Word Recognition

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The question of how we recognize words is one which has been studied by a large number of psychologists. More data has been collected in this area than in any other area of psycholinguistics. In addition the alternative theoretical positions have been better stated than those concerned with, say, meaning, or language acquisition. Indeed, in some cases the issues are so clear that it is possible to test them quantitatively. In this monograph I will discuss the effects of context and of word frequency on the recognition of words, and will present a highly biased account of the kinds of models which have been proposed to account for these and other phenomena. We start with the effects of context since this is the way I became interested in word recognition and because it is closer to my own ultimate aim of explaining how we read and how we understand speech*.

THE INTRODUCTION OF A MODEL

Over the last 15 years I have developed a particular model of the processes involved in word recognition and a variety of other situations (Morton, 1961, 1964a, 1964b, 1968a, 1968b, 1969a, 1969b, 1970, 1977). This model makes it easier to relate together a large number of experimental findings and so may be regarded as a useful expository device. I do not believe it is 'true' in any interesting sense of the word and it is certainly not unique, having many roots in the past and a number of living relatives. Its power is to take a set of confusing-looking data and show that it makes sense if looked at in a certain way, and some apparently conflicting results are easily reconciled when viewed in terms of this model. Its relationship to actual neural and chemical activity in the brain is obscure, but in a sense irrelevant, inasmuch as if the model accounts for data and generates further understanding, it fulfils its purpose as a psychological model. In any case, the *functions* described by the model

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could be equally well implemented by a number of structures and if any of these functions turn out to be necessary constructs it will be someone else's responsibility to find the neural substrate.

The effects of context

The starting point for the model was my observation (Morton, 1961, 1964a) that it is easier to recognize a word in a context than in isolation. To prepare the materials for the experiment I presented 100 subjects with 55 incomplete sentences. The responses for each sentence were summed for each word, the frequencies of occurrence of the responses being used as an estimate of their probabilities in that context. Examples of the configurations obtained are given in Table 1.

Table 1

EXAMPLES OF RESPONSE CONFIGURATIONS

1. *Sentence:* The cup was placed on the ——.

Responses:

table	67	drainer	1
saucer	16	chair	1
sideboard	6	chest	1
shelf	4	cupboard	1
mantlepiece	3		

2. *Sentence:* They went to see the new ——.

Responses:

film	22	baby	2
house	20	bedroom	1
show	14	hospital	1
play	12	exhibition	1
car	6	bungalow	1
building	6	curtains	1
picture	3	calf	1
moon	3	child	1
flat	2	toys	1
shop	2		

Using the data from these systematic observations I put together sets of 24 words and context sentences, which were then used in the experiment proper. The words were flashed on a screen and subjects attempted to recognize them. Sometimes the words were shown in isolation, in other cases the subjects saw one of the context sentences before the stimulus-word was presented.

For each word there was one sentence for which it was a high frequency response, and one for which it was a low frequency response. Thus the incomplete sentence

They went to see the new ——.

often led to the response 'film'. The word 'picture' occurred much less often. With the sentence

They looked intently at the ——.

this order was reversed with 'picture' being more commonly given and 'film' less so.

The visual duration threshold for the words—that is the length of time for which the word had to be displayed for it to be recognized—was lower when the words were preceded by a context sentence. In addition I found that the threshold was a function of the probability of the word in the context. Thus 'film' was seen more easily with the first of the above sentences as context than with the second sentence; for 'picture' the reverse was true. This result was not surprising; it merely demonstrated on individual words presented visually an effect already observed with speech. Earlier, Miller, Heise and Lichten (1951) and O'Neill (1957) both found that words presented auditorily in noise were more readily recognized when presented in a sentence than in isolation. Bruce (1958) played to his subjects sentences, such as 'I hope our team will win the cup this year', in noise. He showed that if the sentence was introduced by a statement of the topic, 'sport' in the example, then more words were recognized than if no clue was given. A misleading description, such as 'food', led to worse performance.

The general conclusion, supported by a number of other experiments, (Tulving and Gold, 1963; Tulving, Mandler and Baumal, 1964) is that the ease of recognition is a function of the probability of the stimulus. This is only a description of the data, however, and is in no sense an explanation. In my own experiment the probabilities were obtained by presenting subjects with the same incomplete sentences as those used with the experimental group. The only difference between the situations was that the experimental group also had some stimulus information, and instead of using the term 'probability' in the account of the data it seems to make more sense to relate the two forms of behaviour, which we can call *recognition* and *generation*, by reference to a single, internal process. We can then account for the data by saying that words which are more likely to occur as responses in a generation situation will be easier to see (or hear) in a recognition situation. (This statement ignores for the moment that some words are intrinsically easier to recognize than others.) This quite general statement would lead us to expect, for example, that word recognition would be facilitated by showing subjects words which were associated with the stimulus word (Taylor, 1956). Equally much of the classic work on the effects of 'set' on word recognition can be accounted for at this level. For example, Postman and Bruner (1949) found that subjects given a set for colour showed lower tachistoscopic thresholds for colour words than for neutral words. Other examples can be found in Vernon (1962) and Haber (1966). The relation between behaviour in the two situations is most easily explained by suggesting that the processes operating in the generation situation also operate in the recognition situation. These processes will not be discussed at length; it is sufficient to regard them as providing semantic or syntactic cues which affect the production of a response.

The logogen system

If you see or hear the word 'table', or if you are asked to free associate to 'chair', or to complete the sentence 'He put the plate on the ——.', or to understand what is meant by 'a piece of furniture with a flat top, usually wooden and with four legs, commonly used for putting things on for the purpose of eating', or if you are shown a variety of objects, photographs or drawings, the same response is available to you. The nature of this available response is uncertain, but since it is in some sense ready to be spoken, it is reasonable to suppose that it is in some articulatory code (as opposed to a semantic, visual or auditory code). It is also reasonable and economical to suppose that the availability of the response is brought about in the same way regardless of the source of the information which led to it. The processes involved in making a particular word available I have called a *logogen,* (from *logos*—word and genus—birth). A logogen is not a word; it is the device which makes a word available as a response and it does so by collecting evidence that the word is present as a stimulus, appropriate as a response, or both. We will be discussing later the precise nature of the linguistic units involved. For the moment 'word' is being used loosely.

The evidence for the presence of a particular word can come from the outside world, by vision or hearing, or from other processes in the brain such as those concerned with context, which we can globally term the Cognitive System. Diagrammatically then we can show the logogen system in relation to other processes as in Figure 1.

The precise nature of the evidence is not important at this moment; we may imagine that visual evidence will include < five-letter word >[1], < initial ascending letter >, < central pair of ascen-

[1] Items enclosed in angular brackets, < . . . >, are merely verbal descriptions of events related to what is described. Thus < five-letter word > might represent a single impulse down a particular nerve fibre, or one particular bit of information in a general-purpose recoding device.

ders >, < final *e* >, etc.; acoustic evidence might be in terms of features, phonemes or syllables such as < initial voiceless >, < final /1/ > and < /t I/ >; semantic evidence would include < noun >, < artifact >, etc.

When a logogen has collected sufficient evidence, the appropriate

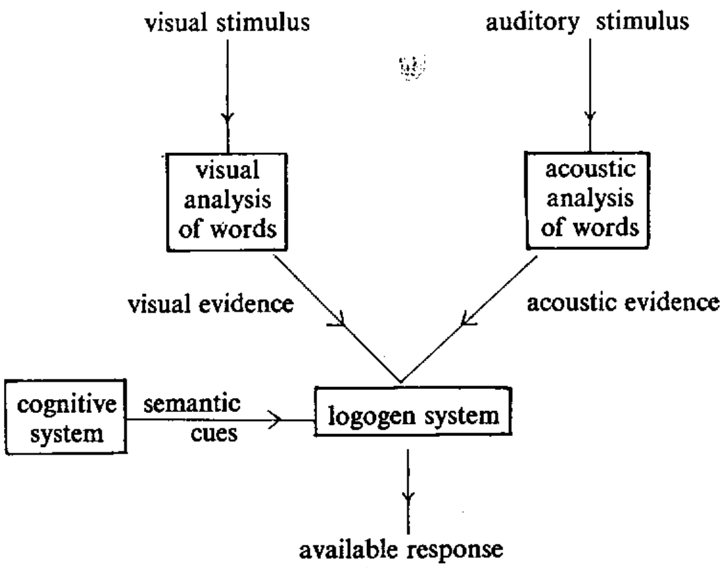


Fig. 1
Simplified version of the logogen model.

response is made available. The amount of evidence necessary for this is called the *threshold* of the logogen. Those who like metaphors based on energy can imagine inputs to the logogen system increasing the levels of activation in particular logogens; when the level exceeds a certain amount in any logogen the response is triggered.

When a word is presented in isolation a certain amount of evidence will have to be provided by the sensory analysis systems before a response is produced. Thus the handwritten word

although imperfectly written would be intelligible to most people. With a context such as 'He put the cup on the ——.' some evidence or cues concerning the nature of the word come to the logogen system from the cognitive system. Thus, according to the model, less evidence would be necessary from the visual analysis to make the response available. In this way

would be perfectly acceptable. With a more precise context such as: 'Before sitting down to dinner she laid the ——.' even the scrawl

would be instantly readable. Note that in the last case it is not possible to identify any of the individual letters with confidence. There might be an initial 'H', a 'd' in the middle and a 'b' at the end. All we have to go on are the length and general shape of the word, a possible cross on the first letter and certain loops. This analysis is not the same as saying that you need to identify fewer *letters* when there is more context. The process is more subtle than that and, I believe, relies on cues below the letter level. Indeed, I think it possible to claim that we can read in context words written in such a way that no letters can be identified. Furthermore this reading can be fluent.

This kind of analysis seems to represent accurately the way in which we can understand rapid speech. It also represents the phenomenological aspects of laboratory studies. Subjects who recognize a word in a tachistoscope often report that they see words exposed for a short duration in a context just as clearly as they see a word without a context at a longer duration. This is not always the case—sometimes subjects report guessing in just the same way as the occasional written word has to be puzzled over at length. The interesting phenomenon, however, is the instantaneous apprehension of a reduced stimulus such as the handwritten words above, without awareness of any intervening processes. For a dramatic demonstration of this I can recommend listening to the record of some suitable song (pop or other), in which the words are difficult to discern, first with no assistance and then with a copy of the words in front of you. The phenomenon is unmistakable: what was previously unintelligible will become perfectly clear, you will *hear* the words.

To recap: I have suggested that context operates in the same way whether we are generating a word or recognizing a stimulus and that the presence of a context means that less information is required from the stimulus for the word to be recognized. Such a proposal seems to accord with our everyday experience. It should be remembered that at the moment we are using 'context' in a very general sense and intend that probability measures should be regarded as no more than a summary of very complex processes. In some of what follows we will assume that a context operates purely linguistically to increase the likelihood of responding with certain words, using only our knowledge of syntax and semantics. I would not, however, want to rule out the case where you see the incomplete sentence 'He put the cup on the ——.', visualize the situation and derive the response 'table' from the visual image.

The effects of context in speech perception are well illustrated by the 'phonemic restoration effect' demonstrated by Warren (1970) and Warren and Obusek (1971). The experiments involved presenting subjects with a sentence in which, for example, the first /s/ or the /dʒIs/ in 'legislatives' was replaced by a 'cough'. Subjects reported hearing the missing phonemes as clearly as if they were actually present, and as a rule located the cough several phonemes away from its actual site—usually earlier. The effect persisted even if the original word was mispronounced (e.g. 'commitating' for communicating') so that the surrounding phonemes 'could not provide acoustic cues to aid phonemic restoration' (Warren and Sherman, 1974). It should be noted though that in the latter experiment the phonemes actually pronounced were still sufficient to restrict the choice of one word only. It remains to be seen whether the same illusion can persist in face of genuine lexical indeterminacy. In fact I would expect it to persist since there is no negative information from the stimulus. One experiment with negative information was run by Cole (1973). His subjects were asked to detect mispronunciations in a passage of prose. The mispronunciations involved single phonemes in trisyllabic words, which differed from the original by 1, 2 or 4 features (e.g. the /b/ in 'busily' replaced by 'p' 'v' or 's'). Detections were a function of the size of the alteration, single feature changes rarely being detected in the prose. When the words were presented in isolation though, all errors were detected. Thus one effect of the context is effectively to suppress negative information.

The model used here has the advantage of avoiding both 'guessing' explanations of word recognition and mystification about the relation between the stimulus and some internal event which we describe as 'seeing', 'hearing' or 'perception'. In the present terms 'seeing a word' is associated with a response becoming available together with an indication that the visual analysis system has provided some (unspecified) evidence towards this event. 'Hearing a word' differs only in that the additional information comes from the auditory analysis system. Within this framework it is easy to accept that Joan of Arc really did *hear* her voices. All that was the matter was that an erroneous signal from her auditory recognition system led her to mistake evidence from within for evidence from the senses.

Such an account of 'perception' is clearly incomplete. For example I make no attempt to explain the *mechanism* of perception, only one of the prerequisites. However the account has the virtue of making it clear that 'perception' does not depend on the stimulus alone. This observation is not new. Gombrich (1960) has discussed the way in which artists capitalize in their paintings on the interaction of the viewer's knowledge and experience with what is on the canvas, and Gregory (1967) has made the same point at length about visual perception, especially with reference to visual illusions.

Quantitative account of the interaction between stimulus and context

Up to now we have discussed the effects of a context on word recognition in purely qualitative terms. It is possible however to use the logogen model quantitatively to predict such contextual effects since it makes the precise claim that evidence from stimulus and context add together in determining the response. The basis for the prediction is relatively simple. First of all we assume that there is activity in the entire system which is unrelated to the prescribed task. Such activity could be brought about by the subject looking round the room, thinking about the experiment, his next meal, or the day's news, and by unconscious thinking. Such behaviour would have the effect of adding a random element to the activity of individual logogens. Such a postulate is required in any case to explain why people do not always give the most common response to a sentence. If they are asked which word they think is the *most common* completion a very high proportion of subjects guess correctly. In the typical generation task however they are asked to give the *first* word that occurs to them and this latter task would clearly be more subject to random interference than the other. Equally, when a stimulus is presented the likelihood of a particular logogen responding is affected not only by the evidence, which may be presumed to be constant for a standard stimulus, but also by the moment-to-moment effects of the random variation. From this we can predict the probability of making the correct response with both stimulus and context, P_{sc}, from probability of being correct with the stimulus alone, P_s, and the probability of making the response with the context alone, P_c. The full derivation is given in Morton (1969a) and the resulting equation is

$$logit\ P_{sc} = logit\ P_s + logit\ P_c + constant,$$

where *logit P* is simply log [P/(1-P)]. The function 'logit' has no special significance; it just arises from the mathematics. To test the equation the data must be in a certain form, and I could only find one experiment which was suitable. This was carried out by Tulving, Mandler and Baumal (1964). They presented subjects with 18 words at successive duration, ranging from zero to 140 milliseconds. All the words were presented at the lowest duration, the subjects responding to each in turn. Then the words were all presented in a

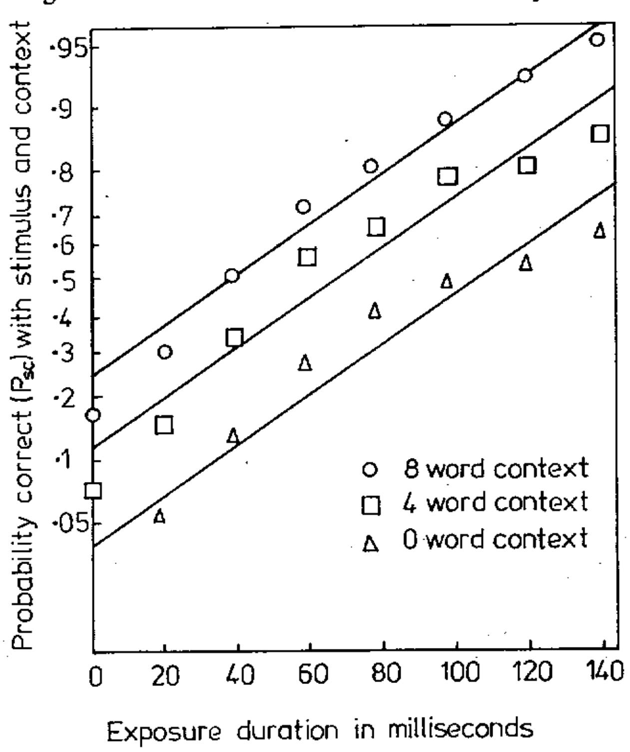


Fig. 2
Data from Tulving, Mandler and Baumal (1964) on the joint effects of stimulus and context. (Points are approximate.)

different order at the next duration, and so on up to the longest exposure. This procedure minimized the effects of successive attempts at the same stimulus which complicates the results of other experiments (including my own) for the present analysis. Tulving *et al.* proposed an empirically derived equation for their data:

$$\mathit{logit}\ P_{sc} = a_i + bx,$$

where a_i is a context depending on the level of the context, b is a constant and x is the duration of exposure of the stimulus-word. In their experiment the context was zero, four or eight words of a context sentence. Their data is plotted in Figure 2 with lines drawn to fit the equation they propose.

The prediction from the logogen model involves plotting this data according to the equation

$$\mathit{logit}\ P_{sc} = \mathit{logit}\ P_s + \text{constant}$$

from which we expect the data for each level of context to fall on a straight line with a slope of 45°. This plot is given in Figure 3. The straight lines are lines of the required slope. It will be seen that the equation approximated the data very well except for the two highest exposure durations at the highest level of context. These discrepancies will be discussed later. The important point about this result was not just that we find a straight line, but that we find two straight lines of the predicted slope. What this means is that the amount of stimulus information available at a given exposure duration is indeed independent of the contextual evidence.

The locus of the interaction

The model described above specifically proposes that stimulus and context information combine directly to produce a response. In this it contrasts with, for example, a model in which first of all the available stimulus information is used exhaustively to produce a list of candidate words and then the context is used to bias the choice among this list. These two broad groups of models were tested by Meyer, Schvaneveldt and Ruddy (1974) using an adaptation of the lexical decision task. This task involves measuring the time taken for subjects to decide whether a string of letters constitutes an English word, and in Meyer *et al.*'s variation, subjects made a string of decisions on items with specific interrelations. Earlier work had shown that a subject processes a word like 'doctor' more quickly after processing a semantically associated word like 'nurse' than after an unrelated word like 'bread' (Meyer and Schvaneveldt, 1971; Schvaneveldt and Meyer, 1973). By itself such a finding would be consistent with a variety of models. Meyer *et al.* (1974), however, introduced a second factor, that of the legibility of the items. They argued that if there was a two-stage process then the effects of context and of stimulus clarity should be additive to the decision time. Their results showed quite clearly, however, that the effects of context were increased if the stimuli were made less legible. This is

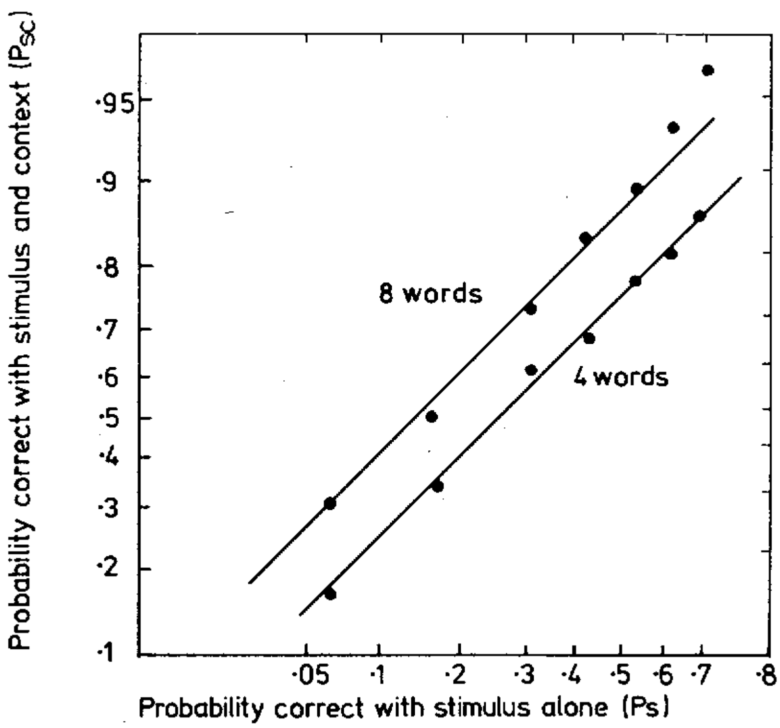


Fig. 3
Data from Tulving *et al.* (1964). Logit P_{sc} is plotted against Logit P_s. Lines of the predicted slope were fitted by eye.
(from Morton, 1969a)

inconsistent with a two-stage model and supports that class of models where the two kinds of information combine directly.

ALTERNATIVE MODELS FOR WORD RECOGNITION

Other alternative accounts of word recognition may be divided into two classes: guessing models and analysis-by-synthesis models, both of which can exist in one-stage versions.

Guessing models

The proponents of guessing models have concentrated on the recognition of words in isolation. The outstanding feature of words in isolation is that common words are recognized at lower durations more easily than rare words. This phenomenon will be discussed extensively below; it is sufficient for the moment to say that according to guessing theory this result is to some extent or other an artifact of guessing habits. It states that there are no basic differences in the ease of seeing common and rare words. In this report it doesn't differ from the logogen or similar models. For a particular stimulus duration, however, there will be a number of occasions on which the word will not be seen. On these occasions the subject guesses. These guesses will be influenced by word frequency and so when the stimulus is high frequency the subject will more often guess correctly, leading to an apparently lower threshold for these words. The effect of a context sentence would presumably be to restrict the guessing in the proportion of the responses in the generation task. The correct responses, by such a model, would be made up in two ways. On a proportion of the trial, P_s, the stimulus would be seen correctly; on those occasions where the stimulus was not seen the subject would guess correctly with a probability given by P_c. The resulting equation is then:

$$P_{sc} = P_s + (1 - P_s) \,.\, P_c.$$

Tulving *et al*, tested their data against this equation and found that the subjects always performed better with stimulus and context than would be expected. It should be noted that this model requires that all incorrect responses result from guessing. The subject can never *see* the wrong word. This is a very strong, and patently absurd, assumption. To start with subjects are often as confident about their incorrect responses as they are about their correct responses. Indeed, in my own experiment, some subjects who made an incorrect response of a more probable word than the stimulus-word accused me of having changed the stimulus in the middle of the run. Secondly, it is easy to demonstrate that incorrect responses are a function of the stimulus as well as the context. If I presented a subject with the sentence

Through the window she saw the ——.

and then flashed the word *hippopotamus*, we can be quite sure that he will not respond with 'sky', a high frequency response in the generation task. The length of the stimulus-word alone will be enough to prevent that.

Sophisticated guessing.

One rational way of modifying the guessing model involves accepting that the subject can make use of partial information from the stimulus. If he does not see the word correctly his guesses are constrained not only by the context but also by the stimulus information. Suppose the sentence was

They went to see the new ——.

and the stimulus-word was *film*. If the subject saw the letter *f* clearly his guesses would be restricted to 'film' and 'flat' in Table 1 and some other words which didn't get into Table 1 but are clearly possible, if unlikely, such as 'file', 'fete', 'folk'. In this way the probability of being correct would be much greater than with the simple guessing model. Unfortunately no one has produced data sufficiently detailed to allow the mathematical implications of this model to be tested with regard to the effects of context, although we shall see that it does not give the best account of the effects of word frequency. There are, however, implications of the theory which can be considered here.

To start with it implies that the information from the sensory analysis systems even if incomplete is always correct—an assumption not required by the logogen model. Now, erroneous responses in the absence of context are usually of the same length as the stimulus-word and have the same pattern of ascending and descending letters. The next most common similarity is the identity of the initial and final letters. Thus we can test the plausibility of the sophisticated guessing model by examining the errors made in the presence of a context sentence. For, by this theory we would expect these features to have equal influence in the presence of a context as without. The errors demonstrated the influence of the context in that they were nearly all of a higher frequency of occurrence in the context than the stimulus-words. What is of interest however is the extent to which they resemble the stimulus-words physically. Table 2 contains a list of such errors made by the subjects in the experiment described on p. 110.

Table 2

EXAMPLES OF RECOGNITION ERRORS INFLUENCED BY CONTEXT

(fom Morton 1964a)

Stimulus	*Response*	*Stimulus*	*Response*
dance	cinema, theatre (2)	birds	bells
coat	dress, hat (3)	shoes	coat
door	window	shelf	table (3)
cinema	station	film	book
saucer	table (2)	prize	race (3)

Now while we can imagine that the *birds-bells* error could b‹ accounted for on the sophisticated guessing theory as well as *shelf-table* and several others, it does not seem possible to see wha stimulus information is shared by *saucer* and *table* or *cinema* an‹ *station* especially when one remembers that the stimuli were presente‹ in lower case. If the subjects had been forced to make some wor‹ response on every trial, a procedure often followed by other experi menters, then one could just say that *no* evidence has been produce‹ by the analysis system in these particular cases. But, it should b emphasized that my subjects were explicitly instructed not to gues: but to report only what they saw and, as one might expect, ther were not many incorrect responses in the context conditions. W cannot claim, then, that error responses are chosen by guessing fro a subset of possible words. For reasons such as these, the sophist cated guessing model does not give a satisfactory account of all th data. This doesn't mean that it would not be an appropriate descrip tion of what happened on particular trials, for some subject: especially under different instructions. But that is not the issue.

Analysis-by-synthesis theories

These are usually restricted in their application to speech recognitio though Eden (1962) has used the same principles in a device t recognize handwritten words. The basic principle of analysis-b synthesis is that a particular word is selected internally on the bas of preliminary evidence and compared with the stimulus. If t internally generated pattern and the stimulus match, that respon is accepted as correct. Otherwise a further pattern is generat‹ internally (e.g. Halle and Stevens, 1962). Words of different pr‹ bability in a context would differ in that likely words would be tri‹ first. By the time an unlikely word was tried the memory of t stimulus would have faded somewhat and the possibility of corre identification would be reduced. Now it is not as easy to imagi how we generate printed words internally as it is to imagine how generate speech (but see the work of Posner, Boies, Eichelman a Taylor, 1969). However, such a model may well be useful und certain conditions. Thus we could possibly account for the d crepancy between data and prediction in Figure 3, by suggesti that with a strong context and after having seen the stimulus fi times previously, the subjects had a single firm hypothesis as to t identity of the stimulus and performed something like a compariso This theory cannot, however, account for the incorrect responses my experiment which did not make any sense in the context nor f the responses which were nonsense words or incomplete wor‹ Specific mathematical predictions from this theory also require m‹

detailed data than are currently available but we shall see that its account of the word-frequency effect is inadequate.

TWO KINDS OF RESPONDING

One distinction we can now establish is that between possible processes involved in leading a subject to respond with a word. The difference is based on the extent to which the subject employs processes of inference about which he can introspect reliably. The difference can be illustrated by considering what the following werd might be: *w-rd*. In the first case many of you will have read 'werd' in the preceding sentence as 'word' without noticing the mis-spelling. In the second case we have something like the crossword situation in which you think 'What letter will fit in place of the hyphen to make a word?'. Very few people would claim that even at first glance they would in any sense *see* w-rd as *word*; the acknowledgement that such is what was intended would be more usually described as inference. This distinction has been known for some time, and is relatively easy to demonstrate in experiments using a tachistoscope. Pillsbury (1897) showed that 'alert subjects, including professional psychologists'(!), would report 'forever' when presented with *foyever*. Sometimes they would report that something was wrong—for example commenting that there appeared to be hair across the 'r'—but often they would see nothing amiss even when the word they reported differed on two or three letters from the one presented to them. Pillsbury observed that for his subjects there was a difference between reading a word and reading the letters of that word. 'In many cases it was noticed that the letters which were most certain and of whose presence the subject is most confident were not on the slide, but were added subjectively.' (Pillsbury, 1897, p. 362). Even in those cases where something amiss was remarked upon, such remarks followed the response of the word. Vernon (1929) has made similar observations. It should be clear by now that I am not here particularly concerned with the inferential kind of response. It has some interest in the field of problem solving but although it is possible to induce such behaviour in experiments which purport to be concerned with word recognition, I do not think they are particularly relevant to understanding normal language behaviour. A further group of experiments I will largely ignore is that by Haber and Hershenson. These authors presented a given verbal stimulus to Ss repeatedly at a constant exposure well below threshold. Intervals between presentation were eight seconds or greater. Subjects were specifically instructed to report in terms of letters and not to guess. Stimulus clarity increased with repeated exposure even when the initial exposures appeared to be blanks (Haber and Hershenson, 1965). That there was some influence of language habits was shown by the greater clarity of English words compared with Turkish (Hershenson and Haber, 1965). Prior knowledge of the stimulus-word eliminated differences in subjective clarity between rare and common words (Haber, 1965) though, under the strict instructions used, the usual *word* recognition was not found, the growth of the percept being gradual. Later work showed the clarity of individual letters to be influenced by the position of eye fixation (Hershenson, 1969a, b). Such results cannot be interpreted at the level of the logogen and any complete integration of them with the other data would have to include an account of the processes of awareness. This is well beyond my current ambitions.

The role of evidence
We can also express some distinctions in terms of the status of the evidence sent to the logogen system. In the model, all pieces of evidence are sent to all relevant logogens; they do not, however, all have the same status. Let us take the content:

The cup was placed on the ——.

There are two pieces of evidence produced by this context: < noun > and < with flat surface >, which place *mandatory* restrictions on the response. (Seeing someone place a cup on a ball and responding with 'ball' in a subsequent generation task would be regarded as a special case.) By comparison, the attribute < furniture > merely defines a *possible* class. Words representing objects other than furniture, such as 'shelf' and 'drainer' are not excluded, whereas articles of furniture without a flat surface would be excluded. In listening to speech and in reading, and in most experimental situations, the evidence from the senses seems to be indicative rather than mandatory. I regard the sensory analysis system as fallible as well as limited in speed, and it is an essential part of the model that sensory evidence be so treated. Only in this way can we account for the more extreme misunderstandings, misreadings and mishearings.

Confusions concerning the source of evidence
From these comments, and from the way in which the model was formulated, it should be apparent that the logogen system is logically indifferent to the source of the information which enters it. This makes the system prone to error and is the source of the 'proof-reader's error' which I tried to illustrate with 'werd' on p. 123. Most readers will be able to draw from their own experience situations in

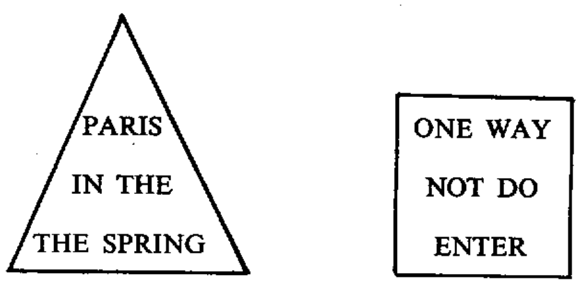
which someone was sure they heard you say a particular word which you are certain you did not utter but which corresponded to what the listener expected you to say.

It is quite easy to demonstrate the effect with speech. Even when clearly spoken in the absence of noise the following trick question works about 50% of the time. It is couched in the form of a riddle and should be read completely naturally.

> An aeroplane flying from Boston to Vancouver crashed exactly on the border of the United States and Canada. In what country would the survivors be buried?

About half the people who are asked this question seem to misunderstand the word 'survivors' and think in terms of *those who have been killed.* After a little time (not more than about half a minute), they can be asked the exact form of the question. Usually they recall it confidently and then after hearing themselves say 'survivors' realize their mistake. Thus it is clear that they have in one sense 'heard' the word correctly, since they can reconstruct its form from their memory. In another sense however they 'heard' a word which meant the opposite. This paradox presents no problems to the way in which we have discussed word recognition. In the first case we are talking about the acoustic form; in the second case it is the interpretation. Ordinary use of 'hear', 'see' and 'perceive' never make the distinction.

Two other examples follow; the first will be familiar to many of you, the second was first seen in a government establishment in the USA. Even knowing that there is a catch will not always prevent misperception.



A system which makes mistakes of the above kind does have a number of advantages. Basically it is a question of optimization: in general we have a fairly good idea of what the next word will be in speech or print, and it would be a waste of time to confirm the identity of each segment beyond all doubt. If a mistake is made in the identification of an individual word we can usually correct ourselves in the light of subsequent information. Only in cases where every single word might make a large difference (as in a poem or a legal document or in a foreign language) do we need to change our strategy. We speak slowly to foreigners and require them to speak slowly to us in their language so that more evidence can be obtained about each single word. When listening to our native language it would be a nuisance to be continually aware of the relative contribution of signal and context in the identification of each word. To blend the two kinds of evidence is thus efficient.

This kind of indifference to the source of information emerges as an interesting disadvantage in other psychological tasks. The Stroop test (Stroop, 1935) illustrates the point. In this test subjects are presented with a list of colour words printed in different colours of ink, as *red* printed in green, *green* printed in blue and so on. When asked to read the word or to name the colours they are very slow compared with reading the words printed in black or naming meaningless patches of colour. I have shown similar effects with a variety of stimuli (Morton, 1969b). In one task I asked subjects to count the number of digits in rows such as the following:

3333
2
66666
111
44

where the correct responses would be '4, 1, 5, 3, 2'. In another task the subjects were asked to say (in effect) whether the following words were on the right of the line, in the middle or on the left:

	left	
middle		
	right	
		left
left		
	left	
	middle	
right		left

Clearly it is difficult for us to keep two kinds of visual information separate. A response is produced, from one or other source, and we then have to check the response against the stimulus to confirm that it is the one we want. It is also easy to demonstrate a confusion between words which have been heard and words which have been internally produced. Subjects are told that they are going to hear a number of words; they are asked to free associate—to themselves—to each word. The list of words might run 'father, knife, blue, sister, grass, blood, spoon, . . .' and so on. The next step is to read out another list of words and ask the subjects to indicate whether or not they were in the original list. The second list includes all the original words, but in a different order, and in addition words which we can be confident would occur as associations for the majority of subjects such as 'mother', 'fork', 'sky', 'brother', 'green', and 'red'. In my (unpublished) demonstrations of this phenomena there is an average of about 25% errors. This is a different kind of experiment from the others in that it involves memory and not recognition, but the principle is the same.

WORD FREQUENCY

Having dealt with the effects of context and stimulus information in word recognition we will now consider the recognition of words in isolation. Some years ago it was shown that the ease of recognition of a word presented auditorily or visually was a function of its frequency of occurrence in the language (Soloman and Howes, 1951; Howes and Soloman, 1951). The estimates of word frequency for these studies, and many others since, were taken from a count of some eight million words organized by Thorndyke and Lorge (1944). The words were taken from popular journals, novels and so on, and while the count is very out of date for a number of words and is scarcely representative of spoken English, its utility is undoubted. Amazingly, Eriksen (1963), in a little known article, pointed out that the relationships originally found by Soloman and Howes were almost entirely due to the most common and most rare words. Erdelyi (1974) has recently pointed to and elaborated on Eriksen's point, and Doggett and Richards (1975) only began to find frequency effects with words above six letters long. In spite of these qualifications it is clear that the effect exists, and there is more clear data with acoustic presentation even with monosyllables (e.g. Brown and Rubenstein, 1961). In any case it has remained unclear as to whether the applicability of the word-frequency count to brain processes referred to the relative number of times we see (or hear) different words or to the relative number of times we write (or speak) them.

Frequency of experience or production

Howes (1954) advanced the opinion that experience in word production played the greater role in visual recognition. This view was supported by Daston (1957) who found that subjects were more successful in the visual recognition of words which they said frequently in therapy interviews than in the recognition of words which they did not use but were of equal frequency in the Thorndike-Lorge count. Contrary evidence was produced by Neisser (1954) and Ross, Yarczower and Williams (1956). These authors required their subjects to study a number of words for a while. A series of words was then presented to them in a tachistoscope and the thresholds were measured. These words included the ones previously seen and their homonyms. Both articles reported that the prior study of a word lowered its threshold but did not affect the threshold of a homonym. Thus, if the subjects had previously seen the words *frays* and *rough* these words had an abnormally low threshold but the thresholds of *phrase* and *ruff* would be unchanged. Neisser concluded:

> Since the same verbal response is employed in reporting the word itself, it appears that the effect of a set of this type is to facilitate recognition processes without generally facilitating the corresponding verbal response . . . (the set) does not merely raise the probabilities of certain verbal responses but rather facilitates the perception of specific visual patterns. (p. 402.)

It should be noted that the subjects in these experiments were showing an influence of recent experience of particular words rather than an influence of the overall frequency of occurrence of the words as measured by the Thorndike-Lorge count, and in fact, Neisser was concerned to reject 'response bias' explanations of word recognition (which we have met as 'guessing theory'), in favour of a 'perceptual' theory. The use of this data in the discussion of whether the word-frequency effect is a result of experience or production (e.g. by Rubenstein and Aborn, 1960) depends upon accepting that short-term and long-term influences have the same cause. This equation has been taken for granted in the literature. Indeed, a number of authors have mimicked the word-frequency effect with nonsense syllables. King-Ellison and Jenkins (1954) had their subjects read 100 disyllabic nonsense words in a pack of cards. The task was described to the subjects as being related to the pronunciation of Turkish words. In the pack were 10 crucial words which occurred 25, 10, 5, 2 times or once. After they had gone through the pack the subjects did 20 minutes of an unrelated task. Then the visual duration thresholds were determined for the 10 words the subjects had previously seen. These thresholds were linearly related to the logarithm of the frequency of occurrence of the words in the pack. Such experiments were regarded as a more exact replication of the earlier studies using English words since the frequencies of experience were precisely known for all subjects.

The trouble with Neisser's account of his own experiment is that while it does account for King-Ellison and Jenkins finding that frequency of seeing a word determines the word's threshold, it does not account for Daston's result, reported above, in which the frequency of *speaking* a word was related to its visual duration threshold. Nor can it account for the fact that pretraining subjects with spoken nonsense syllables affected the visual duration thresholds in just the same way as visual experience (Postman and Rosenweig, 1956; Forrest, 1957; Richards and Hempstead, 1973) and that pre-training with visual experience has a similar effect on auditory recognition (Weissman and Crockett, 1957). These results can no more be explained on the basis of the facilitation of specific sensory patterns than can the results with homonyms be accounted for on the basis of response bias.

The attempts which have been made to explain these phenomena have been rather bedevilled by appeals to a distinction, rather casually applied, between 'perception' and 'response'. In particular the issue has been clouded by a somewhat mischievous experiment by Goldiamond and Hawkins (1958) (I ascribe it so because it is difficult to imagine that the authors were serious—although some later writers have seemingly taken them seriously). What Goldiamond and Hawkins did was to give their subjects differential experience of a number of nonsense words. Then they parodied the normal frequency-threshold experiment by telling the subjects that they would see the words in a tachistoscope. One of the words was then taken as the 'correct' response but only meaningless smudges were shown to the subjects. As the duration of exposure was low, the subjects were never aware of this, and, as instructed, they responded with one of the words following every exposure. The number of trials to the 'correct' response was regarded as a measure of the 'threshold'—the more responses before the 'correct' one, the higher the 'threshold'. The result was that the normal frequency-threshold relationship was found; the reason for this being that the subjects responded more often with the words they had seen more frequently in the pre-training. Goldiamond and Hawkins concluded that since the full word-frequency effect emerged without any stimulus information at all it must act by altering response biases, and cannot be a 'perceptual' phenomenon.

It has been an easy step from the result just described to an explanation of the word-frequency effect based entirely on guessing. The argument runs roughly that words which have been experienced more often are more likely to be given as responses and so the coincidence of responses and stimulus is going to be greater with high frequency words. Guessing theory can give a qualitative account of some results, for example, the findings of Newbigging (1961a) and Savin (1963) that incorrect responses to a stimulus are generally more common than the stimulus itself. However, as we have already seen there are several fallacies in the theory, the most obvious ones being that performance is affected in lawful ways by the nature of the stimulus, and that performance is much too good to be accounted for in such a way (Savin, 1963; Broadbent, 1967; Morton, 1968b). There emerged instead a variant of guessing theory which has been called 'sophisticated guessing theory' or 'fragment theory', which we met on p. 121. This was first proposed by Soloman and Postman (1952) and elaborated by Newbigging (1961) and Savin (1963) among others, and applied to both the visual and acoustic recognition of words. By this account the subject sees only a fragment of the word. Faced with instructions to make a response of a complete word the subject has to guess. The fragment he has seen will be applicable to a number of words but the subject's response will tend to be that of the more frequent words of the possible alternatives. If the stimulus was in fact a low frequency word, the likelihood of the correct response being given from a small fragment will then be low.

By reducing the number of alternatives from among which a guess has to be made, it becomes, in Neisser's words 'statistically possible to create a significant bias towards common alternatives' (Neisser, 1967, p. 117). The theory seems reasonable from one point of view. It is quite clear that subjects sometimes see or hear part of the stimulus word and will say so if they are allowed to give partial responses. In addition the theory accounts for a number of results which the crude guessing theory cannot explain. In particular, Newbigging (1961a) has shown that the incorrect responses to a high frequency word contain fewer letters in common with the stimulus word than the incorrect responses to a low frequency word. The reason for this is that the more letters that are correct the greater the likelihood that the high frequency stimulus word will be the most common remaining in the set of permitted words. It also follows, and has been shown by Savin (1963) and Havens and Foote (1963), that a rare word will have a low threshold if there are no common words that resemble it and will have an especially high threshold if there are common words that are similar.

Neisser (1967) tried to use fragment theory to account for his earlier result with homonyms (see p. 128). He criticized his earlier conclusions where he rejected the response bias explanation, in that he took the term 'response' literally, to mean *verbal report*. He continued:

> The trouble with my experiment was that no one who speaks of 'response bias' has such a restricted definition in mind. They usually wish to include *inner* speech as well as spoken report in the category of "responses". From this point of view a subject who thinks to himself "It looked as if the first letter was S and the last was N. The only word on my list like that is SEEN, so that's what I'll report" has been subjected to a response bias . . . such a subject would not display a lowered threshold for homonyms: seeing SCE will *not* lead him to report SEEN. (p. 119)

The point of this restriction is that it changes the emphasis from external to internal events, which is no bad thing. It still has two drawbacks, however. In the first place, as we discussed above, subjects rarely report that such reasoning takes place even at the level of 'inner speech'. There is, for me, a clear distinction to be made between *reporting* a word and *seeing* it, however illusory the seeing might be. Secondly, Neisser glosses over the peculiar fact that in his example of 'inner speech' there is the *visual* representation *seen* rather than the phonological one /sin/ which could apply to *scene* as well as *seen*. This distinction means that we still have some explaining to do.

Fortunately we can decide on the validity of the sophisticated guessing model compared with other models on a quantitative basis. This will be the purpose of the next section.

Mathematical accounts of theories of the word-frequency effect

Many of the arguments put forward in the preceding pages against the various guessing theories have relied upon the general plausibility of different theories, on subjective reports, and on the general thesis that what we are interested in is the understanding of speech recognition and reading, for which the guessing models seem less attractive. None of these reasons is completely convincing to an adherent of a rejected theory. Fortunately it is relatively easy to collect data of a kind which enable the various theories to be tested mathematically. This was first done by Broadbent (1967) and Morton (1968b). The argument which follows must inevitably contain some mathematics. The equations are fairly simple, though, and follow easily from the descriptions.

The phenomena we are trying to explain is that with a less than perfect stimulus, the probability of the subject giving a correct response is a function of the frequency of occurrence of the stimulus. These probabilities are not the only data we obtain from the experiments. In particular we have information about the frequency of occurrence of the error responses made by the subjects. The tests of the models will use all this information.

Broadbent used data from his own experiment. In this he played lists of words to two sets of subjects. One group heard monosyllabic words and the other group heard disyllabic words. The lists were made up of 20 high frequency words (occurring at least 100 times per million in the Thorndike-Lorge tables) and 20 low frequency words (occurring between 10 and 49 times per million). The words were masked with noise so that the overall percentage of correct responses was about 20%. Subjects were asked to write down whatever word they thought they heard even if they were unsure about it. However they were not forced to respond and could leave blanks. The errors made by the subjects were classified by their frequency of occurrence, and a count was made of high and low frequency errors to high and low frequency stimuli separately. The data for the monosyllabic stimuli are given in Table 3. The definition of high and low frequency for the errors was the same as that for the stimuli and there were a large number of responses which did not fall into these classes as well as a number of blank responses.

Table 3

PERFORMANCE IN RECOGNITION OF MONOSYLLABIC NOUNS

(from Broadbent, 1967)

Stimulus	*% correct*	*% High frequency errors*	*% Low frequency errors*	*% Other errors & blanks*
High frequency	32.50	32.25	15.83	20.55
Low frequency	12.77	41.67	19.17	26.39

In my analysis I used data from Brown and Rubenstein (1961). These authors used 1300 monosyllabic content words 100 of which were drawn from each of 13 ranges of frequency of occurrence. These words were played in a random order to their subjects with masking noise. For each frequency interval they determined the number of correct responses and the number of incorrect responses in the same frequency interval as the stimulus. We can now see how the guessing models match up to the data.

Pure guessing In this model the subject hears a proportion of words correctly, this proportion being the same for high and low frequency words. On some or all of those occasions when he does not see the word he guesses. Since his guesses are more likely to be high frequency words than low frequency words he will be more likely to guess correctly when the stimulus was high frequency, leading to apparently better recognition of high frequency words. The theory thus implies that the apparent correct score, P_c, will be given by the equation:

$$P_c = P_s + (1 - P_s) \, . \, G$$

where P_s represents the proportion of times the stimulus is heard clearly, and G is the probability of an apparently correct response arising from a guess.

In Broadbent's data Table 3 we see that the subjects were correct 20% more often with high compared to low-frequency words. This was also true for disyllables. This difference, according to the simple guessing model, can only be accounted for by guessing habits; that is, the value of P_g must be at least 1/5 for all high frequency words. In other words, for any particular high frequency stimulus, the subject must guess correctly 1/5 of the time. However the sum of all the guessing probabilities has to equal unity. So only a maximum of five words could have a probability of 1/5 of being guessed. Since the high frequency advantage was true for 20 words in Broadbent's experiment (and ignoring high frequency words which were not on the list) the model cannot be true.

Sophisticated guessing In this model the subject hears a proportion of the words correctly, this proportion being the same for high and low frequency words. On those occasions when he does not see the word, he none the less gains sufficient information from the stimulus to restrict the number of possible responses. Thus, if he hears an initial /pa/ but is uncertain as to the nature of the final phoneme his response will be restricted to such words as 'pack', 'pad', 'pan', 'pat' and so on. Given partial information of this kind the set of possible words should be unbiased as to frequency. That is, it is assumed that all parts of the stimulus space contain the same proportion of high and low frequency words. This implies that the stimulus information itself contains no clue as to whether the word is high or low frequency. I have presented evidence suggesting this to be false (Morton, 1968b) and it is intuitively clear that, for example, any word containing a *z* is likely to be of low frequency. Unfortunately no one has worked out the implications of the violation of the assumption for the arguments which follow. If the set of possible words is unbiased, then as the ratio of high to low frequency monosyllabic words (using Broadbent's classification) in the Thorndike-Lorge tables is 1:1.42, there would be the same proportion in the reduced set of words from which the guess was made. This condition would be met (approximately) by there being two high frequency and three low frequency words remaining as possible responses, one of which would be the correct response, together with possible responses outside the frequency classes being used. Let us call the probability of guessing with a particular low frequency word G_l, and the probability of guessing with a particular high frequency word G_h. Now, in our example, if the stimulus was a high frequency word the probability of guessing correctly would be G_h. The probability of guessing the other high frequency word would also be G_h, while that of guessing with any one of the low frequency words would be $3 \cdot G_l$. Thus the ratio of high to low frequency errors to the high frequency stimulus would be $G_h / 3G_l$. If the stimulus were a low frequency word, the probability of a high frequency error would be $2\,G_h$ while the probability of a low frequency error would be $2\,G_l$, since one of the remaining possible low frequency words would in fact be the correct response. The ratio of high to low frequency errors to a low frequency stimulus would thus be $2G_h/2G_l$, a ratio greater than that expected with a high frequency stimulus. The same reasoning would follow whatever the actual size of the set of possible words, which cannot in any case include more than five high frequency words for the reason given in the previous section. Looking now at Table 3 we discover that for the monosyllabic words the ratio of high to low frequency errors is 2.05 for the high frequency stimuli and 2.17 for the low frequency stimuli; a remarkably close correspondence. With the disyllabic words the ratios were 1.46 for the high frequency stimuli and 0.46 for the low frequency stimuli, a difference in the *opposite* direction from that predicted by a sophisticated guessing theory! Thus this theory too must be rejected. I tested the two guessing models in a slightly different way (Morton, 1968b). My conclusion was the same: even a sophisticated guessing model underestimated the number of times a high frequency word was correctly reported.

It should be noted that the mathematical basis for the rejection of sophisticated guessing models has been challenged, and there has been a debate in the pages of *Psychological Review* which has been rather confused (Catlin, 1969, 1973; Nakatani, 1970; Treisman, 1971). Basically the supporters of sophisticated guessing have claimed that the ratio of high to low frequency words in the set of possible responses will not be independent of the frequency of the stimulus-word because this set will actually be centred on the stimulus-word itself. It is thus, they argue, only the *error* responses in the reduced set that will be independent of the frequency of the stimulus-word. It has been overlooked that such a claim hides two assumptions: first that all the evidence gathered is error-free, and second that no response is possible which violates this evidence. I can see no justification for either belief. Subjects often report partial cues, such as word shape or one letter, which are in error (see e.g. Morton, 1964a), and one classical refutation of the second assumption was by Pillsbury (1897), mentioned on p. 123. Given the relaxation of these assumptions Catlin's (1973) arguments, for example, are not to the point.

Alternative models for the word-frequency effect There are two broad classes of alternatives to the guessing models. One of them would suggest that either the sense organs or the central mechanisms are adjusted so that they tend to process high frequency words more efficiently. This would include 'perceptual' models and analysis-by-synthesis models. With such models there may or may not be additional response bias. A further class of model, the evidence bias models, would say that the advantage of high frequency words is that less evidence is required from a high frequency stimulus for a decision to be made. This is the class in which the logogen model would be placed. My test between these two models is easier to follow in summary than Broadbent's, although we both came to the same conclusion. What I did was to take each frequency interval

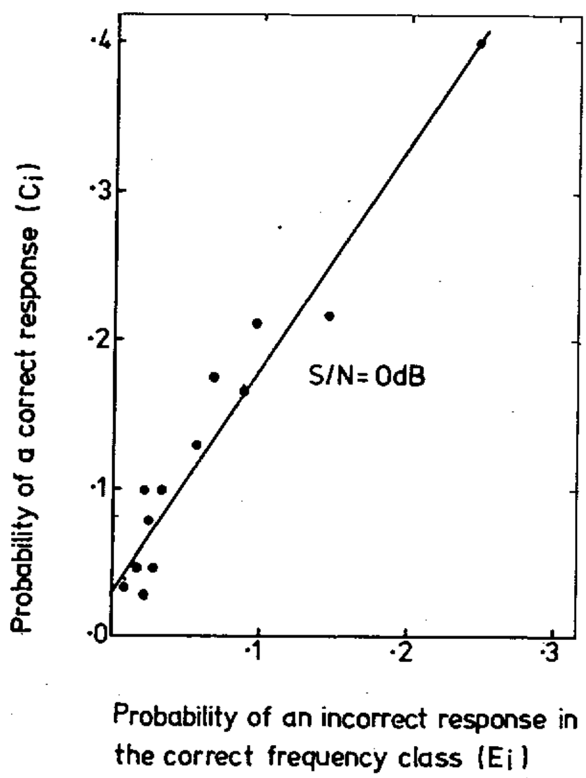


Fig. 4
Performance in the recognition of visually presented words of different frequencies of occurrence.
(data from Brown and Rubenstein, 1961)

against the probability of an incorrect response of the same frequency of occurrence as the stimulus. This is shown in Figure 4. On the logogen model the advantage of the stimulus-word over other words of the same frequency is the same for high and low frequency words. Thus the probability of a correct response should be a constant multiple of the probability of an incorrect response of the same frequency. (This step might not be intuitively obvious; however, its mathematical basis is sound—see Morton, 1968b.) Therefore we expect a straight-line relationship between the two functions. The line in Figure 4 is a reasonable fit, though there does appear to be a slight downward curvature. The perceptual models would expect the stimulus-word to have a *greater* advantage over other words of the same frequency class for high frequency words. If this were true the data in Figure 4 would be concave upwards. If anything the points are concave downwards and so the perceptual models must be rejected in favour of the evidence bias models.

Clearly a further type of model is possible which combines evidence bias and sophisticated guessing. The logogen model could be extended to function in that way, the decision among various alternatives being made in the cognitive system in Figure 1. In particular it seems to me that such might be the way of functioning in processing continuous speech or in reading. This would be especially true in situations where the context was insufficient to decide between homographs such as 'tank'. In this version, however, the set of possible responses would already contain a bias towards high frequency words and the final decision would be made on the basis of other factors.

THE LOGOGEN MODEL AS A SIMPLIFIER

Having decided in favour of an evidence bias model such as the logogen model, we can ask how it accounts for the seeming paradoxes described earlier. In the model, the simplest way of accounting for the way in which the word-frequency effect is set up is to say that the threshold of a logogen is permananently reduced by some small amount every time the logogen is active. This will, of course, happen whether the appropriate word is spoken, seen, heard, written or merely thought. In other words frequency of experience and frequency of production are of equal importance. Since each logogen is appropriate for only one word the occurrence of *phrase* would not affect the logogen for *frays* even though they are spoken in the same way; thus Neisser's (1954) result would follow. Indeed we would want to say that *chop* and *chop* were represented by different logogens even though they both look and sound alike. It is an equally important distinction that they *mean* different things, and so are influenced by different sets of evidence from the cognitive system. This supposition has some support from an experiment by Marshall (1967). He took words which had at least two distinct meanings such as *chop* and *air*. He then estimated the relative frequency of occurrence of the two meanings and obtained two sets of words, matched for frequency in the Thorndike-Lorge tables. The words in one set had meanings which occurred approximately equally often, such as *chop*. For the words in the second set one of the meanings occurred much more often than the other, as with *air*. He prepared a third set of words which were unambiguous. Marshall obtained the visual duration thresholds for these words and discovered that the ambiguous words all had higher thresholds than the equivalent control words. Furthermore, the words whose alternative meanings were equally common had higher thresholds than the ones in which one meaning was dominant. Roughly speaking then, the threshold for *chop* was more appropriate to the frequency of the separate meanings than to the frequency of the visual pattern or the spoken response. This result could not readily be handled by any of the other theories on word recognition.

Slightly more caution may be needed in interpreting a result of Roydes and Osgood (1972). These authors selected words which could be either nouns or verbs. There were two groups: those whose predominant meaning was nominal (N), such as *man*, and those which are primarily verbal (V), such as *can*. They found that if the subjects were given a bias towards expecting nouns, the N group were better recognized than the V group. Under a set of verbs the opposite was found. While this result fits in with the idea of dual logogen representation there is another, more elaborate, explanation possible in terms of differential weighting of semantic or syntactic cues for singular logogens.

Another line of evidence with respect to homographs comes from tasks involving a decision as to whether or not a particular string of letters corresponds to an English word—the *lexical decision task*. Rubenstein, Garfield and Millikan (1970) and Rubenstein, Lewis and Rubenstein (1971) showed that reaction times for ambiguous words—homographs—were shorter than for unambiguous words of the same frequency of occurrence. From this they concluded that an unambiguous word has a 'single representation stored in memory' whereas a homograph has multiple representations. This is essentially the same conclusion that I reached following Marshall's experiment, but notice that in the latter case performance was worse with homographs than with other words of equal frequency whereas in the lexical decision task performance is better. The difference between the two situations lies in the amount of stimulus information available. In Marshall's task the words were only exposed briefly and the limitation in the amount of stimulus information available means that the more important internal variable is the threshold of the individual logogens. With the lexical decision task there is no limitation of stimulus information. The limitations come rather in the processing of the information and in the mobilization of the response. In the case of homographs the advantages of a double representation in the decision part of the task outweigh the decrease in sensitivity of the individual logogens in the initial categorization of the stimulus. Although the original Rubenstein *et al.* experiments have been criticized on statistical grounds the result has been replicated by Jzastramski and Stanners (1975).

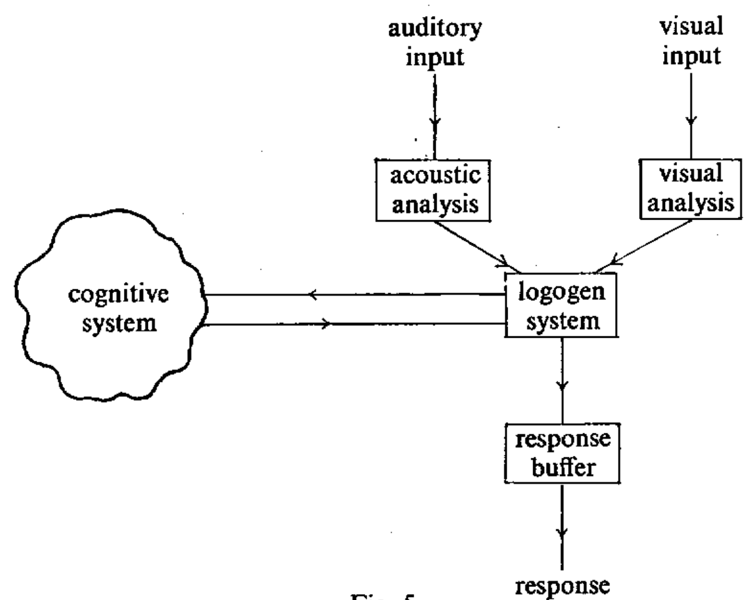


Fig. 5

This represents Mark II of the logogen model. In the logogen system sensory and contextual information interact giving rise to outputs to the cognitive system and the response buffer. The semantic interpretation or the referent of a word may be discovered in the cognitive system. The response buffer contains material in a phonological code. This material may be output as a response. Other connections which have been omitted include routes directly from visual analysis to the response buffer (grapheme-phoneme route) and from auditory analysis to the response buffer (acoustic-phonemic route). In addition there is a route which would permit a phonological code to access the meaning (as in reading a string like BRANE).

The lexical decision task was also used by Schvaneveldt, Meyer and Becker (1976), who were interested in whether all or only one of the meanings of a homograph were made available 'at the level of the influence of semantic context'. It is clear that in the logogen model one would expect that a context would lead to the availability of the primed meaning more rapidly than the non-primed meaning. It is, however, an open question as to whether the other, unprimed meaning would also become available, or whether, in the absence of prior context, both meanings would become available. The difference between the two possible outcomes could be represented by the presence or absence of inhibitory systems. The single-channel nature of the output from the logogen system to the response buffer forced the conclusion that the availability of a response would result in the inhibition (or, at best, delay) of any other response (see Morton, 1969b). There has not, however, been any evidence requiring a similar decision with respect to the output from the logogen system to the cognitive system. So either outcome would be representable. Schvaneveldt *et al.*'s technique was to have subjects make three successive lexical decisions. Among the triples presented were sequences of three words such that the second had two distinct meanings while the first and third were associated with these meanings in various ways. The important finding was that when the first and third words were associated with the same meaning of the second word (e.g. *save-bank-money*) the reaction time for the third word decreased compared with a condition with unassociated words. When the first and third words were associated with different meanings of the second word (e.g. *river-bank-money*) the reaction to the third word was not different from the control. Their conclusion is that only the primed meaning of the ambiguous word is made available to the system. As they observe, with a passive model this means that the alternative meaning must be inhibited in some way. In another condition of interest the first word was unrelated to the second but the third word was related to one of the meanings of the second word (e.g. *quick-bank-money*). In this the reaction time to the third word was lowered relative to the control, but not as much as when all three words were related. Thus, the inhibition of one interpretation of the second word is not simply a function of the context but must occur (in this task) when ever the ambiguous word is encountered.

Word v. Morpheme as the unit

Originally I suggested that each logogen represented a word. I had not, however, really defined what I meant by that except that the word must have been experienced and had to be associated with some meaning or structural description so that it could be subject to contextual influence. These restrictions would mean that there could be no logogen corresponding to a nonsense word, nor to its subjective equivalent, a word not previously encountered. Our ability to repeat or read nonsense words thus requires a separate account. As it stood, then, I realized a separate logogen was needed for 'sing', 'sing', 'singing', 'singer', 'singers'. This seems very inefficient, as one could make do with one logogen plus other devices for recognizing the suffixes and for adding them on in production. Graham Murrell and I devised a way of testing whether the word or the morpheme was the appropriate unit (Murrell and Morton, 1974). Basically we used the technique Neisser (1954) used with homonyms. We had four groups of subjects who would be presented with a list of words including *bored* as the stimuli in a threshold experiment. Prior to this the subjects read through a list of words. For one group this list included *bored*, for another *boring*, and for the third *born*. The fourth group had nothing relevant for that stimulus-word. We would expect that the first group would have a lower threshold for *bored* than the latter by the normal facilitation process. By definition this facilitation takes place in the logogen. The question, then, is whether prior presentation of *boring* also facilitates subsequent recognition of *bored*. The data showed such a facilitation. Prior presentation of *born*, however, had little or no effect, showing that the effect of *bored* was not simply due to the extent of visual similarity.

Osgood and Hoosain (1974) also tried to test the importance of the word v. that of the morpheme. In one experiment they took morphemes such as *ment* and found that they were reported less often in a tachistoscope than words of the same length, like *mend*, even though this morpheme, as letter sequences, occurs more often in the language. This is one of the experiments which these authors claim show the salience of the word as a perceptual unit. It is not clear, though, that the test is fair. In our experiences of morphemes such as *ment* (technically 'bound morphemes') only one end is free. The likely visual code is *—ment* #and not# *ment*#, where # indicates a visual boundary. It is trivially clear that word boundaries are very important, if not if wouldbemucheasiertoreadthissentencethanitis. It remains possible that the 'morphemes' are being dealt with as nonsense syllables. In their Experiment II, Osgood and Hoosain claim to show that three-letter non-word morphemes (e.g. *pre*, *geo*) are better perceived than non-morpheme trigrams of the same occurrence frequency (e.g. *ple*, *pes*), but the generalizability of their findings is questionable. They also show that sequences such as *dashing* are better recognized than non-word morpheme combinations matched for occurrence frequency of the components (e.g. *famness*). This indicates that even if the morpheme corresponds to a perceptual unit the total recognition process depends also upon lexical factors.

Further evidence concerning the role of morphemes is scanty. Gibson and Guinet (1971) found no difference between words of the same length which were inflected (*trying*) or not (*listen*). Equivalently the addition of an inflection made an item more difficult to recognize. These authors did find that the final letters were more often correct if they were an inflection. They interpreted this as evidence of separate processing of inflections.

Osgood and Hoosain (1974) provide other evidence concerning the unit of processing. Basically they show that familiar nominal compounds such as *town hall, stock market,* and *post card* behave as units. Such compounds are equally well recognized as single words of the same length and frequency of occurrence (e.g. *misunderstand*). Furthermore the presentation and recognition of such compounds has no facilitative effect upon the subsequent recognition of the constituent words, unlike such combinations as *street market*. In terms of the model described above this means that the compounds are represented by a separate logogen and in these cases the word is the appropriate unit and not the morpheme, as *town hall, stock market* etc. are composed of two morphemes. The Murrell and Morton conclusion thus has to be modified. The morpheme only dominates the word when the morpheme in question is a free morpheme and the word is made up of a free morpheme plus a bound morpheme.

We thus see that it is possible to investigate a particular level of representation experimentally. While this level has relations with particular linguistic levels it is not bound by them and the manner in which a particular string of letters or sequence of phonemes is represented at the logogen level is an empirical not a theoretical matter since in this case we have an empirical indicator, the facilitation effects. We will hope to find sense in the patterns, but this sense will be a psychological one and need not necessarily have a counterpart in current linguistic theory. It is clear, for example, that frequency of experience will have some influence. For example, *street market* was not a unit for Osgood and Hoosain's subjects; it is likely that it would be for the inhabitants of some parts of London. It would then be a strong test of a linguistic theory to find some criterion to distinguish between the two groups of speakers in their usage of the phrase.

It is also fairly likely that such combinations as *FBI* and *USSR* can be regarded as units at the logogen level. This was indicated by an experiment by Henderson (1974) who showed that same-different judgements of pairs of letter strings were faster when the strings were meaningful than with sequences such as *BLI* and *YSSU*. It would be necessary to check, for example, that one obtained typical facilitation effects with strings like *USSR*, but arguments concerning whether it can 'properly' be a word if it is unpronounceable are beside the point.

A brief note on reading and word/non-word distinctions
I have concentrated almost entirely on word recognition to the exclusion of sentence perception and the recognition of letters and non-words, though it should be apparent that any theory which is constructed for one should have some relevance for the others. I will talk a little about experiments on non-words since they have been used in the literature to draw conclusions about word processing.

The word/non-word distinction is very much tied up with the debate as to whether it is necessary to convert a graphical representation to a phonological one before recognition. The conflict of views on this topic is clear enough. The following quotations appeared in Coltheart *et al.* (1976), who have a good discussion of some of the sillier views expressed on the issues. The first viewpoint is that there always has to be a phonological representation as a result of the application of rules:

> in order to read writing one must have an ingrained habit of producing the sounds of one's language when one sees the written marks which conventionally represent the phonemes (Bloomfield, 1942).

> Learning to read . . . requires primarily the translation from written symbols to sound, a procedure which is the basis of the reading process (Venetsky, 1967).

> The heart of 'reading skill' is surely the process of decoding the written symbols to speech (Gibson, 1970).

The contrary viewpoint is that words can be recognized in a purely visual fashion.

> Reading can be, and for skilled readers often is, a visual process (Bower, 1970).

> Reading does not need to proceed by the readers' forming auditory representations of printed words (Kolers, 1970).

> Readers are . . . regarded as 'predicting' their way through a passage of text, eliminating some alternatives in advance on the basis of their knowledge of the redundancy of the language, and acquiring just enough visual information to eliminate the alternatives remaining (Smith, 1971).

It should have been clear from my description that the logogen system does not require an intermediate stage but rather that the logogens operate directly on a graphical representation. It is equally clear that some other system must exist to allow us to produce a spoken representation of a letter string. Otherwise, when faced with a nonsense syllable, such as *zug*, or a very rare English word, such as *wey*, that we hadn't met before, we would not be able to pronounce it since there would not be a logogen corresponding to it. We are able to pronounce nonsense syllables by applying the rules of language which relate to the normal correspondence between written and spoken forms. These rules are called the grapheme-phoneme conversion rules. The system which uses these rules is the grapheme-phoneme system or route. The big debate is the extent to which the grapheme-phoneme route is used in different tasks and under different procedural variations. The phonic method of learning to read relies almost entirely on the use of the grapheme-phoneme rules while the look-and-say method aims at setting up immediatley a link between the visual pattern and the spoken form of the word. It is not clear whether such a distinction is of importance to the debate on word recognition. Even if we initially learn a particular word by using the phonic method one could easily envisage a learning process whereby we eventually recognize the same word purely visually.

In addition to the debate on the extent of use of the grapheme-phoneme route there is a secondary debate as to the units of conversion in this route. Some authors favour letter-by-letter conversion, others opting for larger units. Smith and Spoehr (1974) provide a good discussion of this problem (and also of experiments and theories relating to the recognition of letters) and provide some experimental evidence in a later paper (Spoehr and Smith, 1975). They conclude that both the 'spelling patterns' proposed by Gibson (1965) and the syllable-like units of Spoehr and Smith (1973) have an influence on our recognition of letter strings.

Interest in word/non-word differences was awakened recently by Reicher (1969) who showed that individual letters are better detected in words than in non-words. His procedure controlled for guessing and his results have been replicated a number of times with different procedures (Wheeler, 1970; Smith, 1969; Smith and Haviland, 1972; Eichelman, 1970; Krueger, 1970a, 1970b). In most cases, however, the non-words were unpronounceable letter strings. Other studies have shown that the ease of identification of non-words depends upon their pronounceability (Gibson, Pick, Hammond and Osser, 1962) so the possibility remains that the word/non-word difference is entirely due to pronounceability. This would be good for people who believe in obligatory grapheme-phoneme conversion and bad for the logogen model and other models which account for the word/non-word difference in visual and lexical terms (e.g. Rumelhart and Siple, 1972). Then Baron and Thurston (1973) not only confirmed that the recognizability of non-words in tachistoscopic presentation depended upon their pronounceability but also claimed to show that pronounceable non-words were as easily recognized as real words. This result has been widely believed as being completely generalizable and establishing the primacy of the phonological route. For example it deeply colours Smith and Spoehr's (1974) discussion of word/non-word differences and their evaluation of different models of word recognition. However, as Chambers and Forster (1975) have pointed out, there are three problems with the Baron and Thurston result: they used such obscure real words that scarcely any of their subjects could be expected to have been familiar with them; only a few exemplars of each type were used; each item was presented several times. Thus all the non-words would have rapidly become familiar to the subjects and the task would have reduced itself to a forced-choice task where, in effect, the alternatives are known in advance. It is established that in this situation word frequency has no influence on recognition scores (Pollack, Rubenstein and Decker, 1959) equally the basic word/non-word distinction would be masked.

At the moment the position remains confused. However there are a number of results which indicate that pronounceable non-words are less easily processed than real words. Thus, Forster and Chambers (1973) showed that pronounceable non-words took longer to name than words and Chambers and Forster (1975) found differences in a task involving same/different judgement on a pair of simultaneously presented letter strings. Spoehr and Smith (1975) found similar differences in a task which involved subjects seeing a letter string in a T-scope and then saying which of two alternative letters had occurred in a particular position in the string. However they only found a word superiority in one of two very similar experiments so the crucial variables remain unknown.

I take the position that if one can find word/non-word differences when holding pronounceability constant then the case is made for the possibility of direct lexical access from the visual stimulus. If other experiments show no differences then that shows that alternative methods of processing also exist (which one should never have doubted). A recent experiment by Hawkins, Reicher, Rogers and Peterson (1976) has demonstrated the flexibility of processing directly. Their technique was to present a word or letter in a T-scope and then give the subjects a pair of alternatives. In the case of word stimuli the alternatives would either be homophones (sound the same when spoken) such as *sent/cent* or not, e.g. *sold/cold.* Both choices involve a visual discrimination between initial *s* and *c*. There were two experimental groups who differed in the proportion of stimuli which were homophones. The idea was that the low homophone group, with about 8% of choices being between homophones, would be prompted to use a phonetic encoding strategy. The high homophone group, for whom about 50% of the stimuli were homophones, were expected to use other strategies. For this group there was no difference in performance between homophones and controls. The low homophone group only managed 58% correct with the homophones (chance being 50%) compared with 72.5% with the control words. These data indicate that the objective of the experiment succeeded and that subjects can be induced either to use a phonetic code or not. This result leaves open the question of which method of processing is the more 'normal' which, if it has any meaning, refers to adult reading. I presume that the final answer will be that it is normal to use both kinds of processing.

An indication of the kind of experiment which might turn out to be persuasive is given by Healy (1976). Her subjects were required to look through printed material for examples of the letter *t*. The basic condition showed that 62% of the *t*s in occurrence of *the* were missed. This wasn't because the *the*s were redundant in the text and so were not read, because the error rate was 67% in a text with scrambled words; neither could the result be explained by observing that the *t* is not actually pronounced separately but is part of the *th* sound since detections of *t* in the word *thy* were up at 92%. Healy concludes that *the* is read as a unit but *thy* is decomposed into its component letters. In the latter case, but not the former, the grapheme-phoneme rules could operate.

The dissociation of name and meaning: perceptual defence and deep dyslexia

We have seen that there are two routes from the logogen system. One of these makes a spoken response available via the response buffer and the other leads to further syntactic or semantic processing in the cognitive system. What is the relation between these outputs? Well, when I observed people reading out loud I noticed that semantic errors were often made. Thus one subject replaced *straight away* by 'suddenly' and another read *Wednesday* as 'Tuesday' (Morton, 1964c). This meant that the meaning of the word became available before the response, the response actually being based on what had been understood rather than on what had been read. This, then, requires that the logogens have two independent outputs with the threshold of the connection to the cognitive system being lower.

This requirement actually confers other benefits, as there are at least two other situations in which the meanings of words are analysed but the response is not available. The first of these is with normal subjects, where semantic information is available without the subject being aware of the identity of the stimulus. The classic paradigm for this is known as perceptual defence. In such experiments subjects are presented with taboo words such as 'penis'. The subject gives evidence of having analysed the stimulus by virtue of some autonomic response such as a change in skin resistance. At the same time the subjects make not report as to the nature of the stimulus and will sometimes even be unaware that any stimulus was present. Dixon (1971) gives a good description of the history and status of such research.

Related effects have been found with very short exposures of words followed by a *pattern mask*. This is a jumble of letter fragments which has very strong effects on visual processing. In certain experimental arrangements subjects can make better than chance responses on a forced-choice semantic judgement under conditions where they are unaware that anything has been displayed (Marcel and Patterson, in press). In these situations I would want to say that enough stimulus information is available to exceed the lower threshold of the logogen and release information to the cognitive system but insufficient to exceed the threshold of the output to the response buffer. Without this the subject has no conscious percept.

A similar account can be given of some symptoms of *deep dyslexia* or *phonemic dyslexia*, a type of aphasia. The outstanding characteristic of these patients is that they produce semantic errors when they read single words. It is important to realize that these are single words, clearly typed and with the patient under no time pressure whatsoever. Thus Marshall and Newcombe (1966) report a patient who read *drama* as 'play', *sick* as 'ill' and *soccer* as 'football'. When presented with the word *nice* he said: 'Name ... in France ... South of France'. Marshall (1976) gives examples from half a dozen other articles and describes these patients in more detail. The only thing which concerns us at the moment is that such patients clearly have access to a semantic interpretation of the word without being able to name it. Within the logogen model we can account for such a peculiarity quite naturally by analogy with subliminal perception. In the latter case I suggested that there was insufficient stimulus information to trigger the output to the response buffer. In the case of deep dyslexia we need only say that the output to the response buffer is blocked for individual logogens. The cognitive system receives information and so the semantic interpretation of the word is apparent. When this semantic information is sent to the logogen system, since the correct response is blocked some word with overlapping meaning is made available instead. It is very agreeable to be able to give an account of such a phenomenon without having to make any additions or changes to the model.

THE TWO HEMISPHERES

Tachistoscopically presented words have generally been found to be better recognized in the right visual field (RVF) than in the left visual field (White, 1969; Ellis and Shepherd, 1974). One account of this difference is that information falling in the RVF, being relayed directly to the left hemisphere, is processed more efficiently, since with normal right-handers, the left hemisphere is better organized for dealing with verbal material (e.g. White, 1969). Whereas some workers claim (sometimes almost hysterically) that the right hemisphere is incapable of any verbal processing, the question that is being increasingly posed is rather that of what is the extent of processing in the right hemisphere.

Evidence on this point has been obtained from patients and normal subjects. Gazzaniga (1970) observed that some split-brain patients were able to recognize some words presented to the right, non-dominant hemisphere. According to Gazzaniga's rather casual account it was 'noun-object words' that were most reliably recognized. Adjectives were occasionally recognized but verbs never were. This account suggested to Ellis and Shepherd (1974) that the right hemisphere may be assoiated more strongly with concrete, as opposed to abstract words. They tested this supposition with normal subjects presenting words in a tachistoscope to one hemi-retina, using concrete and abstract nouns equated for length and word frequency. Their results showed that there was in interaction between field of presentation and the type of noun such that the advantage of concrete over abstract nouns was greater when the words were presented to the left visual field. Such a result requires that some word analysis takes place in the right hemisphere, since the passage of purely sensory information from right to left hemisphere could not be expected to favour the concrete words.

A seemingly conflicting result was found by Marshall and Holmes (1974) using a similar technique. They used words of four categories, nouns and verbs of high and low frequency. Their results showed a difference between nouns and verbs when the words were presented to the RVF, nouns being better recognized. There was only a small and non-significant difference between the two word classes in the LVF (right hemisphere). Instead the right hemisphere appeared to respond more readily to high frequency as opposed to low frequency words. As nouns and verbs generally differ along the abstract-concrete dimension there appears to be a conflict between Marshall and Holmes' result and those of Gazzaniga and Ellis and Shepherd, but the issues are far from clear. In the latter case, for example, one cannot assume that the factor differentiating between abstract and concrete nouns is even applicable to verbs. To compare patients and normals we will have to develop our theoretical apparatus a little further.

We can suppose that 'recognition', in all its forms, is at least a two-stage process. The first process involves the conversion of the visual code to a further, unitary code—as in the logogen system. The second process involves the use of this code to produce either a spoken or written response or some other indication that a unitary representation has been achieved. Note that with the model I have been using it is entirely possible to have either one of these outcomes without the other. With the split-brain patients it is clearly not possible to produce a spoken response from stimuli presented to the right hemisphere; the production of spoken responses very clearly is the final responsibility of the left hemisphere (throughout this discussion I will only talk about right-handers with full lateralization). Any evidence of recognition of words by the right hemisphere, then, must use some other method; and that method must itself be implemented in the right hemisphere. With concrete nouns—specifically with the names of common objects—it is evident that such a method exists—split-brain patients can match the names of the objects with the objects themselves, whether the objects are presented visually or tactually, even if they cannot speak the words (Gazzaniga, 1970). This means that the outputs from the word recognition units, the first of the two processes outlined above, are compatible with the outputs of equivalent object recognition units (there are a range of specific possibilities). In order to give evidence of recognizing other words, colour adjectives for example, it would be necessary that the right hemisphere had available both an appropriately categorized representation of colour and a means of linking that representation to the recognition system. What might one do for verbs? One possibility is to use action verbs and require the patient to perform the action. To accomplish this it would be necessary that in the right hemisphere the output from the recognition system should be convertible to some form of semantic representation and that this in turn should be linked to an appropriate action control system. (Zaidel, 1976, has just published data which indicate that the right hemisphere of some split-brain patients is capable of comprehending a great deal more than even Gazzaniga suspected. This doesn't disturb the present argument.)

It should be clear then that there could be recognition units for both nouns and verbs in the right hemisphere irrespective of Gazzaniga's observations since, in the absence of means within the right hemisphere to use the outputs of such units, the patients could not indicate, and perhaps would not 'know', that the word had been recognized (again, I must stress the highly specific way in which I use the word here). With normals there would be no such problem, the output from the recognition system simply being routed to the left hemisphere for further processing.

CODA

In the preceding section I have talked a little about the hemispheres, which might seem to belie my earlier claim to be concerned only with psychological descriptions. What I have done is to take the easy step of using other people's terminology. Most of the experiments I discussed involved differences between hemifields. To equate differences between the hemifields with differences between the hemispheres is convenient and may be plausible, but it does involve a major assumption which might be forgotten.

The same is true of the model as it is presented here. I have made a number of assumptions about the generality of certain experimental results and have also adopted a strategy of starting with the simplest model. As it stands the model requires that any use of a word will lead to subsequent facilitation of recognition of the word. One set of reasons for this is the facilitation effects across modalities together with the assumption that short-term effects with nonsense syllables indicate the way we process words. A second reason lies in the Goldiamond and Hawkins (1958) result where response frequency matched prior perceptual experience (see p. 129). Again we have a partly conceded assumption—that there is an essential continuity between responses that are determined by the stimulus and responses in the absence of a stimulus. This was one of the starting points of a theoretical treatment which has proved quite profitable.

But suppose someone discovered that, say, naming a picture had no effect on the subsequent recognition of the name. That seeing the picture of a butterfly and saying 'butterfly' had no effect 15 minutes later on the ability to see the word *butterfly*. What would be the implications of this? It would mean that we would have to divide the logogen system into separate input and output functions with all the word-facilitation effects taking place on the input. Well, this result was reported by Winnick and Daniel (1970) and its implications only recently became apparent to me. We have replicated their results in Cambridge and also found other results suggesting the need for other changes in the model. The problem will be to do this in such a way as to preserve the advantages of the original. The evolution of the new form of the model will take some time but its need and its possibility indicate progress.

Of late there have also been elaborated a few welcome proposals which look like genuine alternatives to the logogen concept. One such alternative is the *cohort model* put forward by Marslen-Wilson (Marslen-Wilson and Welch, 1978; Marslen-Wilson, personal communication). All variants of this model have in common an exploration of the role of inhibition in word selection as opposed to the facilitation effects which are stressed in most forms of the logogen model (the 1968a version is the exception). In one variant of the cohort model, inhibitions from sensory and contextual sources interact reducing the number of potential responses until only one remains. This is then produced as a response. Such a model would predict that the recognition of a word in speech would depend more on how many physically similar words (cohorts) could occur in the same context than upon the transitional probability of the target. In this respect it differs from the logogen model in its current form which predicts no effects of the cohort structure but a strong effect of transitional probability. A variant of the cohort model has a set of cohorts produced from an initial stimulus analysis. One of these is then selected on the basis of the context. This production of a reduced set is also suggested by Forster (1976) and Becker (1976) and is, of course, a feature of Halle and Stevens' (1962) analysis-by-synthesis model already discussed. Details of Forster's and Becker's models will not be discussed here. Both authors lean heavily on experiments using lexical decision (saying whether a string of letters is a word or not under time pressure). Alas, this task does not appear to be fully understood. For example, it seems possible that semantic information is consulted in some way in the task which means that some word-frequency effects may be ascribed to the cognitive system rather than the logogen system. As a final note of caution: when reading the literature one must not assume that all authors mean the same thing by 'lexicon'. In general lexicons differ from logogens in having a dictionary or thesauraus element to them. The logogen system, as already indicated, has no such appendage.

The presence of good competitors is a fair guarantee of useful experiments which test the ideas. So I'm afraid that all the models will be subject to upheaval in the next five years—don't become too attached to any of them.

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