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# Talcott Parsons, the Problem of Order in Society, and the Program of an Analytical Sociology

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The persuasiveness of Parsons's solution to the Hobbesian problem of order rests on the conjunction of two premises, one substantive-sociological, the other methodological. (1) Normative consensus may ensure order in a way which can be "generalized." (2) Sociology must be an "analytic" science. Both premises are criticized, the former because it is empirically false, the latter because in its Parsonian conception it would render sociology nonfalsifiable. It is further argued that Parsons, while continuing to cling to his normative solution to the problem of order, eventually abandoned his early interpretation of the analytic status of sociology, and therewith deprived his solution to the Hobbesian problem of an essential prop. Finally, the whole Parsonian approach to social science is shown to issue from a metaphysical view of social reality as a composite of several spheres, each of them *sui generis*.

Ever since *The Structure of Social Action* had its initial impact on the field of sociology, the provision of an acceptable solution to the "Hobbesian problem of order" has been considered an important test for any general theory of society. In the eyes of many authors, Parsons's own formulations do not pass it too well (Feuer 1962; Foss 1963; Mills 1959, pp. 42-49; Rex 1961, pp. 102-3, 110-12; Dahrendorf 1959, pp. 157-63; Gouldner 1970, pp. 332-33), yet this evaluation is by no means universally shared. Parsons himself apparently has not been impressed by the criticisms put forward. It is rather obvious that his later and recent writings rest on the presumption of the satisfactoriness in principle of his early solution. I want to argue here that this attitude is by no means due to ideological blinders, as is often easily surmised. Instead it constitutes a legitimate response to the failure of even the more ambitious critics (e.g., Mitchell 1967; Black 1961; Bershadsky 1973; Rocher 1974; Turk and Simpson 1971; Chazel 1974) to pay more than marginal attention to a point which is crucial to a proper appreciation of Parsons's argument concerning the *problem of order*. This is the intimate interplay of substantive and methodological considerations in his treatment of the question (clearly recognized but left unexplored by Lockwood 1956; Ellis 1971; Kaplan 1968). I shall argue that any criticism of his substantive-theoretical solution loses much of its effectiveness as long as the methodological aspect is disregarded. I shall therefore attempt a critique of the methodological argument which Parsons uses to defend his substantive solution, namely, his advocacy of an "analytical" sociology.

# I

In *The Structure of Social Action*, Parsons's suggestions concerning a solution to the problem of order crystallized as a critical response to earlier theories which attempted to account for order in society by reference to either domination or enlightened self-interest. The alternative view, that a single common system of ultimate ends is "the culminating element of unity" (Parsons 1937, p. 249) which holds society together, was presented by him as "by far the most satisfactory general solution of the problem of the basis of order in society" (Parsons 1934, p. 518). For many commentators this has been sufficient reason to credit him with a somewhat underdeveloped awareness of the empirical importance of private interests and power differentials in social life; the frequent response has been exactly to emphasize this importance (e.g., Lockwood 1956; Kaplan 1968; Gouldner 1970; Chazel 1974, pp. 102–5; Foss 1963). However, in view of a variety of statements by Parsons which emphasize the role played by self-interest and coercion in society (e.g., Parsons 1932, p. 342; 1951, p. 40; 1966, p. 91), this reaction seems to me too easy. Thus it is granted here that Parsons is well aware of the empirical importance of power and self-interest in the determination of social affairs. What, then, explains their neglect in his theoretical model? The answer is given in two places.

First, in his discussions of the Hobbesian solution to the problem of order, Parsons declares that it cannot answer the question, "Who guards the guards?" and thus does not provide a *general* solution concerning the elimination of civil strife (see Bershady 1973, pp. 45–46). Second, in his assessment of Locke he argues that without the untenable assumption of the natural identity of men's interests the relative absence of force and fraud from social life cannot be understood. Therefore the mutual advantages gained from peaceful exchange cannot be considered the *general* cause of social order, either. In contrast, the normative solution, the postulate of a shared system of ultimate values and ends, provides "the most satisfactory general solution" (in contrast, see Bershady 1973, pp. 46–47). For while it is not conceivable that everyone in society profits or can be coerced, everyone can be morally restrained. Or at least Parsons must believe so (since this seems to be implied in his argumentation), and it is for this reason, then, that he thinks that normative consensus founded on the feelings of solidarity and moral obligation is the ultimate basis, or main aspect, of the integration of society, whereas physical sanctions and calculations of self-interest are only secondary, although empirically never totally absent (Parsons 1937, pp. 403–4).

Now it might be asked immediately in what sense the failure of self-interest and coercion as universal guarantors of order in society makes them theoretically "secondary" (see Kaplan 1968, p. 896). But before considering this question it is useful first to examine Parsons's notion that moral restraint

is such a universal guarantor (see Ellis 1971). Here it turns out that the plausibility of the normative solution rests on two separate tenets: One is that norms effectively discipline individuals above all through their moral authority, relatively independent of any instrumentally significant consequences of conformity with them (Parsons 1951, p. 37). The other is that there is a tendency for individuals to develop and maintain attachment to the *same* integrated system of norms and to find solidarity in the pursuit of shared goals (Parsons 1934–35, p. 295; Peacock 1976, p. 265). I want to suggest that neither tenet can be maintained in a version sufficiently strong to lend much support to the claim of a general normative solution to the problem of order.

The idea that every individual (or a sufficient number of them) can be effectively morally disciplined has been elaborated by Parsons into the conception of the actor's adequate socialization through the internalization of norms. Relatively convincing objections to his formulations have recently been voiced by two authors of rather opposite theoretical persuasions. One is Cancian in her criticism of the "socialized actor view" (1975, pp. 105–9; 1976), the other is Scott in his debunking of the "moral man" (1971). Both criticisms converge in one point which is important here: Conformity with norms is not largely independent of ulterior considerations and expectations on the part of the actor; therefore treating the influence of these considerations as empirically of secondary importance is not justified.

The conception that individuals who live together "solidarize," that is, tend to form a moral community, is, of course, of Durkheimian provenance. Parsons accepts Durkheim's evidence, and his interpretation of it, that an individual must be integrated into a moral community in order to be mentally healthy and able to live; it is this vital dependence on group life which constitutes man as a "social" being. Thus Parsons writes that "without a system of common values . . . there can be no such thing as a society. Durkheim's treatment of *anomie* may be held to have definitely established this" (1937, p. 434).

Now, for the sake of the argument, one may accept that Durkheim has "definitely established" that, for their emotional well-being, individuals must be part of a solidary group. Yet he has by no means established that the group can only be the total society. It might very well be a subgroup, and probably is. But if one allows for subgroups, the question arises of their relation to the overall social organization. In view of the potentially divergent "ideal interests" of individuals and groups within a society, what *accounts* for the emergence and maintenance of a shared respect for the *same* value system (Kaplan 1968, p. 901; Giddens 1968, p. 269)? It is not enough now to refer to a solidarizing disposition of men qua social beings, the circumstance that under conditions of human physical proximity and contact common norms and values tend to arise. This idea is too vague with

respect to the problems raised by the integration into more comprehensive social organizations of groups which can provide individuals with all the solidarity they need. For the fact is that overarching normative orders are usually imposed by someone (Chazel 1974, p. 170), imposed not on a life-threatening normative unordered, but on a variety of divergent normative orders adhered to by subgroups or subject groups. And it is incontestable that the imposition relies on appeal to self-interest and fear of sanctions as much as on the moral attractiveness of the advocated values. Furthermore, what is true of the emergence of an overarching value system is also true of its maintenance. Since there is a constant production of new norms and values, there are always variety and competition in the normative realm; there is an abundance of prophets at any time. Under these competitive circumstances it is hardly questionable that the appeal of a normative system can only be maintained if individuals are able to integrate it with the pursuit of their private interests, and if it is backed up by political power; reaffirmation through ritual alone will not save it. This interdependence is the gist of much that Weber had to say, but here as elsewhere Parsons does not read Weber well (Cohen, Hazelrigg, and Pope 1975; Pope, Cohen, and Hazelrigg 1975; also Lockwood 1956; Heydebrand 1972, pp. 393–94).

To sum up the argument, then: Parsons's normative solution to the problem of order in society encounters difficulties analogous to those faced by the self-interest and power solutions. His solution should show how, in spite of all sorts of centrifugal tendencies, the normative interests of the individuals and groups composing a society can be brought together and kept together for some length of time *primarily through moral appeal*. Parsons convinced himself and others that the solidarizing disposition of individuals is sufficient to ensure the authority of values. Yet his solution really consists in nothing else than the postulate of the natural identity of normative interests.

## II

In this and the next section I attempt to show that Parsons's decision to give domination and self-interest only marginal status in his sociology is due not so much to his failure to be properly impressed by their empirical importance as to his metaphysics of social reality (see also Bershadsky 1973, pp. 65–81, 93–124). This comes to the fore in his interpretation of social science theory as "analytic." That methodological argument provides him with the fundamental justification for the systematic disregard of these phenomena, and it is rather independent of empirical evidence (for such evidence, see, e.g., Kaplan 1968, pp. 898–905).

The fact that any *concrete* social order is maintained mainly by the

interplay of individual self-interest, habituation, the threat and application of coercion, and more or less shared normative convictions has frequently given rise to the argument that any theory is inadequate which, in attempting to account for the orderly interrelatedness of social phenomena, pays attention to only one of these factors rather than to all of them. Parsons himself has been critical of the one-sidedness of classical and neoclassical economic theory, and thus one might expect him to advocate a theoretical framework for sociology which accommodates all the relevant and important variables. However, this is by no means the path chosen by him. He rejects such an "encyclopaedic sociology" and instead argues in favor of a plurality of special social sciences constructed along the lines of economic theory, each similarly one-sided, dealing with just one essential "aspect" of concrete social life. In his view the explanation of concrete social phenomena would be accomplished by the combined totality of these special theories. This argument deserves some scrutiny in detail since it provides the basis on which Parsons puts forward a definition of sociology as the science of institutions, in which questions of power, self-interest, and individual psychology do not require systematic treatment (Parsons 1934, p. 529; also 1951, pp. 547-48, 551-52).

According to Parsons, social science (and especially economics) since its inception has been bedeviled by what he calls "empiricism." This he defines as a methodological position which holds that "any science, or at least economics, has the task of delivering a full and complete explanatory account of a given sector of concrete reality" (1935*a*, p. 420). In conjunction with a particular theory this view involves, in its radical version, the claim that everything there is to be known about a set of concrete phenomena can be known in terms of this particular theory. A more moderate version holds that everything there is to be scientifically known about a set of concrete phenomena can be known in terms of the particular theory as long as certain conditions of applicability are given (ones whose presence is more or less taken for granted) (1937, p. 729). The mistake inherent in both these views is what Whitehead has termed the "Fallacy of Misplaced Concreteness," which is "the accidental error of mistaking the abstract for the concrete" (Whitehead 1925, p. 74). Parsons defines it as the assumption that a general analytical schema which works for a certain body of facts adequately reflects concrete reality for *all* scientific purposes (1937, p. 476). As against this he correctly insists that all theoretical concepts are abstract and refer only to some aspects of concrete phenomena; therefore a theory of phenomena in all their concreteness is not possible.

When a given theory receives an empiricist interpretation, a critical situation arises as soon as the theory proves to be obviously insufficient for the complete explanation of the range of phenomena for which its applicability is postulated. A frequently advocated way out of this, which retains the

empiricist basis, is to improve the theory's explanatory power either by broadening its framework through the inclusion of new variables or by rebuilding it more comprehensively from the start. However, when the empiricist interpretation of scientific theory is abandoned, another possibility opens up. This is the "analytical factor view" (1935*b*, p. 646). In contrast with the empiricist tendency "to think of the analytical concepts of science as corresponding directly to observable *concrete* entities, and a classification of the social sciences . . . as corresponding to the different concrete spheres of social life" (1937, p. 452), it views these concepts as referring to abstract aspects of concrete phenomena and views the different social sciences as dealing systematically with different groups of abstract "elements analytically separable from the total complex of action . . ." (1937, p. 465). This interpretation of the methodological status of the special social sciences was first intimated by Pareto, and Parsons makes it his own.

The "analytical factor view" of the methodological status of sciences like economics is predicated on the idea that concrete social phenomena can be conceived as combinations of a number of distinguishable abstract "elements" (Parsons 1935*a*, p. 421), as manifestations of the influence of a variety of different analytical "factors" (1935*b*, pp. 646–47), or as having a number of "aspects" (1934, p. 530). 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 (1935*b*, p. 660). It is therefore never possible to completely explain a particular phenomenon exclusively by reference to one such factor. This precludes the formulation of special social sciences on the basis of the idea that a distinctive theoretical apparatus focusing on just one of these factors or aspects could be sufficient for the full explanation of a separate set of concrete phenomena of social life. Such sciences can only deal each with *one aspect* of *all* human action, not with *some* action in all its *concreteness*. It is in this sense that they are "abstract" (1934, p. 530). When a social science is characterized by preoccupation with one of these factors and attempts to establish its contribution to concrete phenomena, it treats the contribution of other factors not as unimportant or empirically constant, but as a datum for the theory which is not to be subjected to systematic inquiry. "The view of the proper abstraction for the social sciences here put forward is . . . that . . . of abstract analytical systems each of which assumes as data the main outline of fundamental structure of concrete systems of action including the elements other than those immediately dealt with by the science in question" (1937, pp. 465–66). No special social science provides a full explanation of the concrete variation of phenomena since each accepts certain relevant questions as lying beyond its scope. A complete explanation can be accomplished only by all these sciences in their combined totality.

III

The argument that the phenomena of social life are best studied with the help of a variety of analytical social sciences (see Wrong's protest 1961, p. 190) provides Parsons with the justification for his limitation of sociology to the study of the normative aspect of phenomena. From this it follows that those who criticize him for this limitation because self-interest and domination are empirically important are barking up the wrong tree. To Parsons's recognition of the many factors which empirically contribute to social order corresponds his metaphysical conviction of the "ultimate concrete unity of human social life" (1935*b*, p. 667, n. 5, also p. 660). But just as the latter is not at odds with, but (according to Parsons) requires, the formulation of analytical sciences, so the former is not at odds with the position that sociology as an analytical social science is not concerned with power, self-interest, etc. It is clear by definition, of course, that such a sociology is incapable of saying anything systematic about the mutual influences between economic, political, cultural, and psychological processes, and therewith about many processes of social change (see Guessous 1967). Thus the Parsonian program for sociology bypasses those problems in which traditionally, as Parsons is very well aware, most sociologists have been intensely interested (Chazel 1974, p. 169). Yet those who would like to continue dealing with these aspects of social life should not try to impress Parsons by mustering ever more evidence documenting their empirical importance. Instead, they should take a closer look at Parsons's reasoning in support of an analytic sociology.

Parsons advocates an "analytical factor view" (which, incidentally, is for all practical purposes identical with the position adopted by Carl Menger in his controversy with Gustav Schmoller, the famous *Methodenstreit* [Menger 1963; Burger 1976, pp. 140–50]), for two reasons: first, he is against one comprehensive social science, which he calls an "encyclopaedic sociology," and second, he wants to avoid an empiricist interpretation of the methodological status of social science theory. Now, while there is no doubt about the soundness of the latter intent, the rejection of an encyclopedic sociology by Parsons is not very closely argued. The drift of the reasoning seems to be, although there is nowhere a completely explicit statement, that the advocacy of such a comprehensive social science involves an empiricist position. An encyclopedic conception is always associated by Parsons with an empiricist attitude. Its compatibility with a nonempiricist view is never considered (e.g. 1934, p. 535; 1937, p. 454). Now, as we have seen, Parsons defines "empiricism" as the erroneous position that it is the task of any science to produce a full explanatory account of a given part of reality in all its concreteness, and the "fallacy of misplaced concreteness" as involving the mistaken belief that a given theoretical system does indeed accomplish

this. However, the fact that every theory must be “abstract” cannot be used to argue that, whenever someone complains that a given theory does not explain enough and must therefore be modified, he commits the empiricist mistake of assuming that a theory must be capable of explaining everything. Yet some conclusion of this kind seems to be in Parsons’s mind (1934, p. 543). It is therefore well worth emphasizing here the elementary fact that the advocacy of a social science which is more comprehensive than either economics or sociology in the Parsonian sense (i.e., one which explains “economic” or “sociological” as well as other “social” phenomena) is not tantamount to wanting to account for *everything* within one theory. Even Schmoller, an archempiricist in Parsons’s view, recognized the necessity of abstraction. His objection to Menger was that he used the wrong abstractions, not that he used abstraction at all (Schmoller 1883, pp. 979–80; Burger 1976, pp. 148–49).

Parsons’s second main reason for recommending the analytical factor view is his belief that it “avoids the necessity . . . of sacrificing entirely the theoretical work of generations of economists” (1935*b*, p. 665). Having seen the results, unsatisfactory both to economists and to sociologists, of attempts to use economics as a bridgehead for the construction of a comprehensive social science (an encyclopedic sociology), he became convinced that it would be better to keep economics substantively intact (1935*a*, p. 421) while changing its methodological status to that of an analytic science, and that it would be most prudent and promising to develop sociology along similar lines. However, it may be wise to be somewhat reserved with regard to this idea; for, if falsifiability of their synthetic statements is taken as the criterion which distinguishes scientific theories from others, there arises a difficulty for any sociology which takes the course of an “analytic” economics.

The nomological statements of economics are ultimately statements about the behavior of individuals. Yet they are made on the “assumption” that this behavior is economically rational (Friedman 1959; Nagel 1963). Thus the theoretically derived interrelations between economic phenomena, or rather the economic aspects of concrete phenomena, always rest on the presumption of economically rational behavior. The claim that they adequately describe the interrelations among the economic “aspects” of the behavior of various individuals—constituting the abstract economic “sphere” of concrete social life—amounts to the claim that economic rationality is the “law” of the economic “sphere,” that as far as the economic aspect of their concrete being is concerned, humans are “economizing” animals. Now, to “assume” this, in Parsonian terminology and in accordance with his ideas about the division of labor among the several postulated analytic social sciences, means to treat it as a datum, not as an object of inquiry. To investigate it would be to commit the empiricist mistake: “It is true that in any empiricist view economics must have a psychological foundation” (Parsons

1935*a*, p. 431). Yet not to investigate it means to refrain from testing the idea that the interrelations of the economic sphere are indeed of an “economizing” nature. Simply to *assume* that they are makes the theory unfalsifiable in principle. Any analytical political science or sociology which would “assume” that in their political and “social” aspects men are “politicking” and “solidarizing,” respectively, would share the same defects. Furthermore, even if an attempt at testing the theory would be made, using the Parsonian conception it is always possible to explain the divergence between the claims of a special social science and empirical reality as due to factors which are beyond its scope and therefore must be accepted as “given” (Parsons 1935*a*, p. 421). The only thing that could be tested is the synthetic explanation of concrete phenomena by the combined totality of all the special social sciences. Yet how can these sciences be developed to begin with if they are not testable?

To repeat, then: It is Parsons’s position that concrete social phenomena ought to be viewed as composed of diverse abstract (analytical) elements, or as having different aspects, such as the economic, political, psychological, sociological, etc. The special social sciences deal each with the ways in which the concrete social phenomena are related to each other in one aspect only. Thus, the economic elements of different concrete phenomena are treated as standing in invariant relations to each other and the same is claimed to be true of the political, sociological, and other aspects. These relations are *sui generis* for each kind of element, and they define the economic, political, social (cf. Martel 1971, pp. 180–84), etc., “spheres” of which concrete social life is made up. In other words, each sphere is governed by laws of its own, its own special dynamics. “The independence of economics is founded not on the [concretely] separate existence of economic forces, or even actions, but on the fact that the economic elements in action are subject to different causal laws from the other elements—that is, vary independently of them” (Parsons 1934, p. 538). Yet this independent variation is only of a relative nature. For the economic, political, etc., aspects of concrete phenomena are also related to each other, so that variations in, for instance, the economic aspects of certain phenomena have repercussions not only for the *economic* aspects of other phenomena, but for their political, psychological, etc., aspects as well. “Of course the ultimate concrete unity of human social life implies that the division of fields of the social sciences is only possible in terms of a coherent analysis of the whole into primary elements which must be thought of as related to each other in definite ways” (Parsons 1935*b*, p. 667, n. 5).

Parsons never questions the legitimacy of the conception that concrete action is a composite of elements belonging to different spheres, and that the relations among the economic “aspects” of several phenomena are somehow governed by different laws than are the relations among their

political, social, or other aspects, and the relations between differing aspects. Just as Menger (1963, pp. 69–70, 77, 86) operated with the assumption that concrete social life is composed of different abstract “spheres” each with laws of its own, so Parsons came to postulate a number of analytic systems which (according to a rather cloudy expression) “interpenetrate” each other, while each has emergent properties of its own. However, it might be argued that all the talk of the different “aspects” and “elements” of behavior is merely a misleading way of expressing the simple facts that behavior is guided, on the part of the actor, by a variety of considerations which vary with the circumstances; that all interrelations of social life are due to human behavior which is subject to the same laws everywhere in social life; that the latter is not an intersection of a variety of realms each with its own laws; and finally, that the science of this behavior would provide “the body of principles which is equally applicable to any . . . phase of human life in society” (Parsons 1935a, p. 452). Certainly a science which explains how and why people act differently under different circumstances would be basic to all special social sciences; yet it certainly would not be an encyclopedic sociology of Comtean vision (see Runciman 1970, pp. 1–44).

Whether he is aware of it or not, what is behind Parsons’s view is the idea of a somehow “natural” connection between a (necessary) social function and a specific type of action, for example, the idea that the supply of men with various goods “naturally” or “necessarily” involves “economizing” (economically rational action) (see Polanyi, Arensberg, and Pearson 1957; Polanyi 1957; Hopkins 1957; Pearson 1957) and that economic phenomena therefore can be treated as “intrinsically” interrelated in certain ways, and that these intrinsic interrelations are somehow closer, more intimate, or more natural than the connections of economic phenomena with other aspects. Advocates of this idea usually postulate that empirically these intrinsic interrelations occur rarely if ever in their purity, but usually or always in a distorted fashion. The interference of other action aspects with the full actualization of the intrinsic economic relations is considered to be the source of this distortion. In other words, the intrinsic laws of the diverse spheres collide, so to speak, in concrete action, preventing each other from full realization. Economics, political science, etc., are the sciences establishing these intrinsic interrelations which are concretely observable in relative purity only in relative concrete isolation from each other, that is, when institutional arrangements are such as to allow the relative predominance or overwhelming influence of the laws of one sphere. Yet the empirical absence of the requisite type of institutional arrangement and the resulting empirical “distortions” (which are really intermixtures) do not affect or deny the claim of the intrinsic connections’ existence. They merely pose the problem of their empirical “combination” and “collision.”

The fundamental mistake involved in this conception, and the source of

all its problems, is what Polanyi (1957, p. 270, n. 1) has called the "economistic fallacy," namely, the assumption of a natural link between a certain type of action and a particular social function. Thus the provision of men with goods may be accomplished by actions not at all guided by economizing considerations (Mauss 1969; Hopkins 1957; Polanyi 1957; Pearson 1957). This shows that there are no intrinsic relations between the economic aspects of concrete phenomena, and that an analytic economics therefore cannot describe such connections. Naturally the same objection must be raised against an analytic sociology: The war of all against all may be avoided by all sorts of means; the social function of coherence is not "intrinsically" accomplished by being "moral" or "solidarizing."

#### IV

This paper cannot be concluded without a look at Parsons's development after the publication of *The Social System*. For in *Economy and Society* (Parsons and Smelser 1956) he claims to have adopted a view which is different from the one advocated in *The Structure of Social Action* and still underlying *The Social System* (see Caillé 1970). The distinction which he draws between the two positions is as follows: The old view, consciously following Pareto's lead, held that economic theory as an abstract analytical system deals with *some* of the variables which determine concrete behavior in the economic as well as in all other concrete sectors of social life. For a full explanation of the empirical phenomena, it must be supplemented by a number of other distinct abstract theoretical schemes, which deal with the other significant variables of social life (Parsons and Smelser 1956, p. 6). The new view, in contrast holds that economics should be regarded as:

the theory of typical processes in the "economy," which is a subsystem differentiated from other subsystems of a society. The specifically economic aspect of the theory of social systems, therefore, is a *special case* of the general theory of the social system. . . . But the *basic* variables operative in all the special cases are the variables of a more general theory. The peculiarity of economic theory, therefore, is *not* the separate class of variables it employs but the *parameters* which distinguish the special case . . . we call economic in the use of the general variables of social theory from the other important types of special case. [Parsons and Smelser 1956, p. 6; for a critique, see Hopkins 1957]

Parsons does not say in what respect he regards his new position as superior to the old one. Has he changed his ideas about what constitutes the fallacy of misplaced concreteness? Or is he no longer opposed to the idea of an encyclopedic social science? Interpreting economics as a "special case" presumably implies the existence or necessity of such a general science, a general theory of social systems (Chazel 1974, pp. 108–10). Now it might seem that such a science should accommodate the phenomena of power,

self-interest, and normative consensus in their interrelatedness. Yet the theory put forward by Parsons does not do so. Instead, partly through the redefinition of terms (e.g., “power”), and in any case without any attempted explicit justification, power and self-interest are placed outside the scope of the theory which, as a result, considers normative integration to be the essence of social order (see Giddens 1968). But this can no longer be defended as an analytical abstraction since the general theory must be comprehensive. What the early Parsons guardedly treated as the reality *sui generis* of an analytical sphere appears to have become the essence of concrete society (Chazel 1974, pp. 169–70).

In spite of their derived status, Parsons still wants to provide for the distinctive identity of the several special social sciences. This he now attempts to do by apparently reverting to the previously denounced position that they each deal with a specific compartment of concrete social life, namely, a differentiated concrete subsystem of a more inclusive concrete social system (a society; see the quotation above, and also, Parsons and Smelser [1956], pp. 6–7, n. 4; for a divergent statement, see Parsons [1966], p. 104). In this vein, economics and political science are interpreted as dealing with the role behavior of actors in *normatively integrated* subsystems of an overall normatively integrated concrete social system (Parsons 1966, p. 73). This, at least, is one way of making sense out of Parsons’s statements. For at this point his argument becomes rather confusing. It is no longer clear whether and when the reference is to abstract or concrete systems and sub-systems.<sup>1</sup> In any case, however, what Parsons’s new position means is that the theoretical distinctiveness of economics, etc., is tied to the contingent circumstance that in some societies the production and exchange of goods occur through specialized economic roles rather than as one part of more diffuse ones (Pearson 1957). This is obviously implied in the statement that “empirically most so-called ‘economic’ processes must be regarded as resultants of economic and non-economic factors. In the cases which are most favorable empirically to ‘purely economic’ analysis, the phenomena are always a resultant of the operation of the *general variables of systems of social action* and of parameters specifically relevant to the economic case. In cases less favorable to purely economic analysis, the phenomena are a result of *the same variables* and of *other* parameters less relevant to the economic case. Thus, in the strictest sense of general theory, it is incorrect to speak of ‘economic variables’” (Parsons and Smelser 1956, pp. 6–7, n. 4). In other words, if the conditions are right, men are guided exclusively or

<sup>1</sup> The source of this difficulty lies in the fact that for Parsons the transition from system to subsystem usually also involves a shift in the level of abstraction. However, he is by no means consistent, and the various relations between systems and subsystems are confusingly linked to a variety of degrees of abstraction. Indicative of the problem is Rocher’s diagram (1974, pp. 59–61), which even in the translator’s reworked version makes no sense; e.g., there is no room in it for the social system.

predominantly by economic considerations. Empirically this happens to be the case in the concrete modern economy, and here economics does good service. Under different circumstances, however, it does not work so well (see Cuisenier 1969, pp. 582–83).

On the basis of this methodological position, sociology as a special science would have to deal with a differentiated social subsystem of its own, the recently named “societal community,” which presumably is the set of roles in which the problem of order (integration) is tackled (for a criticism, see Caillé [1970], p. 199). Of course, this raises the questions of the exact empirical referent of the societal community and its concrete distinctiveness as a subsystem. If the subsystem has not yet come into its own, sociology as a special discipline apparently will have to wait (Rocher 1974, pp. 62–63, 73). Yet perhaps a more important objection would be that Parsons still (and without either methodological or empirical justification) considers the problem of order in society essentially one of normative integration. This is clearly at the basis of the idea of a cybernetic hierarchy in which systems high in information (norms) control systems high in energy. A discussion of this particular conception, however, would go beyond the limits of this essay with its implication that the problem of social order ought to be recognized as the problem of the interrelations between the phenomena of self-interested exchange, domination, and normative conviction. It is clear that this problem has no solution within an analytic sociology of the Parsonian type. For sociologists, then, the alternatives seem to be either to abandon the problem or to abandon analytic sociology—at least in its Parsonian version.

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## Comment on Burger's Critique

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Professor Burger's article, on which the Editor has asked me to comment, presents a tantalizing combination of essentially correct insight and mistaken interpretation. It does, however, constitute a higher level of theoretical sophistication and critical seriousness than has a large proportion of the negatively critical discussions of my work. It gives one some specific issues on which to "bite."

Burger's two main themes, quite clearly stated in the abstract at the beginning of the article, concern my views about the relation of moral consensus to social order and about the "status of sociology" (his term; I should prefer to say "theory in" sociology) as analytic. Burger discusses the two problem areas in the above order; I should prefer, in this commentary, to interweave them.

Like his article as a whole, Burger's statement that I have contended that "sociology must be an 'analytic' science" is to my mind both correct and incorrect. He is quite correct that, from early in my career, I have been greatly impressed by the importance of analytical abstraction in science in general and social science in particular. This began under the influence of such writers as A. N. Whitehead, Max Weber, Vilfredo Pareto, L. J. Henderson, and Joseph Schumpeter. It is a view which I have never abandoned.

Perhaps the reference above should even have been to the theory *of* sociology, rather than to theory *in* sociology. The essential point is that in the first instance it is for me a theoretical scheme which is analytically abstract. In empirical investigation it is seldom possible to restrict attention either to the variables of a strictly defined analytical system, or to those combined with parameters so defined as to exclude interdependence with variables outside the system, the parameters thus being treated simply as "given."

Thus, to take Burger's special example, I see no need for apology—and indeed he supports me on this—for having been long concerned with the problem of social order, precisely as a problem with respect to which one must take into account not only sociological considerations at the least, but also political and economic ones. It is easy to extend the list to include cultural and psychological considerations, and on various occasions I have explicitly done so.

Burger is of course right that sociology, like many other intellectual disciplines, has long been caught in the dilemma of whether to define its subject matter in analytical terms or in concretely empirical terms. There is simply no way of making the two match neatly so that it is possible to have the best

of both worlds. Thus, the issue between Burger and myself is clear. As *theorist* I have chosen the analytic path, the consequence of which is that, in dealing with many if not most empirical problem areas, it is necessary to invoke a *plurality* of analytical schemes. The alternative, which Burger chooses, is to treat the empirical problem area as the main determinant of the structure of theoretical schemata; thus, rather than sociological or economic theory, there should be a theory of social order, a theory of social conflict, a theory of the distribution of wealth and the like. This I would define, from the point of view of theory as consisting of analytical systems, as the *eclectic* position.

Thus Burger is essentially correct in his contention that, in the analytic conception of sociological theory which I have supported, a purely sociological explanation of the problem of social order cannot be attained. This is most definitely *not* because economic and political and also psychological and cultural factors are empirically irrelevant to the problem of social order, but because they are analytically defined as not belonging to sociological theory.

I gather that Burger extends his objections to my analytical conception of sociological theory to economic theory as well, as is made evident, later in his article, by his espousal of the critique of most "main line" economic theory by Polanyi, Pearson, and Hopkins. Thus the issue of analytical abstraction versus what I have called the theoretically eclectic point of view seems to be clearly joined. To dispose of my position Burger would have to undertake the formidable task of destroying the theoretical legitimacy of a long line of economic as well as sociological theory.

Indeed, usage of the term "theory" has never come to be stabilized as between these two alternative meanings. My personal position has long been that *both* are essential, but it is imperative that they should be clearly distinguished from and related to each other. To help reduce confusion, I have often suggested that, in the latter context, we should speak of "empirical generalization," or sometimes empirical "explanation" of the set of phenomena in question.

When it comes to the disciplines as such there has, historically, been a strong tendency to try to combine the two usages, but for cogent reasons the analytical conception has become dominant in defining the more "basic" aspect of theory of the central disciplines, such as physics, chemistry, genetics, evolutionary biology, psychology, economics, and sociology. Even though, however, there are pulls in both directions and strong pressures to "tip" in one direction or the other, I think that there are ways of mediating between them.

If my interpretation is correct, Burger sees the dilemma as inexorable, with reference to the conception of the scope of sociology as a discipline. He is quite correct in his judgment that, if, without aid from other disciplines,

sociological theory is to be *the* instrument of solving the problem of social order, as he poses it, it cannot be an analytical theory within the confines to which I have thought it ought to belong, especially as it was both distinguished from and related to economic and political theory. To me the principal index of this is, as noted, his apparently unquestioned acceptance of the attempt of Polanyi, Pearson, and Hopkins to relativize economic theory to the analysis of particular economies, in the best "historical" tradition of such writers as Werner Sombart.

It seems to me, however, that the dilemma is not quite so stark as Burger paints it. In this connection it is significant to me that Burger concentrates overwhelmingly on my earliest work, notably, the *Structure of Social Action*, which was published just 40 years ago, in 1937. He does briefly recognize that there has been a phase of, as he puts it, "new" work, namely, my book *Economy and Society*, written jointly with Neil Smelser, published in 1956 and now more than 20 years old. I happen to think that there has been considerable theoretical development in the whole extended intervening period, but that it has been development from the 1937 position through that of 1956, rather than a "conversion to" a drastically different position.

Burger is right in choosing as the focus of what was new in 1956 relative to 1937 the position first stated in *Economy and Society*. That position was characterized by the step from the conception of economic *action*, as a *type* of individual action in which economic orientation is conceived to be paramount, to the conception of an *economy* as a *social system of action*, which could be conceived to be a primary functional subsystem of a total society. Neither Weber nor Pareto—who both used the category of economic action as a subtype of a wider category, though somewhat differently—ever conceived that its "determinants" were confined to economic variables. Its hallmark was rather that, whatever the components, and they could be numerous, they were *organized* to bring about the *primacy*, as I have called it, of economic orientation, including its rationality.

The paradigm of *Economy and Society* was derived essentially by generalizing from the conception of a type of action to that of a relatively highly differentiated type of social system, differentiated and specialized, however, with respect to its functional significance in a wider social system, the society, in this case the function of economic production. The level and modes of differentiation of this subsystem relative to others in the same society, and the relationships of noneconomic variables to each other and to the economic, involve more empirical questions than does defining an economy, as Smelser and I did. The same is true of the principal analytical relations of the economy to the other subsystems of a society, which I call the polity, the societal community, and the fiduciary system.

A main line of theoretical work since the completion of *Economy and Society* has been to attempt to generalize from the concept of an economy, first

to those of the other subsystems of a society which could be argued to stand on essentially the same level of abstraction. I have applied this type of analysis to the polity, the societal community, and the fiduciary system. Each of these is conceived to have its own set of "primacies"; for example, for the polity, collective effectiveness as distinguished from production of utility for units in the economic subsystem, and for the societal community, the primacy of solidarity, in something like Durkheim's sense. The societal community is also conceived to consist of many more or less solidary subgroups, but at the same time a society as a whole has its own levels of solidarity as institutionalized in its societal community.

This mode of analytical specification has furthermore gone well beyond the definition of functionally differentiated subsystems of the society, trying to specify the categories of inputs and outputs involved in their interrelations with each other, and to define and analyze the operation of generalized symbolic media operating within and between them as regulators of their operation and relationships.

In a comparable type of analysis, which is still seriously incomplete, I have tried to go beyond the social system, attempting to place it in a theoretically determinate setting. This analysis is comparable to that by which Smelser and I attempted to place the economy within the social system. Besides analyzing the social system itself, I have tried to relate it to the cultural system and to two components of what I have called the individual, namely, first the personality system, and second, and recently, the "behavioral" system. These complicated themes cannot be further pursued here, but they are on record (see introductory material in *Theories of Society* [1961]; the three essays on social system media in *Politics and Social Structure* [1969]; *The American University* [1973]; and Lidz and Lidz [1976] in *Explorations in General Theory in Social Science*).

Burger starts with the stark dilemma, as I have stated it, between a maximally isolated "system" of analytical theory and what I have called an eclectic mobilization of all concretely relevant variables for the solution of a set of empirical problems. *Of course* the former "theories" are, in the nature of the case, neither verifiable nor falsifiable. This is true in a sense parallel to that in which the "deep structures" of a language cannot be demonstrated to be either "grammatical" or "ungrammatical"; they are *components* in the basis of such judgments, not themselves alone constitutive of them. The judgments apply to sentences, not to deep structures.

The kind of theoretical development of which I have given the barest sketch builds the components of the most abstract analytical schemes, and various levels of their approach to concreteness, into a coherent web of theoretical relationships. Put a little differently, at their various levels the components are built into a conception of *order*, of a theoretical rather than a social-empirical nature. This order, however, must of course be congruent

with the "order of nature" which I interpret to include human life and, in the technical sense, *action*.

The congruence of which I speak, however, is not to be interpreted to imply one-to-one correspondence, except for rare, but paradigmatically very important, limiting cases, of which two of the most prominent in the history of science have been the solar system, as treated by Newton, and the inheritance of blossom color and seed type of peas, as studied by Mendel. In the field of the action sciences there are a few, but very few, cases of "perfect markets" and, in the case of analytically defined sociology, of "perfectly solidary" collectivities.

From the lack of one-to-one correspondence in this sense there has developed what, from the kind of perspective with which Burger presents us, amounts to a slightly modified stark dilemma. If one insists on the "immediacy" of theoretical correspondence between observable facts and schemata of theoretical interpretation, it follows that we need multiple levels of order in the world of empirical experience. The order of analytical theory is one such level, and it is in the nature of the case "removed" from the directly observable order of empirical phenomena. In my understanding, all of the principal older sciences have taken this path. Thus Newtonian theory was relatively adequate for the solar system, but without basic modifications it could not cope with subatomic physics or with the kind of astrophysics which stimulated Einstein. In biology, Mendelian genetics had to be elaborated into something far from the simple 3:1 ratios, even before the appearance of microgenetics and DNA.

It seems to me that Burger advocates a view which would lead to vetoing, for our field, the main path of theoretical progress in science which has come to dominate the developments since the first main syntheses. To me the fruitful way of dealing with the dilemma he has posed is to transcend it, not to accept one horn and repudiate the other. Furthermore, it seems to me that the path I have outlined extremely sketchily in this comment is not more "metaphysical" (to quote his abstract) than is insistence on the importance either of theory in science generally or of empirically established facts.

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