ment would repay tenfold the expensive and tedious effort required in its construction.

REFERENCES

1. Cronbach, L. J., and P. E. Meehl. "Construct Validity in Psychological Tests," *Psychological Bulletin*, 52:281–302 (1955).
2. Daily, C. A. "The Practical Utility of the Clinical Report," *Journal of Consulting Psychology*, 17:297–302 (1953).
3. Davenport, Beverly F. "The Semantic Validity of TAT Interpretations," *Journal of Consulting Psychology*, 16:171–75 (1952).
4. Halbower, C. C. "A Comparison of Actuarial Versus Clinical Prediction to Classes Discriminated by MMPI." Unpublished Ph.D. thesis, University of Minnesota, 1955.
5. Hathaway, S. R. "A Coding System for MMPI Profiles," *Journal of Consulting Psychology*, 11:334–37 (1947).
6. Holsopple, J. Q., and J. G. Phelan. "The Skills of Clinicians in Analysis of Projective Tests," *Journal of Clinical Psychology*, 10:307–20 (1954).
7. Kostlan, A. "A Method for the Empirical Study of Psychodiagnosis," *Journal of Consulting Psychology*, 18:83–88 (1954).
8. Little, K. B., and E. S. Shneidman. "The Validity of MMPI Interpretations," *Journal of Consulting Psychology*, 18:425–28 (1954).
9. Little, K. B., and E. S. Shneidman. "The Validity of Thematic Projective Technique Interpretations," *Journal of Personality*, 23:285–94 (1955).
10. Meehl, P. E. *Clinical Versus Statistical Prediction*. Minneapolis: Univ. of Minn. Pr., 1954.
11. Meehl, P. E. "'Comment' on McArthur, C. Analyzing the Clinical Process," *Journal of Counseling Psychology*, 1:203–8 (1954).
12. Meehl, P. E., and A. Rosen. "Antecedent Probability and the Efficiency of Psychometric Signs, Patterns, or Cutting Scores," *Psychological Bulletin*, 52:194–216 (1955).
13. Paterson, D. G. "Character Reading at Sight of Mr. X According to the System of Mr. P. T. Barnum." Unpublished paper; mimeographed copies obtainable from the author, Department of Psychology, University of Minnesota, Minneapolis.
14. Rosen, A. "Detection of Suicidal Patients: An Example of Some Limitations in the Prediction of Infrequent Events," *Journal of Consulting Psychology*, 18:397–403 (1954).
15. Skinner, B. F. *The Behavior of Organisms*. New York: Appleton-Century-Crofts, 1938.

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On the Logic of General Behavior Systems Theory

SCIENCE, or at least programmatic science, can degenerate into naive and low-grade speculative philosophy by overextending itself in various ways. One typical motive for such overextension is the aim of synthesizing several existing disciplines; another, closely related, is that of extending the concepts and techniques of physics to areas where these concepts have no present application. In this paper, I shall explore an unusually clear instance of such metaphysical overextension: the "scientific" thesis of General Behavior Systems Theory.* Professor Miller's paper, which functions as a general introduction to the symposium, sets forth starkly the major tenets of general systems theory, in its search for an "embracing theory" of behavior. On two subsequent occasions Miller has returned to this theme, so it is especially appropriate to consider his views.†

What exactly are the central theses of general systems theory? According to Miller, this theory "finds formal identities between various physical systems, the cell, the organ, the individual, the small group or species,

* The first published version of this theory is a symposium by members of the Committee on Behavioral Sciences, University of Chicago (1). The publication is undated, but it consists of papers read at a meeting of the American Psychological Association, in September, 1953. Of the five contributions to this symposium, only one, that of Professor James G. Miller, is extensively considered here. (Parenthetical page references are to this publication.)

† The first of these occasions was at another symposium: the Joint Session of the Southern Society for Philosophy and Psychology, at Atlanta, April 17, 1954. My paper is a revised version of some comments contributed at the same Joint Session. The second occasion is Miller's "Toward a General Theory for the Behavioral Sciences," (2). This paper appeared too late to be considered here in detail. In it Miller tries, I think unsuccessfully, to meet criticisms of the kind here offered. Those interested in the question of the impact of methodological criticism on the procedures of scientific theorists, should consider this most recent paper of Miller's in the light of the criticisms offered here.

and the society" (p. 5). These identities "offer promise not only of coordinating the theories of scholars but also of confirming their conclusions empirically, employing the dimensions and units of the natural sciences" (p. 6). Behavior is conceived as energy exchange within a system or between systems. "Every system has its environment, and all living systems are open systems with inputs and outputs, and tend to maintain steady states" (Ibid.). Living systems "are also ordinarily in equilibrium with the environment" (Ibid.).* Inputs and outputs may be either "coded" or "uncoded," and inputs can force a system beyond its range of stability, thus creating stresses and strains which "may or may not be capable of being reduced, depending upon the equilibratory resources of the system" (Ibid.). In individual psychology "reduction of strains is called drive satisfaction" (Ibid.). "The total of the strains within the individual resulting from his genetic input and variations in the input from his environment is often referred to as his values" (p. 7).

Miller recognizes that this "empty shell of general systems theory resounds hollowly without specific illustrations of how it can be applied" (p. 7). And he goes on to present "a few anecdotal examples of its usefulness" (Ibid.). I shall consider several of these examples, raising questions about their usefulness and especially about their significance. But first we must look briefly at the notions of "formal identity," and of "system."

I

The most serious conceptual problems in general systems theory concern the employment of analogies. Miller at first employed the expression "formal identity" to refer to what are generally known as analogies, reserving the word analogy as a term of abuse for "metaphysical or artistic statements that two or more phenomena elicit similar feelings in the observer" (p. 3). Those who recognize the kinship between Miller's organic theories of groups and societies, and the metaphysical theory of the state as advanced by Hegel and Bosanquet, will note the irony in this. In any event, Miller has now lost his faith in the efficacy of this verbal maneuver, and has adopted the sound practice of calling analogies what they are.

* The usefulness of the concept of dynamic equilibrium in the social sciences is investigated in the same symposium by David Eastman, another member of the Chicago Committee. He argues very reasonably that there is little present evidence for the utility of this concept in social science, and that there are many obstacles in the way of its employment there.

The questions which need to be raised about the analogies in which general systems theory is interested are mostly of the "So what?" variety. They take the form of "granting that you have shown that there is *some* analogy, what follows?" A closely related question concerns disanalogy— a subject which is almost completely neglected by general systems theory.* Yet surely such neglect is dangerous, for if there is anything that is clear about analogies, it is that things or systems are analogous in some respect or respects, *not* in all.

The sort of analogies which particularly interest general systems theory, in its search for an "embracing theory" of behavior, are what might be called structural analogies. This, of course, is connected with their concentration on *systems*. A system, whatever else it may be, is certainly the sort of thing that has a structure, and the kinds of analogies general systems theory is interested in are those which relate two systems because of a similarity of structure. The notion of formal identity is, I suppose, suggested by the fact that when two systems exhibit such a similarity of structure, then a sufficiently general description can be seen to apply to both. As an example, compare a chessboard and a well-planned, mixed, dinner party. The dinner party will be such that between any two men there will be a woman, and vice versa. The chessboard will be such that between any two black squares there will be a white square, and vice versa. Now it is obvious that one could construct a sufficiently general statement in terms of "two kinds of things" and the spatial relations between them, to cover both cases. The fact that the sense of "between" is different in the two cases—one roughly a linear and the other a two-dimensional sense—is, of course, part of the disanalogy in this case. If one is tempted to say "All right, so they're structurally analogous, so what?" my answer is "So, nothing!" But I welcome the question, for while I was concerned simply to illustrate the employment of the concept of structural analogy, this kind of question will certainly be in point in dealing with many of Miller's analogies.

I turn now to an examination of the central concept of general systems theory, the concept of "system" itself. There are strong philosophical reasons for deploring the kind of employment which is given here to the concept of system. About systems Professor Miller says that "A

* But cf. Miller's most recent paper (2).

system is a bounded region of space-time, in which component parts are associated in functional relationships" (p. 6). This already is pretty sweeping, for goodness only knows where some sort of functional relationship between two items may not turn up. And the notion of a *bounded* region is itself a good deal less definite and specific than it sounds, as is clear when it is realized that such logically heterogeneous items as an organism, a cell, a jury, a society, and a biological species (salmon) are all to be regarded as systems, and hence presumably as thus *bounded*. The idea of system can be seen as further generalized and extended through the following considerations. First, there is the rather sweeping claim that "Every system has subsystems" (p. 6). Taking this together with "Every system has its environment" (Ibid.), we are indeed confronted with limitless vistas of systems. One is unable to think of anything, or of any combination of things, which could not be regarded as a system. And, of course, a concept that applies to everything is logically empty.

What characteristics are there which any object or group of objects could have, such that they would fail to form some kind of system? In my view, general systems theory not only does not, but further could not, answer this question. And it is partly for this reason that I believe that general systems theory is not in fact science at all, but rather naive and speculative philosophy. Miller approves of so formulating scientific statements that they can be empirically confirmed (pp. 5–6). This is a laudable, and a scientific, ideal. But has he tried to apply this criterion to his own statements about systems? What would it be like to disprove the statement that every system has subsystems? If general systems theory can answer this question, the answer should certainly be provided. For such an answer would have to include a criterion for recognizing something which is not a system; and possession of such a criterion would help immeasurably in clarifying the central concept of system. An important part of the significance of any concept is given by contrast, by knowing the kinds of things to which it does *not* apply. And the trouble with the concept of system here, as I see it, is that this contrast is absent. Here, rather, the situation seems to be that statements such as "I am a system," "The membership committee is a system," "The economy of the United States is a system," "The species, salmon, is a system"—that these statements, in the language of general systems theory, couldn't even be false.

II

I turn now to construct a possible defense against my charge of the emptiness of the central concept of "system." This defense, though not explicitly offered by general systems theorists, seems to me to be one that they very likely would offer, *if* they paid more attention to logical and conceptual problems.

Suppose, then, that the general systems theorist answers my charge of emptiness in some such vein as this:

> We grant your contention that the notion of "system" is an extremely vague and virtually empty one; to say that something is a system is really to say no more than that it is the locus of a set of structural relations. And, of course, it is a consequence of this that some of the statements we make about systems are, as you suggest, non-empirical, are such that they couldn't be false. But we maintain that, while it is true that for general systems theory everything is a system, this statement is to be understood as like a statement which a physicist might make to the effect that all physical objects have mass. Such a statement too is non-empirical, and could not be false. It rather records the physicist's decision to treat his subject matter in a unified way, and this determination is to be justified, though not of course *verified* in the usual sense of that term, by noting the tremendous fruitfulness which this approach has in the actual practice of physics. That is, this determination or decision can be justified pragmatically. And so with "system." Our determination to treat everything as a system justifies itself by its great fruitfulness in the practice of the behavioral sciences.

I have several comments on this hypothetical defense. First, notice that it rests, appropriately enough, on an alleged analogy between the behavioral sciences and physics. (This is doubly appropriate in that (a) it is an analogy, and (b) the analogue here, physics, is quite apparently the model of science at its best, to which general systems theory is attempting to assimilate the behavior sciences.)

Again, the justification here is explicitly claimed to be "pragmatic" in both cases. This could raise general problems about the worth, adequacy or acceptability of pragmatic justifications. But I propose to dodge such questions, offering only two general remarks under this head. (1) Something very much like this justification strikes me as the only one either possible or necessary for the universal application within physics of the concept of mass. And (2) that where the notion of "practice" itself is so watered down as to cover all the heterogeneous activities of the physicist—from theorizing and calculation through experiment and measurement—the reference to practice and the pragmatic is just as well omitted. The stress on practice suggests, what is false, that one is here distinguishing between, e.g., practice and theory, or that one is insisting on utilitarian consequences.

But in order to challenge this justification as applied to general systems theory, one need not offer a general criticism of so-called pragmatic justification. One can, and I propose to, challenge this justification by rebutting the contention that it has been shown to be fruitful to apply the notion of system thus universally. The particular analogies between systems which are offered need to be critically examined. Other analogies of similar merit and closeness should be suggested for purposes of comparison. And the general fruitfulness of analogies should be briefly discussed.

As to the last, the general usefulness of analogies, especially as suggesting plausible hypotheses for subsequent testing, is explained in most beginning logic books. But surely, one feels, general systems theory is doing, or at least is trying to do, something more than this. (And in any event the logic books, or at least the good ones, warn us also to consider and notice the negative analogies, the *disanalogies*, in each case.) Now I am convinced that general systems theory is trying to do something more, but just exactly *what* more it is extraordinarily difficult to discover. And the basic reason for the difficulty is that, after drawing our attention to some positive analogy, these theorists in general simply fail to say anything about what the analogy is supposed to prove or suggest, while nevertheless managing to convey the impression that something pretty momentous has been proved or suggested. Let me offer an example taken from Miller's previously mentioned "anecdotal examples of its usefulness."

From the field of botany we find a remarkable study in systems theory in the slime mold, mentioned by Ralph Gerard (another member of the Chicago committee). Under conditions of adequate water and food supply a colony of this plant is made up of quite independent individuals, each with its own inputs, outputs, equilibratory mechanisms and ability to reproduce. Under more stressful conditions, when the environment is less favorable, however, these individuals flow together to form what is essentially a single multicellular organism, with specialization of function or distribution of labor. Some become central cells, others peripheral cells which always flow toward the center, wherever it may be; some cells reproduce, and others cannot—a remarkable model of how humans band together under stress from a common enemy, as did the Londoners for example, during the fire raids of World War II (pp. 8–9).

At this point I must stress that my above quotation is everything, literally everything, that Miller has to say about this alleged example of the usefulness of general systems theory; nor does Professor Gerard say anything more about its usefulness, though he does go into more detail as to the actual behavior of the slime mold. Well, so what? What are we to conclude from all this? That Londoners are a form of slime mold? That myxamoebae are a sort of city dweller? Or, perhaps, that during the battle of London some citizens, due to their new and more specialized activities, became sterile, while others devoted themselves exclusively to reproductive activities? One finds it difficult to believe that these are the conclusions he is expected to draw, but, if not these, what others? And, if no conclusions, why all the fuss, why bother with the analogy at all?

Or look at the matter in this way. Consider an army, in the days when soldiers used to live off the land. Under stressful conditions, say in winter time when food tended to be inadequate, their behavior was in obvious ways directly the reverse of that of the slime mold. Instead of becoming more centralized, and with their members more specialized, they rather broke up into small units, operating in considerable independence, and ranged far and wide in search of food. Now what does this prove? Does it follow that there is any *important respect* in which Londoners are like slime mold, whereas such an army is unlike it? I don't think so. If one wanted to say anything on the basis of these bits of information, surely it would be rather that the Londoners, the myxamoebae, and the soldiers in our foraging army all displayed reasonably successful adaptive behavior when confronted with changes in their environment.

To guard against the impression that I have selected an isolated and bizarre instance of an analogy from general systems theory, let me cite some others which presumably support the usefulness of these theorists' approach. In the same section in which "examples of its usefulness" are presented, we encounter the following:

". . . generalization can be made from one level of analysis to another. Quantitative similarities may be discovered between the characteristics of an input of impulses into the axon of a neuron; an input of words to a jury; and an input of raw materials into the economy of an isolated community" (p. 7).

Indeed, such similarities almost certainly can be discovered, *if* we can make sense of the notion of "input" in all these cases, and if in addition, as Miller suggests, we select our units and dimensions with such an end in view. But so what? What conceivable difference could such a similarity make?

Again, we are invited to compare (1) the reaction of various cells to the invasion of the lung by tubercle bacilli, in which "certain cells are destroyed, but others come in from the surrounding areas and eventually wall off or destroy the bacilli" (p. 9); with (2) ". . . various isolating or 'walling off' mechanisms of defense recognized in the field of psychodynamics" (Ibid.). These two cases are said to be similar. But is there any important similarity here? Is this any more than verbal magic, employing the words "defense," "walling off," etc. in both cases, and letting this language convince us that there is an important resemblance between the two states of affairs described? But this is not all. We are next urged to compare both (1) and (2) with (3): the way "a committee may at first retreat from a position it formerly held under the attack of a dominant new addition to their membership, but will eventually return to a stable generally acceptable compromise" (Ibid.). And what here? Does the new member kill off a few of the old? Are new resources added to the committee from the outside to cope with the invader? Who knows? We are not told. Finally, in the pursuit of yet further enlightenment, all of these cases are to be usefully compared with the behavior of the Allies in the Battle of the Bulge. Miller remarks of this series of examples that "All this may sound like imaginative analogy" (using the word "analogy" in his abusive sense). But "we believe that it is more. It is the postulation of formal identities between many sorts of response to attack, which can be quantitatively demonstrated if the proper dimensions are used" (Ibid.).

But exactly what is it that one is being urged to believe, and what reasons can be offered in support of that belief? The question of factual justification for our beliefs, and hence for a particular employment of concepts in the formulation of those beliefs, is a vast and difficult one. Still, something may be said on this question, and must be said if I

am to make clear the sense in which I feel that these so-called theories are lacking in empirical foundation.

Let me consider an example of my own devising. First, we have a scientist, A, who with considerable ingenuity devises a means of expressing the rate of frost formation on the coils of a refrigerator as a function of the number of times the refrigerator door is opened. Next, we have another scientist, B, who with similar ingenuity succeeds in representing the rate of carbon deposit on the cylinder head of an automobile engine as a function of the number of times the car is started. Finally, we have a general systems theorist, C, who notices that the mathematical function is the same in both cases. Now, the mathematical function, when interpreted for A's case, or for B's case, can indeed be regarded as a statement about the behavior of refrigerators or automobiles, and as such it can certainly be based on a solid foundation of empirical fact. But what of C? Well, immediately the problem arises as to exactly what C means to imply or thinks he has proved or suggested by drawing our attention to the formal identity here. He seems to be very excited about his discovery; he seems to feel that this is something much more than merely an odd coincidence. But what more? Well, first, it proves that both refrigerators and automobiles are systems, but we have already noticed the strikingly empty character of this assertion. Does it further prove that a refrigerator is a kind of automobile, or vice versa? One just doesn't know. The bare possibility of functional or structural analogies is cited as proving or suggesting that, e.g., a committee is a sort of an organism, or that a jury is rather like the economy of an isolated community.

The truth of the matter seems to be that the only grounding in empirical data which the general systems theorist offers is the data which have already been cited by A and B in support of their special theories. These data, together with the fact that the same function fits both cases, will prove or substantiate only the two special theories and the believe-it-or-not coincidence that the same function really does fit both cases. If the general systems theorist would *be* more than a mere collector of such coincidences, then he must *do* more than make impressive gestures in the direction of various analogies. This last remark requires a qualification. The general systems theorist is seldom, perhaps never, *only* such a theorist. It is much more likely, in any actual case, that A, B, and C in our example will all be the same person. This seems to be pretty clearly so for Professor Anatol Rappaport, the mathematical biologist on the Chicago committee. He has developed equations to describe the spread of neural impulses, the spread of rumors, and the spread of epidemics, and has demonstrated great ingenuity in so doing. And he too has found that "models of all these phenomena look mathematically very much alike" (p. 19). I do not see, however, that it detracts from his genuine achievements to point out that this similarity of mathematical models is, for all he tells us, a sheer coincidence.

But the basic point should now be clear. Either we are to conclude something from the analogies, or we are not. If the latter, then it is difficult to see why the analogy needs to be noticed at all. And if the former, we want to know: (1) What are we to conclude? (2) How extensive is the analogy, and what in each particular case is the disanalogy, together with its bearing, if any, on the conclusion? And (3) is there, or could there logically be, any independent evidence for the conclusion?

Under what sorts of circumstances could these conditions be met? Perhaps they are impossible conditions. I think not! It might well have been that when biologists first began to hit on the concept of homology, they actually proceeded in some such fashion. A biologist might have noticed that a certain fin on a fish stands in relation to the rest of the fish, in a way similar to the relation between, say, the leg of an animal and the rest of that animal. And then, from reflection on several such cases, together with what was already known or guessed about the role of inheritance in determining the characteristics of organisms, he may have concluded from his analogy that certain fish and certain animals must have had common specific ancestors. This would be (1) above—what we are to conclude; and note that it is not merely a repetition of the analogy. The disanalogy (2) in this case would include that legs can be walked on while fins cannot; while (3), the possibility of independent confirmation, would be present in the possibility of discovering missing links, and of thus filling out the phylogenetic story.

III

While the major conceptual difficulties specific to Miller's version of general systems theory lie in these questions about the interpretation of analogies, other problems of a more general character are worth

noticing. Like many other would-be new departures in social science, general systems theory interests itself in "information theory," with its crucial concept of "communication." "Communication" is a fashionable word nowadays. What I want to notice about the word is that its employment to cover all cases of people talking to each other may be seriously misleading, and that this use may involve an implicit commitment to a theory of mind.

The theory is, roughly, that everyone always has complete access to his own thoughts, that these are always transparently clear to him, and that he always knows exactly what he means when he speaks; but that because of a queer sort of barrier other people are often, or even always, liable to misunderstand him and to fail to grasp clearly what he means. The theory would also usually add that A never has direct, but at best only inferential and indirect, access to the thoughts of B. Now this is a theory that many contemporary psychologists and philosophers are concerned to reject. But notice the extent to which the very general use of the word "communication," for all sorts of talk other than soliloquy, tends to reinforce this picture. Look to the antecedents of this use of the word. Think of its uses in, e.g., geography, where the transportation and communications of a country are discussed. Remember the sense of "communication" in which it has been respectively true of the telegraph, the undersea cable, the telephone, and radio—that each was a new form of *communication*. Notice that in all these earlier, and perhaps outside academic circles still standard, uses of the word "communication," there really is some obstacle to overcome, usually distance. Jones is in his office, and his wife is at home, and if she were right there with him, he could tell her that he would be a bit late for dinner. There would be no difficulty about *telling* her this. But she's not there; there is a difficulty; even if he yells, she won't hear him. So he uses a medium of communication, the telephone, with which he overcomes the difficulty.

The basis for my conceptual query about "communication" should now be getting clear. If he and his wife are sitting in the same room, and she wants to know what time it is, and he looks at his watch and tells her, he certainly does not have any difficulty about telling her. But, in the newly fashionable extended use of the word "communication," this latter transaction too is described as *communication*. He wants to let her know what time it is, so he has to get word to her, there, across the room, so he casts about for some means of doing this and finally he hits on it: "I know, I'll utter the words, 'half past six.'" Thus described, it is clear that there's something curious going on here. And the employment of the word "communication" to cover such cases fits in only too well with the theory of mind sketched above. If his wife were only here (i.e., "in" his mind), he could tell her with no difficulty. But, alas, his mind is private, there is a barrier to be overcome, and a medium of communication, namely language, is employed to surmount that barrier.

Another conceptual muddle can easily arise over the notion of "environment," in a context of systems and subsystems. We often speak of someone's *social environment*, and, from this, the move to saying that the society in which he lives is his *environment* is natural and perhaps proper. But such moves can lead to odd results if we now start to specify the relation between the individual and the society, especially if that specification be in terms of the notions of "part" or "member." And general systems theory is peculiarly liable to do this with its treatment of both individuals and the groups to which they belong as "systems." For example, Miller at one time * proposed that we so use the word "environment" that where any system at level n has a component, a subsystem, at level $n - 1$, there the level n system is said to be the *environment* of the level $n - 1$ system. Now this is a strange use of "environment." Consider, e.g., Ted Williams and the Boston Red Sox. In any sense in which it is true that a person is a subsystem in a group, then I take it that a baseball player is a subsystem in his team. But, of course, Williams is a member of that team. So Williams is a member of his environment? But is this even an intelligible, much less a true, thing to say? Do we have any clear idea at all what would be meant by saying that Williams either is, or is not, a member of his environment? And, similarly, with Jones' heart and Jones. (Levels $n - 1$ and n, respectively.) His heart is a part of him. Is it also a part of its environment? The truth would seem to be that, as the term "environment" is usually used, we *distinguish between* an organism and its environment, in a way which logically precludes that an organism should be spoken of as a part of, or as a member of, its environment.

At this point, let me interject a remark about the role of philosophical analysis, vis-à-vis psychology or any other science. I take it that there is a legitimate activity called conceptual or linguistic analysis, and that it can proceed very much as in my last two examples, to locate and, at least partially, to clarify conceptual problems. But in an important sense it cannot solve them. A general systems theorist might reply that his use of "environment" is indeed different from the conventional one, but that his use is preferable. Now it seems to me that, *qua* philosopher, I cannot decide such an issue. Nor does it seem to me that the general systems theorist is in any better case. Any changes of concepts, or alteration of the logical powers of words, have ramifications throughout the set—or *system*—of concepts in which they function. In actual ongoing scientific research, the logical powers of words are constantly undergoing changes. But the test of the adequacy of any set or system of concepts is the adequacy of the body of scientific laws which is formulated in terms of those concepts. And, of course, the latter is tested, at least in part, by the usual scientific procedures of observation, experiment, prediction, and the like. This last reflection is yet another reason for my feeling that in an important sense general systems theory is not science at all. For the experiments and the data which it mentions never seem to be such that they would enable us to decide either for or against the terminological legislation which it explicitly proposes, or implicitly requires. And, hence, in appraising these terminological recommendations, one is driven to fall back on logical analysis, of the kind I have been employing.

There are many other conceptual problems in general systems theory which would need to be investigated in any complete analysis. The notion of coding is a very general notion. You might very well be doing a bit of coding if you say "ouch," on instructions from the dentist, when he hurts you; as well as if you devise and employ a new notation for describing a chess game. The notions of "input" and "output" might well repay attention. They, too, are employed with a tremendous generality, as witness the "input of words to a jury," already cited. And, of course, the idea of "equilibrium," and the related notions of "homeostasis" and "negative feedback" would also have to be treated. Here, the appropriate question to ask would be like those we raised concerning "system." Could there, logically, be a non-homeostatic system? Are these terms used in such a way that one could describe what a non-homeostatic system would be like? My own impression is that

* In his paper read at Atlanta.

general systems theory uses "system" and "homeostatic" in such a way that a non-homeostatic system would be a contradiction in terms. And, if so, the general systems theorist may be doing no more, when he announces that all systems may be rewardingly treated as homeostatic, than I should be if I announced that all uncles may be regarded as males.

But my final specimen from Miller's terminological recommendations involves that delightful pair of terms "adience" and "abience" which he attributes to E. B. Holt, and whose adoption he recommends. He mentions them specifically in connection with his stress on the desirability of physicalizing the language and the procedures of behavior theory. They, like the grams-seconds-centimeters approach, are to do great things for psychology. He says,

Admittedly the dimensions of behavior are highly complex, and to translate them into physical dimensions will frequently be extremely involved, but we believe that a beginning can now be made. For example, take the terms 'adience' and 'abience' of E. B. Holt. He used these terms to represent something like affiliation or love and dislike or hate. Adience may be measured as motion through space over time towards a goal, and abience as motion away. The psychological conception of ambivalence may be equated to the behavior of an ambivalent organism that oscillates back and forth, toward and away from a goal, with motions describable in space-time dimensions (p. 8).

This is indeed Empedocles turned upside down. The ancients, too, had hold of an analogy when they described various changes and motions, e.g., the falling of an unsupported body, on the model of purposive behavior and striving. And physics began to make progress at exactly that point at which these conceptions of natural teleology were abandoned for physical objects *qua* physical. So now we are to resurrect this confusion, but from the opposite end, and assimilate to the motions of physical objects the behavior of goal-oriented, loving and hating, affiliating and disliking, organisms. Here indeed the analogy is very thin. Does one find himself invariably experiencing a rapid upsurge of "something like affiliation or love" as he drives rapidly to the hotel to meet a stranger he has corresponded with; and perhaps a slower building-up of this feeling as he drives slowly to meet someone else? And, when he leaves home in the morning, does something like dislike or hate for his wife well up in him, becoming more intense as he gets

farther away? May we then go on to use the sad fate of Buridan's ass, who starved to death at a point equidistant between two bales of hay, as not merely an amusing instance of conflict of desires, but as a veritable paradigm of such conflict?

Let us grant the obvious fact that physics is a wonderful science, and that both the precision of its conceptual structures and its power to predict and control far exceed those of the behavior sciences. Let us grant too that human beings are physical objects, that, e.g., when they tumble off tall buildings, they fall to the ground with an acceleration of $32 ft/sec^2$. Still it does not follow from all this that the way to solve conceptual problems in psychology is to abandon psychology altogether in favor of the physics of organisms. If the physicalizing of psychology can solve these problems, then there aren't really any problems at all, and there is nothing we want to say or explain about the behavior of organisms which we do not also want to say or explain about the behavior of stones and electromagnetic fields. But this is obviously false, so we would need to complicate and alter the structure of physical concepts sufficiently to enable physics to cope with the kind of subject matter which we all know psychology must deal with, and which we do all of us manage to talk about in daily life. And this, of course, is where "adience" and "abience" fail us. We know that we can much more adequately describe and explain the behavior of organisms in terms of our everyday language, than can Miller in terms of "adience" and "abience."

A final word on physics. Perhaps those behavior theorists who are so attracted by physics as a model science should look not only at its internal conceptual structure, but also at the logical relations between the language of physics and the ways in which non-scientists talk about physical phenomena. And perhaps it would be even more instructive to compare these two at a time when physics was groping its way, very much as psychology is today. There are such relations, plenty of them. They are not neat and tidy equivalences of meaning; these occur only within systems of concepts, whereas the relations I am talking about transcend such systems. It seems certain that the intelligibility and the adequacy of the physics of macro-objects of the kind we see and touch rests in part on the adequacy with which such everyday concepts as weight, speed, force, resistance, etc. find their counterparts in physics without too great distortion.

And, of course, some of my criticism of general systems theory has rested on a comparable conviction for psychology. If, in the adience and abience of Jones, the physical object, we cannot recognize even a caricature of Jones, the lover and hater, then we cannot be sold a science of love and hate in which these concepts are replaced by the directional acceleration of Jones' body.

REFERENCES

1. Chicago Behavioral Sciences Publications, No. 1. *Profits and Problems of Homeostatic Models in the Behavioral Sciences.* Chicago: Univ. of Chicago Pr.
2. Miller, James G. "Toward a General Theory for the Behavioral Sciences," *The American Psychologist,* 10:513–31 (1955).

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The Concept of Emergence

SOMEWHAT over a quarter of a century ago, Professor Stephen Pepper published a paper on "Emergence" (1) which was (and still is) symptomatic of a certain way of thinking on this topic. The paper had the virtues of brevity and clarity, and, which is more important, it went to the heart of the matter. The fact that the crucial step in its argument is a simple *non sequitur* by no means detracts from its diagnostic value as a document in the controversy over emergence.

Before we examine Professor Pepper's argument, two introductory remarks are in order.

1. Our aim is not to defend an emergentist picture of the world, but rather to criticize an argument which, if successful, would make this picture indefensible. As we see it, the question whether the world is to be conceived along emergentist lines is a scientific question which cannot be settled on a priori grounds.

2. The question "Does the world contain emergents?" requires to be answered in terms of a scientific account of observable phenomena, and although with reference to a given scientific picture of the world the question is a *logical* one which concerns the formal structure of this picture, taken absolutely, the question shares the inductive character, and hence corrigibility in principle, of the scientific enterprise. Indeed, since science presents us today not with one integrated interpretation of the totality of observable phenomena, but rather with a large number of partially integrated theories of more limited scope, the question inevitably takes on a *speculative* character, and becomes an attempt to anticipate the logical structure of a theoretical framework which is still in gestation. This speculative dimension must, of course, be distinguished from the previously noted corrigibility (in principle) of any answer to the question "Are there emergents?"