A CRITIQUE OF MOTIVATIONAL THEORIES IN SOCIAL PSYCHOLOGY

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by

KIM RODNER

AN ABSTRACT

Submitted to the College of Science and Arts of
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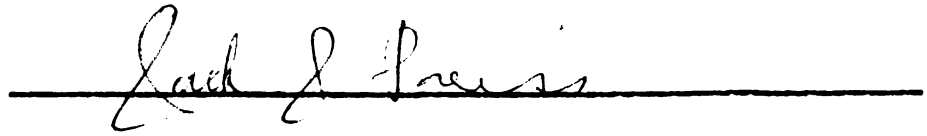
Approved: 

KIM RODNER

The problem of motivation in social psychology has had a long and involved history; numerous theories have been advanced to account for motivation yet none commands universal assent. Apparently the assessment of individual motivational theories is not itself adequate to settle the general question of motivation in social psychology. A new approach is required in which individual motivational theories are placed in classes which share common structural characteristics. So placed the justification of motivational theories may then be assessed in general. If positive assessment is forthcoming, empirical testing of motivational theories may then proceed with greater clarity and direction, but if positive assessment is not forthcoming it will be unnecessary to proceed to the testing of individual motivational theories.

Four classes of motivational theory in social psychology are examined: theories of global correlation, theories of identification, theories of legitimation, and theories of non-motivated purposive systems. The first group--theories of global correlation--comprises a wide range of theories which postulate a fundamental "push mechanism" or basic source of motivational force; the second group--theories of identification--suggest that identification of a social actor with a role constitutes the motivation of that role; the third group--theories of legitimation--stress the role of vocabularies of motive as functioning in role preformance;

and finally, the fourth group--theories of non-motivated purposive systems--stress the role of self-regulative homestatic systems and the position of teleological conceptions in accounting for motivation.

Having presented the classes of motivational theory the analysis moves on to the development of an explicated model of the logic of empirical science and the nature of teleology. Such an analysis is drawn from the literature of modern empiricism and is employed to attain agreement on the nature and meaning of the logic of science. Problems treated include such topics as the nature and manner of concept formation, the structure of scientific laws, explanation of "facts" by concept and by law, and the explanation of laws by more general laws; it also includes an analysis of the role of theory, the meaning of initial conditions, and the structure of classificatory systems, prediction, and generalization in empirical science. In a further section the problem of teleological or self-regulating systems is examined and the meaning of teleological explanation assessed.

Against this background of scientific analysis the four classes of motivational theory are then cast, and it is in terms of such analysis that the general justification of motivational arguments may then be assessed. It is found in each case that the classes of motivational theory examined are either redundant of explicated law statements or that they involve categories which appear to have no empirical

significance. Furthermore such categories suffer from vagueness so that objective assessment of them is nearly impossible. It is concluded that on the basis of the total investigation motivational theory is duplicative and repetitious of the general construction of explanations in science, and that where this might appear otherwise no empirical meaning can be assigned to the various claims that motivational theories either covertly or overtly make. If some further significance is intended, then it is incumbent upon the proponents of motivational theory to develop it.

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I should like to thank the following persons for their interest and stimulation in the genesis and preparation of this thesis. I wish first to recall the aid rendered me by my adviser Dr. Jack Preiss. His many comments and insights helped me greatly in formulating the problem more precisely and dealing with it more directly. In a sense the problem was to handle old matters from a new perspective. The fact that Dr. Preiss was willing and anxious that I approach them in this manner accounts in no small part for the existence of the present thesis. I wish to thank Professor Harold Walsh of the Department of Philosophy at Michigan State University for his interest in my concern with the general area of the philosophy of science and social science methodology. An invaluable reading course covering the work of Richard Bevan Braithwaite which I took with Professor Walsh proved fundamental to the formulation of the intermediate stages of the problem. I should also like to thank Dr. T. Shibutani of the University of California at Berkeley for what perhaps may be called the root of the entire undertaking. His suggestions concerning the present task as well as his insight into systems of social psychology which do not necessitate motivational concerns may certainly be credited with the original suggestion of the problem. Finally I should like to thank Dr. William Form and Dr. Jay Artis for their willingness to serve on my thesis committee and for their considered comments on the present work.

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Preface

Motivational theories in social psychology address themselves to the question of what it is that compels an actor to social action. To make this initial statement more precise considerable attention would have to be given to the word "compels" and in so doing the investigator would be called upon to critique motivational theories. He would attempt to assess their relative claims in accounting for social action and he would do so by first considering the justification for motivational theory in general, and secondly, once the first consideration was answered in the affirmative, by attempting to empirically test particular motivational theories.

The concern of the present undertaking is to examine the justification for motivational theories in general. For if such justification is called in question we need not proceed to the second consideration. Therefore it should not be expected that in a study such as this detailed consideration of the pros and cons of any particular motivational theory will be encountered. To be sure we shall find that in at least two classes of motivational theory--that of identification and legitimation--a more or less detailed presentation will ensue, but in these cases this is simply because these positions have a limited number of adherents so that the broad methodological classes into which they

fall, and which alone merit our attention, are classes of one or two members each. As for the rest we shall find numerous adherents both inside and outside sociology and social psychology and our treatment will necessarily take on a somewhat more generalized form.

When we consider the broad limits motivational theories embrace it is not surprising that they occupy a significant place in the literature of social theory.[1] This is especially obvious when it is realized that many sorts of theories are by some considered anything but motivational, while by others they are taken as exemplars of such theory. In order to cover as much ground as possible what we shall do in this thesis is frame a set of categories of sufficient abstraction to cover the many and various theories which concern us. We shall not maintain that the concepts covering these classes are the only set of concepts useful in motivational analysis, or that indeed they are even fruitful for every sort of concern which an author might entertain. Generalized concepts require, so far as we can see, especially one characteristic in science, and that is that they be fruitful in organizing our understanding of an area of experience. Other investigators may cut the corpus of motivational thought differently, since this will always depend on the purposes and aims of their investigation. In other words we are not suggesting that our present undertaking is the only approach to motivational considerations

in social psychology and elsewhere; what we would hope to claim is that for our purposes it is adequate. And our purpose is, we may repeat, to critically examine the justification of motivational theories when considering human social action.

The classes of motivational theory that we choose to critique appear to us to be inadequate because of (1) certain fundamental methodological* misconceptions which they embody or, (2) because they seem to involve certain unnecessary redundancies which suggest but do not fulfill the logical requirements, of a scientific explanation. Our purpose will be to point out these two inadequacies wherever they occur in the discussion of our general classes of motivational theory.

To accomplish this end we propose to critique these theories on the one hand against a background of the basic structure of scientific explanation and on the other in terms of the analytical work that has been done concerning the foundations of functionalism and teleological systems. Such a critique concerning the logical adequacy and justification of motivational theorizing should, if its results are positive, lay the foundations for empirical testing of

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*By methodology we mean the "logic of science", its abstract structure and not any empirical techniques employed in confirming or disconfirming hypotheses.

particular theories. On the other hand if the results prove negative it should, by that token, obviate the necessity of further consideration of particular motivational theories falling under our generalized classes. The latter result would have been achieved by virtue of having called into question their very necessity. In short, it would seem unlikely that the scientist consider the empirical testing of a theory which is either unnecessary or logically corrigible.

Part I of this thesis will concern itself with a broad presentation of the four major classes of motivational theory as we conceive them. These four classes we shall call the theories of "global correlation", "identification", "legitimation", and "non-motivated purposive systems". The first class--theories of "global correlation"--are not strictly social psychological, but many of them falling within this class have been employed, indeed still are employed, in social psychology and therefore some generalized category capable of handling them was necessary. The last class of theories--theories of "non-motivated purposive systems"--is primarily sociological and social-psychological but at the same time it has and is being employed to explain a broader class of events, mainly aspects of biological behavior. Nevertheless it is certainly fundamental to our considerations as it comprises the general framework of structural-functionalism in present day

sociology. The two remaining classes of theory--the theory of identification and the theory of legitimization--are today centrally contending for social-psychological consideration since in some ways they meet most fully the requirements specified by current social thought.

Part II of this thesis will develop in some detail the logical foundations of scientific explanation and the logical foundations of functional or directively-organized systems. Against this combined background we will then be able to assess the claims of the various classes of motivational theory. This will provide us with a fairly detailed and yet at the same time general framework against which to critique our theories. The importance of this is seen when it is realized that much of what we will have to say will depend upon what we mean by a scientific explanation or prediction and what we have in mind when we talk about the functional relation of a social or biological system. Since traditionally a certain amount of vagueness surrounds these notions the specification of our vocabulary and the referrents to which it applies must be delimited.

Finally, in the conclusion of this thesis we may hope to reap the results of our effort. It is in the conclusion that the actual task of deducing the consequences of Part I and Part II taken together will occur. And it is in this section that we will suggest the broader implications of this critique of motivational theories.

It may be remarked before beginning Part I that the footnotes are intended as an integral part of the total thesis. This is especially true in Part I and only to a slightly lesser extent in Part II. The footnotes are intended to act as further substantiation of the points discussed, but even more they are intended as a necessary addition to the bare outlines developed in the text. The nature of our subject is such that hundreds of pages could be devoted to illustrative digressions which would be fruitful and convincing when so developed. We have attained economy only by the means of employing the footnotes to suggest the avenues that might be explored in various classes of motivational theory. We feel that any attention devoted to them will add much weight and meaning to the necessarily brief presentation in the text.

PART I--CLASSES OF MOTIVATIONAL THEORY

Global Correlations

The first class of motivational theory to which we direct our attention is what, following Lazarsfeld, we may call the theories of "global correlation". This particular class comprises a very large and disparate group not all of which we can even hope to indicate. Furthermore this class, more than the others, contains theories not all of which have always been considered motivational. Nevertheless they *can* be so considered and as such merit our attention.

When we employ the phrase "global correlation" we have in mind those motivational theories that attempt to take some fairly limited aspect of experience and correlate it with some very general or broad area of experience, the latter generally being that which they wish to explain. In particular, when dealt with *as* motivational theories they essay an account of the motivations involved in the second or bread realm of events. This second area is most often human behavior and at times even aspects of animal behavior depending upon how far the author feels it is necessary to explain motivations in nature.

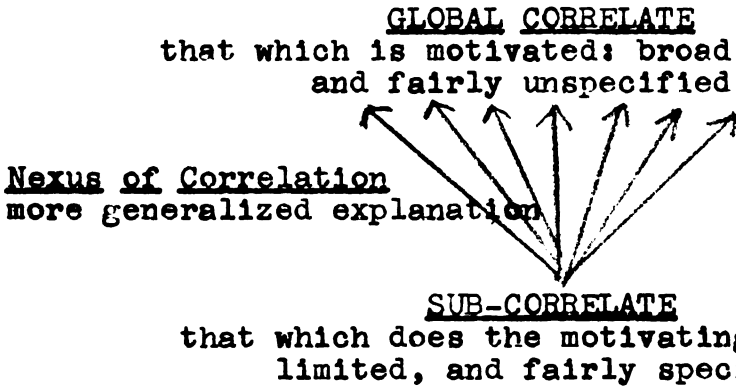
This initial statement is quite abstract but it does serve to indicate the general framework or formula for this class of theories. It is from the tie up of some limited,

fairly definite area of experience with some other unlimited, fairly indefinite area that we derive the name "global correlation". Two examples may serve to concretize these matters. Freud's book Civilization and Its Discontents, presents Freud's theory of culture.[3] It presents a picture in which the essential motive force or more simply motivation in the creation of human culture derives from the correlation of cultural productivity with a fixed source of biological or libidinal energy. There is here a global correlation between the entire, literally global, production of culture in all its manifestations and the relatively limited and fairly definite biological energy of the human system.

The second example is found in A. L. Kroeber's motivational theory of culture change. This theory is found in Kroeber's Anthropology and may be called the theory of "labile structure".[4] In this theory Kroeber correlates a fairly fixed and limited "play impluse" with the motivation of large segments of social and cultural change. A concrete manifestation of this theory is to be found in his study of dress style[5] and of the Hawaiian Islander's "cultural fatigue" as manifest in the overthrow of their religion.[6] The play impluse is correlated with the global manifestations of social-cultural change.

These two examples should suffice to indicate the general formula for a theory of motivation involving global correlation. However to make it even more succinct perhaps

it will help to summarize it with the aid of a diagram.



It will be noted that the connecting arrows in the paradigm are splayed. This suggests that in motivational theories of our first class there is a global correlate that is broad, general, and vaguely unspecified (in Freud cultural production, in Kroeber cultural change). This is connected with a sub-correlate which is narrow, limited, and clearly specified (libido, play impulse). The manner in which this is correlated is referred to as the nexus of correlation, and really amounts to a demand for an even more general correlation under which the first global correlate and its sub-correlate can be subsumed i.e., the "reason" (wider correlation) libido is correlated with culture production is that the frustration of libidinal expression forces sublimation which in turn is manifest in the form of culture productivity, or the "reason" the play impluse is correlated with culture change is that normative patterns of response fix and thus frustrate the normal release of labile energy

and correspondingly result in cumulative explosions and altered culture.

It is not necessary that there be nexus of correlation in any theory of global correlation, but in point of fact they have often been developed and in many cases are felt to be the key points by which the theory lives or dies. The reason for this seems to be that they offer an "explanation" of the first correlation and thus confer upon it this additional confirmation.[7]

Having thus characterized the general nature of this class of motivational theories it is useful to indicate the range of its development. To a considerable extent Kenneth Burke in his more recent writing on motivation summarized this matter. It is not our intention in utilizing Burke's research to subscribe to any of his conclusions or general reflections on the subject of motivation, but he does aid us in pointing out motivational theories of global correlation in areas where they are not generally acknowledged. This is useful for it helps us appreciate the mercurial quality of this class and it suggests a certain ambiguity in regard to them which will prove vital in their justification.

As Burke has commented: "A purely historical survey (of motivational theory) would require no less than a universal history of human culture; for every judgment, exhortation, or admonition, every view of natural or super-

natural reality, every intention or expectation involves assumptions about motive or cause."[8] In the Grammar of Motives and the Rhetoric of Motives Burke has provided us with a chronicle of such theories. Burke proposes that in discussing them we consider a pentad of categories into which they may fit. He comments: "In a rounded statement about motives, you must have some word that names the act (names what took place, in thought and deed), and another that names the scene (the background of the act, the situation in which it occurred); also, you must indicate what person or kind of person (agent) performed the act, what means or instruments he used (agency), and the purpose."[9]

Taking these categories--not necessarily in the order given--as the basis for discussing motivational theories of global correlation we find that "scenic" motivational theories are generally some variant of environmentalism. The work of Semple[10] and Huntington[11] are outstanding in the area of geographic determinism.[12] Arnold Toynbee's work employs this dimension largely in his discussion of the "genesis of civilization".[13] The "frontier thesis" as employed by Webb,[14] Parrington,[15] and Owen Lattimore[16] are variants of the same basic scheme. So too is the still viable field of geopolitics.[17] One may even consider in this category Sheldon's theory of somatic types wherein the physical environment of the body becomes the motivator

of diverse acts related to personality.[18] Marxism and most utopian socialism were scenic in the sense we employ: actions are explained in terms of economic situations (scenes), to produce "good" people they must be provided with "good" environments.[19] In American economic thought Veblen's notion of "the cultural incidence of the machine process" is another variant of this conception.[20] In anthropology if an author chooses to account for human social action in terms of "techniques"[21] or "man and his works"[22] or "man the maker"[23] then again human social action is accounted as motivated by such techniques, or works, or fabrications.

We might go on at great length but mention of a few of the more important applications should suggest that this type of global correlation is fairly common and certainly still in evidence today. With many motivational theories of this first type it has not always been the case that their authors considered them "theories of motivation". Nevertheless it does no violence to common usage to speak of the economic relations of production as motivating the ideology as a class. In fact, if an investigator were to avoid discussion of economic and social structure and focus instead upon social persons within economic structure we would expect that just such a mention of motives would appear: that is, if he acted as social psychologist rather than structural sociologist or

macro-economist.

"Agency" motivational theories--the second sub-type of global correlation--are the sorts of theories found in sophistated discussions of the role of language in human thought, the upshot of which is roughly that without it there is none. Biologists such as Julian Huxley[24] that look upon human culture as an agency of biological survival and anthropologists that think in terms of technological determinism hold to similar thoughtways. The point here is that they focus not upon a general background or scene whence spring--in some manner--social sets, but they rest their attention instead upon a global correlation between the means or agencies and the social acts they make possible. There may be some overlap between scenic and agency theories but the difference is pronounced in certain area. This is no where better exemplified than in the recent studies of linguistic structure and concept formation exemplified in the work of Kluckhohn,[25] Whorf,[26] and Hoijer.[27]

There are a number of global correlations that center upon the social actor ("agent") himself rather than the scene or agencies against and in terms of which he sets. Voluntarism has a long history in Germany[28] and is certainly represented in France[29] and England[30] as well. Nor are representations lacking in the social sciences. Karl Mannheim's discussion of "creative elites" in Man and Society in an Age of Reconstruction leans far out in this

direction,[31] as does Joseph Schumpeter in his propensity to stress the role of unusually creative entrepreneurs in the process of capitalist expansion.[32] Any great man theory is orientated in the same direction. A correlation is attempted between a small segment of the human resources in a population and the gamut of cultural production: men are motivated to act because of their natures ("genius will out") or because of the acts of such great men.[33]

When we consider the "act" itself as a category of global theorizing we find that it is under-represented today but was a steady source of motivational theorizing in the past. Today representations are to be found among the existentialists who consider that the "Act" to be self-creative and that little that came before or will come after can modify it.[34] Mysticism in general has produced motivational theories that correlate "pure workings" or "pure acts" with cultural creation especially in the realm of legal and value production.[35]

Our last type of global correlation involves theories that conceive of motives as flowing from the goals or purposes that persons entertain. These are generally what are called teleological theories; we learn in these theories of such conceptions as "entelechies", "intensities of satisfactions", "elan vitals", "progress", "ends in view", and "goal directed behavior". It is still a viable form of theory though in rather disguised circumstance in present

day thinking. We shall pay close attention to it latter on for it differs from our other global correlates in certain respects, forming a class by itself.[36]

So much for theories of motivation of the first class. In concluding this section the following quotation from the recently published Handbook of Social Psychology is noteworthy of the sort of orientation that has produced the numerous global correlations we have so briefly reviewed. Gardner Murphy in speaking of the problem of social motivation says: "The problem could fairly be reduced to a simple form of asking what it is within us that disposes toward social action, in contradistinction to what it is that determines the kind of action we take. Our psychological theories assume for the most part that although the organism consists largely of devices for adjusting to (the) environment...there is always room for the question: what makes the thing work? Attention will first be given to the biological sources of motivation; thereafter we shall attempt to show the ways in which these energies are elaborated and developed into social motives."[37]

Identification

The second class of motivational theories that require our attention have only a limited number of adherents; all of them are within the social sciences and especially social psychology. It is tempting to refer to this second

class as the extreme sociological position for it does have the characteristic feature of avoiding what has often been referred to, especially in discussions of motivational theory, as "reductionism in the social sciences."[38] Reductionism is not our concern here but it may be suggested that the approbrious application of this term has most often been applied to theories of global correlation, theories that might well take the form indicated in the quotation from Gardner Murphy in the paragraph above. Our second class of theories, the theories of identification avoid reductionism as that term is generally employed.

A major statement of the theory of identification is to be found in an article by Nelson N. Foote entitled "Identification as the Basis for a Theory of Motivation".[39] A somewhat similar statement by a co-author is to be found in Leonard S. Cottrell's and Nelson Foote's Identity and Interpersonal Competence.[40] What is of interest in this latter work is not the broader theory concerning the role of the family in society but the theory of identification that forms part of the conceptual apparatus. In addition to these efforts there is the important section on motivation to be found in the introductory text entitled Social Psychology co-authored by Alfred R. Lindesmith and Anselm L. Strauss.[41] Professor Strauss also has a volume on identification that has not yet been published.[42]

The theory of identification, following Foote, may be briefly summarized as follows. Role theory has suffered from its inception from the lack of an account of motivation. Apparently the phrase "from its inception" refers to the early theorists such as Cooley, Dewey, and Mead. "Roles as such do not provide their own motives."[43] Foote points out that attempts to overcome this difficulty have resulted in recourse to the expedient of eclecticism. Electic theories Foote argues "all share the postulation of motives as predispositions, purportedly inferred from behavior"[44] (global correlations are the sorts of theories he refers to). But this has difficulties as far as Foote is concerned in that it suggests an infinite regress on the one hand and fallacious circularity on the other. With these comments Foote dismisses nearly all other forms of motivational theory.

However there have been intimations of at least two other sociologically significant attempts to account for motivation. In the 1956 edition of Social Psychology Lindesmith and Strauss, comments Foote, included a section on "Morale and Social Structure".[45] The conception there developed is tantamount to the essentials of Foote's theory of identification. Lindesmith and Strauss conceive of motivation as essentially equatable with morale, which is to say, an individual performs social functions "and he is willing to do so because he identifies his own essential interest with that of the party"[46] or social group. And

should this fail to happen, that is: "When a social structure fails to elicit the minimal allegiance necessary for its proper functioning, then we speak of poor morale. Presumably there are different kinds of morale, depending on the kind of group structure, but essentially, it comes down to a lack of effective coordination because of discrepant individual aims."[47]

Foote's analysis follows similar lines but more fully develops the position. By a motivated act he means any problematic consciously chosen course of action. "We take motivation to refer to the degree to which a human being, as a participant in the ongoing social process in which he necessarily finds himself, defines a problematic situation as calling for performance of a particular act, with more or less anticipated consumations and consequences, and thereby his organism releases the energy appropriate to performing it."[48] As contrasted with situations involving physiological processes and long habituation, Foote asks that we contemplate the "game". He notes that such a game as, say, baseball is composed of roles and statuses. Defined behavior accompanies each position and the task of the player is to learn the behavior overtly related to his roles and to learn covertly the role behavior of the others. Foote sees in this conception of the game the exemplication of anomie. This is the stage in which Mead left the analysis and supposedly according to Foote role performance would be

apathetic and generally lack-luster. He continues: "Now by contrast, consider a ball game like last year's World Series, when the Dodgers came up against the Yankees. The roles and statuses are the same, as are the rules of the game."[49] But in fact what a difference. "And what is the nature of the difference? It is the fact that the empty bottle of role and status suddenly has a content. That content is not drives, tensions, energy or needs (not, in other words, a theory of global correlation); it is identity ...Except for the special identify which gives value to their ensuing activity the behavior of the players would be mere rote--a perfect example of anomie".[50]

Identity, in Foote's theory, is concerned with this extra something, "that which makes the thing work". It is the process, so far as we can make out, whereby the learned behavior which is the role is taken in an affirmative manner. One might say that it is a sort of "role expertise", induced under the impetus of "ultimate social values". It is a rather complicated phenomena as we see when Foote suggests that: "To interact intelligently with another, he (a person) must learn correctly to anticipate the responses of that other--that is, to empathize. But implicit role-taking is no metaphysical transmigration of consciousness. It requires playing sub-overtly the role appropriate to the identity of the other in the situation, as accurately as one can read off that identity. In role-playing experiments,

a person may disclose the great range of this latent repertoire. The reason he limits his real or realistic behavior to a select few of all the roles he has learned is that he knows and defined only these certain ones as his <u>own</u>. And he can only ascertain which role is his in each situation by knowing who he is. Moreover, he must know who he is with considerable conviction and clarity, if his behavior is to exhibit definiteness and force, which is to say degree of motivation."[51] This last phrase "definiteness and force" is here seen to be what is <u>meant</u> by the identification theory of motivation. A theory of motivation, in other words, has the job as Foote sees it of accounting for this "definiteness and force" and not for the particular content of the roles and statuses themselves.

Continuing, Foote comments that, "We mean by <u>identification</u> appropriation of and commitment to a particular identity or series of identities."[52] So it is by a process of pigeon-holing, of classifing, that social interaction, at least at this level of abstraction goes on. "The common man is always classifying thus. And to make things harder for the social psychologists, his classifications vary with time and place, as identities are elaborated and re-determined ...If the regularities in human behavior are organized responses to situations which have been classified more or less in common by the actors in them, then names motivate behavior. It is by analysis of the function of language,

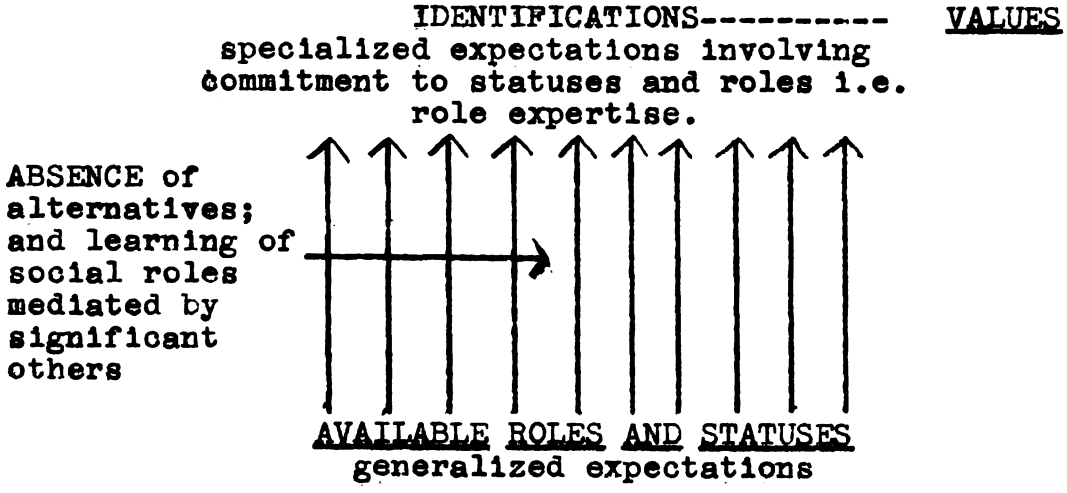
and especially of names as ascribed to categories of people, that we can dispense with predispositions and yet maintain a theory of motivation subject to empirical test...."[53]

For Foote identities are the key to motivation, yet most identities are "habitual and taken for granted".[54] However in problematic situations doubt of identity may occur; if that happens "action is paralized". Such considerations lead Foote to the question of how it is that "one acquires and gets commited to particular identities". His answer is simple, "...the compulsive effect of identification upon behavior must arise from absence of alternatives, from unquestioned acceptance of the identities cast upon one by circumstances beyond his control (or thought to be). From the point of view of the experiencing individual, however, the process is bound to seem much less like a process of limitation to a few among infinite possibilities than a process of discovery."[55]

It follows from this that identity is <u>learned</u>[56] and being learned it tends to be conservative, for it acts as the filter through which further experience is interpreted, "...we can only mobilize for our next act when it or its elements can be construed as similar to acts which have gone before."[57] It follows then that identity according to this account is the source of "value" in the face of Albert Camus' "benign indifference of the universe". Value must be looked upon as something which is "discovered in

experience not conferred upon it from without".[58] For, "without the binding tread of identity, one could not evaluate the succession of situations. Literally, one could say there would be no value in living, since value only exists or occurs relative to particular identities--at least as experienced by organisms which do not live in the mere present, as animals presumably do, devoid of self and unaware of impending death. Moreover, it is only through identification as the sharing of identity that individual motives become social values and social values, individual motives."[59]

It should be possible to summarize the theory of identification more precisely if, at this point, we present a diagram such as we employed in discussing theories of global correlation.



There are a number of features to note about this relatively simple schematization. First of all the connecting

arrows in this diagram, in contrast to the fan pattern in the diagram of global correlation, suggests that there are different identities appropriate to different roles and statuses. There is a match between the elements of one area and the elements of the other. This is rather unlike the situation in global correlations in which from some limited source sprang entire social and cultural worlds. Secondly, the manner in which this matching developes is abetted by the lack of alternative social roles and is structured by the significant groups which socialize the new recruit. At the bottom of the schematization this fact is suggested again by the use of the word "available", and it is also suggested in the middle section that the initial situation in acquiring social roles and statuses is by the process of learning them. They are not built into us nor are they resultants of "basic energy" etc. At the bottom again the use of the phrase "generalized expectations" suggests that roles and statuses are merely the first approximation to the acquisition of role behavior, and this is confirmed in the comment at the top where we learn that identification involves "commitment" via the acquisition of <u>specialized expectations</u>, a kind of "role expertise". This too is learned, being aided by the limited number of complete absence of alternatives open to the social person in life situations. In fact from the point of view of organization theory this "expertise"

appears to be the empirically meaningful kernel about which discussion of "informal" as opposed to "formal" structure revolves. It is only that we have approached it from the point of view of role theory while structuralists approach it from the perspective of organization.

The theory of identification seen in this summary paradigm avoids the derivations of "culture" or "society" from any single source. Identification is a characteristic learned response of roles but it is not itself generative of them. Another feature of this theory that emerges is that problems of identification occur especially in socially uncertain situations. Traditional behavior is traditional precisely because such identification problems are not especially important. And this is more apt to be the case in opaque social situations, situations in which the actors cannot readily distinguish the various expectancies involved. As such the more complex societies would seem the most logical candidates to examine for problems of identity. Yet the fact that this is not always the case suggests that there may be other mechanisms at work which modify this situation. Such considerations form a natural transition to the examination of our third class of motivational theories.

Legitimation

Our third class of motivational theories have a more

extensive representation of authors than did the theories of identification. Among the especially prominent are again Lindesmith and Strauss, and also Hans Gerth, C. Wright Mills, Howard S. Becker, and James Carper. In addition we may again note that Kenneth Burke developed, in an early work (Permanence and Change) a conception very similar to the theories we shall now delineate. In fact Burke's position was directly involved in the conception developed by C. Wright Mills as early as 1940. It may prove useful to review Burke's position at this point. Burke writes: "A man informs us that he 'glanced back in suspicion'. Thus, suspicion was his motivation. But suspicion is a word for designating a complex set of sign meanings, or stimuli not wholly in consonance with one another. The concoction is somewhat as follows: danger-signs ('there is something ominous about that fellow'); social signs ('I don't want to make a fool of myself if there is nothing wrong, but I could just glance back along the pavement as though I had dropped something'), etc. By his word 'suspicion' he was referring to the situation itself--and he would invariably pronounce himself motivated by suspicion whenever a similar pattern of stimuli recurred. Incidently, since we characterize a situation with reference to our general scheme of meanings, it is clear how motives, as shorthand words for situations, are assigned without reference to our orientation in general."[60]

This conception of motives tends to see them as merely the verbal expression standing for complex patterns of interaction; they are symbols of symbols. If a person gives suspicion as a motive then he is really summarizing a process of symbolic interaction which he is about to undertake. This position of Burke's is actually identical to that of the other authors in this section, only its implications remain to be developed.

It is noteworthy that motivations conceived of in the foregoing manner are situated; they apply to actual social occurances and will be as numerous and varied as those situations. Furthermore, such motives would of course be relative and a verbal manifestation meaningful in one situation may not be meaningful in another. Thus "suspicion" may be a legitimate summary of a situation probable in a complex urban environment; it may be quite the other extreme in a small face to face village of late 19th Century Wisconsin.[61] Which is to say that there probably is no patterns of expectation relative to which the summary word "suspicion" may attach when men walk down the streets of such a village.

Gerth and Mills have developed a theory of motivation that is essentially the foregoing. However they have proceeded somewhat further in ascertaining the social usage of such motives, or summary words. They suggest a dichotomy between "opaque" and "transparent" social structures and

their various constituent situations on the one side and the corresponding vocabularies of motive on the other. In a simple society "a single vocabulary of motives may be used by a person for all his roles, or at least he will use the same motives in speaking of the same conduct pattern to his wife and to his neighbor, to his working mates and to the village head. His children will learn these same homogeneous vocabularies of motive. And these vocabularies of motive are not likely to be questioned, for they are used in public, in private, and when alone, and their chances of being integrated firmly and smoothly with the psychic structure of the character will be high".[62] Things are somewhat different in the case of the industrial metropolis. "In an industrial metropolis, the person is confronted with a variety of roles and situations. Not only is there a typical split between his more intimate roles but the differences between any two intimate roles or between any two public roles may be very wide. Different motives may be employed for roles involving one's wife and for those involving one's acquaintances on the commuter train".[63] Therefore a "...person will internalize many vocabularies of motive which may very well be in conflict. Then the individual must keep one set of motives secret from the other, for they may appear 'silly' to some, even though 'beautiful' to others. He compartmentalizes not only his conduct but also his reasons for it, and insofar as he cannot do this, his motives may be in conflict."[64]

But why would such a dichotomy exist between industrial metropolis and isolated village. Gerth and Mills answer that the situations of the latter village are "transparent" that is, they tend to be such that every member of the society understands what is going on and is able to at least implicitly respond to it. Such persons understand the functions and "meaning" that interactions performed by every member of the society have and therefore it is only to be expected that the vocabularies of motives (such as "suspicion") are commonly shared and commonly employed insofar as such situations are actual.

In contrast is the complicated social interaction of the "industrial metropolis". Such interactions are for Gerth and Mills apt to be "opaque", in that it is difficult or often impossible to grasp even a small number of roles adequately relative to the range available. The situations and accompaning roles are so numerous and diverse that it is often impossible to ascertain the functions and social meanings that are attached to various types of expected behavior. In such interactions, such diverse social worlds, there may arise the need to legitimize situations that are not commonly comprehended. This will be all the more the case in situations that are actually in conflict, and they are more apt to be in societies undergoing rapid cultural and social change.

The vocabularies that result in such situations will

tend to reflect the interests of groups engaged in the social situation in question, and they will reflect it in such a way as to legitimize such behavior. This is why to one set of persons in one social world motives may appear "silly" while to another set in a different world they may appear "beautiful".* There arises the fact, say Gerth and Mills, that in such situations different vocabularies of motives will be employed in different situations, as well as the fact that the same social acts may have a different vocabulary of legitimation depending upon the social perspective in terms of which it is orientated. Motives in such cases are not "underlying forces" or "predispositions" or "drives" found in theories of global correlation, nor are they the "role expertise" that may come with the learning of specialized expectations, rather they are social legitimations of a verbal form in "opaque" interactions.

But are we to assume that "transparent" societies have no such legitimating "motives"? As spelled out by Gerth and Mills this is largely correct, for what they are saying is that in such societies a large segment of behavior is mere rote, people simply do not think to question or even speculate about it. And where in transparent societies

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*See: T. Shibutani: "Reference Groups as Perspectives", Amer. Jour. of Sociology, vol. 61, #3, Nov. 1955.

such vocabularies of motive do exist then they are consistent throughout and shared by all members as a common source of normative reinforcement. Motives in such interactions are reinforcing to the in-group rather than legitimating to the out-group. This is of course a relative matter and no society may be expected to exhibit either extreme without qualification. However it is not our purpose in this section to discuss concept formation.

Gerth and Mills sum up their position in the following manner. "When a person confesses or imputes motives, he is not usually trying to describe his social conduct, nor is he merely stating reasons for it. More usually he is trying to influence others, to find new reasons which will mediate the enactment of his role--and in so trying to influence others, he may often influence himself. The verbalization of motives for an act is itself a new act; it is a phase of role playing which lines up the role with or against the expectations of others.[65]

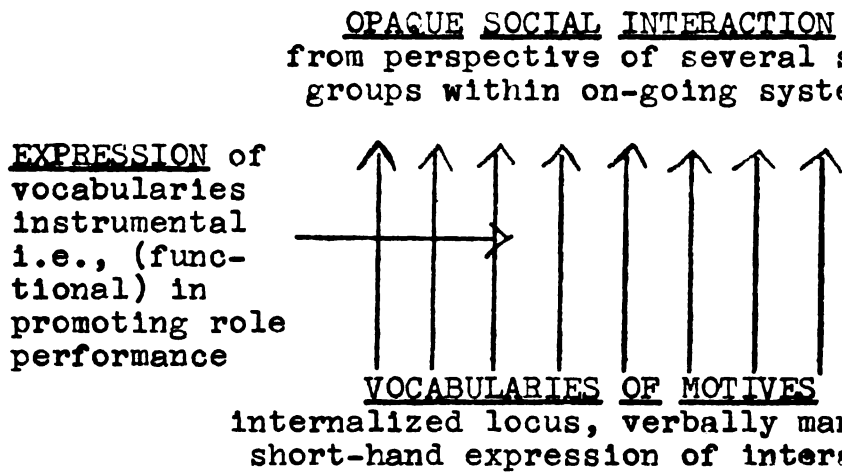
Lindesmith and Strauss hold a position very similar to that of Gerth and Mills as one aspect of their general theory of motivation. In this respect they hold that motives and the imputations of motivation are essentially functional in gaining legitimation of social acts.[66] Howard S. Becker and James Carper independently maintain a similar conception of motivation. However Lindesmith, Strauss, Becker, and Carper do present rather incisive differences from the

over-all position of Gerth and Mills especially when we ask the question of how it is we explain identification, a point they have in common regarding the theory of identification. For the moment however let us take a careful look at the Becker and Carper position on the legitimating character of motives.

In their article entitled "The Development of Identification with an Occupation"[67] the major lineaments are set forth. Becker and Carper list several factors or mechanisms that are involved in developing identification with an occupation. The occupation in this case is that of a university professor in three different academic departments; the mechanisms involved are "peer and informal groups" as instrumental in the acquisition of "occupational ideology"; the "apprentiship situation" and the concommitant development of "interest and skill"; and the "formal academic structure".[68] These are factors that are instrumental in forming identifications. They have, so to speak, a dual aspect. On the one hand there are the "visable" social positions and definitions supporting them; in addition there are the internalized vocabularies of motive that go with an occupational identity. It is this vocabulary of motives that serves in the process of acquiring identities as a legitimating factor. Identification with the role of professor evolves via the objective mechanisms of apprentiship, peer groups, and formal academic structures.

It evolves via the verbal or internalized avenue manifest in vocabularies of motive appropriate to the evolving situations. In short as in the Gerth and Mills position such vocabularies are functional in that they tend to legitimate the newly acquired behavior among competing available roles.

Again it may be useful to attempt to diagram this third class of motivational theory:



As in the diagram on identification we note that the arrows connecting the personal manifestations with the social structure are not splayed out as they were in the global correlates. This indicates that there is a one to one correspondence between acts on the one hand and the vocabularies which stand as shorthand expressions of them. This would be the case in the theory of legitimation almost by definition since such vocabularies are summary statements.

In summary the theory states that in situations of

competition, conflict, or poor communication where the functions of particular roles do not command society-wide consensus, summary vocabularies (apparently by mere iteration) may serve to legitimate what may otherwise be called in question. They serve to defend, enhance, abet situations by providing legitimating schemes for evaluating social acts. The schemes are words standing for the situation in question (as "suspicion") and insofar as they promote interaction they "motivate". Nevertheless they are only another way, so to speak, of performing the interaction in question. The several authors taking this stand wish to distinguish clearly between such functions vocabularies of motive may serve and the older, more traditional notion of motives as causes of social acts. On the other hand these writers do equate the notion of motive with that of purpose. Motives as they conceive them may be thought of as purposes. This interesting conjoining may be summed up by the following quotation from Lindesmith and Strauss: "Purposes or motives, as we have said are formulated. This means that they arise in communication and are either partially or fully stated in words. When thought of in this manner, motives do not exist in a mysterious nonverbal realm such as "the unconscious". As Mills says, 'Motives are of no value apart from delimited societal situations for which they are appropriate vocabularies. They must be situated ....Motives vary in content and character with historical

epochs and societal structures'".[69] And from Gerth and Mills we have the following: "Avowals and imputations of motives seem to arise in interpersonal situations in which 'purposes' are vocalized and carried out with close reference to the speech and actions of others."[70]

In the foregoing passages the equation of motive with purpose suggests that the purpose of an act, that is, its motive, is the function that that act serves in an on-going social system. It should now be clear why Gerth, Mills, Becker, and Carper are well disposed to this particular position. They are interested in analyzing the interrelations of social phenomena and they are thus interested in the <u>function</u> that vocabularies of motive serve in such systems. Thus the equations of motive with purpose seems a logical one given this position. In Lindesmith and Strauss we find the same interesting equation, apparently they think of the two terms as simply interchangeable. Such considerations suggest the final class of motivational theories with which we will concern ourselves.

<u>Non-motivated Purposive System</u>

The title of this final section on motivational theories may seem to be a contradiction in terms. Actually it comprises an interesting and diverse group of theories and forms the logical transition to the second part of this thesis.

The present section however concerns itself much less with motivational theories than it does with certain singular ways of asking questions. Properly considered it covers a great literature in modern sociology and it will be impossible therefore to give anything more than an outline presentation. As we suggested in the last section dealing with theories of legitimation several theorists equate motives with purposes: the motives that compel social action are the purposes that an actor entertains. Yet to talk of the purpose of an actor is to employ a teleological vocabulary, one that involves some attitude regarding future states of affairs toward which an actor or system is orientated. Systems that involve "future states of affairs" as significant elements or variables are teleological systems.[71]

Our final group of theories are those that involve such concerns in explaining the motivation of social acts. For our purposes teleological theories refer to <u>any</u> such use of teleological categories, whether it be that of equating motives with purposes as Gerth, Mills, Lindesmith, Strauss, etc. have done or even the more subtle usage employed in the "functionalist" vocabulary. With regard to the first group it seems straight forward enough to assume that if a person asks for a motive such a motive is a purpose, the purpose that the person in question has in performing an act. That is, the act is performed with some

end in view. Now to speak this way may seem natural enough for human beings, though it must be remembered it does not make sense to ask for the motive or purpose a bridge has in spanning a river or the purpose a river has in wearing down a mountain. We often refer to the "false" imputation of purpose in the natural world as a fallacy, "the pathetic fallacy".

Yet it seems to be the case that this fallacy does not apply to human imputations of purpose. Between running water and steel spans on the one hand, and human beings and perhaps the higher mammals (many would rule them out) on the other, the dividing line between purposive behavior and "mere events" occurs. So much for considerations of meaning; what really interests us in the present context is that this final class of motivational theories is teleological. Because of this it in some ways crosscuts all of the categories of motivation we have discussed, while in others, such as the present class, it forms the substance of such theorizing. In the first group it forms a perspective or accepted framework in terms of which the theorist orientates his questions, in the latter it <u>is</u> the theory. These statements will become more obvious when we analyze in Part II the nature of such systems.

For the moment we must note that theories concerned with teleological systems refer to them variously as "purposive systems", "adaptive systems", "homeostatic or

self-regulating systems", "boundary maintaining systems", or "directively organized systems". All these phrases refer to the same set of phenomena. Modern exponents form a guard of honor in contempory social science: Talcott Parsons,[72] Robert K. Merton,[73] Clyde Kluckhohn,[74] E. A. Shils,[75] Philip Selznick,[77] Abba Lerner,[78] Werner S. Landecker,[79] and in other fields Walter Cannon,[80] Norbert Wiener[81] to mention only a few. The list could be greatly extended to cover many fields ranging from biology to switching theory, from psychiatry to control systems and communication theory. This fact should serve to stress the point that such a group of theorists partake more of an engulfing perspective than an actual theory as such. We employ the phrase "engulfing perspective" mainly because it is engulfing and because we wish to indicate something more general than any particular theory, a difference which will lead us to question whether it is a legitimate perspective at all.

If we explore this conception further we find that, for instance, to ask what a man's motive is in going to work we may be told that it serves a purpose, to attain money, or achieve status, or occupy time. The motive in this case is the purpose, and that is only another way of saying that such motives or purposes serve <u>functions</u>, either in maintaining a social system, or in maintaining an individuals personality system. This conception has nothing to do with the functional legitimation of acts such as

Gerth and Mills discussed but instead refers to the role that an act or series of acts has in maintaining an on-going system. In other words to speak of "purposive systems", "motivated systems", "adaptive systems", "teleological systems", or "boundry maintaining systems" is really to refer to functional systems. In any discussion of teleological entities the terms "purpose", "motive", and "function" are synonyms.

Such a conjunction of terminology may seem difficult to absorb at first encounter, but a moments reflection will confirm the fact that the functionalist vocabulary is quite teleological, referring as it does to the role played by certain select elements in the maintenance of on-going wholes and in the attainment of selected ends. Calling attention to such a conjunction of meanings (purpose-motive-function) is important in that it allows us to discuss the nature of systems to which such terms apply in a general context and apply findings of recent work in the analysis of such entities to any argument of this form.[82]

Even so it may seem idiosyncratic to think of the authors we have listed in this section as motivationalists. But if we are correctly understood it makes as much sense to call them motivationalists as functionalists and vice versa. As for the other meanings of motivation: as global correlation, as identification, as legitimation we will find that the very problems these theorists pose when they

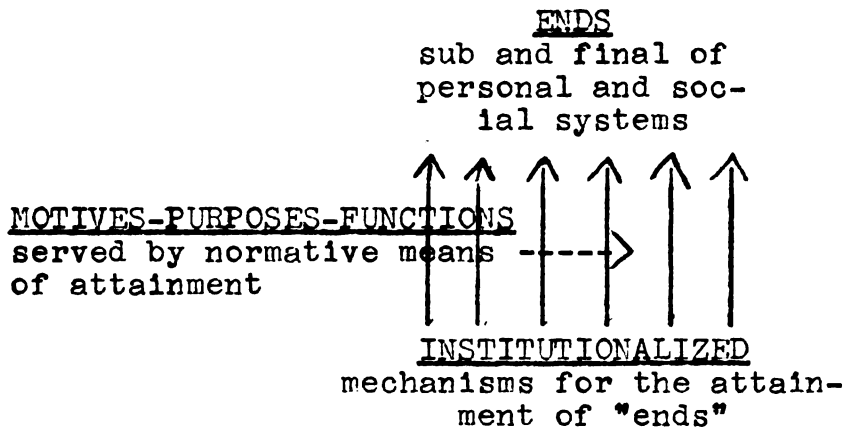
seek a "theory" of motives is itself teleological. Teleological considerations therefore form a framework that is in many respects an ernest of motivationalists generally, and because of this it warrants our careful attention.

Our present class of motivational theories is then a venerable one being largely co-extensive with structural-functionalism in sociology. To ask for the motive of a social act is to ask for the purpose, which is only another, though cryptic way of asking for the function that that act serves.

Yet even if "functionalism" were a non-existent perspective--for it is a perspective and not a theory as we shall see--even if this perspective did not exist we should still have to construe the fourth class of motivational theories along these lines. This is necessary because somewhere along the line we are going to have to acknowledge all those instances in which motives stand as purposes or causes or generators of social action. And furthermore we are going to have to acknowledge the fact that such causes or generators serve functions in social and personal systems. Such usage is fairly common even if not graced with theoretical status and it must be taken into account. We are face to face with such matters if we recall Gardner Murphy's comment "there is always room for the question: what makes the thing work?". The fact that a theorist may analyze social and personal systems into numerous inter-

connected elements exhibiting lawful regularity and then still ask the question "what makes it go" suggests that teleological thinking is not dead.

In ending this section let us again attempt a simple schematization. It will not be as complex as the former paradigms since this class of theories is nothing more than a way of asking questions:



Again the arrows indicate that there is a one-to-one correspondence between the means, the functions, and the ends of "functionally conceived systems". How one accounts for so-called dysfunctions and the extent of a particular function is another problem. As we shall see, these are really pseudo problems but for the moment seemingly real. According to one reasonable construal, for every means employed there is a corresponding function which it serves in the attainment of social ends; and like "situated motives" the diagram indicates that this function is unique in each case. The point being that we thereby avoid the negative

connotations of "reductionism" by avoiding any universal predispositions or push mechanisms.

Such a conception is one of great generality. It suggests that since motives and functions are equatible when we speak of any functional system we have already said everything there is to say about motivated systems. However if this is the case then when we employ such a scheme our job is done, there can be no further problem of "what makes the thing work". On the other hand if this is denied and some further "cause" is felt necessary, the position can only be maintained <u>if</u> some independent sense can be made of teleological conceptions which is not already given in the explication of functionalism and teleology which we are about to develop. If teleology and thereby functionalism turn out to be merely an alternative rendering of the "logic of explanation" then there is no question of motivation. For motivation in the present context to prove viable there must be some independent sense made of it which is not capable of analysis in terms of the general structure of empirical science. We will attempt to show that there is no such additional meaning, that such systems are not peculiar, and that therefore teleology (and functionalism) are without independent status in motivational theorizing and in science generally. Our point in this thesis is going to be that concerns of this sort are unnecessary concerns, that they resolve themselves into one

of three categories: statements of lawful regularity, statements of purpose and function which in turn are replacable by law statements, and finally redundancy statements.

It is in this sense then that the title of our present section, "Non-motivated Purposive System" must be evaluated. It is not a contradiction because the "purposive system" in this case refers to systems that "appear" to possess characteristics or properties that distinguish them from systems that spanned straits, such as bridges, and systems that wear down mountains, such as streams. Actually, as we shall show there is no such distinction to be made. By the same token the phrase "non-motivated" refers to the fact that once such an analysis is provided considerations of motivation in any sense become dubious.

This is not a new conception; Lundberg has intoned for such a handling of motivation for years. One is reminded of Nelson Foote's uneasy treatment of Lundberg's comment that "there is no more justification scientifically to seek the motives of a man who runs down the street than there is of paper which blows down the street...."[84] Lundberg is not fooled by the motivational argument in any guise but on the other hand he has not fully provided for its dismissal. However, this is hardly his fault since certain aspects of the necessary research have only recently been published. His exhortations that sociology become a

"natural science" are actually a salubrious corrective in an area often cloaked by confusion.

METHODOLOGY AND TELEOLOGY

The Logic of Explanation

It is commonly held that there are many kinds of explanation. The process of knowing is supposed to consist of several types of experience: scientific, intuitive, mystic, aesthetic, descriptive, and so forth.[1] It is not our purpose to argue these matters in this thesis. When we speak of knowledge, scientific knowledge, we will mean knowledge that has the logical structure of the sort we are about to develop. This type of knowledge is only an explicit rendering of what scientists implicitly assume. The other sorts of "knowing" have been severely challenged and, as far as we can see, are without foundation, but that will not be our concern here.[2]

Before we begin it is worth noting that for our purposes it will not be necessary to investigate the area of theory construction as such in this exposition (the word theory is here used in the technical sense as a specialized construct in the explanatory scheme of empirical science). The relation between theory and law statements has recently been clarified by some of the advanced students of modern analysis, and the work of R. B. Braithwaite stands out as perhaps the most concise statement dealing with this vexing problem.[3] Nevertheless law statements are, for our purposes, sufficient to explicate the nature of motivation and tele-

ological systems. Therefore the reader is urged to discount the casual use of the word "theory" as it has appeared in this thesis. There is a common usage of the word "theory" which seems to refer to any very general sort of explanation whether of conceptual, lawful, or truly theoretical form. When we speak of motivational "theory" we mean that term to be taken in this manner.

We may begin our analysis of the logic of science by noting that when we speak of concepts (as apposed to precepts) we have in mind not particular definite events but "kinds". We take the world to be composed of an indefinitely large number of particulars each one in some sense possessed of properties or characteristics not shared by any other particular: we refer to each particular as a unique event.[4] Supposedly if we but had the patience we could attain closer and closer approximation to a complete description of such particulars by listing all of the characteristics it possessed. While this may, for certain purposes, be what we wish to do it is seldom what we do in science. The reason for this is that one of our aims in science is to achieve general knowledge regarding broad classes or kinds of events. To be sure science occasionally concerns itself with particulars as in the case of geology. Yet even while the geologist is concerned with a unique and supposedly particular event (the earth) he assumes that the regularities he establishes relative to it would hold for any "similar

planet". Now this amounts to saying that he is interested in "kinds" not particulars. Let us briefly see what this entails.

The scientist is concerned with general knowledge and he attains such generality by the use of abstraction. The distinction between abstraction and generalization is roughly that generality is a predicate that applies to statements; it involves some use of quantification (existentially quantified: "some", "a few", "most", "many", etc.; universally quantified: "all", "every" etc.). Abstractiom on the other hand is roughly a predicate that applies to entities and it refers to characteristics those entities possess.[5] In order to generalize over large areas it is necessary to abstract. This entails the selection of certain characteristics, qua characteristics, and subsequent attempts to establish universally general relations between them (in the case of statistical regularities or conjunctions: existentially general).

Now concepts may be looked upon as statements formed by constant conjunctions[6] of such abstract characteristics. Thus the concept "gold" constantly conjoins several abstract characteristics in a universally quantified manner, that is, in a general statement of the form: all gold is yellow, has a certain measure of ductility, a metric of weight per cubic volume and so forth. The same may be said for the concept "folk". The abstract properties or characteristics

that are constantly conjoined in this concept include "isolation", "limited population", "strong primary relations", "non-literate", "homogeneously shared culture" and so forth. These constant conjunctions consist then of abstract properties held universally. Whatever the nature of the "actual world" really is ("what really is real") may at least be ignored by science and still obtain results. Whether in fact it is even meaningful to speak of concrete particulars or complete descriptions of "things as they actually are" is open to question. At any rate so far as we are concerned it need not detain us.

There is however an aspect of scientific activity that may seem to be crucially concerned with just such particulars qua particulars; this is the reputed interest scientists have in facts. Now the opposite of abstract is generally considered to be "concrete" and the alternative to general, "singular". Facts are, for the purposes of science, singular statements which may be either concrete or abstract. Facts refer to true states of affairs in the world. They may be entirely specific as when we seek the explanation of an individual occurence or event; they may make reference to a single <u>class</u> (still singular) of entities as when we ask for the explanation of a class of laws (Kepler's three laws of planetary motion). They are not the substance of science as is often assumed but they do form the verification basis upon which it rests.[7]

If we turn to scientific laws we find that they too are "constant conjunctions" or "invariant relations" as between entities. But in this case the entities which they conjoin are the concepts of which we have just spoken. Laws are statements that must be true, while lawlike statements may or may not be true even though they have the logical structure of conceptual conjunctions. Also we must note that a further requirement of laws is that they be, like concepts, universally quantified. This restriction must be revised in the light of statistical laws but for our purposes we will consider it accurate enough. Thus if law statements in science are universal conjunctions of kinds of events what does it mean to say we explain something by means of laws in empirical science.

First of all let us be very clear of what we mean in talking about explanations. When we are asked to explain something, generally we are asked "why", that is, for a "reason" or "cause" or "determinant" of the event in question. Often this usage is opposed tc "merely descriptive statements" or "statements of fact". It is not always easy to know just what someone has in mind when they make this distinction or employ any one of the above terms. Very often the use of "description" as opposed to "explanation" refers to a psychological difference; it suggests the "feeling" of comprehension that an individual may obtain in being given an explanation. Feelings of psychological

ease or understanding or unease and lack of understanding are perhaps important but of no concern to us. All that we are interested in is the logical structure of explanation whether or not in any particular case the explanation is accompanied by feelings of comprehension. When a person asks a "why" question as opposed to a request for description we take this to mean a question that distinguishes between concepts and laws. This point requires further comment.

When we classify we are conceptualizing; so if a person asked "why" we called 12th Century Japanese social structure "feudal" we should take this "why" question as a request for the abstract characteristics which comprise the concept "feudal". These would then be compared to the Japanese instance and the social structure labeled (classified) feudal if it possessed these characteristics. The same would hold for the classifications of fish, birds, plants, atoms, compounds, rocks, communities, and so forth. Generally we refer to such a process as descriptive or definitive and while it may at times satisfy a "why" question very often something more is indicated. In a moment we shall see what this something more is, but for the present let us investigate the conceptual or classificatory explanation a little further.

We said that we have explained an event (in a perfectly good sense of the term) when we have shown that event to

be a member of a class of similar events. For what it is worth psychologically we probably reduce our burden of thought as we see generality emerge from particularity. As we have said this is the process of classification that is a necessary part of language and explanation. But as everyone knows there are good classifications and there are bad classifications. Actually the distinction between good and bad classifications has been given an objective meaning; to understand this meaning helps us to understand the nature of classification, concept formation, and laws in science. A classification is "good" when it is a "natural classification"; it is "bad" when it is an "artificial classification".[8] The distinction is objective and refers to the fact that some classifications of phenomena allow us to explain and predict a rather modest range of events that are not already included in the basis of the classification (the defining characteristics of the classification) while others go further. Thus in the case of animals and plants classified in terms of their adult shape we have an instance of an "artificial" classification. Opposed to this the "natural" classification of animals according to their method of reproduction on a phylogenetic scale. In the case of "shape" we can predict and explain very little about the life and habits of a creature classified on this basis. To be sure we can tell in advance a few things about the mode of support or perhaps

locomotion but that is all. Contrast this to the phylogenetic classification. On the basis of a few crucial characteristics, especially reproductive and physiological characteristics a competent biologist can derive an almost indefinite number of generalizations about the living creatures in his classification. Not only can he tell the medium in and through which the form moves, but he can tell as well the probable method of rearing, the sorts of reproductive structures, food habits, and the structures that must be present if the creature in question is to adjust to the various environments and ranges of climate available.

Conceptual explanation can, after all, provide us with a significant body of knowledge. Classifications into kinds or types by use of abstract characteristics is informative. It allows us to predict and explain and depending upon how well the defining characteristics that go into its basis were chosen it will give us more or less information. But generally concept formation goes apace with the discovery of laws in science and cannot be dissociated from it. Medieval herbalists' classifications of plants according to the "doctrine of signature" suggests one type of concept formation. The defining basis consisted of the resemblance of a plant part (seeds, flowers, stems, roots, leaves) to a part of the human anatomy. The law stated that insofar as an aspect of plant anatomy resembled an

aspect of the human anatomy then that plant would be useful in curing the diseased area. As it turned out this relation did not hold and a new basis was discovered. Concept formation and laws can never be dissociated from one another. Furthermore concept formation will depend upon the focus of interest of the classifier: how we will cut up the world will depend upon what we want to know, that is, what we are interested in.[9]

So much for facts and concepts. We suggested a few pages back that while conceptual explanations were common and useful in science as in life they are not the only import of a "why" question. Classification is closely related to what is commonly called description. The other meaning that "why" questions suggest is more generally thought of as a "true" explanation. A person may be satisfied (we employ the psychologically orientated illustrations as heuristic aids only) with the suggestion that the complex social structure he has been studying in Japan is a feudal system. As well, an individual may be satisfied with being told that the radiator of the car he is now viewing--a radiator from which a large icicle is protruding --is of the class of phenomenon that burst under the requisite conditions. Certainly many children will accept this sort of explanation. If a child asks why it is that babies are so thirsty and is told that it is the nature of babies to be thirsty, this explanation will often satisfy them.

What the child has received in this case is a conceptual or classificatory explanation. However the student of Japanese society may remain unmoved by our reply that he is studying a type of feudal society; the chagrined viewer of the burst radiator may be unimpressed by our classification of his radiator as one of a large class of objects that burst (especially if he is a denizen of the equatorial regions visiting the north); and finally the child too may counter with the further question as to why it should be the "nature" of babies to have thirst. It behooves us to give, at this point, careful consideration to the nature of these requests.

"Why" requests of this second sort are generally requests for explanation by general law. As is well known laws not only form the basis of science but its ultimate goal as well. Systems of them form the criteria of coherence which we shall shortly discuss. General laws are often spoken of as generalizations or universal generalizations. There is however some suggestion of uncertainity regarding the question of whether general laws are the same sorts of things as generalizations. Of late this distinction has been clarified, as we shall see, by the analyses of R. B. Braithwaite, but for the present we will use the two terms as simply synonyms.[10] If laws or generalizations form the basis and the end of scientific investigation we must ask: What is the nature of a general law?

As we have suggested they too are constant conjunction statements. They are statements asserting some sort of invariancy between classes of events--as we have already seen. Therefore they are conjunctions of concepts. Generally they take the form--they have the form even though the scientist may neglect to always phrase them in just this manner--"Everything that is of class or kind A is also of class or kind B".[11] Or more simply: Every A is B". Examples of laws (whether true or not) are the following: in constant volume gases increase their pressure as their temperature increases"; all Sierran big trees occur at elevations of 5,000 to 8,000 feet above sea level"; "in situations of perfect competition supply equals demand"; "the angle of reflection of light from a dense surface is equal to the angle of incidence"; "it is a universal fact that suicide rates in a society are proportional to the index of anomie"; "the degree of systematization found in the literary and oral tradition of a people will depend upon the degree to which they are politically centralized"; and so on and on. In order to qualify as a lawful statement the proposition must be true but it need not be metrically stated. It is easy to see that qualitative (and quantitative) statements of this sort cover a class of great magnitude.

Nevertheless they all possess the abstract form of "Everything that is of kind A is also of kind B" and this plus their truthfulness is all that is required to be laws

in science.

Now even though laws have this form and even though the number of laws in science is extremely vast there is a limit to them. This limit is interesting and will aid us in comprehending the nature of scientific explanation. Eugene Wigner has prescribed a criterion of "limit" to what he proposes we call "our science". Wigner adopts as his basis of "our science" all the scientific knowledge that men have actually learned at any particular time. For our purposes it is better to denote by science all that is contained in the literature of science.

Now what is contained in the literature is rather vague but it does suggest that there is some knowledge that never finds its way into scientific books. Such simplistic laws as "redhot objects burn", "the movement of air is experienced by us as wind", "water conducts current", "grass clumps if not grazed", "earthworms come to the surface at night", "trout like earthworms", and so forth are examples. Such rather simple and imprecise laws (not so simple for a child first learning them) could be made more precise but still not find their way into the scientific literature. It is not that they are not laws but rather that they are not of sufficient scope to warrant our concern.[12] In fact, we shall see that most of these laws are already accounted for by more general laws. Nevertheless just what laws will be included in "our science" is not a highly precise

matter. Some of them will be considered too obvious or well known to warrant inclusion, some will not, and the boundary between the two realms will always be shifting as long as science is viable.[13]

Now that we have at least a suggestion of what a law amounts to we must ask how the second "why" question is to be taken. It, just as the first, is a request for explanation. How does a law explain? A law explains in exactly the manner in which a concept explains, that is, by subsumption. When we explain an event we subsume it under a lawful regularity. If we wish to explain why it is that the 12th Century Japanese system was Feudal we do so by subsuming this particular class event (this member of the class of feudal entities) under a general law of feudal systems. We may know of no such law at present but if and when it is discovered it will provide the explanation required. For instance, suppose that you were able to substantiate the hypothesis that feudal systems develop whenever structures of central authority disintegrate in agrarian society. If this hypothesis were substantiated (were a law) then we should explain the Japanese system by subsuming it under our law. Of course, if it was the case that the Japanese system had all of the abstract characteristics that feudal systems have and yet did not have a disintegrated central authority then it would not be a true law or true explanation, but that is not the point

at issue here.

Similar explanations may be given in the instance of the burst radiator and the thirsty baby. The law that water expands when it freezes and the law stating the relative strength of radiator materials may both be evoked to explain why this particular radiator burst. So too may this baby's thirst be explained by the law that certain types of organisms (generally mammals) require water intake of such and such amount, which itself rests upon laws of mammalian physiology, and so on.

All of this may be easier to grasp if we detail an illustration that we have used above. Actually Carl Hempel had done most of the job for us and we will now quote at some length from his interesting paper: "The main function of general laws in the natural sciences is to connect events in patterns which are usually referred to as <u>explanation</u> and <u>prediction</u>. The explanation of the occurrance of an event of some specific kind E at a certain place and time consists, as it is usually expressed, in indicating the causes or determining factors of E. Now the assertion that a set of events--say, of the kinds C_1, C_2,..., C_n--have caused the event to be explained, amounts to the statement that according to certain general laws, a set of events of the kind mentioned is regularly accompanied by an event of kind E. Thus, the scientific explanation of the event in question consists of

(1) a set of statements asserting the occurrence of certain events $C_1 \ldots, C_n$ at certain times and places
(2) a set of universal hypotheses (laws), such that
  (a) the statements of both groups are reasonably well confirmed by empirical evidence (requirement of truth),
  (b) from the two groups of statements the sentence asserting the occurrence of event E can be logically deduced.

In a physical explanation, group (1) would describe the initial and boundary conditions for the occurrence of the final event; generally, we shall say that group (1) states the determining conditions (latter called antecedent or initial conditions) for the event to be explained, while group (2) contains the general laws on which the explanation is based; they imply the statement that whenever events of the kind described in the first group occur, an event of the kind to be explained will take place.

Illustration: Let the event to be explained consist in the cracking of an automobile radiator during a cold night. The sentences of group (1) may state the following initial or boundary conditions: The car was left in the street all night. Its radiator, which consists of iron, was completely filled with water, and the lid screwed on tightly. The temperature during the night dropped from 39° F. in the evening to 25° F. in the morning; the air pressure was normal. The bursting pressure of the radiator material is so and so much. --Group (2) would contain empirical laws such as the following: Below 32° F., under

normal atmospheric pressure, water freezes. Below 29° F., the pressure of a mass of water increases with decreasing temperature, if the volume remains constant or decreases; when the water freezes, the pressure again increases. Finally, this group would have to include a quantitative law concerning the change of pressure as a function of its temperature and volume.

From statements of these two kinds, the conclusion that the radiator cracked during the night can be deduced by logical reasoning; and explanation of the considered event has been established."[14]

While the quotation from Hempel makes specific reference to the physical sciences it does so only because the paradigm of explanation will then be shown to apply to all sciences and even areas not thought of as scientific (see title). In fact this was the intention in writing the article.[15]

Now from this illustrative passage several facts emerge. First and most obvious is the fact that the "Hempel paradigm" (if we may call it that) is the same as the paradigm we have been developing. This suggestion is reinforced by the appearence several years later (in collaboration with Paul Oppenheim) of a long study in the "logic of explanation" following exactly these lines though with greater refinement and depth.[16] Also in this later study there appears a long footnote the import of which shows that this paradigm

is not idiosyncratic but really an explicit formalization of what a great number of eminent writers (from Mill and Pearson on) have had to say on the subject of explanation.[17]

A scheme such as we have developed--really only a summary of the Hempel-Oppenheim paradigm (hereafter referred to simply as the H-O paradigm)--is then, what is meant by explanation in empirical science. This is an analysis of what is going on when an event or a law is explained. So far as we can see, and so far as competent writers have been able to see, explanation consists of this sort of structure (logically) and nothing more.

The second thing that we notice in examining the passage quoted above is that in addition to category #2, the category of general laws, Hempel has still another set of statements (sentences he calls them) stating "determining conditions". The determining conditions figure into all models of explanation. They take a variety of names of which the following are the most prevalent in the literature: "antecedent conditions", "boundry conditions", "initial conditions", "state conditions", occasionally "parameters" when somewhat incorrectly employed, and sometimes "state descriptions" or merely "descriptions". The statement of these conditions is as important as the laws themselves for the process of explanation. Let us illustrate what is meant by these various designations.

Suppose that there has just occurred an eclipse of the

sun by the moon and we ask an astronomer for an explanation of just this event (not of eclipses in general for which general laws would be sufficient). What we want to know is why this eclipse at this time. To answer our question the astronomer needs two sorts of statements: he needs of course the general laws stating the regularities of motion of the satelites about the sun (Kepler's laws) and perhaps certain laws stating the behavior of large bodies generally (Newton's laws). But he also, in addition, needs initial or antecedent conditions relative to the solar system. The initial or antecedent conditions consist of such statements as 1) the relative positions of the earth, moon, and sun one to another at some arbitrarily selected time (t). Coupled with these statements or "state-conditions" must be 2) numbers describing the relative velocity of the earth, moon, and sun, one to another. All of these statements we call initial, boundary, antecedent or determining conditions. They are also often called descriptions of the state of a particular system at some specified time. Such statements of conditions or state-conditions of a system allow the scientist to apply laws to particular situations. They are not aspects of laws as such, but they are necessary if the laws are to be applied to events for explanation. They are always relative to some kind of event that we wish to explain. In the case of the eclipse the particular initial conditions that we

will choose will be selected in terms of our desire to explain just this event and no other. All of this is very obvious and is generally summed up in the adage that we must know the nature of our subject (the facts) before we can hope to explain what went on. Every detective story operates identically; the detective must first "get the facts" (i.e., initial conditions of the crime) and then drawing on the "laws" of human behavior (cultural and social regularities) the sleuth can deduce, in classical H-O paradigmatic fashion the probable hypotheses to be tested, that is, the probable explanations.

So far, in drawing out the implications of what it means to explain something, we have said nothing about prediction. Ideally when we explain an event we should be able to predict it as well. In fact the analysis we have just made of the explanation of solar eclipses suggests that it is identical to what we might do in a prediction. These indications are in fact correct, for as many authors have observed explanation and prediction have an identical logical structure;[18] the sole difference between them is a pragmatic one. It rests on the purposes at hand. Karl Popper has summarized this nicely in his famous The Open Society and Its Enemies,[19] and J. W. N. Watkins, an economist, in full agreement with Popper puts the matter even more succinctly: "It has been established by Professor Karl R. Popper that the formal structure of prediction is the same

as that of a full-fledged explanation. In both cases we have (a) initial conditions; (b) universal statements (i.e., general laws); and (c) deductive consequences of (a) plus (b). We explain a given event (c) by detecting (a) and postulating and applying (b); and we predict a future event (c) by inferring it from some given (a) and postulated (b)."* However while the two processes are only pragmatically disjoined Watkins feels that the distinction is a useful one particularly, though not exclusively, in the social sciences. He notes "...even the social scientist who can provide a <u>full fledged</u> (in contradistinction to what is more often the case in the social sciences: Hempel's "explanation sketch", an incomplete statement of the laws and initial conditions) of a past event will run into difficulties if he tries to predict similar events, because they will occur in a system which is not isolated from the influence of factors which he cannot ascertain beforehand. The Astronomer Royal can prepare a Nautical Almanac for 1953 because he is predicting the movements of bodies in a system isolated (for his purposes though never absolutely isolated i.e., from cosmic radiation or stellar dust) from extraneous influences, but the Chancellor of the Exchequer

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*J. W. N. Watkins, "Ideal Types of Historical Explanation", <u>Readings in the Philosophy of Science</u>, H. Fiegl and M. Brodbeck, New York, 1953, pp. 723-724.

cannot prepare an Economic Almanac for 1953 because, even if he possessed sufficient knowledge to explain completely the 1951 level of prices, production, investment, exports, etc., his predictions of future levels would undoubtedly be upset by unforseeable, world-wide disturbing factors, the effects of any of which might be cumulative."[20] This identical situation of course holds for meterology and also for aspects of geology (certainly a physical science); the geologist cannot predict earthquakes though he certainly has no trouble explaining them. Actually in this situation the social sciences seem better off.

The upshot of this discussion is that prediction and explanation differ in no important respects. Therefore when we speak of explanation in this thesis it will be understood that we mean prediction as well. Also it should be noted that Watkin's suggestions regarding isolated systems is itself merely pragmatic in intent. As Bertrand Russell has remarked[21] the end of science is not the explanation of isolated systems. The elucidation of isolated systems serves as a means. We isolate so that we may discover with ease the laws operative in a simplified system. The final aspiration of science may reasonably be construed as the total lawful explanation of every phenomenon that compels our attention. Nevertheless there is strong reason to believe that explanations of this sort would prove so complex that we simply could not handle them (except in

principle). After enough tests have been performed to verify a universal theory it would probably not find much further use except in challanging further theorizing. Pragmatically there is some justification for being concerned with isolated systems; they limit our focus of attention and to that extent increase our precision in limited explanation and prediction.[22]

Thus far we have talked almost exclusively of the explanation of "events". However events, that is simple occurrences, are not the only entities that we explain in the world. We have seen that both concepts and laws explain by subsumption. But what they explain may be "events", that is "facts" such as an eclipse or a business cycle, or suicide, or, on the other hand they may explain <u>other laws</u>. Now to explain a law is not the same thing as to explain events. Laws are universal statements that are true (general statements universally quantified). To explain laws we do not require initial, boundary, or antecedent conditions. Since a law is not an event occurring in the actual world it may be explained simply by subsuming it under a more general law. Thus it is that Kepler's laws of planetary motion and Galileo's laws of falling bodies were subsumed under Newton's laws, and in turn Newton's laws were subsumed under Einstein's laws.[23] In biology this process occurs constantly. The Mendelian laws of plant hybridization were subsumed under Mendel's own rather

simple genetic theory ("theory of unit characters") and it in turn has been subsumed under a more advanced statistical theory.[24] Natural selection has been "explained" that is, subsumed under laws of genetic mutation and the laws of heredity.[25] The laws governing embryological maturation have been subsumed (hopefully) under the biochemical theory (laws) of "organizers".[26] Such explanations also occur in the social sciences though they are far less precise. For instance, suppose that Durkheim were correct in asserting that anomie is inversely proportional to the degree of social cohesion.[27] Suppose also that culture-wide quantifications of cohesion were obtained and that they bore this out. This law might then be explained by a more general law connecting indexes of social interaction in later life with indexes of social interaction in the first 10 years of life. The law being something to the effect that relative degrees of early interaction are connected with a necessary degree of adult interaction. If these interaction levels are disparate the social consequences are manifest as suicide or some anomic equivalent. Frankly we do not know of any such law, but it is certainly clear that if we had one such as this it would serve as an explanation of Durkheim's law. Or taking Michel's theory of democratic organization we notice that the author himself provides the wider regularity by noting that the abrogation of perogative to a leader rests finally upon an even wider

economic basis: that once in power social differentiation and status considerations tend to pervert the purpose of organization. So we might say that some form of the Marxian situation is the more general theory used to explain Michel's position. Some economists often try to explain the laws of business cycles by reversion to psychological states of the individual actors.[28] Institutional economists are often concerned with similar types of explanations resulting from the engulfing normative structure in which economic relations develop. Social psychology may be regarded as largely concerned with attempting to frame a series of laws that explain the laws of large scale institutional analysis, though many sociologists have their fingers in both pies and it is not always obvious that they are conducting a dual study.[29]

Such considerations incidentally provide additional information on the nature of "reductionism".[30] Legitimate "reduction" occurs in science when we explain one set of laws (with their specialized conceptual basis) by another set of laws (with a wider conceptual basis). Nothing is wrong with this procedure. In fact it is what we mean when we talk of the empirical "unification of science". Empirical scientific unification envisages a situation in which all the special sciences arrange themselves within a hierarchy of deductive relations constituting our knowledge of the world. This goal has not been achieved yet but it is one

which science entertains as a hoped for goal. It should be distinguished from the "formal unification" of science which is what we have been discussing and is a matter of fact. All science has the same formal structure; the methodological "language" of science is uniform. And it therefore makes sense to speak of reductionism as legitimate within the formal unity of science. Reductionism is spurious and pernicious insofar as it empirically suggests but cannot attain the derivation of one branch of science from another. In other words what is generally labeled reductionism in the nefarious sense of the term denotes any attempt to explain the laws of one science by an inadequate theory from another. The meaning of reductionism in this latter sense is the same as that we apply to any unconfirmed theory.[31]

Therefore if we adequately explain events we do so by subsuming them under laws, and in the weaker or conceptual sense by subsumption under class terms. Laws themselves are explained by subsumption under more general laws. And these laws must be true or we are "reducing" in the spurious sense of the term. Finally in any science we eventually hope to attain the most general laws of that science and our explanation stops. The "most general" laws of any science are relative of course to time and place, for they may be replaced by still more general laws at some other time or place. Nevertheless scientific explanation does

cease somewhere at any particular time. This does not mean that there is something faulty about it, or as Whitehead rather foolishly maintains, that it shows the world to be at bottom "irrational and arbitrary".[32] To ask of science, that is, of explanation more than it ever offered is a childish response to the necessities of logical analysis.

In summary we notice that our rather simplified model follows the H-O paradigm closely. To explain events we either confer upon them class membership by conceptual placement or we deduce them by employing initial conditions, general laws, and appropriate formal manipulations. Such deductions constitute the traditional basis for the distinction that exists between description and explanation. To then explain laws we deduce them from still more general laws. We have mentioned the fact that the term "law" and "generalization" are closely related. Since these notions are not, as commonly used, construed as referring to the same thing, and since we will find some use for this distinction it is important that we now distinguish the grounds upon which this difference of usage rests. R. B. Braithwaite in his extremely important full scale study of scientific explanation has hit upon what seems to be the basic difference. A generalization is a statement of the form "Every A is B" where the <u>evidence</u> for this statement is <u>instances only</u>. A law statement is of the form "Every A is B" where this statement is <u>deduced from a generalization as well as</u>

having instances which stand for it. Thus every inductive generalization such as "All crows are black" has instances standing for it. Perhaps one has seen 500 black crows and then inductively infers that "All crows are black". That is, there are 500 instances standing as verification for this induction. Now suppose we discover the following more general law (which so far as we can tell is not true but still will serve to indicate our point) "Every bird that evolves in dark coniferous forest conditions will be black". For it we also have instances supposedly garnered from inspecting coniferous forest faunas. However it is now clear that from this more general proposition we can deduce the less general law that "All crows are black" by the addition of the premise: "Crows evolved in coniferous forests". The latter we take to be a law; the former we take to be a "mere" generalization. And the reason we do so is because we feel that the "law" is more fully confirmed; not only does it have instances substantiating it but it is deduced from a generalization which itself has instances.[33]

We have mentioned the requirement of coherence in empirical science and we now see more clearly what this amounts to. The criterion states that laws in empirical science are mutually reinforcing, and for them to be mutually reinforcing or coherent they must either be the highest level law of that science from which deductions can be made to lower level laws, or such laws must be deductive conse-

quences from some higher level law. It takes only a moments reflection to see that coherence is really only another way of stating a highly valued goal of science: that the laws of science--and eventually all science conceived as the totality of knowledge--should come to form vast pyrmidal hypothetical-deductive systems. At the top occur the most general laws, at the bottom the least general (often referred to as common sense statements). The entire structure is related logically by the rules of deductive inference, it is related empirically by the criterion of verification.

Finally we come to the question of theories in the strict sense. Theories actually form the final pinion of scientific explanation and we must say a word about them. At the highest level they are what "explain" laws in the strict sense. However, as we suggested they are not the only entities that explain laws. Laws are legitimately explained by more general laws. But in some advanced areas of science--though not all sciences--theories are constructed which perform the explanatory--subsumptive--task. Strictly speaking the work "theory" should be reserved for a very special sort of construct in science. Theoretical terms have traditionally been recognized as such: ion, atom, wave-function, gene, energy-levels, organizers, libido, instinct, etc. One thing we note about theoretical terms is that none of them are simple observables. All require that we observe imputed effects of their operation. This

fact has given rise to a full scale controversy as to the "ontological status of theoretical entities" out of which, in part, grew the precept that meaningfulness was related to testibility, at least in principle. As it turned out the "in principle" restriction came to mean either "until we have the technical mean to perform the test" as in the case of the "other side of the moon"; or it came to mean contextual confirmation, as in the case of theoretical entities. By contextual confirmation we mean that while we may never be able to confirm a true theory by direct observation we can confirm deductions from that theory. Also we can say, with Braithwaite, that the meaning of theoretical terms derives from their context in a theory. Nevertheless the problem is extremely involved and requires considerably more space than is available in a study such as this. Richard Bevan Braithwaite's splendid analysis (often cited in this thesis) of theories in his book Scientific Explanation will prove an admirable guide.

We have suggested these considerations because even a thumbnail sketch of the "logic of explanation" must at least mention the role of theories. Nevertheless for our purposes we do not need to draw upon any more difficult an explanatory entity than the Hempel-Oppenheim paradigm of laws in scientific explanation that we have outlined

Teleological Systems and Their Analysis

In our presentation of the various classes of motivational theories we found that at least one of them involved some mention of teleological and functional concerns. And in the discussion of "non-motivated purposive systems" we also saw that the concepts "motive", "purpose", "function" were interchangeable. We must now provide ourselves with a schema which analyzes the systems to which such concepts refer.

There exists today several alternative analyses that deal with teleological or directively organized systems. Some of these analyzes are of a fairly low degree of abstraction. They attempt to discuss actual systems, biological, physical, social in terms of fairly concrete elements. They attempt to show what it is about such systems that makes us think of them as purposive or teleological and how this is to be understood in terms of empirical science. On the other hand there is a fairly large number of logical constructions whose aim is not to examine the structure of any particular system but to ascertain the general characteristics common to all such systems. In short such attempts analyze teleology in terms of logical variables; and it is of course the nature of such variables that they may take any number of concrete constants as long as they fulfill the general requirements of that variable.

It is in particular the latter sort of analysis that

interests us in our discussion of teleological systems; for if we can understand the abstract logical relations involved in teleological systems then we can evaluate teleological claims to ascertain whether or not they possess characteristics unique to such systems. The amount of interest aroused by the modern analysis of teleology is impressive, and before we attempt to present the logical structure of such systems shorn of all concrete reference we may at least mention a few of the more prominent writers in this field and the basic position they occupy. As a matter of fact it is useful in examining the question of teleological systems and teleological explanation to approach it in terms of increasing abstractness. After this very brief review of major investigators of teleological and (by the same token) functional systems, we will then examine in detail the fully formalized conceptions of Ernest Nagel.

Among the major writers in this field an early investigator who did more to illustrate the nature of such systems than to analyze them was Walter Cannon, author of the theory of "homeostasis" the import of which was to show the complex self-regulative effect of physiological organ systems.[34] Today modern physical science exponents include Norbert Wiener, Julian Bigelow, and Arturo Rosenbleuth and the theory of control systems or cybernetics.[35] These authors and their followers are interested in establishing

analogies that will amount to homologies between self-regulative or self-controlling biological systems and certain fundamental physical or mechanical devices such as error-correcting computers and so forth. The most generalized notions to which they attain however are essentially those of universal physical laws (entropy or the second law of thermodynamics) and not formal analysis. These conceptions hinge upon a construal of life systems as reversing the second law by employing part of the energy of a system to control the system.[36] Jacob Bronowski follows in the same vein with his analysis of computer machines and "science as foresight".[37] Again his analysis rests upon either biological categories or very general physical principles.

Two writers partially bridge the gap between concrete analysis and formal systems. G. Sommerhoff, a mathematician, is concerned with certain types of variables and the notion of self-regulative or purposive behavior. In part his study is biologically orientated, but in part it suggests the possibility of general, abstract conceptions that may be handled by the notations of formal mathematics.[38] More detailed and more sophisticated is the analysis of Ross Ashby.[39] We encounter here purely formal presentation which is <u>illustrated</u> by reference to biological analogies (the homostat). Two other authors do achieve the stripped abstract system we seek. By so doing they demonstrate the

necessary foundations involved in such systems and disallow the possibility of confusion often suggested by assuming that what holds for biological systems need not hold for social systems and what holds for social systems need not hold for adaptive machines. In this connection we mention again the work of Braithwaite and his study of "variancy systems"[40] and finally the intensive investigations carried on by Ernest Nagel in directively-organized systems, teleology, and the formalization of functionalism.[41]

It would be impossible and unnecessary to expand each of these positions in the space available without distorting the intention of the present investigation. In addition to the men that we have mentioned there are of course many other investigators in this field. But at this point it seems most fruitful to develop the conceptions of Ernest Nagel rather fully for they provide us with the soundest foundation for discussing the implications of such systems.

The reader's indulgence is requested regarding the length of the following quotations from Nagel's article Teleological Explanation and Teleological Systems. While it may seem unduly long it will prove far shorter than any attempt to render it in non-technical terms. Teleological considerations are not the least difficult of the perplexing problems of science, nor is their analysis the most simple. The already dense nature of Nagel's prose frankly belies further condensation as the reader will see. We

begin with a general characterization of teleological systems in which certain complicating features have not been introduced. These features will be introduced after the general presentation.

"With the homestasis of the temperature of the human body before us as an exemplar, let us now state in general terms the structure of systems which have a goal-directed organization. The characteristic feature of such systems is that they continue to manifest a certain state or property G, or to develop ' in the direction' of attaining G, in the face of a relatively extensive class of changes in their external environments or in some of their internal parts--changes which, if not compensated by internal modifications in the system, would result in the vanishing of G or in an altered direction of development. This feature can be formulated more precisely though schematically as follows.

Let S be some system, E its external environment, and G some state or property which S possesses or is capable of possessing under suitable conditions. Assume for the moment--this assumption will presently be relaxed--that E remains constant in all relevant respects, so that its influence upon S can be ignored. Suppose also that S is analyzable into a structure of parts, such that the activities of a certain number of them are casually relevant for the occurrence of G. For the sake of simplicity, assume that there are just three such parts, the state of each of which at any time can be specified by a determinate form of the complex predicates "A", "B", and "C", respectively; numerical subscripts will serve as indicators of such determinate forms. Accordingly, the state of S at any time causally relevant to G will be expressed by specializations of the matrix "$(A_xB_yC_z)$". One further general assumption must now be made explicit. Each of these state-variables (they are not necessarily numerical variables) can be assigned any determinate values that are compatible

with the known character of the part of S whose state it specifies. In effect, therefore, the states which can be values of "A_x" must fall into a certain class K_A; and there are corresponding classes K_B and K_C for the other two state variables. The reason for this restriction will be clear from an example. If S is a human body, and "A_x" states the degree of dilation of peripheral blood-vessels, it is obvious that this degree cannot exceed some maximum value; for it would be absurd to suppose that a blood-vessel could acquire a mean diameter of, say, five feet. On the other hand, the possible values of one state-variable at a given time will be assumed to be independent of the possible values of the state variables at the same time. Accordingly any combination of values of the state variables will be a permissable specialization of the matrix "$(A_xB_yC_z)$", provided that the values of each variable belong to the classes K_A, K_B, and K_C respectively. This is tantamount to saying that the state variables which are stipulated to be causally relevant to G are so postulated as to be capable of having values at a given time which are mutually independent of one another.

Suppose now that if S is in the state $(A_oB_oC_o)$ at some given time, then S either has the property G, or else a sequence of changes will take place in S in consequence of which S will possess G at some subsequent time. Call such an initial state of S a "casually effective state with respect to G", or a "G-state" for short. Not every possible state of S need be a G-state: for one of the causally relevant parts of S may be in such a state at a given time, that no combination of possible states of the other parts will yield a G-state for S. Thus, suppose that S is the human body, G the property of having an internal temperature lying in the range of 97° to 99° F, A_x again the state of peripheral blood-vessels, and B_y the state of the thyroid glands; it may happen that B_y assumes a value (e.g., corresponding to acute hyperactivity) such that for no possible value of A_x will G be realized. It is also conceivable that no possible state of S is a G-state, so that in fact G is never realized in S. For example, if S is the human body and G the property of having an internal temperature lying in the range 150° to 160° F, then there is no G-state for S. On the other hand, more than one possible state of S may be a G-state, though only one of them will be actual at a given time; but if there is more than one possible G-state, we shall assume that the one which is realized at a given time is uniquely determined by the actual state of S at some previous time. In short, we are assuming that S is a deterministic system with respect to the occurrence of G-states. The case in which there is more than one possible G-state for S is of

particular relevance to the present discussion, and we must now consider it more closely."[42]

We now have a general characterization of the major assumptions and a simplified system. What we need is to gradually complicate this scheme so that it will be able to handle the complex systems that actually occur (biological, social, physical); also it will be necessary to relax some of the earlier assumptions. What we want in other words is to arrive at a system that can make compensations for a series of internal and eventually external changes--boundary maintaining systems, as Parson's would have it. Nagel continues as follows:

"Assume again that at some time t_o, S is in the G-state ($A_oB_oC_o$). But suppose now that a change occurs in S so that in consequence A_o is caused to vary, and that at time t_1 subsequent to t_o the state variable "A_x" has some other value. Which value it will have at t_1 will depend on the particular changes that have occured in S. We shall assume, however, that there is a range of possible changes, and that the values which "A_x" may have at time t_1 fall into some class K'_A (a sub-class of K_A) which contains more than one member. To fix our ideas, suppose that A_1 and A_2 are the members of K'_A; and assume further that neither ($A_1B_oC_o$) nor ($A_2B_oC_o$) nor ($A_2B_oC_o$) is a G-state--that is, a variation in A_o alone would take S out of a G-state. Accordingly, if the changes mentioned thus far were the only changes in the state of S, S would no longer be in a G-state at time t_1. Let us, however, make the contrary assumption. Assume S to be so constituted that if A_o is caused to vary so that the value of "A_x" at time t_1 falls into K'_A, there will also be further compensatory changes in the values of some or all of the other state variables. More specifically, these further changes are stipulated to be of the following kind: if K'_{BC} is the class of sets of values which "B_y" and "C_z" have at time t_1, then for each value of "A_x" in K'_A there is a unique set in K'_{BC} such that S continues to be in a G-state at time t_1; but these further changes unaccompanied by the first mentioned ones would take S out of a G-state--that is, if at time t_1 the state variables

of S have a set of values such that two of them belong to a set in K'_{BC} while the remaining one is not the corresponding member of K'_{A}, then S is not a G-state. For example, suppose that if A_o is changed into A_1, the initial G-state $(A_oB_oC_o)$ is changed into the G-state $(A_1B_1C_1)$ with $(A_oB_1C_1)$ not a G-state; and if A_o is changed into A_2, the initial G-state is changed into the G-state $(A_2B_1C_o)$, with $(A_oB_1C_o)$ not a G-state. In this example, K'_A is the class (A_1A_2), and K'_{BC} the class of sets $((B_1, C_1), (B_1, C_o))$, with A_1 corresponding to (B_1,C_1) and A_2 to (B_1, C_o)."[43]

We now have all of the elements of our system and a specification of the specializations that they may take. The system elements and sub-elements may be elaborated to any degree of complexity that is desired; the degree depending mainly upon the complexity of the system under consideration and the complexity of the "end" or "goal" or simply "event" that must be explained. As the system stands it is not, perhaps, obvious how it is to be applied. For this we need some definitions based upon the purely formal aspects and a slight further complication. In so doing we will see how it comes to grips with actual problems of directively-organized systems and how an explication of teleological terms is possible without recourse to non-empirical entities or to any conception not developed in the "logic of explanation". Nagel continues:

"We now introduce some definitions, based upon the above discussion. Assume S to be a system satisfying the following conditions: 1) S can be analyzed into a structure of parts, a certain number of which (say three) are causally relevant to the occurrence in S of some property or feature G; and the causally relevant state of S at any time can be specified by means of a set of state-variables. These state-variables at any given time can be assigned values independently of each other, though the possible

values of each variable are restricted to some class of values. 2) If S is in a G-state at some time t_o during period T, and a variation occurs in one of the state parameters (say "A") such that this variation alone would take S out of its G-state, then the possible values of this parameter at time t_1 subsequent to t_o but still in T fall into a certain class K'_A. Call this variation a "primary variation" in S. 3) If the state parameter "A" varies in the indicated manner, then the remaining parameters also vary so that their variation alone would take S out of its G-state, and so that their possible values at time t_1 constitute sets belonging to a class K'_{BC}. 4) The elements of K'_A and K'_{BC} correspond to each other in a uniquely reciprocal fashion, such that when the state of S is specified by these corresponding values S is in a G-state at time t_1. Call the variations in S which are represented by the members of K'_{BC} the "adaptive" variations in relation to the variations represented by members of K'_A. When these assumptions hold for S, the parts of S that are causally relevant to G will be said to be "directively organized", if the reference to T and G can be taken for granted. This definition can be easily generalized for a larger number of state-variables, and for the primary variation of more than one state-variable; but the present incompletely general definition will suffice for our purposes.

"It will be clear from this account that if S is directively organized, the persistence of G is in a certain sense independent of the variations (up to a point) in any one of the causally relevant parts of S. For although it is the state of these parts which by hypotheses determine the occurence of G, an altered state in one of them may be compensated by altered states in the other parts of S so as to preserve S in its G-state. The structure or character of so-called "teleological" systems is therefore expressed by the indicated conditions for a directively organized system; and these conditions can be stated, as we have seen, in a manner not requiring the adoption of teleology as a fundamental or unanalyzed category. What

may be called the "degree of directive organization" of a system, can also be made explicit in terms of the above analysis. For the property G is maintained in S (or S persists in its development which eventuates in G) to the extent that the range of K'_A of possible primary variations is associated with the range of induced compensatory changes K'_{BC} such that S is preserved in its G-state. The more inclusive the range of K'_A that is associated with such compensatory changes, the more is the persistance of G, independent of variations in the state of S. Accordingly, on the assumption that it is possible to specify a measure for the range K'_{BC} the "degree of directive organization" of S with respect to variations in the state-parameter A can be defined as the measure of this range."[44]

We now see, as McCulloch has put it,[45] what the empirical significance of "purpose" or "directive-organization" in systems amounts to, and we have seen that the nature of such systems does not involve us in any unique or unanalyzable categories. It remains only to relax the restriction that Nagel placed on the environmental (E) phenomenon for the purposes of exposition to give the full picture of such systems. Functionalism should now appear as a variant in the vocabulary of teleological systems; the difference being that the purposive vocabulary stresses ends toward which elements of systems contribute; functional vocabularies stress the elements of systems that contribute to

these ends (in Nagel's scheme the state-variables and classes of values they may take). Functionalism also will be elucidated by relaxing the environmental restriction. Nagel concludes the formal analysis thusly:

"We may now relax the assumption that the external environment has no influence upon S. But in dropping this assumption we merely complicate the analysis, without introducing anything novel into it. For suppose that there is some factor in E which is causally relevant to the occurrence of G in S, and whose state at any time can be specified by some determinate form of the state-variables "F_W". Then the state of the system S' (which includes both S and E) that is casually relevant to the occurrence of G in S is specified by some determinate form of the matrix "$(A_XB_yC_ZF_W)$"; and the discussion proceeds as before. However, it is generally not the case that a variation in any of the internal parts of S produces any significant variation in the environmental factors. What usually is the case is that the latter vary quite independently of the former; that they do not undergo changes which compensate for changes in the state of S; and that while a limited range of changes in them may be compensated by changes in S so as to preserve S in some G-state, most of the states which environmental factors are capable of assuming cannot be so compensated by changes in S. It is customary, therefore, to talk of the "degree of plasticity" or the "degree of adaptability" of

organic systems in relation to their environment, and not conversely. However, it is possible to define these notions without reference to organic systems in particular, in a manner analogous to the definition of "degree of directive organization" already suggested. Thus suppose that the variations in the environmental state F, compensated by changes in S so as to preserve S in some G-state, all fall into the class K'_F; then if a measure for this class is available, the "degree of plasticity" of S with respect to G in relation to F can be defined as the measure of K'_F".[46]

We have now seen the formal structure of directively-organized systems and we have sampled a few implications of such analysis. It should be clear that it is of no importance how the empirical construal or application of these formal relations is spelled-out, the particular mechanisms that may be biologically involved or socially required are of importance only in the concrete applications of the scheme.

But we must now note an important point, mainly that it is only our parochalism that confers upon such systems whatever priviledged status they possess. It makes as much sense to employ teleological or functional vocabularies as to avoid them in favor of a non-teleological physicalist vocabulary. For understood in Nagel's terms there is nothing that ultimately separates the two phenomena except our particular focus and point of interest. One might quite

truly say why look upon a kitten, for instance, as a special sort of system different from the world around it. Consider the atoms and molecules and compounds that comprise the animal and we are forced to concede that from this point of view nothing in fact separates the "kitten" complex of matter from any other complex of matter. Besides, when does the "food" or the "oxygen" it requires cease to become "food" and "air" and start to become "kitten" and vice versa with the waste products of the "kitten". From one perspective there are simply a series of lawful occurences which explain the events in question. There are no "natural" entities in the world just as there are no "un-natural" entities. In short, functional systems, purposive system, motivated systems --all of them rest upon a focus of attention and, in the last analysis, upon parochalism in the perspectives that we as selective (i.e., valuing) creatures entertain. They are inevitably tied up with what we are interested in and cannot be dissociated from it.[47] It is much like asking whether for a fox living in settled farm country there are any "non-natural" objects.

Such systems employ a vocabulary that is acceptable if properly explicated, but which must not carry the traditional overtones of hidden "push mechanisms" or "unique properties not found outside such systems". This suggests that there is good reason for dropping such an idiom; it adds nothing new to the discussion of systems and it is often guilty of

confusion. It does make sense to talk of the function that a stream has in the degradation of a mountain system, or the functions of degradation generally, but then all that we find ourselves affirming in this rather peculiar usage is that there is a general law stating an invariancy to exist between running water and steep gradients on the one hand, and the process of erosion on the other. Thus too, we may say that the function of the lungs is to oxygenate the blood and remove $C_0 2$. We might even say rather loosely that the purpose of the lungs is to do this. But how much less confusing it would be to say that there is a general law stating a regularity of conjunction between lungs and oxygenation-$C_0 2$ levels. In short, our explication of the entire functional or teleological conception is indicative of the redundancy of such systems at best and their confusing and metaphysical suggestiveness they confer at worst. Let us review these findings now in a more precise and detailed manner.

First, Nagel's scheme is neutral as regards the particular mechanism that will be employed in the empirical analysis of a particular system. The vocabulary may be biological, or it may be physical or social. We should expect that "S" in a biological system would be an organism or complex of organisms (biota), "E" to be the geographical and climatic conditions or the internal body chemistry, and "G" the property or state of food getting, searching behavior, maintenance of internal temperature, reproduction, and so

on. In the case of a physical system "S" would comprise, for example, a thermostatic control system, "E" the variations of temperature in a furnace or home, and "G" the property or state of maintaining constant temperature. Finally in the case of social systems or sub-systems (S) we might have an environment (E) consisting of a threatening "external proletariat" or a new system of warfare (the phalanx or the British Square), and the property of continuance (G) representing the persistence of S system, or more specifically: maintainance of political authority by effective monopoly of the means of force. In short, with regard to this first point Nagel has achieved the goal of formal analysis by providing us with a system of abstract variables into which we may "plug" any and every concrete aspect of a teleological or functional system--be that system social, biological, or physical.

Second, the system he develops is so abstractly general that it seems to suggest that it may be applied to almost any system whether or not it is usually considered teleological or functional. To this Nagel would answer a qualified yes. The distinctions that most writers have maintained are supposed to divide such systems from the rest of the physical world (the world of "mere events") appear to be so vague and imprecise that if there is something that universally differentiates between teleological and functional systems on the one hand and non-teleological systems on the other it remains

to be demonstrated. Nagel has shown what the claims of such writers can be meaningfully said to consist, if there is more to it than this it remains for the claimants to specify.

Third, it appears that from time to time what was once considered teleological or functional is re-classified into non-teleological categories. "Nature abhors a vacuum" and the doctrine that physical objects "sought their own levels" were once laws of science; it was obviously teleological to talk in this manner. We now classify such statements as simply general law formulations. The same shift has, as we suggested, occurred in biological science since Darwin. As science advances it seems to steadily eschew such usage though as we have seen there is nothing ultimately wrong with maintaining the older vocabulary as long as it is explicated.

Fourth, while the trend of science has been in the direction of law statements the system developed by Nagel does have the virtue of conceptualizing features that seem to have been on men's minds when they first suggested teleological explanations. Not every system possesses teleological features in an obvious manner, that is, not every system invites--however adequate--the use of teleological terms. And the qualified yes that Nagel gave to question #2 suggests that some do not possess them at all. Nagel notes that an oscillating pendulum may be thought of as a teleological

system even if it is seldom so considered. The harmonic oscillations of the pendulum are related to the gust of wind that moves the pendulum originally. Nevertheless, the gust of wind (force) "F" and the oscillations of the pendulum "G" are not independent of one another, as is the external temperature and the human body temperature, but instead are causally related. Yet in a more subtle sense this is also so for the homeostasis of temperature beyond a certain point; it is the relative complexity, and the point of interest that seems to produce the necessary "opaqueness" leading to the imputation of purpose--and motives--in such systems. Nagel's scheme at least has the value of formalizing the pragmatic, if not absolute, difference.

Fifth, the reason then that teleological explanations persist is as Nagel suggests, because "teleological explanations focus attention on the culminations and products of specific processes, and upon the contributions of parts of a system to its maintenance. They view the operations of things from the perspective of certain selected wholes to which the things belong; and they are therefore concerned with properties of parts of such wholes only in so far as these properties are relevant to some complex features or activities assumed as characteristic for those wholes. Non-teleological explanations, on the other hand, place chief emphasis on certain conditions under which specific processes are initiated and persist, and on the factors upon

which the continued operations of given systems are contingent. They represent the inclusive behavior of a thing as the operation of certain selected constituents into which the thing is analyzable; and they are therefore concerned with features that are related to the assumed characteristis of those constituents. The difference between teleological and non-teleological explanations, as already suggested is one of emphasis and perspective in formulation."[48]

Sixth, our final point is that the criticism that investigators focus on "wholes" or teleological systems in spite of the fact that they are now known to be without independent status, and that therefore such investigations are redundant, turns out to be a two edged sword. It is true that there is no inherent terminus in events. As Nagel suggests white blood cells are parts of the system "blood stream", of the system "human body", and of the "solar system", as well as the universe. There is no arbitrary reason that one should be judged any more "natural" or "real" than another, therefore the focus upon "wholes" or "culminations" or "ends" does seem pointless. But says Nagel this is too strong a position, for just as there are no inherent termini in nature so there exist no reason that any particular segment may not be studied. Human beings may dictate according to interest and value what will be studied and how the cake of the universe will be sliced, but this is neither true or false since values are never true or false. The laws so

formulated are true or false but the entities and interests, the subjects we choose to investigate are simply given by our particular focus of attention. Thus we may decide to study the system labeled "white oak tree" but we might as well have settled on the system "passage of water molecules x, y, x, in the water cycle". In this latter case we might have found that molecules x,y,x, passed into the root hairs of the system, which for other purposes, we choose to label "white oak tree" and that after moving up through the bole they passed out into the air through the stomata of the oak leaf. From there they formed into a cloud and rained upon the earth. Either system is legitimate, there are no "natural entities" and apparently no limit to independent variation.

And what we have said regarding teleological or adaptive systems goes equally and co-extensively for functional theory and functional analysis. Functional analysis is, as we have suggested, merely a variant of the genre. It is sometimes held that functional analysis is a theory, or at least that it is a perspective in terms of which one theorizes. It is of course nothing of the kind. As our analysis has shown it is a sort of double edged bias; human interest (culturally defined) and the purpose of investigation (also culturally defined) bid us select this and not that as a unit of study, by convention we adopt a functional vocabulary and for no other reason. Functionalism is simply another way of talking

about statements of law. It places the "functional theorist" in the strange and rather embarrassing position of promulgating a "theory" or "perspective" which states that the scientist is interested in discovering lawful regularities in society and biology. While this cannot be denied it is certainly no news. However when a functional investigation spells out specific connections of a functional nature in a society or in an organism then we have something of scientific worth to consider, not merely a statement of one of the goals of science. Example: if the biologist suggests that biological systems are functional systems or if the sociologist or anthropologist suggests that the proper theory of society and culture is a functional theory then he is merely asserting that we must seek lawful connections probably extant in such systems. This of course is the leading proviso of science, that the world is not mere chaos. To make such a statement is not to utter nonsense but is certainly programmatic in terms of an ideal that has been around for several centuries and should stir no emotion. On the other hand to state that the function of the liver is to maintain proper blood chemistry, or that the function of ideology is to legitimize power is to make a substantive claim. But this claim may as well be made in the more generalized language of scientific explanation, mainly law statements. That is, we assert a law to the effect that "if the liver is in proper working order the blood will maintain a constant property"

or "if a position of power is to be maintained there must be a legitimating ideology". Presented in these terms there is no chance of mistaking the import of the claim. There is no chance of imputing special characteristics or "entelechies" to directively-organized systems. Nevertheless if we are clear as to our claims we may employ the idiom of teleology in explaining any system, for such vocabularies are translatable, that is tantamount, to generalized conceptions explicated in the previous section on the "logic of explanation". And this is important: many disciplines especially sociology and anthropology are apparently convinced that such perspectives add something to our knowledge. As we have shown they do not. We do not wish to deplete the vocabulary of these sciences as long as they recognize the ultimate poverty of such conceptions.

One final comment remains to be made. Systems investigated by scientists such as individual organisms, species, societies, and cultures come into and go out of existence. It may seem puzzling that the teleological regularities found in such systems should be universally translatable into law statements. The reason why this may seem puzzling is that supposedly laws are universal and true, which means true here, now, and always. Does it not seem strange, then, that the regularities of laws of a society should come into existence and pass out of existence as that society comes into and passes out of being? It may seem so at first but

in fact there is no problem. Zilsel has noted that laws to be laws need only hold so long as the systems to which they apply exist.[49] Ten to the tenth power years ago astronomers tell us that there were no solid bodies in the universe, clearly then there existed no laws of solid bodies. If again the bodies that make up the universe should become gaseous there would be no laws of solid bodies. This does not mean that while in existence the laws were not genuine. It merely suggests a fact about the systems to which we apply laws. Laws, to be laws need hold only so long as the systems to which they apply exist. The situation is identical for social and biological systems.

In closing we may review our findings in Part II. We have presented an outline sketch of the foundation of modern science as elucidated by modern analysis. We first investigated the "logic of explanation". We found such a "logic" to be the basic structure of all science regardless of specific content and interest. The formulation centered around the notions of "fact", "concept", "generalization", "laws", and "theory"; and we also found that the difference between explanation and prediction was merely a pragmatic one centering around our purposes at hand. This foundation included a simplified statement of the criterion of meaning, as science, by its own precept and percept has spelled it out. To be meaningful in empirical science statements must be of lawful form capable of being tested (at least in

principle) and coherent within the corpus of scientific knowledge. Finally, in this section, we were at pains to analyze the teleological mode of explanation and the nature of functional systems. We saw that in doing this we could draw upon a number of authors all of whom fell into one of two camps: those that thought rather concretely in terms of particular systems such as organisms, and those that were primarily interested in the abstract structure of such systems independent of any particular concrete manifestation. We spelled out the implications of such analysis and found that there was nothing about such systems that could not be stated in the considerably less loaded language that we employed in our analysis of the logic of explanation.

Our final task is to apply these findings to the construal of meaning that we developed for our four types of motivational theory.

CONCLUSION

Our intension in this final section is to deduce the implications of the two sections that preceded it. We will try to show how the investigations carried out in Part II of this thesis help us to evaluate the general claims of motivational theories. In doing this we hope to clear away some of the ambiguity and vagueness that has surrounded motivational theories in the past. But in a study such as this we cannot expect to come up with final answers on questions of motivation; what we can expect is that we may emerge with a clearer idea of what is involved when an author proposes a motivational theory and just what it is that such a theory may be expected to do. If that much is accomplished then our goal will have been reached.

Taking our first class of motivational theories what can we say about them on the basis of the second part of this thesis? Using a concrete example it is clear that such a theory of global correlation as, for instance, A. L. Kroeber's theory of "labile structure" (Discussed on page 8) is not necessarily a motivational theory at all. That is, global correlations such as this are really statements of general law connecting two conceptual areas of experience: that dealing with social and cultural change and that dealing with the "play impulse". "Change" and "play impulse" are concepts as we understand them in this theory they have been tentatively linked in the manner indicated by the Hempel-

Oppenheim paradigm of scientific explanation. Such a linkage amounts to a statement of general law and need not mention motivations at all. It is however the case that the motivational vocabulary employed in discussing global correlations (as in Burke's case) is legitimate if one understands precisely what is meant by it. There is no necessary reason why the vocabulary developed in the explication of scientific explanation should triumph over that of motivational categories. One problem that it does create however is that in using motivational categories in place of law categories (when discussing such theories) the theorist offers the possibility of mistaken identity. It is often the case that the mere use of such a vocabulary is instrumental in suggesting that there is something <u>else</u>, something besides the simple conjunctions of concepts into lawful form. This something <u>else</u> is, it seems to us, responsible for much of the nonsense that surrounds motivational thinking. Speaking loosely, when we ask for the motives that compel social action we tend to think of them as inside the action pushing or driving him onward--Foote's "predispositions"--or we think of them as being external forces operating in the same manner: both types have representatives as we have seen in the theories of global correlation. But to ask such questions is often to create spurious problems. As scientists all we are legitimately interested in is <u>predicting and explaining human behavior</u>; and to predict and

explain such behavior all that is necessary is that individual instances of it be subsumed under general laws, for this is what is meant by an explanation or prediction. Instead, to lead the chase after "forces" or "internal compulsions" is to miss the point completely unless all that is maintained by the use of such terms amounts to an alternative rendering of the vocabulary we have developed. The fact that this is not always what is meant, and the fact that much of the general quest for motivational theories originates in a search for this "something else" is no better seen than in the following passage from Kenneth Burke: "It is not our purpose to import dialectical and metaphysical concerns into a subject that might otherwise be free of them. On the contrary, we hope to make clear the ways in which dialectical and metaphysical issues necessarily figure in the subject of motivation. Our speculations, as we interpret them, should show that the subject of motivations is a philosophic one, not ultimately to be solved in terms of empirical science".[50]

Such a passage as this suggests something about the nature of motivational theory in general, especially as regards theories of global correlation. It suggests that since there have been so many motivational theories, perhaps there is no theory that is really adequate. Perhaps, in fact, there is something about motivation that is ultimately enigmatic. This is suggestive simply because if we look

at the tremendous range of such theories, as Burke has done, and have seen that no single one is adequate then perhaps there **is** "something else" going on that is simply part of the "nature of man" or "motivations" and not capable of being elucidated by empirical science. And even if we do not care to go as far as Burke would go we still end up asking the question (from this point of view) "what makes the thing work?"

But from our perspective one thing is certain, questions of this sort never arise. What we set out to do is devise a body of knowledge that will enable us to explain and predict social action. We do this by means of subsuming particular social acts, and even laws governing social acts, under other laws of wider compass. The question of what makes the thing work never arises simply because having established our laws or regularities we have said all there is to say regarding such action. Such an elimination of global correlations by law statements has at least the advantage of never suggesting more than it states. We know with some precision what we mean by concepts and laws in science and they carry few ambiguous and vague overtones. The same cannot be said for the motivational theories of global correlation.

Second, in our schematization of theories of global correlation we had a category which we labeled "nexus of correlation" and which we suggested was really the request--

whenever it was present in such a theory--for an even more general correlation. Using the example drawn in the section from Freud's book Civilization and Its Discontents[51] we first notice that a global correlation was posited between basic energy and culture. The nexus of correlation in this case amounts to an even more general regularity under which the first correlation may be subsumed. It is on this point that Freud's theory really arouses interest for here he "explains" why libido and culture should be correlated at all. In this case the wider correlation is contained in the theory of repression and sublimation. Now it is often the case in theories of global correlation that more general correlates are really the key point of interest for the theorist, but we see here again that a more general correlation is merely a request for a more general law than the first. It differs in no respect from the structure of the first except that it is more general and can subsumed the former. Thus a linkage is established between frustration of energy and its expression in other areas, and from such a law it is possible to deduce the first law that there is a correlation between libido and culture.

Incidentally this latter point is important in that it manifests the distinction between generalizations and laws as developed in the "logic of explanation". We see here an exemplification of that distinction: the more general law has only instances that stand for it (Freud and others

supposedly observed many instances of frustrated libidinal energy being subliminated). But a less general law connecting libido to culture not only has instances in its favor but in addition there is the fact that it is deduced from a more general law. Such a situation is important in the confirmation of such theories for it adds additional evidence for the lower level correlation.

We see again then that there is nothing said by motivational theories of global correlation that is not said by explicating such theories in terms of the logic of explanation. We may ask therefore what our justification is in calling such theories motivational? Why attach to them a special category as though they assorted something not already asserted by a fully explicated methodological scheme?

Finally, we have said nothing about the empirical confirmation of such theories. Needless to say Freudian theory and Kroeber's theory of labile structure (for instance) are far from being confirmed by the necessary observation statements. And this is also so for other theories of global correlation. Geographic determinism, Sheldon's theory of somatic types, frontier theory, and so forth are not theories that commonly attract large scale endorsement among the knowledgable scientists of the world. This alone is perhaps enough to indicate that as theories of motivation they are suspect. But it is not and has not been our concern

in this thesis to empirically challenge motivational thought. It was felt that we can do a more satisfactory job by demonstrating certain deficiencies in the methodological apparatus, in the tyranny of words, and in the area of concept formation. It may well be that few if any of the global theories can be confirmed. That is to say that few if any of these general laws will stand the test of experience. Nevertheless if they do it only indicates that we now have a general law relating to an area which before we do not comprehend. Whether we would still want to call this a motivational theory is an open question but if we did then it should carry few if any of the connotations that it possessed in the past.

When we turn to our second class of theories, those of identification we find further reason for taking a critical attitude. In the first place we note that identities are learned. Foote cites, for instance, the game of baseball and illustrates what he means by identification by contrasting two sorts of teams: a World Series team and a spontaneously formed team that has never played together. Naturally he notices a difference and he attributes it to identification. The former team members are identified with their roles, and the latter are not, this supposedly makes the difference.

Such a position would seem to make sense until we examine what the various authors, including Foote, mean by

identification. It turns out, as we have suggested, that identities like any aspect of role behavior are acquired by learning them; in fact it turns out that to speak of identity is merely a manner of affirming the performance of role expectations with "definiteness and force". Such "definiteness and force" apparently derives from the acquisition of "expertise", which is certainly learned and not mysteriously given. But if identification is learned--by the theorist's own admission--then does it not merely constitute an aspect of the role in question? Is it not merely the case that the distinction Foote notes between the two baseball teams derives from the fact that one team has mastered its role expectations and the other has not: that the World Series team possess "definiteness and force" simply because it knows the game in detail (even to the point of knowing the idiosyncracies of the other players) and has acquired the formal and informal expectations incumbent upon its members while the other team has not? As to why one group should do this so much better than another Foote himself has given the answer when he stressed the limited alternatives available to an actor in society. Some men acquire role expertise and others do not simply because the life situations (class, status, values) lead them in one direction or another. Yet if all identification amounts to is this acquisition of informal and subtle aspects of roles then what has become of identification as a basis for a

theory of motivation? To be sure men do acquire such expertise but is anything gained by calling it a theory of motivation? After all it is merely an aspect of role, acquired and sustained as roles are acquired and sustained and there is nothing about it that "motivates" social action in any other way than ordinary role behavior.

We are again confronted with the fact that we wish to explain and predict human behavior and to do this we must subsume such behavior under general laws. Role theory--adequate or not--attempts such a subsumption and all that Foote, Strauss, Lindesmith, Becker, and Carper have added to it is to call our attention to certain informal aspects of role theory that had not apparently been given their just preposition. Foote's (et. al.) stressing of these informal role expectations may be useful and it may even be that we wish to retain for it the name "identification". But what his statements regarding identification as a basis for a theory of motivation can mean escapes us.

It would seem that overtly or not Foote and company have that "something else" in mind which we mentioned seems to lurk in the hinterland of most motivational thinking. What that "something else" is we do not pretend to know but it seems to be the sort of thing that has engendered so many of the typical motivational questions. "What makes the thing go" is its prototype and this sort of question we have seen is without foundation. Nonetheless the attentive

reader cannot help but gain the impression from a careful reading of motivational literature that some additional thing is implied beyond simple law statements. And if this is so what alternative is there for a theorist but to attempt to discover it? Half the job in science is constructing the right questions. The presumption is very strong that motivational theorists have not been overly successful at this endeavor.

Second, there is a point closely related to the first: what does it mean to explain identification itself? To ask this question is to approach from another angle the error that Foote seems to embrace. Why are there identities at all: what is their explanation? To a certain extent Foote has answered this question in his discussion of the manner in which social persons acquire new identities but we get an even better picture from the revealing article of Becker and Carper that we already mentioned in another context. The article "The Development of Identification with an Occupation" reveals in point blank form the manner in which identities are to be explained (the wider regularity under which they are to be subsumed). As we have noted this article deals with the acquisition of identity in the role of university professor. How do Becker and Carper explain it? Simply by noting the social agencies (institutions) responsible for the acquisition of appropriate role behavior (role expertise). They note that "peer groups" (other graduate

students) and "informal groups" (parties, cliques, coffee discussions) are in part responsible. The student acquires the norms and expectations from them that are requisite to the future role. Also from the apprentiship situation of the student with the professors other clues of expected and expectable behavior are learned. Finally from the formal academic structure certain over-all and generalized expectations are acquired. But this is merely the point we have been making: identification far from being a theory of motivation is a concept referring to informal aspects of roles. In many ways identification is the obverse side of discussions relating to "informal organization". The wider generalization or law that Becker and Carper subsum identification under turns out to be some form of learning theory, as manifest by the fact that all three areas in which the incipient academician acquires identity are simple learning situations. They "explain" identities by showing how they are learned which only underlines the fact that identities are aspects of roles.

What it can mean to call this position a motivational theory is then, to say the least, puzzling. At best it explains how persons acquire "definiteness and force" in role situations. If that is the theory then good enough but let us be sure that is the theory. Even the most casual reading of the literature reinforces a directly opposite impression; we wish to combat the suggestion of

"something else" that is implied in such theories, and we feel certain that if the type of analysis we have developed were generally available such theorists would be far from willing to grant that all they meant was what we have indicated. Yet if there is some additional import to their position then it is for them to develop it, as far as we can see such is not the case.

We may in fact hazard a normative suggestion. Social psychology would be in a better position to frame comprehensive theory if the motivational category were simply dropped. As in the case of theories of global correlation there seems to be far more to be gained from a depletion of vocabulary then from maintaining the present ambiguities inherent in it. It would seem more plausible to avoid all mention of motives, at least as regards theories of global correlation and identification, and employ a fully explicated system of explanatory categories such as we have developed or some variant of it. Ambiguity is at least minimal in the latter while this is not the case in the former.

Turning to our third class of motivational theories we confront a somewhat different problem. So far as we are concerned there is nothing methodologically amiss in the theory of legitimation. It is in fact the case that the term "motive" is often employed as Gerth and Mills (et. al.) employ it: to indicate how words as shorthand expressions for situations, may function in the promotion of role behavior.

One thing however must be noted, mainly that motives as here employed bear little resemblance to motives as causes ofsocial action. There is nothing wrong with stressing the legitimating nature of motives if the empirical evidence bears this out, since in all cases their authors clearly recognize them for what they are. Since they are merely symbols of symbols it may appear strange that they can aid in the legitimation of roles but that is not a point we will argue. Whether the evidence indicates an affirmative or a negative pronouncement regarding this theory is not our concern, at least not in this thesis.

The only point we wish to make is that when a theory of motivations is purposed, such as the present one we must be careful to evaluate what we are getting. The traditional connotations of motivational theory have been to suggest that if you possessed a theory of motivation then it was possible to get at the causes or push mechanisms that made men act. In this sense it was a truly general theory cross-cutting time and place. The theory of legitimation clearly can do nothing of the sort since by definition motives are merely generalized symbols standing for situations. At best they "motivate" in the manner in which language is supposed to motivate: as symbolic interaction.

And finally even this manner of speaking might better be reduced to law statements if ambiguity is going to be the price that is paid for maintaining the old motivational

vocabulary. We wonder frankly whether any of the authors espousing the legitimacy position would have bothered with it if they understood clearly what they were saying. For all they are saying is that symbols aid and in fact reinforce the process of social interaction. This may be an important law however stated but it sounds very much like old hat in sociology. Indeed the presumption is that Mill's early discussion of "situated motives"[52] might never have seen the light of day had it not been graced with the trappings of a "theory of motives". Seen in its true light the theory of legitimation hatches a mouse, and but for the traditional aura that motivational arguments have attracted it might not have seemed worth the effort.

Such a perspective incidentally suggests that the theory of identification, and that of legitimation are largely redundant. They add nothing that is not in some way accounted for in other theories except by developing a vocabulary that duplicates the meanings already extant. Perhaps it is amusing to play such word games but it hardly seems likely that it can attract serious attention once it is realized what is going on.

We turn finally to the last class of motivational theory which we investigated, theories of non-motivated purposive systems. In presenting this class of theories we allowed ourselves a few critical remarks which formed a transition to the second part of this thesis. Let us now see what

those remarks amount to.

In the first place we commented that teleological considerations form a general "perspective" of almost all motivational theorizing. What do we mean by such a statement? It is difficult to really pin down what is meant by a "perspective", but roughly what we think it indicates is that certain very general unanalyzed, often unacknowledged, categories limit the sorts of questions that an investigator puts to nature. Such a perspective existed (and perhaps still exists) in physics from the time of Newton to that of Einstein. Theorists during that period took it for granted that there existed an absolute space and an absolute time in and against which relationships among bodies could be measured. Until toward the end of the 19th Century no one really considered questioning this assumption: it acted as the almost unconscious starting point for all theory.[53]

A similar position, essentially teleological in nature, is what we have in mind when we speak of a general perspective against which theories of motivation have their being. It strikes us that the "something else" (that elusive and incohate suggestion that motivationalists make) has as its basis a teleological perspective. It is difficult to pin down the statements that exemplify such attitudes but the mere search for "motives" to begin with (as opposed to explanations and predictions) is indicative. For when an investigator makes the seminal decision that a theory of

motives is necessary he is always asking some variant of the purposive question.

However the important point for us is not so much this unanalyzed perspective as it is the actual theory that employs it. Teleology is the theory of any and all theorists that are concerned with purposes, motives, or functions that social actors entertain or perform in social systems. As a functionalist it is possible never to mention motives or purposes, but in fact it is seldom avoided. Any reference to "ends in view", "future states of affairs towards which an actor of system is orientated", or even "goals" involves its author in teleology. And insofar as such states of affairs are employed to account for why an actor acted as he did then some form of motivation is involved. Furthermore functionalism as we have seen is often itself considered a general theory of society (often called an "approach" --whatever that may be taken to mean). It should be clear that directively-organized systems such as Nagel analyzed do have characteristics that distinguish them from non-directively organized systems; however the distinction is not absolute since it rested upon the focus of attention--culturally induced--that the investigator entertains. In other words Nagel's general analysis of such systems established that teleological systems or functional systems are merely variant vocabularies describing the connectedness of our world. They are adeequate vocabularies for dealing with

a range of experience--if properly understood--but in every way they are ultimately interchangeable with the explicated and unambiguous vocabulary that has become standard form in discussing the logic of explanation. Teleology and functionalism far from being theories turn out once again--in the most charitable estimate--to be simply redundancies.

It is with some irony then that the present writer views the utterances of such a "functional" theorist as Talcott Parsons. Parsons is extremely partial to the notion of "system" by which he clearly means functional system.[54] He seems to feel that it must form one of the basic categories of his entire theory. It is unfortunate that so much effort should have been devoted to so small an idea, especially when it turns out to be simply a way of talking about one of the aims of science--the discovery of laws. Nor is Parsons alone; most functionalists who devote themselves to functional theory as such fall into the same error. In short we are confronted in much of sociology's truly general theory not with theory at all but with a hope disguised as theory.

This suggests, we trust, the extremely general penetration teleological considerations have in sociology: they often form the backdrop for motivational theories, and they penetrate the basis of functionalism. Yet we have seen from Nagel's exhaustive analysis that teleology is duplicative.

Several motivational authors--as we have seen--established connections between motives, purposes, and functions but they have not provided us with a theory of motives any more than the other investigators we examined.

It is obvious that teleological and functional analyses cannot possibly provide us with a theory of motives unless, once again, all we mean by this is that they provide us with the laws of society. And in fact they cannot even do that as general theories since we have seen that such general attempts are merely manifestos urging the non-random nature of our world. They can provide us with laws only if they concretely spell out the connections. How do we explain?, by proposing hypothese to be tested, not by proposing that we propose hypothese to be tested. The first is actual, the latter is programatic.

So again we see that theories of motives are generally ambiguous. They assert one thing by suggestion or connotation and they affirm another upon analysis and careful examination. The same redundancy that followed us through the other classes of motivation plagues us here again in the teleological and functional camp. Might we not better abandon the misleading vocabulary in question?

On the other hand we must be fair: perhaps motivational theories are saying something that has escaped us; perhaps they are asserting a meaning which we have simply missed. Certainly the literature of motivation is suggestive of some

deeper, additional meaning, and if it is legitimate it should emerge. Nevertheless try as we may we can not discover it; at best we can refer to the "something else" that seems to be in the minds of motivationalists when they framed their questions. If there is "something else" to be said then by all means they should say it. If this thesis has no other purpose it may provoke a clear statement of what is really meant. But unless such a statement is forthcoming then we are forced to the conclusion that the quest for motives among the classes we have analyzed is chimerical.

The same point may be made more specifically for teleological and functional authors. Perhaps theorists employing these systems are asserting something that escapes the author and has escaped the numerous analysts of such systems. If so it should emerge. But the work of analysis has been around for over 15 years and as yet no counterstatement has been able to suggest any additional meaning. The burden of proof rests with the positive claimant. Let him speak.

Considering our discussion in this thesis it is one of the ironies of modern sociology that the weakest link in George Herbert Mead's system of social psychology should so often be attributed to his lack of a motivational theory.[55] Why, we may conclude, did Mead have no motivational theory? The answer seems to suggest itself that he did not need one. Having laid out--to his satisfaction--the elements

of symbolic interaction, he had said everything there was to say. Having provided for the explanation and prediction of human interaction what could be the possible need of a motivational theory? Whether Mead's regularities are all or in part confirmed, the "modern" theorist will not help in the least by interjecting considerations which Mead himself rightly saw were unnecessary or in error.[56]

FOOTNOTES AND SOURCES

BIBLIOGRAPHY

FOOTNOTES

Preface and Part I

[1]They occupy a prominent place in social psychology though the reaction is not alwasy positive. See: Nelson Foote, "Identification as the Basis for a Theory of Motivation", Amer. Soc. Review, vol. 16, #1, Feb. 1951; Hans Gerth and C. Wright Mills, Character and Social Structure, Harcourt, Brace & Co., 1953; Alfred Lindesmith and Anselm Strauss, Social Psychology, The Dryden Press, (rev. ed.), 1956; George Lungburg, Foundations of Sociology, The Macmillan Co., 1939; Gardner Murphy, "Social Motivation", in Handbook of Social Psychology, Gardner Lindzey (ed.), Addison-Wesley Publishing Co., Inc., 1954; Theodore Newcomb, "Sociology and Psychology", in For a Science of Social Man, John Gillin, (edt.), The Macmillan Co., 1954; Talcott Parsons, "Psychology and Sociology", in For a Science of Social Man, John Gillin, (edt.), the Macmillan Co., 1954; Anselm Strauss, The Social Psychology of George Herbert Mead (ed.) see esp. the introductory essay, The University of Chicago Press, 1956.

[2]We do not intend to enter in this thesis into a discussion of "meaningfulness" in science. In explicating the nature of scientific explanation we will offer a few suggestions as to our position. But it is important at this point to at least state our conception. Basically our positions is that to be meaningful in empirical science a proposition must be testible, it must be capable of verification. What other types of "knowing" there may be frankly escapes us; in this thesis we associate meaningfulness with tesitibily. See: Rudolf Garnap, "Testibility and Meaning", Philosophy of Science, 3, 1936 and 4, 1937 for the basic statement of this position. It has been somewhat modified and refined in the more recent literature. See: Hans Reichenbach, "The Verifiability Theory of Meaning", Contributions to the Analysis and Systhesis of Knowledge, vol. 80, 1951 of the Proceedings of the American Academy of Arts and Sciences.

[3]Sigmund Freud, Civilization and Its Discontents, Doubleday Anchor Books, Doubleday & Co., Inc., Garden City, New York, 1958. For the position actually basic to the thesis developed in this book see "Totum and Taboo" in Basic Writings of Sigmund Freud, Modern Library Education, New York, 1938, A. A. Brill tr.

[4]Alfred L. Kroeber, Anthropology, Harvourt, Brace and Co., 1948, esp. p. 398. See also the source of much of Kroeber's thinking in Johan Huizinga, Homo Ludens, Beacon

Press, 1950. Huizinga's theory is a close parallel to that of Kroeber's. It is interesting also that Bertrand Russell should pick this theme up in Unpopular Essays where he attempts to make the point that much of history might be simply construed as a reaction to ennui.

[5]Alfred Lewis Kroeber and Jane Richardson, Three Centuries of Women's Dress Fashions, University of California Press, Berkeley, 1940.

[6]Alfred L. Kroeber, Anthropology, Harcourt Brace and Co., 1948, pp. 403-405.

[7]The "nexus of correlation" really refers to a law of greater generality than the original correlations in question. This fact is of some importance for the general theory of confirmation, especially the distinction between generalization and law developed on pages 43-44 of this thesis.

[8]Kenneth Burke, The Grammar of Motives, George Braizller, Inc., 1945, p. xvi.

[9]Ibid., p. x.

[10]E. C. Semple, The Influence of Geographic Environment, New York, 1911.

[11]Charles C. Huntington, The Geographic Basis of Society, Prentice-Hall Inc., 1933. Hungtinton's work is more sophisticated than Semple's and as Owen Latimore noted in reviewing Toynbee's Study of History became an important factor in the latter's general scheme. Toynbee has often been represented as essentially concerned with religious systems; the strong strain of motivation derived from environmental factors is however equally important.

[12]"Determinism" is a word found at two or three points in this thesis; it has earned in recent times a certain nefarious reputation. Whenever we employ the word we will mean that the area of experience it is used to modify (i.e., "geographic) is governed by lawful regularities, that it is not random. See the section in Part II of this thesis entitled "Logic of Explanation".

[13]Arnold Toynbee, A Study of History, Oxford University Press, 2 vol., 1946-1957 (abridged edition).

[14]Walter Prescott Webb, The Great Plains, Grosset and Dunlap, 1931.

[15]Vernon L. Parrington, Main Currents in American Thought, Harcourt, Brace and Co., New York, 1927-1930.

[16]Owen Lattimore, Inner Asian Frontiers of China, Capitol Publishing Co., New York, 1940.

[17]Brooks Emeny, Mainsprings of World Politics, Foreign Policy Association, 1956. Even the title of this work is interesting and instructive. The older vocabulary of motivation frequently employed some conception of the "mainsprings" of human action, just as geography is here construed. Compare, for instance, Jeremy Bentham's Table of the Springs of Action, Hermiss Publications, Los Altos, California, 1958.

[18]William Herbert Sheldon, The Varieties of Human Physique: An Introduction to Constitutional Psychology, Harper & Brothers, 1940. It is interesting to note that once the "scenic" element is gotten "inside" the human anatomy we are more inclined to grant it the status of a true motivational theory. Somehow common usage is not quite solidfied on external environments as "motivational forces", at least to the same extent as physiological, anatomical, and psychological factors seem to be. In this case the question of what is "in" and what is "out" may cause some trouble. Burke construes scenic in such a way that no distinction is made between them

[19]Karl Marx, The German Ideology, International Publishers, 1947; and especially see Capital, The Modern Library, Random House, 1906. No more fundamental picture of the "scenic" influence has been presented than the section of Capital (chapter 10), "The Working Day". The depiction of the 19th Century industrial scene is certainly here presented with an eye to its motivational force. The same comments apply to even so indirect a section of Capital as the concluding Part VIII, "The So-Called Primitive Accumulation". Here the famous thesis "...that in actual history it is notorious that conquest, enslavement, robbery, murder, briefly force, play the great part" in capital accumulation (p. 785). Such a view is in line with the scene Marx wishes to expose: proletariet enslaved 19th Century Europe. How different from Henri Pirenne's thesis regarding primitive accumulation. Pirenne paints a picture of energetic St. Godric's beach combing establishing the initial goods of the capitalist "carrying trade". Also note the concommittant role of the Viking's and north Italian merchants. How different are the societies that derive from these scenes: one is the backdrop of an enslaved Europe, the other that of a free Europe. In fact one could employ just such a

notion as "the problem of primitive capital accumulation" as an index of Burke's scenic component. Karl Polanyi's The Great Transformation, Rinehart and Co., Inc., 1944, for instance, involves a mechanism of accumulation on a vast "functional" plane. In this case the "enclosure acts" in England are not derived from a scenic background of rapacious bourgeoisie, ad with Marx, nor form energetic freebooters, as with Pirenne and to some extent Schumpeter, but instead from the "functional requirements of a whole social system". Indeed the permutations of motivationalism are dazzling.

[20]Thorstein Veblen, The Theory of Business Enterprise, Charles Scribner's Sons, Inc., 1940, pp. 302-373. Veblen relied heavily upon Marx and shares many of his "scenic components". Being more detached he apparently preferred the impersonalized idiom of "technology" to the "social relations" of production found in the "class struggle".

[21]V. Gordon Childe, Man Makes Himself, The New American Library, 1951; and What Happened in History, Penguin Books Ltd., 1954.

[22]Melville J. Herskovits, Man and His Works, Alfred A. Knoph, New York, 1948.

[23]R. J. Forbes, Man the Maker, Henry Schuman, New York, 1950.

[24]Julian Husley, Evoluation in Action, New American Library, New York, 1953. This fine little book presents the biologists final view of culture. Derived from a universe of chance in a struggle to persist culture is evolutiont's trump card, the ace in the hole for life on earth. Thus it is not derivative background for personality or social action but an instrument to be employed.

[25]Clyde Kluckhohn and Dorothea Leighton, The Navaho, Harvard University Press, Cambridge, 1946, especially pp. 182-215.

[26]Benjamin Lee Whorf, Language, Thought, and Reality, The Technology Press & John Wiley & Sons, Inc., New York, 1956.

[27]Harry Hoijer (ed.), Language in Culture, University of Chicago Press, 1954. The Kluckhohn, Whorf, and Hoijer materials taken together suggest demensions of language as agency of the human state. Jean Piaget's The Language and Thought of the Child suggests even more directly the function

of language if becoming human at all; is this sense we may speak of language as an "agency".

[28]Bertrand Russell, A History of Western Philosophy, Simon & Schuster, New York, 1945, esp. see Book Three, "Part II".

[29]Henri Bergson, Creative Evolution, The Modern Library, Random House, 1947. Bergson's "dure" in which all the past is captured in the specious present, in which nothing is ever lost, a conception very close to Whitehead's "creative advance" certainly seems to suggest a picture of the agent as locus of motivation and reality. Especially is this so when concepts of freedom and creativity are included.

[30]George Berkeley, A Treatise Concerning the Principles of Human Knowledge, the Liberal Arts Press, 1957. At first brush Berkeley may seem removed from the "agent" conception of motivation, however the train of thought that conceives of agent as motivator is present if in no other manner than as literal creator of anything at all. If existence is the construction of mind then agent is foremost.

[31]Karl Mannheim, Man and Society in an Age of Reconstruction, Harcourt, Brace and Co., 1951. This work is concerned with growing disproportions in capitalist and Western society generally. The development of "mass society" is spelled out in Mannheim's book in terms of the destruction of "elites" via the concommitant destruction of descriminating publics. It is interesting to note the similarities of this position to that of Joseph A. Schumpeter's, Capitalism, Socialism, and Democracy and to the present context. Schumpeter's and Mannheim's conceptualizations even employ the same basic language in discussing elites: "breakdown of exclusiveness of elites" (Mannheim) vs. "destruction of the protecting strata" of elites (Schumpeter). Both men, though with different emphasis, are clearly concerned with what they feel to be the latent rampages of modern society if such elites finally vanish. In other words elites seem to take the role of "agents" or motivators for the whole of Western society.

[32]Joseph A. Schumpeter, Capitalism, Socialism, and Democracy, Harper & Brothers, Publishers, (3rd ed.), 1950, esp. pp. 131-164.

[33]See: Thomas Carlyle, Heros and Hero Worship, New York, 1911. Also Bertrand Russell has commented in A History of Western Philosophy, "in all history, nothing is so surprising or so difficult to account for as the sudden rise of

civilization in Greece". And elsewhere he has made the comment that without Galileo Western science would never have developed. History must always seem of the nature of a mirical if it consists, as it does for Russell, of events waiting around for enough bright men to utilize them. As a historical explanation a great man theory employs agent as force or agency, but from the agent's perspective it is he who is in command.

[34]As fair a statement as any that does not attempt to analyze the foundations of existentialism is to be found in Jean Paul Sartre, Existentialism and Humanism, Methuen & Co., Ltd., London, 1949.

[35]See the following authors for various aspects of "pure working" as a mystical conception. Aldous Husley, The Perennial Philosophy, Harpers & Brothers, 1944; also his After Many a Summer Dies the Swan, Avon Publishing Co., Inc., 1939. Also in the latter sections of the Grammar of Motives Burke suggests that the best way to approach motivationalism is through the "divine act of creation" which should be the prototype of all acts. Notice the reasoning that leads Burke to this conclusion for it is instruc tive of many varieties of motivational thought centering on the "act". Burke notes that the act is more than simply the resultant of the other four aspects of our pentad, it reserves a "modicum" to itself. Burke comments "...if the motives properly assignable to scene, agent, agency, and purpose are already given, there could be novelty only if we could also assign motives under the heading of act itself. That is, there would be something new intrinsic to the act; and this novelty would be the modicum of motivation assignable under the heading of act rather than under the heading of the other four terms, singly or in combination. There must be brief be some respect in which the act is causa sui, a motive of itself", (p. 66). Such a respect is that Husley discusses.

Regarding these matters see Ruth Benedict, The Chrysantheum and the Sword, where she develops a conception supposedly common to Zen believers in Japan to the effect that thought and action when disciplined form a unity: "there is no break, not even the thickness of a hair between a man's will and his act", (pp. 228-252). And she comments that Zen is looked upon as "expertness" in mental discipline. The act in other words is conjoined with the will in pure form. Such conceptions do not have to make empirical sense in order to be culturally effective ("real in their consequences").

[36]Falling under this heading would be any author that employed a teleological or purposive vocabulary. Talcott Parsons, The Structure of Social Action, The Free Press, Glenco, Illinois, 1949 embraces such a conception in employing "goal orientated" or "future states of affairs towards which the action is orientated" in his system. Philosophers of various camps have been prone to the teleological idiom; most recently and heroically Alfred North Whitehead, Process and Reality, The Humanities Press, 1929. "It is notable that no biological science has been able to express itself apart from phraseology which is meaningless unless it refers to ideals proper to the organism in question", (p. 128), which is to say teleological phraseology. Whitehead goes on to apply this to a vacabulary generalized to cover the entire universe of events and thus arrives at a conception of a purposive universe striving for "intensities of satisfactions" in the concresesent prehensions that compose it, (p. 127). It is noteworthy that Talcott Parsons has acknowledged his greatest debt to philosophy to be Kant on the one hand, and the writings of Whitehead on the other. His teleological holdovers find support in his acknowledged mentors.

[37]Gardner Murphy, "Social Motivation", The Handbook of Social Psychology, Gardner Lindsey (ed.), Addison-Wesley Publishing Co., 1954, vol. 2, p. 601. Not only is this quotation of interest when we consider what sorts of questions theorists are asking themselves when they think about motivation, but it is also interesting that this major compendium of recent date should employ the same basic motivational framework as has been current for at least 75 years.

It may be worth noting that the perduring quality of motivational thinking is to be found even in so astute and socially orientated a scientists as Harry Stack Sullivan. For Sullivan nearly everything that constitutes the self is social. Yet he manages, apparently without being aware of it, to slip in a few basic drive mechanisms from whence the system flows. In Conceptions of Modern Psychiatry, W. W. Norton, Co., Inc., 1939, he suggests (p. 19) "Of the very unpleasant experiences which the infant can have we may say that there are generically two, pain and fear. Now comes the third". The third is "anxiety". These forces act as sorting or selective instruments for the self within the larger framework of personality as Sullivan conceives it. They serve the same function as the old style "defense mechanisms" in psychoanalytic theory. Gardner Murphy's "what makes the thing work" is here answered in only slightly disguised motivational vocabulary.

[38]The nature of reductionism in the social sciences and science in general is involved in controversy. Unfortunately we will have to wait for a throughal explication until a latter time. However, it is useful to make a few introductory remarks at this point. In an article on reductionism which we shall follow in Phillip Wiener's Readings in the Philosophy of Science, Ernest Nagel suggests two aspects of reductionism in science. On the one hand we may speak of reducing some proposition or regularity of a science if we subsume that regularity or proposition under a wider generalization, regularity, or proposition (for the moment we are not trying to distinguish the several sorts of entities that figure into a scientific scheme). Thus we may reduce Kepler's laws of planatary motion by deducing them from Newton's more general laws of motion and universal gravitation. In the case of sociology where there are few precise deductive techniques we still make such deductions on the basis of some form of "propositional calculus" developed in logic and employed in our "natural language" (English etc.). So if we know some well substantiated propositions relating to the problem of bureaucratic development then ideally we could deduce, that is reduce, propositions whose generality extended only so far as to cover the regularities of Chinese bureaucracy from Han times to the 19th Century. In short such reductionism is a continuous and healthy process in science as knowledge grows and singular instances take their place in broader, that is, more general contexts.

There is another meaning generally labeled "reductionism" that is not so salubrious as that we have just mentioned. It is the sort of thing that Burke rather crudely entertains when he suggests in the Grammar that: "Its vulgar variant is to be found in techniques of 'unmasking', which would make for progress and emancipation by apply materialistic terms to immaterial subjects (the pattern here being, 'X is nothing but Y', where X designates a higher value and Y a lower one, the higher value being thereby reduced to the lower one)." However this rendition of the second pattern of reduction hardly helped us to clarify the matter, though it does suggest that something is fishy about it. What is actually involved in the sort of reduction that goes on when "mind is reduced to neuronic pathways", when "The social level is reduced to the psychological level", when "biology is reduced to chemistry" may, as we have suggested, constitute an explanation of one level by another via the means of subsumption under more general laws. However such reductionist statements may be merely programatic, there being no theory which can realize such an explanation at time "t". It may be that at "t" such statements serve as directors or indicators for future research, the end of which would be to effect such reductions. On the other hand it may also be

that even if we had a theory capable of such reduction (as apparently we do with atomic theory in chemistry--see physist Eugene P. Wigner's "The Limits of Science", Readings in the Philosophy in Science, Herbert Feigl and May Brodbeck, Appleton-Century-Crofts, Inc., New York, 1953) it might still be the case that technically the derivation of one level of investigation (the biological, social, mental) from another level (the chemical, psychological, physiological) would be impossible. There seems to be a certain sense in which the various sciences that investigate our world are simply economical duplications and re-duplications of one another at each level of abstraction. And this seems to be determined (that is, it seems to be that the selection of levels is determined) by the shear un-economy of attempted reductions, even programatically. In other words while we may have no theory that, even in principle, can derive one science or level of abstraction from another we may yet entertain the hope of acquiring one. Well enough, but on the other hand it does seem likely (as we are informed in the case of quantum theory--op. cit. Wigner above) that even if such theories did exist the practical possibility of derivation might itself be nearly insurmountable, except in principle. In that case there would be strong pragmatic grounds for maintaining each level or science with its own unique vocabulary of terms, rather than attempting a reduction of one level to another. And in the case where no theory exists at all, at least no theory that is adequate even in principle, then clearly it is absurd to suggest such reduction a priori.

This appears to be the status of much of motivational thinking as we shall see. Such theories as those discussed under the class term "global correlation" are often so vague, ill-formulated, and ambiguous that it is not even clear what can be meant by them. Freudian theory, as in the case of Ernest Jones' study of Hamlet certainly does not spell out how the culture (one level of abstraction) of Denmark is "reduced", that is, explained by the Freudian scheme. It is in no way clear how the basic energy sources and id impulses can "account" for this variant of Western European culture. Merton's suggestion that we follow W. I. Thomas' dictum: "If men define situations as real, they are real in their consequences" would seem to go much further in explaining Hamlet's situation. People did believe in ghosts, and in the Devil as deceiver. Does it add anything to be informed that an oedipus situation held Hamlet in its grip when in fact we have the resources to explain the events in question at the cultural level? In short we must ask how does the basic stuff, the libidinal substrate, operate such that it can account for this, that, and the other cultural trait, and do so cross-culturally. If the theory can not accomplish this--and clearly it can not--then it will not qualify

for a useful reduction, even in principle. It may instead exhibit all of the most flagrant qualities negatively attached to the second meaning of "reductionism".

[39]Nelson Foote, "Identification as the Basis for a Theory of Motivation", Amer. Soc. Review, vol. 16, #1, Feb. 1951.

[40]Nelson Foote, and Leonard S. Cottrell, Identity and Interpersonal Competence, The University of Chicago Press, 1955.

[41]Alfred Lindesmith, and Anselm Strauss, Social Psychology, the Dryden Press, 1956.

[42]Anselm Strauss, Identification, (unpublished manuscript), cited in Ibid. above.

[43]Foote, op. cit., p. 14.

[44]Ibid.

[45]Alfred Lindesmith, and Anselm Strauss, Social Psychology, The Dryden Press, 1956, pp. 307-315.

[46]Ibid., p. 308.

[47]Ibid., p. 309.

[48]Nelson Foote, "Identification as the Basis for a Theory of Motivation", Amer. Soc. Review, vol. 16, #1, Feb. 1951, p. 15.

[49]Ibid., p. 14.

[50]Ibid., p. 16.

[51]Ibid., p. 16.

[52]Ibid., p. 17.

[53]Ibid., p. 17.

[54]Ibid., p. 18.

[55]Ibid., p. 19.

[56]By "learned" we refer to the fact that behavior is subject to alteration incident upon experience. Clearly there are all manner of learning theories to choose from,

but for our purposes it is sufficient that we merely indicate the broad nature of learning as opposed to some form of biologically fixed, generally genetic response.

[57]Foote, Ibid., pp. 19-20.

[58]Ibid., p. 19.

[59]Ibid., p. 20.

[60]Kenneth Burke, Permanence and Change, Hermes Publications, 1954, p. 31.

[61]Norman Larson, The History of Neenah, (unpublished manuscript, Neenah Public Library, Neenah, Wisconsin.

[62]Hans Gerth, and G. Wright Mills, Character and Social Structure, Harcourt, Brace and Co., New York, 1953, p. 121.

[63]Ibid., p. 122.

[64]Ibid., p. 122.

[65]Ibid., p. 117, (italics mine).

[66]Alfred Lindesmith, and Anselm Strauss, Social Psychology, The Dryden Press, 1956, p. 298ff.

[67]Howard S. Becker, and James W. Carper, "The Development of Identification with an Occupation", Amer. Jour. of Sociology, vol. 61, #4, Jan. 1956. It may seem singular that we use an article dealing with "identification" when we have only just finished our discussion of this subject. However as we shall show the import of this article suggests lines other than those developed by the authors of "identification theory". Also see: "The Elements of Identification with an Occupation", Amer. Soc. Review, vol. 21, #3, June 1956, by the same authors. And, Howard S. Becker, and Anslem Strauss, "Careers, Personality, and Adult Socialization", Amer. Jour. of Sociology, vol. 62, #3, Nov. 1956.

[68]Howard S. Becker, and James W. Carper, "The Development of Identification with an Occupation", Amer. Jour. of Sociology, vol. 61, #4, 1956, p. 296ff.

[69]Alfred Lindesmith, and Anslem Strauss, Social Psychology, The Dryden Press, 1956, p. 305.

[70]Gerth and Mills, Op. cit., p. 115.

[71]For a fair coverage of the nature of teleology and the defining characteristics of such systems generally see the fdlowing books and articles: in Readings in the Philosophy of Science, Herbert Fiegl, and May Brodbeck, (edt.), Appleton-Century-Crofts, Inc., New York, 1953-"Philosophy of Organic Life", by Moritz Schlick; "Teleological Explanation and Teleological Systems", by Ernest Nagel; and for discussion of the supposed generic properties of such systems see, in the same compendium, "The Philosophy of Science in Gestalt Theory", by Edward H. Madden. Also in Readings in Philosophical Analysis, Herbert Fiegl, and Wilfrid Sellars (edt.), Appleton-Century-Crofts, Inc., New York, 1949, see "Mechanical and Teleological Causation", by A. C. Mace; "Explanation, Mechanism, and Teleology", by C. J. Ducasse. For other analyses and characterizations of teleological systems see: Scientific Explanation, R. B. Braithwaite, Cambridge University Press, Cambridge, England, 1955, the section on variancy systems (ch. 10); also the brilliant and concise article by Warren S. McCulloch, "Mysterium Iniquitatis--of Sinful Man Aspiring into the Place of God", in Phillipp G. Frank (ed.) The Validation of Scientific Theories, The Beacon Press, Boston, 1954, 1955, 1956.

[72]Talcott Parson has as a fundamental concept of his general theory of action the notion of the "unit-act". Action is the basic category of social analysis and the "unit-act" is its subdivision. The fundamental characteristic of unit-acts is that they imply "ends", which is to say "a future state of affairs to which the action is orientated". See pp. 44 et passim in Talcott Parsons, The Structure of Social Action, The Free Press, Glenco, 1949. And, The Social System, The Free Press, Glenco, 1950. Also, "Some Comments on the State of the General Theory of Action", Amer. Soc. Review, vol. 18, #6, Dec. 1953.

[73]Robert K. Merton, Social Theory and Social Structure, The Free Press, Glenco (revd. ed.), 1957.

[74]Clyde Kluckhohn, Navaho Witchcraft, Harvard University, Peabody Museum Papers, 1944. "Myths and Rituals: A general Theory", Harvard Theological Review, #35. Also, "Covert Culture and Administrative Problems", American Anthropologist, #45, 1943.

[75]Edward Albert Shils, with Talcott Parsons, Toward a General Theory of Action, Harvard University Press, 1951.

[76]Marion Levy, The Structure of Society, Princeton University Press, Princeton, 1952.

[77]Philip Selznick, "Institutional Vulneralibility in Mass Society", Amer. Jour. of Sociology, vol. 56, #4, Jan. 1951. And, "Foundations of the Theory of Organization", Amer. Soc. Review, #13, 1948.

[78]A. P. Lerner, Essays in Economic Analysis, MacMillan, London, 1953; also "Alternative Formulations of the Theory of Interest", Economic Journal, vol. #48, June, 1938. Lerner's general Keynesian formulation and elaboration may be construed in the vocabulary of teleology even though economists have been loath to do so. In part one may conjecture that this is because of their mathematical orientation which proceeds with contingent matters in terms of the model we shall develop in Part II of this thesis. The fact that it is possible, as with so many sociologists and anthropologists, to construe the social subject matter of economics in teleological terms is materially demonstrated by Lerner.

[79]Werner S. Landecker, "Types of Integration and Their Measurement", in The Language of Social Research, The Free Press, Glenco, edt. by Paul F. Lazarsfeld, and Morris Rosenberg, 1955. Also, his obvious contribution, considering the foregoing article, to Principles of Sociology, (revd. ed.), Ronald Freedman, Amos H. Hawley, Werner S. Landecker, Gerhard E. Linski, Horace M. Miner, Henry Holt and Co., New York, 1956.

[80]Walter Cannon, The Wisdom of the Body, W. W. Norton & Co., Inc., 1939 (revd. ed.). It is interesting to note that Parson's acknowledges this great work in physiology and the theory of homeostasis as a partial source of his own social theorizing. It is equally indicative that his stress on "system" is, so to speak, derived from it empirically, rather than analytically from more explicit sources.

[81]Norbert Wiener, Arturo Rosenblueth, and Julian Bigelow, "Behavior, Purpose, and Teleology", Philosophy of Science, vol. 10, 1943; Norbert Wiener, Cybernetics, New York, 1948.

[82]See footnote 71. Also, R. B. Braithwaite, "Teleological explanation", Proc. of the Aristotelian Society, vol. 47, 1947; G. Sommerhoff, Analytical Biology, London, 1950; W. Ross Ashby, Design for a Brain, New York, 1952.

[83]Our demonstration of this proposition will take place in the discussion of teleological systems in Part II. However, it rests upon the literature referred to in footnotes #71 and #82 and especially on an article by Ernest Nagel: "A Formalization of Functionalism", in Logic Without Metaphysics, The Free Press, Glenco, 1956. This article is

especially noteworthy in that while our analysis of teleological systems implicitly suggests our conclusion regarding functionalism it does not explicitly develop this position. This article however does; and it is well worth referring to when considering any question of functionalism in social or biological science.

[84]George Lundberg, Foundations of Sociology, The Macmillan Co., 1939. Also the article setting forth the general perspective, "The Natural Science Trend in Sociology", Amer. Jour. of Sociology, vol. 58, #3, Nov. 1952. It is interesting that a general perspective is more at stake in Lundberg's writings than any particular question of the theory of motivation. Given one perspective, and questions of motivation never arise, given another and they arise all the time. And yet this should not be taken to mean that all knowledge is relative or that we must arbitrarily select a position and then engage in scientific investigation in terms of that position. There are procedures that avoid these pitfalls by becoming cognizant of them and making explicit their grounds. But until recently the techniques and the published accomplishment were not generally available. In such a situation Lundberg starting from a strong--perhaps too strong--behaviorist position finds no need for motivational thinking; while many social psychologists and sociologists commencing from quite different positions can think in no other terms. Our feeling is that had the latter been as chary of their presuppositions as they might have been we would not have had to engage in the present corrective undertaking. The burden of Part II and the Conclusion is to demonstrate this.

PART II AND CONCLUSION

[1]John Dewey, and A. F. Bentley, Knowing and the Known, Oxford Press, New York, 1945.

[2]So that the reader may gain a more adequate impression of the basis of "the logic of explanation"without undue digression in the text itself, the footnotes have been made rather extensive. In them will be found what amounts to an annotated bibliography of the concerns at hand. Naturally certain key works stand out as pre-eminently valuable. In general these works are the most recent, a fact which reflects the accumulative nature of the field and the growing precision of its product.

[3]Richard Bevan Braithwaite, Scientific Explanation: A Study of the Function of Theory, Probability and Law in Science, Cambridge, At the University Press, 1955. See also the comments and difficulties envisioned by Carl G. Hempel and Paul Oppenheim in their study, "The Logic of Explanation", Readings in the Philosophy of Science, Appleton-Crofts-Century, New York, 1953, esp. pp. 331-343. Also for various related aspects see, Philipp G. Frank, The Validation of Scientific Theories, The Deacon Press, Boston, 1954, 1955, 1956. For an unacceptable but at the same time very instructive distinction between law and theory see the popular What Is Science, by Norman Campbell, Dover Publications, Inc., New York, 1952, first pub. 1921; and his technical Physics: The Elements, Cambridge University Press, 1920, pp. 120-140, "The Structure of Theories".

[4]Henry S. Leonard, Principles of Right Reason, Henry Holt and Co., Inc., New York, 1957, esp. Part III and Part IV "The Theory of Terms" and "The Theory of Definition". While there may be some objection to the formulation presented in Leonard's book regarding the "existance" of "characteristics" the formulation is quite adequate for our purposes.

[5]Ibid., pp. 190-208.

[6]The phrase "constant conjunction" is borrowed from Morris R. Cohen, and Ernest Nagel, An Introduction to Logic and Scientific Method, Harcourt, Brace and Co., New York, 1934, p. 246 and forms one of the sub-types of "invariant relations" that Cohen and Nagel develop. This book remains a classic in scientific method and is still widely read and cited in sociological literature. It is however, like most scientific classics, badly dated and in some places now considered to be in outright error.

[7]Henry S. Leonard, op. cit., pp. 44-55.

[8]Carl G. Hempel, "Fundamentals of Concept Formation in Empirical Science", Inter-national Encyclopedia of Unified Science, Volumes I and II: Foundations of the Unity of Science, Vol. II, #7, The University of Chicago Press, Chicago, 1952, pp. 52-54. "The rational core of the distinction between natural and artificial classifications is suggested by the consideration that in so-called natural classification the determining characteristics are associated, universally or in a high percentage of all cases, with other characteristics, of which they are logically independent", (p. 53).

[9]Reference to "what we are interested in" or "what we want to know" should not be construed as positioning a residual category for explaining enigmatic aspects of human

behavior. Obviously studies in the sociology of knowledge suggest how we are to explain such phrases as "what we are interested in" or "what we want to know". We are not suggesting that there is some faculty innate in the human being--such as Veblen's "idle curosity". It should be obvious from ideological, economic, and social studies in the history and development of science that it is no longer necessary to posit residual categories in explaining the focus of interest that men have at various times entertained. See, for example, Robert K. Merton, "Science and Economy of 17th Century England", in Social Theory and Social Structure, The Free Press, Glenco, 1949.

[10]Richard Bevan Braithwaite, Scientific Explanation, At the University Press, Cambridge, 1955, pp. 300-303.

[11]Ibid., p. 9.

[12]Ibid., pp. 293-319.

[13]Eugene P. Wigner, "The Limits of Science", in Readings in Philosophical Analysis, Herbert Feigl and May Brodbeck (edt.), Appleton-Century-Crofts, Inc., 1953.

[14]Carl G. Hempel, "The Function of General Laws in History", in Readings in Philosophical Analysis, Herbert Feigl and Wilfrid Sellars, (edt.), Appleton-Century-Crofts, New York, 1949, p. 460.

[15]Ibid., p. 459. This excellent article states in clearer form than nearly any other brief presentation available the basic tenets of explanation in science, natural or social.

[16]Carl G. Hempel, and Paul Oppenhim, "The Logic of Explanation", Readings in the Philosophy of Science, Herbert Fiegl, and May Brodbeck, Appleton-Crofts-Century, New York 1953.

[17]Ibid., pp. 342-343 footnote. Notice the similarity of the Hempel-Oppenheim position to the English school in this summary statement by R. B. Braithwaite: "Any incorporation of a fact--be it a particular instance of a law or the law itself--into a deductive system in which it appears as a conclusion from other known laws is, by virture of that incorporation, an explanation of that fact or law....What matters is that we know more than we did before of the connectedness of the fact or law with more fundamental laws covering a wider range. We have not only attained more knowledge of the inter-connectedness of Nature, but we have also acquired the possibility of a power of making predictions that was not open to us before." In, Scientific Explanation, p. 349.

[18]Carl G. Hempel, "The Function of General Laws in History", Readings in Philosophical Analysis, pp. 462-3.

[19]Karl R. Popper, The Open Society and Its Enemies, Princeton University Press, Princeton, 1950, pp. 443-463.--"Has History any Meaning". This position is incidently identical to that of the Hempel-Oppenheim theory of explanation and constitutes one more illustration of the ubiquitous nature of the position we are trying to characterize.

[20]J. W. N. Watkins, "Ideal Types and Historical Explanation", Readings in the Philosophy of Science, Herbert Fiegl, and May Brodbeck, pp. 723-724.

[21]Bertrand Russell, "On the Notion of Cause, with Applications to the Free-Will Problem", Readings in the Philosophy of Science, pp. 397-3.

[22]Eugene P. Wigner, "The Limits of Science", Readings in the Philosophy of Science, pp. 762-764.

[23]Carl G. Hempel, "The Logic of Explanation", Readings in the Philosophy of Science, Herbert Fiegl, May Brodbeck. Also see: Albert Einstein, and Leopold Infield, The Evolution of Physics, Simon and Schuster, 1938, esp. Part I and III; R. E. Peierls, The Laws of Nature, Charles Scribner's Sons, New York, 1956, esp. ch. 1 and 6.

[24]Theodosium Dobzhansky, Evolution, Genetics, and Man, John Wiley & Sons, Inc., New York, 1955, ch. 2, 4, and 14.

[25]George Gaylord Simpson, Colin S. Pittendrigh, and Lewis R. Tiffany, Life, Harcourt, Brace and Co., New York, 1957, ch. 12, 13, 16, 17, and 18.

[26]Ibid., ch. 14.

[27]Emile Durkheim, Suicide, The Free Press, Glenco, 1951, tr. John A Spaulding, and George Simpson.

[28]Sidney, Schoeffler, The Failures of Economics: A Diagnostic Study, Harvard University Press, Cambridge, 1955. The reader is urged to consult this work for the point in question and general criticism of it; but more important is the fact that Schoeffler's book is an attempt to do for the whole of economics--and the findings have social science significance generally--what we are attempting for only a small sector of social psychological theorizing. Schoeffler's work draws on much of the same critical and analytic material as the present thesis utilizes; it also suggests how the implications of such work extend to much wider fields.

[29]Hans Gerth, and C. Wright Mills, Character and Social Structure is one of the few attempts to relate the two areas explicitly.

[30]See footnote #38 for an introductory statement.

[31]B. F. Skinner, "Critique of Psychoanalytic Concepts", The Validation of Scientific Theories, Philipp G. Frank, (edt.), The Beacon Press, Boston, 1954, 1955, 1956.

[32]Alfred North Whitehead, Science and the Modern World, The New American Library, New York, 1925, pp. 19-20.

[33]Richard Bevan Braithwaite, Scientific Explanation, p. 300-303.

[34]Walter Cannon, The Wisdom of the Body, W. W. Norton, New York, 1939 (revd. ed.).

[35]Arturo Rosenblueth, Norbert Wiener, Julian Bigelow, "Behavior, Purpose, and Teleology", Philosophy of Science, vol. 10, 1943; Norbert Wiener, Cybernetics, MIT Press, 1948.

[36]Norbert Wiener, The Human Use of Human Beings, Doubleday & Co., Inc., 1954, see esp. the Preface: "The Idea of a Contingent Universe".

[37]Jacob Bronowski, "Science as Foresight", in What Is Science, Simon and Schuster, New York, 1955.

[38]G. Sommerhoff, Analytical Biology, London, 1950.

[39]W. Ross Ashby, Design for a Brain, New York, 1952.

[40]Richard Bevan Braithwaite, Scientific Explanation, pp. 319-340.

[41]Ernest Nagel, "Teleological Explanation and Teleological Systems" in Readings in Philosophical Analysis: "A Formalization of Functionalism" in Logic Without Metaphysics, The Free Press, Glenco, 1956.

[42]Ernest Nagel, "Teleological Explanation and Teleological Systems", Readings in Philosophical Analysis, p. 547.

[43]Ibid., p. 548.

[44]Ibid., p. 549.

[45]Warren S. McCulloch, "Mysterium Iniquitatic--Of Sinful Man Aspiring Into The Place of God", in The Validation of Scientific Theories, Philipp G. Frank, (edt.), The Beacon Press, Boston, 1954, 1955, 1956.

[46]Nagel, op. cit., p. 550.

[47]For the nature of "valuing" see footnote #93.

[48]Nagel, op. cit., p. 553.

[49]Edgar Zilsel, "Physics and the Problem of Historico-sociological Laws" in Readings in the Philosophy of Science, Herbert Fiegl, and May Brodbeck, Appleton-Crofts-Century, New York, 1953, p. 720.

[50]Kenneth Burke, The Grammar of Motives, George Braziller Inc., New York, 1955, p. xvi.

[51]Sigmund Freud, Civilization and Its Discontents, Doubleday & Co., Inc., Garden City, New York, 1958.

[52]C. Wright Hills, "Situated Action and Vocabularies of Motive", American Sociological Review, Oct. 1940.

[53]Albert Einstein, and Leopold Infield, The Evolution of Physics, Simon and Schuster, New York, 1938, ch. 1, 2.

[54]Talcott Parsons, "Some Comments on the State of the General Theory of Action", Amer. Soc. Review, vol. 18, #6, Dec. 1953.

[55]George Herbert Mead, Mind, Self, and Society, The University of Chicago Press, Chicago, 1934. It is instrutive that Mead should have referred to himself as a "social behaviorist". This we recall is essentially Lundberg's perspective and in like manner he eschews motivational theories. There is nothing in Mead's writings so far as we can detect that would have led Mead in the direction of motivational theorizing. For his system starting with the perspective he did such questions never arose. It is doubly ironic that later students such as Strauss should feel the need to revise what careful consideration shows to be satisfactory. One begins to wonder how many times the cycle of formulation and revision must go on before someone settles the matter.

[56]Anselm Strauss, (edt.) The Social Psychology of George Herbert Mead, University of Chicago Press, Chicago, 1956. Strauss states that Mead's "...symbolic approach prepares

the way for a sociologically orientated motivational theory akin to that developed by Kenneth Brake and C. Wright Mills", (p. xv).

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