attaining social goals and also in limiting the range of the goals themselves.

Dennis Wrong's initial discourse on the "Over-socialized Conception of Man" directly attacks this man-as-object image, the functionalist view of human nature, the mental set which sees man as oriented toward the expectations of others, as merely striving to fulfill normative prescriptions which it is in his 'nature to internalize.' Such a view of man appears to be the sociological result of

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consciously rejecting, either in part or in toto, the economic, political, self-preserving, or libidinal man proposed by sociologists, psychologists, or philosophers of a different breed. However, Wrong's analysis itself suffers from some major shortcomings. For as the Russian sociologist, Novikov has stated, functionalism, and Parsons' theory in particular, can be taken as polished restatements of those social fictions which constitute the normative support for the bureaucratic social organization of the modern capitalist state. There is no denying the dominance of this type of social organization in American society. There is, according to Novikov, a true lack of manifestation of the types of behavior which Wrong thinks sociology neglects. This may in part, at least to some sociologists' way of thinking, excuse the fact that American sociology has, by and large, ignored the aspects of man that Wrong thinks need to be explored. It may well be that the organization of present-day society effectively squelches any behavioral manifestation of these proclivities. However, to declare, as Wrong does, that such proclivities are internally present but absent in their manifestation, requires that one show the heuristic value of making such an assertion, particularly if others' claim to explain behavior without doing so.

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Wrong is well aware of the fact that there are some things which functionalism cannot explain but he is less than explicit in drawing out what these phenomena are. He does imply that a different logic is required to properly address the theoretical problem, but again he is not clear on how this logic relates to or organizes empirical reality in a fashion that would offer support for an alternative view of man.

Dahrendorf and others have criticized functionalisms' inability to explain both the coercive aspects of social organization and large scale, rapid change. However, one of the areas in which the dominant structural-functional paradigm fails is the very area in which it claims special competence. This area is the sphere of the ideational - culture. In fact, it appears that part of the difficulty that the functionalists have in explaining change is due to the way in which they conceive of ideational factors, their prime determinants of societal phenomena. Parsons has attempted to view cultural phenomena as supra-individual, and has separated the maintenance of cultural patterns from anything but the fulfillment of culturally derived needs of the individual members. Malinowski, of course, did just the opposite in positing that any cultural

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pattern that failed to meet certain biological requirements would soon disappear to be replaced by alternative cultural means. 5

Theorists like Parsons, and particularly Sorokin, have recognized an important aspect of cultural phenomena, namely, that it transcends empirical reality. Yet, to separate the cultural level completely from the cognitive and perceptual capacities of the mind and to posit the cultural level as the determinant of the other levels leads one to the Parsonian position of attributing an internal dynamic to culture—a dynamic which one then uses to explain historical change. There is some question, however, as to whether this is really Parsons' intent, because in stating that social systems tend toward increasing rationality, he also hastens to add that "we are not directly stating an empirical generalization." 6

However, Parsons likewise cautions that the theoretical significance of his nomemprical generalization should not be underestimated. 7 Simply stated, Parsons' theory merely asserts the Skinnerian notion that the individual is engaged in the process of maximizing gratification within the established system of rewards.


7Ibid., p. 502.
Parsons does not attempt to show how 'rational man's' activities feed back into a basically rational system in order to make it progressively more rational. Parsons simply assumes that culture, as a reality sui generis, rationalizes itself as part of its ongoing superorganic nature. Parsons in effect posits no interplay between culture and personality. The question then becomes, what are the specific mechanisms or dynamic through which a cultural system not linked to rational personalities still manages to become increasingly rational? We are left to conclude that's the way culture is, that is its very nature. There can be no specific empirically demonstrable dynamic for as Parsons tells us his generalization about progressive rationalization has no empirical referent.

The most cogent criticism is sociology of several aspects of both the behaviorist and structural-functional points of view has been leveled by those symbolic interactionists of a phenomenological persuasion. One of them, Charles D. Bolton, feels that the assumptions of both behaviorists and functionalists, that man is a behaver who responds to objects, and that social action is 'learned behavior' are both incorrect. The assumption that man is a respondent, in the functionalist view, is somewhat different from the behaviorists' in that the emphasis is upon

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internalization of what were initially external expectations. Though some would quibble with Skinner's emphasis in how this internalization is accomplished, the behaviorist view is certainly a dominant view in mainstream sociology.⁹

In his article, Bolton points out the fallacies involved in such assumptions and proposes an alternative view of social action as an interaction process. Bolton states that: "Objects and subjects are products of the process of identifying potentialities for interaction of events in the situation—identifications made not only from the viewpoint of the actor but from the roles or points of view of others involved in the episode of interaction."¹⁰

Since this view is taken with minor variations by a number of social psychologists, and is opposed to the view of structural-functionalists that action is a product of factors which first play on and, when internalized, play through individuals, it deserves critical examination. For one thing, the interactionist view clearly exaggerates the degree to which individuals are unconstrained. This view implies that action is a process of selection and modification by an individual from a relatively undifferentiated repertoire of segments, according to the way in which the


¹⁰Bolton, op. cit., p. 7.
actor perceives events to be developing. It indicates that there is no constancy of performance, that there are no limiting factors, that there is no structure.11 In the example Bolton uses, that of preparing a lecture, he uses the same point that Chomsky does; namely, that there is no actual repetition in the performance when one sets about to do the "same thing" again. Chomsky's conceptualization of this process, however, indicates that underlying a 'new' symbolic construction is a constant set of hypotheses which both limits the alternatives one may choose and relates them to what one has already chosen. This underlying deep structure keeps the performance from being wholly arbitrary and furthermore vitiates the effects of other organizing principles that are otherwise possible. Though Chomsky would agree that this process could not be learned by any process in which we understand the term now, it is also not a process that is built anew for every linguistic episode, only the surface appearance of the product is 'new.'

Another area of difficulty in both Blumer's and Bolton's framework lies in their criteria for the judgment of the adequacy of social scientific knowledge. On their own terms they can be quite satisfied, but what external criterion of adequacy of knowledge do they offer? Bolton

in rejecting the knowledge goal of control also rejects prediction, which is not necessarily synonymous with control. It seems that he too easily identifies prediction in the social sciences with the behaviorists' definition of predicting frequency of events. There are other senses in which one predicts. For example, on the basis of one's knowledge predicts a set of possible alternatives. Given that events outside the scope of a sociological theory (for instance, climactic factors) can negate ones frequency-of-events predictions there is good reason to rely on other types of predictions as tests of theoretical adequacy.

Such theories as Bolton's and Blumer's reach their logical extension in the works of authors like Erving Goffman. Social performance is characterized as a series of episodes without ties to underlying patterns. The cumulative consequences of action are absent; the explanation only summarizes the datum of performance. This militant anti-psychologism is curiously enough shared by the behaviorists. Each symbolic construct is imbedded in a ritual performance and obtains no reality status independent of the performance. The interactionists emphasis upon interaction is perhaps overstated in opposition to the prevailing views that overemphasize structure, but nonetheless solves few of the problems inherent in the other approaches.

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An alternative conception which deals with cultural phenomena in a way that closely parallels that of Noam Chomsky's conception of language, is that of the French anthropologist, Claude Levi-Strauss. In The Savage Mind, Levi-Strauss attacks the problem of the relationship between social relationships in the Marxian sense and superstructure, or culture. He states that contrary to Marx's theory there is no point by point correspondence between culture and the social organization (although he generally endorses the view that social relations determine culture). On the other hand, he criticises the views of Malinowski on the nature of magic, religion and science. In so doing he again implies that the directionality of cultural change posited by Parsons is inaccurate. Levi-Strauss states that, "Every civilization tends to overestimate the objective orientation of its thought and this tendency is never absent. When we make the mistake of thinking that the Savage is governed solely by organic or economic needs, we forget that he levels the same reproach at us." The assumed objectivity and rationality of Western thought are as overrated as the systematic and empirical aspects of primitive thought are underrated. Both modern and non-literate thought are seen by Levi-Strauss as manifesting the same sorts of mental

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14 Ibid., p. 3.
operations and differ primarily in the interests which the social organization imposes on the thought. Yet, he main­
tains that in neither case, savage thought or scientific thought, does the effect of this social organization go un­
mediated by the universal structure of the mind. Thus while the capacity for thought remains the same cross­
culturally, the mode of performance appears different. This view of actual and potential is of course anathems to the behaviorist. Although his first attempts to apply a trans­
formational model, incorporating a view of the human organ­
ism which includes substantive intellectual capacities that enable men to do more than make inductions from experience, may not be as successful as Chomsky's work on linguistic systems, Levi-Strauss has offered a possible alternative to prevailing views.

In the above paragraphs some of the dimension of the criticisms of the empty organism view of man, that have come from within the social sciences have been presented. The more radical critiques have arisen, by and large, from those who are marginal to these disciplines. Ludwig von Bertalanffy, a biologist who has been frequently cited in the previous chapters is one such example. Another slightly different line of criticism stems from the social sources that Bolton refers to in his essay. He writes:

The commitment to the methodological orientation in the Behavioral Science identity is probably associated with the decline of discussion about the nature of human nature. This old philosophical
problem takes on new significance for a society in the process of transition from an economy of scarcity organized around a competitive work world to an economy of abundance when life must be centered around non-work activities.\(^{15}\)

Bolton asks whether human nature condemns us to the fate that is the personal concomitant of what our present theoretical frameworks would anticipate should such a society materialize.

John Seeley asks the same question in his essay, "Progress from Poverty?". He suggests that once we no longer have the 'problem of production' we may magnify tenfold the problem of order. In present society "work itself, with its routines, formal and informal, has filled so much of the temporal space of life that a great deal of order, private and public, has been provided just by its presence and pervasiveness."\(^{16}\) The prospect that a society of affluence may require a more direct system of control generates anxiety in the minds of the man on the street and the social theorist as well.

The prospect of such a radical change in the organization of man's activities as Seeley and Bolton anticipate raises some theoretical problems for prevailing frameworks. Sociology and modern psychology have developed theories that are particularly applicable to industrial market societies

\(^{15}\)Bolton, op. cit., p. 3.

and their members and have somewhat limited applicability to other types of societies. Sociology, insofar as it has attempted to explain change has traditionally offered explanations of the rise of industrial societies in Europe and more recently has focused attention upon the process of industrialization now taking place in non-industrialized societies. With the exception of Marxian theory little attention has been focused upon the possibility of basic changes in post-industrial societies.

Both S-R learning theories and structural-functional analysis offer a similar view of the future—more of the same. Man's behavior can only change when the reinforcing contingencies in the environment change according to Skinner. For Parsons, societal change, which brings about change in the actors' behavior, will be a continuation of the process of rationalization. Skinner's normative prescriptions for modification of the present culture are quite similar to Parsons' theoretical model (although Skinner labels his as specifically normative). The suggestion that Skinner makes involve the cultivation of control techniques based upon objective knowledge to bring about desired consequences.

An objection could be raised that these logical extensions of the present as theoretical models of the future of industrial society represent an exercise in temprocentrism. Thinking about human phenomena in the past has shown a decided tendency toward conceiving of all
history as being a preparation for the present, and all future history as a mere extension from the present. Certainly past history has provided many examples of the inaccuracy of such temperocentric views. Despite our present sciences of man, it seems a bit unlikely that we are reaching the end of history.

One note should be interjected here on the role of such ideas themselves in the process of change. One need not be a cultural determinist to appreciate the fact that the prevailing behavioral science paradigms themselves can operate to mitigate against or to implement change. In a society which places a high value upon scientific ideas and at least partially basis its programs upon them, the popular versions of such theoretical paradigms play a crucial role.

If behavioral scientists remain content to base their methods of inquiry on a highly restricted conception of acceptable evidence we cannot expect much from a society based upon conclusions from its evidence. Combining Parsons' and Skinner's views man will be propelled by the continuing process of rationalization. We could predict that as the technological apparatus increases its consequences, man decreases his consequences— at least as consequences are conceived of by Skinner, as being productive manipulations upon physical and human objects. Since man summarizes these consequences in emitting further responses, he does
less and less. And since man is what he does, he becomes less and less.

The predictive scheme may be a self-fulfilling prophecy if we lack any alternative paradigm. However, if we have a view of man with a different nature, capable under specified conditions of negating and transforming experience (at the present we reserve this view of man for the psychotic and neurotic) in a patterned way, the possibilities for the future change. This is true also if we have a view of man with emotive content that is not culturally created. Chomsky's work is one example of a theoretical paradigm that incorporates such a view.

Herbert Marcuse develops the implications of alternate views of man in his works *Eros and Civilization* and *One-Dimensional Man*.17 Along with Wrong, Marcuse suggests that the empirical justification for such a view of man and society is not possible within the methodological restrictions of behaviorism and positivism. Marcuse attacks the problem with the aim in mind of providing an alternative philosophy of science, but the validity of his alternative philosophy hinges primarily upon the nature of human cognition. Marcuse argues that present philosophies of science are based upon a view of language that equates a concept with its function. Marcuse suggests that human language enables man to do far more than reflect existing

relationships among functions. While a toaster is an appliance that burns bread in an operational definition the grammar of a language permits other relationships to be entertained; relationships that go far beyond present functions of the object referred to. In discussing universal concepts, Marcuse develops a line of thought similar to Chomsky's. He states: "(The universal) not only abstracts from concrete entity, it also denotes a different entity. The mind is more and other than conscious acts and behavior. Its reality might tentatively be described as the manner or mode in which these particular acts are synthesized, integrated by the individual." If systems of symbolization are nothing but verbal behavior as Skinner says, Marcuse's view are more philosophical speculation.

The analysis of the two contrasting paradigms in this dissertation indicates the importance of methodological considerations in developing an alternative body of theory and evidence. It is not sufficient that one critically appraise assumptions and pose alternative assumptions. If one adopts the same techniques of inquiry and logic of verification, the likelihood of discovering evidence supportive of alternative premises is slight. Not only the assumptions and evidence, but the manner of constructing the evidence must

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be thoroughly examined and the possibility of different techniques explored. In view of the issue at stake, man's future, the effort appears highly worthwhile.
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