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INTERACTION OF INFORMATION IN WORD RECOGNITION[1]

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Quantitative predictions are made from a model for word recognition. The model has as its central feature a set of "logogens": devices which accept information relevant to a particular word response irrespective of the source of this information. When more than a threshold amount of information has accumulated in any logogen, that particular response becomes available for responding. The model is tested against data available on the effect of word frequency on recognition, the effect of limiting the number of response alternatives, the interaction of stimulus and context, and the interaction of successive presentations of stimuli. The implications of the underlying model are largely upheld. Other possible models for word recognition are discussed as are the implications of the Logogen Model for theories of memory.

In previous papers a functional model for word recognition has been developed (Morton, 1964a, 1964b, 1964d; Morton & Broadbent, 1967). The form of description used only lent itself to qualitative predictions and while it seemed to have some heuristic value, the overall system was too complex to allow rigorous specification of its properties. In the present paper the model is first outlined in a slightly simplified way and then certain features of it are isolated in order to make quantitative predictions about performance in word recognition. The various predictions made are largely independent and have in common only the fact that in all situations there is some stimulus information present. The effects of word frequency are taken to indicate relatively permanent changes in the system; the effects of having a reduced set of alternative responses involve temporary changes in the same variable. Different predictions are made concerning the interaction of a context with the stimulus and the effects of repeated presentation, these differences arising from differences in the potential sources of such information. The model contrasts most completely with explanations of word recognition which would ascribe all the observed effects as being due to "guessing" habits.

While in conception the model is very complex and highly interacting, it should be noted that the separate sections can be judged in isolation. In the description of the model a number of variables are introduced to account for primary observations. The implications of most of them are tested in the sections that follow.

Description of the Model

The basic unit in the model is termed a *logogen*.[2] The logogen is a device which accepts information from the sensory analysis mechanisms concerning the properties of linguistic stimuli and from context-producing mechanisms. When the logogen has accumulated more than a certain amount of information, a response (in the present case the response of a single word) is made available. Each logogen is in effect defined by the information which it can accept and by the response it makes available. Relevant information can be described as the members of the sets of attributes $[S_i]$, $[V_i]$, and $[A_i]$, these being semantic, visual, and acoustic sets, respectively. More detailed suggestions as to the properties of these sets are given elsewhere (Morton, 1968b). Incoming information

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[2] From *logos*—"word" and *genus*—"birth." The author is indebted to Hallowell Davis for suggesting the term.

With Compliments
F. G. Eisler

CONTINUITY OF SPEECH UTTERANCE, ITS DETERMINANTS AND ITS SIGNIFICANCE

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CONTINUITY OF SPEECH UTTERANCE, ITS DETERMINANTS AND ITS SIGNIFICANCE

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Pause frequency and word length of speech sequences uttered without break (referred to as " phrases ") were measured, using speech produced under a variety of conditions. Several subjects were recorded in each condition. Individual differences as well as differences due to change of speech situation were highly significant. The significance of continuity in speech production is discussed in the light of the specific nature of these various differences.

INTRODUCTION

The fact that speaking is rarely continuous sound production, but consists of the utterance of sound sequences of varying lengths interrupted by pauses has been pointed out in previous work (Goldman-Eisler, 1956). That such pauses can take up a considerable proportion of the total speaking time has also been shown. No measurements, however, had so far been made of the lengths of the word sequences uttered continuously and of the frequency of the individual phrases alternating with these word sequences. The former is a function of the latter, and is expressed as a number of words per pause which is at the same time a measure of the frequency of pauses in relation to word production.

The sequences of words uttered without break will be referred to as " phrases " and interruptions of the vocal utterance of not less than 0.25 sec. are classified as " pauses " ; breaks of less than 0.25 sec. are not counted because they might be due to changes of articulatory position and delays in the articulatory process as such.

TECHNIQUE OF MEASUREMENT AND MATERIAL

In order to measure the word length of the individual phrases sandwiched between pauses, visual transformations of speech have to be obtained, transferring the sound impulses to paper, using a pen-recorder (see Goldman-Eisler, 1956, 1958) and the visual tracings have to be synchronised with the verbal content of the records. This was done for the speech uttered in a previously reported experiment (Goldman-Eisler, 1961) in which nine subjects were asked to describe the events occurring in serial cartoon stories without captions (of the kind regularly published in the " New Yorker ") and to formulate the meaning, point or moral of the story. They were also asked to repeat these descriptions and the summaries six times. Experimental conditions were thus created for the study of verbal behaviour (a) when speech is produced within a relatively concrete situation, i.e., a given sequence of events (through their description) and (b) in speech uttered in the process of abstracting and generalising from such

SEMANTIC SPACE REVISITED

A Reply to Uriel Weinreich's Review of *The Measurement of Meaning**

CHARLES E. OSGOOD

Psycholinguistics is a relatively new discipline developing along the border between linguistics and psychology. Both Uriel Weinreich and I are sympathetic to this development, and it is with the view of maintaining harmony along the border that I have written this addendum to his discussion of *The Measurement of Meaning*. Provincialism on either side of the border will hinder progress in interdisciplinary relations.

Weinreich's review is framed on the assumption that the semantic differential was developed as a technique for objectifying linguistic lexicography. He gives a convincing demonstration that the instrument is inadequate for this purpose, and then the authors are roundly criticized for their presumptiveness. But nowhere in the book is such a claim made by Osgood, Suci, and Tannenbaum; in fact, the term "lexicography" never appears on its pages. In the very last chapter, in a single paragraph, and in the context of possible applications of the technique, it is true that we mention the possibility of building "a functional dictionary of connotative meanings—a quantized Thesaurus," but this is a far cry from linguistic lexicography and even this development, as the authors clearly state, would depend upon considerable refinement of the measuring procedures.

The main point is this: The semantic differential was not designed as a linguistic tool but as a psychological one—to assess certain symbolic processes assumed to occur in people when signs are received and produced. It was not designed to classify or evaluate the innumerable correlations between linguistic signs and their referents—which really is not a psychological problem. Weinreich's attempt to make lexicographic distinctions with a version of "Twenty Questions" makes it clear that no standardized and limited set of dimensions could serve this purpose. But we have good evidence that the dimensionality of the *psychological* semantic space—the ways in which implicit, representational reactions can vary—is not so diverse and complex. The repeated appearance of the same general factors in replicated factor analyses (not only two, as Weinreich's review seems to

* "Travels Through Semantic Space," *Word* XIV (1958), 346–366, a discussion of *The Measurement of Meaning*, by Charles E. Osgood, George J. Suci, and Percy H. Tannenbaum, Urbana, Illinois, 1957.

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imply, but nearer twenty), involving such varied stimuli as linguistic signs, facial expressions and even sonar signals and such varied subject populations as college sophomores, hospitalized schizophrenics, Japanese mono- and bi-linguals, and Navaho Indians, certainly suggests that this aspect, at least, of 'meaning' is capable of measurement.

Since that ugly word, 'meaning', has crept into the discussion, let's come fully to grips with the points raised by Weinreich on our usage of the term. He takes us severely to task for claiming that we are measuring 'meaning' when it is really the 'affect' of words, he thinks, that the semantic differential taps. Had we read even an elementary presentation of semantics, we would have been aware of the conventional statements of the problem of meaning and, at the very least, would have avoided such a crude dichotomy as 'denotative' vs. 'connotative' meaning. And to cap the point, Weinreich says in exasperated good humor, "But by the authors' own statement, these 'connotations' have (literally!) nothing to do with the referential capabilities or functions of signs."

Now, without claiming to be as sophisticated as I probably should be with respect to philosophical and linguistic semantics, I would nevertheless say that there is nothing more confused and confusing than the literature I *have* read bearing on the usage of "connotative meaning." Furthermore, there are several "traditions" in the technical usage of the term 'meaning', one of which, represented by Ogden and Richards' "thought or reference" and Morris' "interpretant," refers to a representational state or process occurring in sign-using organisms when signs are received or produced. Also this is one of the standard usages in the lay language, e.g., when I ask someone to "Tell me what this inkblot *means* to you," he certainly doesn't assume that I want him to tell me what it refers to. In any case, there is nothing sacred about a "conventional" technical usage, particularly when it obfuscates the solution of a theoretical problem—as I think is the case here. Finally, we went to considerable pains (pp. 318–325) to specify how we were using the terms 'denotative' and 'connotative' and to indicate (pp. 2–10) what very limited *meaning* of 'meaning' the semantic differential is assumed to measure. But since our presentation was evidently unclear, and since I feel this is a critical issue, I will repeat the analysis here with some elaboration.

The *denotative meaning* of a linguistic sign I define as a conventional, habitual correlation between: (1) with reference to the speaker, a non-linguistic stimulus pattern, $\acute{S}$, and a linguistic reaction, $\boxed{R}$; or, (2) with reference to the hearer, a linguistic stimulus pattern, $\boxed{S}$, and a non-linguistic stimulus pattern, $\acute{S}$ (or a response, R, appropriate to this non-linguistic stimulus pattern). I use the symbols $\boxed{S}$ and $\boxed{R}$ to refer to

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A NEUROPSYCHOLOGICAL ANALYSIS OF THE SEMANTIC ASPECTS OF TEXT PERCEPTION

I

The problem of text perception, i.e. its semantic aspect, is the central problem in psycholinguistics. According to the view which is now universally accepted, this process presupposes the isolation of a great number of significant parameters in the material which is to be percepted, and inclusion of this material into a complex system of relations. In this way the source material undergoes a kind of restructuring.

The diversity of the parameters determining the perception of even elementary verbal material–separate words–was shown in an article by A. R. Luria and O. S. Vinogradova (1959). It became clear from this investigation that a word is included not only in a system of semantic relations, but also in systems of relations organized according to phonetic and morphologic principles. In normal cases these principles turn out to be latent, while the adequate semantic relationships are predominant. In cases of a certain cerebral pathology, however, they may become dominant.

It is obvious that in perceiving coherent verbal material–text–the structure of the 're-coding' process is much more complex. The point is that in this process the level of perceiving separate lexical items is supplemented by two higher levels: that of perceiving the system of relations between lexical items, and that of perceiving the general content of the text, and main thought it communicates.

In this way, the process of perceiving and understanding a continuous text is conceived as a multi-level process possessing all the features of problem-solving. This view is borne out both by psycholinguistic facts (Katz and Fodor, 1963; Lieberman, 1963; Chomsky, 1958) and by psychological evidence proper (Luria, 1966; Luria and Xomskaja, 1966).

If we turn to psycholinguistic studies, it becomes obvious that the investigation of the semantic aspects of perceiving and generating a speech utterance is lagging far behind the investigation of the grammatical aspects. This is quite natural if one takes into account that the former are

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THE GRAMMAR OF SIGN LANGUAGE: SOME IMPLICATIONS FOR THE THEORY OF LANGUAGE*

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An experimental study of the sign language of the deaf as used in Israel has yielded some results which seem to have important implications for the theory of language. Specifically, these findings raise some questions concerning the hypothesis of the universality of the base component of language. Various versions of this hypothesis are possible (c.f. Bach, 1968, pp.113-114), and I shall try to examine these in the light of our findings.

The claim has been made by some writers that the sign language of the deaf has no syntax. This is obviously a very unsophisticated statement. It is based on the observation that sign language has no inflexions and, that, furthermore, it does not appear to adhere to strict rules governing the sequence of signs in the sentence.

If these observations were substantiated, they would throw an important light on the question of the universality of language. One way of dealing with the situation would be to dismiss the relevance of sign language to linguistic theory on the grounds that, so one might claim, sign language is not a "real" language, but merely a makeshift one (like the trade jargons described by Jespersen (1922, pp.216-236), and by Reinecke (1938). But such an argument seems quite unconvincing to me. I see no reason to accord to sign language, or to other makeshift languages for that matter, a special status as far as the problem of the universality of language is concerned. We are dealing here not with finger spelling (in which every letter of the language spoken by the environment is represented by a constellation of fingers), but with an independent language in which every gesture or position of the hand stands for a concept. This language is acquired spontaneously, as far as we know, in every community of deaf persons who lack spoken language. Sign language serves them adequately as a means of communication and remains considerably stable over long periods of time. It is not a derived language (unlike Creole languages): although there seems to be a large amount of interference from the spoken language of the environment, sign language is not modelled on the latter, and in translating from sign language to the spoken language one does not get a one-to-one translation. To say that sign language has no relevance to the problem of universality would be to indulge in a circular argument, according to which those languages which fail to fit into a given scheme of universals of language are simply pronounced to be "out of the game".

When we began our investigation of the grammar of the Israeli sign language, we pursued a different line of reasoning. The observation that the sequence of signs in sign language is variable may turn out to be true only in everyday conversation where there is sufficient redundancy to make the message understood. In everyday give and take there will usually be enough linguistic context to make it obvious which sign is, for example, the object, and which the subject of the sentence, and hence there would be no need to indicate these by means of "word"-order. From this it does not follow that the order of signs is syntactically irrelevant. Rather, one may expect that when pragmatic cues are less abundant, as may happen when the subject matter is more difficult or unusual, or abstract, order will serve as an indication of grammatical relationship.

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