THEORY IN TALCOTT PARSONS' SOCIOLOGICAL WRITINGS

(AN EXPOSITION AND CRITIQUE OF TALCOTT PARSONS' CONCEPTION OF SCIENTIFIC THEORY IN HIS METATHEORETICAL WRITINGS AND HIS SUBSTANTIVE SOCIOLOGICAL THEORY IN THE LIGHT OF THE CRITERIA OF HYPOTHETICO-DEDUCTIVE THEORY)

by

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Date **January 6, 1968**
All criticism is the inverse side of a positive affirmation.

Edmund Husserl

It is much easier to point out the faults and errors in the work of a great mind than to give a distinct and full exposition of its value.

Arthur Schopenhauer

For to be possessed of a vigorous mind is not enough; the prime requisite is rightly to apply it. The greatest minds, as they are capable of the highest excellencies, are open likewise to the greatest aberrations; and those who travel very slowly may yet make far greater progress, provided they keep always to the straight road, than those who, while they run, forsake it.

Rene Descartes

The man who makes his entry by leaning against an infirm door gets an unjustified reputation for violence. Something is to be attributed to the poor state of the door.

John K. Galbraith

The value of the cargo does not compensate for a ship's being out of trim ...

Thomas Henry Huxley

Thanks be to God that all that is needed is not difficult, and all that is difficult is not needed.

Grigory Skovoroda

The tree of life is always green, and all theory is grey.

Milovan Djilas
Talcott Parsons, generally acclaimed to be one of the paramount sociological theorists at present, frequently asserts that he is seeking to develop an abstract scientific theory of social phenomena. There is, however, a corpus of notions, principles, and stratagems regarding scientific theory called the hypothetico-deductive approach. It is generally recognized as perhaps the most logically tenable and empirically revealing approach as regards the formulation of significant statements about the world. The problem, therefore, in this study centers on the question: How do Talcott Parsons' conception of the nature and functions of scientific theory (his metatheory) and the basic structure of his substantive theory stand if they are compared with the various aspects of a hypothetico-deductive approach. To accomplish this, several lines of attack are initially pursued. Parsons' early intellectual influences and contacts are roughly traced out in an attempt to illumine the connections between Parsons and certain theorists who imparted a definite direction to Parsons' theoretical thinking. It is discovered that, the influences on Parsons were wide, diverse and, on the whole of a European speculative approach toward social theory. A discussion of the nature and functions of scientific theory and explanation in terms of a hypothetico-deductive approach is, then, carried out. Following this, an extensive elaboration and criticism of Parsons' conception of scientific theory, as revealed in a number of published essays and volumes, particularly *The Structure of Social Action*, is undertaken. It is
found that many of Parsons' views, in this respect, are vague and logically untenable, with an extraordinary emphasis on concept development rather than the formulation of general statements (or laws) which form the heart of all scientific theory. An examination of the basic structure of Parsons' substantive theory reveals that it is structurally modelled in the light of what he conceives scientific theory to be and, consequently, is found to be decidedly lacking in a logical sense, thereby impairing its empirical applicability. It is concluded that Parsons' conception of scientific theory is inadequate and that his substantive theory is not theory if viewed from a hypothetico-deductive perspective. It is suggested, moreover, that Parsons has provided orientations in his substantive work rather than theory.
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CHAPTER ONE

TALCOTT PARSONS' NICHE IN CONTEMPORARY SOCIOLOGICAL THOUGHT: INTRODUCTION

The central purpose of this study is to examine critically one aspect of one man's prodigious intellectual contributions in the discipline of sociology.

A discussion of virtually any facet of Talcott Parsons' vast and diversified writings on topics of sociological import inevitably provides an ample opportunity for a consideration of an enormous complex of inter-connected issues concerning the nature and uses of scientific theory in general and, doubtless, sociological theory in particular. For, there surely can be little question that the paramount theorist in contemporary American sociology, not to mention the entire domain of contemporary sociology, is Talcott Parsons. (1) He is, first and foremost, a theorist in the strictest sense of the word; almost all his writings move in the empyrean of abstractions. A superficial glance at his published work clearly reveals that he has done little else but theorize. Talcott Parsons' ascendance as a notable sociological theorist was first established in 1937 with the publication of the prolix, highly abstract volume entitled The Structure of Social Action. Generally, it embodies his initial formulation of the theory of action along with a
penetrating exposition and critique of social theories propounded by four prominent European theorists, namely, Alfred L. Marshall, Vilfredo Pareto, Emile Durkheim, and Max Weber. It is relevant to mention here, moreover, that oeuvre represents a culmination of his intensive studies of European social theorists, particularly with regard to problems of pointing out the social elements of economic theories, a project which largely occupied his analytical interests for the first decade of his intellectual career. Parsons' subsequent work has been characterized by a prolific output of journal articles and books. A very large portion of these writings deal with issues of broad theoretical import for sociology combined with an appreciable number of interpretational analyses of specific empirical problems and events, as often as not, viewed in the light of the conceptualizations he has devised in his theoretical work. (2)

In essential respects, Parsons' principal intellectual objective has been, and still is, the construction of a comprehensive, abstract theoretical scheme of action in terms of a structural-functional approach. Briefly, by action Parsons means that human behavior is oriented toward the realization of goals in certain situations by means of a system of norms and values usually embodied in roles or in institutions which are constituted of an aggregation of certain roles. The structural-functional approach explicitly pertains to a
characteristic of theory which permits the arrangement of various constituent elements, let us say for example, roles, or institutions in such a manner that the effect of one or more elements on another collection of elements or the entire system in which the elements are situated can be analyzed and determined. With this type of theoretical scheme an immense range of social phenomena which, according to Parsons, has been fragmentarily treated by sociology and other social and behavioral sciences, most notably, economics, political science, and in part, psychology, will be embraced, categorized, and then adequately analyzed. (3) To the eventual realization of this ambitious vision, Parsons has unswervingly and relentlessly dedicated his considerable intellectual energies. What Parsons has thus far produced in the light of these objectives is by no means minimal. However, a caveat is in order at this point. Determining and evaluating the scientific significance of these contributions is altogether another question. And this is an issue, among much else, I will attempt to come to grips with in this study. But of this more later.

Curiously enough, Parsons' intensive theoretical concerns occur at a juncture in the history of social thought when the majority of sociologists, in North America at any rate, have immersed themselves in disciplinal problems of such a low level of abstraction that immediate recourse to empirical research is frequently necessitated to proceed anywhere on what would be
considered significant lines of enquiry. Without a shadow of a doubt, theoretical concerns appear to be regarded of ancillary importance and almost deprecatingly shifted to the realm of metaphysics, an anathema to science. (4) In a sense, then, Parsons' preoccupation with general theory has unquestionably rendered him an anomaly of sorts in the domain of contemporary American sociology, swimming against the currents of conventional sociological thought and practice. Yet, paradoxically, I should think, it is his adamant insistence to pursue his theoretical tasks in this anti-theoretical intellectual milieu (5) that results in Parsons' endeavors assuming a measure of significance and commanding the scrupulous attentions of a large number of contemporary and, if I may say, aspiring sociologists. It is significant that Parsons - rather unconsciously it appears - appositely epitomizes his own general theoretical orientation in a telling journal comment on training methods in sociology at American universities; he states this.

In the present American intellectual climate, there is likely to be no lack of incentive to press forward with rigorous methods in sociology. It seems more urgent to defend opportunities for imaginative ventures into the unknown, regardless of immediate promise of rigor. Openness to these opportunities has, as much as anything else, distinguished sociology among the social sciences. Finally, many non-mathematical methods such as those used in handling historical and comparative data can be highly rigorous. (6)

This, undoubtedly, is the voice of a bold, adventurous theorist, whose forays into the "sociological unknown" are legion to
social scientists. For Parsons, the importance of an enlarge-
ment of the horizon of social scientific knowledge inestimably
outflanks the acquisition of precise concrete snippets of
knowledge about social phenomena.

What has given Parsons the stature he now holds among
social scientists is perhaps the general conviction that he,
much more likely than any other theorist in the past or the
present, furnishes fruitful guidelines for future theorizing
and empirical research in the sociological realm. This
springs from the recognition of Parsons' long-standing and
distinguished contributions to the history of social thought
in the form of numerous expositions and critiques of the work
of earlier social theorists, (7) combined with his unwavering
attempts to set the development of sociological theory in
another, hopefully more scientific, direction, away from the
rarefied heights of speculative social theories and also from
the narrow concerns of those who emphasize only concrete
empirical research. In other words, it is assumed Parsons
possesses a formidable intellectual background in the history
of social thought and a more than adequate technical competence
in the analysis and application of the intricacies of theoret-
cical discourse. Interestingly, this confidence in Parsons has
been bestowed by many sociologists of differing theoretical
orientations. Another factor accounting for Parsons' reputation
as an eminent sociological theorist is that an appreciable
number of sociologists have found considerable favor with Parsons' substantive theory — and presumably, his conception of scientific theory as such — chiefly on account of its plausible argument and its supposedly perceivable congruence with empirical reality.

In no wise is Talcott Parsons' reputation as a significant theorist in sociology unanimously acknowledged. There are several sociologists who do not consider Parsons to be a theorist in any scientifically respectable sense, he proffers social interpretations but not scientific theories. However, the number who maintain this unflattering stance is admittedly low. (8) The fundamental thesis for this aggregate of sociologists is that Talcott Parsons' sociological theory is exceedingly ambiguous in its present formulations and from what can be understood, it is bereft of any discernible structure such that it could be identified as a theory which provides explanations of certain social phenomena. It may be pointed out that an intriguing corollary flows from this argument. If Parsons' theoretical efforts in large part infringe the canons of scientific endeavor, and in spite of this, he is still regarded as an important theorist by other sociologists, it could be argued with justification that the majority of sociologists are generally unsophisticated, nay lamentably lacking, in knowledge of genuine scientific procedure, particularly in the crucial area of theory-construction.
Thus, they would appear to confound obscurity with profundity, intentions with results, insight with discovery, and scientific theory with vague abstract discussion. Whether this type of situation prevails in addition to the other factors just considered is, of course, a matter for conjecture. Much is contingent on how scientific theory is conceptualized. For the moment let us note this observation and return to it later in this study.

Unquestionably, Parsons' theoretical formulations have inspired and facilitated to a very large degree numerous analyses of particular social phenomena, and to be sure, cognate issues in methodology, philosophy and history. (9) At the same time, moreover, his theoretical orientations have indeed been instrumental in the generation of several varying—though connected in substance— theoretical schemes primarily of a structural-functional nature. It is of interest to note that many of the theorists who have adopted a structural-functional approach were formerly Parsons' students at Harvard University. In this connection, there are three noteworthy theorists who readily come to mind, namely, Robert K. Merton, (10) Marion J. Levy, Jr., (11) and Kingsley Davis (12). At base, they all have employed Parsons' fundamental notion of society as a system in which the integral components have functional or dysfunctional implications for each other and also the system as a whole. Where these theorists differ from
Parsons is their emphasis on factors in the theoretical framework which they opine require extensive elaboration or, as in the case of Merton, newer notions and perspectives are introduced to elucidate features of the social phenomena with which the theory is concerned and to further clarify the concept of scientific theory in order that it may be relevant to, and conveniently flexible for social scientists to develop and use theory in their intellectual labors.

Doubtless one of the most singular features of Parsons' theoretical work is his convoluted and labored style of presenting it. For this — more than anything else — he has been subjected to persistent, often captious, criticism and even obloquy. (13) The type of language Parsons employs, it is variously and frequently said, is on most occasions, impenetrable, vacuous, and destitute of any observance of the conventions of clear composition and unencumbered grammatical usage. Subordinate clauses and overly-fine qualifications are cascaded one atop another such that the comprehension of a trend of thought becomes indeed an onerous task, oftentimes impossible. Words and concepts change meaning without prior notice in crucially important passages in the development of his arguments; interestingly, one critic who became exasperated at the evanescent character of a number of Parsons' concepts, was impelled to call them rather petulantly, "chameleon concepts". (14) On other occasions, important definitions are
completely omitted. Classifications are entwined in a com-
plicated fashion to form paradigms which frequently do not 
admit of clear comprehension. Though less important for the 
evaluation of any argument, neologisms appear in alarming 
profusion. Of noteworthy significance is the fact that one 
writer has gone to great lengths to convey the unmistakable 
impression that Parsons is guilty of plagiarism of ideas in 
his theoretical formulations. (15) Clearly, the outright 
allegation or even the tacit supposition of plagiarism cannot 
be flippantly dismissed, for, it constitutes the mortal sin 
of scholarship which, apart from putting into serious question 
the scholar's integrity, generally affects the reception of 
his future intellectual contributions in his field. However, 
in this study, I shall deliberately overlook this delicate 
issue for the fundamental reason that accusations of plagiarism 
and such-like are exceedingly difficult to substantiate in the 
discipline of sociology where a considerable number of similar 
ideas and notions have been in constant circulation for a 
lengthy period of time. Thus, to ascribe plagiarism of ideas 
to someone without definitive evidence other than the dubious 
procedure of citation and remarking on the superficial 
resemblances of certain arguments and ideas, is somewhat 
fragile, if not foolhardy. In any case, however, these 
stylistic and procedural deficiencies, if such they can be 
termed, have no doubt adversely affected a number of social
scientists who have endeavored to employ Parsons' theoretical orientations and insights in their own investigations or to examine his theoretical contributions in a sympathetic but critical light.

In my estimation, the following remarks by a sociologist of note serve as a vivid illustration of the tendencies I have just considered.

The problem of arriving at a reasoned assessment of Parsons' thought is greatly complicated by a remarkable obscurity of structure and style. Even those accustomed to abstract philosophical discussion find it a considerable chore to decide what is being said on any page, let alone also to assess its intellectual worth. I suspect that a great many sociologists, otherwise sympathetic to the need for general theory, have simply abandoned the effort. (16)

I should like to add one brief observation regarding the use of language in scholarly works which I fear escapes the notice of many. Admittedly, the employment of an unencumbered style and lucid expression in the presentation of an argument is highly desirable and should be constantly enjoined and practised. However, in the assessment of an individual's argument, even Parsons', it should always be taken into account that the quality of the language employed is perhaps one of the least important aspects to be put to serious criticism. The substance of the argument, in my view, constitutes the pre-eminent concern when a theoretical contribution is undergoing examination. It is pertinent to note,
moreover, that understanding is relative to the respective competences of the individuals involved in critical endeavors. (17) In these remarks, I do not wish to be construed as one who countenances obscurantism. What I am doing here, however, is fixing a focus on issues - primarily the factors of ideas and the arguments for their validity and reliability - which, to be sure, are of infinitely more importance than an individual's literary manner, which before all is a matter of taste. (18)

Stylistic matters notwithstanding, the sheer quantity and diversity of his intellectual labors renders Parsons an almost insuperably difficult theorist to examine systematically. His theory of action or as he sometimes calls it, "the action frame of reference" (19) is continuously undergoing revision, excision, and addition. What is more, Parsons borrows notions from various schools of psychology, (20) particularly Freudian psychology, (21) which he considers appropriate to his manifold purposes; stresses the similarities of his theory with theories in physics (22) and the biological sciences; (23) maintains that various features of his theory resemble certain principles inhering in Euclidian geometry; (24) and employs his theory in the analysis of both social change and social structure. (25) As well, in recent publications, he has even introduced the notions of "cybernetics" (26) and "evolution" (27) into his analyses of social
behavior. Suffice it to say, all of this becomes at times inordinately confusing to understand and to evaluate critically or sympathetically. At first blush, instead of a theory, or for that matter, anything resembling a theory in the conventional sense of the term, Parsons' formulations appear to be tantamount to a free-floating, amorphous mass of disparate ideas, verging on indiscriminate eclecticism, a veritable pastiche. But, for Parsons, this clearly is not the case either in his intent or in his writings. He has reiterated on numerous occasions that his explicit objectives are theoretical and that his works in large part are successive contributions in this, admittedly, most difficult quest. (28) A careful scrutiny of his heterogeneous writings, I think, undeniably indicates that there is discernible a fairly constant core of considerations which can be viewed as the rudiments of his sociological theory. It is for this reason that I refer to Parsons' diverse theoretical formulations as "theory" rather than "theories" as other critics and expositors happen to do. (29)

All these allusions to Parsons' stylistic obscurities and vast theoretical sweep suggest a curious, yet pertinent, parallel with the productions of another distinguished intellectual. Parsons' stature in the realm of sociology is very much similar to that of Georg F. Hegel in the philosophical domain. They are widely regarded by individuals involved
in these disciplines as thinkers of incomparable profundity and distinction in their respective fields. Their literary styles are doubtless complex and distended, a source of tormenting problems for their advocates and critics. Their respective scholarly outputs are, to say the least, voluminous and diversified. And their basic orientations toward problems in their respective fields are ambitious and largely unfettered by prevailing disciplinal conventions; that is to say, they tackle problems and their diverse implications in one grand comprehensive stroke, oblivious of minor inaccuracies and inconsistencies. To be sure, there are differences. Hegel dealt with issues of philosophical import over a century ago, while Parsons, on the other hand, seeks to develop scientific theory in the context of contemporary sociology.

These general, albeit necessary preliminary considerations of Parsons' works have taken us to the very heart of the main objectives which I intend to pursue in this inquiry. Above all, what I wish to undertake here is an exposition and critical examination of Talcott Parsons' notions of the structure and uses of scientific theory as such which are found scattered throughout his various writings and an exposition and a critical analysis of the type of theory he in actual fact employs in what we can term his substantive theory which has been presented in several books and qualified in a number of journal articles. Basically, several crucial questions are being posed. Apart from his substantive formulations of sociological theory, what
are Talcott Parsons' conceptions of the structure and uses of scientific theory? Perhaps this question can be phrased somewhat differently; What is Talcott Parsons' philosophy of science? What is Talcott Parsons' metatheory, that is, his philosophical theory of scientific theory? Another question, distinctively separated from the first set that was posed, is as follows: In the light of Parsons' avowed intentions to devise scientific theory in the discipline of sociology, how scientific is Talcott Parsons' Substantive Theory? Lastly, it may be inquired: Does Talcott Parsons' conception of scientific theory and his substantive sociological theory in any way conform to the elementary canons of genuine scientific theory as conceived by a majority of contemporary philosophers of science and, it should be noted, a minority of social scientists? Clearly, even to think of these questions, let alone utilize them as essential points of departure in an extended analysis of a theorist's work presupposes a conception of what constitutes the most useful type of scientific theory to be employed as a touchstone. For lack of a better designation I shall call it hypothetico-deductive theory, an approach which is employed in virtually all of the natural sciences with overwhelmingly favorable results, but more importantly, prescribes standards for consistent thinking and formalizes that which is implicit, but ineluctable, in human thinking. (30) In addition to the already mentioned objectives, I shall concentrate a portion of my critical discussion on the
question of \textit{strategy} in theory-construction in sociology and critically examine Talcott Parsons' theory-building strategy which, I think, if viewed from the standpoint of hypothetico-deductive theory, is unfruitful and fraught with fundamental misconceptions and deficiencies primarily of a logical nature. By \textit{strategy}, (31) I mean the choice of alternative ways of constructing a theory, the manner of going about developing a theory. At bottom, strategy is an approach determined by an individual's predilections for undertaking a course of action in a certain manner which is based upon a hunch, or if one permits, intuition, of the effectiveness of the course of action in achieving his stated objectives. Thus, strategy in theory-building, where the objective often is abstract, comprehensive theory, may be characterized by a variety of approaches; to furnish several examples, one may begin his theorizations with exceedingly abstract statements and proceed to interrelate them with each other and test their empirical significance; conversely, one may begin with concrete statements and perhaps progressively render abstract empirical relationships he has uncovered or one may begin with abstract concepts, or ideal types, and then endeavor to incorporate them into relational statements of universal character. From all this, it is evident that strategy is extra-logical, an area of concern where trial and error and experience in scientific theorizing is paramount. Nonetheless, its importance cannot be greatly overstressed for its multiple implications are either adjuvant
or obstructive for further theoretical endeavor. It is my opinion that this factor of strategy in theory-construction has received far too little attention in discussions on sociological theory, an area in which genuine scientific theorizing, interestingly enough, is still anchored on an extremely primitive plane of abstraction (or universality, to put it more accurately); perhaps it is this type of situation which accentuates the ubiquitous tendency among sociologists to attempt to theorize, though somewhat indiscriminately and often blindly, at any level of abstraction. As well as illuminating features of his theoretical work, an examination of Talcott Parsons' theory-building strategy, I trust, will clearly demonstrate the immeasurable value of devoting some consideration on strategy in the critical assessment of substantive theories in sociology.

To achieve all these objectives, I shall consider at the outset two general, yet important, points of interest. I shall first discuss Talcott Parsons' early intellectual influences and accomplishments. By undertaking this, I hope that I shall be able to provide a rough understanding of Parsons' basic conceptual armamentarium which comprises the backdrop, as it were, from which he elects to consider certain issues and to pose problems in the manner that he does. Next, I shall adumbrate what I think represents the logical structures of scientific theory and explanation mainly in order to furnish
a touchstone by means of which Talcott Parsons' theoretical formulations, both metatheoretical and substantive, will be assessed in this study.

It is worthy of note that there have been several attempts - though of a considerably limited scope - to examine Talcott Parsons' sociological theory in a manner very much similar to the inquiry I propose to conduct here, assuredly. This is an incongruous situation indeed for an individual widely considered to be the eminent theorist in his discipline. Much of the work undertaken in this respect has been largely superficial in the sense that Parsons' work has been given only passing critical attention (of the kind I suggest) in the framework of other considerations, and that a good share of his contributions have been criticized in porte-manteau terms, such that any redeeming features which may inhere are painted over with a broad unsympathetic brush. In some quarters, the worst that has occurred is that Parsons has been entirely ignored, presumably on the precarious grounds that Parsons does not have any significant sociological theory to offer because it is not scientific enough. To be sure, all this is too facile. These inadequate ventures at estimating Parsons' theoretical work and its significance, I think, are hardly edifying from the view of advancing knowledge in sociology or reasonable in terms of giving extant theoretical contributions their due recognition, particularly in an immature scientific discipline which
is still struggling to find its theoretical roots. However, I would be remiss not to mention that I am cognizant of one of the most notable, if not outstanding, volumes which purports to provide a comprehensive critique of Parsons' intellectual labors, *The Social Theories of Talcott Parsons*, (1961), edited by Max Black. Of the ten papers presented in this symposium, excluding Parsons' replies to his critics, only one endeavors to approach Parsons' theoretical work in the fashion I suggest. I refer, of course, to Max Black's contribution (32), which is philosophically informed and pungently relevant in its examination of Parsons' theoretical writings. However, it is, unfortunately, too brief. I think it does not concern itself with many of the unique theoretical difficulties that are encountered by those endeavoring to formulate theories in sociology. Furthermore, it does not deal in any significant manner with the logical structure of scientific theory and explanation which, in my view, is necessary when putative "scientific theories" are examined in immature scientific disciplines because they function as evaluative guides for present contributions and future formulations. Any analysis of Parsons' work, I believe, requires analyses of this kind. Thus, as much as I sympathize — and at times concur — with Max Black's approach, there is substantially more to be examined in Parsons' theoretical work. Although I am unmoved by efforts to support a standpoint by quoting authority, whatever that may
signify, or quoting others merely to increase the numbers that concur with the writer, I should think that it is nevertheless significant that two sociologists of disparate theoretical orientations who have reviewed the Black volume proffer the following observations. Alvin Boskoff notes:

It is interesting to note that a prevailing criticism (that Parsons' formulations are "not really theories") is greatly muted in this symposium. (33)

In a much similar vein, Philip Selznick writes:

I reluctantly conclude, not without pain, that Talcott Parsons shares with the rest of us a grievous fault: whenever we dabble in general theory we literally don't know what we are doing. We need to look again at the logical foundations of good sense in scientific discourse. We need to sort out the variety of functions performed by theoretical statement, for otherwise we can make no reasoned assessment. (34)

Assuredly, these statements, even though they appear in reviews and are made by only two sociologists, lend some point to my contention that the study I envision here is justifiable, if not of the utmost necessity.

Not unlikely, there may be strenuous objections concerning the type of criteria which will be employed in this study to analyze Parsons' theoretical work. It can well be argued that my criteria are wholly of an uncongenial character to begin with such that a negative assessment of Parsons' sociological theory and conceptions of scientific theory is almost certain to result. Phrased in another way, my criteria are not Parsons' criteria and vice versa. In a certain sense, this
claim may be true but like most blanket objections it obscures many essential issues and lines of argument. Moreover, it would conveniently ignore the purposes of this study. What I am attempting to do is to focus principally on the logical structures and assumptions of arguments which almost invariably underpin any theoretical undertaking whether it be in the disciplines of physics, chemistry, physiology, psychology, or sociology. Too, since Parsons vigorously maintains that his avowed intention in his theoretical work is to develop abstract scientific theory in sociology, (35) - and his copious writings amply bear witness to the assiduous attempts to realize this intention - it is only appropriate and instructive, I think, to subject his views on scientific theory and the essential features of his substantive theory to a rigorous analysis in terms of a set of criteria for what could be regarded as an adequate scientific theory recognized by the majority of philosophers of science and scientists. To affix the label of "scientific" to an undertaking, particularly a theory, requires the observance and utilization of a corpus of disciplined procedures and above all critical thinking.

Concerning these reflections on the vital importance of correctly adhering to the norms and criteria of scientific procedure, philosopher, Felix Kaufmann, in a series of journal articles and books on the philosophy of science, places the entire matter in a clarifying perspective. He notes:
These principles concerning the meaning of empirical knowledge; in other words, scientific knowledge have not been discovered only during the past few decades; they have now been so corroborated by the analyses of mathematical methods of the natural sciences that they can be looked upon as firmly established and generally accepted by scientists. They do not represent metaphysical dogmas but signify the results of a rigid examination of scientific thought. (36)

The crucial point, however, is this: to think scientifically is to submit implicitly to definite rules. If a scientist intentionally violates these rules in order to advance some extraneous aim, he is lacking intellectual sincerity; cooperation with scientists of this sort may prove impossible, though this need be no cause for regret. But there are other scholars who violate the rules without knowing it. In this case we can employ a "Socratic method", refined by modern logical technique, to show them that they have not observed the rules of method they had implicitly adopted. (37)

...where the consistent application of these rules of scientific procedure leads to results that are undesirable, the chance of the rules being violated will be relatively great; but this affects the validity of the rules no more than mistakes in calculation affect mathematical rules. I do not mean by this that the rules possess the status of pre-established principles but only that they are implicitly accepted by scientists, as can be shown by an analysis of their scientific activity. To violate the rules of science therefore means to think inconsistently. (38) (emphasis in original)

From the point of view of the logician, the procedure of an empirical science consists in the acceptance or elimination of propositions in accordance with given rules. (39)

To be sure, the adoption of a scientific method, as it were, does not imply a docile acceptance of rules and prescriptions from a covey of oracular individuals. What it does mean, however, is that empirical inquiry is conducted according to
the tenets of consistent and critical thinking which have proved their mettle through generations of scientific and philosophical endeavors. Aside from the ludicrously inadequate contributions of intuition and dogma, there appears to be no other intelligible alternative at this moment in the history of ideas. Thus, it is the fate of any theorist to be adjudged in the light of these uncompromising, yet indispensably helpful criteria. As regards Talcott Parsons' theoretical work, it would appear to me a rewarding venture to examine his ideas in this particular fashion and especially so since evidently no one has yet contributed to embark upon such a study.

Lest I should create an impression that the tenets and criteria of scientific discourse are unanimously agreed upon by scientists and philosophers and bereft of consuming conceptual problems, I should like to add a word or two to achieve some balance. In the philosophy of science there are countless questions dealing with various facets of scientific theory which are highly contentious and scarcely admit of any solution. However, one thing is quite clear and that is there is an assemblage of general notions and criteria which are at all events, reasonably stable, precipitating little debate as to their tenability or utility and are widely held by those who specialize in the philosophy of science and by others of different disciplines who have made intensive studies of the crucial issues involved in correct and consistent thinking in
matters of scientific import. In any event, the atmosphere of flux mingled with stability, enables a sufficient amount of individual interpretation to take place. Thus, much of what I will say in regard to the structure of scientific theory and explanation in this study will indeed be of a personal formulation but emphatically not arbitrary, or exempt from the other established canons of scientific discourse, and, of course, those of logic.

Further against my rationale, it may possibly be contended, among much else, that Parsons' theory eminently acquits itself in its ability to cast an explanatory light on social phenomena, structural niceties and language notwithstanding. But, is not this argument patently beside the point? To assert that a theory's merits are determined by its utility (or explanatory value) in empirical research, when it is suspected there are serious deficiencies in the structure of the theory – as it could be said in the case of Parsons – is to indulge in the fruitless enterprise of question-begging. Quite clearly, an instrument, which a scientific theory certainly is, must first be devoid of logical defect in order to be employed properly and effectively. As Alasdair MacIntyre cogently points out in this regard:

Unfortunately no quantity of empirical findings can redeem conceptual confusion; it is rather the case that until conceptual confusion is cleared up we are liable to misunderstand the alleged empirical findings. (40)
At bottom, this goes to suggest that an investigation which poses questions on the nature and objectives of scientific theory and explanations in the light of the criteria generally held in current thought in the philosophy of science (and in various scientific disciplines) attempts to separate out the various elements of scientific theory, that is, the concepts, the postulates, the theorems, the assumptions, the derivations, etc. are necessary. In other words, one must engage in foundation work, that is the philosophical analysis of the basic assumptions and implications of specific theories which in most instances are erroneous, vague, or inadequately expressed. These considerations immediately bring to mind an interestingly apt remark which Edmund Husserl once uttered to a gathering of philosophical students - that is, "...not to consider oneself too good for foundation work". (41) It goes without saying that the admonishment is indeed applicable to sociologists concerned with the appraisal of theoretical contributions in their discipline. Though it scarcely needs emphasizing, a critical analysis of Parsons' theoretical offerings requires abundant foundation work in this sense.

There is one further important point which I think requires brief notice. The immensity of Parsons' writings poses uncomfortable problems in the evaluation - and even exposition - of his theoretical work. Where should one begin? To be sure, this dilemma perplexed me for some time. To provide what I
believe is an illuminating and accurate perspective of his work, I have decided to concentrate on considerations which, in my estimation, appear to be essential to his theoretical thought from journal articles that were published two years before the publication of his monumental volume, *The Structure of Social Action* (1937), which, it may be noted, represents his fundamental departure point for further substantive theorizations and thinking on theory as such, to journal articles and books that have appeared in the intervening thirty years up to the present. According to Parsons, the utilization of *The Structure of Social Action* is indispensable for the understanding of his theory of social action, and, most importantly, for the strategy of theory construction. In his, "the Point of View of the Author", in the Max Black volume, Parsons extensively elaborates (42) on the importance of his first book for his subsequent theoretical work; here I quote Parsons quite liberally in order not to detract from the contention he wishes to establish firmly,

This basic interest [the problem of "the relation between the main traditions of economic theory and the interpretation of many salient characteristics of modern industrial society"] crystallized in my doctoral dissertation at Heidelberg on the Concept of Capitalism, with special reference to the work of Sombart and Max Weber. A relatively clear distinction between the scientific and the ideological aspects of the problem was worked out fairly early, and primary attention given to the former. In this context it became very clear that the problem of empirical interpretation or "diagnosis" could not be adequately handled without attempting to make far more explicit than was ordinarily done the extra-economic theoretical framework within which economic theory would have to be made to fit. If properly approached this could be
seen to be a major theme in the work not only of Weber, but also of Durkheim and, very explicitly, of Pareto. Having worked out this theme to a degree in the writing of these authors, with Marx in the background, I attempted to tackle it in the work of the most influential economic theorist of the generation spanning the turn of the century. Alfred Marshall, The putting together of all these things eventuated in the book The Structure of Social Action (first published in 1937), which is the basic reference point of all my subsequent theoretical work (it is only very casually mentioned in any of the above essays except that of Bevereux). (43)

I bring up this first major work here because it is such an important reference point, not only in terms of context, but also what I may call the strategy of theory-building. The convergence which I was able to demonstrate in that study, between the broad conceptual themes used by these four authors, constituted the first level of integrated general theory in my own work. This was clearly very far from being a logico-deductive theoretical system in the sense referred to by Professor Black, but equally clearly it was very much more than an eclectic collection of unrelated theoretical ideas. Even to employ The Structure of Social Action as a departure point is indeed a perilous task, for, to be sure, in the intervening period (that is, from 1937 to the present), Talcott Parsons has been extremely fertile in his writing production! Thus, I will at once confess that my exposition and criticism will be highly selective, focusing my attention on considerations which I consider pertinent to my immediate purposes in this study. It is also realized that such a procedure may lead to distortions, over-stress, and under-stress on certain aspects of Parsons' theoretical work. But when all is said and considered one must begin somewhere. In view of these perennial, obstructive difficulties prevailing in all expository and critical studies
of an individual's intellectual contributions, we can perhaps take some assurance in Walter Begehat's sententious, but sage, observation that "To illustrate a principle you must exaggerate much and you must omit much." Painfully mindful of these hazards, I shall proceed with my inquiry with the profound hope that the severity of the criticism that can be levelled at me can be reduced to a palatable minimum.

At this point, I should like to dispel immediately any fortuitous misinterpretations or gratuitous inferences emanating from the foregoing reflections in the sense that I am denigrating any sociological contribution as scientifically insignificant which does not explicitly conform to the tenets of hypothetico-deductive theory. Far be it from me to arrogate to myself the mantle of the supreme adjudicator of theories advanced in the sociological realm. Indeed, this would be a form of methodological presumptuousness in a field in which disciplinal procedures of inquiry are somewhat impaired by numerous - though legitimate - philosophical disputes centering on the often claimed qualitative distinction of social science from natural science. The results of such deliberations, if we overlook the dubious qualifications and conditions, are by no means decisive either for the view that there is a complete unit of scientific procedure in both the natural and social sciences or the standpoint that there is undeniable qualitative distinction of the two species of science, as it were. Be that as it may, for I do not propose to launch any examination into this fascinating realm of debate over
fundamental epistemological issues in the philosophy of science, natural and social. What I am affirming is basically this. There are criteria which philosophers of science and scientists - particularly those in the natural sciences - assert are applicable to any theoretical work in any discipline, to determine whether it is to be regarded as scientific or otherwise. The criteria, those of hypothetico-deductive theory, are structured on sound logical discourse and the inescapable actualities of human thinking and have been employed in various ways with incomparable success in the natural sciences. Talcott Parsons maintains that he is formulating scientific theory in the discipline of sociology; he wishes to develop theory to the level of those currently receiving wide broadcast in the more modest physical sciences. Let us apply the criteria of the hypothetico-deductive theory to Parsons' theoretical formulations and observe how they fare.

It is highly important to mention at this point that Parsons has written a substantial amount (books, essays, and journal articles) in collaboration with a number of sociologists, anthropologists, and social psychologists - the most notable being Edward Shils and Robert Bales. Indeed, for a prominent theorist in the field of sociology, where individuality of approach and thought had formerly been the norm (e.g. Spencer, Pareto, Weber, Durkheim, etc.), Parsons' case is a significant departure. Since, however, much of what Parsons has written in collaboration with others bears the imprint of Parsons' thought and style - and also for the mundane reason of a writer's convenience - I shall refer only to Parsons in those works which are also attributable to other writers.
It almost goes without saying that there are innumerable types of inquiries that can be undertaken with regard to any intellectual contribution in any discipline. What specific type of inquiry takes place, of course, largely depends on the aims of the inquiry which are as diverse as their number. Only one kind, however, generally obtains for a critical assessment of the merits or demerits of a scientific theory, or a contribution which represents an initial stage in the development of a scientific theory. Almost without exception, a critical appraisal of scientific theory necessitates attention to be directed exclusively to its substantive content. Understood in its proper sense, this means that an examination must dwell on the adequacy of the theory's logical structure such that inconsistencies and other logical deficiencies are absent and also determine how the theory fares when it is confronted with the pertinent empirical data, that is to say, the theory's susceptibility of empirical refutation. On the other hand, however, a discussion of a theory's provenance in the context of its critical assessment constitutes a fallacy of argument, to be precise the genetic fallacy. (1) This invariably involves
considerations of a psychological or sociological character regarding the theorist's motives, intentions, or biases. To be sure, these factors do not have any significant bearing on the tenability or untenability of the theory's argument which is determined in large part by the rigorous canons of logic and scientific procedure. But, on frequent occasions, a general knowledge of the theorist's early intellectual influences which shaped his thought serves worthwhile and useful purposes. It enables one to thrust the theorist's work into a comparatively larger perspective, thereby rendering it much easier to discern the theory's position in importance with respect to the antecedent and contemporary intellectual currents. Such a study also furnishes general conceptual guidelines so that one can almost reliably anticipate various, but definite lines of argument in the theorist's further elaboration and the defence of his theory. What is more, one is given an opportunity to observe what and how the theorist has drawn from his predecessors and doubtless determine the extent to which he has embarked on his own independent course of theorization. Finally, there is the factor of becoming acquainted in some way with the general approach which the theorist adopts toward theoretical and empirical questions in his discipline.

In the case of Talcott Parsons, I would think these remarks especially apply. Given the catholicity of his intellectual interests, it would be fair to assume that his early
associations with a number of intellectual traditions and outstanding academics in a diversity of disciplines were markedly influential and enduring for his subsequent theoretical work in sociology. The versatility of Parsons' theoretical endeavors has on more than one occasion precipitated considerations regarding his early intellectual contacts and labors. Thus, it is in the light of these general considerations that I shall proceed in this chapter to survey in a compendious fashion Parsons' earlier intellectual background. Although I do not wish to attribute too much to such a cursory study which is, in effect, a minor excursion into a sociological analysis of Parsons' education, it is sufficient to state that, in varying degrees, it should facilitate a greater understanding of his theoretical work which commands paramount importance and respect in contemporary sociological thought.

For an American sociological theorist of such wide repute, Talcott Parsons' early university training in his discipline of specialization was surely circuitous and, in certain respects, unconventional. (2) As an undergraduate student, Parsons attended Amherst College, a relatively small liberal arts institution in the New England state of Massachusetts, which did not offer — and, incidentally, still does not offer — any formal course work in sociology. Parsons' main academic interest at this time was biology, a course of studies in which he received a major for his B.A. degree.
Interesting to notice in this connection is the fact that Parsons was also a laboratory assistant for a "general course in biological evolution" (3) while still an undergraduate student at Amherst. In this respect, it is significant to point out that Parsons undertook some research work in biology at Woods Hole Laboratory, a noted marine biological laboratory in the state of Massachusetts, during a summer when he was still attending Amherst College as an undergraduate student. With such a propensity for the biological sciences, Parsons devoted serious consideration to the possibilities of embarking on a career either in biology proper or medicine. Circumstances, it appears, dictated otherwise. However, it is worthy of note that a significant portion of Parsons' theoretical writings in sociology from the outset of his career to the present are permeated by concepts and arguments adopted from biological discourse, undoubtedly an indication that his early biological interests and training were sharply impressed in his conceptual repertoire.

It should not be supposed, however, that Parsons' undergraduate years were entirely devoid of an introduction to what one could normally consider to be social scientific knowledge. His basic grounding in this area came from course work in economics and, interestingly, a course offered by the Amherst philosophy department rather presciently entitled "The Moral Order". The teacher of the economics courses was
Walter Hamilton, an institutional economist, who, of all things, eventually became a professor of law at Yale University. Principally because of Hamilton's unorthodox shifting between disciplines, Parsons engagingly labels him "a bit of a maverick", an adjective which on occasions Parsons unabashedly applies to himself, on account of the ease with which he steps from one discipline to another in the aggregate which typically constitute the social sciences, oblivious to the disciplinary boundaries in the pursuit of the problems he has set for himself. (4) According to Parsons, Hamilton was his "most important teacher there" (at Amherst College). In what manner Professor Hamilton was of such importance, Parsons himself does not specify, though one can legitimately infer from his remarks that institutional economics, which Hamilton taught, constituted one of the foremost foundation blocks of his future social scientific concerns.

Another professor at Amherst College who exerted an immeasurable influence on Parsons' thinking, the fledgling sociological theorist, was Clarence Ayers, a philosopher, later to become an economist. He was the teacher of the course called "The Moral Order". Here it can be assumed that Parsons received some semblance of a solid introduction to the prevailing literature in sociological thought at that time. Major works by theorists such as William Graham Sumner, Charles H. Cooley, Emile Durkheim, and Thorstein Veblen were read and
studied. For both Ayers and Hamilton, whose intellectual interests reposed in institutional economics, Veblen was a significant writer (or theorist if one may) whose work primarily emphasized notions closely connected with those obtaining in institutional economics. Broadly speaking, institutional economics is the study of the intimate interrelations of economic theory with the interpretation of social phenomena. In consequence of this, Parsons styles his introduction to the social sciences as one via the conduits of institutional economics. It was this type of social scientific study "which provided my main formative interest", (5) in sociology no doubt.

After being graduated from Amherst College, Parsons attended the London School of Economics, in London, England, for one year. For unexplained reasons, Parsons regards this undertaking as "another rather unorthodox move". (6) There Parsons undertook academic work under three eminent social scientists of their day - T.L. Hobhouse, Morris Ginsberg, and Bronislaw Malinowski. Of the three, Malinowski represented the cardinal influence. For a wide variety of reasons, Parsons' intellectual indebtedness to Malinowski is apparently immense. It was Malinowski from whom Parsons acquired a welter of seminal notions in regard to specific areas of social scientific endeavors and also broad directions in regard to crucial meta-theoretical and methodological issues. In a note of
acknowledgment, Parsons himself amplifies on the significance of his studies under Malinowski, who was widely regarded by his disciplinal colleagues in anthropology as a theoretician and researcher par excellence:

He [Malinowski] introduced me to an area of borderline considerations between sociology and psychology, and started out my interest in problems of kinship, family structure, and socialization. All these were things about which I had certainly never had any real inkling in the Amherst phase. (7)

For a further perspective, these remarks should be compared with another of Parsons' acknowledgments to Malinowski which appears in Parsons' exceedingly critical essay on Malinowski's theory of social systems.

In this respect [Malinowski as a teacher] I have a heavy personal debt to Malinowski which I fully recognize and do not wish anything said in this essay ["Malinowski and the Theory of Social Systems"] to invalidate. (8)

Parsons, it appears, was indeed discriminating and selective in what he accepted and used from Malinowski's anthropological offerings. He does not once in any of his writings acknowledge a debt, or attribute any influence, to Malinowski for the general model of structural-functional theory which Malinowski, for the first time in the history of social scientific thought, devised and continuously elaborated upon in a rigorous systematic, and it was hoped, a scientifically correct fashion for the analysis of societies (or social systems). An observer of the current and past developments of sociological and anthropological theories would, I believe, naturally assume that the
theoretical bonds between Malinowski and Parsons would be exceedingly close and fairly strong for the fundamental reason that Parsons is the current prime exponent of structural-functional theory coupled with the knowledge that Parsons took a part of his formative studies in the social sciences from Malinowski. Except for the patent external similarities of theoretical approaches, that is, the structural-functional mode of theorizing, which the theoretical works of Malinowski and Parsons evince, there is no direct indication by Parsons in his work that he was influenced by Malinowski in this crucial connection. If, as Parsons concedes, Malinowski did make a notable impression on him in the more specialized areas of social scientific interest (i.e. kinship, family structure, socialization) it could perhaps be supposed without cavil that the general patterns of structural-functional theory which Malinowski propounded and analyzed these specific areas of study in terms of it were adopted by Parsons and later employed in his theories of social action. The only evidence—and it is tenuous—I can adduce in this case is the argument that Parsons did not encounter another theorist or the writings of another social scientist, inasmuch as we are permitted to infer from his own accounts of his intellectual career and his voluminous theoretical writings, who advocated and, moreover, employed a structural-functional standpoint in his analysis of social phenomena. Perhaps the sole exception to this contention
would be Vilfredo Pareto, an Italian social theorist whose works, as we shall soon notice, Parsons scrupulously examined and used to some degree in his own theoretical formulations. Pareto advanced the notion that society (or a social system) is in effect a system in equilibrium, an argument closely akin to that of the structural-functional point of view mainly because there is a presupposition lurking about that a society is constituted of various components which have positive or negative consequences for each other and for the system as a whole. However, I should think that the similarity of the theoretical structures of Malinowski and Parsons, given the added factor that Parsons came upon Malinowski's work before he plunged into the study of Pareto, gives Malinowski the ascendence in this regard, even though Parsons himself does not explicitly mention it either in his theoretical writings or his accounts of his intellectual career. However, quite apart from this argument whether or not Malinowski was a dominant force in Parsons' adoption of the structural-functional approach which admittedly is conjectural until further biographical evidence becomes available, the repercussions of Malinowski's teachings in disparate specialized areas of the social sciences were indeed profound and enduring, particularly at the outset of Parsons' career. But, it is indeed curious to observe that Parsons himself does not regard Malinowski as even one of the major influences on his sociological thinking. This we shall see later in this chapter.
By all accounts, it seems that the impressions made by Hobhouse and Ginsberg on Parsons were of quite negligible force. At that time, it may be noted, Hobhouse's prime intellectual interests were the study of the evolution of morality and political institutions. (9) Ginsberg's central academic concerns (10) were somewhat similar; he stressed the analysis of norms and values of a society (in other words, morality) and the study of institutions inhering in various types of societies, preliterate and industrial. Except for his exposure to Malinowski, it could be justifiably assumed that Parsons' one year tenure at the London School of Economics was in large part intellectually uneventful in terms of his future theoretical work.

In 1925 Parsons undertook his doctoral work at the University of Heidelberg in Germany, not by choice, but by assignment, after accepting a scholarship offer. For Parsons, this was a critically important development in his intellectual career.

As it turned out it was a most fateful assignment from my point of view, for one of the three persons whose works have had the most important influence on my thinking was then the dominant influence at Heidelberg. That was Max Weber. Now Weber had died a decade earlier, in 1914, so I never met him in the flesh, and his dominance in the intellectual atmosphere at Heidelberg was not without a great deal of opposition. But this opposition meant that there was an extremely lively controversy and that everyone who came there was made familiar with his work immediately. (11)

Like most German social science students at the time, Parsons naturally became thoroughly acquainted with Max Weber's
outstanding sociological-historical monograph, *The Protestant Ethic and the Spirit of Capitalism*. (12) According to Parsons, this work combined with a host of Weber's metatheoretical and substantive writings have long exerted "a very dominant influence" on his sociological thought. Thus, as a result of Weber's emphasis on the comparative studies of diverse social structures and the investigation of various problems in the areas where cultural and social systems merged, Parsons recognizes Max Weber as the most important influence in his personal intellectual evolution. (13)

Parsons' doctoral dissertation, "The Concept of Capitalism", clearly manifests the multifarious influences arising from what would seem at first blush to be a thoroughgoing exposure to the German literature in institutional economics and the social sciences which was largely characterized by its focus on problems closely akin to institutional economics. (14) Essentially, the dissertation centered on the treatment of the concept, "capitalism", in the major works of Werner Sombart, Karl Marx, and, above all, Max Weber whom, in comparison to the other two theorists, Parsons views as "overwhelmingly the most important to me". At this stage of his development, the lineaments of a definitive intellectual interest were coming into bold relief; it was this: "the relationship between this (the relation of sociological and economic theory) and the interpretation of the modern industrial order". (15) But, according to
Parsons, his general background in economic thought was indeed sparse. To continue his concentrated investigations into the particular problem realm he outlined for himself, it was clear that further bolstering in the discipline of economics in the form of more advanced university courses in economics was, to Parsons, urgently required. (16)

In 1926-1927 when he was still writing his doctoral dissertation for Heidelberg University, Parsons became an instructor in economics at his former undergraduate school, Amherst College.

After completing his doctorate studies at Heidelberg, Parsons, in 1927, secured a position at Harvard University as an instructor in economics. At the same time, he enrolled as a graduate student, attending various seminars and lectures in economics. During this phase of his career, Parsons encountered two distinguished economists who were to make a notable impression on his thinking, particularly in the marginal region between sociology and economics - the sociological factor in economic theories. This problem focus, for Parsons, was increasingly becoming a paramount intellectual concern which absorbed his analytical attentions for the first decade of his intellectual career. The two economists were Joseph Schumpeter and F.W. Taussig (17) who both occupied eminently influential positions in the discipline of economics at that time. What were the specific influences of these two economists on Parsons' thought?
From Schumpeter, Parsons acquired the basic outlines of a "theoretical system" which, according to Parsons' further studies, found its more salient expression in the writings of Alfred Marshall, a late nineteenth century British economist who, at this time, was coming to be widely recognized as one of the major theorists in Anglo-European economic thought. It is indeed unfortunate, however, that Parsons does not clarify in any manner as to what he means by the notion of "theoretical system" in this specific context. An attempt may be ventured in this respect. (18) But, at the outset, several general considerations are imperative. (19) Utilitarian positivism in economic theory, as Parsons conceived it, directed its focus on a dimension of conclusively demonstrable economic behavior which was explained by a closed determinate system of notions depending on biological factors (hereditary and environmental); however, these biological factors did not cast light on what Parsons thought were crucially important elements of economic thought, particularly, the actional elements, i.e., means, ends, goals, norms. These factors, argued Parsons, were residual categories, that is, they are not concretely identifiable and segregated out as, let us say, the biological elements. They cannot be ignored if an adequate explanation of human behavior is the prime objective of social scientific enquiry. In action, there is always the element of voluntarism, indeterminacy. If these notions are prohibited,
as they certainly are in a positivist (or determinate) system, then the conceptions of rationality, ethics, values, and norms, the indispensable elements of action, are completely devoid of meaning, hence, of little or no value. But, to Parsons, this is too high a price to pay for scientific accuracy, and is also a cavalier dismissal of an undeniable characteristic of human behavior. Thus, what is required is a theoretical scheme which takes into account both the implacable biological elements and the pertinent actional factors such that their numerous interrelations and functions can be described and analyzed; within this framework, the best of both theoretical worlds (positive and actional), as it were, can be employed together. This, I should think, is what Parsons has in view when he refers to a "theoretical system", (20) at this stage of his intellectual evolution.

Through T.W. Taussig, Parsons became infinitely more acquainted with the theoretical writings of Alfred Marshall. Tausig himself, however, was not of noteworthy theoretical significance to Parsons' intellectual purposes.

It wasn't Taussig's independent thinking that influenced me so much as my becoming thoroughly familiar with Marshall through Taussig, and this has remained a very crucial point to me. (21)

It is indeed relevant to inquire here what aspect of Marshall's thought attracted Parsons. Parsons' extreme interest in Marshall's economic theories derives from his concern with the
"theoretical system" and its emphasis on the action elements of behavior. (22) In his theories, Marshall put forward the concept of "activities" pertaining to economic behavior in which the "wants" of an individual were determined in effect by value factors in distinct contrast to an individual's wants being generated by biological needs. (23) Clearly, this sharp distinction of wants permitted Marshall to explain the choice and means-end facet of economic behavior in his basically utilitarian theory without reference to biological variables as was the wont of most economic theorists in Marshall's day. Parsons conceptualized these "activities" as an integrated value system shared by a large number of the population of a particular society. (24)

It is interesting to note in passing that Marshall was not cognizant of the significance of the value characteristic inhering in his notion of "activities" and consequently failed to pursue the multiple implications which it would have contained for the other facets of his theories and other economical sociological theories of his time. For Parsons, taking account of the actional elements - incidentally, always a Parsonian concern - which necessitated serious scientific considerations of concepts such as "values", "goals", "normative standards" and "means and ends" represented a significant theoretical advance in the social sciences which merited further examination. Several journal articles and a chapter in The Structure of Social Action (1937) devoted to the exposition and analysis of
Marshall's various economic theories amply evidence the significance of Marshall on Parsons' sociological thought. (25) As with Malinowski, Parsons, interestingly, does not recognize Marshall as one of the most important influences in his intellectual development.

Coeval with his gradual acquaintance with the rudimentary elements of the major prevailing economic theories and moreover, those which were put forward in the previous one hundred years in the history of social thought, Talcott Parsons came across the writings and lectures of a British philosopher who was teaching at Harvard University at that time and the writings of the late nineteenth century Italian sociologist-economist, Vilfredo Pareto. Without doubt, both made their intellectual mark on Parsons which became clearly evident in his future theoretical work. In Parsons' views, Whitehead's principal contribution was an explicit emphasis on the critical importance of the conception of "system" in scientific analysis and the general framework of scientific theory. (26) Further concern with the manifold issues pertaining to the essential character of scientific theory in both economics and sociology impelled Parsons to pursue the arguments voluminously advanced by Pareto who laid cardinal stress on an abstract theory of social equilibrium in which the factor of logical and nonlogical action - behavior in terms of values, goals, means and ends - is a prime constituent. On account of the theory's dual
voluntaristic and positivistic nature, Parsons was favorably impressed by its capacity to provide a satisfactory explanation of the multitudinous working of a social system and its potentiality for the future development of an improved logically-tight, and comprehensive theory of action. (27) It is of noteworthy significance that Parsons' comprehension of Pareto's admittedly intricate theoretical formulations and their divers implications for related theoretical problems and the methodology of social science was substantially enhanced by the assistance of a prominent Harvard biochemist, L.J. Henderson, (28) who exhibited an enormously active interest and a respectable competence in questions concerning scientific theory as such and methodology, all combined with an abiding interest in a number of problems hovering in the domain of the social sciences. Perhaps Parsons' collaboration with Henderson in this particular connection was a singularly appropriate intellectual arrangement. They both held roughly similar intellectual interests in the development of a suitable theory in the social sciences as well as in Pareto's futile theorizations. Apropos of these comments on this intellectual relationship, Edward C. Devereux suggestively remarks, in his excellent expository essay on Parsons' sociological theory, that Henderson "shared and fostered Parsons' interest in the parallels between organisms and societies as systems". (29) This is pointedly borne out by Parsons himself. In his earlier - and later - published works, Parsons frequently expresses his acknowledgements to Henderson for various
notions with regard to the critical importance of the notion of system in the formulation of scientific theory and the logical structure of scientific theory and, above all, the complex nature of facts, and their multiple yet crucial relations with a theory. (30)

Whilst at Harvard University during what he terms his "first Harvard phase" (31) (approximately between 1928 to 1932), Parsons undertook an intensive study of Emile Durkheim's prolific theoretical and empirical writings. Prominent among these were the currently standard Durkheimian sociological contributions, classics as it were, - The Elementary Forms of the Religious Life (1912), The Division of Labor (1893), and Suicide (1897). What initially prompted Parsons to return to Durkheim's work was the "two-sided problem" to which he was channeling the bulk of his intellectual energies for some time. In germ, it was this: (a) How was modern industrial order brought about? (b) And, what are the relations obtaining between economics and sociology, disciplines purporting to analyze various aspects of social phenomena? (32) It is Parsons' firm contention that Durkheim's The Division of Labor furnished an adequate treatment of the economics-sociology problem and, it is safe to infer, examined the multitudinous dimensions relating to the problem of order in modern industrial societies in a fashion that intellectually satisfied him. (33) Though not affirmed in explicit terms in his own elaborations
of his intellectual career, there is a tacit assumption that Parsons regarded Durkheim's treatment of sociological and, in a minor way, certain problems in economic theory were largely compatible with his own interests and purposes in sociology. Thus, primarily because of Durkheim's theoretical and orientational relevance in regard to the conception of a social system and the integration of its diverse components, Parsons categorizes him as the second most important influence in his intellectual development. (34)

Whilst we are about, it would be of high interest to take into account a cursory but nevertheless significant evaluation of Parsons' intellectual capacities during the early stages of his intellectual career by Pitirim A. Sorokin who was also lecturing and teaching at Harvard University at this time. In his recently published autobiography, A Long Journey, (35) Sorokin recounts in detailed fashion his influential participation in the establishment of the first Sociology Department at Harvard University in 1930-1931. Interestingly enough, one of the personages indirectly involved was Talcott Parsons, then an instructor in economics. Differences arose in regard to Parsons' qualifications and appointment as the sociology department's faculty instructor. This action initiated a series of developments which eventually culminated in Sorokin interviewing Parsons. In what follows, Sorokin describes these delicate circumstances with which Parsons was
confronted in addition to, and most important for present purposes, throwing some telling light on Parsons' intellectual competences and interests.

In December, 1930, I submitted the committee's plan for the department to President Lowell. He and the administration approved it with the exception of one point: they refused to approve the appointment of Talcott Parsons as the Department's faculty instructor. Somewhat surprised by this, I asked Professor Burbank, chairman of the department of economics (where Parsons was an instructor) what could be the reasons behind this refusal. The gist of Burbank's remarks was that Parsons seemed to be less interested in economics than in sociology, that possibly for this reason his work in the department of economics was not of the best quality, that he probably would do much better work in sociology than in economics, and that therefore the department of economics would be only too glad to transfer Parsons to the new department. My personal impressions of Parsons, formed from several meetings with him, were rather favorable. In our conversations he displayed a good analytical mind and a discriminating knowledge of the theories of Durkheim, Pareto, Weber, and other sociologists. Duly impressed, I strongly recommended Parsons' appointment to the committee and obtained its approval of my recommendation. (36)

It becomes strikingly evident that Parsons, very early in his intellectual career, attained a fairly penetrating knowledge of the works of several European social scientists who were to play prominent roles in his future theoretical formulations. As noted, Sorokin considered Parsons to have a "discriminating knowledge" of these theorists, at all events, a complimentary assessment rendered by a widely esteemed sociologist of his time. Also, Sorokin was favorably impressed by what he terms Parsons' "good analytical mind". Just precisely what Sorokin signified by this is not at all clear. It is interesting to
note that, according to Burbank (then chairman of the Department of Economics at Harvard) Parsons' cardinal interests did not lie in economics as such but in sociology. This perhaps could be well understood. Throughout his early Harvard phase, as it were, Parsons was, above all, concerned with the sociological facets of the economic theories he came across. A swift glance at the list of Parsons' published work from 1923 to 1935 - and presumably this provides an indication of his intellectual interests and teaching tendencies - unmistakably discloses that the analysis of economic theories with a view to ferreting out the sociological elements inhering in them bulked quite large in his thought at that time. (37)

At this point, it is not without profit to weigh the significant factors involved in Parsons' acquisition of a stable working knowledge in psychology, a discipline which his studies and interests in sociology and economics induced him to investigate in an extensive, albeit systematic, manner. As Parsons himself intimates, he did not take formal course work in psychology at any time during his student years in America and Europe. His knowledge and subsequent work in this field came through a lengthy independent reading and study of the available psychological literature. In his early excursions into this area, he read, and to all intents and purposes was markedly influenced by two books dealing with fundamental issues in psychology: The Mentality of Apes (1925) by Wolfgang Kohler and Purposive
Behavior in Animals and Men (1932) by Edward C. Tolman.

Apparently, these discussions enabled Parsons to cope with the allegedly scientific demands of behaviorism which strikingly clashed with the actional aspects of sociological and economic theories and also Weber's interpretive sociology which basically relies upon ideal types and the philosophically lubricous problem of Verstehen - all of which, to be sure, Parsons embraced in his theoretical thought.

I was at least comforted that psychologists like Kohler did not swallow the behaviorist position. This meant you did not have to be a behaviorist to be scientifically respected, although there were circles in which you were made to feel that way at that time. (38)

Evident here, moreover, are the roots of Parsons' long-standing antipathy toward behaviorism, and the assiduous effort toward achieving an apposite theoretical formulation which could be widely considered to be eminently scientific. This, it may be noted, represents a preoccupation which imbues much of Parsons' theoretical work. In any event, it is worthwhile to note that Kohler's and Tolman's psychological works have to a modest degree contributed to Parsons' formulations in the theory of action. A close inspection of Parsons' publications spanning the period between 1937 and the present clearly reveal that Kohler's deliberations particularly shine through in Parsons' discussions on the factor of orientation and their organizing functions in social action. (39) As for Tolman (40), his work on Parsons' thinking is indeed noticeable, specifically as
regards his arguments on the non-actional components of social action, i.e., the physiological and psychological characteristics and, before all, the cognitive ordering of discrete experiences in action situations.

Another psychologist who presumably had a considerable impact on Parsons' theoretical thinking was Walter Cannon, particularly through his book, *The Wisdom of the Body* (1932) [41]. As Parsons readily admits, the book was in large measure semi-popular in intent and tone but it nevertheless contained material of substantial biological and methodological import which he found of immense value for his studies in scientific theory and cognate issues. Cannon outlined his conception of homeostasis and amplified at length on his position regarding equilibrium theory, a notion to which Parsons was increasingly drawn. Cannon's conceptions in these critical areas of methodological contention patently diverged from corresponding notions employed in mechanics. This apparently was a development which Parsons viewed with considerable favor for, it allowed one to devise a theory of action which emphasized norms couched in a generally acceptable scientific frame of reference. Taken together with L.J. Henderson's writings and discussions on questions of general meta-theoretical import, Cannon's book was instrumental in significantly fashioning Parsons' thinking with respect to the idea of systems and also theory construction in sociology.
Parsons was to an extent associated with Elton Mayo of the Harvard Business School (42), in the early thirties, who at the time was engaged in his classic Western Electric Researches. However, Parsons does not elaborate as to how and to what degree Mayo and his type of scientific analysis of social phenomena had made an impression upon him, apart from acknowledging his contact with him. It would appear that the effect was somewhat attenuated for, Mayo was primarily concerned with the examination of the social and mainly psychological parameters of industrial organizations which doubtless necessitated copious empirical research. The theoretical factor, in terms of Parsons' predilection for generality of scope, was less pronounced, if not entirely shunted aside. A vague note of mild interest in Mayo's scientific work is hinted at, in my estimation, when Parsons ventures to describe the nature of Mayo's researches: "Mayo's work, of course, was much concerned with depth interviewing and all that sort of thing". (43) And, to be sure, empirical research in sociology is not and presumably never has been, one of Parsons' most cherished intellectual interests or pursuits. It was Mayo, however, who suggested to Parsons that it would be advisable to become familiar with the writings and thought of a notable psychological theorist, namely, Sigmund Freud, for purposes of further illuminating problem areas that were then being encountered by Parsons. What transpired was as follows. When Parsons was
endeavoring, from a theoretical perspective, to come to grips with the problem of professional roles and their relations to business roles which largely depended on the profit motive, he decided to investigate the medical practice as a particular case in point. This choice invariably induced him to explore a number of kindred sociological and psychological problems which came to the surface as he proceeded with his work. For Parsons, one of the most significant problems emerging in his analysis of the medical profession was the relationship of illness, its medical attention to the motivational aspect of the personality structure. It was at this juncture that Mayo recommended Freud's writings to Parsons. Although Parsons' first major book, *The Structure of Social Action* (1937) refers only twice (44) to Freud, his subsequent theoretical and substantive works, primarily in the area of socialization, from approximately 1950 (45) to the present, contain an enormous assemblage of Freudian notions and modes of argument. Because of Freud's relevance to his own theoretical schemes, particularly in the intricate interrelation of the social system and the personality, Parsons now regards Freud as the third most important influence in his intellectual evolution.

Since my explicit intention in this chapter was to trace out in a fairly abbreviated fashion a select portion of Parsons' early intellectual exposures and influences, I shall not proceed to discuss additional intellectual impacts and
acknowledgments which are evinced after the publication of The Structure of Social Action (1937). Much of what is embraced in the way of influences and new ideas, etc. will be taken into consideration in the expository and critical sections of this treatise. However, I would like to add in passing one word in regard to this matter. As a consequence of a firm crystallization of theoretical perspectives and interests, undoubtedly an indication of intellectual maturation, and the workings of his fecund imagination, the number of additional significant influences on his theoretical thinking sharply diminish, almost to the vanishing point. Throughout the period between the publication of The Structure of Social Action (1937) and the present, one element of his thought seems to be strikingly evident. Parsons' recurrent references to many of these earlier social theorists, mentioned previously here, unmistakably accentuate the deep impress of their fundamental notions and arguments on him and, to a very significant degree, have determined the broad directions which his thought has taken in his abundant theoretical and meta-theoretical writings of the past forty years.

In this connection, I should think that Edward C. Devereux, in his expository essay on Talcott Parsons' sociological theory, rather pithily expresses how the impress of the unsurpassed immensity and diversity of Parsons' early intellectual contacts and labors have long exercised a dominant influence on his theoretical formulations:
If it is true that one absorbs a part of all that he has met, we should not be surprised to find that various strands of Parsonian theory reflect and incorporate elements of biology and medicine, of economics, especially of institutional economics, and of the utilitarian tradition from which it emerged, of German formal sociology, with its propensities for ponderous systematic analysis, together with its traditions of idealism and Verstehen, of structural-functional analysis as developed by Durkheim and the anthropologists, and of Gestalt and Freudian psychology. (46)

Notwithstanding his fustian prose, it is perhaps largely because of this vast highly disparate intellectual background along with a propensity to theorize abstractly, which renders Parsons' contributions at one and the same time, theoretically relevant, insightful, and, above all, exceedingly complex almost to the point of bafflement.

It is now appropriate to proffer several general observations about Talcott Parsons' early intellectual background. Without question, Parsons was steeped in the European tradition of abstract, speculative theories of social phenomena in his undergraduate and graduate training, a tendency which was immeasurably reinforced by his doctoral work in Germany. From this general style of approach in his later theoretical formulations, he seldom diverged. He has always been a theorist in the strictest sense. The North American ethos of an overwhelming emphasis on empirical investigations, specifically in sociology and economics, has been of inconsequential effect; the type of problems he selects to study, perhaps a blend of predilection and his training, are wholly uncongenial
to this piecemeal procedure of analysis. It is noteworthy that Parsons has frequently imported a significant number of concepts and lines of argument into his sociological writings from the two specialized areas of study in which he was previously involved, namely, biology and economics. Whether these disciplinal borrowings have assisted Parsons in his theory construction, however, remains for an analysis of the theory to provide an indication in this regard.

Also worthy of note is Parsons' initial acquaintance with the literature, or for that matter individuals, on what I would term, the philosophy of science, particularly with regard to the logical character of scientific theory and explanation. Virtually all his thinking in regard to this crucial matter, for a theorist at any rate, finds its basic roots in the works of individuals who specialized in biology and whose theoretical concerns, to be sure, were with systems in biological contexts. In his early stages of development and even now, Parsons has been markedly influenced by the arguments of L.J. Henderson and Walter Cannon, who both were biologists. One - and perhaps the only one according to one reference in Parsons' early publications - philosopher figured to some degree in Parsons' conceptions of scientific theory and cognate philosophical issues. This was Alfred Noah Whitehead, who uttered many things of interest concerning
scientific enterprise but whose philosophic forte was clearly not the philosophy of science. To a much lesser degree, Vilfredo Pareto and Max Weber in his methodological writings wielded some influence in Parsons' conception of scientific theory and scientific enterprise; more specifically, Pareto stressed the unquestionable importance of "system" in scientific theory while Weber dealt with a considerable number of issues pertaining to the conduct of scientific inquiry in the natural and social worlds and the "fact-value distinction" controversy in the social sciences. What, then, becomes strikingly conspicuous by its absence in Parsons' university training and the early stages of his career, is a satisfactory working knowledge, or even cursory acquaintance of the literature in the philosophy of science argued by philosophers of science who specialize in the critical examination of the assumptions and rationales currently prevalent in scientific inquiry, whether it be in the natural or social spheres. One could reasonably surmise from this that the views on scientific theory which Parsons assimilated during his intellectual apprenticeship, so to speak, were largely uninformed and lacking in the requisites of conceptual clarification and consistency, thereby rendering what he has presented in his meta-theoretical and substantive works somewhat logically untenable. Presumably, the reasons for such an argument would invariably take the following line. The major influences (Pareto, Weber, Freud, Durkheim, Henderson, Cannon, Whitehead, Marshall, etc.) on Parsons' thoughts
labored under misconceptions of what form scientific theory and explanation should take and that Parsons inherited these deficiencies and brought them into sharp relief in his writings by fusing his personal views on scientific theory with those he acquired. This may very well be the case but an extensive critical analysis of what Parsons has produced would supply, I should think, far more definitive and constructive answers. It may be pointed out, moreover, that guilt by association is not enough in tracing out the evolution of an individual's principal ideas and formulations.

At all events, Parsons' early intellectual contacts and labors were dauntingly vast and diverse and carried inestimable weight in his subsequent theoretical formulations in sociology.
CHAPTER THREE

THE LOGICAL STRUCTURES OF SCIENTIFIC THEORY
AND EXPLANATION

Before I proceed to carry out the bulk of my plans, I should like to discuss in this chapter at considerable length what I take to represent scientific theory and explanation, be it employed either in the natural or social sciences. Suffice it to remark, it is in accordance with these general comments on theory and explanation that I shall appraise Talcott Parsons' main contributions in sociological theory and in his discussions on metatheory. However it should be noted that much of what I wish to say about the structure of theory and some of the cognate issues involved in the philosophy of social science will not be included for the chief reason that it will not immediately touch upon the problems related to the objectives of this study.

It is important to note here that only for purposes of throwing an unobstructed light on the diverse characteristics of scientific theory and explanation - and no other - I have rather sharply distinguished the notions of theory and explanation far beyond what they in actual fact display. Hence, my reasons for dividing this chapter into two main sections, namely,
The Structure of Scientific Theory and The Structure of Scientific Explanation, the third section General Concluding Remarks includes observations on what I consider outstanding implications which flow from the adoption of a hypothetico-deductive mode of theory. I would like to state at this stage that theory and explanation are closely interlaced; they are not sharply separable, though they are distinguishable for analytical purposes. For viability, each requires the other.

A. The Structure of Scientific Theory

It is a commonly held assumption among scientists and philosophers of science that any discipline which purports to call itself scientific must have a theory or even a variety of theories which contrive to explain phenomena. Naturally, a theory, supposedly belonging to a discipline, deals primarily with phenomena of a certain kind which that discipline assigns to itself. Broadly speaking, then, sociology is a field of inquiry which endeavors to explain and analyze the structure and operations of a society and the individual's interaction with others within a social context. However, as a superficial inspection of current contributions in sociological theory discloses, the hypothetico-deductive mode of theorizing in science, among much else, is perhaps the most logically sound and readily capable of undergoing empirical test in
contrast to virtually all types of theories that presently obtain in both the natural and social sciences, though, in the case of the latter, its utilization has been, to be sure, exiguous and in many instances disappointingly misunderstood.

Understood in most general sense, a scientific theory is a system of deductively interrelated general statements (or general laws, if one prefers) (1) which systematically unify knowledge about certain properties of the natural or social world. (2) As well, a theory is an indispensable foundation for one of the paramount tasks of science: that of explanation. (3) The general statements embodied in a theory are employed along with certain descriptive statements and the elementary canons of logic to furnish explanations of a vast diversity of empirical phenomena which are considered to be specific manifestations of, or perhaps more accurately put, deductions from, these general statements. A theory is always antecedent to observations and experimentation. The latter procedures, whether undertaken in a formal or informal manner, are significantly performed only in terms of a general idea of how the phenomena operates which, to all intents and purposes, constitutes a theory. (4) Thus, in a scientific framework, a theory functions as a guideline, as it were, in that it facilitates the observer's selection of the pertinent observational material from an infinite mass of events and objects that confront the eye. This contention is given rather
vigorous expression by the French physiologist, writing on scientific inquiry in the mid-nineteenth century; one should not demur at this, for every word in undeniably applicable to contemporary discussions on the nature of scientific theory and research strategy.

It is impossible to devise an argument without a preconceived idea; devising an experiment we said, is putting a question; we never conceive a question without an idea which invites an answer. (5)

Though writing in a philosophical vein, Alfred A. Whitehead, propounds essentially the same type of argument when he pointedly stresses that:

A great deal of confused philosophical thought has its origin in obliviousness to the fact that the relevance of factual evidence is dictated by theory. For you cannot test a theory by evidence which that theory dismisses as irrelevant. (6)

To be sure, the empirical world itself is mute. It does not say anything about itself to human beings. Roughly paraphrasing William James' famous statement, the empirical world as such is a blooming, buzzing mass of confusion. But sense is made out of the world when we use either implicit or explicit theories about how the world operates. Thus, it is only in terms of a theory that we allow empirical reality to speak to us, as it were, fundamentally owing to the fact that a focus is directed at a specific region of the world and as such it demarcates this region from the infinitude of events and objects that seemingly crowd upon our attention. The empirical world, then, either supports or confutes our theory, thereby forcing
us either to retain the theory if it is supported or revise our theory, our series of assumptions about how the world works, if it clashes with the evidence. In any event, two further significant issues are to be noted in this connection: those of descriptions and views of the world couched in common everyday language. Let us first consider briefly descriptions. In broad outline, descriptions - which, incidentally, are neither theories nor explanations - are verbal replacements for pictures and demonstrations of certain events and objects. (7) To describe something means to dwell on certain aspects of a phenomenon. Since, to be sure, descriptions are characterized by this obvious selectivity in their representation of the events and objects they presumably refer to, they are almost invariably regulated by a theory or even a number of theories, frequently unacknowledged by the describer; moreover, I would maintain that precisely identical comments hold for measures such as classification, categorization, codification and simply the development of an armory of concepts to be employed for various intellectual purposes. For, all these activities can only be undertaken in the light of an array of implicit empirical regularities or general statements - in a word, a theory. (8) Clearly, then, descriptions and such-like automatically presuppose theories of some sort, and not the reverse as is so often presumed in social scientific and even, though to a minor degree, in the natural scientific circles. (9) Not
only are these considerations applicable to scientific endeavors, they are also of immense importance in the appraisal of certain utterances made in ordinary language which embraces, among other things, a plethora of explanatory and descriptive statements, usually erroneous or exceedingly oversimplified if they are viewed in the light of acquired scientific knowledge and the canons of proper logical discourse. (10) Undoubtedly all this serves to emphasize how the formation and advocacy of deductive theory as a fruitful and reliable scientific instrument is inspired and sustained by certain ineluctable aspects, though frequently not recognized, about how we all of us use a language. (11) Notwithstanding his predominantly psychological orientation toward the resolution of problems dealing with human perception and cognition, William James aptly throws considerable light on this facet of cognition, particularly stressed in scientific thinking:

But the moment one thinks of the matter, one sees how false a notion of experience that is which would make it tantamount to the mere presence to the senses of an outward order. Millions of items of the outward order are present to my senses which never properly enter into my experience. Why? Because they have no interest for me. My experience is what I agree to attend to. Only those items which I notice shape my mind - without selective interest, experience is an utter chaos. Interest alone gives accent and emphasis, light and shade, background and foreground - intelligible perspective, in a word. It varies in every creature, but without it the consciousness of every creature would be a gray chaotic indiscriminateness, impossible for us even to conceive. (12)

(Emphasis in original)
Without fear of cavil, it could be stated that what James terms a "selective interest" roughly coincides to the notion of a "theory" in the context of this chapter's discussion. In the large, it is clear that theorizing takes place on many levels of sophistication and abstraction and that it is an indispensable element in our knowledge about and observation of empirical reality.

As general statements shape a logical order out of a broad diversity of empirical events, a theory, in a much similar fashion, produces a logical order out of a number of disparate, though related, general statements about the world. It almost goes without saying that any logical arrangement of statements constitutes in effect a deductive system. This, it is to be noted, permits the explicit unravelling (or deduction, to be more precise) of the diverse relationships possible between the properties of the world which are contained in the general statements.

It can be seen without too much difficulty that one of the most striking advantages to be accrued from employing such a theory is that it greatly facilitates the discovery, as it were, of other perhaps previously unknown empirical regularities. The "discovery" is engendered by deduction. Doubtless another advantage lies in the fact that this type of theory is an immeasurably helpful testing device by means of which a
series of general statements belonging to one particular system (or a theory) can be empirically tested when one statement of that series is subjected to empirical test. How does this occur? As noted earlier, each general statement is related to another in the theoretical system, if such a locution be allowed. The essential point to note, then, is this. The evidence which is required for the assessment of one general statement constitutes evidence as well for the assessment of a number of general statements comprising the theory of which the general statement undergoing empirical test is an integral part; in other words, each general statement of a theoretical scheme is logically inter-connected, hence, what is empirically relevant for one is almost invariably relevant for the others.

Thus, viewed in a much broader perspective, a scientific theory is an inestimably valuable aid to the frail human mind which can only retain a depressingly fractional amount of what it comes across in its commerce with the world. To repeat briefly what has been previously stated: a scientific theory systematically unifies knowledge about a certain type of phenomena, e.g., chemical, physiological, social, psychological, etc., in the form of general statements (or general laws) which enable one with the appropriate logical measures and unambiguous definition of key terms to deduce a host of disparate statements about specific events that have occurred, are presently
occurring, and will occur in the empirical world. In other words, a theory furnishes feasible explanations of innumerable concrete empirical events. Accordingly, a theory empowers an individual to possess relatively more knowledge about the multitudinous facets of a particular type of phenomena without the burdensome task of committing to memory innumerable discrete facts and low-level generalizations that may obtain in an area of study at any one time. The process of deduction is the crucial element in this regard. And needless to say, that which can be deduced need not be memorized. It is this characteristic, incidentally, which renders hypothetico-deductive theory largely parsimonious or simple, qualities which are widely approved of in any theory. What would this mean? The simplicity or parsimony of a theory ensures that any superfluous features of the theory, its concepts, its laws (postulates and theorems) or perhaps needlessly complex formulations which are opaque to the understanding are obviated. (13) This arises from the conviction in scientific thought that a parsimonious explanation of a phenomenon is in all probability much more "true" or "fitting" in the light of the theory from which it is derived and the empirical phenomenon to which it is applied without conflict. Simplicity or parsimony are not bound by the rules of logic; they are extra-logical or perhaps, more accurately phrased, conventional. Though speaking with reference to questions of importance in the field of physics, Ernest Nagel
points out that the criterion of simplicity in theory assessment is at base a conventional rather than a strictly logical factor:

...the function will, in general, be required to have a relatively 'simple' form, even if the 'simplicity' tacitly demanded cannot be articulated precisely may be almost a psychological matter, and is likely to change as mathematical techniques for solving differential equations improve. (14) [My emphasis - E.R.G.]

Parsimony, in addition to being an intrinsically desired characteristic of scientific theory, possesses an instrumental value. It may serve, I would believe, as a mediate criterion for determining one of the cardinal criteria in the assessment of a scientific theory: that of utility.

Utility, too, is in large part a judgmental procedure. In essence, it is this. The more one theory explains with as few assumptions as possible, or the larger the range of its applicability in the pertinent phenomena it is assigned to deal with in the empirical world without clashing with it, in comparison to competing theories dealing with the same type of phenomena, the more useful, the more fruitful, it is as a scientific instrument. Another way of construing the conception of utility, I think, is by using the terms "fit-ness" or "suitability". Thus, theories are evaluated according to how and to the extent they fit or suit the empirical realm they purport to illumine. The notion which we would normally call "truth" is altogether extruded from the purview of scientific thinking and its vocabulary. It becomes evident that the
conception of "truth", that is, generally speaking, the irrevocable congruence of statements about the empirical world with the pertinent empirical events, is indeed a phantom and gives purchase to a host of grossly misleading notions about scientific thought in general. What it would mean to elaborate on this supposed incongruence, as this notion of "truth" implies, is not at all clear and, if I may say, beyond hope of intelligible clarification, let alone allowing for the specification of the congruence because of the basic misconception of scientific enterprise it tacitly carries. In scientific thought, it is generally assumed that the properties of the empirical world are continuously but imperceptibly undergoing change with the march of time. The best one can do is proffer a tentative account of the manner in which certain regions of the empirical world operate. As a result, all we ever have at any one moment are successive approximations. (15) To be sure, this is all that theory endeavors to furnish. This being the case, there is in principle, an infinity of perspectives (or theories) from which successive approximations can be achieved with regard to giving accounts of certain aspects of empirical phenomena. The criterion of utility is almost invariably invoked as a selection device for the purpose of casting light on as much empirical reality as possible with a minimum number of assumptions as possible. As Milton Munitz in his book *Space, Time and Creation* notes in this connection:
Theories are apt or fitting but they are not as such true, where truth is taken to mean "correspondence" of symbol and existence. Indeed, to speak of fittingness can itself be a misleading analogy. In the case of a suit that is made to fit a man, we can measure and describe the body of a man independently of the suit. But in the case of theories, as in the case of languages, it is meaningless to think of nature as possessing its own code which might in principle be explored independently of our symbolism and a test carried out to see which one of our human devices most accurately "matches" the "real" one. Taking seriously the symbolic and constructive character of the theories of science means reorienting our conception of truth as a goal for science. It means giving up the "spectator" conception of knowledge and the "one-shot" criterion of adequacy." (16)

It is plain, then, that scientific theory can only be properly assessed in terms of its utility, its ability to subsume a large range of phenomena without encountering any conflict with the observations that are undertaken. "Truth", in its strictest sense, is consigned to the truth-tables of logic, where the analysis of statements is undertaken regardless of the changes occurring in the empirical world. Theories are useful, apt, or appropriate; "truth" is irrelevant. On the surface, it certainly appears that the much reviled philosophical orientation of pragmatism is much more germane to scientific thought than it originally was made to seem by its unrelenting assailants in scientific and philosophical circles for the past half century. (17)

These considerations bring us to the ubiquitous, though often ill-understood, question: how does theory come about? In essential respects, a theory is an adventurous intellectual
contrivance to affirm something of significance about the empirical world. It is, above all, a hunch, a guess, call it what you will, which is literally imposed on certain phenomena which hold an interest for the theorist. A theory is tried on for size, so to speak, to be judged by its capacity to account for a wide variety of empirical data without refutation. This point of contention is given vigorous expression by Karl R. Popper, who argues that

...scientific theories are not the digest of observations, but that they are inventions - conjectures put forward for trial, to be eliminated if they clash with observations;...

Undoubtedly, a scientific theory in its initial stages of formation is as much a free creation of the mind as, let us say, a painting or a poem. It is only after the theory has been devised and employed that the austere imperatives of rigorous conceptual clarification of the theory's logical structure and empirical testing procedures have been applied that it acquires a significance or notoriety in the scientific sense. It is notable that a question regarding the origins of a scientific theory merely satisfies curiosities for it is above all an empirical issue; the results of the application of the theory to the empirical world with which it concerns itself, that is, whether the data clashes with it or not, constitutes the main question that should be asked of a theory's worth; the results, not the genesis of a theory are of scientific consequence. As a result, to evaluate or defend a theory in terms of its origins is to commit the genetic fallacy.
From the foregoing remarks, another inescapable conclusion clearly comes to the forefront. To argue, as a considerable number of social scientists have, that a scientific theory is an inevitable resultant from an accumulation of numerous empirical observations is indeed vacuous. To do so implies that there is a discernible order inhering in empirical phenomena, prepared, as it were, for our sighting and recording; all we must do is simply look. As noted earlier, this is a completely erroneous conception of scientific endeavor, if not of human thinking in general. It is we who read order and meaning into empirical reality and not the reverse such that we read out the order and meaning embossed, as it were, in empirical phenomena. As yet in the history of scientific thought, there has been no suggestion of a series of rules or procedures as to how one can proceed from an aggregate of discrete particular observations to general statements (or laws), a situation which challenges those who wish to maintain that general statements (or theory) as it were evolve from a mound of concrete empirical observations. Even if we suppose for purposes of the discussion that such rules could be ushered forth, it could always be relevantly inquired why were such and such observations made and why not others. The rejoinder would invariably be couched in terms of a presupposition, of an unacknowledged theory or theories the observer had borne in mind unless he engaged in random selection which would then have very little, if any, theoretical
significance. However, it is safe to say that rules for the formulation of general statements from observations is logically impossible and attempts to do so are, I believe, utterly fruitless and misconceived. To maintain the "observation-to-theory" sequence of theory-construction inevitably entangles one in the coils of the classical problem of induction which, in principle, does not admit of any tenable logical solution. (19)

At this point, the question as to how a theory is evaluated in the light of empirical evidence naturally arises. To begin with, it should be noted that virtually all scientific theories assert far more about the world than can ever be directly tested. (20) That is to say, the general statements and the theoretically assumed entities contained therein do not admit of any direct test by virtue of their highly abstract nature. What occurs when a theory is said to be subjected to empirical test is that the specific implications derived from the general statements are examined; these implications are characterized by strictly existential statements which then provide substantial grounds for asserting that the general statements, i.e., the theory, are empirically tenable or otherwise. The theoretically assumed entities (or concepts) inhering in the general statements of the theory are connected with concrete empirical events in a reliable fashion by a number of procedures. These may take the form of nominal definitions, ostensive definitions, or conditional definitions,
the particular theory's level of abstraction usually determining the type of procedure to be used. (21)

A theory is designed, before all, to be refuted (or to be disconfirmed) (22) not "corroborated" or "confirmed" in the typical sense of these terms. For, to be sure, one can almost invariably confirm any theory without undue difficulty if the only empirical evidence ushered forth is congenial to the theory while other less congenial or even conflicting evidence is, either intentionally or unintentionally passed over. Thus, along with utility, another criterion to be employed in assessing the value of a theory is its ability to undergo tests of falsification. (23) If a theory has withstood numerous attempts at decisive disconfirmation, it can be reasonably asserted that the theory is firmly bolstered by the available evidence; in other words, it displays favorable test results - it is then "confirmed" or "corroborated" in this special sense. As a brief point of elaboration, it is to be noted that a theory has been corroborated or confirmed when these terms are understood as short-hand expressions for lengthy, cumbrous locutions such as "the general statements clash or conflict with the empirical observations" or for negative-sounding, and largely unclear terms as "disconfirmation" and "disprove", or for easily misconstrued words such as "refutation", "confutation" or "falsification"; however, it may be noted that the latter terms, more than any other terms mentioned here, if they are
correctly understood, appositely describe the processes employed in the assessment of scientific theories. In consequence of this, a theory is amenable either to elimination or modification as a useful scientific device when the divers implications of the general statements clash with the pertinent empirical observations; that is to say, a theory's general statements are disconfirmed in some particular way. Whether a theory is to be eliminated root and branch or perhaps strategically altered is usually determined by the degree to which the general statements and their implications conflict with the pertinent empirical observations. However, much of this depends on the scope of the theory, the range of phenomena it encompasses. It may be laid down as a rough and ready procedural rule, drawn from previous theoretical endeavors in the history of science, that the larger the scope of the theory, the less definitive the favorable test results and the more circumspection is needed to be exercised in either the retention or the alteration or the elimination of the theory; the smaller the scope, the easier it is to confute rather decisively the theory and perhaps abandon it altogether. The paramount reason for these degrees of empirical test among theories of varying levels of abstraction is that the higher the level of abstraction - or perhaps appropriately phrased, the level of universality - the further it is situated from the present testing levels or capacities of the science in question such that the verificatory procedures tend to be relatively
incomplete and indecisive, calling for discretion on the part of the theorist in the interpretation of the test results for the theory. (24) On the other hand, theories characterized by a low level of abstraction are frequently testable on account of their minute divergence from the empirical phenomena they purport to take into consideration; in a word, there is less vagueness regarding the phenomena which the theory refers to.

There is one further point of importance concerning scientific theories in general. Contrary to what may seem to be intuitively evident at first glance, theory and fact (roughly speaking, a concrete statement about the empirical world which is verifiable) are not that different in kind. A theory is not necessarily an attenuated, uncertain set of general statements somehow referring to empirical reality, nor is a factual statement, which refers to a concrete empirical event, characterized by a conclusiveness bordering on certainty. As noted earlier, a factual statement about a particular empirical event is always construed in terms of the general statements (or laws) which indicate what type of relations one is expected to find in specific empirical events. On the other hand, general statements, which spell out certain relations between theoretically assumed entities (or concepts occurring under certain delineated conditions, are only of major moment when the observations of concrete empirical phenomena sustain them. Each type of statement, so to speak, nourishes the other.
In consequence of these considerations, it would seem accurate to say that the proper difference lies between statements of a general open-ended sort and those statements regarding specific empirical occurrences.

Before I continue further with my discussion, I should like to ground several of the preceding general considerations in a fairly simple illustration of a theory which evinces the essential characteristics of a scientific theory. Whether the theory is empirically significant or reliable is not the preeminent concern here; the structural features are the important facets to note.

For all human groups in Canada involved in tasks to attain certain specified objectives:

(a) The higher the degree of impersonality, the higher the degree of hierarchical structure.

(b) The higher the degree of the complexity of rules, the higher the degree of impersonality.

(c) Therefore, the higher the degree of the complexity of rules, the higher the degree of hierarchical structure.

Looked at from a conceptual standpoint, there are several things to be remarked about the basic structure of a scientific
theory. The logical skeleton of a theory is characterized by its approximate congruence with an "uninterpreted axiomatic system" found in Euclidian geometry (25) or the structure of a syllogism in the first figure. (26) As regards the latter notion, take into account the following example

(a) m p
(b) s m
(c) s p

In effect, all we must do is substitute the primitive terms of this syllogism for the italicized concepts in the above miniature theory to discern almost identical structural patterns which permit the utilization of the elementary techniques of logical deduction to unravel a latent empirical relationship (c) inhering in the two explicitly presented empirical relationships, (a) and (b).

It will be seen in short order that a theory is comprised of an aggregate of concepts and, as noted earlier, general statements which can be termed postulates (hypotheses, or assumed relationships) and theorems (relationships deduced from combinations of postulates). Hence, the appellation hypothetico-deductive theory. Several comments then, are in order concerning each of these two essential characteristics of scientific theory as such.
One of the fundamental components of a theory is a conceptual scheme. It is merely a set of concepts which are used in the theory. The concepts indicate the area of the world which the theory designedly refers to. More specifically, concepts are class labels or symbols, pointing to abstracted aspects (or properties) which constitute the class in question. Concepts are simply general words for, they are applicable to all instances of empirical phenomena which evince the referred-to aspects (or properties). Coeval with these characteristics, concepts may pertain to objects (or things, such as stones) and the properties of objects (small - the stone is small). As well, concepts refer to events (objects or things in movement or action) and the properties of events; an event is by its very constitution an abstraction to begin with and is exemplified by such cases as "to walk" or "to write" and their properties which could be, say, "briskly" and "furiously". It is to be noted, moreover, that concepts which refer to objects and the properties of objects are in large part typified by nouns and adjectives while concepts pertaining to events and the properties of events are indicated by verbs and adverbs. Most concepts, it may be affirmed, are one word summarizations of countless empirical observations but partially devoid of meaning on account of its de-emphasis of the concrete specificity of an empirical phenomenon. Although concepts may be similar or different in comparison with each other, they cannot, in principle, be logically related as let us say general statements.
(propositions, laws, and such-like). The following argumentation accounts for this development. Relations between objects or events - to which concepts refer - can only be said to hold when there are statements, that is, linguistic structures containing legitimately arranged subjects, verbs, and objects or complements. In effect, a statement is the relation obtaining between the objects or events which are under consideration. Broadly understood, a relation occurs when a statement about an object or event (or property) is made such that a reference to another object or event (or property) is also made. Take note of the following statement: "All sociologists are giants." Here, "sociologists" and "giants" are obviously the object and property related. As respects "deduction" - or to use a looser phrase, logical interrelation - it can only take place between a set of statements with one or more common points of reference (that is similar concepts).

It may be repeated once again in succinct summary form that concepts are not logically related mainly because:

(a) they only refer to abstracted aspects of certain empirical phenomena without any reference to other concepts; they are restrictive in scope, confined to themselves.

(b) they are not statements - hence, not relational; this precludes deduction, or if one may, logical interrelatedness.
(c) they constitute the basic units of a theoretical statement which is relational; without them, no statement of any kind could be made.

(d) they (as will be explicated) spring from within a theory or can be arbitrarily constructed, without reference to other theories or concepts.

Almost invariably, concepts derive their basic meanings in terms of the theory in which they are embedded, (27) and are usually bestowed a greater precision and clarity, if it is so required, by the utilization of operational techniques which were lightly touched upon earlier in this discussion. Clearly, an enumeration of discrete concepts cannot logically precede the formation of a theory or offer anything positive in the formulation of one. Concepts and general statements go hand in hand at all times, during theory construction, testing, and alterations.

If, however, it is still argued for some reason, that concepts are anterior to theories, then either one of two unpalatable circumstances, I think, obtain. One can be that the concepts are acquired in a random fashion. But, surely, this would render the concepts bereft of any significant theoretical meaning, hence, theoretically useless owing to the condition that they are not directly associated with any theory; they would appear to be very much akin to Plato's Ideas or Forms which appertain to the quintessence of things found in the
physical and mental worlds. In this context, concepts are affixed with a significance which they cannot conceivably possess, it seems clear, unless a welter of unacknowledged theoretical assumptions are being made. By partaking of such a procedure and, moreover, maintaining that something of great moment about the empirical world has been said, one is impelled to encroach on the infertile territories of metaphysics which, to be sure, are slightly beyond the pale of scientific thought. Another situation, which is so often the case, pertains to the distinct possibility that concepts are put forth with a vague theory which has not been rendered in any way explicit; it is in the light of this implicit theory that the concepts secure their meaning. But the onerous chore of ferreting out the pertinent general statements (of a theory or of a number of theories) still remains. Attempts to dredge them up are at best ingenious guesswork with, of course, its many attendant and frustrating vicissitudes. Though it would seem that the latter possibility offers faint promise of some illumination as regards the meanings and use of a set of concepts in scientific discourse, both, in my view, are still consummately unsatisfactory ways of constructing and sustaining a scientific theory. Thus, in view of the preceding considerations on concepts and their relations with theory, I would go so far as to say, perhaps brashly, that "to theorize from above", that is, to formulate concepts at the outset and then incorporate them into a later-developed theory is well-nigh an impossible task to perform since one can never know with any
degree of reasonable certainty — a necessary condition to be sure — whether or not the concepts, generated from an unacknowledged framework of general statements (or theories), are contradictory to each other, irrelevant to the questions explicitly at hand, tautologous, or empirically trivial, that is to say, they are too general to be of any fruitful empirical application. Moreover, this type of approach bears the unmistakable traces of an inductivist standpoint which, as we have earlier seen, is singularly indefensible.

In the crude theory I have sketched above, the concepts are as follows: "groups", "impersonality", hierarchical structure", and "complexity of rules". With respect to the notion of "variables" which is frequently mentioned in theoretical contexts, I would say this. Quite in general, variables are comprised of concepts coupled with a quantitative locution such as "degree of", "rate", and "incidence of" and such-like. In some instances, however, a concept alone constitutes the variable; a case in point would be the following: "In Canadian society, all adults exact respect from children." — here, "adults", "respect", and "children", characterize this particular conception of a variable. A particularly valuable distinction of the types of variables that obtain in scientific discourse is proposed by George C. Homans. (28) He suggests that variables may be conceived either as (1) continuous or (2) two-valued. In regard to the continuous variables there is
an arrangement of properties in a rectilinear fashion; otherwise phrased, if one variable (the independent variable) either increases or decreases, then the other variable (the dependent variable) respectively increases or decreases. They are characterized mainly by the quantitative locutions I have considered in the foregoing. Concerning the two valued variable, on the other hand, it is indicated that there is a special entity or property which is either manifested or absent under certain conditions; examples of this would be notions such as "industrialism", "solidarity", and "the division of labor". Referring to my theory illustration of a scientific theory, one will note that the variables are continuous: "degree of impersonality, degree of hierarchical structure", and "degree of the complexity of rules". From all this, one conclusion becomes evident. Though a conceptual scheme is a necessary constituent of a theory, it is in no wise sufficient. A conceptual scheme becomes merely another insignificant agglomeration of disparate concepts if it is not incorporated in a series of general statements. (29)

General statements (or general laws) are empirical regularities which are assumed to be invariant. (30) Broadly speaking, a general statement is a relationship obtaining between a number of properties of the empirical world which occur under certain specified conditions. At the same time, it embraces a variety of phenomena which are considered to be specific manifestations
of it. The fundamental form of such a statement conforms to the following structure: "In all cases when conditions of kind F are realized, conditions of kind G are realized as well." (31) By virtue of its universality, it does not pertain to any particular temporal or spatial locus as, let us say, an accidental empirical generalization. This means that it states a relationship obtaining between certain properties of the empirical world under strictly delineated conditions in any spatio-temporal dimension. A theory is omnitemporal and omni-spatial. (32) To make this point more tangible, let us examine in an abbreviated manner Galileo's law of free falling bodies. It is postulated that: Whenever a body falls from a stationary position in a vacuum on or near the surface of the earth, the distance it travels is \(16 t^2\) feet in \(t\) seconds. This statement along with the requisite descriptive statements of anterior boundary conditions subsumes (or explains) a welter of instances involving free falling objects - i.e., from an egg dropping from the hands of a lethargic cook to an artificial satellite hurtling through the earth's atmosphere. Particular instances of this general statement, that of Galileo's law of free-falling bodies, are indeed immense. Although an accidental empirical generalization is on a superficial glance structurally similar to a general statement, it is vital to note other patent differences in context and use. An illustration, though slightly nonsensical, of an accidental general statement would be something as follows: "All the white pages in this study are
poisonous". In terms of the above pattern of a general statement, \( F \) would be the condition of a white page in the study and \( G \) would be the property of a white page being poisonous. However, the generalization refers to a specific event which unquestionably takes place at one particular place, at one particular time. In consequence, it cannot be extended to take into account other phenomena that occur at various locations and at various times as a general statement undeniably can. Besides this elementary distinction between an accidental empirical generalization and a general statement, there are two other distinctive factors which are equally important. The first is that a general statement (or law) is able to sustain contrary-to-fact conditional statements which are generally characterized by the following structure: "If \( A \) were (had been) the case" then \( B \) would be (would have been) the case", where in fact \( A \) is not (has not been) the case." (33) Let us illustrate this type of statement with reference to Galileo's law of free falling bodies. Suppose a twenty thousand ton slab of gold (or a one pound package of feathers) were inadvertently dropped from atop the Empire State Building in New York City; hence, If a twenty thousand ton gold slab (or a one pound package of feathers) were dropped from the 102nd storey of the Empire State Building, then it would take the gold slab (or package of feathers) to descend the one thousand feet in \( X \) seconds (or it would have travelled \( 16t^2 \) feet in \( t \) seconds). In a similar vein, the second factor is the utilization of subjunctive conditionals; A general statement
(or law) would give support to such a statement. A subjunctive conditional is largely typified by this sentence pattern: "'If A should come to pass then so would B', where it is left open whether or not A will come to pass". (34) Concretely, we would have the following statement: "If a twenty thousand ton gold slab (or a one pound package of feathers) were pushed off the 102nd storey of the Empire State Building in New York City, it would descend the one thousand feet to the pavement in X seconds (or it would travel $16t^2$ feet in $t$ seconds)". It is clear that the accidental generalization, "All the white pages in this study are poisonous" could not maintain a contrary-to-fact conditional statement such as the following: If this white page (presumably any white page not in the study) were bound in this study it would be poisonous. Similar comments are applicable to a subjunctive conditional, i.e. "If this page (any white page not in the study) were a white page in this study, then it would be poisonous". It may further be noted that a general statement functions as a foundation-stone for scientific explanation, as we shall soon see. Conjoined with a description of the antecedent boundary conditions, the general statement provides an explanation for a specific empirical event which is without doubt a specific manifestation of the general statement. (35) An accidental empirical generalization, however, could not provide explanations of any kind of a specific empirical events for it is itself a specific empirical event. Let us take our example
once again to point out this difficulty: "All the white pages in this study are poisonous." To explain that a white page in this study is poisonous by alluding to the generalization that all white pages in this study are poisonous constitutes a circular argument.

General statements play vital roles in all scientific explanations of specific events. But it may be properly inquired what explains a general statement? The immediate answer is another combination of general statements, frequently of a much higher level of abstraction, which permit the deduction of the general statement in question. (36) The process of explaining general statements can continue indefinitely until the level of abstraction almost reaches the point of triviality or is drained of all empirical relevance. It is noteworthy that in the natural sciences, the theory of relativity represents the acme of this process whereas, in contrast, the social sciences, this point is not even distantly approached since the general level of theoretical abstraction is assuredly very low.

A word concerning the levels of abstraction of general statements is necessary at this stage of our deliberations. The level of abstraction of a theory's general statements can be adjusted to accommodate the level of the empirical knowledge that can be unequivocally mobilized by a particular discipline in order to perform significant empirical tests of their existential claims, as it were. Any confirmed theory (in the sense
mentioned earlier in this chapter) can only be denoted in theoretical significance by another theory which is of a slightly higher level of abstraction. It invariably encompasses the denoted theory and puts forth other, more universal testable claims on its own behalf. The history of science is fraught with instances clearly revealing that the currently advanced sciences (i.e. physics and chemistry) attained their eminent positions by gradually proceeding with theories from a limited to greater universality. The extent of a theory's level of abstraction is invariably a joint product of a cautious theoretical ambition of the theorist and the capacity of the discipline to provide advanced testing techniques which enable a relatively confirmed constellation of empirical knowledge to become available to theorists and researchers in their multifarious theoretical pursuits. In his volume, The Logic of Scientific Discovery, Karl R. Popper elucidates at some length on these crucial theoretical considerations which are largely judgmental, programmatic, and empirical rather than strictly logical as some may erringly suppose. He writes:

The methods of testing are invariably based on deductive inferences from the higher to the lower level; on the other hand, the levels of universality are reached, in the order of time, by proceeding from lower to higher levels.

The question may be raised: 'Why not invent theories of the highest level of universality straight away? Why wait for this quasi-inductive evolution? Is it not perhaps because there is after all an inductive element contained in it? I do not think so. Again and again suggestions are put forward -- conjectures
or theories -- of all possible levels of universality. Those theories which are on too high a level of universality, as it were (that is, too far removed from the level reached by the testable science of the day) give rise, perhaps to a 'metaphysical system'. In this case, even if from this system statements should be deducible (or only semi-deducible, as for example in the case of Spinoza's system), which belong to the prevailing scientific system, there will be no new testable statement among them; which means that no crucial experiment can be designed to test the system in question. If, on the other hand, a crucial experiment can be designed for it, then the system will contain, as a first approximation, some well corroborated theory, and at the same time also something new -- and something that can be tested. Thus the system will not, of course, be 'metaphysical'. In this case, the system in question may be looked upon as a new advance in the quasi-inductive evolution of science. This explains why a link with the science of the day is as a rule established only by those theories which are proposed in an attempt to meet the current problem situation; that is, the current difficulties, contradictions, and falsifications. In proposing a solution to these difficulties, these theories may point the way to a crucial experiment. (37)

Doubtless, the intricate articulation of theory with empirical research and vice versa scarcely needs stressing.

Throughout the present discussion on the structure of theory, the accent has been laid principally on the value and cogency of hypothetico-deductive theory and the reasons for them. We must now bring together the main points that have emerged in this connection.

(a) Hypothetico-deductive theory furnishes general statements (or laws) of such scope that explanations and predictions of a vast range of phenomena can be accomplished with fewer assumptions. This precludes any reliance on ad hoc (38) measures which are
exceedingly restricted and generally unilluminating for they are constructions of the moment to cope with problems of the moment.

(b) Additional relationships between certain properties of the world, previously unknown, are now possible through the process of deduction.

c) When one general statement (or law) of a series of inter-related general statements, in a word, a theory, is tested, there are implications arising therefrom which affect the entire series in some fashion; they may be, in varying degrees, confirmed or disconfirmed.

d) Hypothetico-deductive theory enables one to eschew numerous logical difficulties and other faulty assumptions which often adversely plague those who discourse on empirical phenomena, whether it be in the natural or social sciences. It impels one to become aware of his tacit assumptions and their implications and also the implications of his acknowledged assumptions.

B. The Structure of Scientific Explanation (39)

Theory and explanation are intimately interwoven strands; without a theory, explanation in a scientific sense is doubtful and without explanation, a theory loses its raison d'etre. Although both must appear together, they are not synonymous processes. A theory is exclusively a constellation of general
statements whereas explanation is constituted of statements describing specific concrete conditions and a general statement drawn from a theory. Thus, to affirm as George C. Homans (40) and Hans Zetterberg (41) do that theory is explanation and explanation is theory is a shade misleading. Fundamentally, explanation is deduction. An explanation is a form of argument in which the event to be explained is deduced from a general statement in conjunction with a number of statements describing the antecedent, boundary conditions. Thus, a scientific explanation is composed of three cardinal conditions. They are: (42)

(1) A set of general statements pertaining to invariant empirical uniformities. (43)

(2) A set of descriptive statements outlining specific antecedent, boundary conditions.

(3) A descriptive statement referring to a specific consequent condition which is derived (or deduced) from statements (1) and (2).

An explanation, then, can be conceived as a device which is divided into two main components - the explanans and the explanandum. (44) The explanans consists of a general statement (or law) plus a description of the antecedent empirical conditions. Meanwhile, the explanandum consists of a description of the specific empirical event to be explained (or to be deduced). In brief, then, an explanation is an argument in which the
The explanandum is deduced from the explanans. Since this argument is categorized as an explanation by virtue of a deductive subsumption under general statements (or general laws), it is appropriately nominated, by Carl G. Hempel, as the deductive-nomological explanation. (45) The term "homological" derives from the Greek word "nomos", for law: as it is, an explanation is effected by a deduction from a scientific law. Thus, quite justifiably, the ubiquitous query in both the common-sense and scientific worlds, "Why does this happen?" is capable of being construed as "In regard to which empirical generalizations (or general laws) and which initial conditions does this particular phenomenon occur?"

Clearly, without this type of explicit explanatory scheme, excepting a probabilistic model of explanation which is somewhat different in form - and does not concern us here - there can be no explanation of any mentionable kind, in science, natural or social; nebulous conjectures, intuition and ragged, vague attempts at explanation, seem to be the alternatives to this explanatory scheme in which an awareness and lucidity of presenting assumptions and empirical conditions is principally stressed and gratuitous errors kept to a tolerable minimum. Furthermore, it may be said that any explanation of a specific event, however nugatory, presupposes a general law - thus, a theory. (46)

In the light of the preceding remarks, a scientific explanation can be aptly characterized in the following
paradigmatic fashion: (*47*)

<table>
<thead>
<tr>
<th>Explanans</th>
<th>General empirical statements (or laws)</th>
</tr>
</thead>
<tbody>
<tr>
<td>( L_1, L_2, \ldots L_r )</td>
<td></td>
</tr>
<tr>
<td>( C_1, C_2, \ldots C_k )</td>
<td>Descriptive statements of antecedent, boundary conditions</td>
</tr>
</tbody>
</table>

Explanandum: \( E \) Description of the empirical event to be explained

Logical Deduction

It would indeed be instructive, I think, to present a concrete illustration of an explanation by employing the crudely conceived theory of groups which I adumbrated in the above discussion on the structure of theory. I shall include the entire battery of general statements - postulates and theorems - and a specific phenomenon I have selected to be explained in order to demonstrate the implications of an empirical finding on a theoretical structure and to amplify upon the interconnection of theory and explanation.

For all human groups in Canada involved in tasks to attain certain specified objectives.

(a) The higher the degree of impersonality, the higher the degree of hierarchical structure. (*general law*)

(b) The higher the degree of the complexity of rules, the higher the degree of impersonality. (*general law*)
(c) (Therefore) The higher the degree of the complexity of rules, the higher the degree of hierarchical structure. \( \text{a derived general law} \)

(d) The degree of the complexity of rules in the General Motor Assembly Plant at Oshawa, Ontario is high. \( \text{description of antecedent condition} \)

(e) Therefore, the degree of hierarchical structure in the General Motors' Assembly Plant at Oshawa, Ontario is high. \( \text{explanandum} \)

(Clearly, the definition of the concepts have not been provided; they are not really necessary for the purposes of the discussion. But the structural features and logical deductions are, I think, basically correct.)

Here, (c) represents the general statement (or law) which has been deduced from a combination of other general statements, namely statement (a) and (b); statement (d) represents the antecedent conditions describing a specific event. And, of course, statement (e) refers to the explanandum, or a description of the consequent conditions, the event to be explained. If empirical tests indicate that statement (e) does hold, then statements (a), (b), and (c) can be regarded as confirmed, that is, there are no falsifying instances. Should, however, statement (e) be confuted by the data, then statements (a), (b) and (c) are placed in jeopardy, calling either for their elimination or modification.
At this juncture, it is noteworthy to consider a significant variation of the deductive-nomological explanation; namely, the causal explanation. (48) It is essentially characterized by the assertion that a particular event has been about (or caused) by an antecedent particular event. This, however, can only be maintained if there is a general statement (or law) conjoined with the descriptive statements of the antecedent conditions permitting the deduction of an event which is asserted to be caused by the antecedent event. What is required is a general statement (or law) indicating that certain variables (or properties) are related in such a fashion that one changes or modifies when the other is characterized by certain properties coupled with a description of the antecedent conditions, mainly specifying certain temporal and spatial loci and noting that the events or objects are manifestations of the variables dwelling in the general statement. Through the processes of elementary logic, the resultant event is deduced. As Felix Kaufmann notes "...it is elliptical to speak of a cause of a given event without referring explicitly to the law in terms of which it is a cause of the event." (49) An illustration will aid in furthering comprehension of this argument. Let us state that for a particular individual, Leopold Smith, angered shouting increases every time his food is taken away from him when
he wishes to eat. An explanation of this event would entail
the following kind of argument: There are statements, let us
say, furnishing measurements, specifying the state of Mr.
Smith's emotional condition prior to his frustration and other
statements pointing out that he was deprived of food at a
time he intensely desired to eat. At the same time, there
is a general law spelling out the relationship that the
intensity of one's anger increases as his frustration is
repeatedly engendered. Roughly speaking, then, the event,
that angered shouting followed when the food was taken from
Mr. Smith is deduced from these statements of a general law
and antecedent, boundary conditions.

A further observation of extreme importance presents
itself at this phase. A deductive-nomological explanation
does not necessarily constitute a causal explanation. To
be sure, the empirical regularities embedded in the statements
(a) "The higher the degree of impersonality, the higher the
degree of hierarchical structure," or (b) "The higher the
degree of the complexity of rules, the higher the degree of
impersonality", of my example cannot be intelligibly affirmed
to provide causal accounts of anything. (50) A deductive-
omological explanation points out that a particular event
is explained by virtue of it being subsumed under a general
empirical uniformity. It should be abundantly clear, however,
that all causal explanations are of a deductive-nomological
character.
It is indeed pertinent to note that there is no substantial difference between the procedures of scientific explanation and prediction and postdiction. (51) So far as I can ascertain, these operations are characterized by an identical logical structure which I earlier considered. Whether an event has occurred, is occurring, or is to occur, it can be deduced from a general statement (or law) along with the necessary description of the initial conditions. As Gustav Bergmann observes in this respect:

In principle, prediction and scientific explanation are but two sides of one coin. One who can always predict knows all the laws, and conversely." (52)

Similar remarks also apply to postdiction (or retrodiction), as we shall notice in a moment. What does distinguish these three scientific procedures (as it were) from each other, however, is the purely pragmatic factor of time. To be conceived as a prediction an argument, at base, identical to the explanatory argument, must refer to an occurrence of an event in the future; that is to say, the event takes place at a time after the argument, elaborating the character of the expectation and the logical reasons for the expectation, is presented. With regard to postdiction (or retrodiction), the phenomenon to be explained must precede, (one second or a millenium as it were) the presentation of the argument which basically asserts that the event occurred on account
of a certain empirical regularity (a law) and certain initial, empirically concrete conditions. Thus, if one can explain an event, one can concomitantly predict or postdict it. What Robert Brown remarks in this respect in his book *Explanation in Social Science*, can scarcely be gainsaid:

The explanation and prediction are supported by exactly the same information, namely, the relevant generalization and the statement of initial conditions. Given this characterization of explaining and predicting in terms of laws, it is self-contradictory to say that we can predict the occurrence of an event but not explain it, or that we can explain its occurrence but not predict (or retrodict) it. (53)

If explanation, prediction, and postdiction are viewed as qualitatively distinct operations, considerable conceptual difficulties, I think, will inevitably come about. The provision of their respective logical structures such that they can be markedly differentiated is mandatory. interestingly enough, arguments of this nature have not been ushered forth as yet. (54) In my view, such arguments will not appear because the underlying viewpoints in the majority of instances rest on a number of blatant misconceptions concerning the logical character of scientific explanation or confusion with respect to probabilistic accounts and scientific explanations which are similar in several respects but saliently different in other crucial respects. To be sure, prediction and postdiction are not wild conjectures, endeavors at probabilistic accounts, or optimistic hopes
or intuitions about events to occur or about events that have occurred. Above all, they are disciplined procedures; they are conducted according to a definitive pattern of thought.

It would be well to devote several further considerations to the activities, so to speak, of description and explanation now that the logical features of scientific explanation have been explored. As indicated earlier in this chapter, a description (also, classification, taxonomy, etc.) represents a verbal replacement for pictures or demonstrations of empirical events; it is selective in its portrayal of the event which means that it is regulated by an implicit theory or a collection of implicit theories. In a manner of speaking, description is the handmaiden of theory. A description, in consequence, does not explain, predict or postdict empirical events. Hans Zetterberg aptly notes in this connection that

...a concern with taxonomy and descriptive studies does not furnish any explanations.

To know the labels of phenomena and to know their distribution is not to explain them. (55)

Let us further differentiate descriptions and explanation. For one thing, explanation is a logical process while description is a method. That is to say, an explanation focuses on the connection between properties found in the
empirical world; description on the other hand, only indicates the different properties that are noticeable in a given region of the empirical world. An explanation endeavors to answer the question why the particular empirical event is what it is. A description, however, states what the event is and nothing more.

Through its processes of logical deduction, an explanation permits one to select the pertinent empirical phenomena for examination primarily because it explicitly focuses on a restricted region of the entire universe of empirical phenomena. In effect, it contains an intrinsic demarcation property. In contrast, a description has no boundaries, its outer limits, if one may employ such a phrase, are fluid; description can be stretched into an ad infinitum exercise, particularly if the implicit theories upon which it lies remain completely unacknowledged. Virtually any one empirical event can be linked and implicated with every other empirical event in the universe. From these remarks, it appears that description as such does not constitute an enlightening scientific enterprise.

C. General Concluding Remarks:

If certain implications of the conception and use of hypothetico-deductive theory are pursued to their natural termination points, an intriguing, albeit arresting, circumstance becomes manifest. This conception of theory virtually precludes other modes of theorization from possessing any
structural validity - in the sense of logical adequacy and applicability. Scientific theory as such is wholly commensurate with hypothetico-deductive theory procedures. Other formulations called theories seem to be either subtle variations of the hypothetico-deductive model or are, to phrase it quite sharply, non-theories, merely aggregates of descriptive statements governed by a vague theory or collection of theories. Also detecting this curious, if not astonishing, property of hypothetico-deductive theory, Ernest Gellner expresses the main burden of the contention I have put forth here in a much more rigorous fashion:

"hypothetico-deductive method" is a misnomer. One can only speak of method where there is an alternative. But the only alternative to this way of studying things is not another way, but not studying them at all. For this "method" really means only thinking about things and then seeing whether what one had thought is true. (56)

Although the standpoint expressed above may seem stolidly uncompromising and a trifle too imperious, the essential point of the argument - and this is the most important factor - nevertheless steadfastly holds. Inasmuch as I can presently ascertain from the extensive literature in the social sciences and the philosophy of science and social science, serious damaging counter-arguments to this conception of scientific theory are as yet to be put forward for consideration. Perhaps if we view hypothetico-deductive theory as a set of conceptual injunctions and guidelines to think consistently and without covert bias about how the empirical world operates, the ostensibly distasteful, pre-emptive character of this approach could be somewhat abolished.
With these remarks on the structure of scientific theory and explanation firmly entrenched in our minds, we may now turn to appraise Parsons' arguments in his meta-theoretical and substantive writings.
A. The Analysis of a Theorist's Conception of Scientific Theory: The Manifold Problems and Possible Rationales

On superficial inspection, it may be seen that scientific theorizing is closely analogous in pattern and execution to the infinity of tasks that are carried out in the everyday world. Just as a carpenter who constructs abodes without continually explicitly acknowledging his multitudinous presuppositions about the materials and tools he uses or the techniques he brings to play in his work, except for, perhaps, tidbits of prosaic knowledge sufficient to enable him to communicate with others and also to continue his divers tasks, so also the theorist in any scientific discipline, who presumably devises and evaluates refined mental constructions, commonly called theories, about how certain segments of the universe operate need not necessarily know to any far-reaching extent the general logical features, rationales, and the intricate functions of scientific theory as such. These complex problems, it can be plausibly contended, are for the philosophers of science to wrestle with and to explore. To be plain, the theorist formulates theory and does not concern himself with
the discussions of its nature and uses in abstruse terms. In a sense, this is a legitimate position to take; for, to take another example, to walk one need not possess the knowledge of a kinesiologist, as to how the muscles and skeleton of the legs function when one takes a step, likewise a theorist in regard to the philosophical facets of a scientific theory.

There are, I think, several notable lacunae in these views which require elucidation and correction. Briefly they are as follows. For one thing, science is a self-critical and self-correcting enterprise, thereby, necessitating clarification of obscure points and inadequate rationales of the various theoretical schemes and procedures that may be employed in a particular investigation in any one of the scientific disciplines; in other words, science is a rational undertaking, always undergoing critical scrutiny. (1) For another, as previously noted, a theorist does not simply immerse himself in a certain sphere of study with a stock of hunches and assumptions about the phenomenon in question and then proceed with the arduous process of theorizing. To be sure, he formulates his theory in accordance with a constellation of general assumptions and models of what a theory structure should represent, however nebulously or sharply outlined they may be in his consciousness. It is clear that
it is these factors which in large part determine the general structural features of a substantive theory and its relative worth prior to its engagement with the pertinent empirical observations. (2) A theorist's conception of scientific theory may indicate, though in a rough and ready manner, what his substantive contributions will, in all probability, resemble and how they will fare before the somewhat austere court of scientific adjudication, mainly predicated on (a) the canons of basic scientific thought which, incidentally, hypothetico-deductive theory embodies in great measure and, (b) to a degree, with the available knowledge of the pertinent aspects of the empirical world. By and large, then, a theorist's conception of scientific theory is, a reliable bellwether as to what type of an approach can be expected in his substantive theoretical formulations which have been presented or may eventually follow.

Several further observations of a less important nature concerning the analysis of a theorist's notion of scientific theory, in my estimation, present themselves at this point. An inquiry of this sort may afford an apposite yardstick by means of which the theorist's substantive works can be measured in terms of their coincidence with, or conversely, divergence from the conception of scientific theory the theorist explicitly maintains. Thus, for the individual
theorist and critic, whether sympathetic or otherwise, a critical awareness of a theorist's fundamental assumptions and presuppositions concerning the nature of scientific theory seems mandatory for an adequate understanding of his substantive works, particularly with respect to their conceptual weaknesses and strengths, and their ability to confront in a meaningful manner the pertinent empirical data. As well, one could relevantly inquire whether a theorist's substantive theory conforms to the criteria of scientific theory he explicitly enjoins. I may mention, however, that considerations of this character, interesting though they may be, do not necessarily presuppose the criteria of adequate scientific theory such as those contained in the hypothetico-deductive mode of theory. But should they be interwoven with such criteria, an understanding of a theorist's various contributions, I should think would be immeasurably enhanced.

Besides the instrumental value of probing a theorist's metatheory in the manner just considered, this type of examination can be of intrinsic interest in merely answering the simple question: What is this theorist's conception of scientific theory? (However, it is surely obvious that this analytical endeavor can only be ventured if the theorist to start with, has alluded to, or elaborated upon his conception of scientific
theory in his written work and it may be remarked that Parsons has done so in unmistakably prolific and searching terms.)

Although Parsons, quite understandably - he is pre-eminently a theorist in a particular discipline, not a philosopher - has not written a journal article or a book exclusively devoted to a ranging discussion on the structural characteristics and uses of scientific theory as such, an analysis of his writings - both substantive and metatheoretical (3) - from 1935 to the present discloses a spate of widely dispersed and fragmentary passages in which a distinctly Parsonian conception of scientific theory is evidenced. In view of the scattered nature of Parsons' references to this problem, an attempt will be undertaken in this chapter to reconstruct and adumbrate the essential ingredients of his notion of scientific theory in chronological fashion from 1935 to the present. (4) In addition, an effort will be made to discern the modified and stable elements in Parsons' conception of scientific theory. Doubtless my approach will be selective in the highest degree. However, in order to forestall possible serious misconstruals and to insure a reasonably faithful exposition of Parsons' thought in this connection, I shall, generally, though not in all instances, refrain from major critical comment until after I have presented what I consider to be the basic features of Parsons' thinking on scientific theory in each of the relevant works - ten in number -
which I have culled from his vast bibliography of published works. Periodically — when I deem it necessary — I may interpolate brief, passing critical notes in the course of my exposition, indeed, much depending on the length of the exposition, in the sense that, generally, the longer the exposition the less critical comment during its elaboration in order not to disrupt an often tortuous train of thought and to eschew the risk of creating an imbalance of emphasis vis-a-vis other notions Parsons may also present. I am obliged to stress once again that here I shall only be concerned with Parsons' conception of the structure of scientific theory, not the lineaments of his substantive theory or other considerations pertinent thereto. These latter factors, incidentally, will be examined in part in the next chapter. It should be remarked that much of what I shall say in criticism and even exposition of each work I examine will no doubt be repetitious. But I take solace in the fact that for illumination and understanding of a theorist's prolific contributions over a period of time, repetition is an inevitability for scarcely any intellectual begins his deliberations anew each time he writes for wide broadcast.

B. Parsons' Early Polemic Against "Positivism"

Before tackling straightaway the aims of this chapter, however, it is decisive to realize for reasons of proper perspective and accuracy that Parsons maintained a rigorous
polemic (5) during the earlier stages of his theorizing career against what he termed to be the "positivistic view of things" in the social sciences. Neglecting this facet in any assessment of Parsons' notion of scientific theory and his substantive theory leads, I should think, to the distorted impression that Parsons' attempts at theory-construction stemmed from an intellectual orientation largely compatible with the natural sciences which, by all accounts, was not at all the case.

In a particularly telling and intensively argued essay called "The Place of Ultimate Values in Sociological Theory" (1935), (6) Parsons spells out in a detailed fashion his opposition to a positivistic approach for the social sciences. According to Parsons, positivism stresses a scientific orientation which primarily, nay exclusively, directs its attentions on the purely physical aspect of human behavior; that is, it points up the relation of a human being (or certain of its responses) to external events which can be observed without too much difficulty. This indeed was objectionable in Parsons' view for it blurred the indisputable fact that human beings were, at bottom, "active, creative, evaluating creature(s)." (7) Positivism denied the essential premise - which Parsons advocated - that the subjective factor (voluntarism, (8) as he later put it) of human behavior could be scientifically understood in the light of the values harbored by the individuals undergoing
investigation. Within the positivistic framework, ends, means, objectives, ideals, values, and norms, which constitute the subjective aspect of human behavior were conceptions and processes prohibited from appearing in explanations of it. The chief reason for this, it was frequently stated, was that they strongly savored of teleology; and teleological explanations, at all events, were unscientific in the eyes of positivism. But, to Parsons' mind, the subjective factor held an importance of the first magnitude; it was clearly something which captured the heart as it were, of human behavior and could not be discarded as though it were so much cluttering conceptual rubbish in scientific thought. It could not be given scientific short shrift merely because it could not be immediately conceived or translated in terms analogous to those employed in the natural sciences. Generally, the physical sciences which concerned themselves with "'inanimate' subject matter" (9) embodied the positivistic approach in its most thoroughgoing and offensive form. For these disciplines, positivism was entirely satisfactory but unquestionably inappropriate for the social sciences which, after all, dealt with human behavior. For Parsons, then, the most pernicious utilization of the positivistic orientation was manifested by the movement he called "radical behaviorism" which obviated altogether the necessity of studying the subjective component of human behavior and, instead, shifted its attentions on what
could be termed the observably identifiable variables of human behavior and the relevant aspects of the external world for a truly scientific explanation of human behavior. (10)

For purposes of amplifying these somewhat condensed remarks; I should think that several substantial quotations from Parsons may be allowed at this point; as Parsons pointedly writes:

The positivistic reaction against philosophy has, in its effect on the social sciences, manifested a strong tendency to obscure the fact that man is essentially an active, creative, evaluating creature. Any attempt to explain his behavior in terms of ends, purposes, ideals, has been under suspicion as a form of "teleology" which was thought to be incompatible with the methodological requirements of positive science. One must, on the contrary, explain in terms of "causes" and "conditions", not of ends.

Of late years, however, there have been many signs of a break in this rigid positivistic view of things. The social sciences in general have been far from immune from these signs, and in sociology in particular they have combined to form a movement of thought of the first importance. One main aspect of this movement has been the tendency to reopen the whole question of the extent to which, and the senses in which, human behavior must or can be understood in terms of the values entertained by men. (11)

One of the most conspicuous features of the positivistic movement just referred to has been the tendency to what may be termed a kind of "objectivism". Positivism, that is, has continually thought in terms of the model of the physical sciences which deal with an "inanimate" subject matter. Hence the tendency has been to follow their example in thinking of a simple relation of observer
to externally observed events. The fact that the entities observed, human beings, have also a "subjective" aspect has a tendency to be obscured, or at least kept out of the range of methodological self-consciousness. The extreme of this objectivist trend is, of course, behaviorism which involves the self-conscious denials of the legitimacy of including any references to the subjective aspects of other human beings in any scientific explanation of their actions. But short of this radical behaviorist position, the general positivistic trend of thought has systematically minimized the importance of analysis in terms of the subjective aspect and has prevented a clear-cut self-conscious treatment of the relations of the two aspects to each other.

Of course, the results of analysis of human behavior from the objective point of view (that is, that of an outside observer and the subjective (that of the person thought of as acting himself) should correspond, but that fact is no reason why the two points of view should not be kept clearly distinct. Only on this basis is there any hope of arriving at a satisfactory solution of their relations to each other. (12)

Like most Americans growing up in the social sciences since the war, my starting-point has been what may broadly be called the "positivistic" movement in these fields - the tendency to imitate the physical sciences and to make physical science the measuring-rod of all things I quite early reached a conviction of the "inadequacy of these current views /those of behaviorism and behavioristically-based "institutionalist" economics/. That conviction centered primarily on the vague realization that these positivistic theories somehow, by a kind of logical juggling rather than by empirical proof, were squeezing what I have here called the "value"-elements out of their interpretation of social life. (13)

In these passages, it is doubtless evident that Parsons' antipathy toward a theoretical approach emphasizing only the importance of immediately observable variables (that of the natural sciences as he viewed it) was unrelenting and sharp,
yet surprisingly conciliatory in that he accorded it a measure of considerable importance in furnishing accounts and descriptions of human behavior as the last quoted paragraph above unequivocally reveals. He regarded it as a vital part, but only a part. In short, Parsons wished to adopt elements of both theoretical orientations in one comprehensive framework with the dominant emphasis being placed on the subjective aspect of human behavior. (14) Aptly summarizing Parsons' early theoretical orientations, Edward Devereux points out the following pertinent considerations:

Although he was impressed with the principle of emergence and the element of indeterminacy implied in the voluntaristic postulate, he was also impressed with the fact that emergent systems never wholly detach themselves from their more primitive parts or elements. Even the best socialized human being is still, among other beings, a concrete physiological organism and presumably the baby is only that. The stubborn facts of heredity and environment are always there, as crucial parameters for the human personality and the social system alike, and their particular forms always use up many degrees of freedom. Emergent systems are thus never wholly free-floating. The problem, as Parsons saw it, was to construct a single theoretical system which could handle both types of factors and work out in detail the points of articulation and interaction between them .... (15)

Essentially similar considerations which Devereux's remarks (and mine) have alluded to in the foregoing hold for a number of arguments which appeared in Parsons' published works during the mid and late 1930's when Parsons was launching into his lengthy
period of theoretical formulation and social analysis; the most outstanding work to be noted in this respect is Parsons' tome *The Structure of Social Action* published in 1937, the basic contents of which will be discussed in short order.

It may now be relevantly asked what all this signifies in regard to Parsons' conception and advocacy of scientific theory in sociology during the early stages of his theoretical work. To start with, Parsons was intensely desirous of creating and utilizing scientific theory but, as we have noticed, he wished to incorporate teleological elements into his sociological theory, to be sure, a procedure starkly at variance with the then - and present - prevailing scientific and philosophical conventions. On the other hand, Parsons rejected outright any truck with the structure of theories obtaining in the natural sciences mainly owing to their tendencies to concentrate on simple relations between empirically specifiable variables which, according to Parsons, had the perverse effect of reducing all human behavior to inappropriate physical indices and, in consequence, completely ignoring the crucially important actional elements (values, norms, means, ends etc.) which he thought accurately characterized much of what is generally termed as human behavior. The predilections, aims and the canons of scientific discourse as Parsons conceived them at that time, were doubtless tugging in diametrically
opposite directions. However, after a number of years - roughly, after World War II in 1945 (16) -- the insistence for these conditions appears to have receded considerably and ceased to wield such significant force on his theoretical thought. By the 1950's and early 1960's the conflicting nature of these theoretical demands, objections, and conceptions all but vanished, though facets of all these were retained in exceedingly diluted form, playing scarcely mentionable roles in his theoretical works published in this period.

Before going on, I should think it proper to say something of a critical nature concerning Parsons' strictures of the theoretical schemes employed in the natural sciences, which continued for some time in his published work after the appearance of this essay. In his rather sharp broadsides against the heavy emphasis which a number of the theories in the natural sciences put on "observables", he was, in effect, condemning the theoretical practices of all these sciences in a gratuitous, blanket-like fashion which could not possibly stand up to the facts of the situation. For, clearly, many -- virtually all -- of the natural sciences employed quite sophisticated abstract theoretical constructions. Perhaps what would be more accurate to say is that Parsons -- though he himself did not say this -- voiced a strong dismay with a particular type of scientific
orientation, that of "empiricism" which strictly abjured abstract theory and argued that genuine scientific knowledge could only be acquired by transforming every abstract scientific statement by the utilization of conveniently developed series of definitions, into equivalent statements framed wholly in terms of observable notions. (17) Apparently, Parsons woefully misapprehended this, according to all accounts, minority standpoint in the natural sciences and construed it as a representative of the entire domain of the natural sciences. (18)

It is indeed of great interest to note that Parsons' insistence here on a teleological mode of analysis -- or an emphasis on the subjective aspects of human behavior -- betokened a rather frangible grasp of crucially relevant scientific and philosophical considerations. To be sure, a teleological argument is invariably excluded from scientific discourse as a decisive factor for the fundamental reason that it directly relies on the concepts of "ends", "purposes" and "values" for an account of an event, particularly of human behavior. Despite the seeming cogency and attractively easy type of account (or "explanation") it provides, a teleological argument for one thing is wholly inadequate for scientific inquiries since it is, in principle, unamenable to the minimal requisites of scientific test. (19) Another consideration to be taken into account in this regard is that a teleological argument does
not afford any sound reason for an expectation of a specific phenomenon to occur. Clearly, then, the utilization of teleological notions such as "ends", "purposes", and "values", to cite only a few, in statements to account for human behavior, do not contribute to or yield formulations of an explanatory character for they do not contain a sufficiently general statement (or law) from which a number of derivations, i.e., empirically testable statements, can be made.

More often than not, proponents of a teleological account (as Parsons was in this particular paper) confound scientific explanation in the manner I have outlined in Chapter Three with that of justification (frequently ethical) of a human action where an event is accounted for by references which are made to values, ends, or purposes and norms explicitly or implicitly held by the actors. (20) The profound importance of such accounts in the everyday world can scarcely be diminished. But their importation into a critical scientific framework where explanation in terms of causal relations and subsumption by virtue of general uniformities is an imperative procedure, is surely gratuitous, if not completely erroneous. (21) As John Hospers acutely points out in his discussion on the various types of explanation that are possible:

The chief mistake which people are in the habit of making with regard to purposive explanation /for our purposes, teleological argument/ is probably that of wanting an answer
to a why-question in terms of purpose-answer is legitimate are not fulfilled. People extend their questioning unthinkingly from areas in which purposive explanation is in order into areas in which it is not. (22)

In view of all these considerations on teleological argumentation, I think it not too far amiss to affirm that Parsons was indiscriminately commixing arguments — and he has done so for many years -- of fundamental scientific import with those of a distinctively philosophical orientation (primarily in the field of ethics where terms such as "purposes", "ends" and "goals" and "justification" are common coin). To be sure, interdisciplinary borrowing, in itself, is unobjectionable. But when there is a failure to recognize the contextual framework of each borrowed element, serious conceptual difficulties inevitably ensue. This is the problem, I think, which confronted Parsons when he advocated that the subjective aspect of human behavior should be taken into consideration and treated in a scientific fashion; unfortunately he did not perceive it. A fundamental methodological point is to be mentioned in elaboration of Parsons' problem. One of science's basic presuppositions is the resolve that the world is deterministic - that regularities can be found in the empirical world in order to render it sensible to the human interest. On the other hand, concepts such as "end", "goal", "purposes", "values" and "norms" imply an element of
indeterminacy, a freedom to choose on the part of a human agent. (23) It is evident that the respective frameworks are logically incompatible and that elements from one cannot be incorporated into the other without inviting the penalty of confusion and sterile debate, the bane of scientific enquiry. Thus, by incorporating alien notions from an indeterministic framework of reference to a scientific (deterministic) dimension, Parsons, quite unawares, it seems, precipitated considerable conceptual perplexities for himself and others, particularly his readers. He thrusted himself into an intellectually untenable position and from there attempted to proceed to greater theoretical sophistication. As mentioned earlier, Parsons gradually discarded the essential features of this theoretical standpoint in the ensuing years until now only faint vestiges of it remain. However, it is to be observed that all this emphatically does not suggest that behavior involving "norms", "values", "goals", "purposes", and such-like cannot be examined in a scientific fashion; for, undoubtedly, they can be as any other behavior. But these concepts must be employed as variables in statements of relations (factual statements or general statements) instead of their conventional uses and meanings in their proper domains of discourse. (24)

However, granted that Parsons, at this time, entertained theoretical viewpoints which were at sharp odds with those of
the hypothetico-deductive sort, generally in use at that time in the natural sciences, Parsons' general plaint, as the quotations suggest, against this extreme empirical position, had a certain legitimate, albeit implicit, ring about it in the sense that it directed attention to the fact that scientific theory, in order to be scientifically useful, was required to transcend the empirical data it purported to take into account. This, empiricism rejected. As a number of philosophers of science who noted the theoretical developments at that time (during the 1930's) maintained, "empiricism" is a sterile, restrictive approach (25) because all scientific theory, to be of any explanatory significance, must be couched in sufficiently abstract terms to subsume a welter of similar empirical observations for explanatory purposes; basically, this means that in any statement about a singular observation there are properties being ascribed to the constituent notions (let us say, objects) which state far more than any empirical observation can offer; for example, in the statement "All rocks are gold-colored", the notion "rock" goes far beyond what experience has, so to speak, presents; "rock" is a universal concept. Another way of arguing this point - as Parsons does in his subsequent theoretical work (26) -- is to maintain that all observations are made in terms of a conceptual scheme (that is, a theory) whether or not it is acknowledged by the observer or theorist. For emphasis, it is to be noted once again that
this argument was not explicitly stated by Parsons in this essay but it was indeed strongly hinted at. This argument at length was elaborated in another journal essay on sociology and economic theories which was published only four months later!

C. "Sociological Elements in Economic Thought: The Analytical Factor View" (1935)

One of Parsons' first statements on the nature and uses of scientific theory is found in a journal article entitled, "Sociological Elements in Economic Thought: The Analytical Factor View", published in the *Quarterly Journal of Economics* in 1935. (27) Although his remarks in this respect are fairly abbreviated on account of the nature of the article, what he does state is indeed significant in coming to an understanding of his conception of theory and cognate problems. One assumption that is initially made is that reality, especially human reality, is fundamentally an undisrupted continuum, a seamless robe, as it were. It is not conveniently sliced in sections for us to observe and identify. It is we, the common-sense men and the theorist, who dissect reality and make sense of it.

Human life is essentially one and no concretely possible degree of functional differentiation can destroy its unity.

But tho (sic) its concrete reality is a unity, it can like all other complex phenomena, be broken down for purposes of analysis into different factors. However predominant any one of these factors may be in a particular set of concrete activities, it is never present to the complete exclusion of the others. (28)
It is clear that a decomposition of reality into meaningful manageable units does not necessarily imply that if one unit is selected to represent an explanatory factor that it operates in an isolated fashion upon another unit or aggregate of units. Employing economic theory as an illustration, Parsons continues this train of thought:

The only way of maintaining a positive role for economic theory as a systematic generalizing science is to make it the science of one of these factors in concrete human action, to be sure more conspicuous in those concrete activities we call "business" than elsewhere, but neither confined to them nor excluding others there. From this point of view no one social science is capable of a theoretical explanation of concrete social facts but only a synthesis of the principles of various of them. Thus economic theory is necessarily and by its inherent nature abstract. But so, according to the best modern methodology is all scientific theory. (29)

The following type of argument, I think, is operating here. Since a theory is a selective, hence, an abstract, device, it cannot explain everything about pertinent concrete events placed before it for consideration. A concrete event can be taken into account by a number of theories of varying levels of abstraction and subject matter. As an illustration, let us take Parsons' example of "business". Clearly, a concrete event considered to possess a "business" character can be analyzed by an economic theory which focuses on the elements dealing with such factors as "supply" or "demand"; the event can be viewed from a psychological perspective, particularly regarding the traits of
"incentive" and "acquisitiveness" the implicated individuals in this "business" event may display; doubtless, this type of event can be explained from a sociological viewpoint which, broadly speaking, would cast an explanatory light on the institutional aspect, i.e., the interdependence of the diverse roles adopted by the actors in such circumstances. A theory does not - and cannot - explain everything connected with a phenomenon since it only concentrates on certain aspects.

A further point must be made of Parsons' phrase "theoretical explanation". This, I think, serves to point out that theory and explanation are closely intertwined. It may be inferred that to explain an event one employs a theory, to have theory one can explain. But it is noteworthy that Parsons does not demonstrate the nature of this interconnection. By and large, what Parsons states in these contexts appears to coincide quite closely with the criteria and rationales for hypothetico-deductive theory which were outlined in Chapter Three.

Moreover, Parsons contends very perceptively that a theory is, employed either covertly or overtly, in any type of empirical investigation. According to Parsons, a theory throws a guiding light on empirical reality, thereby enabling the theorist or researcher to select and examine relevant elements thereof. This cannot be undertaken without a theory, however vague it may be. In a passage commenting on the tendencies of certain
scientists (primarily economists) who scorn theory as a
needless luxury and direct their concerns solely to what they
can perceive in the empirical world, Parsons pointedly exempli-
fies this mode of argument:

This fallacy [reification or misplaced concreteness] gave the empiricist criticism its opening, and it has had
little difficulty in making a decisive case. Empirically,
discounting the one-sided biases of emphasis resulting
from the empiricist's own non-empirical (and therefore
from their own point of view illegitimate) theoretical
preoccupations, the empiricists are undoubtedly right.
But that does not make them any the less disastrously
wrong theoretically. Their view has quite definitely
resulted in "throwing out the baby with the bath." (30)

In the light of the criteria of hypothetico-deductive theory,
there is very little with which to dispute Parsons in this
particular connection. Indeed, theory whether it is consciously
or unconsciously employed preceded empirical investigation.
Parsons here aptly discerns the self-contradictory position
which is unknowingly adopted by the empiricists. As often as
not, they first aver that they spurn theory and deal with the
empirical phenomena straightaway. But to do this, however,
they must harbor some general notion (or theory) which draws
their attention to this empirical phenomenon rather than that
phenomenon. Hence, what they do is implicitly adopt a theory
or a group of theories and then strenuously deny it and more-
over, construct a scientific orientation predicated on this
rather naive oversight.
In the same journal article, Parsons also furnishes several key remarks on the nature of scientific enterprise as such. Empiricism (31), the tendency to adhere only to the observable data, cannot possibly be an aim of science. For, to be sure, the sheer assimilation of concrete events through one's sensory apparatus does not advance one whit our knowledge or, as Parsons affirms, "understanding" of the world. As it were, one is glutted by experience without being able to say anything about it. If science seeks to comprehend the empirical world, it must, in some fashion, transcend the images grasped by our senses. And the only way this can transpire is by the utilization of general assumptions which venture beyond the data; in a word, a theory is necessary.

Indeed, my own considered opinion, which cannot be further justified here is that a thorough-going empiricism is inconsistent with science itself. The essence of science, the understanding as distinct from the mere photographic reception of concrete phenomena, is theory and the essence of theory is analytical abstraction. Whatever its dangers, there is no other way. (Emphasis in original.) (32)

Just precisely what Parsons means by the locution, "analytical abstraction", ostensibly crucial in this context, is not at all clear. It appears to me that Parsons entertains a notion something on the lines of a general statement (or law) which allows for a selection of pertinent elements in an empirical phenomenon to be readily undertaken. Perhaps by this notion, Parsons intends to signify a concept of high abstraction which
can be applicable to a wide domain of phenomena, (33) and eventually be incorporated into a general statement asserting a general regularity in the empirical world. However, no indication is provided such that one can adjudicate one way or the other as regards this locution. As an aside - but one that can hold some importance - it may be noted that this phrase according to the conventional meaning of its two constituent terms is tautologous, hence, meaningless. To be sure, all abstraction, by definition, is analytical in the sense of dealing with concepts which are by their nature abstract. Indeed, this would be analogous to talking about "unmarried spinsters". Since, however, in a later presentation Parsons elaborates at length on this notion (which, by the way I will further exposit and discuss), I shall defer consideration of it for the moment save for noting once again that Parsons does not furnish any satisfactory adumbration of this notion in the essay undergoing examination here. However, I would suggest that my first interpretation of this notion would in all probability hold, given the general drift and tenor of Parsons' thought in this particular discussion.

For Parsons, it is clear, true scientific theory must be sufficiently general (or what amounts to the same thing, abstract) to be applicable to events taking place at specific and disparate spatio-temporal loci. This consideration seems important enough
for Parsons to deserve extended comment. Referring to the restricted spatio-temporal scope of Werner Sombart's theoretical contributions in economic theory, Parsons explicitly brings this crucial consideration to the forefront:

On the basis of German "historism" he has maintained a greater degree of concreteness for his economics than the orthodox school, but at the heavy cost of sacrificing forever its claim to generality and of limiting its applicability to a particular culture limited in time and space. In his conceptual scheme there is no such thing as general economic theory, but only the economic theory of capitalism, of the handicraft system, etc. This involves an abdication on the part of its claims to generality in which most economists are not willing to acquiesce. (34) /My emphasis - E.R.C./

It should be noted here that Parsons is subtly distinguishing between an approach which is wholly predicated on an empiricist framework dramatically termed, "scientifically fatal", and a theory which could be viewed as operating on a fairly low level of abstraction but nonetheless, affording "significant" information regarding the phenomenon in question. (35)

Finally, Parsons passes comment, albeit in a very succinct manner, on the factor of explanation. It is not appraised in terms of its "truthfulness" or correspondence with the supposed facts in the world. Apparently, to Parsons, scientific explanation which, is, for all intents and purposes, a component of theory, is adjudged by an eminently pragmatic criterion; viz., success. (36) Although Parsons does not further amplify in
this regard, it may be presumed that he is pertaining to its utility, its "fitness" with the data in question, without encountering an instance where it clashes with the theoretically pertinent empirical observations. (37) Apparently, the same criterion is applicable to a theory which provides the explanatory argument.

Thus, in summary form, Parsons makes the following assumptions on reality and observations on scientific theory in this particular journal article:

(a) Reality is continuous and is arranged in convenient identifiable units by our creative interventions to understand the world.

(b) A concrete phenomenon is characterized by numerous facets which are explainable by different theories set at different levels of abstraction and containing different subject matter.

(c) A theory does not throw light on (or explain) every angle and recess of a phenomenon; strictly speaking, it is directed at a specific facet of the phenomenon.

(d) A theory is abstract - it is universal; it does not refer to specific phenomena only, but to specific facets of phenomena occurring in any spatio-temporal context.
(e) Any empirical concern or investigation invariably presupposes a theory explicit or implicit.

(f) The criterion of the worth of a theory or explanation is pragmatically determined by its success to take account of a wide domain of phenomena.

Viewed in the large, then, Parsons' reflections, brief and few as they are, on the character and uses of scientific theory in this article are devoid of serious logical defect and are congruent in large measure to what is commonly conceived by philosophers of science to be the fundamental features of hypothetico-deductive theory. What is remarkable about these views is that Parsons formulated them when he was still in the throes of his implacable opposition toward Positivism, the theoretical approach utilized in the natural sciences, which he deemed malapropos to the social sciences. Interestingly enough, Parsons' deliberations on scientific theory, just outlined in this 1935 journal article are, with few exceptions here and there, those which the majority of individuals working in the natural sciences embraced at that time! (38)

D. The Structure of Social Action (1937)

1) An Exposition of Parsons' View of the General Features of Scientific Theory

One of Parsons' most extensive discussions on
scientific theory as such in all his years as a sociological theorist is probably contained in the first and final chapters of his volume, *The Structure of Social Action* (1937). As mentioned earlier, Parsons considers the discussions on scientific theory contained in this volume to be of immense moment for his subsequent theoretical writings. With this in view, I propose to present and examine critically these arguments in their manifold particulars and implications.

(a) At the outset, Parsons conceptualizes scientific theory as a constellation of "logically interrelated "general concepts" of empirical reference". (39) It should be noted that Parsons categorically stresses "concepts" as being interrelated, not, as one would expect, propositions, statements or laws. However, let us proceed for we shall come to this point later in my criticism. In a rather surprising turn of argument -- but ingeniously put, to say the least -- Parsons regards theory as both an independent variable and a dependent variable (40) in the evolution of a science to its maturity. What he means by this argument, admittedly somewhat reminiscent of an analysis in the sociology of knowledge, is this. A theory does not necessarily grow from an accumulation of empirical facts, collocated without the assistance of a theory. But a theory to be scientifically acceptable must fit the facts, as it were.
In other words, theory and fact interplay, the latter, if clashing with the former, usually indicates the points at which the theory is to be modified, or even discarded in part or in whole. Thus, facts, the independent variable, engender changes or add to a theory's compass the dependent variable. Conversely, and most importantly, a scientific theory furnishes a guide as to which types of empirical facts should be sought for examination and also furnishes indices which spell out in general terms the type of scientific interests which may be fruitfully pursued in a discipline. In consequence, then, scientific theory is an independent variable determining the type of facts to be investigated. That is, a theory provides the categories by means of which the relevant empirical data (facts) are identified. In this fashion, confirmation or disconfirmation of the theory and, perhaps, the discovery of previously unknown facts is usually achieved:

It goes without saying that a theory to be sound must fit the facts but it does not follow that the facts alone, discovered independently of theory, determine what the theory is to be, nor that the theory is not a factor in determining what facts will be discovered, what is to be the direction of interest of scientific investigation. (41)

Although Parsons' employment of the locutions, "independent variables" and "dependent variables" seems awkward and suggestive of an empirical analysis in the sociology of knowledge, it, nevertheless, serves to bring into sharp relief Parsons' argument that theory and the cumulation of facts are indissolubly intertwined. One does not proceed without the other.
(b) As faintly suggested in the preceding remarks, Parsons contends that a scientific theory -- or for that matter any kind of theory -- invariably precedes any observation of empirical phenomena. Knowledge about the world -- or more accurately, knowledge about a specific aspect of the world -- is virtually impossible by the indiscriminate assimilation of countless, discrete (disparate or similar) observations of empirical phenomena. What is required to make sense of these almost infinite sensory impressions is a scheme of concepts and statements respectively identifying and spelling out certain relationships of empirical events in a general fashion. Knowledge of the world, then, issues from a point of view; in a word, a theory or an aggregate of theories. Pure sensory data, as it were, is an elliptical expression for, they are always collected in terms of a conceptual scheme (or, a theory). Broadly speaking, the acquisition of knowledge from sensory experiences entails the identification of the sensory experiences into concepts drained from a conceptual scheme which indicate how these concepts relate to each other and, most importantly, guide in the first instance, the attention and subsequent observations of the investigator in his analysis of an empirical phenomenon. This process is not solely confined to the circum­spect and critical enterprises of scientific theorization and investigation; it is covertly employed in the common-sense
everyday language (in any language) where comments of an
empirical nature are constantly exchanged and utilized. A
welter of tacit theories (or conceptual schemes) are doubtless
relied upon. (42) In Parsons' view, these conceptual schemes
are implicit in the language employed. Above all, then, theory
is an integral part of human thought; but primarily, science
is interested in its full elaboration and consistency for pur-
poses of affording largely accurate descriptions and analyses
of empirical phenomena in which a social scientist may express
an interest.

It is fundamental that there is no empirical knowledge
which is not in some sense and to some degree conceptually
formed. All talk of "pure sense data", "raw experience" or
the unformed stream of consciousness is not descriptive of
actual experience, but a matter of methodological abstraction,
legitimate and important for certain purposes but, neverthe-
less, abstraction. In other words, in Professor Henderson's
phrase, all empirical observation is "in terms of a conceptual
scheme". This is true not only of sophisticated scientific
observation but of the simplest common-sense statements of
fact. Conceptual schemes in this sense are inherent in the
structure of language and, as anyone thoroughly familiar
with more than one language knows, they differ in important
respects from one language to another. (43)

To denigrate the significance of theory, especially in
science (social science in particular) is, it appears, to
betray an insufficiency of scientific knowledge and logical
technique almost verging on scientific ignorance. To be sure,
theory is integral -- and, above all, vital in all of its vast
and crucial implications -- to science where a critical awareness
of the conceptual apparatus and approaches and investigative
techniques is accepted as the paramount guiding rule in scienti-
fic and philosophical circles.
(c) It is highly important to note here before continuing further in another passage in Chapter One of this tome, that Parsons amplifies as to what he means by a scientific theory. As it will be seen in short order, this conception of scientific theory is somewhat discordant with his introductory characterization of the nature of scientific theory. In the latter case, however, a theory is essentially an amalgam of statements of fact and abstract statements of relations between these facts. Theory, then, is a collection of statements of relations between facts, not as he previously stated, "a body of logically inter-related "general concepts" of empirical reference". For Parsons, the facts which are clearly concrete statements about certain phenomena do not constitute the only propositions that can be uttered about the phenomena in question. One must always remember that a scientific theory is abstract. This means that the facts which a specific theory entails do not furnish in any way a thoroughgoing description of the concrete empirical phenomenon undergoing examination. Rather, however, the factual statements secure their meaning and significance by way of the conceptual scheme of which they are integral components. Understood in its proper sense, then, factual statements assert something about a single facet of the phenomenon with which the theory or conceptual scheme is principally concerned. Thus, the facts about a phenomenon being investigated are selected by (or are important to) the
theoretical scheme which is being employed in its analysis. Presumably, then, different theories employed in regard to an analysis of a particular phenomenon would almost invariably disclose different facts about the phenomenon in question. Furthermore, from all these remarks it certainly appears that, for Parsons, scientific theory facilitates, more than anything else, adequate descriptions of certain empirical phenomena.

(d) Theory in varying degrees comprises what Parsons terms "an integrated "system" (47); in other words, it is characterized by a determinate logical structure. This means the theory's collection of general propositions are logically related to each other. Thus, any significant empirical addition or alteration to one proposition materially affects in the same manner the other propositions of the same theory. A proposition has logical implications for the other propositions. It is important to note, however, that these logical relations between the general propositions of a theory do not depend on any one general proposition in the sense that all the propositions of a theory are derivable from one main proposition, as it were. Clearly, if this were the case, as Parsons argues, a theory would be constituted of essentially one proposition, thereby, impairing its scientific value since it would be surely tautologous and hence, vacuous. (48) In consequence of the logical interrelationships of the theory's various propositions, a theory is recognized as a "system". However, it is to be noted
that a theoretical system gravitates toward a logically closed structure which means that it becomes a self-contained unit, all assumptions and implications rendered explicit. This occurs when the logical implications of each of a theory's constituent general propositions is, so to speak, restated in another proposition belonging to the same theoretical system. According to Parsons, a "logical closure" does not necessarily imply that the propositions of a theory are deducible from only one proposition of the same system - if this were so, as just noted here, the formulation of theory would indeed become a fruitless, tautologous enterprise.

In Parsons' view, logical closure represents a property of great moment for a theoretical system. A general proposition which does not find its statement in another general proposition of the same theoretical system can be said to rely for its raison d'être on a number of unspoken assumptions which have not been rendered explicit or perhaps are merely gratuitous assertions without logical foundation in the theory. And, by various measures, they must be prised out to the best of one's abilities from what has been explicitly presented. Since, to be sure, science is a joint explanatory and critical endeavor, it is one of the cardinal objectives of any appraisal of a scientific theory in a substantive field to illuminate these presupposed general propositions which give meaning to the employed concepts and specific statements. The implications
flowing from such an undertaking greatly contribute to the
enrichment of a discipline's theoretical scope and subsequently
its range of empirical knowledge which doubtless is, on the
balance, a direct resultant of an improved theoretical scheme.
One caveat, however, must be entered at this point. To Parsons,
it is indeed almost axiomatic that all scientific theories
eventually become closed systems in a logical sense. But this
should not be confounded with an "empirically closed" system
which arbitrarily precludes, for a variety of erroneous reasons,
the adducement of certain types and accounts of empirical infor-
mation - a test of certain claims about the world. (51) Accord-
ing to Parsons, this development is comparable to the constrict­
ing perspectives of empiricism (or the commission of a fallacy,
that of reification) which doggedly adheres to certain observ­
able as the true accounts of the world and eschews all theorizing
of an abstract nature, a standpoint, as Parsons pointedly observes,
which tacitly adopts a theoretical bias but obstinately refuses
to acknowledge it and prefers to think otherwise.

Interestingly enough, Parsons illustrates the various facets
of a logically closed theoretical system by considering "a
system of simultaneous equations".

Such a system is determinate, i.e., closed, when there
are as many independent equations as there are indep­
endent variables. If there are four equations and only
three variables, and no one of the equations is derivable
from the others by algebraic manipulation then there is
another variable missing. Put in general logical terms:
the propositions stated in the four equations logically
involve an assumption which is not stated in the defini­
tion of the three variables. (52)
A logically closed theoretical system as the type Parsons envisages clearly does not leave crucial assumptions to be rendered explicit by conjecture or to chance. However, it is worthwhile to repeat once again that, for Parsons, a logically closed theoretical system is not synonymous with the scientifically destructive notion of "empirical closure".

(e) For Parsons, it is beyond doubt that the general propositions of a theory always pertain to the empirical world. Should, however, this not be the case, the proposition would automatically forfeit its scientific status. A statement expressly belonging to a particular theory can be construed, with due qualifications taken into account, either as an assertion of fact or the "statement of a mode of relations between facts", much, of course, depending on the level of abstraction of the proposition in question. (53) Just precisely what constitutes these qualifications (or criteria) is not mentioned, apart from the condition of a certain level of abstraction.

Thus, in view of these considerations -- fragmentary though they may be -- and those earlier discussed, Parsons contends that if knowledge of facts in a sphere of study is changed, then, one proposition of the theory involved will of necessity require change and, by implication, other related propositions of that theory will likewise undergo alteration mainly owing to their logical interrelation. Accordingly, as Parsons
However, it is decisive to note that the change in the knowledge of fact, for Parsons, is not merely a change pure and simple, but an "important change in our knowledge of fact in the field in question". (55) By this, it should be understood that Parsons means a "scientific importance of change" (56) in the sense that the fact has implications for a theoretical system by means of which it was discovered. This is to be contrasted with a scientifically unimportant discovery of a fact which possesses no implication for, as Parsons terms it, a system of theory even though it may be interesting for other, less scientific reasons. Even a simple observation, however inconsequential it may appear from various standpoints, may have immeasurably profound logical consequences for a theoretical system specifically in the sense that it may strengthen the claims of the system or it may cause the system to be modified or, if the case demands it, obviated and another compatible theory formulated in its stead. It may be noted that the notions of "triviality" and "inconsequence" as applied to empirical discoveries, themselves presuppose an implicit theoretical framework which enables such a judgment to be effected without reliance on the whims and fancies of the theorist or researcher. Parsons illustrates this argument concerning scientifically important and trivial changes in
our knowledge of fact by briefly considering the theory of relativity in the field of physics.

It is probably safe to say that all the changes of factual knowledge which have led to the relativity theory, resulting in a very great theoretical development, are completely trivial from any point of view except their relevance to the structure of a theoretical system. They have not, for instance, affected in any way the practice of engineering or navigation. (57)

A theory, then, directs our analytical attentions to an area of the empirical world where pertinent facts as regards our scientific problems and questions are to be found and, additionally, permits the appraisal of the significance of these facts.

As well as stating the facts that have been observed and elaborating their logically deducible relations with other facts that also have been observed, a theory, in Parsons' view, also specifies the type of facts that one can expect to observe under certain clearly delineated conditions. Parsons further indicates that this development can only take place if the theory is regarded "empirically correct" to begin with. (58) This condition is determined, it is assumed, by applying the theory to pertinent empirical data immediately at hand, particularly those which the theorist was referring to when he formulated the theory. With this knowledge and theory, one can proceed to analyze data in related fields. If there are relations of a certain type obtaining in one province of phenomena, then in other allied provinces of
phenomena, relations of a specified kind can also be expected to hold. Presumably, this approach also accounts for events occurring both in the past and future. In equal measure, the results of an analysis of the facts of a given "related" phenomena conducted in the light of a theory, designed directly to account for phenomena of a certain kind may yield important implications, positive or negative, for that theory.

Not only are specific theoretical propositions which have been directly formulated with definite matters of fact in view subject to verification. But further, a theoretical system built upon observations of fact will be found, as its implications are progressively worked out, to have logical consequences for fields of fact with which its original formulations were not directly concerned. If certain things in one field are true, then other things in another related field must also be true. These implications also are subject to verification, which in this case takes the form of finding out what are the facts in this field. The results of this investigation may have the same kind of reaction on the theoretical system itself. (59)

From all this, the following consideration seems unmistakably manifest. Broadly speaking, a scientific concern, for empirical facts either in the past or future, and, of course, in the present, is almost exclusively determined by what Parsons terms "the logical structure" of the theory.

(f) One of the most fundamental procedures of science, especially in substantive theoretical concerns, is obviously verification. The verification of a theory does not only entail its successful application to a group of data which perhaps initially prompted the formulator to provide a theoretical account, or
perhaps a period of patient waiting for novel facts to be discovered by chance. To be sure, verification also necessitates, if conditions allow, a deliberate investigation of relevant phenomena in the light of the expectations derived from the theory in question by subjecting them to rigorous empirical tests. (60) In this manner, the facts which are encountered in the examination can be assessed in terms of their congruence or discrepancy with the theoretical expectations. Generally, if there is close congruence, granting errors of observation and technique, are taken into account, the theory can be said to be "verified". It should be observed that these empirical investigations are usually characterized by two essentially similar kinds of contexts: (a) of circumstances which have not been previously examined, and (b) of circumstances which have been previously analyzed but not in the light of the present theoretical problems held by the investigator. (61) However, in Parsons' view, one of the most compelling features of the ideal investigational conditions embracing circumstances which are artificially constructed and employing controlled procedures of analysis is that the chance factors and ambiguities are in large part eliminated or reduced to a minimum, as, let us say, experiments in the natural sciences. It is important to bear in mind in this specific regard that for Parsons, the arrangement of situations for empirical test are issues of practical technique, wholly unrelated to questions of logic and as such should not be confounded with the latter.
When the empirical facts of an investigation coincide (or fit) with the theoretical expectations (hence, verification), significant theoretical matters by no means come to an abrupt conclusion. (62) For, to be sure, the empirical support or lack of support of a particular theoretical statement has vital implications for the remainder of the interrelated propositions in the theory in the sense that all these propositions as well have been, depending on the case, verified or not verified, undoubtedly by virtue of their being logically connected with the proposition or set of propositions which has undergone direct empirical investigation. However, as is frequently the circumstance, the empirical facts may strikingly clash with the theoretical expectations or other unanticipated facts may be discovered in the course of the verification process. Such developments require a critical examination of the theoretical system in order that the confuted aspects or the inadequate explanatory portions of the theory, depending on the extent and nature of the clash with the observations, can be obviated or refurbished, or that the entire theoretical scheme can be somehow reinforced or modified to take into account the novel empirical findings. (63) Speaking very broadly, then, if the empirical results are too refractory to be accommodated in any way into the general patterns of the theoretical scheme as it is structured at that given time, that is to say, if the theory cannot deal readily with— or perhaps cannot do so in any
conceivable way — what appears to be a confuted proposition or assimilate a novel empirical finding, then the theory must undergo extensive reformulation, the degree of discrepancy between the theoretical proposition and the empirical observation determining the extent and manner of the reformulation. As Parsons observes in this respect: "If observed facts of undoubted accuracy will not fit any of the alternatives it leaves open, the system itself is in need of reconstruction". *(64)* From these considerations, it becomes clearly evident that a reciprocal process obtains between factual statements and the theoretical system employed in their analysis. In capsule form, the argument follows these lines. The deduced expectations from a theoretical scheme point toward the pertinent domains of empirical fact to be investigated while the very results of this factual investigation bear significant empirical and logical consequences for the theoretical scheme which originally directed attention to the phenomena containing these facts.

*(g)* It is essential to point out at this juncture of the discussion that the concept of "fact" plays an eminently significant role in Parsons' conception of scientific theory. It is a substantially more complex notion than it appears at first sight. As has always been indicated, Parsons borrows his conception of fact from the writings of one of his former mentors and collaborators, biologist, L.H. Henderson. Essentially, a fact is a concrete statement amenable to verificational procedures.
Consider Parsons' quotation from one of Henderson's methodological works: "...a fact is understood to be an "empirically verifiable statement about phenomena in terms of a conceptual scheme."" (65) A fact is commensurate with a proposition about one or several phenomena and, moreover, should not be confounded with the phenomena itself. Expressed more precisely, a fact affirms something about the particular properties usually appearing in one phenomenon or group of discrete phenomena, for example, "blue", "rational" behavior, an "hostile attitude. A "phenomenon" in the widest possible meaning of the term cannot be construed as an empirical fact "unless one is speaking in an elliptical sense". (66) A concrete, empirical phenomenon is usually a cluster of facts, many of them accounted for by a variety of theories. It, then, becomes clear that the implications flowing from these general considerations for descriptions (or explanations) of a phenomenon or, if need be, various aspects of a phenomenon cannot be overstressed. Thus, in a "description" and this is precisely the term Parsons consistently employs throughout his discussion in this volume -- of a particular concrete phenomenon, a number of facts, logically independent of each other are adduced only in terms of a theoretical scheme. The type and number of factual statements, however, are contingent on the theoretical system in use and the nature and complexity of the empirical phenomenon being examined. (67) The utilization of any theoretical scheme (or conceptual scheme)
renders it possible for an "adequate" description to be proferred. What Parsons means by this, I should think, is that, with a theoretical scheme, one can ascertain comparatively easily a significant number of "important facts" even though they do not remotely approach all the facts that can be known in regard to the phenomenon which one may have in mind. The rather formidable locution "all the facts", contrary to the quantative flavor it may bear, in actuality pertains to the type of "important" factual statements which are logically necessary as premises for the framing of the conclusion which, I would think, ostensibly refers to a description (or explanation?) of the phenomenon undergoing examination. (68) The only way in which facts are classified as important or unimportant is through the employment of a theoretical scheme of some kind which embodies them:

Even when we say "we do not know enough facts" to justify a given conclusion, we do not mean quantitatively that we cannot make a sufficient number of verifiable statements about the phenomenon but rather that we are not in a position to make certain important statements which are logically required by the premises for the conclusion. What facts are important is determined by the structure of the theoretical system. (69)

It is abundantly clear that, for Parsons, facts and theory are intimately associated. Without an explicit theory, a fact is insignificant; without any unambiguous reference to facts, a theory becomes a metaphysical construction, asserting nothing of significance about the empirical world.
(h) One salient issue of undeniable importance for Parsons' philosophic views of scientific theory in this particular context remains to be mentioned; that of Parsons' argument indicating there are three distinctive conceptual levels of a scientific theory which must be taken into account in any evaluation and, furthermore, must be employed in theory formulation. Briefly, these three levels in hierarchical order are as follows: (i) "type" or "part" concepts; (ii) "empirical generalizations"; (iii) and, "analytical concepts". Each level plays a unique role in a theory's scope and logical and empirical tenability. (70)

A "type concept" subdivides an empirical phenomenon which is to be investigated into simpler and manageable elements to function as units in any pertinent general theoretical scheme which will in turn furnish its explanation. (71) Broadly speaking, it provides an indispensable descriptive frame of reference by means of which concrete phenomena of interest to the theorist or researcher can be identified (or defined) and described in further specific detail as a preparatory stage for their eventual explanation. Its scientific importance is beyond question. For, according to Parsons, no empirical fact is capable of being described without such a framework of concepts. But, at the same time, Parsons emphatically holds -- and this is important to recognize -- that a dissection and identification of empirical phenomena in terms of these concepts in no wise provides an explanation of that phenomena. (72)
But their description—that is, facts—within it has, in the first instance, the function of defining a "phenomenon" which is to be explained. That is, of the great mass of possible empirical observations we select those which are at the same time meaningful within such a scheme and "belong together". They thus serve together to characterize the essential aspects of a concrete phenomenon which then becomes the object of scientific interest .... (73)

A "type" or "part" concept also possesses a peculiar two-fold character, depending on how it is employed; it may point to either an actual or hypothetical entity. (74) Particularly in the latter instance when analytical concerns are focused on factors detached, as it were, from their endemic contexts (i.e. an arm from a body) a "type" or "part" concept is very much analogous to the Weberian conception of the "ideal type" in the sense that it is not unreasonable to assert that the events the concept refers to could probably occur in a concrete circumstance, given the requisite conditions. Understood in another sense, they are empirically possible, not complete inventions without reference to reality.

As Parsons, in elaboration, writes:

But whether the concept—the "type" or "part" concept—refers to a mechanistic "part" which can be observed without essential change of properties in complete concrete separation from the whole in which it occurs, or to an organic part which when concretely separated remains a part only in an "equivocal sense", the logical character of the concept remains the same. It refers to an, actually or hypothetically, existent concrete entity. However much the concept of the "pure type", especially in the "organic" case, may differ from anything concretely observable, the test is that thinking of it as concretely existent makes sense, that is does not involve a contradiction in terms.

The concept of any particular physical body or system of such bodies in mechanics is of this character. This is
true even if it is fictional, as are a "perfect" gas, a "Frictionless!!" machine, etc. So are the chemical elements even though some of them are never found in nature uncombined with other elements. So also are such concepts as a "perfectly rational act", a "perfectly integrated group", etc....

Moreover, such concepts are not restricted in their use to their definition and empirical identification as "really" parts of a single concrete phenomenon. Rather this is always the first step of scientific generalization - for such parts may be identified as common to a plurality of different concrete phenomena.... (75)

For Parsons, "part" and "type" concepts are of paramount importance in theoretical endeavors. They are, as it were, the foundation blocks of scientific theory.

As for "empirical generalizations", they are in Parsons' view of another logical order (76) with different functions yet are wholly dependent on "type" or "part" concepts for their viability. For, they comprise the fundamental consituents of empirical generalizations. In brief, basic empirical generalizations are moderately general statements regarding the probable activities of, or relations obtaining between these concepts under specifically defined conditions. It is important to realize that, according to Parsons, these generalizations find their source in the discernible patterns of behavior evinced by the type or part concepts under certain circumscribed conditions. It may also be noted that these statements are considered to be valid (presumably, logically valid and empirically reliable) given their limits of applicability. In this connection, consider Parsons' explication for a moment:
Furthermore, on occasion, a great deal can be said about the behavior of these parts /"type" or /"part" concepts/ under certain kinds of definable circumstances. Such judgments may yield a kind of generalization which is of high explanatory value, and, within limits perfectly valid. General statements about the possible or probable behavior of such concrete or hypothetically concrete parts of concrete phenomena, or various combinations of them, under given typical circumstances will be referred to as empirical generalizations ..." (77)

At all events, an empirical generalization, it can be surmised, affords solid grounds for what Parsons would call a dynamic analysis of empirical phenomena; that is, it mainly enables one to put forth a causal explanation of certain concrete phenomena. However, a caution should be sounded in this respect; an empirical generalization, for Parsons, is not commensurate with a theoretical explanation or an empirical law. (78)

The ultimate -- and hence most abstract -- conceptual level in scientific theory is that of the "analytical concept". (79) It pertains to the general property or attributes of a "type concept". More than that, it can refer to a combination of properties (or "values" (80) as it can be termed in this context) of a class of "type concepts" (or units). An analytical concept, as Parsons conceives it, tacitly assumes the "type" or "part" concept situated on the first conceptual level. To be sure, these concepts are logically necessary for an analytical concept even to be considered, not to speak of explicit formulation. Previously, as indicated, they break down complicated
empirical phenomena into concrete, manageable units. However, once these "type" concepts are devised it is also logically necessary that they can be, in turn, subsumed under general properties of the sort an "analytical concept!" exemplifies. Apart from the mere assertion of this inevitable development of any collection of "type" concepts (or units), Parsons does not outline an argument showing how this set of circumstances logically comes about. Be that as it may for the moment, let us proceed further with the exposition. Another way of looking at this vital notion is to consider it as a universal concept, largely in the sense that it is common, in a universal way, to a group - or groups - of "type concepts" (or units). It follows from this characterization that an "analytical concept" is omni-temporal and omni-spatial; it does not refer specifically to any one concrete event or object at any one place or time. The only manner in which it can be said to be perceived in any empirical sense is through its specific instantiation or corresponding "value" in a concrete empirical event. As Parsons himself remarks in his argument:

Any particular concrete or hypothetically concrete phenomenon or unit must be thought of not as a property in this sense, but as capable of description in terms of a particular combination of the particular "values" of these general properties. Thus a physical body is described as having a certain particular mass, velocity, location, etc. in the respects relevant to the theory of mechanics. Similarly an act may be described as having a certain degree of rationality, or disinteredness, etc. It is these general
attributes of concrete phenomena relevant within the framework of a given descriptive frame of reference, and certain combinations of them to which the term "analytical elements" will be applied. (82) (Emphasis in original)

...An analytical element ... is an abstraction because it refers to a general property while what we actually observe is only its particular "value" in the particular case. We can observe that a given body has a given mass, but we never observe "mass" as such. Mass is, in the terminology of logic, a "universal." Similarly we can observe that a given act has a high degree of rationality, but never can we observe "rationality" as such. (83)

Parsons views the conceptual level of "analytical concepts" as the most fertile and promising of the three levels in the development of a mature systematic theory in sociology. According to Parsons, it is highly probable that a battery of unambiguously presented analytical concepts eventually lead to the formulation of a set of statements expressing highly relationships between these abstract concepts which obtain irrespective of the manifold specific "values" which can be affixed to them. These relationships or, as Parsons laboriously describes them, "uniform modes of relationship between the values of the analytical elements" (concepts) are regarded as analytical laws". (84)

This notion corresponds to what could be termed as a general statement, a general proposition, or empirical law, or an empirical regularity. As can be surmised, the relationship between the constituent analytical elements is abstract to the highest degree that is still empirically relevant. As an illustration, take the following statement: "In all industrial societies, rationality maximizes utility"; clearly, in this illustration "rationality" and "utility" comprise the analytical
concepts which are of such an abstract nature (omnitemporal and omnispatial) that for purposes of empirical test they must surely undergo a variety of operations to be affixed an empirical interpretation or, as Parsons would say, "value".

It is of marginal importance whether or not the relations between these analytical elements are capable of being formulated in quantitative terms as, let us say, Galileo's law of free falling bodies in the field of physics. Non-quantitative formulations of relations between analytical elements which may require the aid of operational definitions or some such similar procedures for interpreting general notions into specific empirical values for evaluation of the theory, are just as adequate in terms of scope, logical validity, and empirical reliability, if not in precision as those theories couched in quantitative terms. (85) As Parsons concedes, however, it is only in the physical sciences where the analytical elements and analytical laws find their ideal substantive expression at this particular juncture in the history of scientific thought.

(i) There is one further point to be noted with respect to Parsons' conception of the various conceptual levels of a theoretical system. This is the problem of residual categories. These categories are curiously of a negative character. They pertain to all the phenomena and ideas which are not subsumed by a particular theoretical scheme. That is to say,
those phenomena which are recalcitrant to incorporation into the "positively defined" categories of the theoretical system in question.

Every system including both its theoretical propositions and its main relevant empirical insights, may be visualized as an illuminated spot enveloped by darkness. The logical name for the darkness is, in general, residual categories. (88)

It is Parsons' basic contention that the functional significance of these negatively defined categories are, "deduced from the inherent necessity of a system to become logically closed". (89)

Above all, it is the logical character of the theoretical scheme which makes it imperative that some form of negative conceptualization be devised. Apparently, the supporting argument is this. The conceptualization of the positive general categories of the theory clearly presupposes that they are differentiated from other general categories and this, in turn, ensures the differentiation of the facts – which are specific empirical manifestations of these concepts (type or analytical). However, perspective must not be lost in the consideration of these remarks. Because a number of general relational statements or factual statements are not immediately relevant to a theoretical system, it by no means implies that they are of inconsequential import for that theory. On the contrary, to be sure, whenever a theoretical system is being constructed or improved upon, a constant reference to, and the formulation of positively defined concepts from the material in the residual categories is, according to Parsons, indeed necessary.
From this rather prolix discussion of Parsons' conception of scientific theory as it is presented in *The Structure of Social Action*, it is assuredly possible to infer that scientific theory as such performs at least four crucial scientific functions. So far as I can ascertain, they are as follows:

(a) A theory enables one to describe a potentially infinite mass of empirical phenomena of interest to the theorist or researcher via the conduits of a frame of reference which includes a complex of interrelated general concepts and statements. These factors furnish the requisite criteria for the selection of the facts considered to be pertinent to an empirical analysis and also the determination of their significance.

(b) A theory also facilitates the development of causal explanations (dynamic analysis) of specific phenomena; the empirical generalizations allow this to occur.

(c) A theory permits the verification of a host of statements regarding the empirical world by one set of observations owing to the interrelatedness of the theoretical concepts and the statements.

(d) A theory permits one to comment in an empirically significant manner on events which are to occur and on events occurring in a sphere of phenomena closely allied to the sphere with which the theory's content is primarily concerned.

Such, in short compass, are Talcott Parsons' views on the logical structure of scientific theory as discussed in *The Structure of Social Action*. 
2) Critical Comments on Parsons' Views of Scientific Theory

I think the following criticisms can be effectively put to Parsons' conception of scientific theory which appears in *The Structure of Social Action*. However, rather than concentrate on each specific matter of dispute, I propose here to dwell primarily on the general, yet what I would consider significant issues in philosophical discussions on scientific theory in the order they were presented in my exposition above.

(a) At the outset, Parsons' initial assertion that scientific theory is, broadly speaking, a corpus of "logically interrelated 'general concepts' of empirical reference" is manifestly erroneous. What does it mean to have a system of general concepts which are logically interrelated? Answers are not forthcoming to this question. A concept is incapable of being logically related with another concept - except, of course, in a tautologous sense when one term "implies" another, for example, puppies are young dogs -- because it only pertains to objects and events and to properties of objects and events and to nothing more. It is bereft of the linguistic structures of a statement (proposition, law, etc.) namely, the subject, verb, and object or complement which permit the expression of a relationship between two or more concepts. Only with a set of statements (relations) endeavoring to state something of the world can logical interconnection or relatedness be said to obtain. Moreover, a concept is an inextricable component of a theory
(or a series of general statements); without its inclusion in statements, it is theoretically irrelevant. It follows, then, that a theory cannot be a collocation of concepts as Parsons here suggests. There is one further point to be noted in this regard. The locution "general concept", is redundant for, fundamentally, all concepts by virtue of their nature are abstract (or general) notions; some more so than others. Nothing is gained by characterizing a concept by an already implied feature. This may seem to be a gratuitous quibble but I think otherwise. Such elementary misconceptions usually lie at the root of major theoretical confusions and ambiguities and vividly point out the depth and breadth of a theorist's acquaintance with the basic tools of his difficult craft, that of the conceptual element, which gives respectively criteria and directions for the evaluation and execution of his tasks.

Parsons' use of the terms "independent variable" and "dependent variable" with regard to the relations between theory and empirical knowledge is indeed ambiguous, not to mention erroneous. It admits of two patently different interpretations. One of these is that Parsons is endeavoring to comment on an empirical thesis in the sociology of knowledge, which pertains to the influences of theory on the development of empirical knowledge and, conversely, the effect of empirical knowledge on the construction and continued viability of a theory. But if this is the case, it seems to me that it is a narrowly conceived hypothesis because it fails to consider
among much else, the omnipresent factors prevailing in the general cut and thrust of scientific activity such as guesswork, chance, serendipity, errors in calculation and hypothesis formulation, the sophistication or malfunction of testing devices or procedures, the societal aspect (the general hostility, indifference, or encouragement manifested in terms of finances and values by the public toward scientific work) and, last but not least, the various psychological characteristics of the implicated theorists and researchers. A consideration of these variables, I should think, would afford a fruitful tack in assessing the developments of theory and the enlargement of empirical knowledge; moreover, they are amenable to empirical analysis. But I do not think this is quite what Parsons has in mind. And here the second interpretation becomes evident -- the one which I chose to employ in my exposition of his argument mainly because it captures the essence of what I think Parsons is attempting to affirm. He is, in effect presenting, a philosophical argument asserting that all empirical observations and statements of empirical fact are made only in the light of a theory and that since a theory and empirical observations are logically connected (they are made only in terms of the theory) have implications for the theory in the sense that it is in varying ways supported or unsupported by the evidence found in the empirical world. This, I think, is the maximum extent of Parsons' remarks in this connection; and in large measure, they are substantial and correct. However,
I would go further and say that this reciprocal relation is analytical (or logical) rather than empirical mainly owing to the logical relations which obtain between the theoretical statements and their concrete empirical manifestations; as it is, the relation between theory and fact is, in principle, irrefutable, hence, non-empirical.

(b) Parsons' argument that a theory always is antecedent to observations and experimentation finds considerable support in the canons of hypothetico-deductive theory. As he contends, all observations are undertaken in the light of a theory because observations are selective and theory, however vague or explicit, it is to the theorist, is the selecting device. With these remarks, one can hardly raise philosophical objections of any significant kind. But, it will be noted, that Parsons employs the locution "conceptual scheme" in this context without seeking at any time to clarify its meaning. This produces, I think, gratuitous confusion. Is he referring to the assemblage of concepts which partially constitute a theory? Or is he using this phrase as a synonym for "theory"? No definite indications are given. Given the topic and implications of his discussions at this point, I would assume that he is pertaining to theory, that is, conceptual scheme and theory are synonymous. I may add here that this failure to define troublesome or crucial terms in his discussions, particularly in regard to notions such as "conceptual scheme" and "theory" persists throughout the entire volume and in his subsequent writings of metatheoretical and substantive import.
(c) In his further amplification of the character of scientific theory in the same chapter, Parsons proceeds from the conception that it is an aggregate of logically interrelated concepts to what I think is a much more improved notion that theory is a related collection of statements of relations (presumably, general) and factual statements. However, this readjustment of conception performed literally in midstream of his explication of scientific theory is, I fear to say, still unsatisfactory and I hope that my opposition to this conception which I will outline in what follows will not appear to be too picksome or unjust. Theory, as conceived from a hypothetico-deductive perspective, is comprised of a combination of general statements (or laws) which are logically interrelated, they are applicable for purposes of explanation to any type of concrete phenomena - potentially infinite in number - deemed relevant by the theory. The general statements subsume (or explain) innumerable concrete events. Theory does not contain factual statements in its repertoire of general statements. If a theory allowed factual statements to be incorporated it would simply cease to be a theory; it would become an unwieldy bundle of unilluminating concrete statements such that it would be entirely without value as a scientific instrument. Why? As mentioned a moment ago and also in Chapter Three, a theory -- in other words, a series of logically interrelated general statements which have a wide empirical application -- are, in effect, convenient summari-
zations of multitudinous actual and conceivable observations, an ideal mnemonic device, as H.L. Post would suggest. The general statements permit the derivation of these discrete observations without having to commit them to memory. It is by virtue of the generality (or universality) of a theory's statements which render theory an invaluable conceptual construction for it can be applied in (and explain) a vast variety of concrete instances. This, a theory could not do if it were a compound of general and factual statements. Thus, when Parsons draws both general statements and factual statements into a theoretical scheme he undermines one of the major raisons d'etre of a scientific theory, that of taking account of more discrete observations which can doubtless range into infinity in terms of general statements.

Parsons' emphasis on the general notion that scientific theory does not provide a "complete description" of the phenomena undergoing examination is largely correct but I think it is slightly an awkward, if not a misleading way to express it. Parsons seems to imply that one of the main functions of theory is description rather than explanation. While it is undeniably true that all description is conducted in the light of assumptions flowing from a theory or a group of theories, it does not furnish anything of an illuminating or informative nature of the phenomenon in question apart from merely characterizing it by such and such properties and no more. On the other hand, an explanation indicates under which conditions and general empirical regularities the phenomenon occurs. Although Parsons'
specific analytical preferences (description over explanation) are, I think, mistaken, one can scarcely take him to task for the general argument which underlies his statements in this connection for it proceeds on firm logical grounds. It is this. A theory does not seek to cast light on every feature of the disparate phenomena to which it may be applied. Reality is too vast and constantly in flux to be encompassed in its entirety by a human conceptual system. Clearly then, the only alternative is that a theory deals with certain aspects of different empirical phenomena; in a word, a theory is abstract and selective in its focus. In consequence of this, suggests Parsons, and rightly so, different theories direct a focus on different facts. All that we ever have are partial phenomena which successively approximate empirical reality or, as Parsons would have it, approach an asymptote.

(d) Several relevant observations present themselves in regard to the somewhat logically ominous locution, "logical closure" which Parsons introduces in his discussion adumbrated here. (89) Although it appears that Parsons follows quite closely a number of scientists and philosophers of science in utilizing this notion, it is, I think, a fairly serious misconception of the general structure of scientific theory. In order to elaborate on this, it would indeed be instructive for present purposes to rehearse very briefly Parsons' fundamental contention in this particular respect. As has already
been noted, Parsons views scientific theory as a system comprised of abstract, logically interrelated propositions with empirical reference. What is most striking about these propositions, according to Parsons, is that they tend to become "logically closed" mainly on account of each theoretical proposition yielding logical implications which, in turn, find their restatement in another proposition inhering in the same theoretical scheme. On the contrary, to be sure, a theory is always logically open-ended. However abstract, the theoretical statements may be, they are, in principle, subsumable (or explainable) by far more abstract propositions (or statements) which, again, are subsumed by other highly abstract statements and so on until triviality or ineffective scientific manageability of these statements is attained. Roughly speaking, then, there are a catalogue of covert assumptions being made in the formulation of a theory which, to be sure, cannot be circumvented and, furthermore, are not, strictly speaking, constituents of the theoretical scheme in question. In view of these considerations it would be, I think, rather gratuitous to say that a theoretical system is logically closed.

What perhaps amounts to a slight variation of the contention just outlined is the infrangible argument -- according to a number of philosophers of science -- which proceeds on the following lines. The terms (or concepts) embodied in a theory's general statements cannot be defined with reference to other terms in the same theoretical scheme without one becoming
ensnarled in a vicious circle and, hence, employing unilluminating procedures. To evade this circularity, one can define the concepts of the theoretical system by employing notions which were not previously defined in the same theoretical scheme. But this also would invariably result in the rather troublous development of an interminable concatenation of definitions of these concepts, the last definition of a concept always relying on a following definition of a concept and so on ad infinitum. Understood in its proper sense, this type of procedure would lead to an infinite regress of sorts. That this unpalatable circumstance should obtain is inevitable but there is no reasonable alternative. (90) To be sure, then, any theoretical system cannot be an autonomous structure, that is to say, logically closed.

There is one further point regarding "logical closure" which requires brief consideration although I must admit it is more expansive and less logically-knit in contrast to the preceding remarks. A scientific theory, being an instrumental device designed to shed light on an aspect of empirical reality with which it concerns itself, is subject to numerous extra-logical procedures by means of which its empirical relevance and explanatory effectiveness can be satisfactorily ensured. This means that the manipulation of a theory's structure in the sense that the general statements are appended or altered or deleted is permissible, given the theory's ability to confront the pertinent empirical observations. The obvious point to be
noted here is that a theory can be modified and adjusted according to what the theorist and researcher sees fit in the light of his observations and investigations. I should think, then, that the contention that scientific theory becomes a "logically closed system" would be a trifle forced, if not entirely inappropriate, given the nature and objectives of scientific inquiry.

(e) Parsons' contention that general theoretical statements and factual statements are found together in a theoretical system does not hold. To do so, I should think, is to give a misleading impression of what genuine scientific theory is. As previously indicated, a theory is comprised of an aggregate of general statements, which, in principle, can be applied to a welter of specific concrete events because the latter, through a series of operational procedures are specific derivations from them or are regarded as subsumed under them, representing their specific manifestations. Since these specific manifestations (or derivations) are potentially innumerable, it would indeed be onerous and self-defeating to include factual statements in a theoretical scheme.

Parsons' views regarding the articulation of theory and facts and the influence each wields over the other are perfectly satisfactory. Quite clearly, facts are discrete, insignificant statements about the empirical world if they are bereft of an explicit theoretical basis. Facts are never intrinsically important but important in terms of a theory.
It is significant to note that Parsons encounters serious difficulties when he asserts that one can expect to see, in the light of a theory, certain empirical occurrences under certain conditions only if the theory is "empirically correct". Viewed in the large, Parsons appears to imply that a theory must be verified before it can throw an explanatory light on what Parsons calls "related fields of fact". Apart from the vague notion of "related fields of fact" to which some discussion will be devoted later here, these views, in my estimation, are highly erroneous. A theory must be applied to a number of diverse yet pertinent phenomena, with each application being looked upon as a test of the theory's tenability. There is, in principle, no one test or set of conditions predominantly determining whether or not a theory is "empirically correct" or, perhaps, phrased more appropriately, the theory has resisted attempts at its falsification. The suggestion that a theory refers to one well-defined dimension of empirical phenomena, particularly the one which prompted the formulation of the theory -- savors of a decided inductivist tinge. Above all, as has already been noted, a theory is a disciplined conjecture usually about the general connections holding between certain empirical phenomena or, as is most often the case, the connection between certain general facets of the same phenomenon. It should be borne explicitly in mind that a theory embraces, as it were, by a series of general statements (or regularities) one dimension of empirical reality, the multiple specific
aspects of which are brought to light by the various implications flowing from these general statements. Thus, when Parsons employs the bewildering phrase, "related fields of fact", he is either speaking of a theory's far-reaching implications in its realm of applicability or he is arguing by analogy, that if one aspect of reality operates in a certain manner, then another aspect of a similar character will by parity of reasoning function in the same way; for a rough and ready example, let us take the following, if an organic body comprised of cells returns to an operating equilibrium after a disorder such as disease or illness then a social structure comprised of norms and values (as harbored by individuals) also returns to an orderly equilibrium after disruptions such as crime wave or war. The main point to note, then, is this. If Parsons is suggesting that a theory can account for a wide range of phenomena owing to its property of yielding numerous implications when he employs the locution "related fields of fact", then his contention, I would believe, is philosophically acceptable but his terminology rather, unfortunately, misleading. If, on the other hand, Parsons is pertaining to an argument by analogy, then his statements are fraught with errors and fundamental misconceptions of scientific argument; for, to be sure, argument by analogy is only clear and effective as the items which are undergoing comparison are relevantly similar and this would require an analysis of each individual argument to ascertain the legitimacy of the analogy; more to the point,
an argument by analogy is by its very nature indirect because one item is being explained in terms of another item, doubtless an inadequate procedure in scientific discourse where critical awareness and clarity is of the utmost necessity. Thus, in regard to Parsons' use of the locution "related fields of fact" all indications point toward the latter argument (that of argument by analogy).

(f) If we construe Parsons' term "verification" in the widest possible sense, that of an empirical statement meeting relevant empirical observations without discrepancy, then a few significant observations, I think, can be proffered. Very generally speaking, there can be little dispute with Parsons' contention that verification is a crucially important undertaking in science, distinguishing it from the uncertain terrain of metaphysics. Also, Parsons' proposal that a theory must be subjected to empirical examination by a devised test, provided, of course, the data admit of some manipulation, is a wholly desirable scientific procedure; such a test enables one to examine the range and utility of a theory through many of its diverse implications. But Parsons is indeed circumspect - and quite rightly so - in his advocacy of this mode of empirical analysis. Improvement of testing conditions and instruments belong to the field of practical techniques and do not issue from the logical facets of scientific theory.

It is worthy of note that Parsons characterizes the relationship between theory and facts as one not of "truth" or "falsity"
but one of "fitness" or suitability. A theory, in varying degrees, "fits" the data with which it concerns itself. It may be inferred here that, for Parsons, the closer the fit, the better the theory, and, conversely, the greater the divergence of the theory from the data, the less useful it is. This criterion which Parsons implicitly adopts, I should think, bears the earmarks of the instrumentalist approach concerning the nature and uses of scientific theory.

There can be no cavil with Parsons' argument that the verification of a theory is essentially a parsimonious affair in the sense that the empirical evidence for a general statement has implications for the remainder of related general statements and their implications in that same theoretical scheme. In consequence of this argument, Parsons correctly maintains that, if the theory's expectations conflict with the relevant empirical observations, it must undergo reconstruction, taking account of the untoward findings, if it is to be of any reliable scientific use.

Once again Parsons does not enlarge upon what he means by terms such as theoretical propositions and theoretical expectations. Whether these statements are analogous to what we would call explanations or predictions, one is not given any helpful indication. In a similar vein, Parsons does not elaborate as to what type of procedure comes about when "expectations" are "derived" from a theoretical system.
Generally, all we can do in these circumstances is to speculate and perhaps infuse meanings as best we can into terms which are inadequately defined at the outset. But, inasmuch as disciplined conjecture is often necessary and fruitful in the ascertainment of various implications of a theorist's conception of scientific theory, conjecture is not enough.

(g) At first sight, Parsons' further discussions on "fact" are, on the whole, sound but, I fear, certain obscurities of a critical nature still remain. That a fact is an empirical verifiable statement uttered in the light of a conceptual scheme is doubtless correct, with the proviso that the locution "conceptual scheme" is synonymous with the word "theory". To be sure, a conceptual scheme in the strictest sense (a list of explicitly defined concepts) does not specify causal relations or empirical uniformities of which factual statements are viewed as specific manifestations; to be sure, only a theory performs this function. As has been his wont in his discussion of theory in this volume, Parsons stresses that description (note, not explanation or prediction) of a phenomenon is understood in terms of a theoretical scheme. In a narrow sense, this is true; description does imply a theory but scientific accounts of phenomena endeavour to go beyond this level and explain; that is to say, a scientific account points out the role of a particular phenomenon or an aspect of it with those of other phenomena or aspects thereof in the form of a series of explicitly phrased general statements.
Parsons' utilization of the phrase "all the facts" in the argument that a theory enables one to come to know all the facts of a phenomenon in question is highly deceptive but, I think, his qualifications partially dispel any difficulties arising in this regard. By the phrase "all the facts" Parsons means important facts. Those facts which are adduced in the light of a theory when one is undertaking a descriptive analysis of a phenomenon. In my estimation, Parsons' contention that only a theory, with its assumptions and concepts providing a focus of attention, makes the facts either important or unimportant, is doubtless tenable.

(h) Parsons' characterization of scientific theory as a triple-tiered logical structure (part or type concepts, empirical generalizations, and analytical concepts) involves an assemblage of patent misconceptions of the structural features and functions of scientific theory. Broadly speaking, a continuum of concepts is established which ranges from the relatively concrete to the relatively abstract; each level is logically distinguishable from, yet dependent on the other levels, particularly the abstract levels depending on the lower levels. There is also a tacit assumption in these considerations indicating that the construction of a scientific theory gradually proceeds from the concrete level to the analytical level, undoubtedly, for Parsons, the apex of abstraction in scientific theory.
Beginning from the lowest conceptual level, *viz.*, the "type" and "part" concepts, there are a number of critical observations to be put forward. What Parsons regards as type or part concepts constitute, I think, merely the general linguistic background of the theory, that is to say, these concepts are a part of the absolutely essential vocabulary of the language in which the theory is formulated and do not as such comprise any distinctive feature of the theory in which they may be employed, as Parsons seems to think. Clearly, a scientific theory is not formulated in a linguistic vacuum; (91) there is always a reference to notions and words outside the purview of the theoretical system, otherwise one could not begin to theorize. Moreover, many of these concepts, almost invariably, are governed by other theories, their exact natures usually unknown. Contrary to Parsons' contentions, reality is sliced into units, as it were, in the light of general statements of relations between variables (or concepts) in other words a theory. Furthermore, Parsons observes that these concepts employed in certain frameworks are closely akin to the Weberian "ideal type". But a moment's reflection of the character of an "ideal type" concept discloses that it, in effect, is a miniature theoretical system comprised of testable hypotheses of varying degrees of abstraction. It appears, then, that the part or type concepts are not the structures which Parsons mistakenly conceives them to be.
The conception of an "empirical generalization" is perhaps one of Parsons' clearest and soundest characterizations of an integral feature of a theoretical scheme. Yet his decision to categorize it as an intermediate conceptual level, between the type concept and analytical concept, is indeed perplexing. At first blush, it appears that an empirical generalization is, in essential respects, comparable to what I have variously called in this study a general statement (or empirical law). Through the employment of part or type concepts, an empirical generalization in what seems to be an arbitrarily restricted degree of abstraction provides explanations of concrete phenomena, almost precisely in the same manner a general statement does. Why this notion (of empirical generalization) is qualitatively distinguished from analytical laws which are only substantially more abstract, is difficult to comprehend. What is more, the manner in which these generalizations are acquired resembles that of an inductivist's approach, taking one's cue, as it were, from the alleged patterns of behavior, the empirical referents of the type or part concepts display in the empirical world. To be sure, this mode of argument runs patently counter to the general theme which Parsons has persistently alluded to on many occasions in this volume -- that the interpretation of empirical events are undertaken only in the light of a theory or a spate of theories.

Though plausibly argued at first sight, Parsons' references to the level of the analytical concepts in theoretical discourse
exhibits, I think, errors of an elementary kind at various points. That there are universal concepts in scientific discourse, particularly in the statement of abstract empirical laws or uniformities, can hardly be contested. To be sure, without them, there would be no abstract laws or statements of general regularities. However, Parsons goes awry, conceptually speaking, when he suggests that the intricate definition of a series of analytical concepts is a preliminary stage for the eventual formulation of a set of highly abstract analytical laws into which these analytical concepts will be incorporated. Here, Parsons is proposing that one "theorize from above"; in other words, he suggests that a collection of refined concepts should be first devised. And, then, by some unexplained procedure, this will be conducive to the formulation of a set of laws of a highly abstract character (or general statements, if one prefers). This avenue of approach, however, carries a decidedly inductivist taint; apparently, there is a mysterious passage from a number of discrete concepts to empirical laws. Understood in its proper sense, the development of a theory begins with several conjectural, yet abstract assumptions, specifying a relationship between certain general concepts (or variables as they may be called). These concepts draw their basic meaning in terms of the assumptions in which they are embedded, not defined antecedently to their inclusion in a general statement.
To be explicit about it, there are no distinctive conceptual levels inhering in scientific theory as Parsons suggests. The general statements (or laws) can be of any conceivable degree of abstraction. The concepts contained in these general statements are, more often than not, affixed certain empirical values by various interpretational operations. It is, then, at this point when the theory is applied to what is considered to be the relevant empirical observations. These observations viewed as specific manifestations of the employed general statements, are phrased in concrete terms which via the same interpretational operations are retranslated into the relevant general statements. Neither, incidentally is more certain than the other; the factual statement requires the theory in order that it may possess meaning and, in turn, the theory requires factual statements to indicate its positive or negative, empirical support; without this support, a theory cannot be adjudged correct or incorrect, (or perhaps better put, useful or unuseful).

Generally, a theory is assessed in terms of the extent of its abstraction. This largely depends on the character and range of its general statements. Factual statements do not constitute a separate conceptual level per se, or are they for that matter, explicit components of a scientific theory. In effect, they are implied in the general statements and, as I previously mentioned, are of potentially infinite number, much being contingent on our empirical investigations of
reality to inform us in this respect. All this sharply contrasts with Parsons' gratuitous tri-partite classification of the conceptual levels of a scientific theory.

Parsons' reference to the argument that theory of a sufficiently abstract nature need not be formulated in quantitative terms, strongly holds. Theories couched in non-quantitative terminology can be equally as precise and wide in scope as, say, a quantitatively based theory. There is no necessary logical superiority of the quantitative formulation over the non-quantitative type (92); the one main advantage that can be perceived is that the former may seem to be more convenient for the avoidance of onerously complex, verbal formulations.

(i) Parsons' interesting notion of a residual category, in my estimation, is a totally superfluous formulation which seizes upon an obvious property of the use of a language and endeavors to raise it to a significant scientific principle. Above all, it is tautologous. It is primarily predicated on the trivial notion that all ideas and concepts which are not employed in a theoretical system automatically become unusable categories, and can be distinguished from those that are used; in a word, residual categories. This is clearly not a fruitful conceptual distinction to make when virtually an infinite diversity, literally a pottage, of notions can be indiscriminately thrown together and labeled under a vague general name. Significantly, this conception in large part resembles
the linguistic background factor of the type and part concepts, earlier discussed here, which is an ubiquitous element in any theoretical endeavor. And Parsons ostensibly converts it into a scientific principle. That disparate ideas, concepts, or empirical knowledge must be utilized in theoretical discourse, particularly in theory formulation and refurbishment, certainly does not imply that the linguistic context from which these concepts are taken represents an especial, separate category in theoretical discourse; for, surely, theoretical work of any kind occurs in such a comprehensive framework.

(j) Concerning the functions of scientific theory which were inferred from Parsons' discussion, there are only several general remarks to be made since many of the critical comments have already been presented in my critical comments on various theoretical issues contained in this volume. Parsons' repeated stress on description rather than explanation suggests a basic misunderstanding of the object of scientific activity. The question, "Why certain events occur, as they do?" is doubtless the very heart of all scientific inquiry, with description playing a comparatively minor role in the attainment of a reasonably adequate answer to it. However, when Parsons does refer to explanations, he dichotomizes the types of explanation such that there are apparently, explanations effected by analytical laws, if any are available in a specific discipline, and, also, "causal explanations" of specific
phenomena which are expressed wholly in terms of specific statements. Regarding the latter point, it is elliptical to speak of an explanation phrased entirely in specific terms paramountly because a general empirical uniformity (or general statement or law) is invariably presupposed. In any event, when Parsons employs the concept of explanation in his discussion, the various logical relationships and processes it entails are never clearly spelled out or even hinted at.

On the whole, Parsons' conception of scientific theory, as presented in The Structure of Social Action (1937) bears the trappings and terminology of the arguments on scientific theory appearing at that time in the natural sciences. Close inspection, however, reveals that his understanding of scientific theory is replete with misconceptions of fundamental issues and logically untenable assertions as these criticisms, I trust, have shown. But, to Parsons' credit, two overwhelmingly important factors about theoretical argumentation are persistently emphasized and, after a fashion, are cogently brought into bold relief. One is that all factual statements are affirmed in the light of theoretical assumptions; in other words, a theory is antecedent to empirical observations and investigations. The other factor pertains to the property of a theory enabling a number of its general statements to be tested when one logically related statement of that theory is subjected to empirical test.
E. "The Role of Theory in Social Research" (1938)

This journal article (93) represents Parsons' first philosophical statement on scientific theory after the publication of The Structure of Social Action. At bottom, I would say that the considerations presented here are closely analogous to those outlined in the larger volume.

It is of significance to note that Parsons still directs his animus toward the "empiricist" in the social sciences who forsakes theory altogether because it is metaphysical and deals solely with concrete empirical events. Parsons considers this a ludicrous methodological position to adopt. For, facts in and of themselves are of little significance. Large aggregates of discrete facts, as a consequence do not advance our knowledge in any way.

In the first place our study of fact, however little we may be aware of it, it is always guided by the logical structure of a theoretical scheme, even if it is entirely implicit. We never investigate "all the facts" which could be known about the phenomena in question, but only those which we think are "important". This involves a selection among the possible facts. Now if we investigate carefully, though few empiricists do, what is the basis of this selection, it will, I think, uniformly be found that among the criteria of importance and the only ones of strictly scientific status is that of their relevance to the logical structure of a theoretical scheme. (94)

And

But it is the essence of the ordering function of theory that any old facts, however true, will not do,
but only those which "fit" the categories of the system. What facts it is important to know are relative to the logical structure of the theory. This is not to be understood to mean that theory should dictate factual findings, but only the definition of the categories into which the findings are to be fitted. (95)

From a hypothetico-deductive standpoint, there is little against which one can significantly argue.

Concerning the structure of theory, it is noteworthy that Parsons still commits the same error as he does in The Structure of Social Action. He views theory as "a body of logically interrelated generalized concepts. Consider what he writes in this connection.

Analytical theory in the sense in which I mean the term here, is a body of logically interrelated generalized concepts (logical universals) the specific facts corresponding to which (particulars) constitute statements describing empirical phenomena. Use of this concept in empirical research inherently tends to establish logical relations between them and their particular content (values) such that they come to constitute logically interdependent systems. Correspondingly the phenomena to which they apply come to be viewed as empirical systems, the elements of which are in a state of mutual interdependence. (96)

It appears that Parsons goes further in his insistence that a theory is a framework of universal concepts than he did in The Structure of Social Action. For now, these concepts by some mysterious process naturally lead to the establishment of logical relations, in the form of statements which describe relevant empirical phenomena. Suffice it to say that Parsons is enter-
taining exotic notions about scientific theory. Parsons' conception of theory here admits of a strong, perhaps unintended, inductivist bias, that concepts inherently produce statements. Clearly, concepts by themselves cannot do anything. They must be embedded in a statement of an explanatory character before they can be considered important. As was noted on previous occasions a logical relation only obtains between statements (or propositions) not concepts. As a final remark here, it may be remarked that the notion of "empirical systems" is indeed baffling. Theories apply to phenomena, to any type of relevant phenomena. A system is a mental construction for various purposes, an artifact, as it were. Empirical phenomena just exist; they are not arranged in any identifiable units as Parsons seems to imply. Thus, to affix the term systems to the empirical phenomena which a theory refers to is to engage in logical confusion and raise it to a scientific principle.

Again there is no reference to explanation, but to description. As was indicated earlier, scientific enterprise endeavors to account for (explain) phenomena instead of describe it which, incidentally, can be undertaken with a bundle of theories which are unacknowledged by the describer.

For the first time Parsons ventures to comment on measurement and its relation to theoretical work. His fundamental argument is that measurement is not logically requisite for scientific work; it is a convenient device when relevant problems and data
demand it. But, he contends, measurement cannot supplant theory. Without the presence of a theoretical scheme, a measured datum holds little significance until it can be incorporated into one. Parsons writes:

One important group of social empiricists is particularly partial to measurement. They point out the extreme importance of measurement in physics and conclude that only so far as its facts are the results of measurements can sociology claim the status of a science. I do not wish to depreciate the value of measurement wherever it is possible, but I do wish to point out two things: First, the importance of facts is relative to the way in which they can be fitted into analytical schemes: measurements are fundamental to physics because many of its variables are such that the only facts which make sense as their values are numerical data. But numerical data are far less scientifically important until they can be so fitted into analytical categories. I venture to say this is true of the vast majority of such data in the social fields.

Second, measurement as such is not logically essential to science, however desirable. Measurement is a special case of a broader category, classification. It is logically essential that the values of a variable should be reducible to a determinate classification. But the classification they admit of may be far more complex than the single order of magnitude which measurement requires. Where nonmetrical, even non-quantitative data can, with the help of such classification, be made to fit directly the logical structure of an analytical scheme it may be possible to establish relations of crucial importance which any amount of numerical data lacking such analytical relevance could not bring out. (97)

With the exception of several minor points of issue, the general drift of Parsons* remarks in connection with the subsidiary role of measurement to the formulation of theory (since it imparts meaning to the former) are fairly sound. (98) Measurement is only useful to the degree that the data, which is subjected
to measurement, is accountable within a theoretical scheme.

In essentials, scientific theory, in Parsons' view serves four functions. As such they are:

1. In the vast welter of miscellaneous facts we face it provides us with selective criteria as to which are important and which can safely be neglected.

2. It provides a basis for coherent organization of the factual material thus selected without which a study is unintelligible.

3. It provides a basis not only of selection and organization of known facts, but in a way which cannot be done otherwise reveals the gaps in our existing knowledge and their importance. It thus constitutes a crucially important guide to the direction of fruitful research.

4. Through the mutual logical implications of different analytical systems for each other it provides a source of cross fertilization of related fields of the utmost importance. This often leads to very important developments within a field which would not have taken place had it remained theoretically isolated. (99)

The first three functions, in my view, are simply different renditions of one theme: that theory guides our observations, assisting us in selecting or sorting what is important and what is not important in any phenomena which may interest us. The fourth function, I must admit, is rather difficult to understand because of its vague generality and presupposition of unstated viewpoints on the range and depth of studies undertaken by those working in different disciplines; moreover, Parsons' locution, "mutual logical implications" can create problems for interpretation because of the many, varying meanings that Parsons has previously ascribed to it.
It is worthy of note that Parsons does not explicitly refer to the notion of explanation as an activity or function of theory. For Parsons, theory organizes our knowledge, gives meaning to, provides criteria, reveals lacunae in our knowledge. However, it appears to me, in an intuitive way, that these diverse terms imply the notion of explanation.

Aside from the inclusion of several remarks on the factor of measurement in scientific enterprise, particularly its relation to a theoretical scheme, Parsons' conception of scientific theory remains the same when it is compared to the extended discussion in *The Structure of Social Action*.

F. "The Present Position and Prospects of Systematic Theory in Sociology" (1945)

In this essay, Parsons tackles a variety of issues concerning the character and functions of scientific theory as such. The approach adopted is in large measure similar to the one that was employed in *The Structure of Social Action* (1937) and "The Role of Theory in Social Research" (1938). However, what is significant about this discussion is the introduction of a general characterization and rationale for the structural-functional mode of theory.

At the outset, Parsons puts into bold relief his criterion of what a sophisticated, fertile scientific discipline should represent. In large measure the degree of the abstraction of
the theory employed in a particular science serves as a significant indicator as regards its maturity.

It is scarcely too much to say that the most important single index of the state of maturity of a science is the state of its systematic theory. This includes the character of the generalized conceptualized scheme in use in the field, the kinds and degrees of logical integration of the different elements which makes it up, and the ways in which it is actually being used in empirical research. (101)

Very broadly speaking, Parsons' general criterion is an eminently acceptable one in scientific enterprise. The level of abstraction of a theory is instrumental in furthering our knowledge about an aspect of the empirical world by explaining more (or to put it in another fashion, a theory takes into account more of the empirical world than was previously possible by the former theoretical frameworks). Lurking behind much of what Parsons suggests in this proposal is the untoward implication that a discipline has (and there should be) one and only one type of theory germane to it. In most scientific disciplines — in both the natural and social sciences, at that time and at the present — this state of affairs was and still is not the case and, moreover, from a scientific point of view, is undesirable for, it is unwarrantedly restrictive and smacks of dogmatism. Phenomena can be viewed and explained from a multitude of theoretical perspectives as, interestingly enough, Parsons himself had stated in *The Structure of Social Action*. (102)

Parsons' conception of scientific theory, in essentials, remains the same as the one presented in *The Structure of Social Action*. Note:
"Theory is a term which covers a wide variety of different things which have in common only the element of generalized conceptualization. The theory of concern to the present paper in the first place constitutes a "system" and thereby differs from discrete "theories", that is, particular generalizations about particular phenomena or classes of them. A theoretical system in the present sense is a body of logically interdependent generalized concepts of empirical reference. Such a system tends, ideally, to become "logically closed," to reach such a state of logical integration that every logical implication of any combination of propositions in the system is explicitly stated in some other proposition in the same system.

In a highly developed system of theory there may be a wide variety of different types of generalized concepts and functions which they may serve. A thorough discussion of the possibilities cannot be undertaken here, so attention will be confined to those most vital to the general status of the scientific field. The two most general functions of theory are the facilitation of description and analysis. The two are most intimately connected since it is only when the essential facts about a phenomenon have been described in a carefully systematic and orderly manner that accurate analysis becomes possible at all. (103)

The main concentration of emphasis on concepts as the main factor of a theory, as I have indicated in my earlier criticisms, is to move in the wrong direction in scientific thought. Concepts presuppose theories, not vice versa as Parsons here asserts. With regard to the property of a theoretical system becoming "closed", it is important to note, as before, that "logical closure does not pertain to scientific theoretical systems for the fundamental reason that a theory is always reliant on basic assumptions which go far beyond its boundaries, as it were. (104) Concerning the functions of scientific theory - description and analysis - some difficulty is encountered. What does Parsons mean by "analysis"? Is it synonymous with explanation"? One does not know because no basis for a definite decision is offered in this regard.
In any event, let us continue. Although it is indisputable that a theory does facilitate description which is, to be sure, a pedestrian activity, science endeavors to undertake something of far greater moment; it, above all, seeks to furnish verifiable explanations of certain phenomena. Much is not gained from description itself, as an analytical procedure in scientific investigation. (105)

In keeping with his heavy stress on concepts as key elements in all theoretical systems, Parsons argues that a theoretical system must have connections especially with two types of conceptual elements. One is the general frame of reference and the second is the structure of systems. The frame of reference is the most general constellation of categories available which enables scientific enterprise to be understood in the sense; that is to say, the categories which are employed in a theory refer for their definition and descriptive adequacy to the general familiar concepts which reside in the frame of reference. As Parsons puts it in his inimitable way:

This is the most general framework of categories in terms of which empirical scientific work "makes sense." Thus, in classical mechanics, three-dimensional rectilinear space, time, mass, location, motion are the essential elements of the frame of reference. Every descriptive statement, to be applicable to a mechanical system must be referable to one or more "particles" each with a given mass, capable of location in space, changing its location in time through motion, etc. Besides providing the specific categories in terms of which a system is described, the function of the frame of reference is above all to provide a test of the determinacy of the description of a system. It is a logical implication of the structure of the conceptual system that there is a limited number of essential categories, specific values for which must be obtained before the description can be determinate. Its use is the
The implications of these remarks admit of two interpretations. One concerns the use of language in theoretical contexts. It appears that what is being laboriously suggested here is that all concepts embedded in theoretical schemes should be defined in terms of the vocabulary of our language from which the terms of theories derive their basic meanings. Another possible interpretation indicates that Parsons is making a reference to a metatheory, a dimension of diverse concepts and assumptions on the nature of the empirical world. Several brief considerations are thus indicated. At bottom, metatheory concerns itself with the critical discussions of fundamental presuppositions and concepts of scientific knowledge and theory work, i.e., how much are we permitted to assume about empirical phenomena in general before we launch our inquiries into specific spheres of study? In other words: Shall we agree that all A's (whatever they may be) found in the world are equivalent to each other (that is $A = A$)? Responses to these questions are predicated on philosophical arguments and must be decided prior to the formulation of a theory, let alone an empirical inquiry. In a similar fashion, queries about the structure, uses, and empirical corroboration of scientific theories are posed. The following type of questions, among many, can be asked: Why should we have general propositions? What are the advantages or disadvantages of employing a structural-functional theory? How are we to assess the evidence in terms of the theory?
Why is deduction important or why is it irrelevant? Again the replies to these questions are framed in terms of predominantly philosophical arguments. In any event, of the two interpretations I have suggested, I would think the latter is the one most probably intended, though there are overtones of the first interpretation present, as well, in Parsons’ remarks.

The second conceptual element is the structure of system. Parsons observes, at the outset, that empirical phenomena which are interrelated compose a system. In consequence of this, they are also interconnected on the rather abstract structural plane for purposes of descriptive analysis. In Parsons’ view, structure is static when a descriptive analysis of a system is undertaken. On the structural level, units, or subsystems and their structural interrelations, comprise what Parsons generally calls a "system"; these units can, in principle, exist in a self-contained manner, detached from the system of which it is a basic constituent.

What purpose this structural aspect is designed to serve is not at all clear. Moreover, the introduction of systems is introduced uncritically. Parsons here seems to imply that empirical phenomena are intrinsically organized in systems. But this is a piece of scientific fiction. To affirm that there is such an entity as an empirical system is to invite gratuitous bewilderment. For, a system is a convenient, artificial construction which brings together a plethora of heterogeneous items for various purposes an individual may have in view.
The functions of the frame of reference and structural categories, in Parsons' view are two-fold; they are nothing less than the major objective of scientific analysis:

The functions of the frame of reference and of structural categories in their descriptive use are to state the necessary facts, and the setting for solving problems of dynamic analysis, the ultimate goal of scientific investigation. Besides the immense possibilities of variation in the scope of analysis, there are two aspects of the goal itself; first, the "casual explanation" of past specific phenomena or processes and the prediction of future events; second, the attainment of generalized analytical knowledge, of "laws" which can be applied to an indefinite number of specific cases with the use of the appropriate factual data. The attainment of the two goals, or aspects of the same goal, go hand in hand. On the one hand, specific causal explanation is attainable only through the application of some generalized analytical knowledge; on the other, the extension of analytical generalization is only possible by generalization from empirical cases and verification in terms of them. (108)

The goal of scientific investigation, for Parsons, is, to put it mildly, vague, almost incomprehensible. What is also baffling is his usage of the terms "casual explanation","prediction", and "law". In effect, Parsons argues that causal explanation is anterior to laws whereas, in terms of the criteria of hypothetico-deductive theory, a law is an indispensable factor in explanations. There is a tacit assumption in all these remarks that knowledge of a certain general order must first be acquired before "laws" can be fully developed, a sort of inductivist tinge imbues this aspect of Parsons' argument. It is further suggested that laws must be applied to a vast range of specific empirical events. But ironically, a reason for this procedure is not furnished. Perhaps for explanation? But this is out of the question since causal
explanation is the first sub-goal of science whilst the development of laws constitute the second sub-goal? Apparently, no answer is forthcoming to this overwhelmingly crucial question either from the remainder of the quoted passage or the essay. It is significant to note that in the latter half of the last sentence in the above quotation, an unvarnished inductivist approach is being suggested: "... on the other, the extension of analytical generalization is only possible by generalization from empirical cases and verification in terms of them". (109) To generalize from empirical cases is to fall into the snare of one camp of inductivists who naively propose that generalization is possible only by an examination of a multitude of concrete empirical instances. Generalization is achieved through the formulation of greater generalized statements or hypotheses, not by extensive empirical evidence; the latter procedure only ensures that a generalization on a particular level has been usually tested in diverse and numerous ways.

It perhaps would be no exaggeration to affirm that Parsons' conception of the goals of science and his understanding of the logical features and functions of theory, in this essay undergoing examination here, are exceedingly obscure, and, on many points, simply fraught with errors and misconceptions. Be that as it may let us observe, however, the manner in which he introduces and justifies the employment of a functional argument.

The advancement of scientific knowledge proceeds on the
sure lines of dynamic analysis. It essentially entails the simultaneous investigation of a corpus of interdependent phenomena.

To amplify:

The ideal solution is the possession of a logically complete system of dynamic generalizations which can state all the elements of reciprocal interdependence between all the variables of the system... (110) (My emphasis - E.R.G.)

This type of analysis must constantly and consistently refer virtually every pertinent problem to the state of the entire system in question. However, this implies that there is a stable structure of a system such that the interdependent elements can be effectively examined. The structure of a system is an "analytical tool" which

... ensures that nothing of vital importance is inadvertently overlooked, and ties in loose ends, giving determinacy to problems and solutions. It minimizes the danger, so serious to common-sense thinking, of filling gaps by resort to uncriticized residual categories. (111)

...Structure does not refer to any ontological stability in phenomena -- to sufficiently stable uniformities in the results of underlying processes so that their constancy within certain limits is a workable pragmatic assumption. (112)

If a system's structure is conceived as an integral element of dynamic analysis, the structure's static categories and their many specific factual statements must be coupled to the dynamic variables embodied in the system. According to Parsons, the concept of function effects this connection by furnishing criteria of the relative importance of dynamic elements and processes within the system in question. Each element or process is
deemed important in the sense that

... they have functional significance to the system, and
their specific functional relations between the parts of
the system and between it and its environment. (113)

The concept of function logically requires the assumption that
the empirical system (the interconnected empirical variables in
other words) must be viewed as a going concern. Accordingly, the
system's structure is constituted of patterned operations which
evince a marked tendency to be maintained, structurally speaking,
or, on a dynamic view, show a propensity to develop in terms of
a pattern which is empirically invariant.

One of the most striking features of Parsons' functional
theory is its eminently teleological character.

Functional significance in this context is inherently
teleological. A process of set of conditions either "contributes" to the maintenance (or development) of the system
or it is "disfunctional" in that it detracts from the
integration, effectiveness, etc., of the system. It is
thus the functional reference of all particular conditions
and processes to the state of the total system as a going
concern which provides the logical equivalent of simultan­
eous equations in a fully developed system of analytical
theory. This appears to be the only way in which dynamic
interdependence of variable factors in a system can be
explicitly analyzed without the technical tools of mathe­
matics and the operational and empirical prerequisites of
their employment.

The logical type of generalized theoretical system under
discussion may thus be called a "structural-functional system"
as distinguished from an analytical system. It consists of
the generalized categories necessary for an adequate descrip­
tion of states of an empirical system. On the one hand, it
includes a system of structural categories which must be
logically adequate to give a determinate description of an
empirically possible, complete empirical system of the
relevant class. One of the prime functions of system
on this level is to insure completeness, to make it meth­
odically impossible to overlook anything important, and
thus explicitly to describe all essential structural elements and relations of the system. For if this is not done implicit, uncriticized allegations about the missing elements will always play a part in determining conclusions and interpretations. (114) (Emphasis in original.)

According to Parsons the utility of such a theoretical approach has been ably demonstrated on more than one occasion in the field of physiology, citing the book which had considerably influenced him in structural-functional theory, viz., W.B. Cannon's *The Wisdom of the Body* (1932).

To confront each single point of dispute and error in Parsons' argument outlining the structure and rationale for structural-functional theory would surely be to engage in a prolix, involved discussion on a multiplicity of minor details. I should like to remark, however, on perhaps the most salient besetting problem of most proposals concerning functional theory which Parsons here, I think displays in bold lines; that is, its fragmentary character as a scientific theory. It should initially be noted that there is, in principle, no objection to functional theory, provided that any one of its relational statements can be reformulated without distortion of meaning into regular general propositions (or general statements or laws) in the form — "If A ... then B ..." Fundamentally, a functional explanation is, as I prefer to label it "an inverted explanation" of the premises employed in hypothetico-deductive theories. As a consequence of this logical symmetry, it becomes evident that any functional theory (which also endeavors to furnish explanations) must refer to a
set of general statements (propositions or laws) of which the particular events taken into account are subsumed rather than move entirely on the plane of concrete terms which, at any rate, implicitly refer to a number of such general statements (or laws). (115) But in his metatheoretical discussion on the necessity of functional theory in social science, Parsons does not once allude to the importance of general laws or explanation. It may be noted that the lack of concern for these elements, which are basic to theoretical discourse, is also manifested in his earlier characterizations of scientific theory.

Although my judgment may be somewhat austere, I would say that Parsons presents chiefly an analogical framework by means of which any diverse collection of elements can be descriptively interrelated. As it is, with Parsons, anything in a social science can form a system. No general theoretical guides or criteria are offered in regard to what would be required for a collection of diverse elements to fail to compose a system. System, then, is a concept with too vast a spectrum; clearly, if a concept applies to everything, it means nothing; it is a tautologous notion if employed with little critical consideration.

Very broadly speaking, Parsons' conception of the nature and uses of scientific theory in this essay is much the same as those contained in his theoretical works in the early 1940's and late 1930's. What is most significant in this presentation, however, is the introduction of a discussion and rationale of a structural-functional theoretical approach in its bare essentials, which
eventually came to characterize a considerable portion of Parsons' subsequent substantive work.

G. "The Prospects of Sociological Theory" (1950)

In a rather expansive discussion here on a number of topics concerning sociological theory, Parsons, at one juncture, devotes some brief yet pertinent considerations to the functions of theory for empirical research and for the enlargement of our vistas of knowledge in general. Basically, the argument represents scarcely any modification of previous discussions in this respect. But it so happens that Parsons expresses his viewpoint with surprising clarity, though with a sufficient degree of generality, such that "capture" by a concentration on specific issues is rendered difficult. In any event let us continue.

General theory, as contrasted with specific theories, is a device which orients the thought and empirical observations of theorists and researchers in a certain manner toward the empirical world. This structures and clarifies their already existing knowledge of an aspect of the world and facilitates attempts in further clarification or acquisition. This, broadly speaking, structures and clarifies their already existing knowledge of certain facets of the world and, moreover, immeasurably facilitates active attempts in further clarification or acquisition of this knowledge. Theory also is a significant factor in the formulation
of hypotheses by providing definite avenues of approach in the
analysis of a certain class of empirical phenomena. According to
Parsons, a theory also supplies a touchstone which can be employed
in the examination of other theoretical contributions and empiri-
cal statements. In capsule form, Parsons notes as follows:

At the very least, then, general theory can provide
a broadly orienting framework. It can also help to
provide a common language to facilitate communication
between workers in different branches of the field.
It can serve to codify, interrelate and make available
a vast amount of existing empirical knowledge. It also
serves to call attention to gaps in our knowledge, and
to provide canons for the criticism of theories and
empirical generalizations. Finally, even if they cannot
be systematically derived, it is indispensable to the
systematic clarification of problems and the fruitful
formation of hypotheses. It is this organizing power of
generalized theory even on its present levels which has
made it possible for even a student like myself, who has
done only a little actual empirical research, to illum-
inate a good many empirical problems and formulate
suggestive hypotheses in several fields. (117)

With much of this, anyone who adopts a hypothetico-deductive
approach, can agree. However, it is to be noted that Parsons does
not furnish his conceptions of scientific theory which generally
speaking, gives force and meaning to these functions.

Another aspect of Parsons' argument which I think is most
important is the reference which is made to "special theories" (118)
which are distinguished from "general theory". The former primarily
deals with problems in concrete, empirical fields such as, let us
say, the family, religion, etc. The statements a specific theory
proffers are largely empirical and seldom general, hence, the
appellation, special theory. On the other hand, general theory is
typified by its high abstract nature and, by definition its wide applicability. It invariably subsumes the special theory. But, I should think that this distinction is indeed spurious and rather unfruitful to maintain. Theories, as was noted in Chapter Three, are of varying abstraction depending on a multitude of factors. The most significant factor is undoubtedly the complexity of the data such that sufficiently noteworthy (in a scientific sense) general statements are rather difficult to formulate. Or is Parsons pertaining to descriptive statements of a specific range of phenomena as constituting "special" theory? Should, however, special theories be tantamount to descriptive statements, then, I should think, much commotion and confusion has been created by an infelicitous employment of terms. I fear there is nothing of a definitive character proffered by means of which one could adjudicate in one direction or another in this connection. Again, there is a conspicuous absence of any comment on explanation or prediction.

H. Toward a General Theory of Action (1951) (119)

Although the monograph in this volume, "Values, Motives, and Systems of Action", mainly concerns itself with problems of substantive empirical import, Parsons, in close collaboration with Edward Shils, still manages to interpolate metatheoretical comments
which, I believe, throw some light on his thought in regard to the structure and function of scientific theory as such. The broad framework of his thought in this respect is not marked by any significant modification or innovation. What Parsons does, however, is that he amplifies to a considerable degree his thinking on scientific theory as such.

Parsons, along with eight other social scientists in the introductory section (120), begins by maintaining that scientific theory for the social sciences are viewed as having three basic functions. The first is codification which presumably means the development of an elaborate taxonomy, a system of concepts. It permits the comparatively easy location and definition of the borders of our ignorance and, as well, our knowledge. It is, among other things, a convenient device by means of which problems can be selected. In addition to all this, codification assists in the formulation of hypotheses of a significantly general nature; for, as Parsons notes:

...the systematic reformulation of existing facts and insights, by extending the range of implication of particular hypotheses, and by unifying discrete observations under general concepts.... In making us more aware of the interconnections among items of existing knowledge which are now available in a scattered fragmentary form, it will help us fix our attention on the points where further work must be done. (121)

What we see here, once again, is Parsons' insistence on laying a preponderant stress on the construction of a scheme of concepts to which he attributes an almost magical efficacy in the sense
that they bring out - or help bring about - general hypotheses which delineate certain links among discrete phenomena and our knowledge of various facets of other phenomena. It should be clear that only explicit statements of relations assert anything of importance, whether it be erroneous or correct. Concepts alone do not assert anything unless they are explicitly chained to a theory. Plainly, concepts and general hypotheses are coeval, neither one precedes, neither one is logically superior to the other. As regards the second function "a guide to empirical research", theory, which, according to Parsons furnishes "systematically derived theoretical hypotheses" will ascertain whether or not the theoretical scheme is empirically reliable and, as a consequence, is in need of alteration. With this, there is little cavil; Parsons is largely correct. The third function, refers to the "point of departure for specialized work in the social sciences". What Parsons understands by this is essentially a programmatic resolve. Theory, then,

...will facilitate the control of the biases of observation and interpretation which are at present fostered by the departmentalization of education and research in the social sciences. (122)

At bottom, Parsons is suggesting the adoption of a general theoretical approach which is sufficiently abstract, in order that it subsumes - and unifies - the disparate contributions which are put forward by related disciplines in the social sciences. With a general theory, the fruits of the various social sciences are capable of being accounted for. General, embracing theory rather than petty disciplinal interests is simultaneously enjoined and
considered as a major function of theory. Since one of the widely accepted goals of science is the expansion of the horizons of knowledge by progressively greater degrees of abstraction of the explanatory devices (theory) that are used, no reasonable objection to these remarks can be launched, I think, with any great effect if the criteria of hypothetico-deductive theory are accepted as working theoretical principles.

In the monograph, "Values, Motives, and Systems of Action" (123), in this volume, Parsons, with Shils, conceives any attempt at systematic theory (conceptual schemes) involves four distinctive levels of abstraction. Beginning with the most primitive level (primitive only in regard to the ultimate objectives of scientific endeavor which, by the way, are not specified) we have the following "levels of systematization":

1. ad hoc classificatory systems.
2. categorial systems.
3. theoretical systems.
4. empirical-theoretical systems.

The first type involves the use of more or less arbitrary classes for the sake of making summary statements about the subject matter. No attempt is made to fit the classes to the subject matter in such a way that the relations among the classes will be patterned upon the relations among the items of the subject matter summarized by these classes. The classes are quite independent of one another and any relations which may be discovered must come from ad hoc researches. Such common-sense classifications as that of "fish, flesh, or fowl" are illustrative of this type of classificatory system.
The second, the categorial type, involves a system of classes which is formed to fit the subject matter, so that there are intrinsic relations among the classes, and these are in accord with the relations among the items of the subject matter. Thus, in these systems, the principles of classification, themselves, include statements of certain relationships among classes. The elements are so defined as to constitute an interdependent system. And the system has sufficient complexity and articulation to duplicate, in some sense, the interdependence of the empirical systems which are the subject matter. A categorial system, thus, is constituted by the definition of a set of interrelated elements, their interrelatedness being intrinsic to their definition. Thus in classical mechanics such concepts as space, time, particle, mass, motion, location, velocity, acceleration and their logical interrelations constitute a categorial system. A categorial system in this sense is always logically prior to the laws which state further relations between its elements. The laws state generalized relationships of interdependence between variables in the system. The laws presuppose the definitions of the variables, and they presuppose those relations which are logically implied by the definitions and by the kind of system in question. Insofar as specific laws can be formulated and verified, a categorial system evolves into a theoretical system. Thus a categorial system whose laws relating elements have been formulated is a theoretical system. But it is quite possible to have a categorial system or many parts of one before we have more than a rudimentary knowledge of laws. (124) (Emphasis mine - E.R.G.)

We have already said that a theoretical system is a categorial system whose laws relating elements have been formulated. The classical mechanics is the commonest example of what we mean here by a theoretical system. By logical manipulation of this system it is possible to make detailed predictions about the consequences of specific changes in the values of specific variables; this is because the general laws of the system are known. It should be noted, however, that the classical mechanics does not tell us how empirical systems will actually behave; it tells us rather how they might behave if an ideal set of scientific or "standard" conditions were to exist. Insofar as an empirical system can be subjected to such standard conditions in a laboratory, or insofar as it exists in some "pure" medium, so far is the theoretical system an adequate tool for the prediction of the changes which actually occur in the empirical system. Thus, in certain empirical fields, such as the astronomy of the solar system, the theoretical system of classical mechanics is, to a close approximation,
empirically adequate. But in other fields, such as ballistics, or practical mechanics, the classical system by itself gives only much rougher approximations. This is because of the intervention of such variables as air-resistance and friction. The latter variables, insofar as they have no place in the system itself, bring about "error" in prediction, that is, error in the fit between the theoretical and the empirical systems. (125)
(Emphasis mine - E.R.G.)

This gives us the basis for our definition of empirical-theoretical systems. We speak of an empirical-theoretical system whenever a sufficient number of relevant variables can be brought together in a single (theoretical) system of interdependence adequate for a high level of precision in predicting changes in empirical systems outside special experimental conditions. This is the long-term goal of scientific endeavor. (126)

Several comments suggest themselves. To assert that a theoretical formulation is characterized by four levels of abstraction is, I believe, to confound properties of a theory with distinctive logical sequences of theory formulation which, in my view, do not exist. To be frank, I would say the entire scheme presented here is quite unnecessary. The construction of theory requires first only the formulation of a series of general statements (or hypotheses) on any level of abstraction the formulation may desire, though often precedence and scientific knowledge conspire to dictate moderate levels of abstraction at the outset to render the formulation empirically relevent by some type of immediate empirical test. Perhaps further revision of the general statements or explanation of their implications or redefinition of the concepts would render the theory either more abstract or more comprehensive, much depending on the problems encountered by the theorist. The essential point to note is this. The formation of
scientific theory, whether it be at the highest level of abstraction or at the lowest level, entails, above all, the explicit formulation of general statements indicating that relations of a certain order obtain in the empirical world. Classification and such similar procedures do not assert anything about relations obtaining in the world or do they intrinsically lead to the formulation of empirical relationships. Almost invariably, a system of categories is contingent on an implicit collection of theories the theorist or researcher bears in mind.

The foregoing critical remarks, I think, are equally applicable to the notion of "categorial systems". Insofar as I am able to ascertain, a categorial system is synonymous with an aggregate of high-order concepts which, by their arrangement to suit the subject matter in question, reveal certain "intrinsic relations" amongst themselves. Moreover - and most importantly - this system of categories, in Parsons' view, is logically antecedent to the formulation of laws. Unless a series of general statements are explicitly stated, disclosing how the concepts undergoing examination are imbedded in them, a system of concepts simply does not suggest anything as has been noted on previous occasions. To be sure, relations among concepts are only discernible in the light of theoretical assumptions we make. (127) Thus, on the basis of these considerations, I would say that Parsons' notion of categorial systems is a wholly erroneous one in the arguments and assumptions on scientific theory which it embodies. In this connection, it is interesting to observe the
pre-eminent importance which Parsons still ascribes to concept development in theoretical endeavors. Needless to say, however, this emphasis on concepts and their capacity to point out relations has an inductivist's ring to it.

A few more words concerning Parsons' conceptions of the theoretical system and the empirical-theoretical system may be added. In his discussion of the theoretical system, Parsons makes explicit mention of laws which he views as a significant factor in any theoretical scheme. They assert general relationships among assorted variables that are constituents of the same theoretical scheme. To Parsons, these laws presuppose the meanings of the variables (or concepts). Unfortunately, however, he does not expand in any way on what constitutes the fundamental features of scientific laws, which would indeed tell us much as to how they would be employed. Significantly, moreover, Parsons refers to the process of prediction; however, apart from remarking on the "logical manipulation" of a theoretical system necessary for the prediction to be effected, an elaboration on precisely how this particular scientific process comes about and functions is not furnished.

It may be noted that a dichotomy of a theoretical system and an empirical-theoretical system betokens a grave misconception of the fundamental principles of scientific deliberation. Regarding the former, Parsons indicates that a theoretical system informs us how a number of empirical variables (or as he calls them,
"an empirical system") "behave" if - and this is crucially important to note - ideal or standard conditions prevail. Whereas on the other hand, an empirical system is comprised of variables found in the empirical world which are of relevance to the scope of a particular theoretical system. In view of this feature, it can then be said that an empirical-theoretical system is an amalgam of theoretical statements and empirical variables such that the theoretical statements refer to these variables, and these variables in turn, refer to the theoretical statements, almost a circular process. I should think, however, that this dichotomy is totally false to begin with. A theory is a collection of logically integrated general statements (or laws) expressing general empirical units which purport to furnish explanations of various aspects of the empirical world. Strictly speaking, they do not describe an ideal state of relationships obtaining among empirical variables as Parsons wishes to make out. What a theory attempts to do is connect a vast diversity of empirical events and show that, at base, they are diverse manifestations of underlying empirical uniformities. Clearly, an empirical system, as has been previously noted, is a chimera. The reasons, in my estimation, are as follows. The empirical events to which a theory applies do not form an intrinsic system; they "exist", they "are" in the empirical world. It is we who construct systems for our purposes. As noted before, systems are mental constructions and consequently, do not inhere in empirical phenomena as Parsons seems to think.
An additional comment is in order. By sharply distinguishing a theoretical system and a theory which is directly applied to the empirical world (i.e., an empirical-theoretical system, Parsons, I think, inadvertently speaks of scientific theory both as a metaphysical structure and moreover, as a scientific device applicable to the empirical world. It is plain that a theory only pertains to the empirical world. Thus, a theoretical system without attachment to the empirical world surely cannot be legitimately termed a theory. I can only conclude that Parsons, for all his claims that he understands and formulates scientific theory — and I say this with due diffidence — does not possess a firm grasp of the functions and philosophical argumentation behind genuine scientific theory.

In closing, it is to be noted that Parsons again does not pass any explicit mention on explanation or, for that matter, on prediction. It appears to me that Parsons either considers these notions of marginal importance to his theoretical purposes or he presupposes them all along or — as I believe the case — he does not possess a satisfactory knowledge of scientific theory to be able to comment significantly in this connection. The reasons for the latter comment are predicated on the arguments he has already presented, which I have found notably lacking in many essential respects.
I. "Comment" On "Preface to a Metatheoretical Framework for Sociology" by Llewellyn Gross (1961)

A comparatively brief comment on sociologist, Llewellyn Gross' journal article, "Preface to a Metatheoretical Framework for Sociology" in the American Journal of Sociology (September, 1961) (128) sees Parsons providing another revealing elaboration of his conception of scientific theory with what could be termed a novel element. He prefaces his presentation by explicating his views of the scientific enterprise. Science fundamentally seeks to transfigure the commonsense realm characterized by its incertitudes, vagueness, and contradictory standpoints toward the empirical world. To achieve this objective, there must be empirical knowledge of a general (universal) and organized order. Notwithstanding the occasional contribution of genuinely novel knowledge engendered by experimental efforts, knowledge advances in depth and breadth paramountly under the aegis of a theory which points our attentions and eyes to a specific area of empirical reality to determine whether what we think is supported by the data. As Parsons remarks:

Science if it is to surpass the level of common sense, must constitute a body of empirical knowledge which is systematically ordered and generalized. New knowledge of fact discovered through technical procedures is one, but only one, component of such knowledge. Even here the problems by virtue of which the knowledge of fact has significance are likely to be at least in part
theoretical in a technical sense, and also the relevant statements of fact are likely to be empirical generalizations rather than the most detailed descriptive statements. (129)

Just precisely what Parsons means by the last sentence in the quotation is not at all clear; the locutions "empirical generalizations" and "detailed descriptive statements" are indeed puzzling. Quite apart from this, however, Parsons is in effect arguing that the avenue toward the enlargement of our knowledge of the world is not effectuated to any significant extent by experimentation or other analogous procedures. What is initially required is theory. Without it, we would not know where to turn in the world to examine critically in order to secure any worthwhile knowledge aside from random conjectures which, by definition, are non-theoretical and guarantee little if any knowledge of a definite kind. To be sure, all this is eminently plausible in the light of the criteria of hypothetico-deductive theory.

For Parsons, scientific theory is now constructed of a

logically integrated set of propositions about the relations of variables, that is, abstract conceptual entities, in terms of which many statements of fact can be systematically related to each other, and their meanings for the solution of empirical problems interpreted. (130) (My emphasis - E.R.G.)

In contrast to his previous characterizations of scientific theory, propositions form the chief constituent elements of a theory. By virtue of their being logically interrelated and hence, mutually supportive, the propositions of the theory permit derivations from one aspect to other aspects of the same theoretical
scheme. The logical integration of these propositions effects, as Parsons writes apropos of this "...their mutual support, so that inference from one part of the scheme to other parts becomes possible." (131) Eventually these propositions are subjected to empirical investigation, the consequences of which are evaluated for the empirical tenability of the theory. Thus, for Parsons, empirical adequacy constitutes the cardinal criterion of a theory's value to science. But there are three other criteria which are applied to a theory to ascertain whether or not it is a "good theory". According to Parsons, these are conceptual clarity, conceptual precision, and logical integration, that is to say, the propositions must exhibit a capacity to generate deductions from each other, and moreover, their "logical compatibility".

Some extended comments are in order concerning the tenability of these views on scientific theory. It is indisputable, I think, to characterize a scientific theory as a "logically integrated set of propositions about the relations of variables..." (132) Although the locution "logically integrated" can prove to be rather refractory in attempts at further clarification, the benefit of the doubt can be given to Parsons if it is construed as synonymous with "logically related" or "logically interrelated", as discussed in Chapter Three, the chapter on the nature of scientific theory. However there is one glaring omission in this characterization. Parsons, curiously, does not qualify the type of propositions that a theory contains. What type of
propositions are there? Specific? Concrete? General or abstract? Given the nature of the journal article, which is a brief comment addressing itself to a host of issues, one perhaps could regard this as an understandable oversight. In all probability, Parsons means general (or universal) propositions. However, difficulties arise when sense is attempted to be made of the statement that logically integrated propositions enable derivation from one segment of a theoretical scheme to another segment of the same theoretical scheme. This is susceptible of several interpretations. One is that the general propositions of the theory are inferred from other more abstract propositions. But if this is the case, a general proposition need not necessarily be derived from a combination of two other propositions in the same theoretical scheme for the more abstract propositions could be members of another more abstract theoretical scheme which subsumes the theory containing the general proposition in question; hence, it would be rather inappropriate to assert that a general proposition can be employed to infer other general propositions in the same theoretical scheme. Another tack to be taken for an understanding of Parsons is by posing the following questions: What does Parsons mean by a scheme, a theoretical scheme in this context? Does he mean only the general propositions? If so, then the preceding criticism can be brought to bear on the argument. Or does he mean, by a scheme, the general propositions plus all their specific discrete empirical instances arrived at by deduction? Quite possibly, for, strictly speaking, one could
deduce one aspect of the scheme to another aspect, namely, the specific instances. Clearly, this would be a bizarre interpretation for the number of specific instances that can be derived are invariably enormous, on occasions verging on infinity, a circumstance which would run against the grain of the various objectives of science. Most assuredly, there is an ambiguity here in the use of crucial terms. Since it is of little avail to confront the matter from this angle, let us continue with our queries in another vein. All the previous interpretations and their deficiencies notwithstanding, Parsons could conceivably -- and quite justifiably -- mean that the inference from one part of the theoretical scheme to another is possible in the sense that if one proposition has been subjected to empirical test, the results for that proposition have implications for the other related propositions of the theoretical scheme (or theory to be less confusing). However, this construal takes considerable liberty with Parsons' phrasing of the matter. It would not be amiss to state that Parsons is obscure as to what he means in regard to the characteristic of propositions of being derivable from other propositions and giving rise to further derivations.

Concerning Parsons' criteria of a "good theory", there can be little argument of the importance of empirical adequacy; after all, without it, a theory could not be considered scientific. With respect to his criteria of "conceptual clarity" and "conceptual precision", it would be difficult to separate the two for they invariably mean the same thing, "clarity"; each
implies the other. It could be contended that the criterion of "conceptual clarity" is a consideration of some significance when a theory is in its initial stages of formulation. But, further clarification (or precision) of the concepts employed in the propositions becomes critical when the theory is to be tested. That is the point when the concepts are defined in such a manner they can be consistently identified with observational data. It is fruitless to expend time and effort perfecting elaborate conceptualizations of variables without taking into explicit consideration the observational material by means of which some aspect of the theory will be tested. In this fashion, then, the concepts of the propositions can be altered in accordance with the varying empirical data with which the theory will be confronted for purposes of testing. As C. Schrag aptly suggests in his discussion on the criteria of an adequate scientific theory, the locution, "operational adequacy"—the translation of some of the key terms of the theory in regard to a specific empirical instance would be much more accurate and less confusing to describe these procedures. (133) "Conceptual clarity", on the other hand, bears the faint earmarks of a standpoint which stresses that an armory of explicitly defined concepts logically precedes the formulation of propositions stating certain relationships between these concepts (or variables, as it were).

Regarding the criterion of "logical integration", it is indeed unfortunate and irksome that puzzlement can only be registered. At this point it is worth recapitulating the bare
outlines of this notion, this time by a direct quotation from Parsons:

...logical integration in the sense not only of the logical compatibility of the various propositions included in a theoretical scheme, but of their mutual support, so that inference from one part of the scheme to other parts becomes possible. (134)

What Parsons means by "logical compatibility" and "mutual support", the prime components of "logical integration", is difficult to establish. With respect to the former ("logical compatibility"), it appears somewhat peculiar to speak of propositions being logically compatible unless, of course, he means that the general propositions of the theory contain at least one crucial concept in common such that deduction is permitted - hence, the propositions are interrelated. This, however, is not clearly specified. With respect to the latter notion of "mutual support", it too admits of sundry interpretations as was noted in considerable detail in my earlier critical comments on Parsons' conception of scientific theory in this particular journal article. Parsons has taken a criterion, it seems, and has proceeded only to provide a number of obscurities, debilitating to a large degree in its use in the fruitful evaluation of scientific theories. If, however, Parsons refers to the logical integration of propositions as a condition in which there is one coterminous term (or concept) in each pair of propositions - hence, interrelated - such that deduction of another proposition can be performed with relative ease, then, I am sure that some of the difficulties of
comprehension can be obviated in his account in this regard. It is to be noted, moreover, that the commonly employed "logical" aspects of a scientific theory are not considered by Parsons - and, do not bear any remote resemblance to what Parsons apparently thinks about the criteria of scientific theory. To mention only two, we have the following: One - there is the issue of parsimony which endeavors to sweep away the superfluous definition and uses of concepts, with the minimal presentation of comprehensive propositions which invariably subsume a large number of concrete instances which in turn, were taken into account by a number of less-general propositions on a previous occasion. The second important logical criterion is consistency. By this, it is understood that contradictory sets of propositions (or derivations) can be acquired from the propositions constituting the heart of the theory.

A final consideration of Parsons' views on scientific theory in this journal article concerns his remarks on what could be best termed as his metatheory. However abstract scientific theory may be, it is lodged in a much more abstract constellation of assumptions and concepts on the constitution of empirical reality, and the feasible approaches that may be adopted toward its systematic study. To Parsons, these series of assumptions, etc. comprise a "matrix" or a "frame of reference".

Examples here are the frame of reference of classical mechanics, involving the basic conceptions of particle, mass, Euclidean space and location in it, motion as
change of location over time, etc. In the sociological field the primary frame of reference seems to be that of "action" as some of us have called it, using such concepts as that of "actors" who are motivated and goal-oriented, who act in situations which impose conditions on their action, and who are subject to normative regulation in the sense of "desirable" goals, values, and procedures which are not directly given in the situation itself. (135)

In short, this is what Parsons means by metatheory; it closely coincides with his earlier presentation of a metatheory in 1945.

Parsons' general conception of metatheory is, broadly speaking, without ambiguity and diverges little if any from conventional notions in this respect. Most of the notions Parsons has selected here are, by and large, germane to the metatheoretical frameworks of both the natural and social sciences. Concepts such as space, mass, particle, etc. are basic notions which theorists in the natural sciences presuppose in their theoretical works. Objections, however, may be raised to Parsons' contention that actors are goal-oriented and motivated and operate in normatively defined circumstances. I do not think that these conditions are as obvious as Parsons thinks they are in the social sciences. These assumptions, I should think, belong to a particular theoretical framework in the social sciences which in no wise has secured wide unanimity. In another view, however it may be argued that human action represents a basic presupposition in the social sciences (136) in the sense that it is a "conception of what it is to be human."
On the balance, Parsons' thinking on the considerations of scientific theory in this journal article disclose several marked changes in comparison to his earlier deliberations on this matter. However, it must be admitted that the general structure and pattern of these considerations on scientific theory follow, I believe, fundamentally similar lines which characterized his thinking in his earlier published works containing metatheoretical considerations.

J. Summary Remarks

From the preceding analysis of Parsons' various studies of the logical structure and functions of scientific theory, several generalizations, I think, can be offered.

(1) Parsons emphasizes the crucial importance of theory for the analysis of social phenomena - or for that matter any other phenomena. Theory guides observation and research, and determines which facts are to be considered significant or otherwise.

(2) There is, in the majority of instances, a heavy stress on the necessity of first developing a catalogue of vigorously defined and standardized general concepts and then incorporating them into various sets of general statements which the concepts themselves will somehow suggest. For the lengthiest period, Parsons' conception of scientific theory was characterized by its reference to "logically interrelated concepts".
(3) The objective of scientific theory apparently is "description", "analysis" or "study" of phenomena rather than their explanation or prediction or postdiction. Any explicit mention of explanation is negligible.

(4) Parsons spurns any suggestion of an inductivist approach in which the individual's sole attention is channelled to the accumulation of numerous, discrete facts whilst casting away theory as an unnecessary luxury. This approach, according to Parsons, does not contribute anything to our store of knowledge for the fundamental reason that facts without theory seldom, if ever, disclose anything of scientific significance.
AN EXPOSITION AND CRITIQUE OF TALCOTT
PARSONS' CONCEPTION OF SCIENTIFIC THEORY
AS EVIDENCED IN THE RECENT FORMULATIONS
OF HIS SUBSTANTIVE THEORY

Perhaps one of the most elementary prescriptions that should be studiously observed in the analysis of an individual's work is that one must clearly distinguish what the individual says and advocates from what he in actual fact does. This should not be viewed as too alarming. Virtually any undertaking necessitates a compromise with general principles and guidelines to some extent because, as often as not, there are numerous empirical factors -- and equally so misconceptions of one's enterprise -- that intervene and precipitate alterations of various sorts. Scientific theory counts as one of these endeavors, especially in the infinitively difficult and complex social sciences. And Talcott Parsons is no exception as we shall see. Now that we have, I trust, a general understanding of Parsons' conception of the logical structure and functions of scientific theory, an examination of his substantive theory seems in order. In this chapter, therefore, I shall attempt to reconstruct what I think is Talcott Parsons' substantive theory from the bulk of his work published from 1951 (The Social System and Toward a General Theory of Action).
to the present. I should mention, moreover, that I shall not discuss any of Parsons' many concrete analyses for, a good number of them are merely specific applications of his major theoretical scheme or are discrete, special theories of concrete phenomena employing a number of notions also drawn from the major theoretical scheme. However, prior to turning to the study of Parsons' substantive theory, I should like to outline, for purposes of background, Parsons' initial formulations of the theory of action which appeared in *The Structure of Social Action (1937)*.

A. *The Theory of Action in The Structure of Social Action (1937)*

Parsons' paramount objective in the introductory sections of this volume is the formulation of a theory of action which he names the "voluntaristic" theory of action. The employment of this term suggests that there is an element of freedom of action in social life.

Considering the utilitarian mode of (economic) theory, which stressed the economic, rational, and individualistic aspect of human behavior, to be highly deficient in its explanatory and descriptive capacities, Parsons sought to develop another theoretical scheme faithful in its account of the events occurring in the social world. The behaviors manifested in the social world, according to Parsons, are scarcely ever
thoroughly rational, including economic behavior; as often as not, behaviors are precipitated by countless non-rational elements and the constraining social aspect, a notion very much reminiscent of Durkheim's conception of social fact. Also, important among these non-rationalistic behaviors which must be examined are the subjective factors which point to the manner in which the actor views the world. But to state that rational (the means-end schema) behavior does not take place would be a patent exaggeration; for, it does, but not to the far-reaching extent utilitarian theories maintained. In this connection, it is well to take note of the following strictures which Parsons levels at utilitarian theory:

The utilitarian branch of positivistic thought has, by virtue of the structure of its theoretical system, been focused upon a given range of definite empirical insights and related theoretical problems. The central fact - a fact beyond all question - is that in certain aspects and to certain degrees, under certain conditions, human action is rational. That is, men adapt themselves to the conditions in which they are placed and adapt means to their ends in such a way as to approach the most efficient manner of achieving these ends. And the relations of these means and conditions to the achievement of their ends are "known" to be intrinsically verifiable by the methods of empirical science. (1)

With regard to the non-rational action which is characteristic of much of human behavior, it is important to realize that when the ends of behavior are unverifiable, then, the means that would presumably be employed in the attainment of these ends are likewise unverifiable. As Parsons implicitly suggests in the above quotation, this could not be accounted for by the
rigidly rationalistic utilitarian theories. But if action is considered non-rational, how is it to be understood? Parsons proposes that another theoretical scheme which adopts a markedly different interpretation of the factors of action is assuredly required. It is, therefore, at this juncture that Parsons introduces his own theory of action.

However, before consideration is devoted to the "unit act" as such, it is essential that one have a clear understanding of Parsons' conception of "unit" in his general theoretical discourse at this phase of his career.

For Parsons, the kernel of his theory is the notion of unit act. It is an abstraction:

...the entity which constitutes the common reference of a combination of statements of fact made within a frame of reference in such a way that the combination may for purposes of the theoretical system in question, be considered an adequate description of an entity which, within the frame of reference, conceivably exists independently. The theoretical unit is the specific combination of logical universals in specific logical relations to each other into which these statements of fact are fitted. (2)

Thus, with respect to Parsons' theory of action the unit act is viewed in the light of an aggregate of properties such that if they are omitted from consideration, a "unit" cannot be said to exist. Parsons writes of these essential properties in the following manner:

In this sense then, an "act" involves logically the following: (1) It implies an agent, an "actor" (2) For purposes of definition the act must have an "end," a future
state of affairs toward which the process of action is oriented, the end. This situation is in turn analyzable into two elements: those over which the actor has no control, that is which he cannot alter, or prevent from being altered, in conformity with his end, and those over which he has such control. The former may be termed the "conditions" of action, the latter the "means." Finally there is inherent in the conception of this unit, in its analytical uses, a certain mode of relationship between these elements. That is, in the choice of alternative means to the end, in so far as the situation allows alternatives, there is a "normative orientation" of action. Within the area of control of the actor, the means employed cannot, in general, be conceived either as chosen at random or as dependent exclusively on the conditions of action, but must in some sense be subject to the influence of an independent, determinate selective factor, a knowledge of which is necessary to the understanding of the concrete course of action. (3)

As noted earlier, in Parsons' view, one of the indispensable requisites of action is a system of norms or an ultimate value system. Without it the selection of alternative means factors in the realization of certain ends would be quite literally impossible. And the voluntaristic postulate as a consequence would pale into insignificance.

It may be noted, moreover, that a voluntaristic theory of action, in Parsons' terms, should employ phenomena such as goals, norms, ideas as variables in the strictly scientific sense instead of epiphenomena which are interesting but scientifically irrelevant.

In Parsons' view, his action scheme is also characterized by its eminently subjective yet scientific aspect.

\[ \text{the frame of reference of the schema is subjective in a particular sense. That is, it deals with phenomena with things as they appear from the point of view of the actor whose action is being analyzed and considered.} \] (4)
It is decisive to note that Parsons regards the systems of norms (or ultimate value systems), the fundamental components of action, as emergent properties of social organisms. They become *sui generis* systems in their own right. Emergence refers to a system that has "emerged" or "come out" of its constituent elements and possesses properties which are derivable from or explainable by the interplay of these constituent elements. Though the emergent system may seem to be a totally independent entity, as it were, it does not at any time become completely severed from its constituent elements. Parsons observes that emergence:

... has a strictly empirical meaning, designating general properties of complex systems of phenomena which are, in their particular values, empirically identifiable and which can be shown by comparative analysis to vary, in these particular values, independently of others. (5)

It may also be remarked that an emergent system can be used as a variable in causal statements or empirical uniformities. Thus, in the light of these considerations, it becomes understandable why, for Parsons, a system of norms wields an empirically verifiable influence on social conduct; in a word, action.

Just precisely what were Parsons' rationales for formulating a theory of this nature are never spelled out. (6) Although I do not wish to enter upon a lengthy critical comment, I will say this. Much of what he has written here seems dreadfully bereft of any structure resembling a genuine scientific theory though he continually speaks of it as such
and, furthermore, he still appears to maintain his contradictory theoretical objectives and persists in committing egregious conceptual errors which he presented in his 1935 journal article, "The Place of Ultimate Values in Sociological Theory." (7)

Suffice it to say, Parsons has abandoned to some degree various aspects of this version of the theory of action though, on the other hand, he has retained a considerable measure of it and has refurbished and reformulated these remnants such that presently appear in his recent substantive formulations. (8)

B. The Theory of Action: Recent Formulations

1) General Comments

In his relatively recent theoretical works, from the publication of The Social System (1951) and Toward a General Theory of Action (1951) to the present, Parsons has constructed a prodigious theoretical system. It is expressly designed to subsume the concerns of related social sciences into what can best be called an unified, goal-directed action frame of reference. Very generally speaking — and rather loosely — the nub of his theory is as follows. The roles of the action framework of reference are structurally organized by the institutionalized and cultural patterns of a particular social system; this unmistakably entails the structural, stable aspect of Parsons' theory which facilitates the identification of the manifold roles and institutions operative in a given social system. There is, moreover, a dynamic aspect of the social
system (the causal relations obtaining between the structural components) which is analyzed in the light of the various functional prerequisites of the social system; for purposes of survival and self-maintenance, the structure of the social system must fulfill these prerequisites to a sufficient extent.

In broad outline, Parsons' theory takes into consideration the valuational and normative factor involved in social behavior. To be sure, N.S. Timasheff is correct when he briefly notes that, for Parsons, "Society is thus essentially a "moral order" in the sense that it is rooted in morally sanctioned terms." (9)

2) The Factors of System and Structure in Parsons' Substantive Theory

It is of the first importance to recognize in any consideration of Parsons' substantive work that the notions of system and structure are paramount factors. They are undoubtedly the central concepts of his thought, imparting, in their own curious way, scope and depth to Parsons' theoretical formulations. It is largely because of these notions that the structural-functional theoretical approach, Parsons so readily advocates, derives its life so to speak.

In any discussion on Parsons' theory, particularly on his use of the notion of "system", it is indeed imperative that a sharp distinction between the conceptions of "theoretical system" and "empirical system" must be effected, else serious confusion in understanding Parsons' substantive formulations
may arise. By a "theoretical system", Parsons refers to the structural features of the theoretical formulation which he thinks should characterize any theoretical work. Broadly speaking, the notion of "theoretical system" casts an emphasis on the coherence and utility of the argument that is put forth.

Several questions may arise about whether the substance of this monograph constitutes a "system" in the theoretical sense. In one sense every carefully defined and logically integrated conceptual scheme constitutes a "system," and in this sense scientific theory of any kind consists of systems. Beyond this, however, there are three questions relevant to the "systematic" nature of a theoretical work. The first has to do with the generality and complexity of the scheme. The second is concerned with the degree to which it may claim "closure"; here the problem is whether the implications of its assertions in some parts are systematically supported or contradicted by assertions in other parts. The third is concerned with the level of systematization; that is, with how far the theory is advanced toward the ultimate goals of science. (10)

As the bulk of the subsequent remarks will principally, nay almost exclusively, center on the conception of "empirical system" in Parsons' substantive work, and to avoid needless repetition, I shall not comment on it at this point.

It is worthwhile to note at the outset that Parsons began the development of a structural-functional theory in The Social System in the light of Vilfredo Pareto's conception of the "system". (11) The social system, for Pareto was conceptualized as a constellation of dynamically interrelated and
equilibrating variables which operate in a similar manner as, let us say, the cells in an organic body. It is to be clearly noted that an equilibrium is to be construed as a continually on-going phenomenon. Whether or not the equilibrium of a particular system seems _prima facie_ stable or static, there are countless changes taking place in the system itself. The utilization of a "system" concept in a scientific analysis evinces several highly fruitful methods. The consequences of the multiple interrelations of variables in each other can be reasonably traced out and the causative factors which disrupt the equilibrium can be ascertained and observed. Parsons puts the advantages to be accrued from employing such a notion in a clarifying perspective.

For such an analysis we have certain resources which we can utilize. First, we can describe the initial state of the system, into which the process of change enters, in precise and technical terms, which among other things can clearly reveal whether the empirical evidence is adequate. Secondly, in the same terms, those describing the structure of a social system, we can specify what has changed into what and through what intermediate stages. If the process has only begun we can specify its direction relative to the various parts of the system.

Third, we can invoke our knowledge of the two classes of structural imperatives of social systems, the general ones and those peculiar to the specific type of system. In these terms we can ask whether the change tends to violate any of these imperatives, to jeopardize the motivational needs of important groups in the population, to weaken the controls over important parts of the power system, to upset the balance in the reward system in specific ways, or to introduce a structure which is incompatible in certain respects with other concrete structures in the system. (12)

Discerning the encouraging theoretical prospects of Pareto's conception of "social system", Parsons construed and subsequently modified it according to his own theoretical purposes. Understood
in its empirical sense, the notion of system pertains to a set of empirical conditions such

... that determinate relations of interdependence exist within the complex of empirical phenomena. The antithesis of the concept of system is random variability. However, no implication of rigidity is intended. (13)

With our discussion of interaction we have entered upon the analysis of systems. Before we discuss more fully personality and social systems, it is desirable to state explicitly the principal properties of empirical systems which are relevant for the present analysis. The most general and fundamental property of a system is the interdependence of parts or variables. Interdependence consists in the existence of determinate relationships among the parts or variables as contrasted with randomness of variability. In other words, interdependence is order in the relationship among the components which enter into a system. This order must have a tendency to self-maintenance, which is very generally expressed in the concept of equilibrium. It need not, however, be a static self-maintenance or a stable equilibrium. It may be an ordered process of change—a process following a determinate pattern rather than random variability relative to the starting point. This is called a moving equilibrium and is well exemplified by growth. Furthermore, equilibrium even when stable, by no means implies that process is not going on; process is continual even in stable systems, the stabilities residing in the interrelations involved in the process. (14)

Expressed in other words, an empirical system is primarily constituted of a constellation of interdependent concepts (or variables) which invariably define and are defined by their loci within that entire constellation. But it should be observed that a system can be viewed as an independent structure because, for various reasons which are not clearly spelled out by Parsons, they are not amenable to being reduced to another system, either of a lower or higher degree of abstraction. Despite this
property of independence, a system can without difficulty articulate with other systems. To illustrate this character of a system, let us take Parsons' concept of personality which is generally regarded as a system. It is considered as such for the fundamental reason that it is an independent structure comprised of a multiplicity of interdependent psychological and physiological elements; the personality is not constituted by one or two of these psychological or physiological elements, or is it an amalgam of all these elements interacting with each other. A personality system, though dependent on these various non-social elements for its genesis and continued functioning, is an emergent, an entity in its own independent right. (15)

Doubtless, for Parsons, one of the most significant features of a system is its ability to become a self-maintaining structure. In other words, it tends toward an equilibrium or a boundary maintaining condition. Parsons writes:

... A special additional property, however, is of primary significance for the theory of action. This is the tendency to maintain equilibrium, in the most general sense stated above, within certain boundaries relative to an environment—boundaries which are not imposed from outside but which are self-maintained by the properties of the constituent variables as they operate within the system. The most familiar example is the living organism, which is a physicochemical system that is not assimilated to the physicochemical conditions of the environment, but maintains certain distinct properties in relation to the environment. For example, the maintenance of the constant body temperature of the mammal necessitates processes which mediate the interdependence between the internal and the external systems in respect to temperature; these processes maintain constancy over a wide range of variability in environmental temperatures. (16)
For this type of condition to obtain, two unquestionably important processes must come about. They are allocation and integration. On the other hand, when allocation is in operation, there are a host of processes governing the apportionment in the system of its constituent parts consonant with keeping the state of equilibrium intact. On the other hand, the processes of integration maintain the system's relationships with the environmental factors which, in spite of the flux and drift of the conditions external to the system preserve and maintain the system's characteristic internal properties and boundaries.

It is important to note, moreover, that a system is a unified structure in relation to its environmental context. Thus, the self-maintenance of a given system does not concern itself principally with boundary maintenance; there is, in addition — and most importantly — the maintenance of the manifold interrelationships among the system's elements taking place within the boundary. As well as regulating the environmental influences, the self-maintenance of a system entails the suggestion that there is a control acting upon tendencies to transform or to change the distinctive state of affairs of the system which, somehow, find their provenance within that system. In both the social and natural worlds, there are an infinity of systems of this general character in operation.

It is essential to note that a system is never a closed structure. This means that a system cannot be an entity unto itself typified by rather attenuated connections with other
systematic entities; there are usually regions of contact with the boundaries of other systems such that any one system can be regarded as an autonomous entity. Should, however, the boundaries of a recognized system dissolve on account of some malfunction which may have been either internally produced or externally induced, the system in question is considered to be extinguished, in a word, dead. Several possible developments can occur in such situations. To begin with, the system which has collapsed can, change and thereby, restore its equilibrium by reintegrating itself. Another is that the original function of the system will be assimilated by another system. Or, as the case may be, the function of the system in a specific environmental context may not at all be required and disappear with the dissolution of the system in question. These considerations indicate that systems function in an hierarchical scale, generally the higher order systems exerting a predominating influence on the functions of the lower order systems; though their general influence is somewhat tenuous in comparison, the lower order systems (which may also include structures commonly known as subsystems viz. religion, economic, political family, etc.) do exert some force on the higher order systems. A concrete illustration at this point will, I trust, bring to light some of the notions that have just been discussed. Let us suppose that, broadly speaking, the American economic system as a whole establishes approximate limits on the quantity of automobiles the pertinent industrial subsystem must manufacture. Should,
for one reason or another, the automobile manufacturing industry fail to attain these previously set limits, then either the supply of the industry or the demand of the American economic structure must change such that an equilibration of the respective systems involved can be engendered without further possible deleterious effects on both. If neither can change, then perhaps the manufacture of bicycles or horse carriages or some other functional substitute can almost invariably equilibrate the respective systems involved. (18)

3) The Action Frame of Reference (19)

Parsons' major theoretical preoccupation at present, as it was in The Structure of Social Action (1937) with, of course, the relevant alterations and emphasis taken into account, is social action.

The "action" frame of reference embodies essentially three elements: the actor, a situation, and the orientation of the actor to the situation. An actor, considered either in the singular or plural sense, is oriented within a situation in which he is motivated to attain a goal or end. As regards the situation, it is characterized by two dominant classes of objects which the actor is oriented to; they are — (a) social objects which include the self and other individuals, and (b) non-social objects entailing physical objects, cultural objects and symbols. The shank of Parsons' theory in this connection is the actor's orientation to the object of his need in the situation. The orientations are of two distinguish-
able types, viz., motivational orientations and value orientations. With respect to the former, the motivational component, it is to be noted that it furnishes the requisite energy which is to be exerted by the actor in the execution of various behaviors to secure his goal. The central focus here is directed on the "needs" of the actor. In short compass, the motivational orientation is constituted of three distinguishable elements: (1) the cognitive aspect, which pertains to the actor's perception of the objects, one of which he will select in terms of his needs in an action situation; (2) the cathectic aspect, doubtless a Freudian notion meaning the discrimination of an object's negative or positive affective values in the satisfaction of the actor(s) needs; and (3) the evaluative aspect, which integrates the information significant to the actor and allots the actor's energy to the objects and interests from which he will choose. As for the value orientations, they explicitly refer to the social norms which obtain in a given situation irrespective of the needs of the actor, which are largely attended to by the motivational orientations. As Parsons points out in this regard:

The classification of the modes of motivational orientation provides essentially a framework for analyzing the "problems" in which the actor has an "interest". Value-orientation, on the other hand, provides the standards of what constitute satisfactory "solutions" of these problems." (20)

In any event, as with the motivational factor, the value orientation is also further distinguished by three modes: (1) the cognitive aspect, which refers to the standards of validity
of cognitive judgments (i.e., logic, correct procedures for making observational statements, the canons of scientific work, etc.); (2) the appreciative aspect which points to the standards on the evaluation of alternatives in choices of cathetic import where there is an element of sacrifice; and (3) the moral aspect, which pertains to "the most comprehensive integrative standards for assessing and regulating the entire system of action under consideration, whether it be a personality or a society or a subsystem of either. They are the "court of last appeal" in any large-scale integrative problem within the system." (21) (Emphasis in original). Quite in general, then, both the motivational and valuational orientations are ways an actor sorts, selects, differentiates and tests ideas, preferences, norms and values in relation to his needs and the social context in which he moves. But it should be clearly borne in mind that these two orientations, "the basic aspects of the action system" are logically independent" ... in the sense that the content under the two classifications may be independently variable." (22) Above all, they are categories to be employed in the description of the orientation of action.

4) The Personality, Social and Cultural Systems

With this general scheme as a fundamental conceptual framework, Parsons proceeds to develop three analytical systems, each of differing levels of abstraction; they are, in short, the personality system, the social system, and the cultural system. Although each system differs in content and degree of
abstraction from the other, they are, according to Parsons, interrelated. Taking each system in the briefest manner possible, we have the following arrangement. The personality system is comprised of a single actor along with a fairly organized system of orientation and motivation and a complex of hierarchically integrated need-dispositions. Though Parsons' conception of the social system fluctuates in his writings, it would be accurate to say that it consists of a dimension of the interactive patterns performed by a plurality of individual actors. Concerning the cultural systems, it is important to realize that it is not a system in the same vein as the personality and the social (society) factors are. It is to be conceived...

...both as an object of orientation and as an element in the orientation of action must be articulated both conceptually and empirically with personalities and social systems. Apart from embodiment in the orientation system of concrete actors, culture, though existing as a body of artifacts and as systems of symbols, is not in itself organized as a system of action. Therefore, culture as a system is on a different plane from personalities and social systems. (24)

As such a culture can be divided into three main classes of patterns; using the three modes of the motivational orientations, we have the following arrangements:

(1) Systems of ideas or beliefs marked by the pre-eminence of cognitive interests;

(2) Systems of expressive symbols, which include art forms and styles and are noted for their heavy stress on the cathetic element.

(3) Systems of value orientations. "Here the primary interest is in the evaluation of alternatives from the viewpoint
of their consequences or implications for a system of action or one of its subsystems." (25)

It may be remarked for proper perspective that Parsons' major interest in the cultural system, is in the manner it bears upon the personality and social systems rather than any intrinsic theoretical concern in this analytical category as such.

Thus, for an adequate description of a system of action, it would seem with the foregoing discussion in view, that the following notions would be required. At the minimum, two people would be necessary along with the motivational and value (cultural) orientations. And these value orientations would have to be internalized by the participating actors, otherwise they could not, in the strict sense of the term, interact.

It is significant to note that the content of these various categories of orientation are learned and assimilated through the socialization process. (26) Atop the foundation of the basic physiological functions and the burgeoning social-relational needs -- particularly based on the infant's dependency on the mother -- and, as well, through the learning process, the child secures the value orientations of his society. To be sure, the cultural system (if I may use this term) furnishes the symbolic system and pattern of values and norms which the "members of the society of a specific culture" internalize and hold in common. It then seems reasonable to assert that social interaction is normatively controlled as a consequence of the intimate interaction of the cultural element with the personality
system. The system of cultural values (value orientations) constitute the linkage between the personality and social systems (society) and the cultural factor.

Attention should now be directed to the concept of role which occupies a position of central importance in Parsons' elaboration of the social system. Essentially, the role performs a similar connecting operation for the personality and social systems as the cultural values do for the personality and social systems in relation to the cultural system. At bottom, a role

"is that organized sector of an actor's orientation which constitutes and defines his participation in an interactive process. It involves a set of complementary expectations concerning his own actions and those of others with whom he interacts possess these expectations. (27)

In further elaboration, the role must be regarded as the processual aspect (or a dynamic aspect) of an actor's participation in patterned interactive relationships; in a word, this means what the actor does to other actors in his interactions with them. To complement this notion, then, it is essential that the structural aspect (the positional aspect) be explicitly considered; this refers to the actor's "location" (or status) in the social system in relation to other social actors. Thus, on many occasions, one may encounter the hyphenated locution status-roles in Parsons' substantive works. It should be noted that these terms, for purposes of analysis, may be distinguished and employed separately, though they cannot be severed for they would then lose their meanings.
It should be made quite clear that statuses and roles, or the status-role bundle, are not in general attributes of the actor, but are units of the social system, though having a given status may sometimes be treated as an attribute. But the status-role is analogous to the particle of mechanics, not to mass or velocity. (28) (Emphasis in original)

From all this, it becomes evident that the role can be distinguished from the personality who performs it. An abstraction of the role from the person can be effected such that one can analyze the social system in which the role is an integral component or the personality system in the divers roles of different social systems which it (the personality) occupies.

The undeniable significance of the concept of role for Parsons' conception of the social system is furthermore emphasized by noting that the structures of any social system or subsystem are comprised of the positions that the societal members maintain. Take, as an example, the factor of social stratification. Analytically situated on the level of the social system, it is constituted of a variety of positions (status-role designations) hierarchically arranged according to the norms, values, and goals generally regarded to be of substantial importance. More than that, the rewards connected with each position (or status-role, to be more precise) are largely determined by a scale of values the particular society apportions to each "unit" or role the function of which is to realize one of the society's many goals.

When the ego (the person) holds a particular status (position) in a social system which is taken into account both
by ego and his alters (the others with whom ego interacts) his role (the dynamic aspect of status) is recognized as being institutionalized which means, in short, that the norms conducting roles of various types are in large part established. In a more technical sense, institutionalization pertains to:

... the integration of the expectations of actors in a relevant interactive system of roles with a shared normative pattern of values. The integration is such that each is pre-disposed to reward the conformity of others with the value pattern and conversely to disapprove and punish deviance. Institutionalization is a matter of degree not of absolute absence or presence. (29)

It should further be noted in this connection that institutionalization, in Parsons' view, is a crucially important factor to be noted in the analysis of any social system (or society). The ostensible reason is that institutions represent the essential prerequisites for the stability of various smaller-scale social structures which can be identified and segregated out from the general social system in which they are implicated.

Not surprisingly, Parsons considers the focal concern of the discipline of sociology, and, by implication, sociological theory - to be the study of institutions.

Sociological theory, then, is for us that aspect of social systems which is concerned with the phenomena of the institutionalization of patterns of value-orientation in the social system, with the conditions of that institutionalization, and of changes in the patterns, with conditions of conformity with and deviance from a set of such patterns and with motivational processes in so far as they are involved in all of these. (30)

There is, among an infinity of other things, a distinctive sociologistic tinge in Parsons' theoretical scheme, as the drift and thrust of discussions clearly shows. On many occasions, he
is concerned with the non-personal aspect of social behavior, notably the meaning of social behavior as determined by the institutional-structural context. (31)

5) The Pattern Variables and Functional Prerequisites

Underlying the three fundamental systems of action (the personality, social, and cultural systems) are what Parsons calls the pattern variables (five pairs in number) and the imperatives or problems of the systems, the functional prerequisites (four in number).

Generally speaking, the pattern variables indicate the dichotomies in the alternative choices of the value orientations enjoined by the culture which every personality and social system must bring about in their orientations to an object of interest prior to undertaking any meaningful action. As Parsons (with Edward Shils) writes:

... a pattern variable is a dichotomy, one side of which must be chosen by an actor before the meaning of a situation is determinate for him, and thus before he can act with respect to that situation. We maintain that there are only five basic pattern variables (i.e. pattern variables deriving directly from the frame of reference of the theory of action) and that, in the sense that they are all of the pattern variables which so derive, they constitute a system.... They are:

1. Affectivity - Affective neutrality.
2. Self-orientation - Collectivity-orientation.
3. Universalism - Particularism.
4. Ascription - Achievement.
5. Specificity - Diffuseness. (32)

As such, the pattern variables are a "derived" classificatory system of the dilemmas or alternatives an actor or a social system encounters in the selection of norms, role patterns,
values, and, as the case may be, personal choices.

As for the four functional prerequisites, they may be viewed as the four functional problems that all systems (personality, social and cultural) must satisfactorily overcome if they each of them are to survive. According to Parsons, it is within this general context - the "interaction" between the pattern variables and the four functional requisites - that the condition of equilibrium comes to prominence and, in consequence, the notion of "function" acquires its teleological flavor in the sense of being system-determined and system-sustaining. In brief, the functional prerequisites (33) are as follows:

(1) Pattern maintenance and tension management.
(2) Adaptation.
(3) Goal attainment.
(4) Integration.

It would indeed be profitable at this point to enlarge further upon these critically important elements of Parsons' theory in social action.

(a) The Pattern Variables (34)

If the pattern variables are arranged in all their diverse combinations and sub-combinations, they would, in Parsons' view, exhaust all conceivable situational conditions an actor can be confronted with. Pattern variables, moreover, represent a formula by means of which the codification of the various structures of any society can be effected without too much problem. They are apposite conceptual devices for both cross-
cultural and intra-cultural comparative studies as they are exhaustive and, most importantly, universal.

Basically, the pattern variables make for an adequate description of the manner in which an actor comes to terms with and canalizes his motivational needs and various actions such that the definition of orientational patterns in a welter of role-expectations can be effected and thus eschew behavioral anarchy.

The pattern variables articulate with the action frame of reference at various significant points. Take the following statements of Parsons into consideration.

These pattern variables enter the action frame of reference at four different levels. In the first place, they enter at the concrete level as five discrete choices (explicit or implicit) which every actor makes before he can act. In the second place, they enter on the personality level as habits of choice; the person has a set of habits of choosing, ordinarily or relative to certain types of situations, one horn or the other of each of these dilemmas. Since this set of habits is usually a bit of internalized culture, we will list it as a component of the actor's value-orientation standards. In the third place, the pattern variables enter on the collectivity level as aspects of role definition: the definitions of rights and duties of the members of a collectivity which specify the actions of incumbents of roles, and which often specify that the performer shall exhibit a habit of choosing one side or the other of each of these dilemmas. In the fourth place, the variables enter on the cultural level as aspects of value standards; this is because most value standards are rules or recipes for concrete action and thus specify, among other things, that the actor abiding by the standard shall exhibit a habit of choosing one horn or the other of each of the dilemmas. (35)

Apart from their curious linkage with the concrete factors in the action frame of reference, the pattern variables constitute
fruitful descriptive categories of value and normative standards found on the levels of the personality, social and cultural systems and, as well, the divergences from and conformities with actual instances of action and the patterns of normative expectations.

It is worthwhile to notice in passing that the alternatives offered by the pattern variable scheme are logically independent of each other. Thus, when the pattern variables were originally presented in The Social System, Parsons examined sixteen combinations, whilst arguing that there were thirty-two possible combinations. (36) Very recent analyses of the pattern variable scheme in explicit relation to the action system in Parsons' theory have disclosed that there are from six hundred to one thousand possible categories of social acts. (37)

An illustration taken from one of Parsons' discussions will indicate the manner in which the pattern variables enhance our comprehension of social phenomena. Alluding to the American occupational system, Parsons states, ""The American occupational system is universalistic and achievement-oriented and specific."" (38) Phrased less elaborately, this statement indicates that the norms governing employment practices in the U.S.A. treat those occupying certain roles (i.e. employees) in universal terms (that is, without differentiation based on inherent traits such as skin color, language, etc.) and also in specific terms, particularly in regard to the evaluation of a certain aspect of an individual's performance; and regarding
the achievement aspect, the actor must display a proficiency of performance.

It should be clear that it is the cultural system which supplies the values. But ironically, these values engender the selection dilemmas for the social and personality systems.

In the subsequent explication of the five dichotomous pairs of pattern variables, I shall concentrate my references and remarks on the level of the social system.

(i) *Affectivity vs. Affective Neutrality*

Affectivity refers to the role-expectation which gives freedom to the role-incumbent to give expression of any feelings or desires he may have at a given period. Affective-neutrality requires the role-incumbent to keep in check any expressive tendencies (for example, hate, sexuality, joy, etc.) in order to eschew negative consequences that may be incurred from others or even oneself (one's "conscience").

(ii) *Self-Orientation vs. Collectivity*

A role incumbent, in self-orientation, is allowed to give precedence to his own interests without giving explicit consideration to the group to which he may belong. Collectivity is the subordination of self-interest to group-interests.

(iii) *Universalism vs. Particularism*

Universalism calls for the role-incumbent to treat certain objects of universal applicability without differentiation (e.g.
evincing respect for the British monarch. Particularism requires the actor to distinguish between one object from another and to conduct himself accordingly.

(iv) Ascription vs. Achievement

Ascription (or ascriptive modality pertains to an object which has certain fixed attributes (universalistic or particularistic) to which the actor is compelled to orient himself. (Note: the British monarch has an ascribed status-role.) Achievement refers to the performance of the object to which the actor must orient himself.

(v) Specificity vs. Diffuseness

Specificity points to the role-expectation that a role-incumbent will give specific significance or obligation to the object. (This is best exemplified by the obligations displayed in a doctor-patient relationship.) Diffuseness means that a role-incumbent will give complete significance and obligation to the object in question. The obligations in the typical parent-child or husband-wife relationship bring this notion into bold relief.)

An additional word or two may be indicated concerning the properties of the pattern variables in Parsons' theoretical scheme. Virtually all of them can be subsumed under more comprehensive categories. (39) Two sets of variables - notably, universalism - particularism and ascription - achievement - are viewed as dilemmas which the actors must come to grips
with in the choice of how the objects themselves will be arranged relative to each other and the motivational facets of the actor. In the second set of variables — namely, specificity — diffuseness and affectivity — affective neutrality —, the actor is required to make a decision as to how his attitudes toward the objects in question will be arranged. With regard to the remaining set of variables (self-orientation — collectivity-orientation), Parsons and Bales contend that it is not subsumable under the object or attitude categories as mentioned above. Their reasons are as follows:

This is because it is concerned with problems internal to the system of interaction rather than with problems internal to each act considered in isolation. It concerns whether the individual actor's orientation in some particular area of activity should be directly constitutive of his solidarity with others in a collectivity, or whether it may remain or become independent of this within certain limits. For the most general purposes of the analysis of systems of action, then, this fifth pattern variable may be neglected. (40)

(b) The Functional Prerequisites (41)

With the introduction of the functional prerequisites, we perceive the nub of Parsons' functional theory in its bold ramifications. Let us briefly examine how this development comes about.

It will be seen that, attention has been called to the notion of system, on the first dimension of Parsons' theory, that is the systems of the personality, social, and cultural. The strategically significant system of these three for Parsons,
is the comprehensive social system or society with its subsystems - structures composed of a patterned cluster of roles such as institutions (family, political, religion, and economic) and the sub-subsystems of the subsystems (usually reinforcing systems of the subsystem - the automobile manufacturing industry is an integrant of the all-inclusive economic system). In regard to the second dimension of Parsons' theory, the "action" framework in terms of the pattern variables was mortized with the "systems" framework, resulting in a framework termed the structural system of action. To Parsons, it seems that these two dimensions as they stand, are not satisfactory for fruitful empirical research. (42) What is required is the functional element which will connect the theory to the empirical world. As a consequence, the third dimension of Parsons' theory is formed - the functional aspect is conjoined with, the structured system of action. What develops is a theory termed structural-functionalism. Essentially, an institution, whichever it may be, is a structure which has certain problems to overcome, or duties to carry out, such that it can survive as a system, in other words, an institution has functions to perform for purposes of survival.

Broadly speaking, functional prerequisites are indicators which enable one to note as to what extent any system has functioned or is functioning, both in its internal and external contexts, in realizing its needs for survival.
It should be clear that any system, in the Parsonian theoretical framework, must meet certain needs to ensure survival. Although in specific terms, needs vary and differ from one system to another, they must, on a general level, fulfill these four functional requisites.

(i) **Pattern Maintenance and Tension Management**

When the patterns of a system, that is, their intricate structures, have been firmly established, it is important that if it is to exist, they must be maintained by its various sub-systems and role-incumbents. This is especially applicable to a social system. Thus, in this regard, the socialization process is a mechanism by means of which the members of a social system incorporate and develop a regard for the many patterns of the social system. Likewise, rituals, maxims, dogmas, creeds, etc. are mechanisms, serving as aids to the memory by which certain modes of conduct are prescribed and encouraged, and certain pursuits (e.g. intellectual work and practical scientific research) are reinforced.

In a similar vein, mechanisms for tension management are necessary to the society members such that the system can function efficaciously and avoid collapse. Illustrations in this regard abound; firefighters, police, hospitals, play, prisons, agencies of diverse kinds, etc.; these all function to release tensions and emotional energies, confine those who would imperil the system, and restrain any socially untoward impulses of the actors.
(ii) Adaptation

A system, to remain in existence, is required to adapt to the surrounding social and non-social environments. Whether it be a small system such as, say, a family or a society, mechanisms must be acquired by means of which the system can to as many degrees as possible, control and adjust itself to any new situation. Thus, as an example, for purposes of meeting the economic demands of the members, a society must institute a division of labor, or, as in the case of small systems like a family or group; role differentiation is imperative.

(iii) Goal Attainment

According to Parsons, every system has certain goals to attain, if it is to continue functioning as a system. If the goals of a system are to be reached, it is, then, mandatory that the system is required to contrive reasonably effective methods such that the members of the society are allocated to their appropriate positions in that particular system. It may be remarked here that the allocative factor is germane to considerations on the process of adaptation as well. At base, the difference is subtle. Let us consider the following two examples. If an employer reduces the amount of the employee's monthly salary from $300.00 to $250.00 because he wishes to increase his profits (the employer's goal), the employee, as a result must adapt to his new salary. Similarly, in the social system, the political system (a subsystem) may
establish a certain goal for the expenditure on housing; the economic system comprised of industrial concerns and labor (another subsystem) now is impelled to adapt itself in attending to the government's housing demands through the allocation of the requisite resources such as equipment, materials, and labor.

(iv) Integration

When the members of a system co-operate in a vast dimension of activities, that system is said to be integrated or, as Durkheim would say, it exhibits solidarity. These activities may take the form of events taking place in small systems such as the members of a university chess club actively seeking funds from their fellow students and the university's board of governors to, cite another case, the members of a nation (a large social system) voicing their displeasure at a political figure from another country who has made disparaging remarks about that country. For integration (or solidarity) to obtain within a system and between a number of disparate systems, a constellation of regulative norms must be in operation. In addition to referring to the legal system and its manifold laws of a generally non-affective nature it is to be observed that regulative norms may be characterized by an affective element; thus, the detestation or the love of a certain type of person may be enjoined by a set of norms. Nevertheless, regulative norms should in large measure be flexible in order that in changing conditions the re-organization of the relevant
norms and system structures can be engendered in a comparatively spontaneous fashion by the members of the various systems. To be sure, a self-interest or, as Parsons puts it, an "institutionalized individualism" (43) can flourish if the flexibility of the regulative norms is sufficiently wide, even in the framework of a fairly integrated system.

Interestingly, each of the four functional pre-requisites has a corresponding subsystem of the social system (or society). According to Parsons, the economy can be viewed as an adaptive functional subsystem of society. Roughly speaking, an economy represents the production of materials, food and services for families, business organizations, governments, and various groups. The polity - the political aspect of the social system - attends to the goal attainment factor. A number of institutions are primarily concerned with the pattern maintenance and tension management problems of society; they would be, to name but a few, the medical profession, police, religion, education and, above all, the family. With regard to the integrative problem, virtually all subsystems in some fashion are instrumental. In passing, it may be remarked that each subsystem as well can be regarded as a separate system with its own functional imperatives. (44)

6) The Concept of Equilibrium and its Theoretical Importance

The concept of equilibrium for Parsons is, I think, an inevitable consequence of his preoccupation with the notion
of system. It is primarily a biological concept very much akin to the notion of homeostasis. The introduction of this notion into sociological discourse in any significant theoretical sense was made by Pareto along with his conception of "social system" which, as was earlier noted, Parsons readily adopted. It is sufficient to state for the moment that the conception of equilibrium looms large in Parsons' theoretical system, drawing many sympathetic followers on account of its ability to render analysis of social phenomena more orderly and, in equal measure, dissenters who argue that it renders static the analysis and description of social life which is not at all the case.

To begin with, a system can be viewed as an analytically closed entity. It is boundary maintaining and in an equilibrium condition in which the components are interdependent and its essential nature remains intact. From one point of view, this makes for a static conception of system. But it matters little if the notion is employed for analytical reasons. However, if the notion of system is employed on the empirical level, several significant considerations come into view. For, here, a system is a dynamic, on-going process, or an on-going concern. Viewed in this light a system can be considered to be a moving equilibrium which, is vividly illustrative, by, let us say, the growth of an organism (which is a system). A system in equilibrium, it is to be noted, imposes an
intrinsically generated restraint or limitation on the way the constituent variables or elements should be compatible in their relations to each other. In consequence of this, a system can be regarded as a non-random, determinate constellation of phenomena. Thus, if we were given the laws of one element, then the order or arrangement of the other known elements of the system could be ascertained with comparative ease. A notable case in point is the solar system in which the orbital position of only one planet permits the determination of the orbits of the other eight plants which are constituents of the same system. Another characteristic of systems in equilibrium must be noted. Should an incompatibility of elements come about in the system in question, then either one of two conditions would subsequently occur. One is that the incompatible structure (variable) would be required to change to fit the system. The other is that the other structures (or variables) would have to accommodate or adjust to the deviant structure (or variable) such that the equilibrium of the system is restored. In Parsons' view, a strain toward equilibrium is the common property of all empirical systems.

A special additional property, however, is of primary significance for the theory of action. This is the tendency to maintain equilibrium ... within certain boundaries relative to an environment - boundaries which are not imposed from outside but which are self-maintained by the properties of the constituent variables as they operate within that system. (45)
There are two basic processes which are indeed essential for the maintenance of the equilibrium of a given system, namely, allocation and integration. Allocation, in brief, pertains to the processes which sustain the distribution of the system's constituent elements which facilitate the maintenance of the boundaries and equilibrium of a given system. By integration, it is understood that there are processes such that the effectiveness of the relations of the environment are mediated with the given system in order that the boundaries of this system are maintained. It should also be taken into account that self-maintenance of a system must also include a satisfactory maintenance of the elements or parts within the boundaries of the system.

It should not be overlooked that the maintenance of the equilibrium of a social system is also facilitated by the four functional prerequisites and the five pairs of pattern variables. Needless to say, equilibrium occurs on all three levels of system (viz., personality, social, and culture).

A social system, as an on-going process, in equilibrium must somehow attend to the goal-directed motivation of personality systems and the goals established by actors in the political system and other systems. There is always the possibility of non-conformity or deviance such that the marked impairment or even disequilibrium of the social system or of its various subsystems can occur. According to Parsons, there
are two reliable mechanisms (or processes) which channel motivations into generally approved modes in order that the continued existence of the system is not jeopardized; they are, simply put, mechanisms of socialization (47) and mechanisms of social control. (48) By socialization, the personality incorporates the generally approved norms and values of the culture and the basic elements of the pattern variables. Accordingly, the personality is empowered to differentiate between compatible actions with, or disruptive actions to, the social system. Generally, the failure or inadequacy of the socialization process to inculcate the pertinent motivations of conformity to the approved social expectations usually precipitates deviant behaviors. If this type of behavior continues beyond a certain threshold level, the equilibrium of the society is imperiled by possible or actual disruption.

In regard to social control, a discussion of the various distributive functions of the social system is initially required. As stated earlier, the allocative processes generally satisfy the need-dispositions of personalities by the distribution of rewards, and for social systems by the facilities, and the arrangement of personnel. Concerning the former, it may best be illustrated by the division of labor in the economic system for the satisfaction of the need for food. It is to be noted that by authoritative ascription or competition, allocative processes arrange personnel within the relevant subsystems.
of the comprehensive social system in order that the goals of economic needs can be attained. If an integrated social system obtains, then the dispensation of the facilities and rewards are operating without conflict and the system is said to be in equilibrium. But when there is competition over parsimonious yet highly desired positions, facilities, or entities the system is subjected to a strain and, as a result, tends toward disequilibrium. It is at this juncture where the mechanisms of social control enter to preserve the equilibrium of the system; they can take the form of civil and criminal laws, law enforcement agencies, penal institutions, certain punishments, etc.

A moment's reflection on the notion of system and equilibrium in relation to social phenomena, I think, will clearly disclose that Parsons' pivotal concern in his theoretical work is the Hobbesian problem of order. There is always a strong implication that the basic axiom of Parsons' theoretical thought is the existence and maintenance of a broad consensus of norms, a distinct social order.

7) The Theory of Social Change (49)

Not unexpectedly, Parsons' paramount theoretical interests have not centered about questions of social change or conflict. But issues connected with disruptions and alterations in systems have brought him to an outright confrontation with
social change as a complement to his concerns with social structures. Of all his copious works, there are, I would judge, only two extended inquiries into the factor of social change, which were written ten years apart. The first discussion is found in Chapter Eleven of *The Social System* (1951) and the second, in a journal article written in 1961, entitled "Some Considerations on the Theory of Social Change".

According to Parsons in *The Social System* (1951), considerations of social change presuppose adequate treatment of two important theoretical problems. One is

the attempt to work out a conceptual scheme in which the major structural components of the social system could be identified, described and their interrelations in systems, both as internally differentiated and as variable from case to case, worked out. (50)

The other is

the analysis of motivational processes within the system. (51)

For Parsons, it is indeed possible that theoretical accounts of specific processes of change occurring within the contexts of social systems can be made; and these accounts do not necessarily require theories of change of an entire social system. Take note of the following:

The theory of change in the structure of social systems must, therefore, be a theory of particular sub-processes of change within such systems, not of the over-all processes of change of the systems. (52) (Emphasis in the original)
There is, therefore, an important distinction to be noted: that of change within the system and change of the system. As regards the latter species of change, it is Parsons' contention that a theory to account for such events cannot be formulated in terms of our present knowledge of social phenomena. This would require, argues Parsons, a thorough knowledge of the laws regarding the processual features of systems which we do not presently possess and perhaps never will. (53) Whether or not a theory is deemed "static" or dynamic" is irrelevant. For, if it is general in scope and fruitful in its account, it should, in Parsons' view, say something noteworthy about issues relating to change and process within an integrated system.

In a substantial journal article in 1961 ("Some Considerations on the Theory of Social Change"), Parsons advances several novel considerations in this regard, even though his general position is, in fundamental respects, the same as the one that was evinced in *The Social System*. Interestingly enough, Parsons, for unexplained reasons, confines the applicability of his discussion to the processes of change occurring in the subsystems of a society rather than the change of the larger system. Presumably, these considerations which refer to our fragmentary knowledge of the laws of the process of the system are kindred to those presented in *The Social System*. (54)
To start with, Parsons lays heavy stress on the importance of the differentiation of two important processual factors in theoretical concerns involving social change. Processes which maintain a system's equilibrium should be sharply separated from the structural changes in which a system shifts from one equilibrium state to another. (55) When there are disruptions within or without a relatively stable system such that they are powerful enough to cancel the centripetal forces generated by an equilibrium, structural change will take place. Among others, a significant source of disruption for the equilibrium of social systems is the change in the interrelations of a social system to its surrounding environment. What occurs in this respect is that there is a marked reduction in the number of units ("the role of the participating individual actor") (56) which facilitate the system's prerequisite of goal attainment. When these units are performing a welter of functions which very often conflict with each other, there is a general drift toward a condition which Parsons calls structural differentiation incidentally a category which Parsons regards as significant in the consideration of structural change. It is decisive to note, however, that differentiation comes about only when there are accompanying measures of social re-organization; affording the necessary facilities for the execution of these functions within the newly differentiated framework and the
normative patterns and support the normative aspect being the most important. Roughly speaking, effective differentiation entails a re-arrangement of a system of norms on four distinct levels. They are as follows:

(1) The availability of opportunities (37) as a result of the availability of facilities from ascriptive ties. (This means that there are degrees of freedom of action and choice which were absent prior to the process of differentiation.) (58)

(2) The inclusion of units, which are differentiated, in the higher level collectivity structures. (This pertains to the requirement that newly emerging units must be viewed as constituents of a generally approved collectivity structure which gives support or a social sanction to units considered to be part of it. (59)

(3) The upgrading of norms to greater generality. (This refers to the universalistic character of norms (a pattern defining desirable behavior) which do not discriminate as lower-order norms do - e.g. competence is more general than kinship membership.) (60)

(4) The extension of values which legitimize the new functional units. (For the attainment of a satisfactory equilibrium, the extension of values is necessary in order that the newly differentiated units have social support or a recognized single
formula permitting each unit "to do what it does and, equally, essential, not to do what the other does."

These four factors are instrumental in strengthening three conditions of control which are hierarchically arranged viz., (a) the access to facilities, (b) the integrative aspect, and (c) legitimation. Behind all these postulations lies, I should think, an unspoken premise that change within a formerly stable social system occurs when one of the three conditions of control, whatever the source of the disruption may be, does not hold. (61)

C. Critical Comments

I should now like to offer several criticisms of Parsons' substantive theory in terms of the criteria of hypothetico-deductive theory. I shall do so by briefly considering each major aspect that was discussed here. Since my objectives are to examine the logical features of Parsons' theoretical structure, I shall refrain from commenting on substantive matters. For, quite clearly, substantive matters can only by discussed and examined in a theoretical framework if and only if the theoretical structure is properly devised. (62)

Lest I should be accused that I have overlooked some crucial passages in Parsons' substantive works and have inexcusably erected a straw man for purposes of a critical argument
I should like to say a brief word on the objectives with which he has prefaced his various formulations of accounts of social action. In the Preface of *The Social System* (1951), Parsons states definitely that his formulations contained in this volume are to be deemed as a statement of a theory.

The present volume is an attempt to bring together, in systematic and generalized form, the main outlines of a conceptual scheme for the analysis of the structure and processes of social systems. In the nature of the case, within the frame of reference of action, such a conceptual scheme must focus on the delineation of the system of institutionalized roles and the motivational processes organized about them. Because of this focus and the very elementary treatment of processes of economic exchange and of the organization of political power, the book should be regarded as a statement of general sociological theory, since this is here interpreted to be that part of the theory of the social system which is centered on the phenomena of the institutionalization of patterns of value-orientation in roles. (63) (Emphasis is mine - E.R.G.)

But, in another volume which was written in collaboration with Edward Shils and a number of prominent psychologists, sociologists, and anthropologists, and, moreover, which was a direct outgrowth of a series of seminars at Harvard University in 1949 and 1950 from which *The Social System* took its basic inspiration, Parsons reverses his decision. Of course, I am here referring to the book entitled, *Toward a General Theory of Action* (1951). What is interesting about all this is that virtually similar subject matters were formulated and examined in both volumes.
Take note:

This statement does not itself purport to be the general theory which will adequately fulfill these three functions [the theory's function of codification, being a research guide, and a departure point for specialized and empirical research in the social sciences]. It is rather a formulation of certain fundamental categories which will have to enter into the formulation of this general theory, which for many years has been developing through the convergence of anthropological studies of culture, the theory of learning, the psychoanalytic theory of personality, economic theory, and the study of modern social structure. (64)

In another section, it is mentioned that

The present monograph is a straightforward exposition of a conceptual scheme. (65)

However, these affirmations of purpose and intent are again thrown into doubt when Parsons writes in a journal paper in 1953 that

The first important point I would like to make is that, while I, and I think several of my colleagues, felt that the two books in question documented what was in certain respects a considerable advance in differentiation and integration of the conceptual scheme we have been calling the theory of action, they were in no sense meant to suggest any fundamental break in the continuity of theoretical development in the field as a whole;... (66) (Emphasis is mine. E.R.G.)

Indeed a confusing state of affairs! How are we to construe these conflicting claims? Since Parsons generally views his work as theoretical and one of his first books on the topic of social action and numerous subsequent journal articles explicitly state that it is, I have decided, perhaps arbitrarily, that all of Parsons' extended discussions on social action such
as those found in *The Social System* and *Toward a General Theory of Action* are theoretical in the strictest sense of the word. Another reason for the adoption of this standpoint on my part springs from my conception of theoretical enterprise (which in turn is based on the criteria of hypothetico-deductive theory) that the development or discussion of concepts is to a certain degree a theoretical endeavor.

(a) The Action Frame of Reference

As Parsons himself concedes, this aspect of his general theory is primarily a *system of concepts*, a taxonomy. They furnish no explanations whatever, or are they a collection of general statements (or laws) which could be explicitly articulated more fully and subjected to test. What basically Parsons does is that he devises a scheme of concepts of those social factors which he deems pertinent to his theoretical purposes. (67) It may next be pertinently inquired as to where he acquired them. Were they derived from a series of general statements? But Parsons does not mention any definite relational statement at any place in his formulations which would permit derivation of statements entailing these notions.

It is evident that Parsons employs either an unacknowledged theoretical scheme or that he is entirely unaware of the general relationships (or laws) that he is making. It will be remembered from the discussion in Chapter Three that any descrip-
tion or taxonomic activity is by the nature of the case highly selective. Thus, whatever the degrees of abstraction of an enumeration of explicitly defined concepts, a theory - or as is frequently the case - a spate of theories is tacitly employed. (67) Discussing the "logical source" of these concepts (the problem which besets us here) in his critical essay on Parsons' theory of action, Max Black appositely observes that...

... So long as the system in which the concepts are embedded is furnished with an adequate supply of "coordinating definitions" for drawing observational consequences from theoretical premises, that is all that can reasonably be expected. I see no reason why this should in principle be impossible in Parsons' case. The ingenuity of the experimentalist can be trusted to invent reliable tests for the presence of any of the concepts that Parsons needs, though I do not wish to underrate the difficulties of this program in particular cases.... (69)

... Now, if we had a clear view of the process by which the concepts are obtained, we might be in a position to judge whether they have been well chosen and in what ways the choices might be improved .... (70)

In consequence of the absence of any theoretical statement (or premise) renders it extremely difficult to adjudge Parsons' contributions. To delve into the depths of a conceptual scheme's background and attempt to reconstruct these general premises is, very often, an irksome, unfruitful task. There are no definitive guides for evaluation or comparison and one must depend to an inordinate degree on unsatisfactory, often baseless conjecture. How then are a set of concepts to be evaluated as useful or unuseful in terms of other proposed sets of concepts? For indeed one set of concepts cannot be
adjudged more useful for theoretical purposes than another unless the general statements of which they are constituents are presented.

... For it is easy enough to provide some set of definitions or some conceptual scheme: the difficulty is to provide one that is not capricious or arbitrary. The supreme virtue of a scientific classification, whether in physics, chemistry, or biology is that it arises from, and is in some sense demanded by, a system of well established empirical generalizations and theories ... (71) (Emphasis in original)

Whether the concepts Parsons has devised can be by some chance or deliberate formulation incorporated into theoretical statements (or hypotheses) remains only a vague possibility. (72)

However, an enumeration of concepts at the outset with the hope of eventually inserting them into a congenial theory (or theories) has one serious drawback for the scientific analysis of a certain kind of phenomena. It is, at base, an approach replete with what I think are unnecessary procedural impediments, especially in a scientific context. Parsons narrows the possibilities of a formulation of theoretical statements by presenting a bundle of concepts which, for no cogent theoretical reasons, he thinks are important for a theory in sociology. Moreover, he adumbrates a context in which the development of general theoretical statements is considered to be sanguinely possible instead of proffering a set of general statements which can be examined and empirically tested, irrespective of their level of generality. Considerable
scientific gains, even if they are negative, could be garnered from the latter approach in comparison to the approach Parsons adopts. Fundamentally, this is a question regarding effective strategy in theory-construction. Which approach or strategy will aid us in reaching the goal of fruitful explanation of a social phenomena, the development of concepts first or the formulation of general statements preferably with a low degree of abstraction to begin with? As I have argued in Chapter Three, the latter strategy is much more promising. It permits one to test at once what he has hypothesized. If this is undertaken in such a gradual fashion - that is, there are progressive steps in the levels of abstraction employed - one will have at the least empirically tested theories or statements about the world. On the other hand, if a battery of concepts is initially developed, or if statements of an exceedingly abstract nature are formulated, testing will be commensurately, exceedingly difficult — impossible for the system of concepts for concepts in themselves are not susceptible of empirical test. In this fashion nothing of significance about our knowledge of the world will be advanced one whit; what usually occurs in such approaches is that the theorist moves in a framework of hope — frequently incapable of realization — rather than offering anything of substance, whether it be positive or negative in character.
It may be remarked in passing that an emphasis placed on the development of an elaborate conceptual scheme (a catalogue of concepts) is required prior to the formulation of general principles (or laws or empirical uniformities) as Parsons does, conveys an impression of a covert inductivism which, to be sure, is out of place in scientific inquiry.

There is, I believe, a somewhat perplexing implication in Parsons' action frame of reference deserving brief notice. It mainly concerns the notion, that (the following is my paraphrase) "all human action is goal-directed", is an empirical characteristic (or generalization). But a reconsideration of this issue plainly suggests an altogether different interpretation. Clearly, the assertion is one of a logical (tautologous) nature rather than one of empirical significance as is customarily understood. It should be patent that the concept of "goal-directed behavior" and "purpose" are logically embodied in the notion of "human action" and as such represent what we all of us conceive being human entails. (73)

(b) **The Personality, Social and Cultural Systems**

In essence, the criticisms which were raised in the preceding section on Parsons' "Action Frame of Reference" are applicable in virtually every respect in the evaluation of this aspect of his theory. Parsons has put forward another
framework of concepts - though I may say they are quite interesting - without connecting them in any way to a general statement (empirical laws or uniformities).

If we momentarily overlook the conceptual factor, another problem of notable dimensions springs into view. It refers to the analogies that Parsons boldly draws between social systems and personality systems. Parsons imposes a common set of concepts into both types of systems (which are conceptually and factually disparate) without undertaking pertinent modifications at points where they would be necessary. To be sure a social system or one of its many subsystems cannot be intelligibly said to display feelings of a negative or positive sort or maintain goals as a human actor does. Justified bewilderment can only be registered in these respects. (74)

It could be suggested, furthermore, that this lack of clarity or improper analogy if one will, arises from Parson's failure to furnish explicit general statements of relations where, I should think, ambiguities of this nature would be swiftly discovered and rooted out.

(c) The Pattern Variables

Perhaps the first - and most important - question that can be legitimately posed in regard to these concepts is this: Whence do they come? Are they elements of empirical assertions? Are they elements of general statements (empirical laws or uniformities) belonging to a theory? As with the concepts
embedded in the action frame of reference and the framework of the three systems, so also with the pattern variables - no unambiguous indication is provided in this regard. They appear to be an arbitrary collection of concepts devised for purposes of description without any theoretical scheme in mind. As a consequence, they are subject to all the onerous problems of concept formation which obtain when there are no explicit references to a theory.

It is significant to note that Parsons views these concepts as directly derivable (75) from the frame of reference of the theory of action. What this can possibly mean is wholly unclear. For one thing, concepts by themselves are not capable of being derived (or deduced) from other concepts which constitute the entire action frame of reference. As mentioned earlier, only propositions from a set of other logically interrelated propositions can be said to be deduced. One is compelled to conclude, then, that Parsons' contention that pattern variables are derivable from another system of concepts indicates a bizarre usage of the term, "derive".

Although I stated that I would endeavor to avoid any remarks on substantive matters, there is a property about the pattern variables to which I must address a critical note. To repeat, a pattern variable involves five specific paired choices which an actor must make before a situation will have a determinate meaning for him. Should, however, the notion of choice be
construed in a very constricted sense, then it would seem, contrary to all intuitive assumptions we maintain by virtue of our being acting human beings and also scientific knowledge, that an individual must make only five choices. For, there are always choices with differences of degree. (76) But, as Black points out, where the qualificatory phrases "explicit or implicit" are appended as in the following statements, only confusion ensues:

These pattern variables enter the action frame of reference at four different levels. In the first place, they enter at the concrete level as five discrete choices (explicit or implicit) which every actor makes before he can act. (77)

Though it may be rather harsh in tone, the only concluding comment I can offer with regard to the pattern variables is that they are not testable statements or concepts with any explicit theoretical reference, hence they are of meager, if any, theoretical value.

(d) **Functional Prerequisites**

Parsons' employment of the notion of functional prerequisite brings to sharp relief several fundamental elements -- and weaknesses -- of his structural-functional theoretical approach. Again Parsons does not demonstrate as to how he acquires these concepts; he does not indicate whether they are derived from general theoretical statements or perhaps empirical statements. One could assume, however --- and nothing more --- that they are imbedded in a series of implicitly held general statements.
In my estimation, the significant factor in Parsons' formulation of the functional prerequisites to be considered is the concept of goal. (78) If it is used uncritically -- as it frequently is -- this concept and its attendant implications precipitate endless conceptual confusion, thereby, rendering the theory inapplicable to the empirical world. However one may look at it, or avoid it, the concept of goal implies the concept of purpose. And, a purpose presupposes a holder -- in other words, an actor. Thus, in functional accounts of social phenomena, one must be prepared to specify the ways and means a goal is actively reached and the expressed purpose (or purposes) of the implicated individual (or individuals). Without this two-fold knowledge, a functional account employing the notion of goal-attainment is destitute of explanatory significance chiefly because it excludes mention of the indispensable information upon which an explanation can be effected. In addition to this, it is to be noted that incomplete information of this type of account renders impossible the formulation of certain requisite conditions which would indicate the kind of empirical data necessary to discard the sentence stating that a goal of a certain system is such and such. To be sure, it is in the light of these statements that a readily understandable meaning can be imparted to statements asserting that a certain event has certain consequences, or is functional (positive or negative) for the system undergoing analysis.
The ascertainment of the goals of various social organizations is, then, imperative in a functional analysis. If the topic of analysis is small-scale organizations such as political parties, families, industrial concerns, religious organizations, this task can be executed with comparative ease. However, problems of a large order occur when the goals of a society or even an institution are requested. What does it mean to say that the goals of a social system (a society are such and such? Where are these goals (or purposes) held? A large part of the population? The governors? The writers of the constitution of that society's government? The business class?

This is the juncture, I should think, where a consideration of Parsons' argument of the functional prerequisites of a social system, particularly the goal-attainment factor, relevantly enters. These arguments, I fear, become mired in an inextricable logical problem. It is as follows. For purposes of illustration, let us take as a case in point the country of Canada (1967) in which there is an exceptionally strong Separatiste sentiment expressed in the province of Québec. To start with, how can it be asserted with any confidence that such and such are the goals of Canadian society in 1967? What criteria could be used to determine what they are? None, so far as I can ascertain. Interestingly, on Parsons' account,
if the goals cannot be discovered, the only other possible consideration that can be advanced is that Canada is not a social system. Thus, what Parsons' notion of goal-attainment as a functional prerequisite for systems of action, in effect, amounts to is a vacuous concept, a tautology; for, it is not an element in a general theoretical statement or even an empirical assertion. It, clearly, does not differentiate between systems with goals or without goals.

In bare essentials, the proper utilization of functional prerequisites in functional theory, is analogous to that of an applied social science where considerations of adaptation and adjustment in the adjudication of consequences of certain actions are typically employed. Noticing this tendency of functional theory, Hans Zetterberg puts forth several pertinent observations:

At present, functionalist formulations enjoy wide currency in sociology. I have some misgivings as to their usefulness to the sociological theorist, since they assume either self-maintaining "goal states" that have not yet been discovered, or universal "imperatives" that are subject to disagreement and probably are unrelated to at least some sociological knowledge. Less doubt can exist, however, of the usefulness of functionalist formulations to the social practitioner. The logic of functionalism—to judge consequences in terms of "adjustment" and "adaptation," or "goal states," or "imperative" problems specified in advance of analysis—is the typical logic of applied theory.
(e) **The Concepts of Systems and Equilibrium**

Parsons' comprehension of the concepts of "systems" and the accompanying notion of "equilibrium" suffers from the enfeebling shortcomings which characterize most attempts at its incorporation into sociological theory. A system or "empirical system" as Parsons terms it suggests that there is a multiplicity of interrelationships obtaining among the constituent variables of a bounded dimension but it does not explicitly spell out just precisely how these relationships are structured. There is no mention of general statements (or laws) which are necessary to express relationships between a variable or a set of variables and those of other sets of variables and so on about the roster of variables comprising this structure called the system (the social system). (80)

At best, what Parsons is doing is resolving to treat certain social phenomena in a unified fashion. Indeed, this is a laudable aim. But the results are none too encouraging. His conception of system assumes the form of an analogy in which a phenomenon composed of manifold elements can be viewed as a system, for example, an organism, an industrial organization, a jury, a society. Then he implies that every system has a subsystem and that the system has an environment. Clearly, the notion of system employed in such a flaccid sense can be applied to any object or combination of objects, mental or
physical. But here is the rub, a concept that is applicable to everything is logically vacuous. Surely, one must, in addition, know what type of conditions or characteristics of objects or combinations of objects would not be able to comprise of some type of system. It will be noted that Parsons applies the concept of system to the personality, social, and cultural values and each, in turn, has innumerable subsystems. Moreover, in this connection, how could one confute the statement that every system, particularly those which Parsons presents, has subsystems? I doubt whether one could because of the rather loose nature of his conception of system. To be sure, the empirical significance of any concept is furnished by a procedure of contrast, a knowledge of the areas of the world to which the concept (such as system) is not applicable. This, I fear, characterizes in essential respects Parsons' conception of system which he further curiously qualifies as "empirical system". It may be further mentioned that there are no such things as systems in the empirical world; a "system" is a contrivance of the human mind to aid it in its attempts to familiarize itself with the world. This problem, I think, could have been effectively circumvented if a series of explicit general statements (or laws) mapping out various relations between divers variables were furnished.
Concerning Parsons' conception, "equilibrium", I would think that much of what I have said in the foregoing would be relevant. For, to be sure, equilibrium is another notion which is abused in the social sciences, as the notion "system" is, if a series of general statements pertaining to the relationships of variables and the condition of a given structure (the equilibrium of a system) are not explicitly supplied. I fear Parsons abuses it as a consequence. However whether such a theoretical explanation is possible in the present state of knowledge and theoretical refinement in sociology is another question. One possible answer is ventured by G.C. Homans:

In spite of the endless discussion of the "equilibrium concept" in sociology, a discussion to which I myself have contributed too much ..., no such equilibrium proposition (not concept) has ever been both stated and rigorously used in a sociological deductive system. Unless one is both stated and used, I consider any further discussion of equilibrium to be wasted breath. (81)

(f) The Theory of Social Change

A theory of social change in genuine scientific endeavor is, in my estimation, a fantasy. (82) Parsons along with a host of other social theorists engage in the fruitless pursuit of its formulation and the discussion of its theoretical relevance for sociology. A fundamental understanding of the logical structures of scientific theory and explanation particularly in terms of the criteria of hypothetico-deductive theory,
unmistakably indicates that the notions of static theories (of social structure) and of change theories (of social dynamics) are spurious to the first degree. Hence, they are totally unnecessary. Concrete events can only be accounted for (are explained) by a pertinent general statement (or law) from a theory combined with descriptive statements of antecedent boundary conditions irrespective of their spatio-temporal loci such that one event in question is subsumed. In regard to the appearance of a theory of social change in Parsons' work, it is perhaps well-nigh an inevitable development because his constant preoccupation with mere description and conceptualization and the attendant neglect of formulating general statements for purposes of explanation leads him to maintain a static characterization of social phenomena. As a result, he cannot account for variation or change in the phenomena he is dealing with. It is indeed evident, that Parsons' failure to consider issues in scientific explanation and, concomitantly, the development of a body of general theoretical statements has led him to conceive scientific problems which have the ring of the spurious about them. More generally, however, this failure underscores his notable lack of the requisite philosophical sophistication to engage fruitfully in discussions and critical evaluations of "theory", in both metatheoretical and substantive considerations.
CHAPTER SIX

CONCLUSIONS

With the discussions of Parsons' substantive theory and his metatheoretical considerations now in perspective, several general comments, I think, can be reasonably advanced at this juncture.

Parsons' conception of scientific theory from the very outset of his theoretical writings to the present is a curious blend of conceptual elements which has remained fairly stable. It is chiefly comprised of outmoded assumptions used by theorists of a bygone era in scientific thought; confused renderings of contemporary theoretical notions; and, admittedly, a number of assumptions and ideas finding support in contemporary philosophy of science and scientific practice. Nonetheless it still remains a blend. Parsons' various remarks in this regard often reveal that he has not kept abreast with the developments or literature in the philosophy of science in regard to thinking on scientific theory as such. What is particularly salient about Parsons' conception of scientific theory is that there is almost a complete absence of any reference to the explanatory and predictive capacities of theory and the role of general statements or laws may play in this respect; to be sure, in terms of hypothetico-deductive theoretical thought these notions are the very nerve-center of all scientific theory. Instead, however, there is a preponderant, or to be more exact, inordinate stress placed on the importance of concept
development in highly abstract terms which, Parsons assumes, effect in some unspecified manner, the formation of laws. Needless to add, this is scientific nonsense in the extreme. I shall remark in what follows further on the implications of this standpoint, if such it can be called.

With regard to Parsons' substantive theory, it may be said that he has constructed an intricate conceptual scheme which, perhaps, is useful for casually describing an enormous range of social phenomena. By doing so, Parsons, in my estimation, does not introduce a theory, but, contrary to what he and his followers may expostulate in reply, employs a large variety of implicit theories, of psychological, sociological, culturological (anthropological) natures. (1) It is beyond dispute that no theoretical statements (laws or propositions) or even empirical hypotheses are advanced.

It is indeed apparent that the structure of Parsons' substantive theory in large measure, mirrors the structure and form which he maintains in his metatheoretical considerations, his conception of the nature and uses of scientific theory. Most striking in this connection is Parsons' preoccupation with the erection of a system of concepts antecedent to the formulation of general statements (laws or propositions). This indeed is interesting. For, despite his pungent criticisms of anything approaching an inductivist's position (that is, an exclusive concern for facts which altogether forsakes theory), it is ironic that a similar criticism can be levelled against Parsons
when he stresses, quite unjustifiably, the primacy of concept development to the straightaway formulation of general explanatory statements. To be sure, the selection of concepts, as with facts, are wholly dependent on the implicit or explicit theories the selector has in mind.

Also in Parsons' substantive theory the very presence of concepts pertaining to questions largely dealing with phenomena considered to be either psychological or sociological, or cultural, indicates that Parsons' problem range focuses on a vast diversity of phenomena. What Parsons seeks to do is to give accounts (describe?) through a single theoretical scheme a welter of phenomena (psychological, sociological, and cultural) which form a complex. Now a complex is a convenient arrangement of diverse events such that they are subsumed under a single expression; "war", "revolution", "athletics" are illustrations. But it should be noted that because a variety of phenomena is referred to by one name, it does not mean that it is explainable by one theoretical scheme any more than a typhoon or a thunderstorm which, incidentally, requires a number of theories to explain certain aspects of each, e.g., aspects such as electricity, air pressure, precipitation. These aspects are regarded as varied manifestations of general empirical uniformities (or, alternatively phrased, general statements, propositions, or laws). Thus, strictly speaking, nothing can be said to bring about a typhoon or a thunderstorm. Clearly, then, each phenomenon in a complex
is explainable by (or what amounts to the same thing, subsumable under) a general statement or law). (2) In Parsons' case, however, the complex is, broadly speaking, a large range of social behavior, from the personality level, to the strictly social, to the cultural aspect. All this, it is proposed, is to be accounted for (or described or analyzed, as Parsons intimates) by the theory of action. Given the present state of knowledge in the social sciences and the limitations imposed by the logical structures of theory and explanation, an effective execution of any programme in this connection, including Parsons', I think, is foredoomed to failure; it is a virtual impossibility.

Thus, in the light of the criteria of hypothetico-deductive theory, Parsons substantive theoretical contributions, I reluctantly conclude, do not constitute a theory, though it must be mentioned in the next breath that he affords an immense number of suggestive guides and leads for possible inclusion in theoretical formulations. Harsh though my judgment may be, the notion of orientations, I believe, appositely describes the bulk of Parsons' written work on substantive matters. That is to say, orientations are approaches which

... involve broad postulates which indicate types of variables which are somehow to be taken into account rather than specifying determinate relationships between particular variables. Indispensable though these orientations are, they provide only the broadest framework for empirical inquiry....

The chief function of these orientations is to provide a general context for inquiry; they facilitate the process of arriving at determinate hypotheses.... (3) (Emphasis in original)
Although Parsons' theoretical contributions, when viewed from the perspective of hypothetico-deductive theory, have not yielded much of a positive nature, either in a substantive or philosophical vein, it emphatically does not mean that Parsons has not proffered anything else of value. More than any other theorist, Parsons' active concern, though often misguided I should think, for the development of a highly abstract theory has prompted and, no doubt, inspired others in sociology and related disciplines to confront innumerable issues of theoretical import that cannot be dismissed out of hand. It is for this reason that Parsons is an important man in sociological thought in the mid twentieth century. To be sure:

A man can be most important in the development of a subject even if all his theories were wrong, provided they stimulated others or that he opened up new problems which were taken up by others. (4)

Parsons' herculean and ingenious efforts directed toward the construction of theory and the advocacy of its inestimable importance in furthering our comprehension of social reality are undoubtedly an outstanding testimony to the patience and devotion required by those who truly esteem intellectual illumination and scholarship. But equally so, however, they exemplify the unnecessary labor and confusion that are frequently engendered when they are bereft of a basis in critical thought.
(1) I am obliged to stress here that this statement represents, I should think, a consensus of opinion prevailing among contemporary social scientists rather than a corroborated empirical assertion about the opinions held by a significant number invaluable in the social sciences. Also contributing to this impression is the sheer frequency of allusions to, and acknowledgments of, Parsons' theoretical works in other substantive and metatheoretical (philosophical comments on scientific theory) writings in the social scientific domain.

In the series of abbreviated quotations that follow, it will be plain that Parsons' eminence as a sociological theorist is widely recognized and taken for granted.

(a) "It appears reasonable to state that the major theoretical system in sociology of the last decade has been that of Talcott Parsons." J.B. Gittler and E. Manheim, "Sociological Theory", In J.B. Gittler, 1957, p. 15.

(b) In an explanatory note on the contents exclusively devoted to considerations of Parsons' theoretical writings in the Winter, 1959 issue of the Alpha Kappa Delta, the editors saw fit to remark that:

"Currently a lively interest has developed in sociological theory. This issue of the Delta honors the scholar [Talcott Parsons] who, perhaps more than any other living sociologist, is responsible for this interest." See the note, "The Articles in This Issue," Alpha Kappa Delta vol. 21, no. 1, Winter, 1959, p. 2. Clearly, it is implied here that Parsons has been instrumental in engendering active concern in sociological theory by his voluminous contributions. Although this may be a shade exaggerated, it, nevertheless, indicates the prominence bestowed upon Parsons as a theorist.

(c) Bertil Pfannenstil, in a Swedish publication, provides a distinctive international color to this discussion on Parsons' influence and reputation as a theorist when he says: "Only a sociologist with an established position in sociological circles
can formulate a system for interpretive sociology (the presentation of broad perspectives and a large body of empirical material which evidence them). Such a system may be found in Talcott Parsons, perhaps the most influential theory-builder in contemporary American sociology. Since Robert Merton belongs to this orientation, and we may also add that the sociologists already mentioned, we find an imposing array of American sociologists on the side of interpretive sociology. Parsons and Merton call their approach structural-functional.” See B. Pfannenstil, "The Construction of Theories in Contemporary Sociology: An Orientation", in H. Brat et al., 1963, p. 172.

Even one of Parsons' most mordant critics, George C. Homans, concedes that Parsons is a theorist of great repute. As Homans puts it in a critical essay: "Some sociologists sound as if they do not know what a theory is. If, in what follows, I single out Talcott Parsons as an example of confusion, I do so only because he is the most famous of contemporary theorists, so I cannot be accused of hitting a man when he is down." See G.C. Homans, "Contemporary Theory in Sociology". In R.E.L. Faris, 1964, p. 957.

In a paper surveying the explanatory character of various theories in contemporary sociology, R.A.H. Robson, another trenchant critic of Parsons' theory, observes that "Talcott Parsons is without doubt the most well known "theorist" in American sociology." and "...I would like to emphasize that Parsons is generally regarded as the foremost theorist in sociology." See R.A.H. Robson, "The Present State of Theory in Sociology." (Presented at the International Colloquium on the Philosophy of Science, June 16, 1965, at the London School of Economics, London, England) Mimeograph copy, p. 3 and p. 8.

Helmut Wagner, in a recent paper on general theoretical currents in American sociology, gauges the effects of Parsons' major theoretical works on the discipline of sociology. He declares that: "To a considerable degree inside and generally outside the discipline, Parsons was recognized in the 1950's as the theorist of present-day American sociology." (p. 283) and "In the next decade also in the 1950's large-scale theory became practically synonymous with Parsonianism." (p. 283).

During the same period, according to Wagner, Parsons' mode of theorizing became one of the prime "schools" in American sociology:

On the surface, these expectations seemed plausible. Most of all, empirical positivism and Parsonianism appeared as the two dominant "schools" of American sociology; their rapprochement would quite automatically reduce the influence of all other sociological orientations. Even without such a pressure, the exponents of non-positivistic approaches seemed everywhere on the defensive. The theories of action which
significantly differed from Parsonianism, like those of Pitirim Sorokin, Robert MacIver, Florian Znaniecki, and Howard Becker, were largely relegated to the background. The few sociologists of the German humanist tradition, like Paul Honigshem, could be ignored as alien elements on the American sociological scene. And finally, Robert Lynd's earlier and passionate plea for a sociology of the social conscience seemed to have lost its meaning in the face of war boom and post-war prosperity.

By and large, from all this, it seems fairly safe to maintain that Talcott Parsons has definitely been since the publication of *The Social System*, not to mention *The Structure of Social Action* - and still is - the dominant theorist in contemporary sociology.

(2) For a complete bibliography of Parsons' published works from 1928 to the present, I would suggest that one of the following references be consulted:

R.H. Ogles, "A Complete Bibliography of Talcott Parsons and Selected Reviews and Critiques of His Work", *Alpha Kappa Delta*, vol. 29, no. 1, Winter, 1959, pp. 73-80. (This collection of references contains all of Parsons' published works from 1928 to 1958. It is particularly valuable in that it includes a fairly exhaustive list of the important reviews and criticisms of Parsons' theoretical formulations that have appeared from the late 1930's to 1958.)

There are bibliographies included in the two following books.


(3) This intention finds its clearest and most pointed expression in Talcott Parsons, "General Theory in Sociology." In R.K. Merton, L. Broom and L.S. Cottrell, 1965, pp. 36-37:

"Finally, it is quite clear that the present order of codification could not be worked out if the theorist remained a narrow "purist" of sociological theory and refused to be interested in the theoretical resources of a whole series of neighboring fields. Not only sociology, but economics, political
science, and psychology have contributed vital parts to the structure I have outlined, and a comparative perspective would bring in much anthropology and history. Indeed, it is my conviction that good general theory in the field of human action, no matter how firmly grounded in one discipline, is inevitably interdisciplinary theory."


(7) At the outset of his intellectual career, for approximately ten years, Parsons devoted almost his total published output to the analysis and exposition of the formulations of early European social theorists, primarily those who interrelated the common aspects of economic and sociological theories.
In large part, Parsons concentrated on such theorists as Werner Sombart, Karl Marx, Alfred L. Marshall, Vilfredo Pareto, Max Weber, Emile Durkheim, Thomas Malthus, and the American economist, F.M. Taussig. In what follows I have compiled a bibliography of his works from his first published work to the publication of *The Structure of Social Action* in 1937.

In chronological order they are:

- ""Capitalism" in Recent German Literature: Sombart and Weber - Concluded. *Journal of Political Economy*, vol. 37, Feb., 1929, pp. 31-51;
- "Pareto's Central Analytical Scheme." *Journal of Social Philosophy*, vol. 1, April, 1936, pp. 244-262;

(8) Prominent among these sociologists - primarily because of their inclination to express themselves in this matter in their published work - are the following sociologists:

Llewellyn Gross, George C. Homans, Reginald A.H. Robson, and Hans L. Zetterberg. Their specific observations in this connection, *viz.*, that Parsons is not proffering scientific theory are found in the following publications:


The mode of theory these sociologists recommend for contemporary sociology is outlined and argued in their publications which are enumerated in the "Selected Bibliography" of this study. In brief, it is of the hypothetico-deductive kind which, as I shall argue in chapter three, is perhaps the only kind of theory acceptable in the dimension of science as such. In any event, of this more later.

It may be remarked, furthermore, that philosopher, Max Black is another significant writer who has styled Parsons' theoretical work as essentially non-scientific. Though unversed in the problems and difficulties of social science - sociology in particular - his remarks on the structure of Parsons' theory and the deficient logic undergirding it are indeed apt and illuminating; see M. Black, "Some Questions About Parsons' Theories," In M. Black, 1961, pp. 268-288.

(9) Out of a plethora of reports on concrete studies, too numerous to mention here, and papers pertaining to problems arising therefrom that have been undertaken, two outstanding cases in point are R. Williams, American Society: A Sociological Interpretation, New York, A. Knopf, 1951 and P.M. Blau, "Operationalizing A Conceptual Scheme: The Universalism-Particularism Pattern Variable," American Sociological Review, vol. 27, no. 2, April, 1962, pp. 159-169.

For a further account of Parsons' influence on current research and theory-construction in sociology, refer to R. Williams, "The Sociological Theory of Talcott Parsons," In M. Black, 1964, pp. 91-92.

Very recently, Alasdair MacIntyre has interestingly suggested that Talcott Parsons' work, particularly the theory adumbrated in The Structure of Social Action, can be viewed as a substantial contribution to fruitful reflections in the speculative philosophy of history. He notes:

"The key book here" - (how the questions which were critical for the speculative philosophy of history have been revivified in modern sociological theory)" is Talcott Parsons'
The Structure of Social Action. The common accusation that Parsons' work is anti-historical, that Parsons views social systems in ways which rule out any adequate view of historical change, must make this remark seem odd unless it is amplified. But this accusation holds, if it holds at all, of the Parsons of The Social System rather than of the Parsons of The Structure of Social Action; and, more importantly, Parsons even perhaps in spite of himself inherits and transmits to contemporary sociological theory what was the key question of classical philosophy of history. "Men make their own history, but not just as they please" (Marx). The relationships between human purpose and agency and those larger structures which although formed and informal by such purpose and agency have a life of their own and impose constraints both felt and unfelt upon how men live constitute the central problem which the modern discipline inherited from the older one...


By far, the most unsparing strictures of Parsons' mode of expression are contained in Faris' lengthy review of The Social System and Mills' substantial chapter also on Parsons' The Social System in his The Sociological Imagination. The pages are noted above.


(15) Pitirim A. Sorokin, the bilious doyen of contemporary sociological theorists, has devised a table by means of which striking similarities between his own theoretical formulations largely found in his Social and Cultural Dynamics, 4 vols. (1937); Sociocultural Causality, Time, Space (1943); and Society, Culture and Personality (1947) and those of Parsons in his The Social System (1951) and Toward a General Theory of Action (1951) can be conveniently arranged side by side and examined without undue hindrance. The correspondences clearly are close but whether they are attributable to unacknowledged (deliberate or incognito) incorporation, hence, plagiarism, or perhaps coincidental convergence is another question; a difficult question at that. See P.A. Sorokin, Sociological Theories of Today, 1966, pp. 419-432.


(17) Cf. L. Gross, "Rejoinder", /To Talcott Parsons' Comment/ American Journal of Sociology, vol. 67, no. 2, September, 1961, p. 140. Take into account this: "Clarity is relative to competence of reader as well as writer, and neither Parsons nor I can support opposing claims in this regard without analysis of conditions and criteria for judging this quality."

(18) Cf. R.M. Williams, "The Sociological Theory of Talcott Parsons," In M. Black, 1961, p. 93. We are in essential agreement here. As he notes, "Nevertheless, there is an intellectual content which can be grasped, and it is that content in which we are interested."

It is interesting to note Parsons' reaction to the chronic plaints about the opaqueness of his prose. From the literature, it is evident that Parsons elects to withhold comment on this matter. But in his reply to a number of critics in the Black volume Parsons forthrightly confronts this complaint in a
curious fashion. He does not appear to be swayed by these incessant animadversions.

"It is perhaps in the nature of the type of pragmatic development which has been sketched here, both that it should be, as a theoretical enterprise, the generator of substantial amount of resistance even to its scientifically meritorious features, and that it should be to a peculiar degree thrown on the critical judgments of the relevant professional groups - the latter point is relevant particularly because of abandonment of the protection of a rigid ideological position which has figured so prominently in Continental Europe. It is at least tempting to think that this situation may have something to do with the recurring complaint, for many years now, about my being so hard to understand. Having reached what I hope is a certain "age of humility" I am not at all prepared to discount entirely the view that there are peculiar and unnecessary obscurities in my writings. At the same time I can claim to be somewhat sophisticated in the sociology of knowledge and hence in the interpretation of resistances to certain types of intellectual innovation. In this role I cannot entirely dismiss the possibility that some of the complaints may be manifestations of such resistances. In any case, it is not possible for an author to be fully objective about the reception of his work; any more ultimate judgment will have to be left to the outcome of the process of natural selection through professional criticism by which scientific reputations ultimately come to be stabilized."


(20) Most notable here are Gestalt psychology under Wolfgang Kohler and E.C. Tolman in regard to purposive behavior. As concerns Kohler, see Talcott Parsons, The Structure of Social Action, 1964, p. 482; as for Tolman see Talcott Parsons,
For further comments on these psychologists vis-a-vis Parsons, see notes 39 and 40 in chapter two.

(21) During the past decade and a half, the rather large utilization of Freudian postulations and insights have become increasingly evident in Parsons' theoretical formulations. Some outstanding examples in this regard are as follows: Talcott Parsons, "Psychoanalysis and the Social Structure," (Originally published in 1950) In Talcott Parsons, Essays in Sociological Theory, 1964, pp. 336-347; Talcott Parsons and Edward A. Shils, "Values, Motives and Systems of Action," In Talcott Parsons and Edward A. Shils, 1964, pp. 67, 85, and pp. 118-119.


(23) On this, see Talcott Parsons, "An Outline of the Social System," (1961) In Talcott Parsons et al., 1965, p. 37; Talcott Parsons, "The Point of View of the Author," In M. Black


(25) For this, it is suggested that the collection of essays in Parsons' volume, Structure and Process in Modern Societies, 1960, should be consulted.

Extended treatments of these topics are found in the following works of Parsons: The Social System, 1964, pp. 428-479 (on social structure) and pp. 480-535 (on social change); "Some Considerations on the Theory of Social Change," Rural Sociology, vol. 26, 1961, pp. 219-239; "An Outline of the Social System," (1961) In Talcott Parsons et al., 1965, pp. 41-70 (on social structure) and pp. 70-79 (on social change).

(26) Talcott Parsons, Societies: Evolutionary and Comparative Perspectives, 1966, pp. 9, 11-14, 17, 21, and 113.


(29) An example is Max Black in his paper "Some Questions About Parsons' Theories," in M. Black, 1961, pp. 268-288. Even the title of this paper is indicative. Throughout the years, Parsons has retained a fairly basic framework of ideas i.e. the action framework. But he has altered and varied the external features, as it were, of the theory on many occasions. He has not renounced the general principles on which these renovations are undertaken. Thus, it is rather misleading to say that Parsons has presented "theories". It is clear that a theory can be refurbished without losing its identity as the same theory when it was just subjected to revision and test. For, to be sure, this is the basis of all scientific endeavor - that is, change. But change does not necessarily imply abandonment nor abandonment change.


(30) As I shall indicate later in the text, I consider this term to represent ideally what this notion of theory compels one to undertake and to observe; Other names for this type of theory which are generally used are: "axiomatic theory," "deductive theory," and "explanatory theory".


C. Schrag suggests a notion, "Style of inquiry" which can be viewed as commensurate with what I would regard as "strategy" in theory construction. C. Shrag, "Some Demerits of Contemporary Sociology," Pacific Sociological Review, vol. 4, no. 2, Fall, 1961, p. 44.

See also Barrington Moore, "Strategy in Social Science," In M. Stein and A. Vidich, 1963, pp. 66-95, esp. p. 76. Although Moore's discussion in many respects diverges from what I consider germane to the issue of strategy, his remarks are nevertheless provocative and stimulating.


(35) Refer to note no. 28 of this chapter.


I should like to add that my thinking in regard to matters of scientific evaluation and cognate concerns closely correspond to those of Kaufmann, who, I believe, more than any other philosopher - or scientist for that matter - has assiduously endeavored to highlight the measureless importance of a knowledge of the fundamental canons of scientific discourse to both natural and social scientists. Moreover, I concur with Kaufmann's argument that the rules and precepts of scientific procedure are constantly undergoing significant alterations and that they are essentially conventions held by both philosophers and scientists alike, with the former, hopefully, subjecting them to continuous critical scrutiny. Rules - and arguments in their defense - of scientific procedure are, to be sure, neither eternal verities or a bounteous afflatus.

(37) F. Kaufmann, "The Significance of Methodology for the Social Sciences (Part II)," Social Research, vol. 6, 1939, p. 538.

(38) F. Kaufmann, "The Significance of Methodology for the Social Sciences (Part II)," Social Research, vol. 6, 1939, pp. 539-540.


(42) Talcott Parsons, "The Point of View of the Author," In M. Black, 1961, pp. 316-317.

(43) In this parenthesis, Parsons is referring to E.C. Devereux's essay on "Parsons Sociological Theory" which is the first discussion in M. Black's volume on Parsons' theoretical contributions in sociology.
(1) I may add here, somewhat haltingly, that it is embarrassing to remark on this matter since it is such an elementary consideration in philosophical and, to be sure, scientific discussions. But I am convinced of the necessity of reiterating it in this context because many sociologists - and I believe Talcott Parsons is one of them - still persist in thinking that the origins of a theory constitute a significant factor in its assessment. In regard to this latter point, cf. Bernard Suits Review of The Social Theories of Talcott Parsons, edited by M. Black Philosophy of Science, vol. 31, 1964, pp. 194-195.

It would be well to note several penetrating statements in this particular connection advanced by Felix Kaufmann who observed that the logical disjunction between the analysis of a theory or other similar constructions in philosophy and science are surprisingly often glossed over and confused. In his article, "The Significance of Methodology For the Social Sciences (Part II)" Social Research, vol. 6, 1939, p. 548, Kaufmann asserts that:

"Methodology does not inquire how men really think but asks what sort of thought complies with intersubjectivity accepted rules. To be sure, these rules are arrived at by analysis of actual thinking but the definition of "correct thought" implies no reference to this genesis."

In similar tone and substance, Kaufmann puts forward the following reflections in his later published book, Methodology of the Social Sciences, 1964.

"Failure to realize this leads to the genetic fallacy, which consists in confounding analysis of meanings with casual explanation of facts, a fallacy that has time and again blocked the path to a thorough understanding of logical and philosophical problems." (p. 16)
"Unfortunately, the issue has almost always been obscured by the failure to distinguish between logical and genetic priority. The philosophical problem is not to give a causal explanation of the appearance of ideas in the soul (mind), but to clarify the presuppositions implicit in thought and to arrange them in their proper logical order." (pp. 18-19)

"Here it is particularly important to separate logical analysis of rules of procedure from psychological description of the cognitive process. From the fact that understanding often occurs with lightning rapidity and already at early stages of mental development we must not conclude that it possesses a simple logical structure. Failure to discriminate between genetic and logical priority has obscured discussions concerning the nature of understanding, particularly the issue whether, in understanding the behavior of our fellow men, we draw analogies with our self-observations. The question essential for the logical analysis of understanding is not 'How does the belief arise that certain fellow men find themselves in a particular psychical state or perform particular psychical acts at a given time?' It is rather 'What are the criteria for a warranted belief of this kind?' And this question implicitly refers to given rules of scientific procedure." (pp. 156-157)

(2) In addition to the widely dispersed allusions to Parsons' early intellectual development in his voluminous writings over the years, my principal sources for this intellectual biography, as it were, were: Talcott Parsons, "A Short Account of My Intellectual Development," (Recorded and edited by H. V. Ball) Alpha Kappa Delta, vol. 29, no. 1, Winter, 1959, pp. 1-12; Talcott Parsons, "Some Comments on the State of the General Theory and Action," American Sociological Review, vol. 18, no. 6, December, 1953, pp. 618-631; Talcott Parsons', "The Point of View of the Author," In M. Black, 1961, pp. 311-363, esp. pp. 315-320.

E.C. Devereux's section on the early exposures and influences on Parsons' career in his essay, "Parsons' Sociological Theory," In M. Black (1964), pp. 3-7 was also helpful. N.S. Timasheff's very brief sketch on p. 242 of his Sociological Theory: Its Nature and Growth, (1957) proved to be of some value in regard to dates and what I thought general suggestions for this chapter.

What I affirm here does not offer anything strikingly novel in regard to Parsons' past intellectual life but its major merit,
I hope, lies in the perspective that it provides from which his copious sociological contributions can be tied to his numerous early contacts with and exposures to the social scientific literature and certain prominent academicians who were either his teachers or collaborators.


(4) In Parsons' view, his own maverick qualities were given shape and fostered during his undergraduate days at Amherst College.

"I might say at the outset, I have always been pretty much a kind of maverick from the point of view of disciplinary identification. I think there are some who have wondered whether I was really a sociologist or not. But my maverick status goes way back, because as an undergraduate I began at Amherst College, which had no sociology or anthropology..." (p. 3)

When queried of the effects of bureaucratization on independent thinkers, viz., mavericks, and the danger of the elimination of mavericks as a consequence, Parsons' response was indeed curious and revealing, not because of the analysis he furnished in regard to the question, but what he said of himself.

"I don't know. That is a very difficult question. I, myself, of course, belong in the maverick family, and some people sometimes say to me, 'Oh, you started as an economist'. But as you now know, that is not strictly true. I never had more than one foot in economics...." (p. 6)

These two quotations are extracted from Parsons' account of his career in the Alpha Kappa Delta, vol. 29, no. 1, Winter, 1959.

Apparently, Parsons takes great relish in his disciplinary marginality which in itself is not objectionable.


(6) Ibid., p. 7.
(7) Ibid., p. 8.


(9) For a sample of some of Hobhouse's sociological work with a number of expositional essays on various topics to which he directed his analytical attentions, see J.A. Hobson and M. Ginsberg, T. Hobhouse: His Life and Work (With Selected Essays and Articles) London, George Allen and Unwin, 1931; see also a satisfactory adumbration of Hobhouse's thought in H. Becker and H.E. Barnes, Social Thought from Lore to Science, vol. III, 1961, pp. 806-809.

(10) The gist of Ginsberg's sociological deliberations are to be found in a number of volumes containing compilations of previously published essays and works. See, M. Ginsberg, On the Diversity of Morals, vols. I and II, London, Methuen and Co. 1932. I should think that the latter volume would include a representative offering of what Parsons may have intellectually encountered when he studied under Ginsberg at the London School of Economics.


(15) Talcott Parsons, "The Point of View of the Author," In M. Black, 1961, p. 316. 


(17) Ibid., p. 6. 

(18) It must be admitted that at this point I embark upon a largely interpretational discussion for the next several paragraphs on the extent to which and in which particular manner Parsons considered Marshall's theoretical thought to be of such noteworthy significance. For the development of my argument I have utilized a number of Parsons' published comments in this regard. Since, as Parsons himself notes, the chapter on Alfred Marshall in The Structure of Social Action is in the main composed of two earlier published journal articles on Marshall's theoretical thought, I have decided to omit mention of them and simplify my acknowledgments by referring to the chapter on Marshall in The Structure of Social Action (See p. 129 of The Structure of Social Action).

(19) To say the least, Parsons' embrace of Marshall's theoretical thought is based on a comparatively extensive and sophisticated knowledge of the major economic theories that were propounded in the nineteenth century combined with an equal immersion of sociological thought of the late nineteenth century. Without comprehending the dominant yet coterminous lines of argument of these various economic theories, it would be rather difficult to grasp the interestingly insightful reasons for Parsons' concern with Marshall's theoretical formulations.

(21) Ibid., pp. 6-7. It should be noted, however, that Parsons wrote in 1936 a generally favorable, expository essay on Taussig's own theoretical formulations in economics. What is indeed interesting about Parsons' analysis is that it concentrates on what Parsons considers to be the sociological facet, i.e., the norms, values, and institutional factors. See Talcott Parsons, "On Certain Sociological Elements in Professor Taussig's Thought," In J. Viner, editor, *Explorations in Economics: Notes and Essays Contributed In Honor of F.M. Taussig*, New York, McGraw-Hill, 1936, pp. 359-379.

(22) In delineating the approach and rationale for his volume, *The Structure of Social Action*, Parsons amplifies on the elements in Marshall's theoretical thought which captured his attentions.

"The real starting point was this sort of an idea. You looked at Marshall; you looked at Weber -- at Pareto, at Durkheim. You saw they were divergent in so many respects, but you saw that you could also look at them with respect to a certain central set of interests. I said to myself that if you take this as a key then an economist such as Marshall must have an implicit sociology in his work. So I set myself the job of working through Marshall with a view to dissecting this out. It was that study which was published independently in two papers in the *Quarterly Journal of Economics*, but the main part of it was also incorporated into the book; *The Structure of Social Action* and constituted the real jumping-off place of the book. In other words, I tried to discriminate what was the core economic theory of Marshall from his views on a lot of things - population, social evolution, class structure, and many other things that could properly be called sociology - and then to build the cognate ideas of these other people into the same single framework."

This quotation is taken from p. 8 of Talcott Parsons' article, "A Short Account of My Intellectual Development," (Recorded and edited by H.V. Ball) *Alpha Kappa Delta* vol. 29, no. 1, Winter, 1959, p. 8.

(24) Cf. ibid., pp. 174-177 and p. 703.


(27) This is abundantly made clear when Parsons in the preface of The Social System, 1964, p. vii, remarks that

"This book [The Social System] therefore is an attempt to carry out Pareto's intention, using an approach, the "structural-functional" level of analysis, which is quite different from that of Pareto, and, of course, taking advantage of the very considerable advances in our knowledge at many points, which have accumulated in the generation since Pareto wrote."

Cf. also Talcott Parsons, "An Outline of The Social System," In Talcott Parsons et al., 1965, p. 37. Here, Parsons refers to Pareto's notion of "equilibrium" in a social system which still remains a cardinal Parsonian concern in sociological theory.


All these references were taken from a scattered number of Parsons' essays and books which were published within the period of the last forty years.
(29) E.C. Devereux, "Parsons' Sociological Theory," In M. Black, 1961, p. 5.

(30) As concerns the notion of "system" in scientific, Parsons, in the preface of his volume, The Social System, states this:

"The title, The Social System, goes back, more than to any other source, to the insistence of the late Professor L.J. Henderson on the extreme importance of the concept of system in scientific theory, and his clear realization that the attempt to delineate the social system as a system was the most important contribution of Pareto's great work."


Regarding Parsons' explicit acknowledgments of L.J. Henderson's works in the predominantly philosophical area of the nature of scientific theory and facts, they can be found in Talcott Parsons, The Structure of Social Action, 1964, p. 28 and pp. 41-42.


(32) Ibid., p. 78.

(33) Ibid., p. 8.


(36) Ibid., pp. 243-244.

It may be noted, in addition, that the bulk of Parsons' publications from 1933 to 1937, the year when his The Structure of Social Action was published, dealt with problems of a similar theme.


Interestingly, Parsons incorporates one of Kohler's central contentions dealing with the problem of "Gestalt" (the integrated unity of a whole comprised of countless disparate elements) into his own arguments on the notion of "system" in scientific theory. See in this connection Talcott Parsons, The Structure of Social Action, 1964, p. 482.

Parsons points to the similarity obtaining between his concept of "cognitive orientation" and Talman's notion of "cognitive mapping" which the latter proposed in his Purposive Behavior in Animals and Men. In this regard, refer to Talcott Parsons, The Social System, 1964, p. 7.

Furthermore, Parsons expresses his debt to Tolman in regard to his elaborate formulation of "the components of the frame of reference of the theory of action". Here, it seems that Parsons relied to a considerable degree on Tolman's conceptions of the processes of orientation and cognition; for this see, Talcott Parsons and Edward A Shils, "Values, Motives and Systems of Action," In Talcott Parsons and Edward A. Shils, 1962, p. 64 and p. 234.

Even as recently as 1961 in his "An Outline of The Social System," In Talcott Parsons et al., 1965, p. 37, Parsons reaffirms his debt to Cannon for his concept of "homeostasis" which apparently assisted Parsons in formulating his argument concerning the basic equilibrium of all social systems.


Ibid., p. 9.


Inasmuch as I am familiar with Parsons' total published work, the first discussion with thoroughgoing Freudian overtones appeared in his paper, "Psychoanalysis and the Social Structure," *The Psychoanalytic Quarterly*, vol. 19, July, 1950, pp. 371-384, later republished in his volume of collected papers, *Essays in Sociological Theory*, 1964, pp. 336-347. Since that juncture to the present, the distinctive Freudian element has permeated to a notable degree his theoretical formulations, particularly in discussions focusing on socialization.

For other observations on Parsons' adoption of Freudian lines of argument, etc., see C.N. Apostle, "Parsonian Sociology," *Sociology and Social Research*, vol. 51, no. 3, April, 1967, pp. 276-277.

Cf. these remarks with those of E.C. Devereux's "Parsons' Sociological Theory," In M. Black, 1964, p. 7.

"If it is true that one absorbs a part of all that he has met, we should not be surprised to find that various strands of Parsonian theory reflect and incorporate elements of biology and medicine, of economics, especially of institutional economics, and of the utilitarian tradition from which it emerged, of German formal sociology, with its propensities for ponderous
systematic analysis, together with its traditions of idealism and Verstehen, of structural-functional analysis as developed by Durkheim and the anthropologists, and of Gestalt and Freudian psychology. But Parsonian theory is not simply an eclectic amalgam of elements drawn from these sources. The main point is how its author responded to these influences and constructed from them a single systematic theory which is uniquely Parsonian."
CHAPTER THREE

THE LOGICAL STRUCTURES OF SCIENTIFIC THEORY AND EXPLANATION.

(1) My locution "general statements" for all intents and purposes is equivalent in meaning and use to locutions such as "general propositions", "explanatory propositions", "universal statements", "universal laws", "general empirical laws", "empirical regularities", or "empirical uniformities". It may be mentioned in passing that in the majority of discussions on scientific theory and explanation, the typical choice of terms is either "universal statements" or "universal laws. The reasons for my decision to employ, instead, the phrase "general statements" is as follows. To begin with, constructions called well-confirmed universal statements or laws currently in use in the social sciences are exceedingly small in number, if not altogether absent. Thus, since practically all my comments here are directed in one way or another to the social sciences, I opined that it would have been rather idle to speak in a fashion which implied that the social sciences are employing theoretically significant universal statements or laws. Quite clearly, for there to be any scientifically worthwhile universal statements or laws requires sufficiently highly abstract theories which, as I view it, the current social sciences do not in any way evidence. Apropos of this, the phrase "general statements, in my view, points appositely to the few attempts at the development of a scientific theory in the social sciences in which the scope is limited; it employs modest statements of general empirical regularities such that they could not be considered, properly speaking, as universal laws. Hence, my inclination to speak of general statements. It should be clear that in the contexts of theories and explanations, the logical roles of "general statements" are identical to those of "universal statements", "laws", etc. (all the terms mentioned above). Au fond, the difference is semantical, but the implications proceeding from these locutions are, I think, notably important.


(3) It may be observed in this connection that G.C. Homans robustly argues in several of his recent publications that a theory of a phenomenon is an explanation of the phenomenon; note: "A theory is nothing - it is not a theory - unless it is an explanation." I should think that this is an overstatement, if not fallacious. As I shall attempt to argue, explanation is one of the processes, among others, a theory by its structure facilitates. Broadly, a theory is composed of general statements (or laws) which means it does not exclusively refer to any one particular phenomenon; it does not contain specific statements about a concrete empirical phenomenon as an explanation which is a blend of general statements from a theory and several indispensable statements describing aspects of an empirical phenomenon. It may be mentioned, moreover, that an explanation need not necessarily depend on a theory for its general statement (or law); only a singular general statement may suffice to provide an explanation of a specific empirical event, though I must add this type of procedure is usually suspect because of many potential logical defects in the assumptions that are initially made and in the implications that follow therefrom.

For Homans' remarks, see his "Bringing Men Back In," American Sociological Review, vol. 29, no. 5, December, 1964, pp. 811-812. The quotation I have used in the above remarks is found on p. 812 of Homans' article.

Hans L. Zetterberg adopts a position very much similar to that of Homans. Apparently, a theory is tantamount to an explanation. "To ask for an explanation in science is thus to ask for a theory. A scientific theory, then, is a sword that cuts two ways. On the one hand, it is a system of information-packed descriptions of what we know; on the other hand, it is a system of general explanations." See H.L. Zetterberg, On Theory and Verification in Sociology, 1964, p. 11.

Even as far back as 1939, Sigmund Koch in a thesis written under the supervision of the eminent philosopher of science, Herbert Feigl, maintained that explanation equals
theory. He states: "The simplest type of explanation may be called the level of empirical law; the successively more complex levels might be called first order theory (Theory1), second order theory (Theory2) and so on. It will be noted that this account of explanation equates scientific explanation with scientific theory." See S. Koch, "The Logical Character of the Motivation Concept," Psychological Review, vol. 48, January, 1941, p. 25.


In passing it may be remarked that Bernard's book written in 1865 affords one of the most lucid and penetrating discussions on scientific theory that has yet been published. Despite their vintage, Bernard's arguments are pertinent at virtually every point to contemporary discussions and problems in the philosophy of science. It truly deserves to be regarded as a classic in its field. Indeed it is unfortunate that only individuals of wide and enduring reputation in both philosophy and science attract large audiences - hence, their works are accorded much consideration - while an unknown, like Bernard, who possessed brilliant, yet tenable, ideas is almost entirely ignored.

(6) A.N. Whitehead, Adventures of Ideas, 1933, p. 284.


(9) This is an impression I have developed after numerous conversations with individuals directly involved in the social and natural sciences and a substantial survey of the social scientific literature. Occasionally, I have found that those in the natural sciences advocate views closely akin to an inductivist's standpoint. It is in the light of these findings, informal though they may be, which reinforces my conviction that discussions of the type that I am attempting to conduct here are vitally important and do not constitute a tedious rehearsal of the commonplace if such a thing exists. Of course, it serves little purpose to preach to the converted, as it were, unless they have prodigious gaping lacunae in their knowledge. But, as I have perhaps implied, I am not at all convinced that knowledge of the assumptions and procedures of scientific enterprise is satisfactorily comprehended by an appreciable number of theorists and researchers in the social sciences.

Cf. K.R. Popper's observations as regards the widespread misconception of scientific method which leads to the uncritical acceptance of induction as a scientific procedure in his Conjectures and Refutations, 1965, Chapter One, "Science: Conjectures and Refutations," p. 46.

(10) Cf. K.R. Popper's views in this connection. "...our ordinary language is full of theories; that observation is always observation in the light of theories; and that it is only the inductivist prejudice which leads people to think that there could be a phenomenal language, free of theories and distinguishable from a "theoretical language"..." (emphasis in original).

This quotation is taken from a lengthy explanatory footnote on p. 59 in Popper's The Logic of Scientific Discovery, 1961. On p. 423 in the same book, he further adds: "There is no sharp dividing line between an 'empirical language' and a 'theoretical language': we are theorizing all the time, even when we make the most trivial singular statement." (emphasis in original).

Even Talcott Parsons, interestingly enough, points out that the knowledge of everyday life is replete with tacit assumptions or theories. In his, The Structure of Social Action, 1964, p. 10, Parsons quite properly observes:
"The implications of these considerations that theoretical arguments are predicated on a number of unstated assumptions/ justify the statement that all empirically verifiable knowledge - even the common-sense knowledge of everyday life - involves implicitly, if not explicitly, systematic theory in this sense. The importance of this statement lies in the fact that certain persons who write on social subjects vehemently deny it. They say they state merely facts and let them "speak for themselves". But the fact that a person denies that he is theorizing is no reason for taking him at his word and failing to investigate what implicit theory is involved in his statements."

Also, cf. ibid., p. 28.


"It must be borne in mind that all thought implies a selection of data. Selective processes take their direction more or less consciously from certain postulates/ general statements or laws/ with which the selected data should comply. Such postulates are: intertemporal and intersubjective invariance; and maximum implicational value. The better the data satisfy these postulates, the more essential they are."

(11) Admittedly, this is a psychological statement. Appropriate scientific arguments in its favor, are advanced by K.R. Popper in Conjectures and Refutations, Chapter One, "Science: Conjectures and Refutations," 1965, pp. 42-45. Also, compare my observations with those of Popper's when he contemplates that the rules of logic "are laws of certain descriptive languages - of the use of words and especially of sentences", on p. 207 (this quotation) and also on p. 208, Chapter Nine, "Calculi of Logic and Arithmetic" in Conjectures and Refutations, 1965. It is worth noting here that a theory is basically a logical device, that is, it is an arrangement of statements (or laws) which permits a host of logical deductions to be performed.


(13) For other pertinent comments on simplicity or parsimony in scientific theories refer to the following works: R. Abel, "Pragmatism and the Outlook of Science", Philosophy and Phenomenological Research, vol. 27, no. 1, September, 1966, pp. 48-49;
It is to be noted that K.R. Popper, who does not countenance the notion that the criterion of simplicity is mere convention as I style it, maintains that the concept of simplicity in scientific theory is equivalent to the degree of falsifiability of the theory in question. In other words, the theory that can be swiftly falsified (or is highly testable) is the simplest. In this manner, Popper affirms, epistemological questions associated with the notion of simplicity can be effectively met. This, I would argue in turn, is still convention though put in a different form. Here, falsifiability is believed to be an indication of simplicity. The ultimate question, I think, is merely shunted further back.

For a theory to be highly testable in the first place, it must have general statements which are devoid of superfluities; futile in their implications; and far-reaching in scope - all the characteristics of a parsimonious theoretical statement. Thus, we are still in the ambit of convention whatever the ploy, unless we maintain that there is a one-to-one correspondence between what the theory asserts and the world. But this, as we have noted, is epistemologically preposterous.

(14) E. Nagel, *The Structure of Science*, 1961, p. 190. I notice that R. Abel also employs this quotation in his recent provocative journal article entitled, "Pragmatism and the Outlook of Modern Science", *Philosophy and Phenomenological Research*, vol. 27, no. 1, September, 1966, p. 49. Coincidence rather than conscious, but unacknowledged borrowing on my part accounts for this selection. It was not until after I had prepared this chapter in conspectus form that I encountered Abel's article which, if I may say, attracted my immediate interest because of the similarity of views I shared with it.

(15) Rather piquant but relevant remarks in this connection are furnished by R. Abel and K.R.Popper in the following accounts. Abel notes:

"Moreover, every scientific theory represents a compromise; no one theory is ever in quite complete accord with all the data."


Cf. also the excellent and relatively thorough discussion on the notion of simplicity in scientific work by H.R. Post, "Simplicity in Scientific Theories," *British Journal for the Philosophy of Science*, vol. 11, no. 41, May, 1960, pp. 32-41, esp. p. 40 where he discusses "pragmatic simplicity", a notion similar to the one I have suggested in my argument.
In a rather picturesque fashion, Popper views the issue as follows:

"The empirical basis of objective science has thus nothing 'absolute' about it. Science does not rest upon rock-bottom. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or 'given' base; and when we cease our attempts to drive our piles into a deeper layer, it is not because we have reached firm ground. We simply stop when we are satisfied that they are firm enough to carry the structure, at least for the time being."


In another passage in the same book, Popper emphasizes in a more technical fashion the perpetual transience of scientific theory:

"The old scientific ideal of episteme - of absolutely certain, demonstrable knowledge - has proved to be an idol. The demand for scientific objectivity makes it inevitable that every scientific statement must remain tentative for ever. It may indeed be corroborated, but every corroboration is relative to other statements which, again, are tentative. Only in our subjective experiences of conviction, in our subjective faith, can we be 'absolutely certain'." The Logic of Scientific Discovery, 1961, p. 280.

It may also be noted that Popper expressly rejects the suggestion that scientific theory is an instrumental device (a pragmatic construction, as it were) as I have proposed here. Moreover, he spurns, though with certain reservations, the suggestion that scientific theory discloses anything "true" or "false" about the world. For Popper, theory is adjudged successful in terms of its confirmability or ability to be corroborated by various measures, "or by other measures designed to investigate empirical reality". By this he means the following:

This certainly does not mean that we are forbidden to use the concepts 'true' and 'false', or that their use creates any particular difficulty. The very fact that we can avoid them shows that they cannot give rise to any new fundamental problem. The use of the concepts 'true' and 'false' is quite analogous to the use of such concepts as 'tautology', 'contradiction', 'conjunction', 'implication' and others of the kind. These are non-empirical concepts, logical concepts.
They describe or appraise a statement irrespective of any changes in the empirical world. Whilst we assume that the properties of physical objects (of 'genidentical' objects in Lewin's sense) change with the passage of time, we decide to use these logical predicates in such a way that the logical properties of statements become timeless: if a statement is a tautology, then it is a tautology once and for all. This same timelessness we also attach to the concepts 'true' and 'false', in agreement with common usage. It is not common usage to say of a statement that it was perfectly true yesterday but has become false today. If yesterday we appraised a statement as true which today we appraise as false, then we implicitly assert today that we were mistaken yesterday; that the statement was false even yesterday—timelessly false—but that we erroneously 'took it for true'.

Here one can see very clearly the difference between truth and corroboration. The appraisal of a statement as corroborated or as not corroborated is also a logical appraisal and therefore also timeless; for it asserts that a certain logical relation holds between a theoretical system and some system of accepted basic statements. But we can never simply say of a statement that it is as such, or in itself, 'corroborated' (in the way in which we may say that it is 'true'). We can only say that it is corroborated with respect to some system of basic statements -- a system accepted up to a particular point in time. 'The corroboration which a theory has received up to yesterday' is logically not identical with 'the corroboration which a theory has received up to today'. Thus we must attach a subscript, as it were, to every appraisal of corroboration -- a subscript characterizing the system of basic statements to which the corroboration relates (for example, by the date of its acceptance).

Corroboration is therefore not a 'truth value'; that is, it cannot be placed on a par with the concepts 'true' and 'false' (which are free from temporal subscripts); for to one and the same statement there may be any number of different corroboration values, of which indeed all can be 'correct' or 'true' at the same time. For they are values which are logically derivable from the theory and the various sets of basic statement accepted at various times.

This is quoted from K.R. Popper, The Logic of Scientific Discovery, 1961, pp. 274-275. In regard to his further comments on this matter, see the section from p. 274 to p. 276.

In this connection, it is interesting to notice that Ernest Nagel maintains a similar position. See E. Nagel, *The Structure of Science*, 1961, pp. 129-140 on the instrumentalist view of theories and pp. 141-152 on the realist view of theories.

In a certain restricted sense, Popper and Nagel are correct in their arguments. But in another direction their arguments, in my view, begin to founder. In science, the abandonment of hypotheses are not spontaneous undertakings even though the empirical evidence may, on first blush, warrant it. There are always considerations of an extra-logical character to be given some due. For example, a theory may be falsified in several important areas but in the balance it appears to withstand other attempts at confutation in a fairly consistent manner; the question is this: Will certain falsifications of a theory's implications overshadow the success of the theory to account for a wide constellation of other empirical phenomena? The answer depends upon what one's theoretical purposes are and these in no wise are subject to the canons of logical discourse, unless outlandish requirements, which exceed the limitations of theoretical endeavor, are set. Doubtless, there are numerous other considerations and problems of a similar character that must be handled in this judgmental fashion. Thus, in view of these remarks I deem the notion of "pragmatic" (taken in this context) to be an adequate description of the procedures and enterprise of scientific theory. I am emphatically not referring to the vulgar species of pragmatism which construes any endeavor to be useful if it satisfies a personal whim or fancy, a conception of thought which virtually throws overboard all vestiges of rational discourse which is indispensable to scientific deliberations.


(17) The curious relevance of pragmatism, a philosophical orientation, to the general approach of scientific approach is concisely and cogently outlined by R. Abel in his journal article which was mentioned here on several previous occasions, namely, "Pragmatism and the Outlook of Modern Science," *Philosophy and Phenomenological Research*, vol. 27, no. 1, September, 1966, pp. 45-54.


S. Toulmin appositely describes the procedures of induction as "'mere bug-hunting' - a matter of collection, rather than insight". In S. Toumin, The Philosophy of Science, 1960, p. 54.


Since theory, as I shall later argue, constitutes the foundation of explanation by furnishing general statements which transcend empirical phenomena to a marked extent, I am completely in accord with F.A. Hayek's epigrammatic consideration in this connection:

"...science does not explain the unknown by the known... but on the contrary, the known by the unknown."

See F.A. Hayek, "Degrees of Explanation." British Journal for the Philosophy of Science, vol. 6, 1955-1956, p. 211. It may be noted that it was K.R. Popper who originally suggested this deceptively simple, yet pregnant, statement. Refer to his Conjectures and Refutations, Chapter Six, "A Note on Berkeley as Precursor of Mach and Einstein," pp. 166-174, particularly to p. 174 where he says "...in science we always try to explain the known by the unknown, the observed (and observable) by the unobserved (and perhaps, unobservable)." (emphasis in the original). Or, see K.R. Popper, "The Aim of Science," Ratio, vol. 1, no. 1, December, 1957, p. 24.


See, also, E. Nagel, The Structure of Science, 1961 p. 132.
See the particularly perceptive discussion on these methods I have enumerated and also on the fragility of operationism as a method of ascribing empirical meaning to theoretical terms in scientific enterprise in R.A. Plutchik, "Operationism as Methodology," Behavioral Science, vol. 8, 1963, pp. 234-241, esp. p. 240.


A fair portion of what I have said here is predicated on Popper's discussions on pp. 112-135, pp. 136-142, and pp. 276-281 of The Logic of Scientific Discovery, 1961.

This is suggested by C.G. Hempel in Fundamentals of Concept Formation in Empirical Science, 1952, p. 33. Concerning the crucial role of deduction in scientific theory, there is an interesting, yet pertinent, argument in F.S.C. Northrop's The Logic of the Sciences and the Humanities, 1960, pp. 59-75, see esp. p. 61.
Presumably, under the aegis of Herbert Feigl, S. Koch points out the resemblance of the logical structure of an explanation to a syllogism in Barbara: See S. Koch, "The Logical Character of the Motivation Concept." Psychological Review, vol. 48, January 1941, pp. 24-25.

A simple example of a syllogism in Barbara would be a syllogism in the first figure as follows:

All men are mortal ............... M P
Socrates is a man ................. S M
Therefore Socrates is mortal ..... S P


"Classification is not sufficient for scientific development; but it is assuredly necessary — logically as well as historically. The history of biology bears out this contention most unequivocally."

Except for the last sentence and the latter part of the first sentence, I concur most heartily with Naegle's contention.


I borrow in whole this verbal model of a general law from C.G. Hempel, Philosophy of Natural Science, 1966, p. 55.


(34) C.G. Hempel, *Philosophy of Natural Science*, 1961, p. 56. Refer to the books and articles listed in note no. 11 for further discussions on subjunctive conditionals.


(40) Refer to note 3 of this chapter.

(41) Refer to note 3 of this chapter.

(42) This tripartite reduction of the structure of a scientific explanation is particularly found in the works of R. Brown, F. Hayek, F. Kaufmann, L. Gross, C. Schrag, J.H. Yolton, and, above all, C. Hempel and K.R. Popper in the books and articles referred to in note 38 of this chapter.

(43) As I indicated in note 1 to this chapter, "empirical uniformities" is equivalent to "general statements", "general propositions", "explanatory propositions", "universal statements", "universal laws", "general empirical laws."

(44) Unquestionably, I am employing here C.G. Hempel's terminology - "explanans" and "explanandum" - for precisely the same reasons he does. That is to say, the terms, "explicans"
and "explicandum" which several writers in the philosophy of science use in their discussions on topics of scientific import are exclusively reserved for philosophical purposes, particularly in "explication" in the Carnapian sense. It is noteworthy that K.R. Popper advances a similar dichotomization of the structure of scientific explanation, though he employs "explicans" and "explicandum" as his terminology; this discussion, incidentally, is found in his article, "The Aim of Science," Ratio, vol. 1, no. 1, December, 1959, pp. 24-25. Quite clearly, the choice of one set of terms over another is purely for semantical convenience and really for nothing more.

(45) C.G. Hempel, Aspects of Scientific Explanation, Chapter Twelve, "Aspects of Scientific Explanation," pp. 335-347. In passing, it is worthwhile to observe that W. Dray styles Hempel's notion of a deductive-nomological explanation as an explanation by a covering law model. For Hempel, it should be mentioned if it were not already, the general law (or general statement) in his conception of explanation is plainly labeled a deductive-nomological law. See Dray's lengthy discussion of the covering law model in his Laws and Explanation in History, 1957, pp. 1-21.


It may be worthwhile to mention at this juncture that most explanations which are advanced in the social sciences and in commonsense arguments should be termed "explanation sketches", a notion suggested by C.G. Hempel. In brief, an explanation sketch pertains to the attempt to explain a given specific event in such a manner that the multifarious background suppositions (or laws if we wish to be precise) are left unstated and the essential antecedent boundary conditions are taken for granted. In a word, the explanation is constructed in an elliptical fashion. It is then somewhat vague, yet it points in a general direction where an adequate explanation may be found. Oftentimes, by further unravelment of the implications and the components (concepts, statements of relationships, etc.) of these elliptical arguments, clearly stated assumptions and conditions can be acquired and properly assessed in terms of the logic of explanation, and if it passes muster in this regard, it can be evaluated in terms of the empirical evidence. Cf. C.G. Hempel, Aspects of Scientific Explanation, 1965, Chapter Twelve, "Aspects of Scientific Explanation," pp. 423-424.
The essential elements of this paradigm are to be found in Hempel's discussion on "The Logic of Functional Analysis," p. 299, Chapter Ten in his volume *Aspects of Scientific Explanation*, 1965. In his other studies, Hempel reverses the sequence of the "L" factor (Laws) and the "C" factor (Conditions) for perhaps a much more accurate graphic representation of scientific explanation. But this is of little moment as regards the nub of Hempel's argument.


Of worthy note in this regard are Popper's pointed remarks concerning causal explanation in his discussions on history in his *The Open Society and its Enemies*, vol. Two, New York, Harper Torchbook, 1963, p. 262, which echo those of Kaufmann's

"...we can never speak of cause and effect in an absolute way, but that an event is a cause of another event, which is its effects, relative to some universal law. However, these universal laws are very often so trivial...that as a rule we take them for granted, instead of making conscious use of them."

The latter consideration, to be sure, applies in essential respects to a law which subsumes specific events by virtue of a non-causal empirical uniformity.


Postdiction, the explanation of an event that has already occurred, may be used synonymously with the term "retrodiction" which has received some currency by several scientists, historians, and philosophers of science. Hempel employs the term "retrodiction" in his studies on explanation: *Philosophy of Natural Science*, 1966, p. 73.


(54) Viewpoints maintaining that prediction and explanation are altogether disparate operations are strongly voiced by N. Rescher and F. Scheffler. So far as I understand their basic arguments, I am convinced that they both equate prediction with probabilistic explanation - which I have earlier mentioned is another distinctive operation - in a puzzling fashion. By doing so, they blatantly miss the mark of the arguments and rationales mobilized by those who deem prediction and explanation to be structurally identical. In this respect, see N. Rescher, "On Prediction and Explanation," *British Journal for the Philosophy of Science*, vol. 8, 1958, pp. 281-290 and F. Scheffler, "Explanation, Prediction, and Abstraction," *British Journal for the Philosophy of Science*, vol. 7, 1957, pp. 293-309.


"Regardless of their content or reliability, however, descriptions are never sufficient for explanation or prediction because they make no claims beyond the information given."


(1) Parsons' comments in this connection are most instructive:

In conclusion I should like to say a brief word about science and philosophy. The task of sociology, as of the other social sciences, I consider to be strictly scientific— the attainment of systematic theoretical understanding of empirical fact. The failure of the positivistic schools of sociology to attain such a goal I do not attribute, as so many do, to the inherent impossibility of the goal, but rather to their own inadequate methods of approaching it. Their inadequacy consists essentially in trying to apply both modes of thought and substantive concepts developed in the study of and suited to one kind of empirical fact—mainly that of the physical world—to quite another, human action in society. It is surely not altogether heterodox to say that the basic concepts of a science should be developed in connection with a study of its own subject matter— not imported from other sciences.

Thus I hold the goal to be scientific. But I do not believe that there is in social or any other science a rigid line between science and philosophy. The positivists, while officially denying it, have most certainly made very far-reaching philosophical assumptions about social life, if not as to the "ultimate nature" of it, at least as to what was "ultimately" capable of scientific analysis. Thus for my scientific purposes it is essential to be a philosopher at least in the negative sense— it is necessary to uncover and criticize on philosophical grounds the assumptions which block the way to doing justice to the empirical facts as I see them. Beyond that it is necessary to be clear about the philosophical implications of one's own positive concepts. In this sense the role of philosophy in science is merely a consequence of the rationalism inherent in science. The concepts of science must
attempt to be consistent with each other and with the known facts of experience, whether in its own concrete field or not. In this sense science must be able to meet the criticisms directed against it from the people who attempt a rational apprehension of reality as a whole, the philosophers. But equally the philosophers must be, in ideal, ready and able to meet the criticisms directed at their views from any of the sciences.


(2) See the text for note 40 in Chapter One.

(3) Metatheory, as I said in Chapter One, will refer to the philosophy of scientific theory. For further comments on this, see p. 14 of Chapter One.

(4) Suffice it to say that I am presenting here an interpretation, a point of view the significance of which lies in the manner in which it binds together and illuminates various facets of a man's systematic thinking on a particular issue of importance to himself and to others. Consequently, what I shall say should not be construed as canonical pronouncements whatever the degree of audacity with which I may on occasions express my opinions. One should always distinguish the vigorous expression of an argument from hardness of position; the two do not necessarily dovetail.


(10) Ibid., p. 283.

(11) Ibid., p. 282.

(12) Ibid., p. 283.

(13) Ibid., p. 313.

(14) Ibid., p. 283.


(22) John Hospers, "What is Explanation?" In A. Flew, 1956, p. 113.


(28) Ibid., p. 660.

(29) Ibid., p. 660.

(30) Ibid., p. 661.
(31) Again, for a broad overview of empiricism, refer to note 17 for this chapter.

(32) Ibid., p. 661.


(34) Ibid., p. 661.

(35) Ibid., pp. 660-661, especially the latter.

(36) Ibid., p. 661. Take note of the following sentence: "In science the first criterion is the pragmatic one of success—in explanation."

(37) Parsons' approach toward "verification of scientific theory is, in essential respects, similar to the one adopted by an instrumentalist. See Section A of Chapter Three for more in this regard.


(40) Ibid., pp. 6-7.

(41) Ibid., p. 6.

(42) Ibid., p. 10 and p. 28.

(43) Ibid., p. 28.

(44) Ibid., pp. 9-10 and p. 754.

(45) Ibid., p. 28 and p. 41.
(46) Ibid., p. 41.

(47) Ibid., p. 7.

(48) Ibid., p. 10.

(49) Ibid., pp. 9-10.

(50) Ibid., p. 10.

(51) Ibid., p. 10.

(52) Ibid., p. 10.

(53) Ibid., p. 7.

(54) Ibid., p. 7.

(55) Ibid., p. 7.

(56) Ibid., p. 7.

(57) Ibid., p. 8.

(58) Ibid., p. 8.

(59) Ibid., p. 9.

(60) Ibid., p. 9.

(61) Ibid., p. 8.

(62) Ibid., p. 7.
(63) Ibid., p. 7 and p. 8.

(64) Ibid., p. 9.


(66) Ibid., p. 41.

(67) Ibid., p. 42.

(68) Ibid., p. 42.

(69) Ibid., p. 42.

(70) Ibid., pp. 27-28 and p. 34.

(71) Ibid., pp. 30-33.

(72) Ibid., p. 30.

(73) Ibid., p. 30.

(74) Ibid., p. 31 and pp. 33-34.

(75) Ibid., p. 33.

(76) Ibid., pp. 33-34.

(77) Ibid., p. 33.

(78) Ibid., p. 34. Note:
What should be insisted upon is the radical logical distinction between these two kinds of concepts, "type-parts" and "empirical generalization," and another kind which may, in a strict sense, be called "analytical" concepts. This kind of conceptualization really presupposes the first. For whatever concrete or hypothetically concrete units or parts a complex concrete phenomenon may be broken down into, once these units are established they will of logical necessity have general attributes or qualities.

(79) My main sources for this notion were *ibid.*, pp. 34-37 and pp. 748-753, especially p. 751.

(80) *Ibid.*, p. 750 and p. 35 (point number 2 of footnote number 1) and p. 36.


(82) Talcott Parsons, *The Structure of Social Action*, 1964, p. 34.


(94) Ibid., p. 15.

(95) Ibid., p. 19.

(96) Ibid., p. 18.

(97) Ibid., pp. 19-20.

(98) Apropos of Parsons' generally accurate remarks concerning the non-essential nature of measurement for scientific enterprise it is important to note that physics, chemistry and astronomy are not examples of applied mathematics. Instead, however, they are natural sciences, of course with abstract theories, in which mathematics are applied.

Cf. these considerations with K.R. Popper, Conjectures and Refutations, Chapter One, "Science: Conjectures and Refutations," 1965, p. 62:

...measurements presuppose theories. There is no measurement and no operation which can be described in non-theoretical terms. The attempts to do so are always circular: the description of the measurement of length needs a (rudimentary) theory of heat and light - measurement; but these, in turn, involve measurements of thought.
(99) Ibid., p. 20.


(101) Ibid., p. 42.


(104) See note 90 for this chapter.


(107) Ibid., p. 44.

(108) Ibid., p. 45.

(109) Ibid., p. 45.

(110) Ibid., p. 46.

(111) Ibid., p. 47.

(112) Ibid., p. 47.

(113) Ibid., p. 48.
(114) Ibid., p. 48.


(117) Ibid., p. 354.


(121) Ibid., p. 3.

(122) Ibid., p. 3.


(124) Ibid., p. 50.
(125) Ibid., p. 50.

(126) Ibid., p. 51.

(127) Cf. in this connection my arguments in section A of Chapter three.


(129) Ibid., p. 137.

(130) Ibid., p. 137.

It may be noted that in another article in the same year, Parsons views theory, not as a body of interrelated concepts, but one of interrelated general statements. To wit: "Theory is a body of interrelated generalized propositions about empirical phenomena within a frame of reference." This statement is taken from the "Introduction" to the section on "Culture and Social System (Part Four) in Theories of Society, 1965, p. 965.

(131) Ibid., p. 137.

(132) Ibid., p. 137.


(134) Ibid., p. 137.

(135) Ibid., p. 137.

CHAPTER FIVE

AN EXPOSITION AND CRITIQUE OF TALCOTT

PARSONS CONCEPTION OF SCIENTIFIC THEORY

AS EVIDENCED IN THE RECENT FORMULATIONS

OF HIS SUBSTANTIVE THEORY

(1) Talcott Parsons, The Structure of Social Action, 1964, p. 19, Cf. also pp. 51-60.

(2) Ibid., p. 35.

(3) Ibid., pp. 44-45.

(4) Ibid., p. 46.

(5) Ibid., p. 749.


(7) In this connection, refer to my discussion of Parsons' argument in this article in the introductory section of Chapter Four.


(12) Ibid., p. 495.


(15) Ibid., pp. 110-120.

(16) Ibid., p. 108.

(17) Ibid., p. 108 and pp. 198-204 and pp. 219-223.


For a comprehensive and lucid discussion of Parsons' views on socialization, see A.L. Baldwin, Theories of Child Development, 1967, pp. 539-575. Some attention should also be given to Urie Broffrenbrenner, "Parsons' Theory of Identification," In M. Black, 1964, pp. 191-213.


(32) Ibid., pp. 77. It is worth noting the manner in which Parsons has classified the pattern-variables in terms of dilemmas.

"I. The Gratification-Discipline Dilemma
Affectivity vs. Affective Neutrality
II. The Private vs. Collective Interest Dilemma
Self-Orientation vs. Collectivity
III. The Choice Between Types of Value-Orientation Standard
Universalism vs. Particularism
IV. The Choice Between "Modalities" of the Social Object
Achievement vs. Ascription
V. The Definition of Scope of Interest in the Object
Specificity vs. Diffuseness"

This list (and accompanying discussion) is found in Parsons' *The Social System*, 1964, p. 67 ff.


Perhaps one of the most intriguing applications of Parsons' "theory of action", particularly his pattern variable scheme, has been suggested by Edward Tiryakian. It is contended that Parsons' formulations are wholly relevant and supportive to the arguments of existentialist philosophers who assert that an individual must make his crucial choices within certain circumscribed situations.

Finally, there appears to be another area of sociological concern which could well profit from an interaction with existentialism. This is general sociological theory, particularly that part which has come to be known in sociological circles as the "theory of action," developed by Talcott Parsons and his associates. The theory of action is primarily interested in formulating a conceptual scheme at the highest level of generalization, concerning the orientation of the social actor to his situation. A significant convergence could be made between the theory of action and the existential perspective on the individual's orientation to the world. Thus, Jaspers' notion that the existing individual is always involved in a situation and the further notion that existence is always bounded by limit-situations might well be integrated in an action frame of reference as structural aspects of the actor's orientation to others. Similarly, the ego-alter relationship analyzed by Sartre has many aspects which could fruitfully be woven into action theory.

Moreover, the pattern variables which have been formulated by Parsons to involve both personalities and social systems may be seen in a new light not only as structural aspects of social action but also as fundamental existential choices which have to be made by an actor or a collectivity of actors -- they may even be reformulated as social boundary-situations. In this respect, one may point out an important convergence between sociological theory and existential thought in the latter's stressing choice and decision in human existence. The theory of action could well profit from existentialism by seeing to what extent this and other existential notions may be incorporated in its frame of reference, for example, Heidegger's notion of ambiguity as a fundamental existential situation in the everyday world of the One. Not only may ambiguity be viewed as a fundamental condition of the actor's situation, but also the pattern variables might be seen as an attempted solution to the problem of ambiguity by structuring the actor's situation. Of course, the existential notion of ambiguity could have a greater application than just action theory.

These quotations are taken from Edward Tiryakian, Sociologism and Existentialism: Two Perspectives on the Individual and Society, 1962, p. 166.


(40) Ibid., p. 67.

(41) My primary references are those found in note 33 of this chapter.

(42) Writing in the preface of the second edition (1949) of The Structure of Social Action, Parsons states his case for the necessity of introducing the functional element in the analysis of action.

A central problem, therefore, has been and is, how to bring theory of this sort closer to the possibilities of guiding of and testing and refinement by technical research, especially with the use of technically refined instruments of observation, and of the ordering and empirical analysis of observational data.

At least at many points, an important series of steps in this direction seems to be made possible by a shift in theoretical level from the analysis of the structure of social action as such to the structural-functional analysis
of social systems. These are, of course, "in the last analysis" systems of social action. But the structure of such systems is, in the newer version, treated not directly in action terms, but as "institutionalized patterns" close to a level of readily described and test empirical generalization. This, in turn, makes it possible to isolate specific and manageable action processes for intensive dynamic study. Such processes, that is, are treated as action in relation to institutionalized roles, in terms of balances of conformity with and deviation from the expectations of the socially sanctioned role definitions, of conflicting role expectations impinging on the individual, and the constellations of motivational forces and mechanisms involved in such balances and conflicts.

The isolation of such problems to the point of empirical manageability can, however, within the framework of a structural-functional system of theory, be achieved with a relatively high level of attainment of the advantages of generalized dynamic analysis. Treating dynamic problems in the context of their relation both to the structure of a system and the relation of the processes to the functional prerequisites of its maintenance, provides a frame of reference for judging the general significance of finding and for following out systematically its interconnections with other problems and facts.

This quotation is taken from pages D and E of the preface.


"This" unit-individualism -- unit rather than personal, for much of it concerns collectivities -- is not emancipation from all control through institutionalized norms. Rather, as Durkheim so clearly brought out, high levels of "responsible freedom" can be obtained only through positive institutionalization, through systems of norms and sanctions imposing the obligation of accepting responsibility and utilizing freedom over wide areas. It may thus be referred to as "institutionalized individualism."
(44) It is worth noting that Parsons and Smelser have undertaken such an analysis. They examined the economic system and explored its interrelations and interchanges with three subsystems which they viewed as the most important facets of the "external environment for the economic subsystem; the three subsystems were the family (the pattern-maintenance and tension management system), the polity (the goal attainment system and various components of the integrative system. All this is found in Talcott Parsons and Neil J. Smelser, *Economy and Society*, 1956.


(51) Ibid., p. 480.

(52) Ibid., p. 486.

(53) Ibid., p. 534.

(55) Ibid., p. 222.

(56) Ibid., p. 223.

(57) Ibid., p. 230-233.

(58) Ibid., p. 236.

(59) Ibid., p. 236-237.

(60) Ibid., pp. 237-238.

(61) Ibid., p. 230.

(62) See the text for note 40 to Chapter One.


(67) Hans Zetterberg relevantly notes in this regard when he categorizes Parsons' approach toward theory as that of a "dimensionist".

"The first camp, the dimensionists, define to which sociologists pay (or should pay) attention. Their
task is to develop an orderly schema of dimensions of anything social and to give these dimensions general labels (representative work: Parsons, *The Social System*). Thus when faced with almost any subject of research they can immediately identify its crucial aspects or variables. To "test" his theory, the dimensionist takes a fresh look at subject X and shows that the general terms defining his dimensions have identifiable counterparts in X... This is referred to as "deriving X (or "explaining" X) from the theory, a speech habit not often found outside the dimensionist camp. Dimensionism in modern sociology is dominated by Talcott Parsons. He specifies what aspects his campers shall seek, assigns names to these aspects, and stimulates his followers to discover their concrete manifestations in economy, polity, science, religion, education, and family life...."


(75) See the text for note 32 to this chapter.

(76) Cf. the following remarks by Marion J. Levy, Jr. in his recently published volume Modernization and the Structure of Societies: A Setting for International Affairs, vol. I,

"Parsons insists that the distinctions drawn are binary distinctions. Thus a relationship is either particularistic or universalistic. Empirically this is simply not the case. The concepts so defined must always be used in a manner other than their definitions would lead one to expect. In fact the uses of the distinctions by Parsons in his own works and in that of his followers are almost always differences of degree are in fact differences of kind and to try to handle consequent overstatements by ad hoc qualifications to the effect that of course other elements are involved to some degree is to beg the very question one intends to answer.

In this regard, see also M. Black, "Some Questions About Parsons' Theories," In M. Black, 1964, p. 286.


It should be noted that M. Black in his criticisms of the pattern-variables brings this consideration to the forefront.


(82) For perhaps one of the most perceptive arguments of the type I am presenting here, I would strongly suggest K. Rodner, "Logical Foundations of Social Change Theory," Sociology and Social Research, vol. 51, no. 3, April, 1967, pp. 287-301.
CHAPTER SIX

CONCLUSIONS

(1) By culturological, I refer only to general statements of relations embracing notions which are pertinent to the study of cultural phenomena. Since such studies are usually undertaken in anthropological circles, I interpolated the word "anthropology" in parentheses in order to forestall any confusion or misunderstanding that may occur in the usage of a somewhat pretentious term such as "culturological".


BIBLIOGRAPHY
Bibliography of Talcott Parsons' Works Used in this Study.

(PRIMARY SOURCES)

A.

To have included bibliographical references of the entire published works of Talcott Parsons in a study of such limited scope was clearly out of the question. Instead, what I have done is concentrated on his major theoretical works and a number of his journal articles of the past thirty-nine years in which direct and indirect references to the logical character of scientific theory and pertinent comments on his substantive theoretical formulations were presented.

If, however, a fairly complete bibliography of Talcott Parsons' published works is desired, I would strongly recommend recourse to the following publications.

Ogles, R.H. "A Complete Bibliography of Talcott Parsons and Selected Reviews and Critiques of His Work, Alpha Kappa Delta, vol. 29, no. 1, Winter, 1959, pp. 73-80. (This bibliography contains all publications from 1928 to 1958 inclusive.)


B.

In the following, I have endeavored to include the date and name of the publication in which the journal articles first appeared. Essentially the same procedure was undertaken for the books.


Parsons, T. The Structure of Social Action: A Study in Social Theory with Special Reference to a Group of Recent European Writers. New York: The Free Press of Glencoe, 1964. (Originally published in 1937.)


SELECTED BIBLIOGRAPHY

In this section of the bibliography, I have also included a number of reviews, critiques, expositions, and discussions of Talcott Parsons' sociological works by other writers. This was deliberate. Because of the frequent allusions to Parsons' writings in the majority of the items enumerated here, I decided that the division into two parts such as "Talcott Parsons: Secondary Sources" and a "Selected Bibliography" would be needlessly confusing and, moreover, unfruitful. The criteria for the separation of these bibliographical items, I thought, would be somewhat arbitrary and without value when the appraisal of ideas constitutes the paramount purpose of this inquiry.

Wherever I could, I included the original date of publication of the journal articles, essays, and books which have been amended or republished in different form.


Watling, J. "Propositions Asserting Causal Connection." 
*Analysis*, vol. 14, no. 2, December, 1953, pp. 31-37.

Watling, J. "The Problem of Contrary-To-Fact Conditionals." 
*Analysis*, vol. 17, no. 4, March, 1957, pp. 73-80.

Weingartner, P. "Can One Say of Definitions That They are True or False?" 


(Originally published in 1954)