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COMPLEXITY AND ORGANIZATION*

from K. F. Schaffner and R. S. Cohen, eds., *PSA-1972 (Boston Studies in the Philosophy of Science, volume 20)*, Dordrecht: Reidel, pp. 67-86.

In his now classic paper, 'The Architecture of Complexity', Herbert Simon observed that "... In the face of complexity, an in-principle reductionist may be at the same time a pragmatic holist." (Simon, 1962, p. 86.) Writers in philosophy and in the sciences then and now could agree on this statement but draw quite different lessons from it. Ten years ago pragmatic difficulties usually were things to be admitted and then shrugged off as inessential distractions from the way to the *in principle* conclusions. Now, even among those who would have agreed with the *in principle* conclusions of the last decade's reductionists, more and more people are beginning to feel that perhaps the ready assumption of ten years ago that the pragmatic issues were not interesting or important must be reinspected. This essay is intended to begin to indicate with respect to the concept of complexity how an *in principle* reductionist can come to understand his behavior as a pragmatic holist.

I. REDUCTIONISM AND THE ANALYSIS OF COMPLEX SYSTEMS

A number of features of the reductionistic orientation contribute to a point of view which is ill-suited to an adequate treatment of the concept of complexity:

(1) There is a bias towards theoretical monism. In biology and the social sciences, there is an obvious plurality of large, small, and middle-range theories and models, which overlap in unclear ways and which usually partially supplement and partially contradict one another in explaining the interaction of phenomena at a number of levels of description and organization.¹ In spite of this plurality all of the models, phenomena and theories in a given area (however that be defined)² tend to be treated as ultimately *derivative* from one primary theory. This means that questions concerning their relationships to *one another* tend to be ignored on the supposition that all will be made clear when their relationships to the perhaps as yet unknown reducing theory are determined.

But scientists must work with their plurality of incompletely articulated and partially contradictory, partially supplementary theories and models. The requirements of this situation have not been extensively investigated by philosophers, though this kind of theoretical pluralism has played an important role in the analyses of some biologists,³ and I will argue that it is central to the analysis of our intuitive judgments of complexity.

(2) Given the difficulty of relating this plurality of partial theories and models to one another, they tend to be analyzed in isolation, with frequent unrealistic assumptions of system-closure (to outside 'disturbing' forces) and completeness (that the postulated set of variables are assumed to include all relevant ones.)⁴ But these incomplete theories and models have, *individually*, impoverished views of their objects. Within each, the objects of the theory are just logical receptacles for the few predicates the theory can handle with manageable degrees of theoretical simplicity, accuracy, closure, and completeness. Nobody attempts to put these views together to see the 'resultant' objects. It is as if the five blind men of the legend not only perceived different aspects of the elephant, but, conscious of the tremendous difficulties of reconciling their views of the same object, decided to treat their views as if they were of *different* objects. The net result is often not to talk about objects at all, but to emphasize predicates, or the systems of predicates grouped together as theories or models.⁵

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Thus, although biologists, social scientists, and others who work in areas where 'complexity' is a frequent term talk almost invariably of the complexity of *systems* (thereby meaning the objects, in the full-blooded sense, which they study), most analyses of complexity in the philosophical literature have been concerned with the simplicity or complexity of sets of predicates or of theories involving those predicates in a manner jumping off from the pioneering analyses of Nelson Goodman (1966, pp. 66-123).⁶ But the Goodmanian complexity of a theory even if generally acceptable is a poor measure of the complexity of the objects of that theory unless the theory gives a relatively complete view of those objects. Short of waiting for the ultimate all-encompassing reduction to an all-embracing theory, one can only talk about the internal complexity of our different theoretical perspectives or 'views' of an object. Nor could one avoid this conclusion by taking the complexity of the object as some aggregate of the complexities of the different views of the object, since part of its complexity would be located at the interfaces of these views -- in those laws, correlations, and conceptual changes that would be necessary to relate them -- and not in the views themselves.

(3) It thus would be profitable to see how we tend to relate our different views or theoretical perspectives of objects and in particular of complicated objects. This would be an enormous task for even two views where these views are theories or theoretical perspectives if we take that task to be equivalent to relating those theories conceptually, thus unifying them into a single theory. Fortunately, there are ways of relating the different views through their common referents or objects -- if we are willing to assume, *contra* Berkeley and modern conceptual relativists (for different reasons in each case) that these different views *do* have common referents. An appreciable amount of work has been done by modern psychologists and others on the identification, reification (or, as Donald Campbell says, 'entification') delineation, and localization of objects and entities.⁷ Most interesting in the present context is the emphasis on the importance of boundaries of objects. This work would have been ignored just a few years ago as irrelevant to philosophy of science and appeals to it would have been regarded as 'psychologism'. Nonetheless, it has an important bearing on the ways in which we decompose a system into subsystems and upon how we conceive the results.

II. COMPLEXITY

There are a number of factors relevant to our judgments of the complexity of a system, though I will here discuss only two, which I will call 'descriptive' and 'interactional' complexity, respectively.⁸

Kauffman (1971) advances the idea that a system can be viewed from a number of different perspectives, and that these perspectives may severally yield *different non-isomorphic* decomposition's of the system into parts. A modification of his point has an application in the analysis of complexity: systems for which these different perspectives yield decompositions of the system into parts whose boundaries are not *spatially coincident* are properly regarded as more descriptively complex than systems whose decompositions under a set of perspectives are spatially coincident.⁹

Assume that it is possible to individuate the different theoretical perspectives, T_i , applicable to a system. Each of these T_i 's implies or suggests criteria for the identification and individuation of parts, and thus generates a 'decomposition' of the system into parts. These decompositions, $K(T)_i$, I will call 'K-decompositions'. The different $K(T)_i$ may or may not give spatially coincident boundaries for some or for all of the parts of the system. The boundaries of two parts are spatially coincident if and only if for any two points in a part under $K(T)_j$ these points are in a single part under $K(T)_k$, and conversely. This is, of course, spatial coincidence defined relative to $K(T)_j$ and $K(T)_k$, but it can be generalized in an obvious manner. If all of a set of decompositions, $K(T)_i$, of a system produce coincident boundaries for all parts of the system, the system will be called *descriptively simple* relative to those $K(T)_i$.¹⁰

If two parts from different $K(T)_i$ are not coincident, but have a common point which is an interior point of at least one of them, then there are a number of different mapping relations which can hold between their boundaries, each

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of which contributes to its *descriptive complexity*. Specifying these mapping relations for all parts of the system under both decompositions gives a complete description of this complexity from a set theoretic point of view.¹¹

Different level theories of the same system (e.g., classical *versus* statistical thermodynamics) generally exhibit many-one mappings from the microlevel to the macro level. Far more interesting, however, is the relation between different $K(T)_i$'s which apply at roughly the same spatial order of magnitude. Thus, the decomposition of a piece of granite into subregions of roughly constant chemical composition and crystalline form, $K(T)_1$; density, $K(T)_2$; tensile strength (for standard orientations relative to the crystal axes), $K(T)_3$; electrical conductivity, $K(T)_4$; and thermal conductivity, $K(T)_5$, will produce at least roughly coincident boundaries. The granite is thus descriptively simple relative to these decompositions (see Figure 1).

By contrast, decomposition of a differentiated multi-cellular organism into parts or systems along criteria of being parts of the same anatomical, physiological, biochemical, or evolutionary functional system; into cells having common developmental fates or potentialities, or into phenotypic features determined by common sets of genes will, almost part by part and decomposition by decomposition, result in mappings which are not 1-1 - which are not even isomorphic, much less coincident. This surely involves substantial 'descriptive complexity'.

Fig. 1. Descriptive simplicity and complexity

In biology, at least, the picture is further complicated by another factor -- that different theoretical perspectives are not nearly as well individuated as in the physical sciences. Thus, anatomical, physiological, developmental, and biochemical criteria, not to mention paleontological information and inferences of phylogenetic relations and homologies, all interact with criteria of evolutionary significance in the analysis of organisms into functional systems and subsystems. This borrowing of criteria for individuation of parts from different and diverse theoretical perspectives is one of the factors which make functional organization in general and biology in particular such a conceptual morass at times. This is further discussed in Wimsatt (1971, Chapters 6 and 7).

Descriptive complexity has a point, in large part because of the existence of what I will call 'interactional complexity'. This is a kind of measure of the complexity of the causal interactions of a system, with special attention paid to those interactions which cross boundaries between one theoretical perspective and another.

Many systems can be decomposed into subsystems for which the *intrasystemic* causal interactions are all much stronger than the *extra-systemic* ones (see Figure 2). This is the concept of 'near-complete decomposability' described by Simon and others (see, e.g., Ando *et al.*, 1963; Levins, 1970, and Simon, 1962). Such systems can be characterized in terms of a parameter, \hat{a}_c , which depends upon the location of the system in phase space and is a measure of the relative magnitudes of inter- and intra-systemic interactions for these subsystems.¹² This notion will be called S- e_c -decomposition, and the subsystems produced according to such a decomposition will be denoted by $\{s_{cc}^i\}$. A system is *interactionally simple* (relative to \hat{a}_c) if none of the subsystems in $\{s_{cc}^i\}$ cross boundaries between the different K-decompositions of a system, and *interactionally complex* in proportion to the extent to which they do (see Figures 2b and c).

The importance of interactional complexity is as follows: The parameter \hat{a}_c can also be used as a measure of the accuracy of a prediction of the behavior of the system under a given decomposition if intersystemic interactions are (perhaps counterfactually) assumed to be negligible. The larger \hat{a}_c is for that system under that decomposition, the less accurate the prediction. Alternatively, if a specific value of \hat{a}_c , say \hat{a}_c^* , is picked in order to achieve a certain desired accuracy of prediction of the behavior of a system, and the system turns out to be interactionally complex for that value of \hat{a}_c , then the investigator *must* consider the system from more than one theoretical perspective if he is to be able to make predictions with the desired level of accuracy.

Fig. 2. Near-decomposability and interactional complexity. Note that views in this figure represent decompositions into *sets of state variables* in different perspectives, whereas those in the preceding figure were in terms of *sets of parts*. The two are not to be confused although they are not unrelated. Thus, strong causal correlation among state variables would amount to a generalized version of Campbell's (1958) 'common fate' criterion for the individuation of objects

(or parts of objects), though the other criteria he discusses, such as spatial proximity and similarities in other properties would conflict with this, and the entities we pick out as parts would represent a compromise.

Obviously, the value of \hat{a}_c^* is an important factor here. If a system is interactionally simple for a given value of \hat{a}_c^* it will remain so for all larger values of that parameter, since larger values of \hat{a}_c^* denote lower standards of predictive precision. A system which is interactionally complex for one value of \hat{a}_c^* may be interactionally simple for larger values. Thus the interactional simplicity of a system also increases as the minimum value of \hat{a}_c^* for which it is interactionally simple decreases.

In any case of interactional complexity, the investigator is forced to attempt to relate the different K-decompositions in order to trace and analyze the causal networks in the system. This is a relatively straightforward task if the system is descriptively simple, since the spatial decomposition of the system into parts in one perspective automatically gives the spatial decompositions (but not all the properties!) for the other perspectives. But if the system is descriptively complex and is also interactionally complex for more than a very small number of interactions, the investigator is forced to analyze the relations of parts for virtually all parts in the different decompositions, and probably even to construct connections between the different perspectives at the theoretical level.

Many investigators of biological, social, and other complicated systems have claimed that no one perspective appears to do justice to their objects of study, or, somewhat more obscurely, that their systems are unanalyzeable into component parts -- or at least that there is no clear way to perform this analysis. Kauffman (1971) and Levins (1970) both claim that in complex systems there are a number of different possible decompositions and often no way of choosing between them. Levins' remarks suggest something even stronger:

... [for] a system in which the component subsystems have *evolved* together [the subsystems] are not even obviously separable.... It may be conceptually difficult to decide what are the really relevant component subsystems.... This decomposition of a *complex* system into subsystems can be done in many ways... it is no longer obvious what the proper subsystems are. (Levins, 1970, p. 76)

It seems reasonable to construe these claims in part as claims that the systems in question are interactionally and descriptively complex. Levins' claim about evolved systems raises further questions about the origins of complexity.

III. EVOLUTION, COMPLEXITY, AND ORGANIZATION

Why should some systems be interactionally complex? If this question is examined in an *a priori* manner, it is perhaps more amazing that some systems should be interactionally simple. The systems, as analyzed to apply S-decompositions, are composed of state variables and causal relations between them. State variables are properties, and different properties, picked at random, would be expected to be found as properties of parts or systems in different theoretical perspectives. Unless causal relations or state variables were organized in a rather specific way relative to the different theoretical perspectives, one would expect systems to be interactionally complex.

But isn't this what successful theories do for us -- that is, isn't it the mark of a successful theory of a range of phenomena that it unites and embraces the causally relevant parameters and state variables within a single theoretical perspective? This question suggests that if our theories are successful, then they should produce descriptions of systems according to which the systems are interactionally simple. I think that this would be to put the conceptual cart before the phenomenal horse. As the criterion (one of many) for the adequacy of a theory of a system, this statement seems correct but it is hardly sufficient. Also, one should not automatically assume that our *existing* theories are adequate theories of complex systems. The belief that they are is based largely on a still unfulfilled reductionist promise.¹³

It is true that our existing theories work well on simple systems -- simple in part (but only in *part*) because these theories are constructed so as to render them interactionally simple. But one cannot assume that it is always possible to find a theory which will render a given system interactionally simple. As W. Ross Ashby said some years ago:

Science stands today on something of a divide. For two centuries it has been exploring systems that are either intrinsically simple or that are capable of being analyzed into simple components. The fact that such a dogma as 'vary the factors one at a time' could be accepted for a century shows that scientists were largely concerned in investigating such systems as *allowed* this method; for this method is often fundamentally impossible in complex systems. (Ashby, 1956, p. 5)

Nonetheless, the *a priori* argument for the interactional complexity of systems given at the beginning of this section is intrinsically defective, for it ignores facts which every scientist takes for granted -- namely, that systems *are* constrained and that state variables are not causally related at random. Thus, some of the arguments of 'The Architecture of Complexity' (Simon, 1962) appear to suggest that there *are* physical constraints on evolving systems which would render them interactionally simple and that they are descriptively complex only after the manner of a multi-level theory, with many-one mappings of parts from lower to higher levels. It is an open question whether this is indeed the conclusion Simon intends, but in any case, it seems to me to be mistaken.

Simon makes an elegant case for the conclusion that all evolved systems containing many causally interrelated parts will be hierarchically organized. This is *via* an argument that for two systems of roughly equal complexity, each to be built out of simple components (and each subject to perturbations tending to cause decomposition), one which arises out of the successive aggregation of individually stable subassemblies into larger subassemblies will have a much higher probability (or lower expected time) of formation than one which does not. (See his parable of the watchmakers, 'Tempus' and 'Hora' in 1962, pp. 90-95.) Thus, one would expect that at least the vast majority of complex evolved systems would be organized.

But Simon's use of the concept of near-decomposability in the same article sometimes appears to suggest that he believes such hierarchical systems to be nearly-decomposable in a nestable manner -- with smaller subassemblies (at lower levels) having successively stronger interactions and no S-decompositions crossing boundaries between levels. Indeed, if the subassemblies which go to make up a hierarchically organized system are stable, isn't it the case that these subassemblies at all levels must be the subsystems which emerge for various characteristic values of α_c in the different level S-decompositions of a system?¹⁴

To accept this opinion is to fail to distinguish the decomposability or stability of the subassemblies before they aggregate from their decomposability or stability (in isolation) after they have aggregated -- especially a long time afterwards, when they have had time to undergo a process of mutually coadaptive changes under the optimizing forces of natural selection. The optima and conditions of stability for a system of aggregated parts are in general different in a non-aggregative way from the optima and conditions of stability for its parts taken in isolation.¹⁵

Naive design procedures in engineering, in which the organization of the designed system was made to correspond to the conceptual breakdown of the design problem into different functional requirements, with a 1-1 correspondence between physical parts and functions, have given way to more sophisticated circuit minimization and optimal design techniques. These methods have led to increases in efficiency and reliability by letting several less complicated parts jointly perform a function that had required a single more complicated part, and simultaneously letting these simpler parts perform more than one function (in what might before have been distinct functional subsystems) where possible. This has the effect of making different functional subsystems more interdependent than they had been before, and of encouraging still further specialization of function, and interdependence of parts. It is reasonable to believe that the optimizing effects of selection do just this for evolving systems, and if so that hierarchically aggregating systems will tend to lose their neat S-decomposability by levels, and become interactionally complex.¹⁶

This argument is buttressed empirically by considering what happens when natural organized systems are artificially decomposed into subassemblies which are the closest modern equivalents of the subassemblies from which the systems presumably came. Few modern men (or better, couples, for bisexual organisms) could survive for long outside of our specialized society. The same goes for mammalian cells -- at least under naturally occurring conditions, even though multi-cellular organisms are presumably descended from uni-cellular types. Even many bacteria cannot survive and reproduce outside of a reasonably sized culture of similar bacteria. The current belief of some biologists is that mitochondria and chloroplasts originated as separate organisms, and acquired their present role in animal and plant cells via parasitic or symbiotic association. According to this view these once independent

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organisms (or 'subassemblies') are now so totally integrated with their host that only their independent genetic systems are a clue to their origin (Lynn Margulis, 1971).

With increasing differentiation of function in systems, different subsystems become dependent, not only upon the presence of other subsystems, but also upon their arrangement. Experiments with the transplantation of imaginal discs in the larvae of holometabolous insects demonstrates that the developmental fate of these discs (which develop into organs in the pupal stage) depends not only upon the disc and its 'age' but in some cases also upon its location. Slime moulds are remarkable among organisms for their ability to function undisturbed if they are pushed through a sieve in their undifferentiated form. This is quite unusual for multi-cellular animals. An adult man or mouse would do considerably less well under similar conditions.

The end result, I think, is that one cannot expect hierarchical organization resulting from selection processes to be S-decomposable into different levels -- or at least, not into the different levels of organization relevant at the time of aggregation. Nor could one expect that such organization would be interactionally simple when decomposed according to any other theoretical perspectives bearing no intrinsic relation to the selection forces acting upon it.

Nor, unfortunately, is there even any guarantee that functional organization in terms of the operation of selection mechanisms (Wimsatt, 1971, 1972) is the road to a descriptively and interactionally simple analysis of such systems. The use of functional criteria might lead to more simplicity perhaps, (I would argue strongly that they would!) but functional systems are still subject to physical, chemical, and biological constraints at a number of levels, and never completely lose the marks of the systems which they have evolved from -- even down to the level of the basic chemical elements of which they are composed. These simultaneous constraints seem almost certain to result in interactional complexity.

IV. COMPLEXITY AND THE LOCALIZATION OF FUNCTION

In the last section, I attempted to suggest how considerations of efficiency in evolution would lead to the co-adaptation and increased interdependence of parts of a functional system, and that this would lead to increases in the descriptive and interactional complexity of that system. One aspect of this increase in complexity is a trend away from I to I mappings between functions and recognizable physical objects. It seems plausible to suggest that one of the main temptations for vitalistic and (more recently) anti-reductionistic thinking in biology and psychology is due to this well-documented failure of functional systems to correspond to well-delineated and spatially compact physical systems. (Richard Gregory's excellent and suggestive remarks (1959, 1961, 1962) on the problem of localization of function in the brain (and his humorous illustrations with engineering examples) offer too many riches to mine them superficially here, but they are heartily recommended to the reader!)

It is only too tempting to infer from the fact that functional organization does not correspond neatly to the most readily observable physical organization - the organization of physical objects - to the howling *nonsequiter* that functional organization is not physical.¹⁷ A tantalizing explanation for our tendencies in this direction is hinted at in Donald Campbell's (1958) suggestion that our willingness to 'entify' things as real is directly proportional to the number of coincident boundaries we find.¹⁸

Thus, organisms count as systems because of the coincidence of a number of boundaries at, roughly, the surface of the skin or its functional equivalent. In addition to the relatively discontinuous change of a number of physical and physiological variables at what is taken to be the organism/environment interface, the systems thus picked out are usually relatively independent agents biologically, since what we call organisms usually live, die, metabolize, mate, and move relatively independently of one another. Indeed, we tend to marvel at the problem cases -- eukaryotic cells, slime moulds and social insects - where one or more of these boundaries do not coincide, and have problems deciding whether a given unit is an organism, assemblage of organisms, or part of an organism.

But what holds true of an organism (that many boundaries coincide at its skin) need not hold true of its parts. Inside the complex system, there is a hegemony of different constraints and perspectives and boundaries. If what Campbell says is correct, this hegemony leads us to be slow or dubious about objectifying the parts of such a system. What is unobjectifiable is to that extent unphysical, and so functional organization becomes a thicket for vital forces and mental entities. It is no accident that those systems for which vitalisms and mentalisms have received spirited defenses are those systems which are also paradigmatically complex.

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The difficulties with the spatial localization of function in complexly organized systems suggest a more positive approach to at least one aspect of the psycho-physical identity thesis.¹⁹ In 1961, Jerome Shaffer took account of the frequently discussed non-spatiality of mental events and proposed that the spatial location of corresponding brain events could, as a *convention*, be taken as the location of the corresponding mental events. Norman Malcolm (1964, p. 119, note 6) argued that as this would be just a convention, talk of the location of mental events would just be taken as a shorthand way of locating the corresponding physical events.²⁰ Malcolm and Shaffer's later discussions (Malcolm, 1971; Shaffer, 1965) raise other issues that lead off in other directions, but neither of them seem to take seriously the implications of the interactional and descriptive complexity of functional organization.

It is not merely that functionally characterized events and systems are spatially distributed or hard to locate exactly. That much can be said for bulk terms like water, or even more, like fog and smoke. But ordinary bulk matter, like ordinary fields, can be conceived of as homogeneous. The problem is that a number of different functionally characterized systems, each with substantial and different powers to affect (or effect) behavior appear to be as interdigitated and intermingled as the infinite regress of qualities-within-qualities of Anaxagoras' seeds. Furthermore, the high degree of redundancy and plasticity of the cortex - pointed to by the work of Lashley and his doctrine of 'equipotentiality' - make it seem as if functional systems are not essentially located *anywhere*. The apparent contradiction of having a number of (functionally) distinct organized systems, each of which appears to occupy all or most of the same space, and at the same time, none of it, leads to the tendency to deny spatiality at all,²¹ or in less extreme forms, to invent special kinds of quasi-physical predicates. The non-physicalistic and anti-reductionistic strains in the writings of Jerry Fodor (1965, 1968), Hilary Putnam (1967), and Margaret Boden (1970, 1972) who speak with a strange ambivalence about their 'functional roles', 'programs', and 'internal models' reflect this no less than did 19th-century vitalism.

This tendency is I think at least partially explained by the 'pathological' behavior of boundaries in complex systems and what this does to our normally workable criteria for spatial objectification. This way of putting it may be misleading for it could be argued that we conceived of the mental as non-spatial long before we had any idea (gleaned from neurophysiology) about the problems with localization of function in the cerebral cortex. But there are alternatives to suggesting that active awareness of these problems led us to conceive of the mental as non-spatial. One might suppose, for example, that spatial objectification is an *active* hypothesis that we apply to those groups of phenomena which tie up into sufficiently neat packages in the right ways. On this account, the mental realm was not denied spatiality. It just has not yet been added to the list.

The denial of spatiality as a category to mental entities has the ring of a philosopher's invention. Now that we see one reason why we might not have been able to attribute an exact location to mental events, we can wonder perhaps whether the common man need ever have been more than an agnostic about their spatiality. If he has asserted more, he has probably only succumbed to the seductions of philosophy -- deifying as conceptually true a hypothesis which future writers may well decide was empirically false.²²

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NOTES

* Parts of this paper are based on my doctoral dissertation (Wimsatt, 1971) and on work done during the tenure of a Woodrow Wilson Dissertation Fellowship at the University of Pittsburgh and a post-doctoral fellowship with The Committee on Evolutionary Biology at the University of Chicago, supported by the Hinds Fund for Studies in Evolution. I gratefully acknowledge their support.

¹ This also seems to be true in physics in active research areas such as (but not limited to) meteorology and magnetohydrodynamics, though those arguments for complexity based upon evolutionary phenomena have no obvious application in these areas.

² The problem of how to delineate the domain of a scientific theory has received relatively little attention until recently. See Shapere (1971) for a close examination of these issues.

³ It is an intriguing fact that discussions of theoretical pluralism have arisen in the context of scientific change, but implications of this pluralism (including the hotly debated problems of translation and meaning variance) have generally not been investigated for *simultaneously held* partially overlapping theories and models. Richard Levins' views on the nature and use of theories and models in biology (Levin, 1966, and 1968, Chapter 1) are a notable exception, and Stuart Kauffman's (1971) views on the plurality of ways of seeing or describing systems are also suggestive. It is tempting to dismiss this as a kind of pre-paradigm or multi-paradigm science, but this is to ignore the fact that many of the scientists must then be viewed as simultaneously (or alternatively) using several of the paradigms at any given time.

⁴ See Bergmann (1957), pp. 93-96 for discussion of the assumptions of closure and completeness of a system.

⁵ On this, see also Bishop Berkeley (1709), paragraphs 41- 51, especially 48-50. There should be obvious application of these remarks to the problem of interrelating information from different sensory modalities in the construction of 'external objects'. Why, and when, do we objectify?

⁶ In the light of this fact, it is quite ironic that Goodman's calculus of individuals or something quite like it (*Ibid*, pp. 46-61) appears to be admirably suited to the analysis of what I have described below as 'descriptive complexity'. I thank Leonard Linsky for this reference.

⁷ Some of the most interesting contributions in this area are Campbell's (1958, 1959, 1972). Campbell's most relevant point in the present context is an attention to the boundaries of systems. He argues, in part, that we consider objects the more real and substantial the more there is a coincidence of boundaries on different criteria of individuation. He applies this, among other things to the individuation of social units (1958) and the order in which we learn different kinds of concepts (1972).

For more analyses of the importance of the boundaries of systems on different criteria, see Platt (1967) and Simon (1962, 1969). On the localization of functions and questions concerning whether functions are objects, see Gregory (1959, 1961, 1962) and Wimsatt (1971, 1972) and this article.

⁸ These two factors represent in part an attempt to give a conceptual basis and motivation for some of the ideas expressed by Richard Levins (1970, 1973), and personal conversations.

⁹ Kauffman is primarily concerned to argue that these perspectives may not be reducible, one to another, and so he emphasizes the possibility of non-isomorphism. Spatial coincidence of the parts' boundaries under two decompositions implies spatial isomorphism of the decompositions but not conversely, and it is coincidence, not isomorphism, which is important here.

¹⁰ The importance of being able to unambiguously identify and individuate the different theoretical perspectives appropriate to a system cannot be underestimated. *Any* system is trivially descriptively simple relative to just a single T_i and $K(T)_i$. This case thus must be ruled out as a case of descriptive simplicity, with the requirement that two or more theoretical perspectives and correlative decompositions be considered. But if criteria for individuating these are in doubt, then it might be open for someone to claim that a system purportedly classified as descriptively simple relative to a set of perspectives (like the granite case discussed below) is not, because the perspectives are not in fact distinct, but are parts of the *same* perspective.

I believe that it is possible to give criteria for identifying and individuating theoretical perspectives, and in a way that a given T_i , for a given system and set of conditions of and on that system, has a unique $K(T)_i$. Further work needs to be done on this matter, however.

¹¹ For two such parts considered as an ordered pair, the mapping relations 1-1, 1-many, 1-part, 1-many part, many-1, part-1, and many part-1 are exhaustive. There are also many other considerations which are important, such as whether the mappings are continuous. Any discontinuities in a mapping will greatly increase the complexity of description of a system.

¹² A number of measures are possible, and different ones would be preferable under different circumstances. For illustrative purposes, $\hat{\alpha}_c$ can be thought of as the average strength of intersystemic interactions divided by the average strength of intrasystemic interactions. This ranges between 0 and 1, approaching 0 as intersystemic interactions become negligible.

¹³ I wish to emphasize that I am *not* an anti-reductionist. There is not even any reason to believe that these theories are *incorrect* for what we intuitively regard as complex systems. On the other hand, there is no reason to believe that they will describe other systems as simply as the systems they were generated to explain, and adequate theories of complex systems may require new categories if they are to be described and analyzed more simply. Statistical mechanics owes its acceptability to just such considerations.

¹⁴ The view criticized here is Levins' reading of Simon, and this line of attack is basically an elaboration and adaptation of a point Levins has made many times in conversation. Levins' interpretation seems not to be too

idiosyncratic: Howard Pattee (1970) also reports Simon's analysis as showing that hierarchically organized structures are nearly-decomposable (see note 10, p. 136). Nonetheless, I am not convinced that this is a fair reading of Simon. The evidence is at least equivocal: In addition to the 'stable subassemblies' argument, this point of view is also suggested by Simon's discussion of near-decomposability in physical systems (p 103-104) and the first part (pp.114-115) of his discussion of genetic control of development in terms of hierarchically organized computer programs. (The latter part, pp. 116-117, appears to suggest that Simon is aware of the issues raised here.)

¹⁵ Some of the most interesting cases of this are to be found in so-called 'cooperative phenomena' in polymers. Thus, it has been suggested that hemoglobin (a tetramer) might have evolved from its monomeric precursor because interactions among the 4 subunits facilitate binding of the hemes (which carry oxygen). See Jukes (1966; chapter 5) for further details.

¹⁶ If this argument holds, it produces firmer grounds than the 'randomness' assumption for a belief that hierarchically aggregating systems will become interactionally complex through co-adaptation. In fact the argument here seems to be just the other side of the argument made by von Neumann and Morgenstern (e.g., (1946), pp. 11-12) that a game-theoretic interaction is not a simple maximum or minimum problem. Their point there is that with two or more individuals acting, there are two (or more, as appropriate) functions each of which some individual is trying to maximize, subject to the constraints of the actions of the others, and there is no overall function which tends to an extremal value.

In our case, we begin with independent systems with their own (relatively) independent optima. When they aggregate and begin to coevolve, their individual survival becomes dependent upon the 'collective welfare' with something more akin to a single optimum. I will assume, as von Neumann and Morgenstern do, that it is overwhelmingly improbable that the conditions for individual and collective optima coincide. The failure of this coincidence results in selection for interactional complexity.

Indeed, the situation is more complicated than indicated here, in which it is suggested that either individual optima or the group optimum dominates absolutely. In many cases (see Lewontin, 1970) selection can operate independently upon units at two or more levels of the *same* hierarchy, in concert or in opposition. In these cases, we have a hegemony of forces, and a compromise among conflicting optima is inevitable.

¹⁷ I have attempted detailed analysis of functional organization in Chapters 6 and 7 of Wimsatt (1971). Some of the conceptual complexities inherent in this problem are discussed in Wimsatt (1972).

¹⁸ Campbell has argued in conversation that even for those systems which I have chosen to call complex in one of the two above ways, there is still probably a coincidence of boundaries for the vast majority of properties. This seems reasonable: if there were an arbitrary thicket of overlapping boundaries we probably would not be able to pick out *any* systems. Furthermore, the use of modular construction techniques (both as new as third generation computers and as old as multicellularity) would appear to imply the coincidence of at least *many* boundaries at the boundaries of the modules.

Simon has recently made much the same point, arguing that these remarks on complexity are a second-order approximation to modify the first-order approximation of describing systems as nearly decomposable (personal communication). I strongly agree with both of these remarks, though not with the implication one might draw that '2nd order' effects are, in the relevant sense, always ignorable relative to '1st order' effects. Whether they are or not depends upon which phenomena you are interested in. Weak hydrogen bonding is, chemically speaking, energetically negligible. It happens however to be crucial to the proper functional behavior of biologically important macro-molecules.

Another interesting and powerful line is suggested by Levins (1973) when he argues that an initially arbitrarily complex system will tend towards greater simplification, and perhaps this is implicit in Campbell and Simon's arguments. Thus bounded (by coevolution) away from aggregative simplicity and (by the need to have parts of the system responding semi-autonomously to selection pressures) away from total interactional complexity, it would appear that there must be a relatively stable intermediate level of complexity. Whether this would involve an intuitively and immediately recognizable degree of modularity is an open and important question. Might it be, for example, that there would be different modes of modularity or near decomposability for systems that arise by aggregation of stable subassemblies (*à la* Simon) and for those which rise by specialization and differentiation of sub-parts of a single system (*à la* Levins)? This is not an 'academic' question. A recent challenge to Margulis's (1971) aggregative account of the origin of eukaryotic cells is offered by Raff and Mahler (1972), who suggest that eukaryotic cells evolved by specialization. It would be extremely useful to have criteria for adjudicating this and other similar disputes.

¹⁹ Jaegwon Kum (Kim 1971, pp. 329-334) discusses views which both suggest that problems with spatial localization of function has been a problem in this context and seems himself (p. 334) to suppose that events of this type must have a precise location.

²⁰ Obviously, talk about the location of mental events in this sense is not intended to apply to those cases where we most frequently *do* talk about location -- cases such as feeling an 'itch in the leg' or a 'pain in the tooth'. These kinds of cases are probably best explicated as Margaret Boden does (1970, pp. 207-209), as events which are occurrences in an 'internal model' representing states of the organism. As such the events can refer to occurrences at the locations in question without themselves occurring at those locations. I discuss this problem and several others not mentioned here in Wimsatt (1973). See also Globus (1972).

²¹ Keith Gunderson (1970, p. 303) has suggested that one of the problems with conceptualizing the self arises from the feeling that the concept of the self requires that one entity be in *two* places at the same time (In one place as *observer* and in another place as *observed*). It seems reasonable to suggest that the belief that such spatial paradoxes bedevil the mental realm is or was influential in denying its spatiality.

²² I think that this will be the judgment of the future even though I admit that these points about functional organization and localization go but one small and not even very important part of the way there. In Wimsatt (1973) I argue that there are three classes of paradoxical phenomena to be handled in attributing spatiality to the mental realm: those respectively associated primarily with 1st person knowledge, with 3rd person knowledge, and with interactions between 1st and 3rd person knowledge. The terms, '1st person knowledge' and '3rd person knowledge' must first be reanalyzed in terms of asymmetries among and limitations on the locational information given by the various sensory modalities. The functional localization problems discussed here are then seen to be pure 3rd person problems, whereas most of the interesting problems discussed by philosophers are seen to be one of the other two types. Interestingly, there is a sense in which 1st person knowledge of the mental realm *is* non-spatial, though this fact turns out to be of no comfort for those who would wish to use it against materialism. This argument builds substantially upon key points raised by Gunderson (1970) and Globus (1972).

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