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The status of information processing models of language

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An introduction is given to the nature of information processing models in psychology. It is then claimed that, so far as cognitive and linguistic processes are concerned, there are no practical constraints from biology into psychological theory. Exceptions to this principle may be found where there is a precise one–one mapping of psychological and biological units. However, the study of the effects of brain damage makes it implausible that such relations exist for higher mental functions. Finally the possibility of relations between psychological and linguistic descriptions is explored by examining the results of experiments on the influence of word morphology in a number of tasks. It is concluded that psychology and linguistics do not constrain each other, but might exchange useful pointers to possible advances.

In this paper an attempt will be made to place psychological theory in relation to the biological sciences on the one hand and to linguistics on the other. In the course of doing this I shall also give a particular view about the nature of information processing models.

Let me start by indicating the magnitude of the enterprise. Figure 1 illustrates the model with which I currently work. The boxes, in general, represent either a process that converts information from one code to another or a store of information of some kind. The directed lines indicate the more important or more usual passage of information. The model appears to be a minimum structure for talking about word recognition in two modalities, object recognition, speaking and writing single words, together with some concern for larger units of language and for phenomena of memory for language materials over short time intervals. Virtually all the components of the model are supported by a variety of observations from different laboratories. The labels on the processes have been omitted since their mode of functioning is not self-evident. The model becomes more complex when the details of the component processes are spelled out. In addition there appears to be evidence that the interconnections are actually richer than those shown. Also there is evidence that the instructions given to a subject in an experiment, or the strategy he adopts spontaneously, can affect the mode of function of the system. Thus, one of the routes can dominate under certain conditions and another under other conditions.

One example of this will suffice. It concerns performance in the Lexical Decision task. In this task, subjects are presented with a sequence of strings of letters and have to decide whether or not each one corresponds to an English word. This technique has proved useful in exploring the storage and retrieval of our vocabulary. Davelaar *et al.* (1978) performed an experiment in which the crucial measure was the time that it took to respond affirmatively to a word like KERNEL which had a homophone (COLONEL) of higher frequency. This was done under two conditions. In one of these, the non-words used, such as GRONE, would sound like real words if they were pronounced. In the other condition the non-words, like SLINT, did not have this property. The data showed that in the latter condition the response times were longer for the homophones than for control words. This indicated that some phonological coding was being used in the task. When the non-words sounded like real words, the subjects performed the task entirely graphically and there was no difference between the time taken to respond to homophones and control words. Such data show that the system we are trying to describe can change its effective configuration.

In addition to this we have the problem that we cannot possibly represent everything we want to do in the same format. The format in the figure has the particular advantage that the

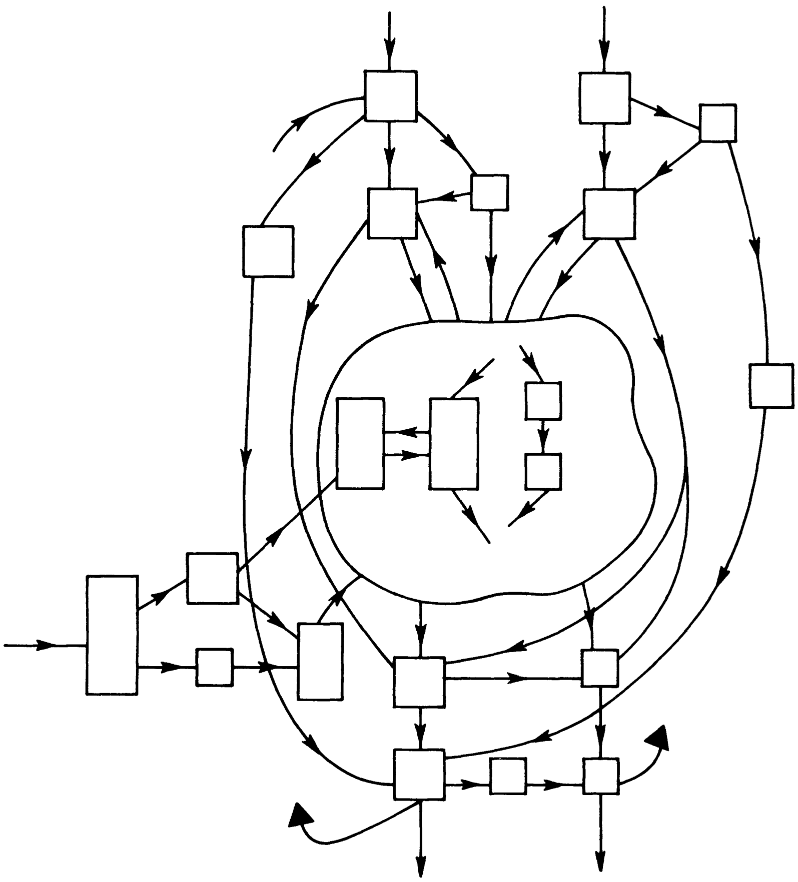

FIGURE 1. Current extent of the information processing model used by the author to discuss the processing of words and pictures. At the top are processes concerned with the analysis of speech and written material, at the left are processes concerned with object recognition and at the bottom the processes concerned with spoken and written responses.

interconnections can be kept track of. The same relations could, of course, be represented in a verbal or algebraic notation. Each has its advantages and limitations. Furthermore the model shown does not accurately represent the order of events in, say, the recognition of a word. It must not be inferred that all the processing in one component is completed before an output is passed to the next process. The format encourages such a belief, perhaps, and is often interpreted in that way. A different format would be needed to discuss such problems in detail, and yet another if we were to talk about conscious decision making. All of this complexity we are slowly coming to terms with.

One of the operating assumptions of this kind of model is the modularity principle. This means that it is assumed that the components of the model function independently. Thus the decision as to how any one component operates has no bearing either on the decision as to how any other component functions or on the connectivity of the components. Marr (1976) provides a general discussion of the principle. Attempts to falsify the model, then, have one of two forms. Either the mode of operation of a component or the relations between the components can be challenged. I have succeeded in the latter myself by showing that a postulated component did not react the way it should have done to inputs from different places. The consequence was that the model had to be changed and two further components added (Morton 1979). A good example of the former kind of challenge is found in a paper by Marslen-Wilson & Welch (1978). They provided evidence indicating that the system responsible for recognizing spoken words did not function in the way I had previously assumed. Note that this does not affect the overall configuration nor does it bear directly on the mode of functioning of any of the other components. I have used two examples relevant to my own work for the usual reasons, but many other examples could be found to show that information processing theory behaves in the same way as theory in other sciences.

Note that the format itself cannot be falsified. It could be shown that it is less useful than some other notation in answering particular questions, or that it is less effective in communicating ideas or that it biases one towards a particular way of thinking, but not that it is wrong. Of course there are a number of alternative models available (see, for example, Forster 1978; P. J. Barnard, unpublished 1980). The decisions between these alternatives depends simply on the psychological evidence. Nothing else is relevant. It is to this point that I now turn.

Psychological models and biology

There seems to be an important sense in which science is stratified. The strata of interest to us are the levels of psychological description and those of anatomy and physiology. There appear to be three separate kinds of activity in this region of science. Two of them are within the levels and the third has to do with the problem of mapping between the levels. The interrelations of these activities are of great interest. There are two general views: one is that the form of psychological theory is dependent on the biological facts; the other is that psychological and physiological descriptions are independent of each other. Putnam (1980) has put forward such an anti-reductionist position recently in quite general terms. My own feeling is that so far as linguistic and other cognitive processes are concerned there is no dependent relation between the levels. In its most extreme form, that is, I would claim that there is no piece of physiological or anatomical data that could verify or falsify a purely psychological model.

There are at least two reasons for claiming the independence of psychology from biology. The first resides in the difference in scale between the observations of the two disciplines and, therefore, in the descriptive elements used. Thus, there appears to be no currently available information processing model in which the smallest elements that the data require one to specify are established as being at the level of the neuron. There is no model for which it would be possible to say that it could not function in the way described because, for example, the synapse does not work in the required way. Paradoxically, perhaps, possible counterexamples to this are instructive. Hull (1943), Hebb (1949) and the Gestalt psychologists (e.g. Koffka 1935) all fell foul of physiological observations. But the reason that they did so was because they mixed their purely psychological models with an attempt to map the psychology onto the brain; that is, they tried to do two things at once. It was the inadequacy of the physiology that caused the downfall of the psychology.

The second reason for doubting the possibility of constraints from biology to psychology lies in the mapping relations between the two. In that there is a good correspondence between the elements of a psychological model and the anatomy, then one might suppose that anatomical facts could inform the psychological theory. But this history of the study of the breakdown of language and other cognitive function must lead one to believe that these mapping relations are not only complex but variable. Towards the end of the nineteenth century a number of people tried to construct information processing models of brain function, largely using data from patients with brain damage. Wernicke (1874) and Lichtheim (1885) are, perhaps, the best examples of this. This trend came to an untimely end as patients were discovered, with well localized lesions, who did not behave as the 'diagram makers' (to use Henry Head's (1926) contemptuous phrase) could have demanded. If only these model builders had not assumed that function, to be describable, had to be precisely locatable, we might have been saved a lot of trouble. Lichtheim was an exception, but his influence was swamped by the need of his contemporaries to pinpoint functional units down to the nearest convolution in the cortex. I see no reason for retracing that particular path of history. There are exceptions to these strictures, perhaps, in the sensory and motor parts of the brain, where the psychological units may well correspond to the anatomical ones. But we should not assume that even this is so. Thus, feature-based theories of visual pattern recognition (see, for example, Sutherland 1968) appear to get support from the finding of complex cells with particular propeties (see, for example, Hubel & Weisel 1962, 1965, 1968). However, if the whole of the physiological work were shown to be artefactual the status of the psychological theory would not be changed since it is based on sound psychological data.

The limits of this argument have been examined by J. Mehler & J. Morton (unpublished, 1981), who have attempted to specify the conditions under which biological data might influence a psychological theory. Their conclusion is that such interaction will only be possible under rather restricted conditions. In addition, however, I must introduce some qualifications. The first is that the activity of mapping psychology onto biology, while exciting, can be no better than the psychology and the biology that go into it. Such activity is neither inferior nor superior to the other two, it is merely different.

Secondly, it should be noted that this suggestion of the independence of the two levels should not be taken as a proposal that data from brain-damaged patients should not be used. For most cognitive psychologists, in this country at any rate, specific defects from such patients are a valuable source of data (see Coltheart *et al.* 1980). The point is that it is of no concern to the form of any of the models as to exactly where the lesion might be located, or how big it is. The apparent exceptions are where there is an attempt towards a gross localization of the functional units in the model; that is, a move towards solving the mapping problems (see, for example, Coltheart 1980; Shallice 1981; Warrington & Taylor 1978). In general, the data from patients which is of interest is behavioural data, and the models used to describe the underlying brain function remain psychological models.

Finally, let me meet one possible argument, which was put forward by Chomsky in discussion at a recent meeting where some of these issues had been raised. He suggested that there might come a time when two alternative models were being proposed that accounted equally well for the relevant data. In this case, he suggested, it would seem rational to prefer the model that more closely fitted the available biological facts. My reply to that argument is that it does not seem to me that psychological models have the form that would permit them to be compared in this way.

Psychological studies of morphology

The relation between psychology and linguistics has an equally muddy history. The relation between the two, even in the area of grammatical function, which has received a great deal of psychological attention, is still not agreed. In this final section the topic will be morphology, where the problems on the side of the linguists are less severe. Morphology concerns the structure of words. Linguists have defined the morpheme variously as the minimal unit of meaning or as the minimum unit of syntactical analysis. In most cases these definitions agree with each other. Thus, there would be agreement that the word *dogs* can be split into two parts, the 'root' or 'base' morpheme *dog* and the inflection or 'bound morpheme' *-s*. Equally, the word *talking* can be split into *talk* and *-ing* without provoking much debate. There are, of course, many problem cases and these have been solved in different ways by different writers. Our current concern is to establish the stages of processing at which there is evidence for some or any of the divisions that have been proposed.

One of the techniques that have been used to study the way morphological factors enter into the psychological models is that of the facilitation of recognition of one word after the presentation of another, related word. In the first set of experiments I shall describe, there is an interval of between about 10 and 45 min between the two events. This time interval, together with certain conditions of the experiments, seems to minimize the effects of memory in this task – memory, that is, defined as conscious retrieval of the earlier events. The effects to be described appear rather to be automatic consequences of the nature of the processing systems. In one such experiment, Murrell & Morton (1974) showed their subjects one of the words SEES, SEEN or SEED, together with a number of other words. (In this section, SEE will refer to visual stimulation, 'see' to acoustic, and *see* to the general linguistic case.) Later on, the subjects were shown the word SEES in a tachistoscope, a device that only exposes the word for a short period: something of the order of 20 ms. The subjects who had previously been shown SEES performed best, but there was also substantial facilitation for the group which had been shown SEEN. This effect could not have been simply due to the physical resemblance between SEEN and SEES, because the group that had been shown SEED, which is as close as SEEN is to SEES, from a physical point of view, performed no better than a control group, which had not been shown any relevant word. It seems, then, that this experimental technique taps a particular stage of processing at which there is equivalence between SEES and SEEN. In other words there seems to be evidence for a morphological representation at that level.

The question can now be asked as to the status of irregularly related words at that stage. The clearest answer comes from an experiment involving spoken words, which was carried out by S. T. Kempley & J. Morton (unpublished 1981). We were interested in the extent to which pairs of words like 'stink/stank', 'man/men' and 'bad/worse' affected each other in a paradigm similar to that just described. Subjects first listened to a set of words spoken clearly. We guaranteed that they paid attention to the words by requiring them to rate them for the extent to which they evoked mental images. Then the subjects listened to the test words, which were played in a background of noise. The test words were either the same as one of the pretraining words, were regularly related, such as 'looking/looked', irregularly related, such as 'stink/stank' or acoustically similar, such as 'looking/cooking' and 'stink/stint'. There were also test words, which had had no equivalent in the pretraining session. The responses were scored by the proportion of correct morphemes. We took the most generous interpretation of this; thus STANK would be counted as a correct response for 'stink'. The results are shown in table 1. For regular words, performance in the Derived group was not different from the Identical condition. That is, prior experience of 'looked' led to as much facilitation of recognition of 'looking' as did prior experience of 'looked' itself. The Similar group, however, did not differ from the control. Hearing 'cooking' in the pretraining session had no subsequent effect on the recognition of 'looking'. For the Irregular word group we again find strong facilitation in the Identical condition but absolutely no effects in the Derived condition. That is, hearing 'stank' or 'men' had no subsequent effect on the ease of recognition of 'stink' or 'man' respectively.

TABLE 1. MORPHEMIC SCORING IN KEMPLEY & MORTON'S STUDY

(Figures labelled with the same symbol were not significantly different from each other.)

	pre-test condition			
word group	Identical	Derived	Similar	Control
Regular	0.55†	0.51†	0.40‡	0.40‡
Irregular	0.46	0.36§	0.37§	0.37§

Further evidence as to the unit of processing at this stage of the model was provided by Osgood & Hoosain (1974). Basically they showed that familiar nominal compounds such as STOCK MARKET and TRAP DOOR behave in visual recognition as units rather than as pairs of words. The relevant experiment was one in which they showed that the presentation of these compounds in a pretraining session had no subsequent effect upon the recognition of the components. Thus, the fact that a subject had previously encountered STOCK MARKET had no effect on the recognition of MARKET. However, noun phrases such as STREET MARKET and STEEL DOOR did have an effect in the recognition phase of the experiment. A more comprehensive experiment was done with speech by Gipson (1981). He found no effect whatsoever of pretraining with a compound such as 'scarecrow' in the subsequent recognition of either of the components 'scare' or 'crow'. In another condition of the same experiment he found no effect of pretraining with the two components on the subsequent recognition of the compound.

It should be noted at this point that the resemblance between the results in audition and vision cannot be attributed to the operation of a process that is shared by the modalities. Gipson (1981) in the same set of experiments found absolutely no transfer from seeing a word to the subsequent recognition of the same word spoken in noise. Jackson & Morton (1981) found similar results, though their data were slightly less clear since they did find a little transfer from vision to audition. However, these authors found that the voice in which the pretraining words had been spoken made no difference in the recognition phase. This indicates that, although the effects are to be attributed to the auditory system, they are not acoustic. In parallel experiments, Clarke & Morton (1981) could find no effects of auditory pretraining on visual recognition. Again, the precise form of the stimulus did not matter since handwritten words had nearly as much effect as printed words on the recognition of printed words. These experiments were the ones referred to earlier, which forced the change from the earlier model to the one shown in figure 1.

Altogether, these experiments show that there exists a structural unit for which it is true that the physical similarity between words is irrelevant; that regularly related words are equivalent;

that irregularly related words are independent; that familiar nominal compounds act as units and are separate from their components, but that the components of less familiar nominal compounds are treated as separate units. These statements are substantially true for both vision and audition in spite of the fact that the two modalities are separate with respect to the stage in question.

There is one further finding of interest from the Kempley & Morton experiment on regular and irregular relations in speech. If the responses were scored by the correctness of the whole word rather than the morpheme, then for the Regular word group there is now a difference between the Identity condition and the Derived condition. This is in contrast with the data

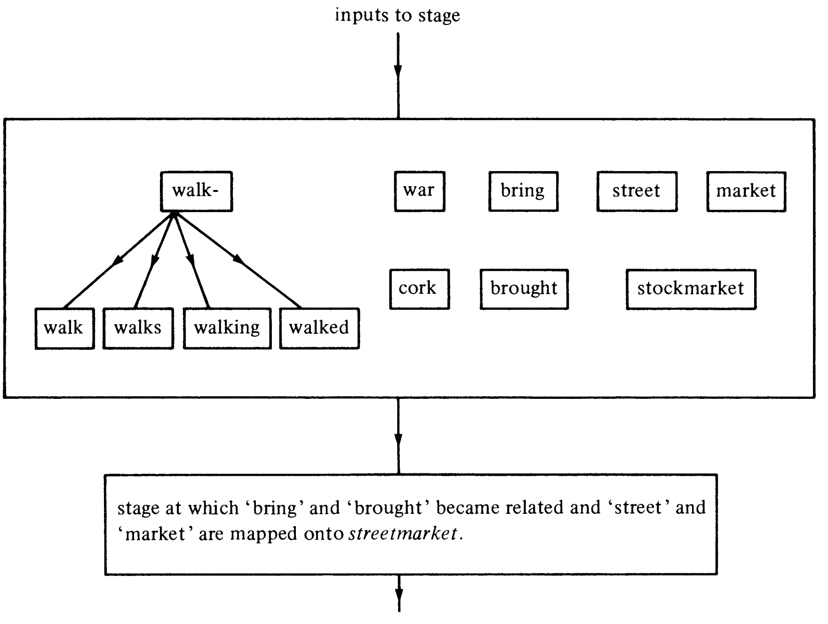

FIGURE 2. Some detail of the structure of the lexical categorization processes as determined by experiments on the facilitation of word recognition.

shown in table 1 where the two conditions were not separable in their effects. Thus there appears to be a specific facilitation for the bound morpheme in the context of the free morpheme. (We also tested the hypothesis that this result could be accounted for by some complex guessing strategy contingent on recognition of the free morpheme, but the data did not support such a proposal.) The units in question, then, appear not to be simple morpheme recognizers, but rather they must represent the morpheme plus the permitted inflections. It cannot be true that the free morpheme and the bound morpheme are separately recognized and simply recombined at some later stage in the system. We then have a picture of the units at the stage under consideration that looks like that shown in figure 2. Note that the facts represented in this figure are independent of the mode of operation of the recognition device. In different recognition systems – for example a bottom-up recognition tree (see, for example, Selfridge 1959) against an analysis-by-synthesis system (Halle & Stevens 1962) – the units would have different roles in the recognition process, but the relations between the units would remain the same: the data demand it.

The question of broader interest is the possible relation between the model-like description of the data given in figure 2, and linguistic theory. Would it be true, for example, that this set of data would lend support to any theory in which a unit, such as a morpheme, was defined that matched those in the figure? Would a linguistic theory be falsified that treated the *bring–brought* relation as equivalent to the *walk–walked* relation or which treated STOCK MARKET like STREET MARKET rather than like BREAKFAST? The answers to these questions must be 'no'. A linguistic theory is not restricted in such a way that it must focus on the same stages which are dissectible with the kinds of techniques used in experimental psychology. A linguistic theory which described the overall functioning of both stages shown in figure 2 is perfectly legitimate. Indeed, for the kinds of question that linguists often ask it might be necessary to encompass more stages still, or to ignore completely the more peripheral parts of our language system.

Of course, if it turned out that there are problems inside linguistic theory in the definition of terms, then the linguist might be able to turn to psychological theory to justify the existence of a number of different ways of treating the same material. These would correspond to the functionally separable parts of a psychological model. To split the lexicon linguistically would make sense psychologically. The dissociations in breakdown of language function pointed to by Allport & Funnell (symposium), Patterson (1981) and Shallice (1981) are perhaps the most persuasive line of evidence. But there is also a variety of seemingly discrepant results in the literature on the normal processing of words which can be resolved by appeal to multiple lexicons. These separate structures would be used differently in different tasks. In the kinds of experiment described above, where the amount of available stimulus material is restricted and the subjects are not encouraged to guess, we can expect to find results that reflect the structure of more peripheral units. If subjects are encouraged to guess, we could expect to see the influence of other kinds of knowledge. With different tasks we should also expect to see different patterns of results.

Another task which has been used to investigate morphology is the Lexical Decision task. As noted above, in this task subjects have to decide whether a string of letters is a real word or not. Using this technique, Taft & Forster (1975) showed that stems such as -SIST, as in RESIST, had psychological reality. Their data were that the time taken to reject non-words such as BESIST was longer than the time taken for a non-word like BESCUE. In the latter case, the original word from which the non-word was derived for the experiment, RESCUE, would not be split into two morphemes by any analysis. Taft & Forster concluded that some part of the processing system had recognized -SIST as a meaningful element and this had delayed the negative response. Stanners *et al.* (1979) also found evidence, by using the same task, that regularly inflected words shared a representation, but irregular past tenses and derived words did not. Taft (1979) later presented evidence that whole words also played a role in this task, separate from the role of the morphemic constituents. He concluded that the data could best be accounted for in terms of the involvement of two lexicons in the task. In the peripheral lexicon we find effects of morphological separation, but in the central one we have words represented in a unitary way. The fact that Manelis & Tharp (1977) and Rubin *et al.* (1979) could find no evidence for segmentation in the same task can be accounted for in terms of differences in experimental procedures, which minimized the role of the peripheral lexicon through their subjects' adopting particular strategies. It is at moments like this that the need for complex models becomes evident.

The picture becomes more complex when we consider output processes. MacKay (1978) presented his subjects with infinitive verb forms. The subjects had to respond as quickly as possible to this spoken stimulus with the derived noun form. MacKay found that derivatives with a relatively simple phonological relation with the stem verb (*conclude–conclusion*) were more rapidly produced than those where the relation was more complex (*decide–decision*). He concluded that the nouns were produced by the application of a complex set of rules from the single verb representation. The alternative hypothesis would be that the verb forms and derived noun forms were separately represented. After all, this is what Stanners *et al.* (1979) found in the study already mentioned. If this had been the case for the output processes, however, we would not expect the differences reported by MacKay to be obtained.

At the moment, then, we are left with a composite model in which an input stimulus word is first segmented into its component morphemes (for regular relations, that is) and then recombined in the central part of the lexicon. On output we seem to have an initial state of morphological separation with derived words being produced by the application of a set of rules. There are ways of encompassing all of this in a processing model without too much difficulty, as long as one maintains a number of lexicons and allows the operation of different strategies by the subjects to change the mode of processing. Perhaps this might encourage linguists to look at the advantages of multiple theories. Which linguistic theory one uses would then depend on the question being asked.

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