MODELING THE FORMATION AND USE OF CONCEPTS, PERCEPTS, AND RULES

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1. Introduction

In his fascinating recent investigation of "The Structure of Scientific Revolutions," Thomas S. Kuhn [5] argues that scientific research has two phases. There is "normal science," characterized by work within a shared conceptual and evaluative paradigm or framework; and there is "revolutionary science," involving efforts to develop new and more adequate frameworks. Each such framework comprises assumptions about how one works, what one works towards, what the important questions of the field are, and so on.

Work now going on in psychology under such rubrics as computer simulation, heuristic programming, and information processing or dynamic modeling constitutes an effort to introduce a new paradigm into that field. Basic to the paradigm is conceptualization of psychological activity in terms of structures and processes more or less analogous to certain classes of complex computer data structures and programs. Miller, Galanter, and Pribram [6], for example, take "image" and "plan" as their basic concepts, and in the work we consider here psychological propositions are stated in actual computer programs.

The origins of this paradigm can be traced at least to Turing [23], who tried to make a case for computers as complex information processing systems capable in principle of intellectual activity in the same sense that humans are. Five years later, Selfridge [16] and Dinneen [1] published descriptions of a system actually capable of very limited pattern recognition (discriminating A's from O's). Shortly thereafter, Newell, Shaw, and Simon [9] reported their Logic Theorist, a program capable of proving theorems in elementary symbolic logic, and in ways that seemed to them to parallel the kinds of activity going on in human problem solvers attempting the same kind of tasks. This, furthermore, was deliberate. The Logic Theorist was designed to incorporate strategies, procedures, rules, and heuristics that had been observed on a more or less informal basis in human subjects. The next large step was the General Problem Solver program, again the work of Newell, Shaw, and Simon [10]. This system was designed to handle a *variety* of problems rather than being limited to logic or to any other single domain. Not quite so general as its name suggests, it nonetheless represented a sizable advance in conception, and to some extent in capability, over previously existing models.

Since the early 1960's, however, there have not, I think, been any radical new developments within the information processing paradigm itself, and I would like to take this apparent plateau as an occasion to examine here what the paradigm involves for psychology. How does it work, what are its limitations, and what are its general prospects? In doing this, we will examine two recent examples of what I see as outstanding research within this paradigm and then draw some conclusions from these examples. Both investigate categorization processes. Simon and Kotovsky [18] deal with the induction, representation, and utilization of concepts or rules. Uhr and Vossler [24] show how one might design a system capable not only of processing sensory information with respect to stored patterns or percepts, but also to some extent of improving its performance by learning from the errors it makes in categorizing inputs and recognizing patterns.

We will consider the goals, the organization, and the assumptions behind each of these programs, concentrating in particular upon what seem to be their virtues and limitations. Where do such programs get us? What do they tell us we didn't know before? Where do we go now that we have them?

2. Motivations and assumptions

It is useful to begin by looking at the motivations underlying each investigation. Simon and Kotovsky seek "to explain in what form a human subject remembers or 'stores' a serial pattern; how he produces the serial pattern from the remembered concept or rule; and how he acquires the concept or rule by induction from an example." Imagine a student taking an intelligence test. These tests may contain items in which one is given a sequence, perhaps something like $p\ o\ n\ o\ n\ m\ n\ m\ l\ m\ l\ k \cdots$, and asked for the next letter. These items may be written at any level of difficulty. Simon and Kotovsky want to provide a theory to account for the information processing behavior involved in working at such tasks. They postulate that human beings are able to form an internal representation of a rule, pattern, or concept satisfied by such a string of letters, using a small set of elementary relations ("same letter," "next letter in the alphabet," and so on). They also postulate that in addition to this language for representing rules humans have systems of processes—*programs,* capable both of inducing rules from such sequences and conversely of interpreting these rules so as to be able to generate and extend the corresponding sequences. Simon and Kotovsky argue that the rule *is* the concept embodied in the sequence, and that the ability to produce and extend sequences implies the existence and utilization of rules similar to those they give.

These assumptions are embedded in a computer program described in their paper in several versions. The program contains some one thousand statements in a nonnumeric computer language (IPL-V, Newell [8]), not a large number as such models go, but large enough to suggest the radical increase in complexity by comparison with conventional paradigms for formally specified psychological models.

Uhr and Vossler's [24] program aims at somewhat different goals. Most of Newell and Simon's models are nondevelopmental. They are attempts to approximate the structure and process systems held to exist at some point in time in some human heads. Newell and Simon are not concerned in their models with how these cognitive and problem solving systems get that way; but Uhr and Vossler are. They want to know how one might build a system that would learn to recognize new patterns, to represent patterns to itself, and to develop this capability over time, correcting itself as it goes. "Unknown patterns are presented to the computer in discrete form, as a 20×20 matrix of zeros and ones. The program generates and composes operators by one of several random methods, and uses this set of operators to transform the unknown input matrix into a list of characteristics. . . . These characteristics are then compared with lists of characteristics in memory, one for each type of pattern previously processed. As a result of similarity tests, the name of the list most similar to the list of characteristics just computed is chosen as the name of the input pattern. [Now the program is told the correct name of the input.] The characteristics are then examined by the program and, depending on whether they individually contributed to success or failure in identifying the input, amplifiers for each of these characteristics are then turned up or down. This adjustment of amplifiers leads eventually to discarding operators which produce poor characteristics, as indicated by low amplifier settings, and to their replacement by newly generated operators."

As Uhr and Vossler note, the program is capable of two kinds of learning, a relatively simple kind of rote learning involving storage of characteristics and weights in memory, and a somewhat more complex form that involves the program's analysis of its ability to deal with its environment and its attempt to improve this ability.

Having introduced these two examples, what can we say about the present or potential value of such models as tools for psychological research?

3. Programmed models as tools for psychological research

3.1. *Uhr and Vossler pattern recognition program.* Most of what follows refers to the Simon and Kotovsky serial extrapolation model. But let me begin with some remarks about the Uhr and Vossler pattern recognition program. Their results, first of all, are quite impressive when judged against what was available before. For example, the program was tested with a twenty six letter alphabet as the set of possible inputs. On the sixth pass, it was one hundred per cent correct on the patterns on which it had been trained, and ninety six per cent correct on unknown alphabetic patterns, that is, patterns not present in the training sequence. Note also that the program has an interesting general purpose quality about it, discriminating reasonably well within small (five to ten element) sets of cartoon drawings of faces, of handwritten segmented arabic letters, and of digitized spectrograms of speech, all presented of course as patterns on the 20×20 matrix. In discriminating among meaningless 20×20 patterns the program actually performed at a higher level than human subjects did. That, however, is a point raising questions of its own, as we shall see.

But though we want to acknowledge such evidence of the performance capabilities of the program, and though Uhr and Vossler do not in this paper propose their program as a psychological model, as Simon and Kotovsky do, it may be useful to point out explicitly what to me seem interesting differences between the behavior of the Uhr and Vossler program and that of human pattern recognizers. These observations are in no sense based on experimental evidence. They do however involve what seem to be reasonable assertions about the characteristics of human information processors, and they are relevant to much of the work now being done within the information processing paradigm.

3.2. *Specifics in the program.* First we may note that in the Uhr and Vossler program a number of very specific operations underlie the dynamics of learning and the effects of success or failure in recognizing patterns. For example, it uses a particular scheme for averaging over differences to decide among the patterns it works with. It modifies scores, evaluates operators, and increases or decreases the contributions of particular functions to the total pattern recognition decision rule in similarly fixed and specific ways. This multitude of specifics is highly characteristic of information processing models. They are needed to make the programs go. But they are extremely difficult to check out, since there really is no relevant psychological evidence, at least to my knowledge. It is possible in principle to make sensitivity analyses of the extent to which any particular set of specific values affects the success or failure of the program. If we can show that performance is more or less invariant over a fairly broad range of weights, probabilities, and combining rules then the selection of particular values will not appear terribly important. On the other hand, if program performance is very sensitive to such variations we may begin to get uncomfortable about the arbitrariness of these key decisions. Generally, however, information processing programs are reported without this kind of sensitivity analysis, and so when we read them or try to interpret them, we simply do not know just how dependent any particular program is on any one or more of the many arbitrary decisions it incorporates.

3.3. *Limitations of programs.* Compare pattern recognition as it goes on in the Uhr and Vossler program with what seems to hold in humans. Given a set of stored patterns, the program is, for example, unable to decide when the fit of the best of these to some input is too poor to be acceptable. In other words, as it now stands, it is unable to conclude that it needs to form a new pattern. This is a rather interesting and important point. How should a system decide on the inadequacy of its current set of categories? How sure are we, for example, of the adequacy of the current stock of psychological categories? In Kuhn's terms, how does a scientist decide that the old paradigms are not working adequately and that a new paradigm is needed? It obviously is unfair to complain that the program cannot make this decision when it is so very difficult to frame, let alone answer, the problem for ourselves. But somehow or other we do it, and the program does not. The point illustrates the limitations of such systems: they have their boundaries.

No particular boundary has any absolute or permanent significance. It would be no great trick, for example, to add to the Uhr and Vossler program procedures for posing and resolving the question of conceptual inadequacy in one way or another. It also is obvious that no theory ever commences by taking a whole natural system as its immediate domain. But a few illustrations of some of the kinds of things particular programs do not do may convey some sense of the distance information processing systems generally still have to go.

Other more prosaic limitations also are worth noting. People seem to have the ability to pick out subunits and then to use these subunits as clues to things to look for in other portions of a total input pattern. The program has no such ability to segment complex patterns into components in this way.

Recall also that in one test the program did better in identifying meaningless patterns than people did. There is a sense in which this outstanding performance can be taken as a measure of the limitations of the program. If we take performance on meaningless patterns as a baseline for comparison of the program with people, then the fact that relative to this baseline people can do so very much better at meaningful patterns might suggest that they are functioning in ways other than those included in the program, ways involving the use of semantic cues and differential recognition strategies as a function of feedback on these cues.

Another obvious limitation of the program is the fact that each pattern gets a separate set of characteristic values. It is difficult to imagine that every pattern we are capable of recognizing consists of a distinct vector of values unconnected to any other such vector. Note once again, however, that within the information processing paradigm there are other treatments that avoid this difficulty, notably Quillian's model [13], which utilizes networks of interrelated concepts such that the meaning of a concept depends upon the linkages it has in the network.

Though the Uhr and Vossler program can revise its set of operators, it cannot compare the set of patterns it already has stored in memory to interrelate them or to derive more general rules or concepts from them. Here again is a boundary, one having to do with the ability of the program to deal with semantics. It doesn't use differences as significant information, as clues to the next characteristics to look for. The program examines any input pattern against all the operators and checks it against all possible stored patterns. This works reasonably well for small input pattern sets and small stored pattern sets, but it seems an impossible procedure for large stored sets. People almost certainly must use some sort of branching conditional procedures, making use of the semantic interrelations among patterns they already know in trying to determine how to classify a new input.

Another set of limitations of the program have to do with the extent of its flexibility. Remember that one of Uhr and Vossler's main goals is to build a flexible system capable of modifying or adapting its procedures as a function of feedback from its operations. But it is important to recognize the restrictions of the framework within which this flexibility obtains. For example, the program has impressive procedures for building new operators to use in characterizing its experience, but it has no way of using its experience to build new *procedures* for building new operators, as opposed to building new operators themselves. Here is the crux of the problem of intellectual development. How from past experience do we get new concepts that we use not simply as data, but as new tools in processing new experience? This higher order learning is something the present Uhr and Vossler program does not get at.

We can phrase the same point with respect to the notion of learning sets. On a fixed set of patterns the performance of the program will improve with experience, but the program never learns to learn in the sense of modifying the kinds of operations it makes on the data. It is not able to teach itself new skills as a function of its experience. It possesses, for example, an interesting and imaginative procedure for forming new operators by abstracting from an input. But the procedure itself is fixed and cannot be modified in the present program as a consequence of learning or experience. The flexibility that the program provides is flexibility within a strict fixed framework, which we may think of as a five stage sequence. There is an input pattern, operators are applied to the pattern to produce characteristics, the characteristics are matched to stored characteristics, a decision rule selects the pattern with stored characteristics closest to the characteristics of the input set, and then learning processes are called to modify the weightings of the operators in accordance with the feedback received.

In conclusion, the Uhr and Vossler program is a great advance over the Selfridge and Dinneen work, but still seems limited by comparison with what humans can do. As for the relevance of the program for psychological theory, to date the strongest claim one can make is that it is *sufficient* to do some of the things we know human information processors can do. Nothing has been adduced that forces us to take this as a model of human information processing in any stronger sense, and as we shall see, reliance on sufficiency or gross functional equivalence is characteristic of arguments for computer programs as psychological theories. But if we really want to validate computer models of psychological processes, then we must ask how we might get other, more adequate kinds of evidence. That is the problem we now take up in the context of the Simon and Kotovsky sequence extrapolation program.

4. Validation of programmed models

4.1. *Evidence cited by Simon and Kotovsky.* What evidence do Simon and Kotovsky cite in support of their theory? They make three basic assertions. The first is a sufficiency argument: the processes, mechanisms, and representations in their program are sufficient to permit a mechanism endowed with them to induce rules and to produce and extrapolate sequences. Next, they assert that in at least one sense their program is parsimonious. The kinds of processes used can be found in other information processing programs. Third, the predictions of the theory show good qualitative agreement with the gross behavior of human subjects working at the same tasks.

The sufficiency or functional equivalence argument raises some very interesting questions, for example, functional equivalence with respect to what. Airplanes, kites, and clouds all are functional equivalents for birds if we measure only the ability to get up and stay off the ground for a while. No one proposes them as theories of how a bird does it. Clearly the Simon and Kotovsky model is sufficient to carry out the sequence extrapolation task. But is its performance similar enough to our own for us to feel obliged to take it seriously as a model of human information processing? How do we measure these similarities, and precisely which similarities ought we be concerned with? The first problem is discussed elsewhere in some detail (Reitman [14], pp. 16–37). Most of the rest of this paper is an attempt to come to grips with the second, in the context of this particular rule or concept forming program.

The parsimony argument seems even more debatable. True, most information processing models match, test, use lists or strings, and so on. But so do many programs that have no psychological significance. Furthermore, higher order mechanisms in the Simon and Kotovsky program are not simple variants of a stock of mechanisms common to many such models. There are family similarities, but that is very different from parsimony in any strict sense.

Finally, the correspondence to human data is not really that strong. Simon and Kotovsky state that "the program solved none of the problems we have previously labeled hard and all but one . . . of the problems previously labeled easy. Hence, the pattern generator also provides excellent predictions of the relative difficulty of the problems for human subjects." But they are dealing with only fifteen patterns. They describe some four variants of their basic program, and nowhere to the best of my knowledge have they stated at what point in the program writing process they first looked at their data. It is quite possible that at least later variants of the program were written after examination of some or all of the data they report. If so, with fifteen data points and something on the order of one thousand parameters (the instructions comprising the program) to work with, it is difficult to weigh the substantiation by the empirical data very heavily.

Simon and Kotovsky also assert that the theory casts "considerable light on the psychological processes involved in series completion tasks. It indicates that task difficulty is closely related to immediate memory requirements." But they themselves note that simply by inspection of the pattern rules, as specified in their representation language, the easier problems have simpler descriptions. ". . . We could have made an almost perfect prediction of which problems would be above median in difficulty simply by counting the number of symbols in their pattern descriptions." What predictive residue then is contributed by the thousand odd instructions of the program? The incremental purchase of those instructions is very small. A mathematical model in the three or four significant variables they discuss probably would give as good predictions of the item difficulties as those obtained with the program.

Note that these variables come out of their language for representing pattern rules, and note also that this representation in turn was motivated by their desire to build an information processing model. But there is little in the data they report to substantiate this *particular* set of one thousand instructions, as opposed to any number of more or less similar ones.

4.2. *Limitations of the Simon and Kotovsky program.* As we did with the Uhr program, we also should note explicitly certain important limitations on the domain this theory applies to. The series to be extrapolated involves a minimal semantics. That is, letters as used here have meanings only insofar as they have positions in alphabets. Furthermore it is true for this domain that once you have a rule you can produce an item corresponding to the rule. Few of the things people deal with are that straightforward. We may be able to induce enough from instances to recognize a man, but we cannot produce one by interpretation of a rule.

Simon and Kotovsky deal only with nonprobabilistic concepts; there is no error variance to be considered. Few of the things people deal with have this characteristic. Then, too, there is nothing in this model to handle disjunctive or relational concepts. Finally, as we noted above, the model is nondevelopmental. In their experimental situation, Simon and Kotovsky give subjects ten training trials and treat them as stationary systems from that point on. But while this simplification strategy is reasonable as far as it goes, we probably do not want to take it as more than that. It seems unlikely that their subjects came to the experiment with a ready made special purpose program for the serial extrapolation task. Once again it seems necessary to go beyond this model, perhaps in the direction of Hormann's [4] attempt to write programs which assume humans can assemble appropriate systems of routines as they gradually gain experience with a task domain.

In such a frame of reference we might expect that the subject's sequence extrapolation program, which Simon and Kotovsky take as given, actually may be constructed by him in the course of their experiment. As the subject induces hypotheses about the structure of the task, he pulls in promising strategies, tries them out, learns more about the task, and modifies his operations as a result. If his activity appears to have succeeded in abstracting aspects of the problem, he works from that base on the reduced problem. If it seems to have failed to do so, he goes back and tries something else. In short, one might think of the Simon and Kotovsky program as something the Newell, Shaw, and Simon General Problem Solver [10] might construct, in principle at any rate if not, at its current level of competence, in fact [17].

4.3. *Difficulties of adequate test of information processing models.* Psychology frequently asserts that no theory is worth much unless it is testable. But what constitutes an adequate test of information processing models of the sort we are considering? Generally, a model is evaluated by testing its crucial assumptions. But what are the crucial assumptions of an information processing model? As DuCharme, Jeffries, and Swensson [2] note, the advantage of the information processing model is also its disadvantage. It has been claimed and it is true that writing programs forces the theorist to make his assertions explicit. But it does not force him to make explicit which of the one or five thousand program statements are crucial.

Some of the propositions in a model like the Simon and Kotovsky program are testable in isolation. For example, they store the alphabet as a list or linear string which in every case must be entered at the beginning. Preliminary studies by Sanders [15] suggest, in fact, this is not the case for humans. His data indicate that the stored alphabet behaves as a random access string. The time subjects need to respond with a letter some fixed distance from a stimulus letter varies with the direction of movement and the number of intervening letters, but not with the position of the stimulus letter in the alphabet.

This particular assumption hardly seems crucial to the Simon and Kotovsky model, but the example suggests something of the kinds of tests of isolated assumptions we might make. Many of the assumptions are more difficult to assess because they cannot be isolated. They are propositions involving the whole system. For example, to what extent can a single model describe the behavior of all or even most of the subjects? Are there any strategies that can be defined as *necessary* to an adequate model of sequence concept formation. After all, our fundamental research aspirations will vary depending upon whether there are a hundred different strategies for doing this task or only two or three. What about alternates to the Simon and Kotovsky assertion that the induction of a rule is prerequisite to sequence solution? Simon and Kotovsky treat successful sequence extrapolation behavior as clear evidence of the development of a rule for a sequence. The evidence provided by DuCharme, Jeffries, and Swensson indicates that these are not necessarily coincident. Their work suggests that in order to extrapolate the sequence, you do not need to have the whole rule stored in the sense the Simon and Kotovsky model assumes it is stored.

The basic difficulty in testing information processing models in the traditional sense, the absence of clear distinctions among the assumptions as to their importance, becomes obvious when we consider the work of DuCharme, Jeffries, and Swensson [2], in more detail. Using an on line computer with a cathode ray tube, they presented sequences for extrapolation in such a way as to get a complete record of every overt examination of an element of a sequence by a subject. In the initial presentation, each letter of the sequence appeared and then disappeared in turn. If the subject wished to see the letter again, he had to indicate with a light pen the location in the sequence of the letter he wanted to look at. They also required subjects to extend sequences for several letters, rather than just one. Finally, subjects were asked to think aloud and their verbal protocols were recorded. The modified set of sequences employed included a subset of the Simon and Kotovsky items together with others chosen to provide an exact test of the Simon and Kotovsky assertions concerning the sequence variables determining difficulty of solution. Specifically, DuCharme, Jeffries, and Swensson varied cycle length (the length of the basic repetitive units out of which sequences are constructed), number of memory locations required, as specified in the pattern description language Simon and Kotovsky employed, and number of fixed elements per cycle.

The data in [2] are consistent with the Simon and Kotovsky model at a great many important points. But if we are trying to decide whether to accept the model, we must pay special attention to those points at which the model fails. These failures are of several sorts. At the lowest level, it turns out that people do not appear to follow induction strategies as fixed and invariant in order as those specified in the program, and they sometimes use relations among elements other than those in the model. Surely, however, it would be unreasonable to reject the theory simply because of minor local differences of this sort.

But at what point do the discrepancies become critical? The program operates in such a way that fixed element sequences will be easier or, at the very least, no more difficult to learn than comparable sequences without fixed elements. This turns out to be incorrect. On all measures, the fixed element sequences are significantly more difficult to learn.

Finally, DuCharme, Jeffries, and Swensson are able to show that at least some subjects who are able to extrapolate a next element of a sequence nonetheless do not seem to have induced a Simon and Kotovsky rule. Their subsequent behavior involves substantial light pen interrogation of the CRT display, sometimes in patterns suggesting that they have disaggregated the sequence into components and are extrapolating on an *ad hoc* basis. For example, when required to give the 14th letter in a sequence of cycle length three, they first proceed to examine the letters in positions 2, 5, 8, and 11.

4.4. *Is the Simon and Kotovsky theory disproved?* Do facts like these disprove the theory? In some sense they do. Yet it should not be too difficult to find modifications that will account for them. In fact every such result that suggests a new modification may yield new insight into the behavior and an occasion for re-examining and improving the theory. But what can we say in the context of the paradigm as a whole? What kinds of facts would allow us to reject such a theory as a whole, rather than in effect simply chipping away at individual subroutines.

Simon and Kotovsky note that "a theory of the sort we have proposed permits one to examine the microstructure of the data, and develop quite specific hypotheses about the processes that human subjects use in performing the tasks." The experiment in [2] does just this. But where do we go from there? Simon and Kotovsky assert that the ability to extrapolate a sequence implies possession of a complete rule, and as we have seen, the study in [2] shows that some subjects also extrapolate using what we might term incomplete rules. Does this contradict or disprove the Simon and Kotovsky model? That will depend on our subjective estimate of where, within this general family of models, one model ends and the next begins. Once again, it is hard to know what one should mean by validation or invalidation under these circumstances.

The typical information processing model is in some degree a collection of miscellaneous bits and pieces, at least when we attempt to specify the key assertions we must verify if we are to accept the model as a whole. We do not yet know how to specify subsets of assertions such that we feel free to reject the model as a whole if they do not hold.

Note that information processing models are not unique in this respect. The same problem applies with respect to such globally stated theories as Hebb's [3]. As I have noted elsewhere, most of Hebb's specific neurophysiological mechanisms have since been shot out from under him. His theory, however, remains. It does not seem inaccurate to paraphrase Hebb as arguing that there are certain basic psychological problems, that they might be handled by means of his cell assembly concept, that the cell assembly concept might be realized neurophysiologically along the lines he has suggested, but that, if not, then it must be realized in some other way because the psychological evidence makes it necessary to assume the existence of elements and processes of the sort he postulates. Nor was Harry Harlow castigating information processing models when he wrote in 1952 of "present-day rubber band theoretical systems [that] can stretch to encompass any data regardless of how opposed they may be to the original predictions of the theorist."

If information processing theories also are subject to stretching, they still retain one very substantial advantage over all of their competitors in the field of cognition and thought. They cannot be stretched inconsistently. If they are, they will not work. Even granting that, the validation problem nonetheless remains a major source of difficulty for those who argue for the acceptance of the information processing approach as the basic paradigm for work in this area.

5. Conclusions

What conclusions can we draw about the prospects for this paradigm in psychological research on higher mental processes? The "program is the theory" version of the paradigm clearly will not stand in its literal form. Modification of one program statement alters the theory. The need to do so disproves it. Exactly what, then, is the theory? I wish I knew. Better documentation and more stringent attention to this problem in the future will aid us in making clear the critical aspects of a theory, as opposed to the inconsequential details built to run the program incorporating it. But that is no answer to a present tense question, and I have none to offer.

Another problem stems from the variety and size of information processing models. We run the risk of having as our theories simply large collections of integrated heuristics, not a very satisfactory situation when viewed against our hopes of discovering general psychological laws.

I have tried elsewhere ([14], pp. 41–47) to show that these aspirations themselves may be unrealistic. It may be that, at least for a while, psychology is going to have to content itself with a great deal of highly detailed description about the information processing strategies humans use in carrying out psychological activities. If so, it is not necessarily a fault of the models, as opposed to the systems modeled, that they tend to grow in the direction of large systems of specific mechanisms and heuristics. Whatever the merit of that argument, no such set of general laws seems to be emerging from research within the information processing paradigm.

We need also to return to the problem of validation by sufficiency or functional equivalence. Within limits we now have explored at some length, these programs do carry out activities until now uniquely characteristic of human information processors. Furthermore, in such systems as the Logic Theorist, the General Problem Solver, and the Simon and Kotovsky programs, the paths taken in these activities show many close parallels to human performance. The sufficiency argument takes its most powerful form, however, only as we increasingly add psychological constraints to the statement of functional equivalence. That is, if one knows something about the limitations of the organism that is carrying out the function, then one can insist that those limitations be put in as constraints on the program. The more such constraints there are, the more impressive the functional equivalence becomes.

The problem is we do not know very many of these human information processing constraints, and information processing models have yet to prove useful in discovering them directly. They may be beneficial indirectly. We already have seen, in Sanders' work, how a well defined assertion in an information processing model may lead to empirical results. The findings in this particular case suggest that fixed and well learned ordered data sets, such as the alphabet, may be stored and retrieved in a manner consistent with a random access memory model. Similarly, empirical research motivated by information processing models, for example [2], may one day provide new bases for inferences about fundamental human process and capacity constraints. But they haven't yet.

Several investigators, Neisser [7], Pew [11], [12], Sternberg [20], [21], [22], and Smith [19], are trying to tackle questions of this sort directly, primarily in the domain of short term processing phenomena. To my eyes, the work is promising, beautifully done, but as yet rather frustrating. Taken as a whole, these studies so far seem to show that the human information processing system is complex enough to be able to pick and choose how it will perform on a given occasion. The invariances we get hold only under very specific conditions, much as in the experiments conducted by stochastic learning theorists. Across situations we still don't seem able to characterize the constraints human information processing must obey. In short, if the goal of such research is to describe the components of human information processing systems, as a computer manufacturer might describe the memories, access times, and processor characteristics of a new computer, then it seems fair to say that the outcome of the endeavor remains in doubt. In particular, we cannot yet reject the frightening possibility that human information processing is carried out by a messy, irregular conglomerate of structures, the end result of millions of years of evolutionary opportunism. If so, it will be a while before we learn enough to describe it in a sales brochure or, to return to our own problem, to incorporate it as a package of constraints in our information processing models.

Our catalog of questions about the information processing paradigm concludes with one last related problem, that of distinguishing process structure from task structure. How much of the Simon and Kotovsky model, for instance, is about the psychological processes in sequence extrapolation as opposed to the structure of the sequence extrapolation task? Could the sequence extrapolation task be carried out by *any* information processing system whatever so that it did not require detection of cycles? If not, if cycles must be found in one form or another in order for the problem to be solved, then it appears that "humans find cycles" is a psychological proposition only in a weak sense. It follows from two basic assertions, (a) that the human information processing system is a subset of all information processing systems, and (b) that *any* information processing system must find these basic structural units if it is to solve the problem. In this case, the model remains relevant to psychology by dint of proposition (a), but there is nothing about it *specific* to humans or psychology. The same question applies to the Uhr and Vossler program, and probably also to others.

One caveat is in order. Many of my colleagues who build and use information processing models no doubt would take issue with my one sided emphasis here on the problems of the information processing paradigm. This certainly is a minority report, and may even somewhat be overdrawn, for dramatic effect. Nonetheless, the statements about the programs and what they can and cannot do are reasonably accurate; and though the peculiar problems of the information processing paradigm occasionally tickle me, I do not feel they should be sneezed at.

I certainly am guilty though of having ignored in the foregoing what is perhaps the main value of these models to date, their utility as frameworks for thinking about thinking. We are much more likely to make progress when we have ways of grasping mentally the phenomena we wish to study. These our information models uniquely provide. Much that we are concerned with still falls outside the boundaries even of the best of our models, and it is important to be aware of this. Nonetheless, there is every reason to anticipate continuing progress within the paradigm as we encompass increasingly more in our models and become increasingly better able to state and assess critical points of similarity and difference with respect to human performance.

The study of cognition, thought, and higher mental processes is, in Kuhn's terms, probably best characterized as preparadigmatic. We are still searching for the best ways of going about our work, and as far as I am concerned, the information processing approach is without question the strongest contender we have had to date. It has given us key concepts enabling us to disaggregate complex mental processes, pulling out substantial portions of behavioral variance in particular areas and simultaneously providing schemes in terms of which to organize and investigate adjacent phenomena.

In conventional factor analysis when we take out a factor we create a new subspace, reducing the complexity of the original space. Somewhat the same thing occurs when we build an information processing model. Even though it has its boundaries, taking it as given effectively reduces the complexity of the total system of phenomena and provides a set of concepts keyed to the model which may be used in categorizing those aspects of the phenomena occurring beyond the boundaries in an organized integrated way, *in terms of the model's concepts.* These are the points of tangency of these as yet unanalyzed phenomena with the model. The Simon and Kotovsky model applies only to single sequences, but it also provides a system of structures and processes for thinking about the learning and other changes that might carry over from one trial to the next. The Uhr and Vossler model evaluates new input patterns against each of its operators. But it also provides a starting point for a more realistic representation of human information processing since we may now ask how we might modify this concrete and well specified system so as to enable it to take advantage of cue and semantic relations among operators. When we look at such models and ask what they leave out we are simultaneously making use of them as tools for organizing our analysis of the phenomena left out. Thus they enable us to discover their own inadequacies and to develop their successors. They are means of exploration with respect to the structure both of complex tasks and of the information processing systems that carry out those tasks. In this light, the main contribution of Newell, Simon, and their associates is not so much in any particular model as in having provided this new paradigm candidate, a rare event, as Kuhn will testify. The study of higher mental processes will never be the same again and that is a very good thing.

The two models examined in this paper were discussed in detail during the past trimester in my course on models of thinking, and many of the points considered here were first suggested by members of that class. I am particularly indebted to Messrs. W. M. DuCharme, R. E. Jeffries, and R. G. Swensson both for the contributions specifically acknowledged above and also for those undoubted others the traces of the origins of which were melted away in the heat of our conversations. R. W. Sauvain and D. D. Wheeler also read the paper and suggested several very helpful clarifications.

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