PARSONS' "THEORY OF ACTION" AND "STRUCTURAL FUNCTIONAL" APPROACH TO SOCIAL SCIENCE: A CRITIQUE

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It is the general view among competent scientists that the development of the social sciences as compared to the physical sciences is lagging behind.[1] This state of affairs, however, does not indicate the lack of able men in the social sciences. There are eminent social scientists like Parsons, Merton, Homans, to name only a few, who have recognized this state of social science and who have tried to remedy the difficulties in social science theory.

The contemporary efforts toward making social science more "scientific" and fruitful may be exemplified by the contributions of the men just mentioned. Robert Merton advocate "theories of the middle range" because he was convinced of the futility of aiming at a comprehensive social theory.[2] On the other hand, Parsons believed that there is a need for an integrated and comprehensive theory of social action or behavior to guide research.[3] Along Parsons' closed system approach, Homans tried to construct a behavioristic theory of human groups in axiomatic form.[4] Another proponent of the need for formalizing sociological theory, starting from simple statements of perceptions to more complex theorems about social phenomena, is Zettersberg.[5]

Partly to suggest some answers why the development of the social sciences lags behind that of the physical sciences, this paper aims to

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[1] For example, see Morris R. Cohen, "Reason in Social Science," in *Readings in the Philosophy of Science*, ed. by Herbert Feigl & May Brodbeck (New York: Appleton-Century-Crofts, Inc., 1953), pp. 663-673; Edgar Zilsel, "Historico-Sociological Laws," *loc. cit.*, pp. 714-722; and George C. Homans, *The Human Group* (New York: Harcort, Brace & co., 1950), chap. I.

[2] Robert K. Merton, *Social Theory and Social Structure* (New York: Free Press of Glencoe, 1957), pp. 5-10.

[3] Talcott Parsons, *The Structure of Social Action.* (New York: The Free Press of Glencoe, 1937), chaps. I & II.

[4] George C. Homans, *op. cit.*

[5] Hans L. Zetterberg, *On the Theory and Verification in Sociology* (New York: The Bedminster Press, Inc., 1963), chap. I.

examine Parsons' theory of action and structural-functional analysis based on "scientific" considerations. It may be admitted that a satisfactory evaluation of Parsons' efforts at theoretical formulation is not possible in one paper. For an evaluation would call for an examination of the logical structure or consistency of Parsons' theories on the one hand and empirical verification on the other. This paper, perhaps, can only deal with the basic and elementary aspects of Parsons' theoretical "system." It may be asked: Is this system consistent with acceptable scientific methods? And what are the difficulties involved?

An Overview of the Scientific Method

As basis for subsequent comments, the scientific method may be outlined here. It must be admitted at the outset that there are disagreements about the nature of the scientific method. They are mainly due to the diversity of fields and objects of scientific investigation, such as the physical, biological, and social phenomena where the nature of the object of and method of investigation naturally differ.[6] Nevertheless, there are basic considerations common to all scientific endeavors.

The scientific method is nothing more than the application of accurate observation and of logical analysis and interpretation over phenomena hitherto new to human experience. In other words, it involves the senses and logical thinking in organizing sense-perceptions of the external world. It rejects metaphysical or ontological propositions because of the difficulty, if not impossibility, of demonstration and verification.[7]

As pointed out by Whitehead, it is on practical grounds that science should rely on the human senses, i.e., to avoid unnecessary and inconclusive argumentation about reality and human experience.[8] Closely connected with this is the necessity of demonstrability and ve-

[6] Cf. Henryk Mehlberg, *The Reach of Science* (Toronto: University of Toronto Press, 1958), pp. 45-78.

[7] See Max Black, "Observation and Experiment," in *Philosophical Problems,* ed. by M. Mandelbaum, *et al* (New York: Macmillan Co., 1957), pp. 22-35.

[8] It is interesting to note Whitehead's position on the complementarity of science and metaphysics. While he appreciates necessity of grounding knowledge on experience and logic, he also believes that metaphysics could play a complementary role to science. He wrote: ". . . One of the points I am urging in this address is, that the basis of science does not depend on the assumption of any of the conclusions of metaphysics; but that both science and metaphysics start from the same given groundwork

rifiability. It requires that experience and observation must not be private to only one person but subject to the inspection and confirmation of other competent persons. This implies that an observation (of an event or object or their relations) can be repeated under the same conditions.[9]

The problem of recognizing significant observation, of course, arises from the obvious fact that not any observation may constitute scientific knowledge. This leads to a consideration of what is "fact." An observation can only be considered a "fact" with respect to a certain inquiry or investigation. In other words, a "fact" is any observation that either confirms or denies the validity of a "hypothesis."

Here, one may note that out of a mass of observations or perceptions[10] rational man tends to make out patterns or uniformities, tentative at first and provisionally stated in the form of a proposition. This is called a "hypothesis." When this hypothesis is confirmed, it assumes the status of "law," stating the invariant relationships of the variables involved. And when this law agrees with other laws, these laws together may be integrated to constitute a theory.[11]

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of immediate experience, and in the main proceed in opposite directions on their diverse tasks.

. . . From an abstract point of view this exclusion of metaphysical inquiry is a pity. Such an inquiry is a necessary critique of the worth of science, to tell us what it all comes to . . . . It is possible that some distant generations may arrive at unanimous conclusions on ontological questions, whereas scientific progress may have led to ingrained opposing veins of thought which can neither be reconciled nor abandoned. In such times metaphysics and physical science will exchange their roles. Meanwhile we must take the case as we find it." A. N. Whitehead, *The Aim of Education* (New York: The New American Library, 1964), pp. 106 & 117.

[9] Max Black, *op. cit.*

[10] There is no need to go into an involved epistemological discourse here. It is enough to point out that efforts had been taken to purge science of metaphysical or *a priori* content. Russell, for instance clarified the relation of sense date to the object of physics as "reconstructions" rather than "inferred" and made possible empirical verification, i.e., by exhibiting physical objects "as functions of sense-data." — Bertrand Russell, "The Relation of Sense-Data to Physics," in *Philosophy of Science*, ed. by A. Danto & S. Morgenbesser (Cleveland: The World Publishing Co., 1962), pp. 33-34, 41-54. Ernest Nagel commented, however, that Russell's efforts were just "needless excursions into sterile epistomological speculations." It was just a matter of "analyzing or defining the sense of such expressions as 'physical,' 'point,' 'electron,' and so on." — "Russell's Philosophy," in Danto & Morgenbesser, *loc. cit.*, pp. 55-68. Here, Nagel was referring to an appeal on "operationism" in connecting the senses with the objects of physics. Hempel, however, could only grant to operationism the status of a program, not an established philosophy. — Carl G. Hempel, "Operationism, Observation, and Theoretical Terms," in Danto and Morgenbesser, *loc. cit.*, pp. 101-120.

This is rather a simple way of indicating the relations of fact, hypothesis, law, and theory in science. It is implied above that the ultimate goal of science is the formulation of a theory, starting from sense-perceptions to constitute facts for the confirmation of observed regularities among observed phenomena and the establishment of laws stating the invariant relationships of variables. In the ordinary conception of knowledge, this process of and hierarchy in scientific knowledge is equivalent to "understanding" observed phenomena. This means relating observed phenomena to previous experiences.

In more technical parlance, "understanding" (in popular usage) is called "explanation" in the philosophy of science. It means the deductive process of logically showing a phenomenon (its description) to be a consequence of the operation of a law or set of laws, given the conditions prescribed by such law or set of laws.[12] The only difference between "understanding" and "explanation" is that "previous experience" in "understanding" is couched as invariant relationships of variables (laws) in the language of science.

Both "understanding" and "explanation" exhibit the deductive process.

The formulation of hypothesis and laws involves both the inductive and deductive processes. As Russell pointed out:

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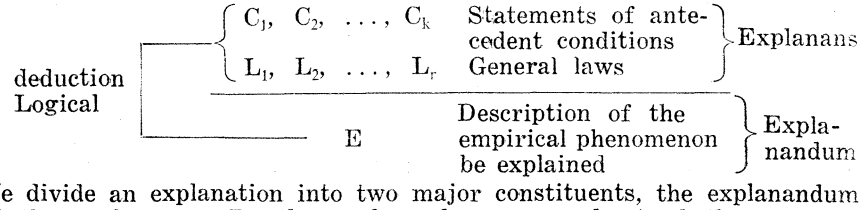
[11] For a well illustrated exposition on fact, hypothesis, laws and theory, see Irving Copi, "Fact and Hypothesis," in M. Mandelbaum, *op. cit.*, pp. 35-46.

[12] The logical form of explanation and prediction is illustrated in the model shown below:



"We divide an explanation into two major constituents, the explanandum and the explanans. By the explanandum, we understand the sentence describing the phenomenon to be explained (not that phenomenon itself); by the explanans, the class of those sentences which are adduced to account for the phenomenon. As was noted before, the explanans falls into the subclasses; one of these contains certain sentences $C_1, C_2, \ldots, C_k$ which state specific antecedent conditions; the other is a set of sentences $L_1, L_2, \ldots, L_r$ which represent general laws." — Carl G. Hempel and Paul Oppenheim, "Logic of Explanation," in *Readings in the Philosophy of Science,* Herbert Feigl & May Brodbeck, eds. (New York: Appleton-Century, Crofts, Inc., 1953, pp. 219-353.

> There is first a body of observed facts, then a general theory [i.e., hypothesis] consistent with them all, and then inferences from the theory which subsequent observation confirms or denies.[13]

Scientific knowledge thus expands as more hypotheses are confirmed into laws and laws become integrated into theories. At the same time, theories show gaps in knowledge for investigation and suggest the nature of still obscure phenomena. When the logical structure of a theory is developed and implications could be derived from it, some discoveries might be predicted as in the case of Yukawa predicting the existence of "a free particle (meson) about 150 times as great as the electron."[14]

It can be seen, therefore, that the scientific method is a tightly organized process of synthesizing human experience. At the same time, it enriches human experience by enabling man to reach beyond

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[13] Bertrand Russell, *Human Knowledge, Its Scope and Limits* (New York: Simon & Schuster, 1948), p. 409. This is quoted in R. Pascual, *Fundamentals of Logic* (Manila: G. Miranda & Sons, 1952), p. 265. It may be noted that Pascual is of the view that deduction rather than induction is responsible for the development of the physical sciences. For example, in the case of Newton's general theory of gravitation, it "was not the result of tedious induction . . . but one which follows methodically and deductively from a set of undefined terms, definitions and axioms. It is not the repeated occurrences which gave rise to the formulation of a law, but rather the rare perception of the invariant relation between the elements relevant to the occurrences in question." (p. 265) Of course, the inductive process is not shown in Newton's *Principia,* but certainly, it took place in his mind, consciously or unconsciously, that enabled him to formulate series of hypotheses about the phenomena that led him to the final form of his law of gravitation. — Cf. Arthur E. Bell, *Newtonian Science* (London: Edward Arnold Ltd., 1961), esp. 97-130.

[14] C. F. Presley, "Laws and Theories in the Physical Sciences," in Danto & Morgenbesser, *op. cit.,* p. 221.

[15] The efforts to break down the mystery of the atom has demonstrated the inadequacy of common sense to wrestle with duality in the quantum level. This refers to the anomalous situation where the constituents of matter (i.e., atoms) must be interpreted both as waves and as particles as indicated by experiments. Here, the common sense expectation that what is true when a thing is observed is also true when unobserved in the macro-level, is not applicable in the world of the quantum. The quantum does not follow the postulates of causality. For this reason, Reichenbach proposed the revision of logic where there are only two truth values, either true or false. A third truth value is needed, an intermediate one or "indeterminate." Thus, "by the help of such logic, quantum mechanics can be written in a sort of neutral language, which does not speak of waves or corpuscles, but speaks of coincidences, that is, of collisions, and leaves it indeterminate what happens on the path between two collitions." — Hans Reichenbach, "Are There Atoms?" in *The Structure of Scientific Thought,* ed. by Edward Madden (Boston: Houghton Mifflin Co., 1961), p. 105.

what could be experienced. This is illustrated in the gradual development of physical theory from classical Newtonian mechanics which is rooted in the immediately observable to quantum mechanics which deals with the less remote world of the atom and electron.

While science starts with the observable, the development of scientific knowledge shows how far man's senses could be relied upon and how far contemporary logic may be useful.[15] It also shows how the direction of scientific advance may be influenced by the nature of accumulated knowledge and the habits of thought that such development imposes.[16]

Before leaving this discussion on science and the scientific method, it is well to correct the impression that might have been created, i.e., that advances in science consist of orderly processes starting from sense-perceptions to hypotheses to theories. This is not the case.

Science starts as a question about a phenomenon. It generalizes, and checks such generalization deductively against particular instances. When there is no agreement between generalization and particular instance, another hypothesis or generalization is constructed. This is the sense in which Russell's inductive-deductive process of theory construction (see page 5) may be integrated. It is a trial-and-error process. The abstraction or conceptualization process is not like that of a fiction writer who invents a reality:

> Rather, it is similar to that of a man engaged in solving a well-designed word puzzle. He may, it is true, propose any word as solution; but, there is only one word which really solves the puzzle in all its form. It is an outcome of faith that nature — as

[16] David Bohm makes this point relevant to research: "To sum up this talk, we wish to call attention to the relationship between the methods of scientific research, and the content of scientific knowledge. The method must be tailored to the content; and if one loses sight of this, one is in danger of being artificially limited in a way that easily escapes conscious realization. Method is determined in part by the effort to ask relevant questions in our researchers; and it is essential to understand that the relevance of a question depends on the character of the material under investigation. Such questions help determine the forms of the facts that can be elicited in further researches. These questions are, in general, limited firstly by our concepts, laws, and hypotheses, and secondly, in a less obvious but equally important way, by our general habits of thought. Such habits can easily blind us to the need for altering our ways of thinking in accordance with the nature of the material under investigation as we penetrate into new domains." — David Bohm, "On the Relationship Between Methodology in Scientific Research and the Content of Scientific Knowledge," *British Journal for the Philosophy of Science* Vol. XII, No. 46 (August, 1961), pp. 103-116.

> she is perceptible to our five senses — takes the character of such a well formulated puzzle. The successes reaped up to now by science do, it is true, give a certain encouragement for this faith.[17]

Paradoxically, although concepts and theories must be tested against empirical data, Einstein took pains to point out that theory (specifically theoretical physics) "cannot be an inference from experience but must be free invention." He explained:

> The aim of science is, on the one hand, a comprehension, as *complete* as possible, of the connection between the sense experiences in their totality, and, on the other hand, the accomplishment of this aim *by the use of a minimum of primary concepts and relations.* (Seeking, as far as possible, logical unity in the world picture, i.e., paucity in logical elements.)
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> Science concerns the totality of the primary concepts, i.e., concepts directly connected with sense experiences, and theorems connecting them. In its first stage of development, science does not contain anything else. Our everyday thinking is satisfied on the whole with this level. Such a state of affairs cannot, however, satisfy a spirit which is really scientifically minded; because, the totality of concepts and relations obtained in this manner is utterly lacking in logical unity. In order to supplement this deficiency, one invents a system poorer in concepts and relations, a system retaining the primary concepts and relations of the "first layer" as derived concepts and relations. This new "secondary system" pays for its higher logical unity by having, as its own elementary concepts (concepts of the second layer), only those which are no longer directly connected with complexes of sense experiences. Further striving for logical unity brings us to a tertiary system, still poorer in concepts and relations, for the deduction of the concepts and relations of the secondary (and so indirectly of the primary) layer. Thus the story goes on until we have arrived at a system of the greatest conceivable unity, and of the greatest poverty of concepts of the logical foundations, which are still compatible with the observation made by our senses.[18]

But how can theory be tested against experience? What is the relation between theory and experience? To Einstein, "the relation is not analogous to that of soup to beef but rather of wardrobe number to overcoat."[19] Feigl, after pointing out the divergence of views on this point, took the position of Russell in regarding "the relation between physical object statements and phenomenal data statements as one of probabilistic inference."[20] He explained:

> No matter where the line is drawn between observables and inferred entities, the most adequate reconstruction, it seems to

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[17] Albert Einstein, "The Method of Science," in *The Structure of Scientific Thought,* ed. by Edward H. Madden (Boston: Houghton Mif-

[18] *Ibid.,* p. 83.

[19] *Loc. cit.*

[20] Herbert Feigl, *The "Mental" and the "Physical"* (Minneapolis: University of Minnesota Press, 1958), p. 76.

> me, has to be rendered in any case in terms of nomological nets. To return to the temperature example, we may say that the intensity of heat in an oven is indicated by various observable effects, but is not identical with any single one of them, nor is it identifiable with a disjunction (or other logical function) of the observable indications. The intensity of heat is nomologically, and hence synthetically, related to the indications of indicators. This is not to be confused with the quite obviously synthetic character of the functional or statistical relations between the incantions themselves . . . .
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> But even when theories (spelling out nomological networks) are adumbrated only in the form of extremely vague "promissory notes," the practice of scientific thinking clearly demonstrates that theoretical concepts (hypothetical entities) are never reducible to, or identifiable with, observable data (or logical constructions thereof). . . . Theoretical concepts are "anchored" in the observables, but are not logically (explicitly) definable in terms of the observables. To be sure, it is the "congruence," "consilience," "convergence," or whatever one wishes to call the testable correlations between the observables that allows for the introduction of fruitful theoretical concepts. It is indeed this consilience which provides the empirical basis for the specification of the meaning of theoretical concepts. Abstract postulates alone determine only their logical or mathematical structure, but never their empirical significance.[21]

Parsons' Theory of Action and Functional-Structural Method

Although it is difficult to simplify and interpret Parsons' Theoretical "system" and exposition, it is necessary for purposes of this paper. To minimize misinterpretation, therefore, extensive quotations dealing with important aspects of the theory will be made. In this connection, it is important to note the difficulties in sifting Parsons' "theory" or "conceptual scheme" from his scattered discussions on sociological theory, philosophical criticism, and his too ponderous style and complicated prose. Moreover, there are variations in emphasis in the three works where he expounded his theory of action, namely: (1) *The Structure of Social Action* (1937), (2) *The Social System* (1951), and (3) *Toward a General Theory of Action* (1962). Consequently, the summary of the theory of action and structural-functional method, to some degree, might miss what Parsons really meant, making subsequent criticisms irrelevant. Where Parsons made revisions, efforts are taken to incorporate them in the summary.

The "Voluntaristic Theory of Action." — Although nowwhere in his books did Parsons state his theory categorically or in terms of integrated propositions from which consequent empirical and testable statement could be deduced, it may be said that his theory actually

[21] *Ibid.*, pp. 76-77.

consists of a conceptual system of the "means-end" of human action and interaction.[22] The reason why he conceived of action-interaction as a system is that it would make parts of such a system determinate in relation to the whole as in a differential equation.[23]

If I understand Parsons' discussion correctly, the "action system" consists of three sub-systems, namely: (1) the personality system, (2) the social system, and (3) culture system. In the words of Parsons and his associates:

> The elaboration of behavior to which this conceptual scheme is especially appropriate, however, occurs above all in human action. In the formation of systems made up of human actions or the components of human action, this elaboration occurs in three configurations. First, the orientation of action of any one given actor and its attendant motivational processes becomes a differentiated and integrated system. This system will be called personality, and we will define it as the organized system of the orientation of action of one individual actor. Secondly, the action of a plurality of actors in a common situation is a process of interaction, the properties of which are to a definite but limited extent independent of any prior common culture. This interaction also becomes differentiated and integrated and as such forms a social system. The social system is, to be sure, made up of the relationships of individuals, but it is a system which is organized around the problems inherent in or arising from social interaction of a plurality of individual actors rather than around the problems which arise in connection with the integration of the actions of an individual actor, who is also a physiological organism. Personality and social system are very intimately interrelated, but they are neither identical with one another nor explicable by one another; the social system is not a plurality of personalites. Finally, systems of culture have their own forms and problems of integration which are not reducible to those of either personality or social systems or both together. The cultural tradition in its significance 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 systems of concrete actors, culture, though existing as a body of artifacts and as

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[22] This is the key concept that Parsons abstracted from the historical background of social action, specifically the utilitarian tradition. It views man as pragmatic and motivated by rational considerations of his wants and the situation which affects their fulfillment. In fact, this is the thesis of his *The Structure of Social Action* (New York: The Free Press of Glencoe, 1937), chap. XVIII. This goal-seeking emphasis, is however, modified in T. Parsons, E. A. Shils, *et al, Toward a General Theory of Action* (Mass.: Harvard University Press, 1962), pp. 6-7, making goal-seeking as a special case of action, and taking into consideration the influence of culture patterns and role-expectations. Cf. also *The Social System* (New York: The Free Press, 1951), pp. 3-7.

[23] *The Structure . . ., op, cit.*, pp. 8-12.

> 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]

The extensive quotation above, although simply said, illustrates the complexity of the relations of the subsystems as much as the subsystems themselves which Parsons calls system in the quotation but calls subsystem elsewhere. It may be noted that he calls such relations between the three subsystems, interpenetration and interdependence:

> Thus conceived, a social system is only one of the structuring of a completely concrete system of social action. The other two are personality systems of the individual actors and the cultural system which is built into their action. Each of the three must be considered to be an independent focus of organization of the elements of the social system in the sense that no one of them is theoretically reducible to terms of one or a combination of the other two. Each is indispensable to the other two in the sense that without personalities and culture there would be no social system and so on around the roster of logical possibilities. But this *interdependence* and *interpenetration* [italics mine] is a very different matter from reducibility which would mean that the very important properties and processes of one class of system could be theoretically *derived* from our theoretical knowledge of one or both of the other two. The action frame of reference is common to all three and this fact makes certain "transformation" between them possible. But on the level of theory here attempted they do not constitute a single system, however this might turn out to be on some other theoretical level.[25]

Nowhere are interdependence and interpenetration defined and distinguished from each other, notwithstanding the vital position of these concepts in the total theoretical scheme — thus illustrating the loose and haphazard way Parsons built up his theoretical scheme. The meanings of the two relational terms (i.e., interdependence and interpenetration), may be inferred, however, from the way each system (personality, culture, and social systems) shares the "frame of reference" or "orientation" scheme.

The first frame of reference is the actor defined as "an empirical system of action. The actor is an individual or a collectivity which may be taken as a point of reference for the analysis of the modes of its orientation and of its processes of action in relation to objects."[26] Second is the situation of action defined as "that part of the external world which means something to the actor whose behavior is be-

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[24] *Toward a General Theory . . ., loc. cit.*
[25] T. Parsons, *The Social System, op. cit.*, p. 6.
[26] T. Parsons, *Toward a General . . ., op. cit.*, p. 56.

ing analyzed... Specifically, it is that part to which the actor is oriented and in which the actor acts. The situation thus consists of the object of orientation."[27] And third is the orientation of the actor to the situation defined as "the set of cognition, cathexes, plans, and relevant standards which relates the actor to the situation."[28]

Thus, in the interactive relationship where the social system, personality, and culture determine the nature of the unit act, interpenetration and interdependence are illustrated. Ego (the actor whose act is under consideration) acts according to his "need-disposition," "social role," and the "culture pattern." Here, "need-disposition" represents the integrated components of the personality, namely: (1) "motivation, gratification-deprivation balance, primary viscerogenic and social-relational needs, cognition and learning, and the basic mechanisms of cognitive and cathectic-evaluative learning and adjustment"; (2) "the allocative processes by which the strivings toward gratification are distributed among the different available objects and occasions and gratification opportunities are distributed among the different need-dispositions"; (3) "the mechanisms, classifiable as those of defense and adjustment, by which the different components of need-dispositions are integrated internally as a system and directed toward objects"; and (4) "the integration of the various need-dispositions into an 'on-going' personality capable of some degree of self-control and purposeful action."[29] Social role, on the other hand, implies the orientation of an actor with respect to situation-objects and his and Alter's expectations in accordance with the prevailing culture pattern. Culture pattern also constitutes the symbols, value orientation and norms built into the personality system and social system of ego. Alter's (the actor with whom ego interacts) act is similarly determined as ego.[30]

From the preceding discussion, it may be said that interdependence means that each subsystem is indispensable to the others. The "articulation" of the personality system depends upon the "integration" of the biological needs with the social and cultural imperatives. Likewise, the role in social relations is defined by both the personality and culture, and culture develops out of the shared symbols and values and the established regularity of relations between alter and ego. Interpenetration, on the other hand, may be interpreted to mean the

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[27] *Ibid.*
[28] *Ibid.*
[29] *Ibid.*, pp. 8-19.
[30] *Ibid.*, p. 7.

process by which one subsystem (the three systems when considered as a unity) affects the other. It is difficult to go beyond what Parsons explicitly affirms since this is the basis of subsequent evaluations. In fact, interpenetration seems to contradict Parsons' postulate of "boundary-maintenance" which is mentioned below.

The structural-functional method. — This is simply a method in the attempt to construct a social science, complete with a comprehensive conceptual scheme. It starts with the mapping of social science phenomena, putting significant aspects into a system of categories, and defining the relationships of such categories. In setting up the system of categories or structure, Parsons explained:

> A particularly important aspect of our system of categories is the "structural" aspect. We simply are not in a position to "catch" the uniformities of dynamic process in the social system except here and there. But in order to give those we can catch a setting and to be in the most advantageous position to extend our dynamic knowledge we must have a "picture" of the system within which they fit, and, where changes take place, of what changes into what through what order of intermediate stages. The system of structural categories is the conceptional scheme which gives this setting for dynamic analysis. As dynamic knowledge is extended the interdependent explanatory significance of structural categories evaporates. But then scientific function is nevertheless crucial.[31]

On the other hand, function refers to the relation of the structural parts:

> If we have a sufficiently generalized system of categories for the systematic description and comparison of the structure of systems, then we have a setting within which we can mobilize our dynamic knowledge of motivational processes to maximum effects. But precisely relative to the problems which are of significance in most social system terms, the knowledge we possess is both fragmentary and of very uneven and unequal analytical status. The most effective way of organizing it for our purposes is to bring it into relation to a scheme of points of reference relative to the social system. This is where the much-discussed concept of "function" comes in. We must, of course, "place" a dynamic process structurally in the social system. But beyond that we must have a test of the significance of generalization relative to it. That test of significance takes the form of the "functional" relevance of the process. The test is to ask the question, what would be the differential consequences for the system of two or more alternative outcomes of a dynamic process? Such consequences will be found to fit into the terms of maintenance of stability or production of change, of integration or disruption of the system in some sense.[32]

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[31] *The Social System*, *op. cit.*, pp. 20-21.
[32] *Ibid.*, pp. 21-22.

Parsons was, of course, referring to the application of the structural-functional method to social systems. But it is shown below that he extended the application to the theory of action, i.e., to the personality and culture systems.

The concepts of structure and function are tied up with the concept of system. In Parsons' theoretical scheme, it is assumed that each part has a differential effect to the whole system. Thus, he postulated a "closure" of the system "so that it is possible to say if the facts in A sector are W and X, those in B sector must be Y and Z."[33] Other postulates regarding this concept of a system besides the interdependence and interpenetration of parts of the system, and closure of the system, is "self-maintenance" or "equilibrium." This means that each subsystem preserves and adjusts its relations with the others as changes take place in one part or the whole system. This is illustrated with the relation of the organism to its environment:

> 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 temperature.[34]

In terms of the theory of action, Parsons elaborated:

> The two fundamental types of processes necessary for the maintenance of a given state of equilibrium of a system we call, in the theory of action, *allocation* and *integration*. By allocation we mean processes which maintain a distribution of the components or parts of the system which is compatible with the maintenance of a given state of equilibrium. By integration, we mean the processes by which relations to the environment are mediated in such a way that the distinctive internal properties and boundaries of the system as an entity are maintained in the face of variability in the external situation. It must be realized that self-maintenance of such a system is not only maintenance of boundaries but also maintenance of distinctive relationships of the parts of the system within the boundary. The system is in some sense a unity relative to its environment. Also, self-maintenance implies not only control of the environmental variations, but also control of tendencies to change — that is, to alteration of the distributive state — coming from within the system.[35]

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[33] *Ibid.*, p. 20; Also *Toward a General . . ., op. cit.*, p. 49.
[34] *Toward a General . . ., Ibid.*, pp. 107-108.
[35] *Ibid.*, p. 108.

Evaluation and Criticism

The main questions we posed at the outset were (1) Is Parsons' theoretical scheme consistent with the scientific method? and (2) What are the difficulties involved? Corollary to question number one is an evaluation of whether or not Parsons' theory of action is consistent with scientific usage of theory. Moreover, since a theory can only be evaluated in terms of logical consistency and of correspondence with the empirical reality it purports to represent, the method by which it is constructed might be compared with the general historical background of how well-established theories in the physical sciences became formulated. Question number two takes up from the last question. This involves the problems directly involved in Parsons' theoretical scheme and other problems pertinent to the social sciences.

The theory of action. — A repeated reading of Parsons' three books dealing with the so-called theory of action — the result of which appears as a summary in this paper — yields the conclusion that it is not a theory in the strict sense of the term in the philosophy of science. At least, it may be conceded that it is an attempt to integrate scattered hypotheses about social behavior drawn from Psychology, Sociology, and Anthropology. And at best, it may indeed justify Parsons' more modest claim of constructing a system of categories or a taxonomy of social action.[36]

If theory were taken to mean "a deductively connected set of laws,"* or "a law, or principle, which has reached a high stage of generality, or a group of such laws considered as forming a single body of doctrine... [which] forms a conceptual scheme under which a wide class of physical sequences is subsumed,"[37] then, Parsons' so-called theory of action is not a theory. In the first place, the theory is not stated formally. In the second place, there are no laws that it integrates or subsumes.[38]

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[36] *Supra,* pp. 15-16. For a concise evaluation of taxonomic typological conceptualization, see Carl C. Hempel, *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science* (New York: The Free Press, 1965), pp. 137-171.

* May Brodbeck, "Models, Meaning, and Theories," in *Readings in the Philosophy of the Social Sciences,* ed. *idem.* (New York: The Macmillan Co., 1968), p. 583.

[37] Quoted in R. Pascual, *Fundamentals of Logic* (Manila: G. Miranda & Sons, 1952), p. 307.

[38] *Supra,* pp. 11-15; & Cf. Henryk Mehlberg, *The Reach of Science* (Toronto: University of Toronto Press, 1858), p. 214.

This assessment agrees with that of the American Sociological Association in its special session in 1950. It went further to criticize "that Parsons' terminology was largely jargon rather than designed to give greater precision."[39] The quoted discussions of Parsons above are adequate illustrations of this evaluation. However, the Association admitted that Parsons' contribution "amounts to a conceptual framework' from which hypotheses could be drawn for empirical testing."[40]

As a conceptual scheme, it may be asked: What does it lack to qualify as a theory? Hempel suggests "basic requirements for scientific theories" to fulfill in connection with his examination of "taxonomic concepts in the study of mental disorders" which may be used to evaluate Parsons' theory of action:

> (1) A clear specification of the basic concepts used to represent the theoretical entities (objects, states, processes, characteristics, and so on) in terms of which the theory proposes to interpret, and account for, the empirical phenomena in its domain of investigation;
> (2) A set of theoretical assumptions (basic laws, fundamental hypotheses) couched in theoretical terms asserting certain interrelations among the corresponding theoretical entities;
> (3) An empirical interpretation of the theory, which might take the form of operational criteria for the theoretical terms, or, more generally, the form of a set of laws, statistical or strictly universal in character, connecting the theoretical traits, states, or process with observable phenomena;
> (4) Testability-in-principle of the theory thus specified; i.e., the theory together with its interpretation, must imply, deductively, definite assertions about observable phenomena that should be found to occur under spefiable test conditions if the theory is correct: the occurrence or nonoccurrence of these phenomena will then provide confirming or disconfirming evidence concerning the theory.[41]

These requirements are similar for or applicable to the Weberian "ideal type" theoretical schemes, except that since ideal types implicitly express hypotheses of the relations of concepts or since they go beyond mere classifying (in contrast to taxonomic conceptualizations), one of the requirements included is a theory's incorporation as a special case in a more comprehensive theory as a long-range objective.[42]

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[39] R. A. H. Robson, "The Present State of Theory in Sociology," in *Problems in the Philosophy of Science* ed. by Imre Lakatos & Alan Musgrave (Amsterdam: North-Holland Pub. Co., 1968), pp. 354-355.

[40] *Ibid.*, p. 355.

[41] Carl G. Hempel, *op. cit.*, 150-151.

[42] The requirements that make ideal types "interpreted theoretical systems are: (1) specifying a list of characteristics with which the theory is to deal; (2) formulating a set of hypotheses in terms of those

The preceding summary of Parsons' theory of action showed that it was not lacking in concepts, which fulfills number (1) to some extent, but for the imprecise way they were defined and used. In fact, he used more concepts than was perhaps necessary such as the concept of boundary maintenance which seemed to contradict the concept of interpenetration. It was later shown in the illustration of an organism and its environment that this referred to the system being "not assimilated" by the environment. But this must have been implied in the "closure" postulate. The difficulty and perhaps the reason why Parsons committed this unnecessary redundancy is the lack of organization of his system. In other words, he did not begin from the simplest elements of his conceptual structure (axioms, postulates, and primitive terms) and then proceed to more complex principles and theorems. This formalization of the conceptual scheme obviously enabled Newton in reconciling the phenomenon of the elliptical behavior of the comet in 1680 with Galileo's theory of the solar system by coming out with his general theory of gravitation, systematically proved in his *Principia*.[43] His development of the calculus, of course, helped him, but without the simple and systematized concepts, the power of the calculus would not have been availed of.

Requirements numbers two, three,and four are obviously strange in Parsons' conceptual scheme. Parsons himself admitted that his "articulated conceptual scheme" "consisting of working out the structural outline" is "formidable" enough, but that he fell short of "setting up a system of variables." However, he claimed that his work will pave the way for the formation of laws, an example of which he provided:

> "In any concrete system of action a process of change so far as it is at all explicable in terms of those elements of action formulated in terms of the intrinsic means-and relationship can proceed only in the direction of approach toward the realization of the rational norms conceived as binding on the actors in the system." That is, more briefly, such a process of action can proceed only in the direction of increase in the value of the property of rationality.[44]

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characteristics; (3) giving those characteristics an empirical interpretation, which assigns to the theory a specific domain of application; and (4) as a long-range objective, incorporating the theoretical system, as a special case, into a more comprehensive theory. — *Ibid.*, p. 171.

[43] See Arthur E. Bell, *Newtonian Science* (London: Edward Arnold Ltd., 1961), pp. 97-130.

[44] T. Parsons, *The Structure . . ., op. cit.*, p. 751.

For comparison, Newton's law of gravitation is stated:

> Between any two particles of matter there is a force which is proportional to the product of their masses and inversely proportional to the square of their distance.

The preciseness and elegance of the law is shown in the equation:[45]

$$f = G \frac{m_1 m_2}{r^2}$$

where G = gravitational constant (32 ft./sec./sec.

m_1 & m_2 = masses of particles

r = distance of particles

Despite the hope and optimism of Parsons that sociological theory will reach the analytical stage that classical mechanism had already achieved in the 17th century, more than thirty years since he suggested a law to the present, not one established law in sociology has been formulated. There is, however, proliferation of inconclusive hypotheses about social phenomena. As late as 1945, Parsons was still preaching his structural-functional theory of action.[46] And in 1950, he was still optimistic about the prospect of a sociological theory from a structural-functional approach.[47]

There is no harm in being an incurable optimist; but this may not be equally true for being an "incurable theorist"[48] like Parsons. But what is harmful in being an "incurable theorist"? The harm lies in perpetuating theories that can not be tested that shackle the further development of more fruitful ones.

Parsons thinks that for social science to develop a working theory which guides and integrates researches should be provided. He believes that it is the lack of "an adequate working theoretical tradition" which is "probably the most crucial factor" in the "disappointing" advance in social science.[49] Moreover, he believes that in connection with the problems of objectivity and of the "tendencies to value-bias" among social scientists, and the problems of selection among the "numerous number of possible variables in social science, "perhaps theory is even more im-

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[45] W. G. V. Rosser, *Introductory Relativity* (London: Butterworths, 1967), 9.

[46] T. Parsons, "The Present Position and Prospects of Systematic Theory in Sociology" in *Essays in Sociological Theory* (rev. ed; Illinois: The Free Press, 1954), pp. 212-237.

[48] *Idem.*, 'The Prospects of Sociological Theory," *loc. cit.*, pp. 348-369.

[49] *Ibid.*, p. 350.

portant in our field [social science] than in the natural sciences."[50] As to how this can be so, he did not care to elaborate.

In this connection, it has been shown above that the development of science proceeds not from an integrated conceptual framework from which separate investigations follow but from guesses or hypotheses based upon a grasp of the implications of sense-perceptions about the external world. These are tested and elaborated, some to be discarded, others to be modified, and others to be confirmed by their empirical validity. In this process, it is important to note that as Einstein pointed out, theories are not the result of tedious inferences from particular instances or experiences but rather the "free inventions of the mind" that seek to mirror the logical nature of empirical objects.

Parsons is on the right track when he correctly distinguished the importance between density and mass in physics, the former being useful only in descriptive physics, the latter in theory and analytical physics. Assuming an analogy between framing theories in Physics and Sociology, he pointed out "that many of the variables now thought to be most fundamental in the Social Sciences will turn out to be in the same category as density, not as mass or velocity" and therefore "be as deceptive in our field [Social Science] as it is in physics."[51] From this premise, he asserted that "increase in operational precision, *by itself* would not advance us toward our goal of 'marying' theory and operational procedures in the fruitful manner of the physical sciences.[52]

In other words, Parsons thinks that development in social science proceeds from conceptualizing or selecting categories and concepts (structure) to formulating the relations of such concepts (function). As indicated above, he does not appreciate much the need for precision in determining the nature and relations of variables but puts primary importance to concepts, implicitly assuming that such variables may not, after all, have anything to do with concepts as the precision in the determination of density which does not affect the theory of mechanics.

It may be suggested at this point that Parsons missed the proper interpretation of the analogy. While it is true that concepts are not tedious inferences from experiences or perceived variables or objects,

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[50] *Ibid.*, p. 349.
[51] *Loc. cit.*, p. 14.
[52] *Ibid.*,

it is not true that an accurate determination of such relation is not important, since it is precisely this relation which are to be reflected in the concepts. In connection with his analysis of the role of taxonomy in the development of a science, specifically in concept formation, Hempel pointed out that:

> . . . the specification of a classificatory system requires a corresponding set of classificatory concepts: Each class provided for in the system is the extension of one of these concepts; i.e., it consists of just those objects in the universe of discourse which possess the specific characteristics which the concept represents. Hence, the establishment of a suitable system of classification in a given domain of investigation may be considered as a special kind of scientific concept formation.[53]

Thus, if concept must represent accurately, what it represents must itself be accurately determined.

In brief, Parsons thought that by providing a conceptual scheme (ie., taxonomic categories) he facilitates the formulation of laws. But it has been the contention of this paper in accordance with the practical considerations in the scientific method that in constructing a theory or a conceptual scheme, one has to follow a step by step process: conceptualizing and hypothesizing, testing and validating, discarding invalid ones and integrating valid ones into laws and theories, and repeating the process indefinitely as phenomena after phenomena appear or become recognized. One should not build his edifice on shifting sands if he is wise. In other words, it would have been more beneficial to Sociology if Parsons went down from his ivory tower of hypothesizing and started testing his concepts and validating the law he suggested in 1937.

The implications of the preceding discussion to the structural-functional method is the inevitable conclusion that if one follows the step-by-step process of theoretical formulation, there would be no need for it. Previous hypotheses, valid and invalid, suggest other hypotheses or other areas of investigation. There is no need for a man purporting to represent an unexplored area. To comprehend the extent, terrain, and geographic characteristics of that unexplored area, it is better to explore it rather than sit down and imagine or wait for hearsays about it or construct an imaginary map.

At this point, there is a need for some comments on other postulates of Parsons' conceptual scheme. First, in connection with a

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[53] Hempel, *op. cit.*, p. 139.

conceptualization of the "frame of reference," he postulated the goal-seeking tendencies of man as commonly done in biological "theorizing." He explained:

> There is implied in the relations of these elements [end, means, conditions and norms] a normative orientation of action, a teleological character. Action must always be thought of as involving a state of tension between two different orders of elements, the normative and the conditional. As process, addition is, in facts, the process of alteration of the conditional elements in the direction of conformity with norms. Elimination of the normative aspects altogether eliminates the concept of action itself and leads to the radical positivistic position. Elimination of conditions, of the tensions from that side, equally eliminates action and results in idealistic emanationism. Thus conditions may be conceived at one pole, ends and normative rules at the other, means and effort as the connecting links between them.[54]

In the shorter version of his "theorizing," a clearer meaning of his use of teleology is indicated. It implies the "striving toward the attainment of 'goals;' of 'reacting' emotionally or affectively toward objects and events, and of, to a greater or lesser degree, cognitively knowing or understanding his situation, his goals and himself.[55]

Without going into detailed discussion on a criticism of functionalism where teleology is a basic concept, it is enough to say that there is no necessity in assuming it as Hempel showed in his analysis. He concluded:

> In all of these cases, the laws of self-regulation exhibited by the systems in question are capable of explanation by subsumption under general laws of a more obviously causal form. But this is not even essential, for the laws of self-regulation themselves are causal in the broad sense of asserting that for systems of a specified kind, any one of a class of different "initial states" (any one of the permissible states of disturbance) will lead to the same kind of final state. Indeed as our earlier formulations show, functionalist hypotheses, including those of self-regulation, can be expressed without the use of any teleological phraseology at all.[56]

He pointed out, however, that as a heuristic device in suggesting hypotheses, functionalism and teleology may be useful. And

> if the advantages it has to offer are to be reaped in full, it seems desirable and indeed necessary to pursue the investigation of specific functional relationships to the point where they can be expressed in terms of reasonably precise and objectively testable hypotheses. At least, initially, these hypotheses will likely be of quite limited scope. But this would simply parallel the present

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[54] T. Parsons, *The Structure . . ., op. cit.*, p. 732.
[55] *Idem., Essays in Sociological Theory, op. cit.*, p. 228.
[56] Hempel, *op. cit.*, p. 326.

> situation in biology, where the kinds of self-regulation, and the uniformities they exhibit, vary from species to species. Eventually, such "empirical generalizations" of limited scope might provide a basis for a more general theory of self-regulating systems. To what extent these objectives can be reached cannot be decided in *a priori* fashion by logical analysis or philosophical reflection: the answer has to be found by intensive and rigorous scientific research.[57]

The second objectionable postulate is emergentism.[58] Parsons explained:

> A word should also be said about the sense in which the term emergent is here used since it has acquired various connotations elsewhere. Here it 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. So far they are no different from any other general properties. What distinguishes the emergent from the elementary properties is only the fact that upon unit analysis of the system in question beyond a certain point they evaporate and are no longer observable. This has been amply illustrated for the case of economic rationality. The existence and empirical importance of emergent properties in this sense is, as has been seen, a measure of the organicism of the system. They are basically important to action systems.
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> . . . There is no mysticism whatever about this concept of emergence. It is simply a designation for certain features of the observable facts.[59]

The main reason for his postulating emergentism and organicism evidently arises from his use of system in conceptualizing human action or behavior. While he denounced Spencerian evolutionism[60] which postulates as anthropomorphic view of society (i.e., that it grows like a human being), he, himself, postulates a view of a system patterned after the systemic organization of the human body or organism.

It may be noted that the problem of organicism in biology is still hotly debated between its proponents and the proponents of "mechanistic" biology. However, Nagel reduced the problem to the simple question of whether or not the whole organism is not explainable to the properties of its parts, and proposed a clear analysis of the tangled issues:

> Let me first state the suggestion in schematic, abstract form. Let T be a definite body of theory which is capable of explaining

[57] *Ibid.*, p. 330.

[58] In Parsons' theorizing, emergentism arises from his organismic view of the system of action. See his *The Structure . . ., op. cit.*, p. 31.

[59] *Ibid.*, p. 749.

[60] *Ibid.*, p. 3.

a certain indefinitely large class of statements concerning the simultaneous or successive occurrence of some set of properties P_1, P_2, . . . P_k. Suppose further that it is possible with the help of the Theory T to explain the behavior of a set of individuals i with respect to their manifesting these properties P when these individuals form a closed system s_2 under circumstances C_1; and that it is also possible with the help of T to explain the behavior of another set of individuals j with respect to their manifesting these properties P when the individuals j form a closed system s_2 under circumstances C_2. Now assume that the two sets of individuals i and j form an enlarged closed system s^3 under circumstances C_3, in which they exhibit certain modes of behavior which are formulated in a set of laws L. Two cases may now be distinguished: (a) it may be possible to deduce the laws L from T conjoined with the relevant initial conditions which obtain in C_3; in this case, the behavior of the system s_2 may be said to be the sum of the behaviors of its parts s_1 and s_2; or (b) the laws L cannot be so deduced, in which case the behavior of the system s_3m may be said not to be the sum of the behaviors of its parts.

Whether the above proposal to interpret the distinction between wholes which are and those which are not the sums of their parts would be acceptable to organismic biologists, I do not know. But, while I am aware that the suggestion requires much elaboration and refinement to be an adequate tool of analysis, in broad outline it represents what seems to me to be the sole intellectual content of what organismic biologists have had to say in this connection. However, if the proposed interpretation of the distinction is accepted as reasonable, then one important consequence needs to be noted. For, on the above proposal, the distinction between wholes which are and those which are not sums of parts is clearly relative to some assumed body of theory T; and, accordingly, though a given whole may not be the sum of its parts relative to one theory, it may indeed be such a sum relative to another. Thus, though the thermal behavior of solids is not the sum of the behavior of its parts relative to the classical kinetic theory of matter, it is such a sum relative to modern quantum mechanics. To say, therefore, that the behavior of an organism is not the sum of the behavior of its parts, and that its total behavior cannot be understood adequately in physico-chemical terms even though the behavior of each parts is explicable mechanistically, can only mean that no body of general theory is now available from which statements about the total behavior of the organism are derivable. The assertion, even if true, does not mean that it is in principle impossible to explain such total behavior mechanistically, and it supplies no competent evidence for such claim.[61]

In brief, as Brodbeck put it, the problem of emergence "is a matter of explanation rather than of description."[62]

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[61] Ernest Nagel, "Mechanistic Explanation and Organismic Biology," in Madden, *op. cit.*, pp. 37-138.

[62] May Brodbeck "Methodological Individualisms: Definition and Reduction," in Brodbeck, *op. cit.*, p. 286.

To forestall attack on his postulating emergence, Parsons pointed out that "there is no mysticism" involved. Of course, mysticism would refer to the assumed occurrence that "upon unit analysis of the system in question beyond a certain point they evaporate and are no longer observable." A close look into his illustration of economic rationality as an "emergent property of action which can be observed only when a plurality of unit acts is treated together as constituting an integrated system of action,"[63] One can see that economic rationality is a postulate or assumed principle in economics where a person acts in the market, in the firm, or as consumer according to alternatives presented in the situation (e.g., he buys at a lower price and sells at higher price). Here, it is claimed that to "carry unit analysis to the point of conceptual isolation of unit act is to break up the system and destroy this emergent property."[64]

If "buying" guided by tastes, preferences, and prices were to be considered a unit act, and if rationality were a "property" of the unit act of buying; and following Parsons' line of reasoning, will analysis of the elements in a "rational" buying act "destroy" the "emergent rationalism"? Of course, it is absurd to attribute rationalism to taste, preferences, and price singly but only to the act of buying. In fact, it is similarly absurd to say that "rationality" is a property of an act because this would imply that an act is a physical object, if "property" were construed as the combustibility in the case of gasoline, which refers to the decomposition of gasoline to its components (i.e., carbon and oxide) given specifiable conditions. It may be noted that "property" in this usage refers to the chemical predisposition of a compound to decompose into its elements according to chemical laws, given certain conditions. It is obviously not applicable to an act which is just a concept. It can be said, therefore, that rationality is simply a description or classification of an act according to the criteria of taste, preference, and price. For example, if one buys a pen at ₱20 when similar pens are available at ₱10 is not rational, and the converse is rational.

There is, therefore, no necessity for postulating emergence even in the case of rationality. Its use is just a self-deception by one's terminology and conceptualization, which demonstrates the need for strict definition of terms and the hierarchical structuring of concepts. In the case of the theory of action and its unit parts, it is difficult to

[63] T. Parsons, *The Structure, op. cit.,* pp. 739-740.
[64] *Ibid.,* p. 740.

postulate an analogy between it and an organism because an organism and its organs have indeed "properties" while that of action is just a concept or abstraction. On the other hand, there is no need to belabor on the analogy between the organism and the concept of action because in the light of Hempel's discussion, emergence is not a necessary postulate. And this should be equally true to the theory of action.

This leads to the question of whether or not the parts of the action system namely the personality system, social system, and culture system, are not explainable under psychological laws, or even physical laws. It seems that this is one reason why Parsons postulated emergentism — the desire to preserve Sociology as a separate science from Psychology. His main argument of the parts not equal to the whole has been shown to be untenable. This makes questionable the assumption that the social system and culture system cannot be explainable (Parsons used the word "reducible" incorrectly in this context in page 13) under the laws of Psychology. Although at present, Psychology is not developed as to have comprehensive laws and theories for reduction purposes, Brodbeck wrote:

> The possibility of "reduction" is the issue raised by asking whether the phenomena of one field, say chemistry or psychology, can be explained in terms of the phenomena of another, say physics or physiology respectively. Reduction, as I understand it, involves deduction. Explanation, in one firm meaning of that term, is achieved by deducing what is to be explained from true premises. Only statements, never concepts, can serve as either premises or conclusion of a deduction. Explanation, therefore, is always of statements by means of other statements. (Obvious and trivial as this point is, as long as the confused notion of "explanatory concepts" lingers on, it is worth remaking.) The deduction by which reduction is achieved also serves to explain. Explanation is in fact a major reason for reduction. It is consequently a matter of laws and theories, not of terms or concepts. What is sometimes called "reduction" of terms is, strictly, definition of the kind we have just discussed. Not all deduction, however, achieves reduction. We explain a law by deducing it from another law or laws.
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> Among chemists, reductionism is hardly a controversial issue. Naturally not, since it is an accomplished fact. But even among psychologists, where it is at best a program, though the range from optimism to pessimism is very wide, vociferous extremes at either end, ultimate physiological reduction is accepted as a frame of reference. The area concerned with group variables is rather more sensitive, however. Further removed, both historically and systematically, from the biological sciences than are psychologists, those concerned with group sciences tend to exhibit greater emotional reactions to the reductionism issue. They are darkly

> suspicious that the proponents of reductionism aim primarily to put them out of business by denying them any real subject matter. Nor perhaps are their suspicions wholly unfounded. However that may be, a firm grasp of the distinctions between the definitions of terms and the reduction of laws and between perfect and imperfect knowledge should considerably reduce the decibel count of this clamor either for or against autonomous group science.[65]

Some methodological problems in social science. — As indicated in the preceding, there is a proliferation of social science theories. In fact, as deplored by Parsons, sociologists themselves admit that there are as many sociological theories "as there are sociologists."[66] This may be partly attributed to the absence of crucial experiments in the social sciences which would enable the rejection of invalid or inadequate hypotheses instead of their elevation to the status of theories. This is to say that as Copi pointed out, although scientific hypotheses, theories, or laws are not "wholly discrete and independent" or the "theoretical structure of science grows in a more organic fashion" and that there is no such thing as *ad hoc* hypotheses for which crucial experiments may be applicable in their validation or invalidation, crucial experiments are useful in putting to the test "dubious hypotheses together with accepted parts of scientific theory." Moreover, it helps in "dragging 'hidden assumptions' into the open" "for critical examination as the case of the assumption before Einstein developed his Special Theory of Relativity "that it always makes sense to say of two events that they occurred at the same time," which is not possible for two observers because simultaneity depends "upon their locations and velocities relative to the events in question."[67]

It is pertinent at this point to quote Cohen's cogent observations on the difficulties that confront social science:

> In the first place, agreement based on demonstration is less easy and actually less prevalent in the social than in the natural sciences, because the greater complexity of social facts makes it less easy to sharpen an issue to an isolable point and to settle it by direct observation of an indefinitely repeatable fact. The issue between the Copernican and the Ptolemaic astronomy in the days of Galileo was reduced to the question whether Venus does or does not show phases like the moon's and this was settled by looking through a telescope. If Venus did not forever repeat her cycle, and if the difference between a full circle of light and one partly covered by a crescent shadow were not so readily perceived, the matter could not be so readily settled.

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[65] May Brodbeck, *op. cit.*, pp. 286-288.
[66] T. Parsons, The Structure . . ., *op. cit.*, p. 774.
[67] Irving M. Copi, "Crucial Experiments," in Madden, p. 33.

> With the greater complexity of social facts are connected (1) their less repeatable character, (2) their less direct observability, (3) their greater variability and lesser uniformity, and (4) the greater difficulty of isolating one factor at a time. These phases are so dependent on one another that we shall not treat them separately.
>
> The last observation [the difficulty of isolating a factor and performing a crucial experiment] suggests that the greater complexity and variability of social fact also make its purely theoretical development more difficult. In general, social situations are networks in which one cannot change one factor without affecting a great many others. It is, therefore, difficult to determine the specific effects of any one factor. Moreover, social elements seldom admit of simple addition. The behavior of the same individuals in a large group will not in general be the same as their behavior in a smaller group. This makes it difficult to apply the mathematical methods which have proved so fruitful in the natural sciences. For these mathematical methods depend upon our ability to pass from a small number of instances to an indefinitely large number by the process of summation or integration.[68]

To these problems, although Cohen was pessimistic about the usefulness of statisical methods in social science because of the inapplicability of the continuous curve to a few discrete observations in social science, Brodbeck and Zilsel are of the opposite view, especially with the use of stochastic equations where the weights of variables could be approximated.[69] There is nothing more that can be said, except to wait for developments.

Conclusion

The inescapable conclusion that may be drawn from the discussion is that Parsons' theory of action is rather a program rather than a verified theory. It is at best a conceptual scheme designed to guide research. It is, however, suggested by the nature of scientific development that Parsons' solution to the problem of hastening the maturity of social science is by putting the cart before the horse. But who knows? As Hempel left open the fruitfulness of taxonomic and ideal or typological theorizing, including functional analysis, Nagel that of organiscism and emergentism, and Feigl that of giving substance to the mind-body puzzle,[70] one can do no less in the case of the pros-

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[68] Morris R. Cohen, "Reason in Social Science,"

[69] May Brodbeck, *op. cit.*, p. 296: & Edgar Zilsel, "Physics and the Problem of Historico-Sociological Laws," in Feigl, ed., *op. cit.*, pp. 714-722.

[70] Herbert Feigl, *op. cit., passim.*

pect of social science. This includes the usefulness of *verstehen* which has been shown by Abel to be useful in guiding hypotheses.[71]

In this connection, Bohm's suggestion that our habits of thought and our scientific knowledge might inhibit us from seeing a different kind of phenomena not implied by present scientific knowledge is well taken. Who can say that social phenomena is not altogether different from physical phenomena, and that they require an altogether different method? As shown by Reichenbach, even in the world of the quantum, two-truth value logic is not adequate to deal with the indeterminate nature of the quantum world. It might also be the case that social phenomena are not amenable to the methods used fruitfully in the physical sciences. In fact, Bohm suggested that the atoms of living matter might be different from the non-living.

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[71] Theodore Abel, "The Operation Called 'Verstehen'" in Madden, *op. cit.*, pp. 158-166.