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Will Cognition survive?

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The most important problem for cognitive psychologists is that of facing the complexity of the task we are engaged on. The key to this lies in the way we develop theories rather than in our experimental methodology. Not that the latter should be ignored, but without better notions of the mechanisms of the mind we cannot begin to understand what our data mean.

Information processing models

An early decision for the theorist is that of deciding on a form of expression. My own decision in this respect has been to adopt the conventions of information processing modelling. The main principle here is to try to isolate processes which can be regarded as functionally modular. These processes are symbolised by boxes with inputs and outputs specified, usually in terms of the form of the code they carry. The extent to which sub-processes are differentiated will depend upon the data currently being considered. Equally, the extent to which the mechanism of a particular process is specified will also depend upon the data being considered. There is the assumption, however, that the nature of a particular process has no bearing on the form of the model as a whole. Let me take one example. On the basis of a number of experiments on long-term facilitation effects on the recognition of tachistoscopically presented words my co-workers and I have proposed the existence of a modality-specific input lexicon, also called the 'visual input logogen system' (Morton, 1979). This process is responsible for categorizing a string of letters in terms of its morphemic content. It has inputs from processes which analyse the visual input and form central processes which use the available context. Its output is in terms of the morphological constituents of the input, and this output is passed to processes responsible for syntactic and semantic analysis. The data indicates that this input lexicon is separate from an equivalent process which performs the same function for speech inputs. At its most elementary, the resulting model is as in Figure 1. The data seem to require a configuration of this form. However, the way in

*I am grateful to Debra Bekerian, Lindsay Evett and Pat Wright for attempting to restrain me. I regret they only partially succeeded.

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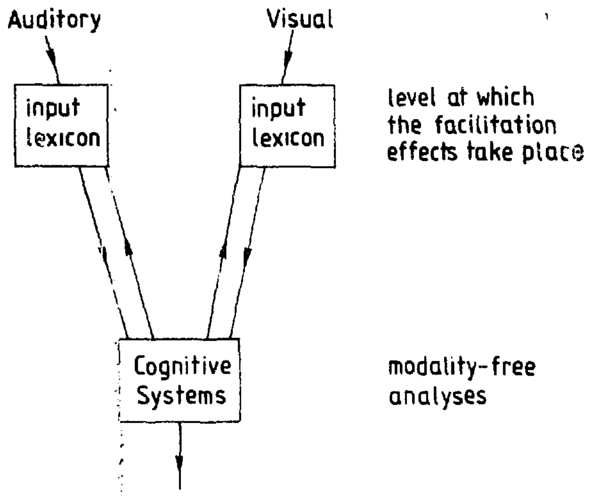
Figure 1. *The fragment of a model necessary to account for certain facilitation effects in word recognition.*



which the two modality specific categorisation devices operate has no bearing on the model as shown. Thus, one or both could operate as sets of simple, independent, evidence-collecting devices as I originally proposed (Morton, 1969). Equally, however, the auditory system could well have one or more of the characteristics of the cohort model proposed by Marslen-Wilson and Welsh (1978). For example, it could well be the case that negative information is used to inhibit inappropriate conditions. Figure 1 would be unaffected by the decision.

Of course, the model in Figure 1 is only a fragment of the model I currently work with. Precisely, it is just that fragment which is required by the facilitation data. There would be other ways of accounting for the same data, but the modelling is constrained by a variety of facts which enter into the definition of the functions shown (see Morton, 1968, 1979). The technique of long term facilitation of the recognition of impoverished stimuli serves now to define further the properties of the input lexicons. Thus, Murrell and Morton (1974) showed that prior presentation of SEEN facilitates the subsequent recognition of SEES. Prior presentation of SEED, on the other hand, has no effect on SEES. Thus we can conclude that the input logogen system is based on the morpheme rather than on the word. An unpublished paper by Steve Kempley and myself shows the same to be true for auditory stimuli. In addition there is no transfer between irregularly related words. Thus, having heard the spoken 'bring' has no effect on the subsequent recognition of the word 'brought' heard in noise. Other experiments by Osgood and Hoosain (1974) in vision and Gipson (unpublished) for audition show that *stockmarket* has a unitary representation at this level but *street market* does not.

We should note here the need for careful replication not just in the conditions of stimulus presentation but also in the state of mind of the subjects. What we have to do is to guarantee that the subjects are behaving in a controlled way, and are only using the resources which we think we are testing. Let us have a thought experiment. Suppose, one day, I read out to a subject the words 'lion, tiger, zebra, hippo, rhino' several times, telling the subject that these words will come in useful on the following day. Next day the subject is seated in front of a tachistoscope in the same room with the same experimenter. 'See if you can guess the word I am showing you', he is told. The first item is a short word beginning with *H*. 'Hippo', says the subject. Would we then want to say that the subject has shown long-term cross-modal facilitation effects in word recognition? And would this experiment constitute a non-replication of experiments where the relation between the tasks was less clear to the subjects, where the subjects were instructed not to guess and where no cross-modal effects were found? The answers could only be positive in the case that a superficial view were taken of our objectives. In terms of the underlying reality—i.e. the hypothetical constructs of the model—the two experiments have little in common. They encourage different strategies in the subjects who, then, are calling upon different resources. A possible diagram of the processes involved in the second task is given in Figure 2. Missing from this is the control device which determines when and how the appropriate memory record is accessed and used; but this doesn't present any conceptual difficulty. The task for the cognitive modeller is to express the two tasks in a common framework. This is sketched in Figure 3, where the central component is clearly in need of enormous expansion. What the model lacks, as shown, is a statement as to the conditions under which one or another route is used. What does seem to me clear is that the data can only be interpreted in terms of a model, that the model should give an account of a variety of tasks and that we should expect different tasks, or variants of the same task, to have different descriptions in terms of the model. The apparent chaos in the data on word recognition will only be resolved when we treat separately such tasks as lexical decision, word monitoring, reading time, semantic classification and t-scope recognition. To regard them all as equivalent with respect to the study of *Word Recognition* is a recipe for disaster. Our models should point plausibly to the differences between the tasks in addition to the similarities. Equally, a model which accounts perfectly for, say, the data of lexical decision, but which cannot, in principle, account for, say, the interaction of context and stimulus information in recognition, must be of less value than one which can account for

Figure 2. *The fragment of a model required to represent the use of a guessing strategy in responding to partially seen words.*

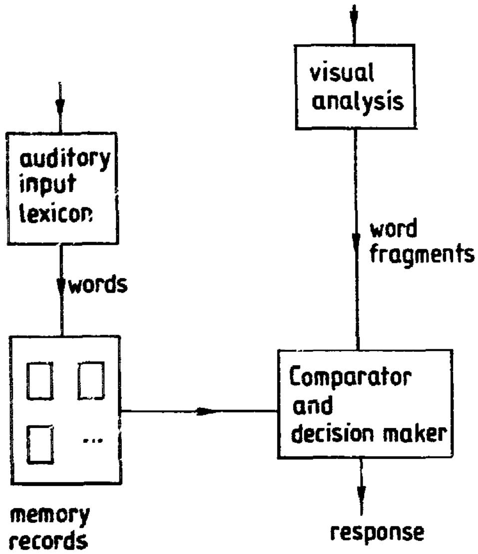
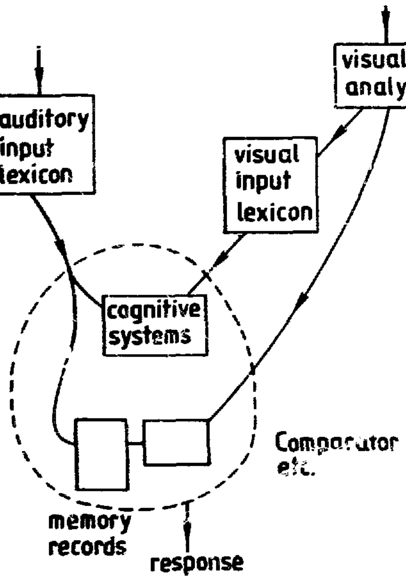


Figure 3. *A means of representing in the same model the effects represented in Figures 1 and 2.*



both, even though the latter may be more complex. There will be an indefinitely large number of ways of representing a particular set of data. Only by extensive use of converging operations can we constrain our choice in a reasonable fashion.

It follows from what I have said that the real objects of study for a cognitive psychologist are the cognitive functions. The experimental techniques and the data which these yield must simply serve to illuminate the processes and not become objects of study in their own right.

The same principle applies to dependent variables. Thus when we find that the variable of word frequency applies to recognition, reading time, lexical decision and picture naming, we can no longer assume that the effects in all these tasks are to be located at the same point in our model. Word frequency is going to be reflected in the frequency of experience of a word (and so, possibly, in any recognition process) in the frequency of production of the word (and so, perhaps, in the ease of mobilising or producing the spoken response) and in the diversity of its associations (and so in any task sensitive to this). Attempts to treat all frequency effects as equivalent seem perverse given the complexity of the models we already have.

Alternative models

Information processing models are probably only useful at a particular level of theorizing. They seem particularly powerful for relating together a wide variety of data. Sometimes, for a more detailed study of a particular task (if one has reason to do this) as for a more precise description as to how a particular function is implemented, then other notations may be preferable. What is necessary is that the objectives be clearly defined. Thus we should distinguish clearly between an information processing model of a set of processes and a flow diagram of the successive operations of these processes. The latter is exemplified in the analyses of picture-sentence matching (Chase and Clark, 1972). These are legitimate analyses, of course, but unless the operations depicted in such flow diagrams are linked to functions which operate in other tasks, their utility seems limited.

The second type of alternative model involves the more detailed specification of the operation of a process. For this, one of a variety of notations may be used. Production systems and augmented transition networks (ATNs) are two such notations. What we must beware of here is believing that the notations themselves have model status. There may be model implications in any particular production system (for example, in the way the memory buffer is handled), but productions and ATN's are not themselves models. Neither is 'psychologically real' (Bresnan, 1978); they are just means of expression which may be more or less convenient for particular circumstances. The result will be a model, but the model status of a simulation lies at a level above the specific implementation in the same way that any computer program has its essential functional description which is independent of the language in which it is written.

Levels of theory

In the previous section I made a distinction between model and notation. The importance of this distinction resides in the differences in the nature of the scientific discourse which are relevant to the two. Basically, a notation cannot be falsified. This is as true for the information processing notation as for Productions and ATN's. Models, on the other hand, can be falsified if they incorrectly represent the data. Thus a theory can be falsified without affecting the status of the framework (or notation) used in the model. The justification of the framework springs from its utility which has three components, communicability, generality (already discussed), and applicability.

Applicability of theory

Experimental psychology has a disastrous history with respect to its relevance. The lack of applicability of the verbal learning literature to real learning situations is the most often quoted. Those of us who live by application of our work have long ago seen the advantages which accrue from looking at real tasks as well as those in the laboratory. To start with it prevents us from taking too narrow a view of human processing capabilities. Secondly it provides a source of data which can feed into the models we create for laboratory tasks. One of the successes of information processing models for word processing can be found in the way the models can be used to describe the effects of brain damage. Specifically, the models are being used to provide a new kind of taxonomy of the effects of brain damage which appears to have a productive and systematic power lacking in previous taxonomies (Patterson, 1981; Shallice, 1981). Furthermore, a number of theorists use data from brain damaged patients to justify and refine their models (Coltheart, Patterson and Marshall, 1980; Marshall and Newcombe, 1973; Seymour, 1979). A genuine extension of this trend (other than the typical addition to a grant proposal whereby any study of word recognition is represented as 'having clear implications for the teaching of reading') can only improve our theories of cognition.

The danger of 'Cognitive Science'

Finally I would like to point to what I see as a trend which is in danger of becoming a cult. 'Cognitive Science' is an attempt to marry cognitive psychology, artificial intelligence and neuro-biology. There seems to me to be a right way and a wrong way to do this. The two dangers for cognitive psychologists are that they be led into one of two activities which can become ends in themselves.

1. Simulation
2. Reductionism

It is clear that the theoretical repertoire of a cognitive theorist can be greatly increased by a knowledge of devices of all kinds. Computers are particularly seductive in this respect. It could be argued that the computer analogy had a very serious effect on psychological theory in the '60's and '70's by providing the strong analogy of the central processor. This led to focussing on single channel hypotheses and general purpose computation devices to the excessive exclusion of more distributed computation. This arose, in part, from the, then, impressive simulation of human-like activity on computers. A similar error could occur with the new generation of machines. Further, simulation could become an end in itself for psychologists. The argument seems to be that with tasks as complex, say, as language comprehension or object recognition, there can only be one solution as to how it is done. The computer solution is then going to correspond to the human solution. The flaw here lies in the definition of the problem. If the computer does not make the same kinds of errors as the human, then the equation is difficult to justify. At best the claim that the simulation is humanoid can only be justified at the level of description of the model which is appropriate for the detail of data considered. And even this would only be the case if it could be established that all other possible models, at the level of concern or above, in principle could not accomplish the task. Inasmuch as this has been attempted it has rather been on the failure principle. Thus we find that since no-one has succeeded in segmenting speech by computer by a bottom-up process (i.e., on the basis of only the speech signal without using syntactic, semantic or pragmatic constraints) the claim is made that the human system cannot do it this way.

Now the failures of computer speech recognition clearly constrain us. Certain kinds of bottom up recognition devices are ruled out (specifically, those which failed) if one requires all of one stage of processing to be successfully completed before the next begins. But there is nothing to prevent us from supposing that the auditory input lexicon in Figure 3 is a purely bottom-up device which does the best job it can with further devices cleaning up the representation on the basis of the various constraints. (There are actually good psychological reasons for believing this not to be the case, but these are irrelevant for the argument.) The constraint from A.I., then, on cognitive theory seems weak. The danger I see is that A.I. will become a substitute activity for people whose abilities are needed in mainstream cognition.

The danger in the link with the neuro-sciences is more subtle. Basically it is that it will be thought that the only proper aim of a cognitive theorist is to explain how his constructs are actually implemented in the brain. Mehler and Morton (in press) have discussed this issue at length. I will restrict myself here to the claim that the achievement of a purely psychological description of cognitive processes is an end in itself. The mapping of this description onto the brain is an equally worthwhile but different activity. To have the two activities confused would also take energy away from the purely psychological task. It is claimed that biological facts must constrain cognitive theory. I have not seen such claims substantiated.

Conclusion

There seems to me no need to doubt the future of cognitive psychology. Our models will become more complex and we must learn to relate together models of different kinds which aim at doing different jobs. We must avoid being diverted from the proper task and learn to develop in theoretical sophistication. This way *Cognition* will survive its editors.

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