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Bridging Levels of Systemic Organization[1]

by Robert A. Rubinstein and Charles D. Laughlin, Jr.

THE RECENT ATTEMPT by Paredes and Hepburn (1976) to relate the split-brain phenomenon in man to cognitive-behavioral features of interest to anthropologists has raised a number of interesting questions. Several discussants of their paper have dwelt at length on Paredes and Hepburn's interpretation of the data associated with the split-brain phenomenon (e.g., Armstrong 1976, Harnad and Steklis 1976, and Rohrl 1976), and TenHouten (1976) has provided a much needed corrective in this area. We feel that their paper raises a wider issue which, given the increasing movement towards and integration of "traditional" anthropological approaches with neuroscientific data, is of profound importance. This is the issue of how to bridge levels of systemic organization. Although we have grounded our discussion in biogenetic structural theory, we believe that this issue is quite general in nature and must be faced in any attempt to integrate behavioral scientific and neuroscientific approaches. We shall freely alternate in this discussion between empirical and philosophical considerations in an effort to clarify the implications of biogenetic structuralism for the convergence of anthropology and the philosophy of science.

Social scientists have recourse to a number of different "levels" of structure in their explanations of social phenomena (e.g., neurophysiology, cognition, psychology, "deep" and "surface" structure, social structure). Biogenetic structural theory has utilized several of these levels, particularly those pertaining to brain function, behavior, social action, and ecology. The movement among various levels of systemic organization raises two important questions: (1) What is the ontological-epistemological relationship between levels of systemic organization? (2) Has biogenetic structural theory anything to offer to an understanding of the relationship between these levels? Consideration of these questions necessarily involves the examination of the process of theory reduction as traditionally conceived and, to a minimal extent, the nature of explanation in science.

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ANTHROPOLOGY AND THE PHILOSOPHY OF SCIENCE

The most profitable approach to science, and to the analysis of the process of sciencing, is, in our view, a disciplined integration of theoretical-empirical considerations and philosophical reflection. The two approaches are not only compatible, but mutually beneficial (Rubinstein 1974; Rubinstein, Laughlin, and McManus n.d. *a*, *b*).

Social scientists often remark that bureaucratic policy statements based upon social scientific formulations reveal a lag of ten years behind theory and research (e.g., Gardner 1974). Similarly, philosophers of science may well bemoan the lag between developments in their discipline and anthropological work based upon it. For example, in their desire to create a scientific archaeology, archaeologists rushed to embrace the received view of explanation (see Suppe 1974) just as the philosophical community was coming to realize the inadequacy of that view (see Binford and Binford 1968, Fritz and Plog 1970, Watson, LeBlanc, and Redman 1974; cf. Suppe 1974, Salmon 1971, Rubinstein 1974, Rubinstein and Donaldson 1975). Perhaps a more telling example of discrepant developments in anthropology and the philosophy of science is offered by the common abuse by anthropological theorists of the relationship between deduction and induction (see Laughlin and d'Aquili 1974: chap. 6). Some theorists proceed as "mind-less empiricists," firm in the belief that the social world of man will order itself if we but collect sufficient data. Others feel that their formulations about the world are complete if they are internally consistent and account for the data from which their formulations were drawn. Neither position is tenable in the light of current philosophy of science. Moreover, from the biogenetic structural perspective we must view the process of scientific theory construction as involving an alternation between deduction and induction (Laughlin and d'Aquili 1974: 136–46).[2]

The fundamental problem in bridging from philosophy of science to anthropology is precisely the existence of discipline boundaries. Anthropologists tend to rely upon the philosophy of science as a source of "solutions" to logical and methodological problems, rather than as a body of rational problems with which to interact. The effect has been to establish a one-way exchange between the two disciplines. Anthropologists often uncritically accept philosophical positions and introduce these into the anthropological literature via argument from authority rather than rational dialectic. One serious result is that anthropologists have held the philosophy of science at arm's length, to the detriment of both disciplines.

Over the past two and a half decades, many philosophers have realized the value of drawing empirical considerations—largely historical—into their discussions of the philosophy of science (see, e.g., Kuhn 1970, Feyerabend 1970*a*). As Suppe (1974) has pointed out, the introduction of historical considerations into the philosophy of science has provided the impetus for many of the recent developments within the discipline. Unfortunately, philosophers have continued to view questions pertaining to the biopsychological basis of sciencing as though they were totally irrelevant or satisfactorily answerable by means of introspection. While Reichenbach's (1939) distinction between the context of discovery and the context of justification is now conceived as viable only as one of analytical convenience, in practice it is rigidly maintained.

Piaget (1969, 1973*a*, *b*) has been particularly forceful in taking philosophers of science to task for their neglect of empirical (and biopsychological) considerations. He has argued (1973*a*:12) that, when raising biopsychological questions,

> Introspection alone is not enough, because it is both incomplete (it grasps the results of mental processes and not their intimate mechanisms) and distorting (because the subject who introspects is both judge and party, which plays a considerable part in affective states, and even in the cognitive sphere where one's own philosophy is projected into the introspection).

Biogenetic structuralism requires that science and sciencing be subjected to a biopsychological analysis based upon empirical observation and explanation (Laughlin and d'Aquili 1974). Although it may appear a paradox for an understanding of sciencing to require the use of science, it is no more so than for humans to use their brains to gain an understanding of their brains. The remainder of our discussion will assume the necessity of empirical considerations—particularly biopsychological ones.

THE PROBLEM OF THEORY REDUCTION

A principal source of confusion in bridging levels of systemic organization via social theory is the question of theory reduction. After sketching a biogenetic structural theory of science, we shall show that the received view of reduction in the philosophy of science is inadequate for both philosophical and empirical reasons. We shall then offer an alternative framework for theory reduction, one that allows us to utilize materials from biopsychological research.

A Biogenetic Structural Theory of Science

A central concept in biogenetic structural theory is that of neurognosis (Laughlin and d'Aquili 1974, d'Aquili, Laughlin, and McManus n.d.). The term neurognosis denotes both the patterning of neuroanatomical tissue in the nervous system and the information coded via the neural structure as a model. It is by integration of sensory input and these neural models that *Homo sapiens* experiences reality. Neurognostic models, it is argued, are initially patterned by the genes and subjected to physical modification through the empirical modification cycle. This cycle operates as follows: On the basis of the information coded in the models, the organism generates probability statements (expectations) about the results of its interaction with its environment. On the basis of those expectations, the organism then acts on the environment. It receives feedback about the results of that action. Depending upon the degree of discrepancy between the expectation and the environmental response, the neurognostic model is modified so that it comes into more adaptive isomorphism with the environment.

Sciencing is the extension (usually into consciousness) of these fundamental biopsychological processes. Therefore, when science operates at its optimum, it does so by the continual refinement of its theoretical models, as we have said, through an alternation of induction and deduction. Moreover, the feed-forward function of the empirical modification cycle is replicated in the conscious insistence upon prediction as one of the most important criteria of a "good" scientific theory. Our view, in a word, is that sciencing is a special function of cognition, the process by which the brain constructs an adaptive model of its environment, and that no understanding of the process of sciencing is possible apart from the study of the cognitive function of the brain.

The Received View of Theory Reduction

As a first approximation of the received (or classical) view of theory reduction, we can say that a Theory A is said to be reduced by Theory B only when A may be deductively derived from B (Nagel 1961). Often, however, there are terms in A that have no (obvious) corresponding terms in B. Under these circumstances, "bridge laws" (statements) are supplied that specify the relationship of terms in A to terms in B. The received view of theory reduction then states that Theory A is reduced by Theory B only when A can be deductively derived from the conjunction of B and the bridge laws. Reduction by deductive derivation is seen as a special case of deductive nomological explanation (Hempel 1965, and below) in which a theory, rather than an event, is the explanandum and another, more powerful theory and the bridge laws are the explanans (see Nagel 1961).

This account of theory reduction in science has, until recently, enjoyed widespread acceptance within the philosophical community; disputes concerning its appropriateness have for the most part revolved around the problem of how the bridge laws are to be construed (Suppe 1974). Ager, Aronson, and Weingard (1974:119) point out that "the necessity of bridge laws has been largely taken for granted in discussions of reduction, and controversy over them has focused on the choice between the three alternative analyses Nagel originally offered: bridge laws are either logical, conventional, or factual

[2] The abuse of this distinction may be due, in part, to the common misunderstanding of the nature of induction and deduction. As Rudner (1966:66) puts it, "The vulgar notion that *induction* and *deduction* are 'opposites' as well as its equally untutored concomitant notions that deduction is 'going' from the general to the particular, and induction is 'going' from the particular to the general, are not only quite mistaken, but seriously misleading.... There is surely no longer an excuse for repeating such bits of foolish folk-lore 'logic.' "

connections." Several philosophers have criticized the received view of theory reduction on philosophical grounds, and some have proposed alternatives (one of which will be discussed shortly). Rather than present the philosophical objections here, we will demonstrate the inadequacy of the view on biopsychological grounds.

The conception of theory reduction by deductive derivation has some consequences that are unfortunate in the light of current understanding of the biopsychology of cognition. The view says that if we have reduced Theory A by Theory B (e.g., reduced classical mechanics by relativity theory), then we understand why Theory A produces usably accurate predictions for a limited set of phenomena; yet, at the same time, we understand that Theory A is wrong and unnecessary. One corollary of this view is the notion that the reduction of one theory (say social theory) by another theory (say neurophysiological theory) renders the concerns and formulations of the reduced theory frivolous. (Why study social phenomena as suggested by the weaker theory when this second, more powerful theory explains those things and more besides?) A second corollary is the notion of the incompatibility of meaning between theories (so-called intertheoretic incommensurability). We shall discuss these notions below.

The Structural Basis of Theory Reduction

As we have suggested, sciencing must be viewed as an extension of the basic biopsychological process of cognition and examined in the light of our understanding of this process (see Laughlin and d'Aquili 1974, Rubinstein, Laughlin, and McManus n.d. *b*). Several features of this process are particularly germane here. Piaget (1962, 1971, 1973*b;* see also Flavell 1963, Ginsburg and Opper 1967, and Feldman et al. 1974), Harvey, Hunt, and Schroder (1961; see also Schroder, Driver, and Streufert 1967), and others have demonstrated that cognitive systems develop by passage through a series of invariant stages of complexity. The organism's conceptual structure develops by bracketing—by allowing the organism to make finer and finer distinctions among sensory stimuli, thereby forming and reforming the conceptual schemata that are used for the evaluation of objects or events abstracted from experience. This process of differentiation and integration results in the formation of coherent structures that define a developmental stage. Importantly, these stages are ordered hierarchically in development so that earlier stages are necessary but not sufficient conditions for later ones. Furthermore, structures acquired (constructed) in earlier stages are incorporated into later ones by transformations and elaborated by empirical modification.

Theories, we would argue, are schemata that are used for evaluating various stimuli in the environment. Moreover, the analysis of the development of scientific theories reveals that they may be viewed as coherent structures produced by the same processes as underlie other aspects of cognition (Piaget 1969). That scientific reasoning characteristically exhibits the same logical structures as nonscientific thought only serves to reinforce this view (Horton 1967, Laughlin and d'Aquili 1974). We are suggesting, then, that for both the diachronic, developmental progression from one theory to another and the synchronic reduction of theories, the features of the ontogenesis of cognitive structures that we have outlined are *strictly preserved*. Thus a biopsychological view of theory reduction must make explicit that the reduced theory is necessary to the reducing theory. This means that the move from one theory to the next involves the transformation, incorporation, and elaboration of the reduced theory into the new framework of the reducing theory; unquestionably, the meanings of some terms will be in part transformed (see below) by the new structural matrix in which they are embedded.

Pursuant to this line of reasoning, we argue that theory reduction in science is analogous to the transformation from one developmental stage to the next in general cognition. It follows the specific course of development taken by the transformation and elaboration of conceptual schemata at any particular stage and the subsequent inclusion of these schemata in higher levels of structural development. This view seems fundamental both to conceptual theories of development (e.g., those of Piaget or the conceptual systems theory group) and to a biogenetic structural theory of the relationship between neurophysiological events, conceptual structures, and sciencing. (Feldman et al. [1974] have been particularly perceptive in seeing this as fundamental to an account and measurement of conceptual development.)

In the received view of theory reduction, as we have seen, the reduced theory is unnecessary because we can explain all the phenomena for which it accounts by reference to the reducing theory. For many it follows, either explicitly or implicitly, that where interdisciplinary theory reduction is concerned, not only the reduced theory itself, but the entire set of concerns of the field of study whose theory has been reduced becomes superfluous. It is perhaps this widespread conception of theory reduction, more than any other factor of logic, which has led behavioral psychologists to reject attempts by cognitive psychologists to deal with behavioral phenomena (Skinner 1969), social anthropologists to rally periodically against "psychologisms," and Durkheim, Lévi-Strauss, and other notables to look askance at possible biological bases of structure. To the detriment of all, exchange of theory, insight, and observation has been obfuscated.

Because it requires recourse to the process of equilibration between systems internal to the organism and the environment, biogenetic structuralism disallows any arbitrary constraints upon empirical epistemology. We would reject both the antireductionist position that sociological or psychological facts require sociological or psychological explanations respectively (Durkheim 1966) and the opposite, orthodox logical positivist position requiring science to reduce to "ultimate constituents," a view associated with the logical atomism of Russell (1956) and the early Wittgenstein (1961). The former position is based upon the naive conception of the nature of systems, summarized by the now classic cliché "The whole is more than the sum of its parts." The latter is based upon the equally naive view that a sufficiently diligent inquiry will divulge the concrete elements from which the higher levels of system are formed.

With reference to the first position, which might be called the "sociological fallacy," we agree with Buckley (1967) and Blalock (1969) that insofar as the parts and *the relations between the parts* of a system have been explicated, one has defined the whole. It is, in other words, the job of science to "reduce" to the parts *and* relations comprising a system. As Hinde (1970:7) has said:

> The material with which we start is usually at the behavioral level. If the prediction of behavior, given the antecedent conditions, was the sole aim, there might be no need to reduce to a physiological level: reference to underlying mechanisms would be unnecessary. But even if the complete prediction of behavior were possible, we should still have advanced only one stage towards its full understanding: a further stage would be reached if the regularities in behavior could be understood in terms of the physiological organization which they reflect. Thus hypotheses must be judged not only at the behavioral level, but also in terms of their compatibility with lower ones [and higher ones?].

To the logical atomist position there can be no better response than that of Whitehead (1960): that if (as we believe) the universe is constructed of organismic processes in an infinite concatenation of systems within systems, the search for a level of ultimate constituent elements is futile. The level or levels of systemic organization one chooses for analysis, therefore, is not a matter of social norm, but rather is logically entailed in the problem one has set.

In the life sciences we are concerned with the nature of organic systems, systems that are comprised of organized subsystems whose arrangements emerge in equilibration with their environments. From this and other considerations mentioned above we may stipulate what we will call the *rule of minimal inclusion: any explanation of behavior must take into account any and all levels of systemic organization efficiently present* ("statistically relevant," in Salmon's [1971] sense) *in the interaction between the system operating and the environment of that system.* The rule of minimal inclusion will require the theoretical consideration of systemic levels at least one step below and one step above the level or levels appropriate to the phenomenon being explained. Rather than requiring the obsolescence of reduced theories, the rule of minimal inclusion requires the structural merger of reduced and reducing theories when they account respectively for different levels of systemic organization and both levels are efficiently present in the behavior being explained. Thus, for example, the reduction of cognitive theory by neurophysiological theory would require the incorporation of both into a single theoretical structure. To reduce one theory by another, then, is to cut across theories (and often disciplines defined by those theories) so that the conceptually relevant notions in one theory are unified with the conceptually relevant notions in the other. It thereby becomes possible to speak of conceptual schemata and neurophysiological models ("engrams," "cell assemblies," and so on) in the same breath without threatening the poignancy of either theoretical approach. As a matter of fact, this view of theory reduction forces the scientist to focus attention more sharply on the several theoretical approaches impinging upon the problem at hand.

Applied to neuroanthropology in general, and to biogenetic structuralism in particular, this view of theory reduction provides two cogent points: (1) During the neuroanthropological (biogenetic structural) analysis of an anthropological problem, *it is not necessary* that the scientist abandon more "traditional" anthropological concerns. Rather, the relevant data are to be distilled from the traditional approaches, focused, and combined with data from less traditional approaches in the effort to resolve the problem at hand. (2) The data, insights, and theories from all levels of systemic organization contributing to the resolution of the problem at hand may and must be incorporated into a single, coherent (biogenetic structural) analysis.

Hanson (1958), Kuhn (1970, 1974), Feyerabend (1965), and others have argued that scientists working within a particular theoretical framework have no way of understanding the terms and phenomena of other frameworks. Hanson attributes this to the development of a gestalt, a way of viewing the world that prevents the scientist from seeing the world in the same manner as does his cross-theoretic colleague. He calls this phenomenon to our attention to emphasize the fallaciousness of the distinction between the context of discovery and the context of justification—he offers the distinction between "seeing" and "seeing that." The thrust of Kuhn's and Feyerabend's treatments of this problem has been to attack the "layer-cake" conception of the development of science; if a term (e.g., culture) has different meanings in Theory A (e.g., Taylor's evolutionism) and Theory B (e.g., Geertz's hermeneutics), in what sense can the latter be said to build on the former? Elsewhere (Rubinstein, Laughlin, and McManus n.d. *a*, *b*) we have shown that Kuhn's approach to this problem suffers from a lack of biopsychological sophistication. Here we will consider the problem only briefly in order to indicate how it articulates with our view of theory reduction.

Relevant conceptual material from the reduced theory is incorporated into the reducing theory. Unquestionably, not all of the content of theoretical terms will be shared, partly because of differences in the structural matrices in which they are found and partly because of differences in the problems being addressed. Neither will their contents be completely incommensurate, however, and while the exact degree of meaning overlap in a specific case is a matter of empirical-logical demonstration, we suggest that it is often great. The reason is a structural one. As Lévi-Strauss (1966) among others has noted, scientific concepts derive their meaning from their structured, conceptual surround—that is, from the structural context of the theory in which they are embedded. Now, if the two theories we are discussing have the relationship to one another of reduced and reducing theories, then, as we have shown, the reduced theory is structurally incorporated into the reducing theory, thus providing a single theoretical framework for relating the meanings of terms in the two theories. Part of what we are suggesting, then, is that the contextualists have presented us with a false dilemma because, in large measure, their accounts of the intertheoretic incommensurability of terms lack a biopsychological underpinning. The choice is not between complete and simple absorption of theoretical terms from one theory to another (which leaves us, as the contextualists have certainly demonstrated, with an unrealistic account of scientific progress) and total incommensurability of meaning of theoretical terms (leading to inevitably narrow gestalts). Nor, as some people have suggested, is there a compromise to be made via the logical structure of theories alone leading to partial commensurability of terms. Rather, we have suggested, at least partial commensurability obtains because of the nature of the biopsychological processes mediating scientific inquiry.

Reduction by Incorporation

Having found the received view of theory reduction wanting for a variety of biopsychological reasons, and having outlined a few of the biopsychological considerations we see as important to developing a more sophisticated account of theory reduction in science, we would like to summarize a philosophical approach to some of the problems raised.

Ager, Aronson, and Weingard (1974) and Aronson (n.d.) have outlined an approach to theory reduction that they have termed *reduction by incorporation.* Starting from the premise that "to be scientifically respectable, bridge laws should be more exposed to experimental test" (Ager et al. 1974:121), they demonstrate that the received account allows the adequacy of bridge laws to be decided on logical rather than empirical grounds. They propose that reduction can occur through the incorporation of diverse scientific systems by means of reducing theories (Aronson n.d.). To do this they modify the traditional account of reduction by replacing bridge laws with identity statements of the form (for the reduction of mental $[M]$ and neurophysiological $[N]$ events) $(P)[MP \supset (P = N_1 \vee P = N_2 \vee \ldots P = N_n)]$.[3] In the traditional view of reduction, the identity statements are premises. In this view, they are theorems which, along with the laws of the scientific systems being reduced, tell us the possibilities of identity. Leibniz's law allows the identities to be pinpointed (Aronson n.d.).[4]

Reduction by incorporation can be clarified by considering the specific case of reduction involving neurophysiological and mental material presented by Aronson (n.d.; see Ager et al. 1974 for a treatment of the reduction of empirical gas law and

[3] The logical operators used in this and the following formulae are $\vee$, alternation; $\supset$, the conditional; $\equiv$, the biconditional; and $=$, identity. They are read as in the following examples: (1) A $\vee$ B; A or B; (2) A $\supset$ B; if A then B; (3) A $\equiv$ B; A if and only if B; (4) A = B; A is identical to (the same as) B. A slash through a symbol (as in $\not\supset$) makes it negative.

[4] Essentially, Leibniz's law states that two properties or states that are the same in their behavior with respect to other properties or states will be the same property or state.

kinetic theory). Aronson proceeds: "(II) (P) $[MP \supset (P = \mathcal{N}_1 \vee P = \mathcal{N}_2 \vee \ldots P = \mathcal{N}_n)]$; (1) laws relating to neurophysiological states; (2) laws relating to mental states; (3) identity(ies) between a mental state and a neurophysiological state."[5] He continues: "If we discover for example that there are neurological and mental states that are related to other states in exactly the same way—say, $\mathcal{N}_{25} = f(\mathcal{N}_3, \mathcal{N}_4)$ and $M_{10} = f(\mathcal{N}_3,\mathcal{N}_4)$ and $f = f$ then we can eliminate various disjuncts in the consequent of (II) and pinpoint our identities, in this case (3) $\mathcal{N}_{25} = M_{10}$."

Aronson notes that the use of identities in reduction has certain desirable consequences not provided by the use of correlation—specifically, identities allow the use of Leibniz's law, and, further, "it becomes clear . . . that identities can explain things that correlations cannot because the former has implications which the latter lacks, viz.; (1) $M_{10} = \mathcal{N}_{25} \supset (x)\ (M_{10}x \equiv \mathcal{N}_{25}x)$ while (2) $M_{10} \equiv \mathcal{N}_{25} \not\supset (x)\ (M_{10}x \equiv \mathcal{N}_{25}x)$. Thus correlation cannot explain in any non-*ad hoc* manner why M_{10} and $\mathcal{N}_{25}$ are states of the same individual or are located at the same time and places whereas $M_{10} = \mathcal{N}_{25}$ renders such explanations elementary."

An important ramification of the reduction-by-incorporation view is that it allows the incorporation of information from diverse scientific systems in addressing the solution to a problem. A corollary ramification is that, unlike correlational approaches, this one does not support a dualistic ontology—that is, does not encourage what Whitehead called the "fallacy of the bifurcation of nature."

Since this approach to theory reduction allows the elaboration, transformation, and incorporation of concepts across theories, it is consonant with our present understanding of the development of conceptual systems in ontogeny. It is conducive, therefore, to a more sophisticated, biopsychological theory of scientific theory reduction.

SUMMARY AND CONCLUSIONS

We have presented some important features of the biopsychological processes underlying scientific inquiry. These features, we have argued, form a minimal set of primitives with which a sophisticated, biopsychological view of theory reduction must agree. We have shown that the traditional view of theory reduction is not compatible with these primitives and argued that a view compatible with them would eliminate several pernicious results of the traditional view. In particular, we have shown that the reduction of one theory by another does not require the obsolescence of the reduced theory. Also, we have shown that data and theories from different levels of systemic organization need to be structurally unified in order to facilitate the solution of problems in the life sciences. Structural unification of theories, including reducing and reduced theories, results in partial commensurability of meaning between terms of different theories. An extant philosophical approach to theory reduction has been outlined and presented as a possible logical infrastructure for a more biopsychologically sophisticated account of theory reduction.

Comments

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The question of how systemic levels are bridged in theory is a timely one for anthropology. Rubinstein and Laughlin have made two important observations in regard to this question: (1) that what is commonly known as reduction does not in fact "reduce" one theory to another and (2) that, in any event, higher-order systemic levels are as important in explanation as are the lower-order levels involved in reductive explanations. Although I heartily agree with both conclusions, I am less enthusiastic about the argument by which the authors arrive at the first.

My difference with Rubinstein and Laughlin is that they define the problem of bridging systemic levels as one of cognitive structure and employ cognitive theory to reach their first conclusion. I see the problem of bridging systemic levels not only as one of cognitive structure, but rather as one of the relationship between theory (which is cognitive) and systemic organization. Let me outline these alternative approaches.

Rubinstein and Laughlin observe that one important feature of cognitive development is that it is organized hierarchically and arises in hierarchically ordered stages. Theories are a form of cognition and also demonstrate hierarchical structure. This feature is preserved even for the "diachronic, developmental progression from one theory to another." In other words, the argument is that theories are a kind of cognition, cognition is ordered hierarchically, and therefore theories are ordered hierarchically. Theoretical levels are therefore a function of cognitive structure. The process of bridging levels is explained by subsuming it under the rules of cognitive developmental process.

My approach, on the other hand, is that systemic levels and levels of analysis (theories referring to systemic levels) originate in systemic organization, not in cognitive organization. General systems theory affords the most sophisticated formulation of systemic structure and therefore may be drawn on for an initial definition of the problem.

Systemic levels are often called hierarchies. Hierarchical organization exists because the laws governing the behavior of a system at a given level are different from the laws governing the behavior of its constituent units. For example, atoms bond together to form molecules. Atoms themselves are composed of elementary particles such as protons, neutrons, and electrons. The laws that govern the interactions of protons and neutrons to form atomic nuclei are quite different from the laws that govern the interactions of the same atoms in forming molecules (Simon 1973:9).

For the problem at hand, the central question raised by this formulation is how theories are related to these hierarchic levels. Systems are defined by relationships between their units, and theories proceed by defining or explaining relationships between units, so it is reasonable to conclude that theories are in some way connected to or derived from systems per se. Since different laws govern the interactions of units on different systemic levels, each hierarchic level requires its own theory—or perhaps several theories. It is an important point in this regard that although different *systemic levels* are clearly functionally related (because the units of one level form the constituents of the next higher level, and so on), the *theories* that refer to adjacent levels are abstractions from these levels and *are not* necessarily connected (see the discussion on process theories in Bergmann 1966:89, Brodbeck 1968:302, Boggs 1974:154–58). This, then, is why theoretical bridge principles are needed: we know that different systemic levels interact in single hierarchically complex systems, and yet theories referring to adjacent hierarchic levels are often complete in themselves or their connections to different levels of analysis are tenuous.

Theories of inheritance may provide an example of a systemic hierarchy with which most anthropologists are familiar. Mendel discovered the laws of particulate inheritance, the level of analysis being unit traits or genes arranged in two sets of paired chromosomes. Molecular genetics provides a theory of

[5] Notice that (3) would be a premise in a standard identity-theory approach to the reduction of mental and neurophysiological theory.

the chemical constituents of genes and population genetics a theory of the way genes behave as constituents of gene pools or populations. These theories are connected to each other *because they can be referred to the same hierarchically complex system*, not because they are arranged in and originated as part of a single hierarchically organized developmental cognitive structure.

In other words, the *cognitive* order that Rubinstein and Laughlin have accurately described belongs to a different domain than the *systemic* orders that present the problems they are trying to resolve; it is different, that is, both from systemic hierarchies and from the sets of related theories that must be referred to systemic hierarchies. The levels of development of a child's cognitive understanding of mathematics are not the same as the systemic levels which he may later understand theoretically by applying these same mathematical principles. To argue otherwise assumes an identity between cognitive structure and the organization of natural systems for which there is no justification beyond the fact that they are both organized hierarchically.

It is only this similarity that has made it possible for Rubinstein and Laughlin to come to a correct and important conclusion by means of the wrong argument. Otherwise, this is a strategically conceived article to which its authors have brought the level of philosophical and interdisciplinary sophistication the topic requires.

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Although they skirt as many important issues as they deal with directly in this paper, including the intrinsic value of the biopsychological theory attended to, the authors are to be congratulated for presenting a coherent and persuasive argument on a much entangled topic. Aside from this general comment, I have two others, and I shall make them somewhat telegraphically because of space limitations.

1. There has been a long debate in anthropology over the "appropriate" level for studying structure and performance in human societies. Intolerance for interdisciplinary research and theory construction in this quest has been buttressed by the received wisdom, where reductionism is like ethnocentrism: it is something to be avoided. The charge of reductionism, however, generally has two meanings. One is that "my special interest cannot be reduced to your field because the facts and principles of the two are in reality disconnected." Thus the cultural anthropologist tells the biologist that biogenetic models of kinship do not sort out the cultural models of kinship he has discovered and that biological models do not account for the various kinship behaviors documented in the world's societies. Cultural models of kinship have an ontogeny independent of biology and therefore cannot be reduced by biological theory or to biology as a structural level of reality. One concludes erroneously from these premises that a biocultural model of kinship is irrelevant to scientific inquiry or impossible to construct with any degree of analytic satisfaction. The other common meaning is that "my field of special interest is so important in the organism's ontology that it can only be transcended or displaced through reduction at great risk to the development of powerful models and scientific truth." The biologist elicits this response from the cultural anthropologist when attempting to explain such things as human altruism and exchange behavior on a purely biogenetic model of relationships. Culture is eliminated from direct consideration, and the anthropologist objects. What is missing in both cases, of course, aside from rational interdisciplinary communication and cooperation, is a systematic approach to "bridging" laws and concepts. As Rubinstein and Laughlin argue, the proper arena for negotiating these issues is necessarily an interdisciplinary one grounded firmly in the philosophy of science.

2. In arguing against a priori rejections of reductionism, the authors make an additional point of importance to interdisciplinary growth in theory production: reducing one theory by another does not automatically make the reduced theory obsolete. That is something that must be dealt with explicitly in terms of the value of the reduced theory to the reducing theory. The parameters of inclusion and exclusion are ideally set by logic, empirical reality, and the nature of the problem to be investigated—not by a priori decisions in favor of disciplinary boundary maintenance. If the reduced theory is necessary to the reducing theory as applied to a particular set of problems, or if it improves the power of explanation and prediction in the reducing theory, then it may be incorporated in the latter to advantage. The key operative in this case is systematic and scientifically defensible evaluation of the relationship between the reduced and the reducing theory for potential articulation in a wider model. The operation fails as imperfect from the outset if the bridging concepts are not rendered accurately, if parts of the reduced theory are not eliminated where they are wrong or superfluous to the wider effort, and if contradictions that obtain on or between levels of analysis are not reconciled explicitly. Moreover, if the authors' "rule of minimal inclusion" is followed as suggested, it forces theoretical evaluation and model construction across both structural and disciplinary boundaries. It requires sensitivity to various levels of structural and operational reality in the human organism, and is perhaps best accommodated by an "organismic" model of society on the order of Whitehead's philosophy and Count's biocultural anthropology. Whether or not anthropology is prepared to advance on such structural ground is a matter for empirical determination in the future. In any case, abandoning the dogma against reductionism should aid our development of a coherent and broadly integrated scientific tradition.

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Particular formal approaches to the furthering of knowledge, whether applied or theoretical (i.e., disciplines), maintain their coherence through core areas of concern which imply areas not of immediate concern (i.e., some sort of boundaries). As research, data, cybernetics, social needs, and the desire for comprehension amass, these intensify an impetus for bridging such perceived distinctions (i.e., an interdisciplinary focus). Often the bridging involves hitherto widely disparate areas. Accordingly, Rubinstein and Laughlin's article speaks to some key concerns. Granting that more and more of us want it, how do we get it, and how do we get it right?

Given the negating complexity of simultaneously meshing and distinguishing "levels," one observation from our experience is that the idea of "level" could well and deservedly go out the window. Obfuscations of circularity, closure, and asymmetric bias within formal exegesis and paradigms, e.g., involving explicans/explicandum, inductive/deductive, linear causation, excluded middles, and discontinuous levels of discourse, reinforce this position, as does any stint of serious fieldwork (Pataki-Schweizer n.d.). The authors could use language which would engage those outside their own subdisciplines, especially for the medium of CURRENT ANTHROPOLOGY, and still retain their message; in part, this dual commentary is an intentional interdisciplinary response from anthropology/psychiatry to the handling of their metalanguage (cf. Hartog 1974).

Resolution is hard to find and still harder to generate (consider, for example, no less a figure than Einstein and his unresolved search for a unified field theory). It should be very evident that some of the conceptual world and, certainly, the orthodoxy of science writ large is quite arbitrary in that it is learned (enculturated) and hence culture-dependent, if not at times culture-specific. This statement need not stop short of epistemology and metaphysics. The authors' implied position on the continuing viability of our various panhuman research efforts is excellent, and so is their evocation of "environment-context" as one prime datum in a quest we all to some degree share. There appears, however, protean difficulty in actually employing a minimal-inclusion principle in practice, and certainly in fieldwork.

These issues are particularly important for those involved in transcultural work. There is now a multiplicity of methods for "handling" the ethnographic domain, many of which in the process substitute increasingly removed levels of abstraction. Some of these are excellent. Yet that domain is based on a real-life context common to the many disciplines tilling this soil (Merton 1948). For example, liaison psychiatric services wherein the psychiatrist moves into the lion's den of his more organic colleagues by following the full sequence of an illness have derived conceptually from an old tradition in human thought, which espouses a view of man as a somatopsyche in continuous dynamic interaction with the social, cultural, and inorganic environments (Burton-Bradley 1976). This holistic, antireductionist conception is achieving a currency in the field of medicine.

The existence of disciplinary boundaries is also a reflection of the self-conceptions and authority needs of individual professionals confronted, and at times threatened, by the inroads of interdisciplinary studies and newer trends in research. Hence the need to argue from authority and the elevation of fallen heroes, rather than a rational dialectic *linked* to empathic discourse. Put more bluntly, one devoutly hopes that the curiously unconstructive apologetics and mea culpas of various intercultural researchers in the past several years will soon have peaked out. Given the issues at stake, such bathos is not so much throwing baby out with bathwater (here redolent of clan infanticide) as entirely ceasing to bathe. Profound and deeply related elements of cognition and affect, e.g., the meaningful recording of panhuman behaviour, transference-countertransference phenomena, and the linkage between cognitive and affective domains, appear to us as some of the seminal issues underlying the article and the newer science we seek.

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Rubinstein and Laughlin call for a more rapid interchange between philosophers and anthropologists, hitting what is the one point of agreement for most current philosophers of science: that hitherto their discipline has been impoverished by its relative failure to engage specific theoretical quandaries occurring in individual sciences. Yet what is offered here is a *global* view of science based upon a biogenetic structuralism in which the different "levels" of structure are to be related by theory reduction.

Unfortunately, Rubinstein and Laughlin present their account of theory reduction in opposition to a straw man, the "deductivist" view. Whilst they correctly indicate that in Nagel's (1961) work one theory is reduced to another only if it is deductively derivable from it, they falsely associate two failings with this. First, they claim that the deductivist account entails that the reduced theory is false.[1] Falsehoods, however, follow only from falsehoods, so if the reduced theory is false and the deduction is correct, then the reducing theory is also false. This conflicts with the nonformal condition, which Nagel imposes on reduction, that the reducing theory be supported by empirical evidence possessing some degree of probative force (Nagel 1961:358). Secondly, Rubinstein and Laughlin claim

[1] This was pointed out to me by Robert Carnes in a conversation about a previous version of Rubinstein and Laughlin's paper.

that reduction, on the deductivist account, implies superfluity of the reduced theory. It is unfair to hang this charge anywhere near Nagel, since he explicitly disavows it (p. 366): "the reduction of one science to a second . . . does not wipe out or transform into something insubstantial or 'merely apparent' the distinctions and types of behavior which the secondary science recognizes." The authors are aware of the emotional overtones of "reduction," but it is important that these be located as misperceptions of the deductive model rather than as elements of the model itself.

In their positive doctrine, Rubinstein and Laughlin have embraced what may turn into a specific theoretical quandary, since they are in some doubt as to whether the theory reduction they envisage is analogous to or identical with the transformation from one developmental stage to another. They claim that "a biopsychological view of theory reduction must make explicit that the reduced theory is necessary to the reducing theory." There are at least two relevant senses of "necessary" here: logical and psychological.

The logical one is in a sense trivial, since one needs a reduced theory in order to have a reduction of it at all. A more important and clearer sense is provided by the deductive model itself: the reduced theory is a logically necessary condition (a consequence) of the reducing theory. Rubinstein and Laughlin's espousal of the Ager et al. (1974) view of *reduction by incorporation* militates against this sense, however, since Ager et al. are concerned to distinguish reduction by incorporation from simple deduction. That such a distinction can be made within a purely deductive framework is problematic. Insofar as what is involved is the deductive reorganization of a body of information, the problem arises of whether there is an identifiable subtheory of the reducing theory which has at least partial isomorphism with the reduced theory. Incorporation does not appear to necessitate that this is the case. Indeed, it is conceivable that there are observationally equivalent axiomatizations of a body of knowledge that are not partially isomorphic above the observational level. If this is the case, *how* levels of systemic organization are bridged remains unanswered, and the very notion of "bridging" is perplexing, for if theories are incorporated they are not bridged. The psychological sense of "necessary," to which Rubinstein and Laughlin are committed, has profound implications for the history of science; specifically, that the development of scientific theory could not have been different from what it was. Needless to say, these implications are empirically arguable.

Rubinstein and Laughlin make moves to identify these two senses of "necessary" because of "the nature of the biopsychological processes mediating scientific inquiry" and the "strict preservation" of the ontogenesis of cognitive structures. Yet if this is so, it is difficult to follow the differentiation that they want to make when they say: "The level or levels of systemic organization one chooses for analysis . . . is not a matter of social norm, but rather is logically entailed in the problem one has set." How, then, do logic and social norms differ?

Perhaps this puzzle follows from the biogenetic structural theory of science itself and is not directly relevant to reduction at all. In this theory, neurognostic models are modified by feedback from the results of action on the environment: "Depending upon the degree of discrepancy between the expectation and the environmental response, the neurognostic model is modified so that it comes into more adaptive isomorphism with the environment." "Adaptive" can be tacitly evaluative. Are neurognostic models adaptive on creation or on appraisal? That creation is sufficient is elegantly questioned by Sidman's (1960) work, in which coincidences of normal stimuli give rise to pathological expectations. Nor should we completely forget that human cognitive activities may be ultimately maladaptive, as is brought out by Hume's problem of induction, so starkly captured in Russell's example (1946:63): "The man who has fed the chicken every day throughout its life at last wrings its neck instead, showing that more refined views as to the uniformity of nature would have been useful to the chicken." Imagine mankind in place of the chicken. Interestingly enough, Hume himself chose psychological necessity for his epistemological basis; yet that was a simple dodge. We have not solved his problem, even though we are strongly inclined to dismiss it; we feel that there are centuries of evidence for the progress of science: that scientific change is adaptive. But, if one is willing to grant that neurognostic models are adaptive, then why not the models of science itself? We would thus be led to say that our current models (the deductive one included) provide us with *some* (contra Rubinstein and Laughlin's "no") understanding of science even though they eschew the study of cognitive functions of the brain.

Creation and appraisal can be linguistically distinct activities, and they may be governed by psychologically distinct activities —one of which we currently dub "logical." The move against psychologism in logic promulgated by Frege (1884) has had huge intellectual dividends, in particular, those of laying the foundations of the very structuralism that Rubinstein and Laughlin endorse. This means that we should look to incorporate these dividends into any reduction of Reichenbach's (1939) distinction between the context of discovery and the context of justification.

It would be a delightfully simplifying result if the ultimate cognitive and logical development of science were identical. In such a case, a model of neurognostic models of science might be more adaptive than our current models. But it is doubtful that we should talk of further "development" in such circumstances, for would we not have the stasis of a completed science?

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Reduction and expansion. Rubinstein and Laughlin never entertain the possibility of conditions under which reduction may not obtain. Kepler did not reduce circular, uniform-motion ideas to something else; he transcended them in his shaping of the conceptual constellation of an elliptical orbit sweeping equal areas in equal times. The actuarial data summaries of Tycho Brahe "remained the same"; the shape of our appreciation changed. Newton's *Principia* did not reduce cosmological concerns to terrestrial ones, or vice versa· it provided a keystone idea of gravitation. Newton constructed a conceptual plot which transcended the existing theoretical contentions of Descartes (vortices) and others. Einstein expanded our view of the inherent symmetry of the four-dimensional continuum of space-time; his relativity is a new form of homogeneity. The uncertainty relations which delineate the conceptual foundations of quantum theory are expansions of our conceptual horizons. Niels Bohr introduced a qualitatively new theme into scientific inquiry: *complementarity* (see Bohm 1957; Butterfield 1957; Hall 1954, 1960, 1963; Hanson 1958, 1963, 1971; Holton 1973; Humphreys 1968; Kuhn 1962, 1970; Toulmin 1961, 1970, 1972). As Hanson (1963:66) expresses it, "The punctiform mass, primarily a kinematic conception, is the starting-point of classical theory. The wave pulse, primarily a dynamical conception, is the springboard of quantum theory. Languages leaping up from such different platforms are likely to perpetuate this logical difference throughout their development and subsequent structure; and this is indeed the case."

All of the above shapes of ideas have been employed to represent the nature of the cosmic glue. The continuity of what we call theoretical physics is to be found in emerging *anomalies*, not in a mythical partial commensurability because of the nature of the biopsychological processes mediating scientific inquiry. A major area of ignorance concerns the reason for incompatibility (Chaney 1972, 1973, 1974*a*, *b*, 1975, 1976). Another is how diverse shapes of understanding, expectation, and procedure come to be invested with emotion. "The salient connection of human nature and human nurture is emotion. Semantic reticula are emotive links in our comprehension of the world and ourselves. Turner (1967:30) has employed the term transmutation to suggest how symbols become saturated with human emotion . . ." (Chaney 1976:753). My wonderment concerns the diversely transient pockets of disparate regularities (and irregularities) in space-time. Why asymmetry in the overall flux density of human endeavor (Chaney n.d.)?

Reduction and emergence. Rubinstein and Laughlin make no distinction in their discussion of "reduction" between (1) going from one theory to another while discussing physical phenomena and (2) discussing the nature of one kind of phenomenon (symbolic forms) in terms of another kind of phenomenon (neurophysiology). Whether there are phenomenal differences is the basic question (see Bidney 1953, 1973; Black 1962; Cassirer 1944, 1946, 1960; Ortega y Gasset 1941). The *systems epistemology* of von Bertalanffy directs our attention to the problem of a comparative systems approach. Von Bertalanffy (1968:235) says: "Our perception is essentially determined by our specifically human, psychophysical organization. This is essentially von Uexküll's thesis. Linguistic, and cultural categories in general, will not change the potentialities of sensory experience. They will, however, change apperception, i.e., which features of experienced reality are focused and emphasized and which are underplayed."

Piaget's valuable maturation studies of the unfolding of an idea such as "time" for an individual in a culture possessing an idea of "time" provide little insight into the "discoveries of time" in human history (see Toulmin and Goodfield 1965).

A fundamental human characteristic is the *potentiality* for changing fundamentally (i.e., as to what one becomes emotional over) within one's own lifetime.

Strands of thought and blindspots. "Neurognosis" is another way of talking about the panhuman quality of learning from experience, but verbal thrusts such as "empirical modification cycle," "feed-forward function," "alternation of induction and deduction," "more adaptive isomorphisms," etc., lead us nowhere. Russell (1903:42) hoped for "a classification not of words, but of ideas." Wittgenstein (1953) provided frames of understanding such as "forms of life," "language games," "family resemblances," etc. He tried to teach us differences, differences inculcated through usage. His students, such as Watson (1938), Hanson (1958), and Toulmin (1961), have directed our attention to differences in the shape of ideas with their respective meta-ideas of "methods of representation," "seeing as a theory-laden undertaking," and "ideals of natural order."

Rubinstein and Laughlin's modeling of the "empirical modification cycle" gives us no insight as to why human beings learn such different things from experience (and sometimes kill each other over them). I suggest that this shortcoming is related to their treatment of the structure of "scientific inquiry" and "cultural life worlds." They state that "scientific reasoning characteristically exhibits the same logical structures as non-scientific thought . . . (Horton 1967, Laughlin and d'Aquili 1974)." Horton states that he lives in Africa because he finds life there more poetical. Of what could he speak? Horton speaks of similarities *and* differences (see also Horton 1973:249–305 and Barnes 1973:182–98). His key difference concerns "closed" traditional cultures and "open" scientifically oriented cultures. Even in an "open" culture, however, "the 'open' predicament has nothing like universal sway. On the contrary, it is almost a minority phenomenon. Outside the various academic disciplines in which it has been institutionalized, its hold is pitifully less than those who describe Western culture as 'science-oriented' often like to think" (Horton 1970:171). Horton feels that science has made its mark more in areas distant from deepest emotion.

Proptosis. What is the neurophysiological basis of human transmutative "emotive glue"? Are "strands of thought" kinds of "mind parasites"? How are "strands of thought" employed in "the manufacture of madness" (see Szasz 1970)? How do metaphors work (and not work) within and between persons?

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This paper, I hope, will prove a landmark. Its substance is intrinsically worthy and arguable. More important still, it bespeaks the very profound metascientific rethinking that has characterized our age for several decades, which argues *relation* as a *prime*, whereas "classical" physics and biology argued atoms (in a sense akin to the original Greek).

This comment will be the more intelligible for some preliminary notes on current anthropology and on current metascience.

1. To this day, anthropology lacks a sustaining corpus of theory—what Heisenberg, à propos of physics, calls a "closed system of concepts and axioms." This is why it remains a shrewd but unprofound "science"—not at all the catholic "science of man" its 19th-century founders seem to have hoped for. For simplicity's sake—since Rubinstein and Laughlin have in mind more particularly a bioanthropology, despite their inspiration from Lévi-Strauss—in this note I shall set aside the social science facies, after the remark that in sociocultural anthropology man is self-referencing, so that unless the statement "man's culturized life-mode requires man the animal" should become substantial, not merely rhetorical, its treatment would not further this comment.

Bioanthropology has considered itself a particular subdiscipline of a general biology; as such, it needed but take over, uncritically, whatever "concepts and axioms" were generated by the primary, parent science. This afforded the possibility that it might chalk up successes; it guaranteed that, in measure as the parent science fell short, it would too. Now, what has been the condition of biology?

2. Of course, biology has evolved "concepts and axioms" of its own. Yet its history records a long and intense debate over the issue as to whether these should be treated as primes or could be "reduced," at least mentally if not experimentally, to physics and chemistry. Roughly speaking, the former identified the vitalists, the latter the mechanists. Eventually, the mechanistic stance prevailed; this meant that biology committed itself to the physics of its time as its model. The irony is that this occurred about at the moment when "classical" physics obsolesced in the presence of quantum mechanics and indeterminism. I shall return to this later, for on it will depend the meanings of "reductionism" and "levels of systemic organization."

Now to characterize, if but sketchily, some metascientific principles of "classical" physics. It was atomistic; that is, it sought to analyse ever more finely, to discover, insofar as possible, "infinitesimals," to simplify and isolate factors. Causality was linear. Complexity was a handicap, to be obviated if feasible. Issues of epistemology : ontology were to be eschewed. It was but common sense to trust the senses. No question that failed to conform to these canons was to be allowed the name of "scientific." We all recognize these attributes as "positivism." A variety of subdisciplines had been brought together via most ingenious reductive processes, the last discipline to be incorporated or conjoined being chemistry. In long event, physics gathered itself into a great document of entropy.

At the risk of a simplicism—unavoidable in this brief span—a biology that took care to conform to and model itself upon a contemporaneous physics by a like token became a great document of metabolism. Life's primary problem was that of exploiting "successfully" the energy of an entropic universe. Only a limited variety of its guises was usable; ecology is a study of adapting to those limitations.

Retrospectively, I cannot imagine how physical and biological sciences in their formative stages could have achieved so spectacularly except by these intellectually Spartan procedures; they still are valid, where information about the parts of a whole is the desideratum. The difficulties begin as soon as it is realized that such dissection forever annihilates the information about the relationships that constitute a whole. Here, merely to restore a balance of record, be it said of the long-discredited vitalists that at least their instincts were sound: the atomistic-mechanistic-reductionistic model simply never yielded an answer to the question What is life? With our hindsight, we can see that the vitalists were gropingly aware that the very definition of life is an argument of relations, not just of parts.

We may not fault the formative stages of sciences for choosing to operate on a very strictured philosophy; we must rise to object if the strictures become entrenched, so that they disbar problems that do not conform. Here is a small roster of misleadings (Koestler would say "pillars of unwisdom"): "Form" is to be assumed as axiomatic; only constituent parts are investigable, are problematic. Evolution can adequately be accounted for via random mutation of genes of a genome at one end and natural selection of phenotypes at the other. (To *randomize* gene mutations for epistemological purposes is a legitimate device; to convert the device into an ontological *randomness* is a case of Whitehead's "fallacy of misplaced concreteness." Natural selection is not an explanans, but an explanandum. In truth, however, evolution is properly a question of *what happens within a system to transform it*.)

Ironically, at about the time that mechanistic biology triumphed (deservedly) over vitalism, Planck's quantum mechanics and Heisenberg's principle of indeterminism were introducing (in our American argot) "a new ball game" in physics, and biology was to be quickly caught up in it. (As yet it seems not to have affected bioanthropology.) The "sophisticated biopsychological view" that Rubinstein and Laughlin call for must reckon with it.

Again this note must be overly brief: The issue of epistemology : ontology is a cardinal problem, commencing with physics. Heisenberg (1958) observes that, in measure as physics has developed progressively its closed systems of concepts and axioms, the subjective element has entered further into the fabric; it becomes increasingly clear that the questions put to nature are always in a priori terms of human science—and this is particularly acute in biology, where the questioner is a biological entity. There are accompaniments. At the subatomic level, at least, indeterminism is written into the facts themselves: position and velocity of any particle are so related that increased accuracy in determination of the one decreases accuracy in determination of the other. If, then, we seek a "macrodeterminacy" at the levels of systems where we organicists would operate, we must generate it from this microindeterminacy. We are operating in a world of *undecidability*—a technical concept. Linear causality is no longer entertainable. Experience has taught that the application of logical principles to natural phenomena sometimes gives erroneous results; now it is realized that these may be due to (shall we say) an illogic of the logical principles whose sway has remained undisputed from time immemorial. "Postclassical" physics—that of a Planck and a Heisenberg—introduced the concept and principle of *potential*, fraught with meaning for us organicists. Our universe of discourse argues systems of biological events and processes, replacing the earlier one that classified data in terms of objects, traits—"things." Biological systems are multidimensional; we argue phase hyperspaces, fields within these, degrees of freedom descriptive of them. (These statements enlist other thinkers besides Heisenberg; they are but further concepts within the same universe of discourse.) Heisenberg suggests that, if we would explain life, we must develop yet a further closed system of concepts and axioms, one in which physics and chemistry would be positioned as but "limiting cases." History would stand as an essential dimension. (It is interesting that, years ago, Rashevsky also recognized this dimensionality of history; furthermore, years before that, Whitehead predicted that if some day physics and biology arrived at some kind of merging, it would not be biology that would be swallowed.) Physics-chemistry combined with Darwinism would not be enough to explain life.

Of this last point, we already may be assured. Organic evolution has been that of systems—nothing less will do for argument—and it is *potential*, not what a system *does* but what it *can* do, that evolves. The basics of theory that can handle this truth exist already.

There remain three heads of argument: (1) order, negentropy, and information; (2) conceptual properties of living systems; (3) "neuroscience" as an argument of system. Presumably they all must shape future reductive argument.

1. *Order, negentropy, and information.* Boltzmann (1872) submitted the formula and theorem $S = k \log \pi$, where S is the entropy of a thermodynamical state, k is "Boltzmann's constant" (I shall not explain it further), and π is the number of dynamical states in which the thermodynamical system might conceivably exist. Informally, it is a measure of the disorder among the constituent particles. Then $-\log \pi$ becomes a measure of order. It proves to be isomorphic—and not by coincidence—with the "information function" of information theory.

Schrödinger promoted this basic idea to that of order-from-order: living organization is incomparably the most complex instance of this negentropy. It has a vast body of consequences: they demand a knowledge of the second principle of thermodynamics—of the character, in fact, of Maxwell's demon.

Briefly, positive "entropy" is a statement of irreversible degradation of energy in the universe, which, following the trajectory of "time's arrow," converges upon zero order. Maxwell's demon, which stood against this trend, was "proven" to be a logico-physical impossibility. But "order," "information" are the antithesis of positive entropy, as just indicated. They argue the aggradation of relations; they diverge open-endedly. Now, while indeed information always "rides" entropic energy, the universe being what it is—information is never found except as embedded in energy—neither astrophysicists nor theoretical physicists have ever encountered an instance of zero order in the universe; randomness is coming to be seen as a convenient heuristic abstraction, with no evidence whatever for giving it an ontological reality, and "the whole universe is too big for thermodynamics and certainly exceeds considerably the reasonable order of magnitude for which its principles may apply" (Brillouin 1962). With respect to order, the universe appears to be "bottomless" (Bohm 1969). If "order," "relation" is a prime property of the universe, and not a secondary one, an unaccountable derivative of disorder, living organization is no anomaly in principle, however rarely it be encountered. Ashby (1960), arguing from a different direction of approach, once remarked that the development of life on earth actually was "inevitable." As utter lack of order is complete probability, life's extreme complexity is extreme improbability—extremely rare yet inevitable.

Out of this argument has emerged an extremely relevant point for our discussion: quite antithetic to "classical" atomism, instead of being an obstacle to be eliminated, complexity becomes a cardinal concept that we must argue; it cannot be left out of considerations of reduction or of other "bridgment." If the second principle of thermodynamics ruled Maxwell's demon out of action, *negentropic principle makes it a central character*.

2. *Organism in systems perspective.* "The whole is more than the sum of its parts." I confess to puzzlement that Rubinstein and Laughlin should dismiss this "classic cliché" and so pull their rug out from under them. I would like to restore it, for the cliché argues relations—and this is an essential part of the gist of their argument. A modern restatement would be that a "whole" includes the information inherent to the parts plus the information of the constraints, the *relations* that give configuration to the "whole." This latter is not recoverable from an analysis of the former, practically or theoretically. In fact, at this point I am uncertain of Rubinstein and Laughlin's own meaning; I suspect that "parts" still has for them a "classical" idea about it. In theories of system, one prefers the term "variables," which gets away from the elderly classification by objects, traits, "things," and admits processes and events. Or, "subsystems." Or, on suitable occasion, Pask's (1966) idea of "packages," which is felicitous when we are considering developmental differentiations, emergence of heterogeneities from pristine homogeneity. (Weiss [1969] has defined "system" holistically, in terms of how its variability is ratioed to the variability of its variables.) As soon as Rubinstein and Laughlin stated "systemic organization," they were arguing relations (but their two-word *term* is an unnecessary redundancy).

Viewed as an argument of information, of system, and not as an argument of energetics, living organization offers the following landscape features, among others; they should suggest why we speak of "organismal" and "mechanismal," but not of "mechanistic," biology.

a) Living organization conforms to the same principles as nonliving organization, but it has developed these principles to far higher levels of complexity.

b) By the same token, it is a particular case of "self-organization" (but this term demands a very sophisticated analysis).

c) It is a prime, an inherent property of living organization that it has evolved in a schema of hierarchical levels of order (again a concept demanding very sophisticated analysis).

d) Any level of order has its own system of constraints. Those of a next-higher level have been channeled from these antecedents.

e) Living organization constantly exploits redundancy. (This becomes a very important consideration in the matter of the nervous system and behavior.) Redundancy effects stability of a system and accuracy of response, but these two stand in a negative complementation (i.e., high degree of the one involves low degree of the other). Redundancy entails that a (living) system will "get away with as many errors as it can" ("Dancoff's Principle" [Dancoff and Quastler 1953; Quastler 1958]). (In a neuropsychology, this will rationalize the value of multimodal inputs; the organism matches their information, arrives at a viable but not a machine-precise answer, and remains capable of a new solution next time. This rather clearly must have significance for considerations of "natural selection.")

f) The molecular codings of a living system possess a *reliability* far beyond that of any nonliving system. This cannot be explicated in a few words; it is a property of hereditary storage of information and the dynamics of its transmission. It far exceeds what may occur in a scheme of "classical" physics.

g) To add in from earlier statements—a living system is multidimensional; therefore it locates in phase hyperspace and within fields located therein, which possess their characteristic degrees of freedom; and the system is to be argued in terms of its *potentials* to make choices, which increase with complexification.

h) A living system has a "stake" in the outcome of its own computations (whatever content we put into the last word). It directs itself constantly toward a variety of autochthonously defined goals; at "higher levels" we see these as "values," "creativity"—which terms, incidentally, are viable units of discourse within the frame we are negotiating. It "seeks for its own purpose through a process of learning" (Wiener 1954, crediting Ashby).

3. "*Neuroscience*" *as an argument of system.* I think it a safe remark that no other scientific discipline has contributed as much to our knowledge of what man's brain has produced as anthropology; this makes it all the more paradoxical that its contribu-

tion to knowledge about the producer is virtually negligible. This sharpens Rubinstein and Laughlin's already acute discernment that it is now crucially strategic that anthropology negotiate the substance of neuroscience.

In that long evolutionary account of man's emergence, anthropologists have found it more congenial to argue the morphology of, e.g., orthogradation, which suits well a "classical" biology, and to leave the evolution of brain function to others. For instance, the speech function ("phasia"), as we all know, has nothing remotely comparable among nonhumans. (This decidedly is not to say that phylogenetic anlagen do not exist.) But the brain is a concentrated specialty of the nervous system, and the nervous system is a concentrated specialization for processing information, which processing paraphrases negentropic order-from-order, which in turn gives body to the principle of *divergent* series. In contrast, *mechanical* structures, such as limbs, as they develop greater efficiency phylogenetically, do it by diminishing their degrees of freedom; the series is *convergent* upon an unbreachable limit; generalization narrows to specialization.

The vertebrate brain, from fish to man, has manifested the power of generating from its existing information-processing mechanisms further heterogeneous "packages" that can process it yet more finely. But note: (*a*) This new processing capacity is channeled into existence from the qualities of antecedent mechanisms. (I am thinking now of the course from the cyclostome brain-stem through a reptilian one to the neocortical climax of mammals.) (*b*) The antecedents remain essentially intact processers, while yet developing new relationships with the consequents (the evolutionary "novae"; cf., e.g., thalamocortical traffic). (*c*) "Classical" biology is helpless even to suggest what might induce such an evolution. *What* should bring about a stimulation to process information yet more finely, where none existed before? (*d*) No new mechanism ever supersedes a phylogenetically older one; all of its processing must accept first the "hypothesis" submitted to it by that antecedent. Every new capacity level for finer processing implies further degrees of freedom, which nevertheless have been channeled by the constraints of the antecedent capacity level. Complexity is compounded.

What "classical" biology cannot handle is negotiable within the universe of dynamic systems. Under the principle of *potentiality*, a prime and inherent property of *any* animal organization, no matter how lowly, is the capacity for coping with novelty (however far we may yet be from understanding it completely). If Ashby, on premises of cybernetics, has observed that "life was inevitable," in related vein I would observe that, given the principle of negative entropy, intelligence was inevitable.

If we did not already know, empirically, that a complex of perception-cognition-programming enlists the gamut of neural-mechanism organization, from reticular system to differential areas of the neocortex, we would have to postulate it on speculative, philosophical grounds. An act of brain is an *orchestration* of all these nonduplicating "instruments." To reemphasize: complexity is an existent; it would be fallacious to try a mordant simplification upon it. "Reduction" can only destroy the orchestration.

I have left "neural mechanism" intentionally vague: it would serve no purpose to distinguish between function and structure. Of course, we are visualizing an active cytoarchitecture and neurohistology and bearing in mind that relays, circuits need complementation from holography, but we need some further implementation. Neurophysiology indeed has its "grammar," but it is a "grammar of the parts." We require the metalanguages of symbolic logic (à la McCulloch, for instance), topology, and especially the systems theories to substantiate a grammar of relationships. The neuromechanisms all speak the same language, but certainly different dialects of it, and every distinctive mechanism must understand every one it "hears." The objective facies of all this is coming rapidly to be known; the subjective, that of "mentation," still eludes. Here is an instance of "coping with novelty," for each time one mechanism speaks to another it is using its own dialect (it can never use any other), which the recipient mechanism must understand, and saying something that has never been said before. (Of course, one cannot but recall the digital computer.) But, to return to the orchestra, what and where is the conductor, and what and where is the composer of the score?

At some point we must consider the meaning of "theory." What is it that we are proposing to "bridge" or "reduce"?

Waddington (1970) has suggested the following working-scheme for levels of theorizing:

1. Metatheories concerned with deciding which topics it is profitable to have theories about.
2. Theory Proper, which attempts to define in general terms the logical structure of the problems selected, and to provide an appropriate language in which they can be discussed.
3. General Hypotheses, which specify the types of mechanism invoked to engender these logical structures.
4. Particular Hypotheses, describing how various elements from this array of possible mechanisms are involved in particular cases.

It is my impression that anthropologists, when theorizing, usually have fared best at Level 4. Rubinstein and Laughlin seem to be arguing at Levels 2 and 3, which is where they should be, although I am not sure when it is the one, when the other. Their insight into the strategic importance of the neurosciences lies at Level 1.

This note, I think, speaks rather to Levels 1 and 2; the question of appropriate metalanguages lies at Level 2, and I believe that the desirable "integrations" will be promoted more by these than by essays at "reduction." Of the latter, my further impression is that to 19th-century atomism it constituted something of an ideal; our present *Zeitgeist*, with its concepts and problems, would instead effect bridgments with its new armamentarium of metalanguages, and therewith demote reductionism to an ad hoc instrument. For example, because "behavior" is the investigative province of one discipline while a neurological psychology investigates another dimension of the same phenomenon, a traditional attitude would "reduce" behavioral theory to neuropsychological theory. If, on the other hand, we accept the fact that at one end of an isolable brain ("brain-mind") performance continuum there occurs perception-cognition and at the other the externalized consequence of programming, the call of theory is for a metalinguistics that can carry the continuum throughout. If I say, "behavior is but the presenting symptoms of neuropsychological processes," I am making an identification, but not a reduction.

Rubinstein and Laughlin have wrought well, and they know that a long road lies ahead.

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If one accepts the view that the scientific paradigm which prevailed till recent decades is fast being replaced by another which is in keeping with the subject rather than the object, the knower rather than the known, the observer rather than the thing observed, then Rubinstein and Laughlin's efforts in this paper would appear to manifest all the signs of a rear-guard action.

This is evident (1) from their view that "sciencing is a special function of cognition, the process by which the *brain* [italics mine] constructs an adaptive model of its environment"; (2) from their blithe dismissal of the antireductionist position that "sociological or psychological facts require sociological or psychological explanations"; (3) from their characterisation of the profound proposition "The whole is more than the sum of its parts" as a naive conception and a classic cliché; and (4) from their adoption of the Buckleyan position that "it is, in other words, the job of science to 'reduce' to the parts *and* relations comprising a system."

To keep on using jargon such as "bridging levels of systemic organization," "integrating behavioral scientific and neuroscientific approaches," "biogenetic structuralism," and the like (a curt bow towards Whitehead notwithstanding) is to get caught up in a reification of words and phrases and thus to hinder the newer understanding of reality that is struggling to be born, with its promise of a gigantic leap into the realm of "psycho-magnetics."

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To link the "neuroanatomical" structure and molecular biology of the central nervous system to organismic behaviour (a genetic "a priori") seems obvious. This idea is implicit in all neuroanatomical works and is fundamental in ethology. "Neurognostic model" is only an other name for Jung's "archetypes" (cf. Jung 1975:411–12). That man generates "expectations" according to cognitive "models" and on receiving "feedback" modifies the model "so that it comes into more adaptive isomorphism with the environment" has already been formulated by Margenau (1950:33–122). However, the authors' synthetisation of these ideas in a "biogenetic structural theory of science" is welcome.

The difficulty is that their theory is based on the assumption that we already know enough about the "cognitive function of the brain" and its development. They seem to presuppose that the present system of theory building (quantified and descriptive conceptualisations, communicated by means of mathematical equations and based on current concepts of space and time) will not change from "one developmental stage to the next in general cognition." I suggest that it will.

"Sciencing" has developed not solely through the refined application of unchanged fundamental assumptions (often subliminal) about the nature of reality. It has also made "quantum jumps" (observed in natural evolution as well: Dobzhansky 1969:408; 1963:147; Decker 1963; Gallus 1977*a;* in relation to man, compare Quigley 1971:520–21, 537; Clark 1973:465), which involve a restructuring of the basic assumptions themselves. Theoretical structures current at the time become radically altered. Quantum jumps can be seen in the Milesian School, Bacon-Descartes, Copernicus, Galileo-Kepler-Newton, etc. (cf. Kuhn 1959). It seems obvious that such developments did not take place via "theory reduction." I do not suggest that "theory reduction" is not useful. I only point out that it is not an ultimate stage in human cognitive development as the authors seem to believe.

Scientists working within a "particular theoretical framework" will in no way understand the "terms and phenomena" of a new mutational framework, and "theory reduction" will not be possible. All that will be possible is a restructuring according to new fundamental conceptualisations, acceptable to a new generation of thinkers. This apparently will become the situation during the last quarter of the 20th century. There are signs of growing dissatisfaction and malaise concerning relativistic and quantum mechanical conceptualisations of reality (derived by deduction from quantified mathematical equations). Real progress will depend not so much on "theory reduction" as on the creation of *new* theoretical concepts, which will make the currently popular ones obsolete. This, happily, has happened before and will happen again.

The authors' strategy of theory reduction marks a final stage in the elaboration of current "Western sciencing," and as such it is most useful. It cannot, however, be regarded as the ultimate wisdom on the direction of human cognitive development. If, with the authors, we did not allow for the emergence of a qualitatively distinct, new, creative Gestalt, but only looked for reductive compromises, cognitive evolution would remain forever stunted, confined to one "particular theoretical framework." Even in terms of their biophysical evolutionary orientation, however, we can equate such a quantum evolution of thinking with quantum evolutionary phenomena in biology in general and the evolution of the central nervous system in particular.

If we grant the presence of inherited "neurognostic models," based on the structure and molecular organisation of neurons, we must also grant the possibility of mutational changes within this system, which of course lead to new and different "neurognostic models" (e.g., biological "a prioris"). Substantial quantitative changes within a short time will be so great with certain individuals and will lead to such differences in fundamental conceptualisations about reality that no theory reduction will be possible between the new and the old.

Such changes, of course, do not and cannot happen at any time and at any place. They follow the rules of an "evolutionary series" or "line" (Gallus 1942, 1974, 1977*b*). All the potential of the old "theoretical framework" must apparently be exhausted before a final stage of lack of progress-potential and the accompanying frustration is attained. When the last possible synthesis of current "sciencing" based on currently valid fundamental assumptions about reality has been reached (possibly on the lines of the authors' concept of "theory reduction"), then a mutational situation arises.

Historical thinking (e.g., "evolution," "process," "development") dissolves the controversy mentioned by the authors as to whether or not different "theoretical frameworks" (Gestalts or world views) lead to unresolvable differences and fixations. They do, but only when the time is ripe for a quantum mutation. Until then, scientists participate in the smooth process of developing the cognitive potential of given fundamental conceptualisations (resulting from a previous quantum jump). This is where the authors' "theory reduction" makes sense—not in a mutational situation, when a new Gestalt emerges.

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Rubinstein and Laughlin call for a dialogue with the philosophy of science which is long overdue. Anthropologists have been all too reluctant to undertake an anthropological analysis of their own scientific culture. Certainly awareness of alternative modes of organizing cultural reality systems provides a unique perspective on scientific assumptions rooted in cultural definitions of time, space, and system organization. Philosophers of science such as Kuhn function as anthropological observers of the scientific community. Kuhn (1962) points out that science progresses not through the incremental addition of knowledge, as the participants report, but rather through an observer's model of paradigmatic revolutions. Certainly the culture of science and the process of sciencing are appropriate topics for anthropological investigation.

Rubinstein and Laughlin slight the importance of analyzing relationships in theory reduction by incorporation. They state, "we agree with Buckley (1967) and Blalock (1969) that insofar as the parts and *the relations between the parts* of a system have been explicated, one has defined the whole. It is, in other words, the job of science to 'reduce' to the parts *and* relations comprising a system." This is the only reference to changes in relationships in the process of bridging levels of system organization. Equilibration, or theory reduction by incorporation, is actually a *change in the nature of the relationships* between the elements of the reduced theory. Reduction by incorporation changes the contextual relationships of the reduced theory by incorporating it into a larger system. Elements of the reduced theory are recombined in a larger matrix. The nature of relationships and the processes involved in incorporation or other sorts of transformation are poorly understood. Bateson (1972) points out the cultural bias in Western science toward examining only the elements in a system and ignoring the relationships between them. He relates this bias to Biblical assumptions about the nature of reality. The same cultural bias exists in the analysis of the process of theory reduction in the philosophy of science. Rubinstein and Laughlin acknowledge that the analysis of relationships is necessary, but they do not deal with the fact that anthropologists and other scientists barely know how to begin such analysis. Piaget (1973*c;* cf. Geilhufe 1975) has sketched some possible directions for analyzing meaning and information channels in systems, but this is the least developed aspect of his interdisciplinary system model.

It is the analysis of relationships—their nature, function, and perhaps meaning—that will allow anthropologists to understand more fully the epistemology of theory development. Theories delimit categories and put those categories in some sort of relationship to one another. Western science focuses on the categories as elements and ignores the relationships which connect them. The analysis of context (such as is attempted in biogenetic structuralism) is crucial in understanding the cultural limitations of the philosophy of science. Biogenetic structuralism combines the biological, psychological, and social in a new matrix of relationships, transforming the elements of each by placing them in a new context. A thorough examination of the transformations of relationship is necessary to understanding the epistemology of this approach. Anthropology, with its interdisciplinary, holistic focus, is an ideal discipline for developing more adequate understanding of such epistemological relationships.

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The article boils down to the idea that theories should and can —with reasoned justification—coexist peacefully and might therefore prove to be a useful "ideological" underpinning for interdisciplinary joint ventures by delegitimizing reductionist imperialisms. The view that "the reduced theory is necessary to the reducing theory" or that "the reduction of cognitive theory by neurophysiological theory would require the incorporation of both into a single theoretical structure" could easily be connected with the Hegelian or Marxian conception of dialectics (thesis, antithesis, and synthesis). The authors should be asked to include bibliographical references to Geertz's hermeneutics.

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Rubinstein and Laughlin attempt to reconsider the question of reduction from a biogenetic structuralist viewpoint. They argue that an analogy exists between the developmental stages in human cognition—as per the genetic epistemology of Piaget—and "sciencing." What they seem to be saying is that the ways of describing the interrelations between one cognitive developmental stage and another can be applied to describing theory reduction in the sciences. They write "theory reduction in science is analogous to the transformation from one developmental stage to the next in general cognition." Thus, they reason, from a biogenetic structuralist position theory reduction does not destroy the efficacy of the reduced theory, for the theory to which it is reduced incorporates and elaborates on it. The reduced theory becomes, so to speak, a special case of the reducing theory. The result is a more comprehensive theoretical structure that includes both theories.

Wimsatt (1976), in a seminal article on reduction and systemic levels of organization, argues for one of the points made by Rubinstein and Laughlin: that it is wrong to assume that reduction entails the eliminability of concepts of the reduced theory. He also argues, however, that a distinction must be made between *intralevel* theory reduction and *interlevel* theory reduction. This is a distinction Rubinstein and Laughlin do not recognize. They cite the case of the reduction of classical mechanics by relativity theory. This is a case of intralevel theory reduction, and, while the relation between these two might more properly be considered a "transformation" (as in other cases of intralevel development), interlevel theory reduction may not be so construed (Wimsatt 1976; see also Nickles 1973). Failure to distinguish between intralevel reduction and interlevel reduction is the most telling fault of their argument.

One of the tenets of biogenetic structuralism (Laughlin and d'Aquili 1974:195) is that "there is no level of reality intervening between *Homo sapiens* as a biological phenomenon and that organism's environment." This tenet can be agreed to, but—and this cannot be stated too strongly—that environment (particularly the parts of it that a human organism interacts with) is already ordered systemically prior to any individual's participation in it. As the neonate grows and matures, it participates in a variety of semiological systems. Though it may combine and recombine the elements of those systems in unique ways, the fact remains that the systems are an isolable phenomenon or, rather, exist as a distinct level(s) of systemic organization. Culture, then, taken as a *heuristic* device and not as something with separate ontological status, would comprise a group of interacting systems, including the environment, with, I might add, strong explanatory power.

The tendency, as Wimsatt points out, is often to seek explanation on a lower level. He writes (p. 249): ". . . the reductionistically inspired feeling[s] that upper-level explanations are always possibly and perhaps even preferably reformulatable in lower-level terms . . . are not only heuristically ill-advised. They are simply incorrect, in at least the following sense: some things have no further explanation at a lower level and for them there is no point in talking about lower levels." Explanatory strategies in which higher-level rather than lower-level explanations turn out to be more fruitful have been used in linguistics. For example, given the two related forms *pater* 'father' (Greek) and *fadar* 'father' (Gothic), a lower-level explanation of the difference between them might investigate the articulatory musculature used in each as well as, perhaps, the neuroconnections, etc. At a higher level, the example would be placed in the more general context of a systemic shift from the Proto-Indo-European labial phoneme /p/ to the Proto-Germanic labiodental fricative /f/. It is far more parsimonious to speak of the sound change in this way than to reduce it to neurological processes—though either would be descriptively accurate.

The biogenetic structuralist approach as originally set forth (Laughlin and d'Aquili 1974) seems to be different from the position taken by Rubinstein and Laughlin. The earlier work says of Durkheim (p. 5, emphasis mine): "he did not consider individual psychology, much less neurophysiology, as a *locus for explanations of society.*" In another passage it equates "mind," "personality," "culture," and "society" by saying (p. 11, emphasis mine) that when philosophers and behavioral scientists use these concepts they are really "referring to patterns abstracted from *behavioral equivalents of internal brain processes.*" Yet another passage (p. 196), labeled a "claim" of the biogenetic structuralist approach, reads: "The modes of 'thought,' 'reason,' 'cognition,' 'sciencing,' 'mythologizing,' 'magical causation,' and the like are actually the behavioral equivalents of internal, neurophysiologically structured, and systematic channels of sensory association and processing characteristic of the human brain, as well as of the brains of other organisms."

Against these claims about biogenetic structuralism—which is, essentially, central-state materialism (see Campbell 1970:86–89)—Rubinstein and Laughlin follow Whitehead: "if (as we believe) the universe is constructed of organismic processes in an infinite concatenation of systems within systems, the search for a level of ultimate constituent elements is futile." Thus, on the one hand they speak of Whiteheadian conceptions of reality as process and levels of systemic organization, while on the other they say that those systems are not really systems at all, but epiphenomena of neurological processes. This is clearly inconsistent.

Among the issues I hope will be clarified by Rubinstein and Laughlin, then, the most important is whether or not a biogenetic structuralist perspective can be fruitful in discussing intertheoretic reduction. Though there is some evidence for its usefulness, along with Piagetian developmental schemata, to explain intralevel theory development,[1] its contribution to insights about interlevel relations is doubtful at best. Secondly, clarification is needed as to whether or not the obviously much stronger statements on biogenetic structuralism outlined in Laughlin and d'Aquili (which remain naively reductionistic despite Rubinstein and Laughlin's efforts) remain in force in light of the latter's inclusion of Whitehead in the biogenetic structuralist fold.

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However commendable their intentions, I have reservations about Rubinstein and Laughlin's furtherance here of interdisciplinary dialogue. I shall summarize my criticisms in the five points following, the first two having more to do with the form of their paper, the latter three concerning more its substance.

1. Inadequately defined concepts hinder evaluation (points lose their poignancy). What criteria distinguish the optimum operation of science? More critical to the exposition, they tell us "the neurognostic model is modified so that it comes into more adaptive isomorphism with the environment." This model is insufficiently characterized: is it to be understood as the iconic model of Suppe (1974), or as the operational model of Caws (1974), or perhaps as the epistemic subject of Piaget (1970: 68–69)? While I understand isomorphism as an identity of structure (cf. Weyl 1949, Rosenbleuth 1970), I understand adaptive isomorphism not at all. And in what sense do structures become *more* isomorphic? Could this be the authors' way of restating Piaget's concept of equilibration?

2. I find puzzling the absence from their citations of works particularly pertinent to the topics addressed. Quine (1951), Hempel (1952), and Scheffler (1957), among others, have along with the authors contributed richly to the seminal discussion of the value to science of "theoretical-empirical considerations and philosophical questions." Does the "biogenetic structural perspective" constitute a special case whereby previous considerations of theory construction by inductive and deductive processes are irrelevant?

3. Closer to the substance of their argument, I am unable to accept the congruence posited between science and cognition. At one point suggesting the integrity of these constructs "(. . . biopsychological processes mediating scientific inquiry"), elsewhere the authors collapse this distinction ("sciencing is a special function of cognition," "theories . . . are schemata that are used for evaluating various stimuli in the environment"). They go so far as to assert that cognitive successions in Piaget's ontogenetic scheme are commensurate with preexisting scientific theories synchronically combined under a single theory. Since I am unable to see how diachronic, hierarchically generative cognitive stages are comparable, strictly or otherwise, with this course of scientific development, I remind the authors of their obligation to demonstrate under the terms of biogenetic structuralism exactly how ontogeny recapitulates this particular phylogeny. It might also be appropriate here to ask them to specify under what conditions they would abandon their position (cf. Lakatos 1970).

4. The authors' treatment of the problems of intertheoretic incommensurability is unjustifiably complacent, suggesting their failure to appreciate fully the issues in question. They give short shrift to the contextualists who embrace alternatives to the Received View, accusing them of offering a false dilemma because "their accounts of the intertheoretic incommensurability of terms lack a biopsychological underpinning." Surely the authors are in error, unless cognitive and semantic concerns be divorced from biopsychology. Hanson (1958), Feyerabend (1970*b*), Bohm (1974), and Kuhn (1970, 1974) have all taken up incommensurability from the perspectives of cognitive and semantic disunity. Whether or not their views and the authors' are in accord is another question, but their respective differences require more than the superficial gloss they receive here (see also Suppe 1974:234–35). I am entirely unfamiliar with the authors' term "the received view of theory reduction," but the *Received View of Theories* is commonly understood in its simplest form as the analysis of theories wherein theories are logically axiomatized such that explicitly defined correspondence rules specify the theoretical terms' relationships with the observational terms (cf. Putnam 1962). In a strict sense, theory reduction is not a part of the Received View (Suppe 1974), although the former follows logically from the latter and the tenability of each rests upon the establishment of correspondence rules. Since the most damaging attacks against the entire structure have been the contextualists', by designating as the primary question here the construal of bridge rules the authors seriously misrepresent current debate. Suppe (1974:4) summarizes it:

> The situation today, then, in philosophy of science is this: the Received View has been rejected, but no proposed alternative analysis of theories enjoys widespread acceptance. More generally, the positivistic analysis of scientific knowledge erected upon the Received View has been rejected, or at least is highly suspect, but none of the alternative analyses of scientific knowledge which have been suggested enjoy widespread acceptance.

These questions being of such central importance to their own thesis, I am again puzzled by the authors' failure to direct our attention to them.

5. The authors outline an approach to theory reduction whereby identity statements replace logically derived correspondence rules. The problem with this tack is that identities are adduced by properties ascertained empirically; this, it seems to me, amounts to a revival of the operationalism made prominent by Bridgman (1927). The deficiencies of this position are well-known (Hempel 1953, 1956). One is that properties observed in the course of operational procedures tend to become as numerous as the operations themselves. Would the authors propose to distinguish "good" operations from "bad"?

Finally, I have no particular objection to the authors' programmatic statement, apart from my preference for *domain* (cf. Shapere 1974, Nickles 1974) over *levels of systemic organization.* Those who subscribe to the synthetic theory of evolution have long managed comfortably with a recipe of this kind. But any suggestion that its application makes theoretical revision unnecessary is naive.

[1] Gardner (1972) briefly discusses the idea that Piaget's developmental schemata can be put to use in explaining the development of science, but points out that there is already counterevidence within just one science, geometrical thought. Gardner points out (pp. 235–37) that the child begins with a topological scheme and only later comprehends in terms of Euclidean space. In the development of geometrical thought from the time of the Greeks, this sequence is reversed.

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The negative attitude toward "psychologizing" in sociology and anthropology and toward "physiologizing" in psychology has flourished in the context of the extreme environmentalism that has until recently characterized the behavioral sciences. If the brain is infinitely plastic and if the mind is a tabula rasa, then the anthropologist does not need to worry about psychological determinants of cultural events and the psychologist does not need to worry about physiological determinants of mental events. A completely plastic brain will obviously set no limits on mental events. Analogously, the mental tabula rasa will not constrain, determine, or influence cultural events. In this view, each lower level is essentially a passive receptacle for events at the higher level. As such, it can be ignored. If these beliefs are false—if both brain and mind are structured in predetermined ways—then antireductionism becomes untenable, since it becomes obvious that physiological processes will at least constrain psychological processes and the latter will in turn constrain or determine cultural ones. With the increasing rejection of extreme environmentalism, explicit attention to the sorts of issues raised by Rubinstein and Laughlin becomes necessary.

While I agree with the desirability of multilevel explanations and react favorably to Rubinstein and Laughlin's cognitive theory of science, I find myself wishing that they had kept their philosophy of science separate from their theory of science. Presumably, in reacting favorably to the two parts but unfavorably to the amalgamated whole, I am myself exhibiting an antireductionist attitude. Rubinstein and Laughlin seem to misunderstand the reason for antireductionism. They ascribe it to fear that theory reduction will be so successful that one's whole discipline will be reduced out of existence. The real reason would seem to be not a fear that reduction will succeed too well, but just the reverse: a fear that it will fail. Antireductionism in the behavioral sciences is in reality based on distaste for speculation.

If scientific theories are schemata for evaluating environmental stimuli, then we can derive predictions concerning the affective response to such theories from biopsychological work on hedonic tone (Berlyne 1971). Very large discrepancies between schemata and reality should eventuate in negative affect, while medium discrepancies (e.g., a theory that leaves some possibilities for further work) should lead to positive affect. A perfect fit between schema and reality should elicit boredom and inattention. Thus, if multilevel behavioral theories tend to be more discrepant from reality than single-level theories, this would explain why they tend to be disliked.

Any theory has some probability of being "correct." In the behavioral sciences, this probability is never very close to 1.00. It follows, then, that amalgamating probabilistic theories from more than one level will lead to a theory that is appreciably less likely to be "correct" than either of its component theories. For example, Rubinstein and Laughlin reduce or amalgamate their theory of science to a neuropsychological theory of cognition. The reducing theory is plausible but far from certain; the reduced theory is also plausible but not certain. The resultant multilevel theory will necessarily be less plausible than either the reduced or the reducing theory. If the probability of each theory taken by itself is, say, .70, then the probability of the amalgamated or multilevel theory must be $.70 \times .70 = .49$. A theory with a .70 probability of "correctness" will be moderately discrepant from reality. It should, thus, elicit positive affect and verbal labels such as "interesting." A theory with a .49 probability of "correctness" will be rather discrepant from reality. It should, because of this alone, elicit negative affect.

An analogous difficulty with multilevel theories concerns bridging. Rubinstein and Laughlin speak of being able to establish identities between constructs at different levels. While this is obviously possible in theory, such identities tend to degenerate into correlations on the empirical level. These correlations may be rather weak. Mischel (1968), for example, shows that internal traits such as "honesty" account for so little of the variance in "honesty behavior" that they can almost be ignored. If this is the case, then it is certain that the neurological anlagen of "honesty" would exert virtually no real behavioral effect.

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Rubinstein and Laughlin's paper, taken as a whole, brings back memories of the frustrating discovery of early childhood that it is physically impossible to pick yourself off the floor even though you have the physical strength to lift a weight equal to that of your own body. I am reminded also of the timeworn metaphor of the dog chasing its own tail. This is not to say that the task the authors have set for themselves is futile. Indeed, their work seems to take a direction which must be pursued if science is to rise above itself, as it were, in understanding understanding. What is needed, of course, are extrascientific ethnographers of science. In the absence of such, we must do it ourselves. Thus, even in our "empirical considerations" we find ourselves again with the problem of being both "judge and party," which Piaget identified as the shortcoming of "introspection." This, I think, explains why "anthropologists have held the philosophy of science at arm's length"; they generally recognize the extreme difficulties entailed in "objectively" studying their own culture—particularly its most fundamental premises. Science itself, after all, is a complex of cultural traits which in theory should admit of the same kind of scrutiny anthropologists have given to, say, the "guardian spirit complex" or "the state."

Even though Rubinstein and Laughlin seem to imply that the conception of "levels of systemic organization" is given in nature, and not merely in our conception of nature, their "biogenetic structural theory of science" is a contribution toward revealing the arbitrary, artifactual nature of concepts and categories within the "science complex" which stand as our own culture-specific barrier to the further extension of the "empirical modification cycle."

In passing, I was amused by Rubinstein and Laughlin's tactful ambiguity in their opening paragraph, wherein it is not clear whether it is our paper, the remarks of our critics, or both for which "TenHouten has provided a much-needed corrective."

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Rubinstein and Laughlin bravely attack the philosophical misconceptions that have hindered recognition of the value of "neuroanthropology." Accounts of the biopsychological bases of human adaptive strategies—known collectively and variously as culture, society, language, cognition, kinship, exchange, religion, marriage, politics, art, technology, and even science—promise to reintegrate our splintered "social sciences." Unfortunately, the fragmentation of knowledge into imperiously autonomous disciplines has been exacerbated in many fields by a pugilistic model of scientific development wherein the scientists exploring a common problem engage in prolonged and often interminable battle, each attacking all of the others while defending some particular "correct" way of viewing the problem. Such an atmosphere is hazardous to the health of any species of reductionists, no matter how clearly they may state their allegiance to a correspondence theory of reduction, wherein the integrity of various levels of analysis is to be enhanced and clarified rather than eliminated. Scientists, especially "social" scientists, have invested so much psychic energy in the development and maintenance of their respective disciplines, subdisciplines, and schools that they tend to respond with great anxiety to any suggestion that their work is subject to "interpretation" at some lower, more "physical" level of analysis. Their reaction is, however, analogous to that of a weatherman who fears the theoretical physicist: "Almost certainly, every hurricane is a physical event; but from the fact that hurricanes are not occult, it hardly follows that particle physics provides the appropriate vocabulary for doing meteorology" (Fodor, Bever, and Garrett 1974:xiii).

At least, this is one way to view the contribution of Rubinstein and Laughlin. Another, less comforting aspect of their account concerns "structural merger" among various levels of analysis. If theory reduction truly is "analogous to the transformation from one developmental stage to the next in general cognition," then a reducing theory would appear to be insufficient to account for all of the phenomena accounted for in the theory it reduces, just as Piaget's theory of formal operations in the adolescent is insufficient to account for the incorrect analyses of experience exhibited in the behavior of a two-year-old. This developmental analogy thus seems to transform the present plea for "bridging levels of systemic organization": instead of exhorting us to broaden our horizons and learn to benefit from other types of social analysis, both the more "macro" and the more "micro," Rubinstein and Laughlin urge us to put aside childish approaches to the analysis of human behavioral patterning and begin to grow up. Their insistence that prior modes of organization are "*strictly preserved*" will hardly mollify the sophisticated student of, say, rules of reciprocity who sees self-styled neuroanthropologists proposing to reduce his sensitive historical-symbolic research to so many neural circuits doing minimax cost-benefit analyses of personal experience (the example is fictitious, but its thrust is not). The neuroanthropologists, in this light, take on the countenance of yet another know-nothing horde to promise a revolutionary view of the field.

A more sympathetic reading of Rubinstein and Laughlin, however, suggests that the analogy between the structural merger of scientific theories and the structural progression of mental development is to be interpreted more abstractly. As they say, the levels of organization in scientific theory are mutually dependent rather than competitive, mutually instructive rather than conflicting. Accounts that are explanatory at two different levels of analysis can therefore yield any one of three possible outcomes in the process of structural merger: (1) they can prove to be bridgeable without modification of the elements or relations at either level (the least likely outcome, given our current level of scientific advance); (2) they can prove to be bridgeable after modification of various aspects of the substantive and formal claims made at either or both levels of analysis (the hoped-for salutary outcome and clear purpose of the whole theory-reduction procedure); or (3) they can prove to be incapable of merger, indicating that the two levels of analysis are insufficiently developed for structural bridging (the most likely outcome in most areas of social-scientific investigation). The crucial type of outcome is, of course, Type 3. Whenever demonstrably relevant variables at one level of analysis are left "unbridged" to variables at the other level, it must be determined whether this gap is fortuitous, to be filled as our knowledge of the second level expands, or systemic, categorically incapable of being filled because of inherent incompatibility of the assumptions and methods across the two levels.

A pointed example of an allegedly fortuitous gap that is now gradually being recognized as systemic should serve to indicate how theory reduction can sometimes result in the elimination of a theory by showing it to be "wrong and unnecessary" (an outcome that Rubinstein and Laughlin explicitly reject but that may in fact be the most valuable potential contribution of neuroanthropology). In linguistics, it has long been assumed that an abstract model of language structures is in principle bridgeable to a concrete model of language processes. The system of linguistic elements and their actual and potential relations and orderings that constitutes the linguist's view of *langue* or grammar or competence (the terminology differs from school to school) is believed to be mergeable with the psychologist's account of linguistic activities variously termed *parole* or behavior or performance, although each of the schools has its own set of prejudices as to the nature of the bridge laws between language as a social or associational or cognitive fact and language as an individual or triggered or pragmatic fact. The problem is that mounting evidence (much of it recent and some of it explicitly neurological) shows that the sorts of elements, strategies, and processing components needed for a psychology of language that would be compatible with established facts about people's comprehension and production of language are simply incompatible with the sorts of elements, formalisms, and structural components postulated in linguistic theory (see Straight 1976). It is reasonable, and, to my mind, exciting, to expect that attempts to formulate the bridge laws between levels of systemic organization in the explanation of social phenomena will lead to the debunking and early abandonment of erroneous theories. And if linguistics is an accurate predictor, structuralism (even Rubinstein and Laughlin's biogenetic structuralism) may be among the first to fall.

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The article by Rubinstein and Laughlin serves as a very good reminder that theory development does not take place in a sterile, nonbiological environment. Theory development in all the sciences is the result of complicated interaction between the human body and brain and an environment. This same idea underlies the discussion presented in the Paredes and Hepburn (1976) article, which the authors mention.

Paredes and Hepburn attempt to link apparent cross-cultural differences of cognitive styles to a "split-brain" explanation. While theirs is an interesting hypothesis, the fact remains that explanation of these differences in cognitive styles is examined through a researcher's own neurognostic model, developed through experience derived from interaction with his/her own environment. Rubinstein and Laughlin's article is an important statement reminding us of this point.

According to the article, sciencing must be an extension of "fundamental biopsychological processes." It is clear that we must be prepared to examine the nature of these biopsychological processes as intensively as we examine the phenomena that arise out of them, not only as regards the subjects of behavioral research, but also as regards the personnel conducting the research and developing the theory to explain behavior. Laughlin and d'Aquili's (1974) work is a major step in that direction.

Unfortunately, the present article does not deal as substantively with this issue as I would have liked. The authors seem to be most concerned instead with a specific problem in the philosophy of science, that is, theory reduction and bridging. They tackle the question by suggesting that there is a parallel between the Piagetian formula of developmental stages and the concepts of "elaboration, transformation, and incorporation." They do a good job of showing that the parallel does exist, yet they present it as if it were a "major" discovery. Repeating a

Piagetian formula and relating it to the process of sciencing is not enough proof that here indeed is the way sciencing progresses cognitively. Empirical data should have been marshalled to exemplify and substantiate the conclusions that are drawn.

By the end of the article the authors feel that they have created more favorable conditions for the convergence of the philosophy of science and anthropology, but that remains to be seen. We still need to know whether their own neurognostic model can be confirmed, and only more research and further "sciencing" will be able to provide the necessary proof.

by INA JANE WUNDRAM
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The paper by Rubinstein and Laughlin speaks to a significant issue in current anthropological thought. The behavioral sciences and the biological sciences have developed to the point where each would benefit by integration with the other, and yet the means by which such integration might be effected remain elusive. The recent attempt by sociobiologists to explain behavior in terms of genetically based altruism is representative of this need for integration. Similarly, the reaction against these ideas by many social anthropologists and other behavioral theorists reflects concern that the more traditional concepts of behavior not be wholly eliminated. Rubinstein and Laughlin, from the standpoint of scientific philosophy, offer a solution to this dilemma. By presenting an approach to theory reduction called "reduction by incorporation," they suggest that it is indeed possible to speak of conceptual frameworks and neurophysiological models in the same breath without undermining either theoretical approach. In other words, there is no need for social anthropologists to react against "psychologism"; the fact that culture may have biological correlates does not destroy the concept of culture itself.

In discussing biogenetic structuralism, the authors use the word "science" as a verb; thus, "sciencing" becomes a basic cognitive process which can be subjected to a biopsychological analysis. By analogy one could also use the word "culture" as a verb, whereby "culturing" would be the cognitive process by which an individual experiences and uses his/her culture. Like "sciencing," "culturing" is a process by which the brain constructs an adaptive model of its environment, and no understanding of the process of "culturing" is possible apart from the study of the cognitive function of the brain. "Culturing," like "sciencing," is a cognitive system that develops through a series of stages of increasing hierarchical complexity. (Indeed, the two systems are in all probability different aspects of a more basic developmental process common to all biological organisms.) The development of "culturing" depends on the interaction between the patterning of neuroanatomical tissue in the nervous system and the input provided by the cultural environment. The relative contributions of the two components in a given individual (or in a given culture) have yet to be described, but it is the major task of neuroanthropological theory to determine their nature and their interaction.

By showing that the reduction of one theory by another does not require the obsolescence of the reduced theory, Rubinstein and Laughlin provide a theoretical basis for the development of a comprehensive neuroanthropological methodology. It is unfortunate that the article is written in the terminology and phraseology of the philosophy of science, for the ideas therein may seem obscure to some anthropologists, especially students. For those who will make the effort, however, it is encouraging to discover the possibility of a structural unification of theories about the nature of human beings and perhaps, ultimately, the nature of life itself.

Reply

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The purpose of our article was to discuss, and provide an alternative perspective on, a central but controversial aspect of contemporary anthropological theorizing. A number of commentators (Brady, Burton-Bradley and Pataki-Schweizer, Count, Wallace, and Wundram) are in basic agreement with our approach. Others, however, express disagreement with either specific claims in, or the general orientation of, our paper. In this reply we will respond to these comments by elaborating the themes in our paper that seem to be at the roots of these disagreements.

Gallus, Korey, Martindale, and Straight focus on our view of the structural basis of theory, on the growth of theory *qua* structural elaboration, and on the implications of our views for an understanding of science and scientific theory. It is important to emphasize that our approach to human cognition is a structural one. That is, we take as basic structural, rather than content, variables (see Schroder et al. 1967, Cole and Scribner 1975). This distinction is of profound importance for our understanding of human cognition, and we will briefly elaborate our discussion of the structural basis of human cognition here. In our paper we sketched what we felt to be the major features of the growth of cognition, and we indicated the relevance of the work of the conceptual-systems group (Harvey, Hunt, and Schroder 1961, Schroder, Driver, and Streufert 1967, Schroder and Suefeld 1971) to the understanding of these processes. It seems to us that making the relevance of this work explicit will help to clarify our position and will provide the base from which to respond to several commentators.

The conceptual-systems approach to human cognition is a stage theory in which each stage arises out of and incorporates the previous stage in a sequence that is thought to be invariant (Harvey et al. 1961). As we move from one stage to the next, we find that each stage is differentiated from the preceding one by greater structural complexity (discussed below, and see Schroder 1971, Schroder et al. 1967). Development proceeds from an initial stage of undifferentiated globality to a stage which is highly articulated and organized (Harvey et al. 1961, Schroder et al. 1967). The higher stages are considered to be more efficient and comprehensive information-processing structures that are able to deal with larger and more diverse bodies of information (Schroder et al. 1967, Schroder 1971). These higher stages are considered to be more stable and generally more adaptive than the stages that precede them.

Structural complexity is determined, in the conceptual-systems account, by two variables: dimensions and rules. The variable *dimensions* refers to the number of units or parts of information considered during information processing. It denotes the ways in which a set of stimuli can be ordered or scaled. Dimensions are, then, ordering principles, or categories, used for interpreting information from a stimulus domain (Schroder 1971). Each information-processing structure (conceptual system) will be made up, in part, of dimensions representing independent attributes along which stimuli can be ordered (Schroder et al. 1967). *Integrating rules* are of two types: *fixed rules*, which are rigid, minimally modifiable guides for information processing, and *emergent rules*, which are highly flexible and capable of generating many, and new, perspectives.

Structural complexity, then, is a measure of integrative complexity of information-processing structures. This complexity is dependent on *both* the dimensions and integrative rules in the structure. As Schroder et al. (1967:7) point out,

> The number of dimensions is not necessarily related to the integrative complexity of the conceptual structures, but the greater the

number of dimensions, the more likely is the development of integratively complex connection rules. Low integration index is roughly synonymous with a hierarchical form of integration, in which rules or programs are fixed.

... High integration index structures have more connections between rules; that is, they have more schemata for forming new hierarchies, which are generated as alternate perceptions or further rules for comparing outcomes.

Conceptual structures of differing complexity have different ramifications for information processing. Schroder (1971, Schroder et al. 1967) distinguishes four stages of structural development, with five structural patterns. These stages and patterns, which correspond roughly to the stages of ontogenetic development described by Piaget and his associates (see Flavell 1963, Phillips 1969), are:

Stage 1: Unidimensional, single-rule structures.

Stage 2: Multidimensional, single-rule structures.

Stage 3: Multidimensional, multirule structures.

Stage 4, Structural Pattern A: Multidimensional, multiconnected rule structures of moderately high integration.

Stage 4, Structural Pattern B: Multidimensional, multiconnected rule structures of high integration.

Information processing employing the lower levels of structure tends to be concrete, to be oriented towards external standards, to utilize categorical thinking, and to avoid ambiguity and conflict (fig. 1). In addition, Schroder (1971:257) says of these lower levels of structuring (especially single-rule structures) that they show a "tendency to standardize judgments in novel situations; a greater inability to interrelate perspectives; a poorer delineation between means and ends; the availabilities of fewer pathways for achieving ends; a poorer capacity to act 'as if' and to understand the other's perspectives; and less potential to perceive the self as causal agent in interaction with the environment."

Information processing using higher levels of structuring, in contrast, displays flexibility and mobility and can generate multiple perspectives on and solutions to a given problem (fig. 2).

While Martindale agrees with our theory of science, he suggests that antireductionist biases in anthropology, sociology, and psychology rest on the fear that reduction will fail. He argues that "amalgamating probabilistic theories from more than one level will lead to a theory that is appreciably less likely to be 'correct' than either of its component theories." He provides the example of the combination of two theories each of which has a probability of .70; he suggests that the resultant multilevel theory will have a probability of .49. While his example is correct in the instance where theoretical amalgamation is effected through the use of an additive combinatory principle, it fails to capture an essential aspect of our view of theory reduction—that reduction is a process of structural elaboration. The implications of development through struc-

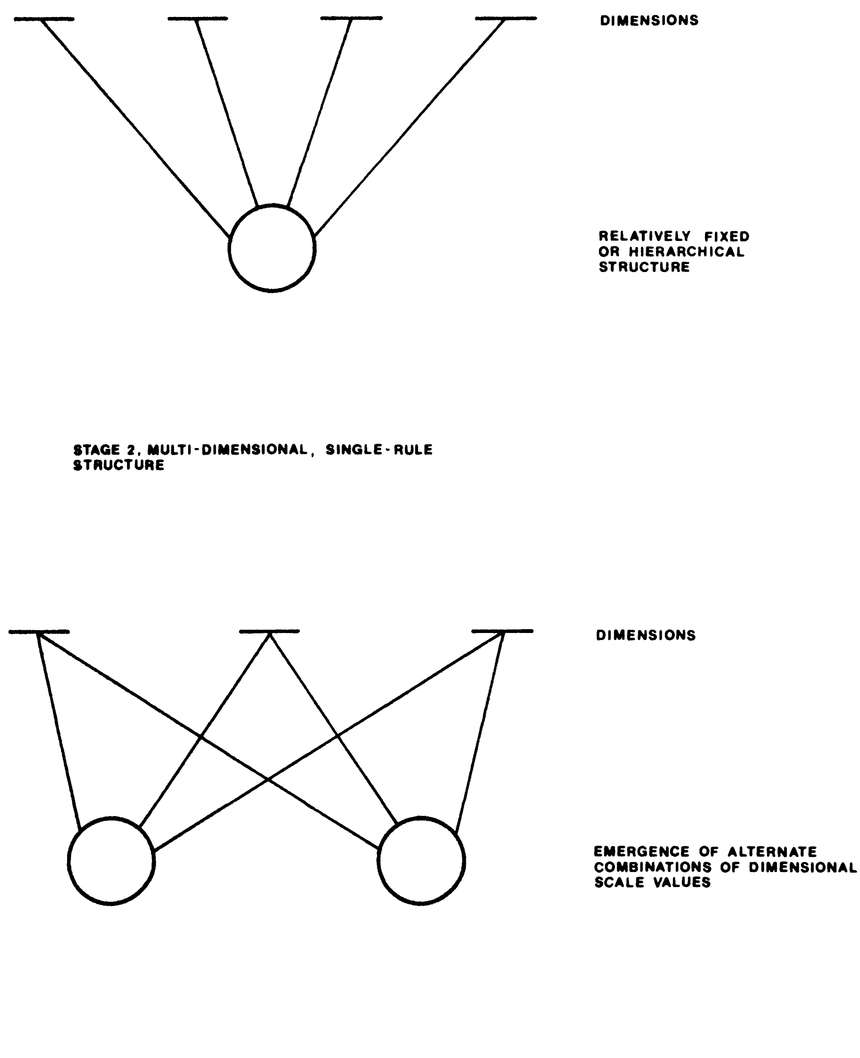
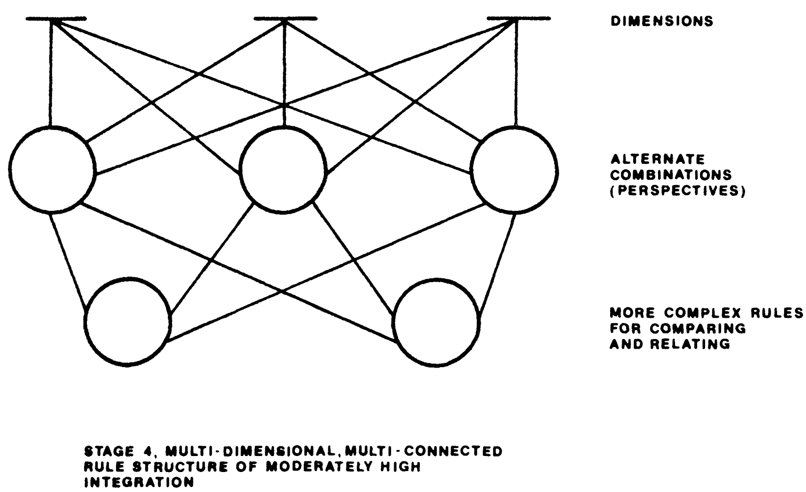


FIG. 1. Lower levels of conceptual structuring (after Schroder, Driver, and Streufert 1967).

tural elaboration are many. Principally it appears, however, that development through structural elaboration is a process of *both* quantitative and qualitative change. Thus, for example, although there are quantitative changes between Stage 3 and Stage 4 structures (see figs. 1 and 2) that may be measured on an additive scale, the increased interconnectedness of the structure has profound qualitative ramifications for the stability and flexibility of the structure and for the mode of information processing it employs. This kind of qualitative shift, due to quantitative growth, has been demonstrated in both biological evolution (Rensch 1959, Laughlin and d'Aquili 1974) and developmental semantics (Rubinstein 1976), and its operation in the transition between states of consciousness has been suggested (Tart 1975:243–57). A structural-elaboration view of reduction, then, suggests that the combinatory principles which function during the incorporation of theories from different levels are distinct from the additive principle underlying Martindale's example and that an a priori rejection of reduction based on the fear that the "correctness" of the multilevel theory will be less than that of its component theories is unwarranted.

Viewed as a process of structural elaboration, the reduction of theories requires the structural merger of the reduced theories. A ramification of the resultant change in the structural matrix in which the theories are placed is the conceptual revision of the reduced theories. Thus, Korey misreads us when he suggests that we see theoretical revision as unnecessary. In fact, as we have said, theoretical revision is a necessary outcome of reduction *qua* structural elaboration. Regarding Korey's queries about our understanding of "isomorphic" and "more isomorphic," we direct him to Piaget (1971) and to d'Aquili, Laughlin, and McManus (n.d.). To the extent that Korey and Boggs read us as suggesting that Piaget's ontogenetic scheme is commensurate with existing theories, they fail to understand our use of the Piagetian position. It is not the ontogenetic scheme itself that is of importance to our understanding of sciencing; rather, it is the theoretical system which underlies the ontogenetic scheme (see Furth 1969).

Straight notes that we have said nothing about cases where the multilevel theory requires for its confirmation the falsification of a single-level theory. We agree with him that we can expect cases of this sort to yield some of the more exciting results in science. This (along with, as Korey suggests, intertheoretic commensurability) deserves more detailed treatment from our biopsychological perspective. However, the nature of these cases seems to us to be so radically different from that of cases in which the multilevel theory can successfully incorporate



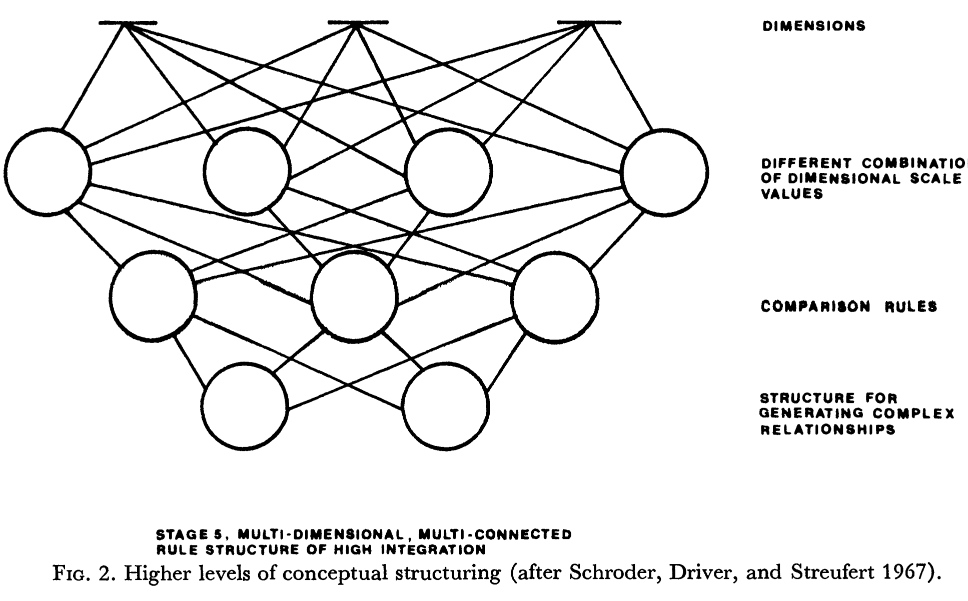


FIG. 2. Higher levels of conceptual structuring (after Schroder, Driver, and Streufert 1967).

single-level theories that we have left the issues raised by these cases for extended consideration elsewhere.

Gallus misconstrues our position in a number of places. First, as we have indicated above, our view of theory reduction *does* consider the construction of multilevel theories a process involving qualitative change. Although we may disagree with Gallus on exactly what the ramifications of this change are (Rubinstein, Laughlin, and McManus n.d.), we would emphasize that we do not view theory reduction simply as an additive process. Second, we do not hold that theory reduction is "an ultimate stage in human cognitive development" or that "we already know enough about the 'cognitive function of the brain' and its development." Rather, we are arguing for the efficacy of theory reduction for the development of scientific understanding and for the employment of what we do know about the cognitive functions of the brain as an aid in self-understanding (cf. Stent 1975:1055).

Indeed, with Piaget (1971), we hold that all knowledge is information structured by the brain. This view implies, among other things, that all knowledge is incomplete—fallible in Peirce's sense (Almeder 1973, 1975). Knowledge may be fallible because of incomplete information content (the most common sense of the term) or because the structural organization of that information is far simpler than the reality of concern. Either way, with Pribram (1971) and Chaney, we would pay utmost attention to anomalies and "paradoxes." Also, we do not hold that reduction by incorporation is an end in itself. Rather, it is a means to the most complex model building available to us at the moment, one that has a reasonable chance of transcending the more deleterious effects of disciplinary involution. Furthermore, recognition of epistemological fallibility requires that we constantly maintain conscious distinctions between (1) the "logic-in-use" (how the cognitive system processes information about the organism and its world) and the "reconstructed logic" (the fallible but conscious model of how the cognitive system processes information; see Kaplan 1964); (2) "content" (information about the world) and "structure" (the organization imposed on information by the cognitive system); and (3) the "cognized environment" (or set of neurognostic models, constructed by the nervous system, depicting the environment) and the "operational environment" (or set of efficacious relations actually obtaining within the organism and between the organism and its surround; see Laughlin and Brady 1977, Laughlin, Shearer, and McManus 1977, Shearer, Laughlin, and McManus 1977).

Failure to maintain these distinctions underlies a number of criticisms among the comments. For instance, all the diversity Chaney indicates as being of interest to him is, to the best of our view, diversity of content. It is ironic that he should cite the later Wittgenstein—the Wittgenstein of the *Investigations*, who had moved away from the essentially structuralist inquiry of the *Tractatus* to the content-laden "emics" of language games. Biogenetic structural theory is interested primarily in the structure of information processing and only secondarily with the content being processed. Life in Africa may well be more poetical in content, as Horton (cited by Chaney) suggests, but analogous to our own more mundane thought in structure.

Burton-Bradley and Pataki-Schweizer as well as Hepburn point to the "protean" methodological difficulties involved in carrying out the rule of minimal inclusion. We agree that the difficulties that they envision are real. Rather than abandoning the principle, however, we would suggest developing more sophisticated team research methodologies. Our biogenetic structural group, for example, has responded to this problem by instituting an efficient information-exchange network that thus far controls theory and research in a variety of disciplines, including anthropology, sociology, philosophy, cognitive, developmental, and transpersonal psychology, psychiatry, ethology, neurobiology, theology, and archaeology. The products of this exchange are various (e.g., Laughlin and Brady 1977, d'Aquili, Laughlin, and McManus n.d.) and have been facilitated by a commitment by all to a pyramiding (rather than a zero-sum) style of theory construction. At the moment, various members of the group are researching such diverse topics as depression syndrome, sleep and dreaming, community intervention, the relationship between cognitive and linguistic development, the neurobiology of hoarding and altruism, and the possibilities of a cognitive archaeology—all within a single, emergent theoretical framework. None of this would be possible without, as Burton-Bradley and Pataki-Schweizer note, a maximal effort on the part of everyone involved at communication and ego-control.

Byrne is correct to point out that what we identify as undesirable consequences of the received view of theory reduction are not formal aspects of Nagel's model. Indeed, it was not our intention to suggest that this is the case; to the extent that this is the meaning that is conveyed we have expressed ourselves poorly. What we wished to indicate is that the undesirable consequences of the received view of theory reduction that we identify have been drawn as corollaries by many practicing scientists. He is also correct to suggest that the thesis of our paper might better be expressed as follows: that no understanding of science can be complete unless it is firmly grounded in a sophisticated biopsychology. We disagree with Byrne, however, when he suggests that in our view of the development of science the course of that development could have been no different. While we do think that the cognized environment (E_c) moves towards isomorphy with the operational environment (E_o), we have not suggested, nor does it follow logically, that the course of this development is either steady or unilinear.

Implied in Count's remarkable comment is the growing intolerance many of us share for simple models of complex reality. In the human brain and its functional by-products we are confronted by the most complex system in the known universe. Simple, unidimensional models of human behavior will no longer do. Anthropology is especially prone to oversimplification. Everything with which anthropology, as the "science of humankind," is concerned derives from nervous-system processing. Yet, in the face of more than 100 years of neuroscience, anthropologists still treat the brain as a "black box." This is particularly embarrassing when nonbiologically trained spokesmen like Lévi-Strauss discuss the brain and the physiology of perception (Lévi-Strauss 1972) and simultaneously maintain an ontological distinction between cognition and action (Lévi-Strauss 1971, on the relation between myth and ritual). If one contrasts Lévi-Strauss's position with that of a biologically astute scholar (e.g., Count 1960, Reichel-Dolmatoff 1976), the point becomes evident (see also Geertz 1973, as suggested by Göhring).

This, combined with an attitude of intransigence toward pyramiding theory, has led to an endless series of polemics. Contrast, for example, (1) simplistic models of culture change common in anthropology with topological models (e.g., the "catastrophe theory" of Thom 1975); (2) arguments over whether enculturation involves transmission, transmutation, or innovation of information with the developmental theory of Piaget (1952, Piaget and Inhelder 1969; cf. Hallpike 1976); (3) the descent-alliance argument (see Needham 1962), the formalist-substantivist debate (see LeClair and Schneider 1968), and the diffusion-functionalism argument (Driver 1966) with Count's view of humans as a phenomenon within fields of phase hyperspace, a complex view in keeping with modern ecology (Odum 1971).

Chaney rightly points to the importance of understanding the relationship between cognition and affect. The brain, of course, mediates both, and it seems highly unlikely that an adequate explanation will be forthcoming from either anthropologists or philosophers in the absence of a thorough grounding in the neurobiology of affect. More specifically, we would suggest close attention to works on affect and cognition by Gellhorn and Loofbourrow (1963), Gellhorn and Kiely (1972), Lex (n.d.), Chapple (1970), Berlyne (1971), Beck (1967), and Seligman (1975). In reference to the symbolic function and the brain we recommend the works of Pribram (1971), Hebb (1968), Sperber (1975), and Laughlin (1977). Finally, it is Piaget's explicit position that many of the "discoveries" in the history of logic and mathematics are actually formalizations of principles inherent in cognitive operations (Piaget 1970, Beth and Piaget 1966). Thus, genetic epistemology is eminently applicable to an understanding of the progress of science (a point disputed by Boggs, Chaney, and Korey).

It would seem that Gallus has misunderstood the concept of neurognostic model by equating the term with Jung's archetype. An archetype is a genetically inherited symbol for an aspect of personality (e.g., mandala, anima-animus). A neurognostic model is the neurophysiological structure underlying some cognitive or other neural function. The concept has more in common with Hebb's (1949) cell assemblies or Graf's (1975) condensed experiential systems (see Laughlin 1975).

Hepburn is not the first to point up the apparent reductionism in various statements made in *Biogenetic Structuralism*, and the fault for this confusion is borne solely by the authors. What they attempted was an (admittedly clumsy) rejection of epiphenomenalism—that is, views such as Durkheim's (1966) that facts at the societal level of systemic organization could only be explained by models structured at that level, or views such as those in which "mind" and "culture" have existence independent of the brain. We see our paper as one step toward clarifying the biogenetic structural position in relation to this question.

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