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Review of C.I.J.M. Stuart (editor), Report on the Fifteenth Annual (First International) Round Table Meeting on Linguistics and Language Studies

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Reviewed by EMMON BACH

In his "Introductory Remarks: On the Nature of Linguistic Theory," the editor of the latest Georgetown Round Table monograph, C. I. J. M. Stuart notes three areas of issue in linguistics: the subject-matter, the purpose, and the logic of linguistic inquiry. The papers of the volume and the recorded discussions bear witness to extreme diversity in all three areas. Unfortunately, in some cases they also add considerable weight to the pessimistic concluding words of Stuart's opening remarks: "It is perhaps significant that the results of linguistic investigations can be safely ignored by fellow workers, and that apparently contending resultant statements are regarded as just so many alternative ways of saying much the same thing. Perhaps this is because linguistic results, in general, do not change our understanding of nature; they ought to if what we are doing involves an explanation of language."

Except for two luncheon addresses by Martin Joos and Charles A. Ferguson, the papers are divided into three panel meetings (with discussion after each group of papers) devoted to "Current Research in Syntax outside the United States," "Achievement in Linguistic Theory," and "Subject-Matter Relations between Linguistics and other Disciplines." Final appendices give the complete program, record an exhibition of equipment and books, index the speaking participants (including discussion from the audience), and list all registrants for the two-day conference. I shall characterize as briefly as possible the contents of each paper in the order printed (with one exception) before returning to some more detailed and critical discussion.

In the opening paper of Panel I, C. E. Bazell discusses "Three Misconceptions of Grammaticalness." Two are noted briefly. "The notion that the syntactic deviations of the 'colourless-green-ideas' type are not linguistic deviations at all, but... 'ontological' deviations, or have something to do with the 'real world', or are 'aleuthetic' rather than grammatical, and so on," is dismissed as absurd and mentioned merely because it is apt to be
attributed to anybody who attacks one of the other misconceptions. The second is that grammar is “tailor-made” for the semanticist. Having established morphemes, the linguist asks “What are their meanings?” as if this question necessarily had an answer. Bazell holds that “in respect of semantics, the morpheme is a neutral unit.” For instance, obligatorily chosen items like the suffix of English verbs in the present tense cannot be said to have any meaning. “Meaning implies choice,” hence where no choice is possible meaning cannot be involved, though there may be what Bazell calls “semantic tie-up.” Both of these misconceptions have been attacked by the transformationalists, according to Bazell, but they have done nothing to dispel the third, which consists in the failure to distinguish “ungrammatical” from “non-grammatical” sentences. Examples of the first are *He seems sleeping* or *When he will come, I will see him*; an example of the latter, *Colourless green ideas sleep furiously.* This distinction is “intuitively obvious to all fluent and sensitive speakers of a language.” The main body of the paper is an attempt to characterize this difference, and to correct certain misleading ways in which it has been presented. According to Bazell, an ungrammatical sentence is “corrigible” in the literal sense. (For instance, in correcting homework a language teacher can always write in the margin a grammatical version of the ungrammatical sentence and usually cite a rule which has been violated. Faced with a non-grammatical sentence he can at best ask the student what it was he was trying to say.) “An ungrammatical sentence is one which is replaceable, *salva significatione*, by a grammatical sentence.” The distinction is raised to another level of terminology by introducing a distinction between *grammatical constraints* and *grammatical restraints*. The former are involved in ungrammatical sentences. An analogy with chess (rules of the game and rules of strategy) is drawn. At best, it would seem, the distinction must be relativized at present to a particular description. A borderline case, such as the one offered by Bazell—*He is seeming good*—might be ungrammatical in Mr. A’s description of English but non-grammatical in Mr. B’s description.

In “Syntax and the Consumer,” Michael A. K. Halliday begins with the question: Do the various aims for which a description of language may be used “presuppose different ways of using the same description, or are they best served by descriptions of different kinds?” He mentions with approval recent insistence on discussing the goals of a linguistic theory, but defends the view that “different coexisting models in linguistics may best be regarded as appropriate to different aims, rather than as competing contenders for the same goal.” This plea for tolerance is balanced by a brief presentation of some notions of Halliday’s own system, called by him here a “scale-and-category” grammar. The discussion is not quite understand-
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able by itself, but can be amplified by Halliday's presentation in Word XVII (1961), 241–292. The main alternative approach mentioned by Halliday is that of transformational grammar. Halliday characterizes the work of his group as follows: its aim is "to show the patterns inherent in the linguistic performance of the native speaker" (as opposed to a characterization of his linguistic competence), the analysis "should idealize as little as possible, in the sense of excluding the minimum as 'deviant'." To do this "presupposes a general description of those patterns which the linguist considers to be primary in the language, a description which is then variably extendable, on the 'scale of delicacy', in depth of detail." The main argument is based on the concept of 'delicacy' which "proves useful in providing a means whereby a linguist analysing a text can select a point beyond which he takes account of no further distinctions and can specify the type of relation between the different systems in which he is interested." Halliday claims only that his view of linguistic analysis is appropriate—but not more appropriate than other views (p. 23)—for various purposes. In the face of such diffidence, it is difficult to say anything critical at all.

The next two papers represent what has been called "functional syntax." The approach may be described as being generally in the area of "universal grammar." It attempts to understand the workings of language by starting from a presumed general framework of the communication situation and seeks to epitomize the typical devices used by the linear system of language to communicate experience. In the first of the two, "The Foundations of a Functional Syntax," the principal proponent of this view, André Martinet writes, for instance, "Any linguistic communication implies the use of . . . a succession of speech segments each corresponding to one aspect of the total experience, and the basic linguistic problem is to determine how the hearer is in a position to piece them together" (p. 26). Martinet concentrates on three types of devices. First, "segments corresponding to one aspect of the experience to be communicated may suggest not only that aspect, but also the relation of that aspect to the rest." Second, relations between aspects of the communicated experience may be made explicit by "placing the units in such an order as to suggest the nature of their mutual relationships." Finally, certain units may be used "for specifying the nature of the relationship between a given element and the rest of the utterance." Martinet goes on to draw various conclusions from his view of the nature of linguistic inquiry, for instance, on the difference between grammar and lexicon, on the advisability of using his term "moneme" (rather than "morpheme") to refer to the minimal significant unit in language since there is no neat way of identifying these minimal units with particular segments, and so on. In general, one wonders whether very much can be
accomplished in trying to answer these important questions on the basis of such informal and vague primitives as 'aspects of the communicated situation,' 'relations between aspects,' and so forth. All in all, Martinet's paper is what the editor, following Bertrand Russell, calls a "heroic" solution as opposed to painstaking thought and careful [logical] analysis.

In "Functional Syntax and Syntactic Operations" A. G. F. van Holk proposes "to take up the analysis of syntactic structure from a 'functional' standpoint" (following Martinet's understanding of the term) and to derive "new functional dimensions from a given set of primary functions." The author gives as his three primary functions (along with a fourth: intonation) the lexical function, the time of the utterance, and the actualising function. Using special symbols for these—at best vaguely characterized—functions, the author proceeds to describe various derived functions.

"A Chapter on the English Verb" by Martin Joos represents a compressed version of the results reported in Joos's monograph on the English verb (Madison 1964). Typically Joosian, the paper is a rather strange combination of startling assertions, embarrassing divagations, and interesting ideas. As an example of the first two: "no linguist has a duty to believe everything that another linguist has conclusively proved ... various linguists start out from different sets of axioms, and ... de gustibus non est disputandum." "When it comes to name-dropping, as my acquaintances know, I am not one to let modesty delay the proceedings." As an example of the third, the notion that meanings can be additive or privative and that grammatical items (as opposed to lexical ones)—in particular, the grammatical morphemes of the English verb phrase—are purely privative in their contribution to the meaning of the sentence. Joos's remarks on the meanings of the passive, progressive, perfect, past, and modal constructions (with the positing of three distinctive features of meaning associated with the eight modals) are suggestive, but it is necessary to damp out a good deal of noise to hear the suggestions.

Like the two papers on "functional syntax" the two papers by H. A. Gleason, Jr.—"The Organization of Language: a Stratificational View"—and by Sydney M. Lamb—"On Alternation, Transformation, Realization, and Stratification"—should be read together, even though they are separated by another. Gleason's paper is devoted to a general presentation of Lamb's "stratificational linguistics" while the scale of Lamb's paper is somewhat smaller. In both, various claims are made for stratificational grammar as compared to other models (the "Bloomfieldian" and "transformational-generative" models in Gleason, mainly the latter in Lamb's contribution). Lamb has presented various facets of his system before in lectures but aside from a semi-official publication of a pedagogical manual
there has been little in print on which to base an appraisal of his ideas. Hence, the inclusion of these papers is to be welcomed, even though they are slightly redundant. The basic hypothesis of stratificational grammar is that a language is best described as a system of four strata: sememic, lexemic, morphemic, and phonemic. Every utterance is represented by certain abstract combinations of objects on each stratum. There are two basic kinds of rules: those dealing with the representation of utterances within each stratum, and those which map a representation in one stratum into representations on the next one "down" (or conversely depending on the "direction" in which one is going). Parallel sets of terms (built with the suffixes "-on" and "-erne" and the prefix "allo-") are suggested for the various units and components of these abstract objects at each level. Since the claims made for this system are quite broad and the criticisms of other approaches (in particular transformational theory) are quite sharply stated I feel justified in making an extended critique. I shall however defer this to the end of my survey of the contents of the papers.

In "The Role of Paraphrase in Grammar," H. Hiž presents a tightly organized discussion of grammatical ambiguity, paraphrase, and an explication of one meaning of "meaning." Hiž first excludes from discussion questions of meaning in the strong sense (just what is said, what is denoted, truth values), then "occasional ambiguity" ("It's cold is sometimes true, sometimes false"), and dictionary ambiguity ("Springs are useful"), leaving open the question whether all instances of the last can be reduced to grammatical ambiguity. He shows that the notion of ambiguity presupposes (or at least can be explicated by) the notion of paraphrase: a sentence is ambiguous if and only if there are two sentences which are paraphrases of it, neither a paraphrase of each other. In grammatical ambiguity, the paraphrases contain only words already appearing in the ambiguous sentence together with grammatical constants—inflections, function words, etc. It should be stressed that this explanation depends on having some independent way of distinguishing grammatical constants from other items (hence, the openness of the question mentioned above). Starting from this basis, Hiž states five theorems about "paraphrastic sets." The paper—which I cannot paraphrase without taking up too much space—ends with a hypothesis to the effect that "every pair of paraphrases is used in explaining some ambiguity." Starting from a very small base—the notion of synonymy as an empirically given relation between sentences—Hiž works his way up to an explanation of "mood" as a partitioning of the readings of all sentences in a language, that is, a suggestion about basic sentence types (which includes the usual more narrowly conceived notion of grammatical mood as one instance).
In ""Sequence' and 'Order'"" F. R. Palmer starts from two quotations from Firth in which the two terms are distinguished, tries to give them clearer meaning than can be extracted from Firth, and discusses various problems that arise from the distinction. For Palmer (following Halliday) ""order"" refers to some relationship in the linguist's constructs (formulas, rules, statements about constructions, etc.) while ""sequence"" refers to the linear progression in time of the ""observable speech events."" (I am not at all sure that the relationship identified with ""order""—that is, the linear sequence of abstract elements in the linguist's constructs—is a ""fair interpretation"" of Firth's idea. It seems more likely that Firth's words—""elements of structure . . . share a mutual dependency in an order which is not a sequence""—refer also to such things as order of constituency, or dependency relations much like Bloomfield's ""structural order.""”) One can restate Palmer's distinction as follows: suppose we have two or more levels of description each with its set of elements and a concatenation operation. Call the order relations defined by concatenation on the ""lowest"" (phonetic) level ""sequence,"" those on higher levels ""order"" (this is already an unnecessary distinction). To what extent will the linear relations among elements on one level and their correspondents on other levels (in particular, the lowest level) conform in the sense that if \( x \) and \( y \) are two elements and \( r(x) \) and \( r(y) \) their correspondents on another level then (with ""\&"" symbolizing concatenation or any other order relation we might care to define) \( r(x \& y) = r(x) \& r(y) \). Palmer shows that strict conformity is unworkable, that the decision to dispense entirely with order relations on higher levels is absurdly uneconomical. He then shows the untenability of two positions: first, to leave unstated the relation between orderings on the various levels, second, to introduce various elements—e.g., morphemes—whose only realization (""exponent") is order, or to introduce special categories (e.g., subject) which can be characterized completely by order in a particular structure. Finally, Palmer criticizes transformational theory for obscuring the problem of the relationship between ""order"" and ""sequence."" It is not clear to me why one should expect a general answer to the question: ""To what extent is order determined by sequence?"" As long as one does not insist on strict conformity, particular decisions will be taken for one or another case. Thus the departure from ""sequence"" in descriptions of the English auxiliaries and their cooccurring morphemes is well motivated by considerations of simplicity. The only general answer that can be given would be something like this: the linguist lets order conform to sequence except where it is more convenient not to do so.

The remaining papers (except for the luncheon address by Ferguson) are concerned with ""Subject-Matter Relations between Linguistics and other
Disciplines." The intent of such papers is surely laudable. One of the ways
of testing linguistic theories is to see how they agree with, contribute to, or
build on other theories (this in opposition to the strange view that linguistics
should remain as pure of contaminations from other fields as possible). In
the first, "The Biological Background of Man's Language," Jarvis Bastian calls attention to the "semantic openness" of human language as
an important factor in explaining the jump which occurs between other
systems of animal communication and language. Bastian discusses the
notion of "analysis-by-synthesis" as a fruitful explanation for the problem
of language perception. He suggests that the openness of human languages
depends on or is concomitant with the fact that language is self-feeding (to
coin a term), that is, new items can be introduced and explained which refer
only remotely to direct experiences, conditioning, and so forth of the in-
dividual. In "Linguistics and Speech Behavior," Dr. Donald S. Boomer
makes a plea for attention by linguists to questions of linguistic perform-
ance, to more and better use of laboratory techniques, and to the use of
quantitative, in particular statistical techniques for testing and arriving at
their results. One can hardly quarrel with such a plea, but it must be men-
tioned that it is defensible to argue that the distinction between linguistic
competence and linguistic performance can be fruitfully maintained (this is
merely to argue that algebraic and statistical models are both relevant to
the whole job of linguistics). In "The Development of the Brain and the
Evolution of Language," Norman Geschwind presents a brief sketch of the
anatomy and physiology of the brain followed by arguments for his main
thesis: "the ability to develop language in man probably depends on his
ability to form cross-modal associations between two non-limbic modal-
ities" (e.g., visual to auditory, etc.; emphases in original omitted here). This
idea—the technical details of which I am not competent to discuss—is
based among other things on the fact that a monkey cannot carry over a
distinction learned visually, say between a circle and a cross, into the cor-
responding tactile distinction. To say that a human subject's ability to do
this quite easily depends on his language ability (the subject recognizes a
"circle" in both tests) is according to Geschwind to put the cart before the
horse. Rather, the naming function depends on the ability to form the
association. Geschwind presents evidence that the exceptional develop-
ment of the inferior posterior parietal region in man is admirably suited to
explain this ability, since this area is "ideally located" to form a "way-
station" mediating between the association cortexes for the three non-
limbic modalities of vision, audition, and somesthesis. Geschwind's paper
is modest in its claims and ends with a call for further research, particularly
into the problems of the neural mechanisms associated with grammar-
learning (his discussion is exclusively devoted to naming). J. F. Glastra van Loon’s paper, “Language and the Epistemological Foundations of the Social Sciences,” is a very general discussion of the problem in the social sciences which arises from the fact that the agents studied by the social scientist have their own conceptual systems by which they interpret their behavior. (This problem has been pointed up in recent discussion of the extent to which a linguistic description should account for the “intuition” of the native speaker.) Van Loon argues for an approach which recognizes the difference between, say, chemical compounds and people, which attempts to construct a metatheory that can receive particular interpretations in the intrinsic referential systems of the societies (speech communities, etc.) being studied. There is little discussion in the paper of problems peculiar to linguistics among the social sciences.

In the second luncheon address, “On Linguistic Information,” Charles A. Ferguson talks “informally . . . about some practical considerations” involved in gaining, applying, and making available “linguistic information” on such topics as the number of languages in the world, useable descriptions of (ideally) all of them with particular attention to certain ones that are important for various reasons.

I return now to some comments on Gleason’s and Lamb’s papers on “stratificational” grammar, with particular attention to the claims made for them as compared to transformational grammars. To recapitulate: a stratificational grammar consists of four strata: sememics, lexemics, morphemics, and phonemics. Within each stratum there is an inventory of elements and components and a set of tactic rules defining the structures associated with utterances on that stratum. For instance, the phonemic stratum (or “subcode” in Gleason’s terminology) has elements called phonemes, each a combination of phonons (i.e., distinctive features), junctions, and so on. Linking each adjacent pair of strata (in the order given above) there is a set of rules called variously “realization” and “recoding” rules. The types of structural descriptions given to an utterance on each stratum (again in the order given) are “networks” (from diagrams on p. 83 and elsewhere and the rather vague discussion, apparently finite directed graphs), “trees,” “strings,” and “matrices.” The realization rules are, then, mappings of representations on one stratum into representations on the next stratum.

A good deal of this is simply a new set of terms for old concepts. Linguists are notoriously “terminus-freudig” (to coin a barbarism), Lamb and followers perhaps more than most. Thus, in 1955 Chomsky gave a precise and general characterization of the notion of linguistic level which covers exactly the same ground as the notion of stratum and even includes explicit
mention of mappings from one level to another (realization rules). The authors attempt to justify new terminology by the need for parallel terms on different strata. Occasionally the justification exhibits serious misunderstanding. On p. 78 (fn. 8) Gleason rejects the term "distinctive feature" on the one hand because of its "phonetic implications" (in line with the curious view that phonetics is not a part of linguistics) and on the other hand because of the binary interpretation given to distinctive features in Jakobsonian theory. But this bit of discussion fails to keep clear the difference between classificatory features and strictly phonetic features. The former are necessarily binary because they are a way of dealing with classes of sounds (and not because of "neuro-physiological" arguments). That is, a given rule in the phonological component of a grammar either does or does not apply to a particular phonological unit.

Beginning with Chomsky's "Logical Structure of Linguistic Theory" in 1955 (unfortunately it is still unpublished, but enough has been extracted and published in sometimes improved form to make its major results readily available), an impressive body of results in transformational theory has been accumulated. These results may be broken down into three major areas: the construction of a formalized general linguistic theory; the creation of a rigorous mathematical study of grammars and languages in the abstract, a study which has been subsequently tied in with results in abstract machine theory and related areas of pure mathematics; the application of the general theory to many languages including notably English (but numerous other languages are being intensively studied from this viewpoint). Many problems remain, and every few months new results or revisions are reported. Various inadequacies in "traditional structural linguistics" (one is at something of a loss for an appropriate label) have been pointed out, in particular, the failure of traditional phonemics to accommodate various natural generalizations about sound structure, the failure of traditional IC analysis to account for certain properties of natural languages, the failure of traditional phonemics to justify a level of biunique phonemes, and so on. One would expect that new theories, claims, or approaches would make some explicit reference to these results, or substantive arguments against them. Yet Gleason and Lamb make no such arguments and attempt to justify their claims on the basis of future results of stratificational grammar. The proponents of transformational theory have been continually forced to restate various alleged alternative theories in such a way as to make a comparison possible. One such comparison was made by Paul Postal in his Constituent Structures (Blooming-
dismissed by Gleason with the statement: "Postal's understanding of stratificational grammar completely misses the point. Lamb's 'representational rules' . . . do 'affect generative power' . . . precisely because they do 'map one type of representation into another.'" But this is no argument, merely the assertion of a thesis. I can just as well dismiss any statement by saying that it isn't so. It seems rather obvious that the burden of proof remains with Gleason and Lamb.

A general linguistic theory must provide a precise characterization of the form of grammars which is as narrowly restricted as is compatible with known properties of natural languages. It must make explicit the way in which grammars of particular languages "describe" those languages in the specification of well formed sequences of phonetic elements, and in the assignment of structural descriptions to the generated sentences and sets of "readings" or interpretations to each pair of sentences and structural descriptions. It is fair to say that transformational theory has successfully met these requirements in an initial way (it would be ridiculous to object to the fact that it has changed rapidly since its inception). Moreover, despite statements to the contrary (in the exchange between W. O. Dingwall and Gleason, pp. 136-137) this theory has been largely and successfully "formalized" in the only meaning of that word that I understand (i.e. axiomatized), and related at least partially to the abstract study of grammars as formal systems. It would seem to be self-evident that any proponent of a rival theory would have to show first that his theory was really different from the one he is attacking. Neither Gleason nor Lamb does this in a sufficiently precise way to test their claims. Assuming for the sake of the argument that this necessary condition is met, a rival theory must show that it explains certain facts better, or explains the same facts with a narrower—i.e., stronger—set of hypotheses, or that it explains certain facts that the rival theory cannot explain. In attempting a comparison, Postal was forced to explicate the realizational rules of Lamb's system in one of two ways: either as finite transductions of structures (in fact, strings) on one stratum into structures on another stratum—in which case it follows that stratificational grammars fail in the strongest possible sense as theories for natural languages (since the entire system is then simply a context-free grammar)—or as unrestricted rewriting systems—in which case stratificational grammars fail in the other direction since they become merely equivalent to Turing machines, i.e. arbitrary computer programs for a potentially infinite automaton. It is significant that it was left to the critic to interpret the system in such a way as to make a comparison possible at all. As indicated above, Gleason simply dismisses this argument with no discussion. Lamb does not even mention it.
What then are Gleason's arguments for stratificational grammars over transformational grammars? (1) Gleason has not yet been able to write a successful transformational grammar (!). (2) "A stratificational grammar can provide a workable basis for understanding and formalizing the process of both transductions through language [i.e., producing and interpreting sentences]. A transformation-generative grammar has insuperable difficulties with one of the two." Even if the latter statement were true—and it is not; cf. Miller and Chomsky in Handbook of Mathematical Psychology II (1963) and recent work by Petrick—it is impossible to check this claim for stratificational grammar (as with most other blank-checks drawn by Gleason on future successes of stratificational grammar). (3) "A stratificational grammar can provide a simple and natural explanation for such phenomena as anaphora, and so for many of the grammatical features extending beyond the sentence." To draw any conclusion from this statement it is necessary to show that a transformational grammar cannot explain such phenomena (and not that it has not yet dealt with such—even this is untrue; the reviewer has suggested rules which take into account contexts beyond the sentence boundary (Language XXXVIII [1961], 268); moreover, as Katz and Fodor have argued, every such relation between sentences can be matched by a relation within the sentence. (4) Problems of cross-classification. Once again Gleason says merely that they can be handled easily by a stratificational grammar without giving any evidence. And again it is necessary to ask why one should prefer a complete junking of transformational theory to a revision (as currently underway). (5) "For a reasonable and useful degree of comprehensiveness, a stratificational grammar will prove (!) simpler and clearer than a transformational grammar." Blank checks again.

If we disregard the sememic stratum, there seems to be no reason to withdraw Postal's claim that a stratificational grammar is weakly equivalent to a context-free phrase structure grammar. From Lamb's discussion it is apparent that the realization rules linking the various strata do not affect the generative power. This is clear from the fact that the sets of elements on each stratum are disjoint (p. 113) and the realization rules are all one-way (semolexic, lexomorphemic, etc.); i.e., the recursive power of the system is attributed entirely to the tactic rules (which are context-free). Moreover, although the realization rules do take contexts into account, in the proofs (and I mean proof in the strictest sense) that context-sensitive grammars are more powerful than context-free grammars it is necessary to use the capacity to change items back and forth from (in effect) one stratum to another repeatedly (although this capacity figures also in the demonstration that such grammars fail in strong generative capacity). Gleason
suggests that the realization rules form a counterpart to transformations (p. 90, fn. 34). But none of the examples given show that these rules incorporate the essential property of transformational rules that makes them transformational, namely the use of a structural analysis of terminal strings in terms of their related phrase markers. And if the realizational rules are so formulated in some future explanation of the system, we are still in need of the initial necessary premise to the whole argument, namely that stratificational grammars are essentially different from transformational grammars in their formal properties.

We are left then with the sememic stratum and (possibly) the semolexic rules. But beyond the statement that the structural descriptions on this stratum have the form of "networks" and several diagrams we are still completely in the dark as to the nature of the tactic rules on this stratum (assuming that they are different from those on the other strata) and the way in which they are used to specify the "networks." The entire evidence for the quite broad claims made for the power of this component (including the capacity to generate narratives) consists in a reference to a grammar that is not yet in publishable form. And even this "evidence" is qualified by the statement: "In addition, the best form of statement for the semotactic rules has not yet been established" (p. 94, fn. 40).

Most of the above has referred to Gleason's paper. Lamb's paper is concerned mainly with phonology and can be dealt with rather more briefly. Lamb makes a great deal out of the fact that the phonological rules of published transformational grammars are ordered. Actually, they are partially ordered (e.g. in Halle's *Sound Pattern of Russian*). As a matter of fact, Lamb's rules are also partially ordered (p. 112: "a given subrule applies only if none of the preceding does"). If Lamb insists that an integer be added to each "mutation" rule (his term for the rules of transformational grammar, chosen to support his argument against the straw man of ordering as a reflection of ordering in time), then he must also add an integer to each of the subparts of his rules. But the whole idea that the use of ordering—simple, cyclical, or partial—can be supported or attacked in terms of the number of symbols used is misguided. Empirical evidence must be brought to bear on the question. Such evidence has been presented for the use of cyclical orderings in phonological rules and for the "reality" of other orderings in phonology (e.g. differences between dialects of the "same" language which result from different orderings in the rules). This evidence, including the whole discussion of rules of stress assignment in English and parallel situations in other language, is ignored. Similarly, the arguments against a separate level of "taxonomic phonemics" (reflected in Lamb's distinction between "morphons" and "phonemes") is not mentioned. Finally,
Lamb discusses at length the fact that phonological representation by means of distinctive features is introduced early in the rules of a transformational grammar and inadvertently gives examples toward the end of his paper which illustrate precisely why this specification is given (to specify the classes of segments on which various rules operate without using designations like "(p, t, k)", p. 117; this is also of course why they are binary).

I have tried to keep this discussion on a fairly calm level of substantive argument. It must be mentioned, however, that the two papers and associated discussion are couched in a language which is highly polemical, often condescending and even insulting in tone. It would be possible to deal point by point with various further unsupported claims and misreadings of the literature, but that would extend a discussion which is already too long. Until something more substantial is offered, it would seem that the claims of Lamb and Gleason can be safely ignored.

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This is an enormous volume. There are 102 contributions, ranging in length from a page-and-a-half or less to 23 (Janssen’s on “The Acoustics of Consonants”) with a majority of the papers in the range of 4–8 pages in length. Eighty-one papers are of 10 or fewer pages; only 9 are 17 or more. In a way this brevity is a help to the reviewer; if someone really has something to say, he can’t keep you from finding this out fairly soon. In another way it’s a nuisance because it has left the way open for scholars to make ambitious programmatic utterances which scarcely deserve perpetuation in print. The range of quality is great, but a very respectable portion of the contributions are very good, giving strong evidence that the field is really alive (even in the U.S.), and that numbers of old falsehoods are being cleared away and old puzzles drawing near to solution.

Obviously it is impossible for me to discuss, or even list, all the contributions. So after a little statistical information, I will make comments on a
few of the pieces, not guaranteeing thereby that these are the best, but only that they happened to interest me.

The classification A, Acoustic and Physiological Phonetics (3–310) comprises 29 articles, of which 15 are concerned mainly with acoustic data (using the sound spectrograph most often, but various other instruments as well), 12 with photographic studies, chiefly x-ray movies, and 2 using only other methods of observing articulation or muscular movements; section B, Psychological Aspects of Phonetics (313–399), contains 11 contributions (several of which could equally well have been included under A), dealing chiefly with hearing and with speech pathology; C, Phonetics and Phonemics, seems to be subdivided into two subseries, the first (403–518) containing 11 papers of a somewhat more general or theoretical nature, the second (519–815) 51 papers concerned more with particular languages. I may be quite wrong about the basis of the subdivision of C; the editors do not give a clue, and some of the papers in the last half-volume might very well have been classified under A or B. It may or may not be significant that the last alphabetic series includes 51 papers, exactly half of the total.

Of all these papers, 65 (including 27 from non-English-speaking lands) are in English, 20 in German and 17 in French. The only non-French-speaking country which can be said to favor French is Rumania (3 to nothing), while German is slightly ahead in Poland (2 to 1 over English) and Czechoslovakia (2 to 1 over French). The heavy pro-English delegations are Sweden (9 to 1 each French and German), the Netherlands (6 to 0), Finland (4 against 1 German) and Soviet Union (3 to 0). The one French paper from the U.S. is Pierre Delattre's, and the only German one is Herbert Penzl's.

Now for my comments on individual papers. Peter Ladefoged has an interesting contribution (73–91) on "Sub-glottal Activity during Speech." This is in the main a summary of previously reported research, ultimately inspired by Stetson's theories, most of which are shown to be incorrect. The chief points are quoted here: "Two separate phenomena . . . can be correlated with the bursts of intercostal activity: one is the increase in the rate of flow of air out of the lungs which occurs . . . before nearly all [h] sounds" (79). "In normal conversational English the abdominal muscles are in action only at the end of a very long utterance" (81). "There is a fairly linear relationship for any one vowel between the intensity in db and the logarithm of the subglottal pressure . . . a given pressure between 10 and 30 cm aq will produce an /a/ which has a little more than 5 db greater intensity than an /i/ or an /u/ produced with the same pressure" (84). "The subject sat with his eyes shut, and tried to maintain a constant note while one of the experimenters pressed against his chest at unpredictable
moments. . . . The fact that there is no delay between the fundamental frequency changes and the sub-glottal pressure changes indicates that there must be a direct link between the two” (86). (This, incidentally, helps to show why stress or emphasis naturally involves both greater amplitude and higher pitch.) “Thus the examples of bee which have sub-glottal pressures of 18, 19, 20 and 24 cm aq are judged as being equally loud as the corresponding examples of bar which have similar pressures, despite the . . . difference in intensity of at least 5 db” (89). (In short, it is not amplitude but inferred muscular effort which is judged.) “We do not find a peak of sub-glottal pressure corresponding to each syllable. . . . So it seems reasonable to conclude that there is sub-glottic activity which may be correlated with phonetic stress; but there is insufficient basis for a chest pulse theory of the syllable” (91). I would make that stronger; the chest pulse theory (as Stetson proposed it) is clearly incorrect.

Björn Lindblom’s report on the “Accuracy and Limitations of Sona-Graph Measurements” is very interesting and informative, particularly for those of us who are familiar with the work of Fant and his colleagues. Of the specific points made, the most interesting is the proposal to supply an inventory of standard envelopes, so that total spectrum-matching may take the place of independent estimation of \( F_1, F_2, F_3, \) etc. The discussion afterward (with Eli Fischer-Jørgensen) makes clear that there is no important difference between correct pole frequencies and correct peak frequencies.

Arne Risberg’s report on “Fundamental Frequency Tracking” (227–231) is excellent and valuable, but defies summary and needs no comment from me.

Two papers mention the problems implied by the randomness of the distribution of vowels (recorded in normal conversation) in the \( F_1-F_2 \) plane. The first, by J. N. Shearme and J. N. Holmes (234–240), bears the clear if unwieldy title “An Experimental Study of the Classification of Sounds in Continuous Speech according to their Distribution in the Formant 1–Formant 2 Plane”; the second by H. Mol (320–329) is entitled “Aural Stimuli and their Interpretation.” The two papers start from very similar collections of data; where they go is surprisingly different. Fig. 1 on 233 is strikingly like Fig. 2 on 322, even though Mol plots period length (i.e., the reciprocal of frequency) rather than frequency for his formants. The numbers are illegible on Mol’s figure, but I conjecture that the lower left is the [a] region and the upper right the [i] region, which is a combined reflection and rotation from the arrangement on 233; even so, they look very very similar because the two regions mentioned are in any case the most thickly populated, with the space between, as well as the other corners, quite sparse. What Shearme and Holmes do about this is to propose a device for marking
vowel areas (the smallest closed curve which includes some part of the
track of every token of the vowel in question) which reduces (but does not
eliminate) the overlap; to study a number of charts prepared in this way;
and to draw two conclusions: (1) [237–238] “The presently accepted for-
mant frequencies of vowels (... measured from isolated monosyllables) are
not applicable to connected speech”; (2) [240] the “relationship between
acoustic properties and phonemic values ... not ... a simple rela-
tionship.” Mol links this up with his long-standing attack on accepted methods
of considering the acoustics of speech. “The lesson ... is that the phonic
data the ear extracts from the sound waves do not form the only source on
which the listener bases his identifications [325].” “One reads ... that fre-
quency, intensity and time are the physical components or parameters of
speech sounds, as if the Fourier components were physical realities in-
deed [328].” “We have a hunch that in running speech the consonants form
the cues for identification of the words rather than the vowels which, as it
were, are filled in by the listener on basis of interpretation [328].” Here we
would do well to distinguish clearly two cases: (1) when the speaker and
listener are dealing with familiar words and combinations only; (2) when
the speaker is making use of a rare or novel word, at least one that seems
rare or novel to the hearer or one that is very easily confused with another
similar word. Case 2 occurs particularly during introductions: “Mr. X, this
is Mr. Y,” where we may often encounter names outside our previous
acquaintance or pairs of names which are very much alike (e.g., “Sterling”
and “Sturley,” “Bock” and “Buck,” “Graham” and “Gram,” etc.). In this
latter case we may expect (a) careful articulation, (b) close listening, (c) re-
petition and requests for spelling. In case 1, however, a considerable amount
of overlap is not only tolerable but will normally pass unnoticed. It may be
true that it is generally the vowels that do so overlap, but this is apparently
due to the absence of any firm contact or sharp articulating boundaries, for
deliberate experiments with substitution of consonants have often suc-
cceeded in escaping the notice of listeners (above all, of course, in noisy con-
ditions where vowel distinctions are likely to come through better anyway),
and we are all familiar with the way in which speech defects such as lisps,
labial l’s, or f’s for ð’s may arouse only a vague feeling that something is
odd rather than a specific block to communication. So we should rather
say that both consonant and vowel features are “filled in by the listener on
basis of interpretation.” As for Mol’s point about the non-reality of fre-
quency, intensity and time, we must say that however the ear and brain
manage it, pitch and loudness are certainly perceptual realities—even
several simultaneous pitches. This is a bit like complaining that we shouldn’t
talk about separate wave-lengths of light in discussing color perception.
Certainly perception corresponds in no obvious way to an oscillographic representation of the pressure-time function, but does correspond in many obvious ways to a sound spectrographic representation.

In connection with the perception of pitch, A. J. Fourcin (355–359) gives an interesting report on binaural listening to white noise with delays, the gist of which is to give “support for the spirit, if not the letter, of Locklinder’s correlation theory.”

Morris Halle’s interesting paper on “Speech Sounds and Sequences” offers a theory of how we hear speech. Like most such theories this makes the assumption that we hear in terms of articulations in some sense (not necessarily implying sub-vocal movements or even innervation waves); and surely this is correct (as shown most strikingly by p-t-k discreteness for virtually all perceivers—though someone should study perceptions of labials by monolingual Iroquois). But like most other theories this one leaves much unaccounted for. Halle’s theory runs (432): “The signal under analysis is first subjected to a ‘preliminary analysis’ which includes various transformations that have been found useful in speech analysis, such as segmentation, identification of segments . . . etc. The fact that all of these procedures can be only partially successful is no bar to their utilization here since the aim of the preliminary analysis is not positive identification. After being subjected to ‘preliminary analysis’ the signal is sent to the ‘comparator,’ where it is processed as before. The results of the preliminary analysis are communicated also to the ‘control,’ which systematically supplies the ‘generative rules’ with the items not excluded in the ‘preliminary analysis’ and prints out the phoneme sequence resulting in the closest resemblance with the signal under analysis.” It is not clear from this account that there must also be a dictionary look-up step included somewhere here, otherwise all we would get would be the phonologically most probable match, when in fact we get the semantically and syntactically most probable on the first try, even when it is only the tenth most probable on a purely phonological basis, except in rare and difficult cases. This model is very possibly appropriate for certain special situations: for instance, where we are trying to identify the words of a new song (here phonology clearly outweighs communication) or listening to snatches of conversation in an inadequately known second language whose phonology we have under good control. Let us list some facts that must be taken into account: (1) as Halle notes, in rapid speech entire segments or syllables are often completely omitted, and their omission is usually not noticed by either speaker or hearer; (2) under the same conditions of rapid speech, it is sometimes impossible for non-native speakers, even skilled phoneticians, to be sure of the order in which the phones occur, even if by chance they get the number of segments and their
identities right; (3) some of the oddities of children's speech, including distant metathesis of features and segments (cf. A. Avram's contribution, 343–346), point the same way; (4) although the fact has often been glossed over by linguists, it is nevertheless a fact that many speakers have unique pronunciations of individual words which do not easily fit into the analysis (whether phonemic or feature), e.g., in my own dialect the word pretty, some sort of [prti] with a retroflex stressed syllabic, does not rhyme with dirty—or any other English word—nor does its first syllable rhyme with any other; in my brother's speech the word last has a nasalized vowel, but nevertheless differs from lanced and rhymes with nothing else. (5) All of us have encountered people who steal the words out of our mouths; an acquaintance of mine on the faculty here at Bloomington can speak three or four segments behind you and scarcely ever make a mistake. Most of us perform the same feat silently; that is, we not only correctly identify words and phrases after they're spoken (as Halle's model allows) but in large measure before they're spoken. Some very reliable predictor mechanism must also be incorporated into any satisfactory theory of speech reception. (6) Presumably the intonations of a language are entered in a different lexicon than the morphemes or words (or whatever the strings are that Halle's device matches); how then is it possible to have Canary Island or Pyrenees whistle speech (see 533–546), or American vaudeville fiddle talk, in which whole conversations can be very well understood with no clues other than the rhythm and the intonation? I think the basic difficulty here comes out in statements like this (431): "It would have to contain a dictionary so large as to rule it out as a plausible model of speech perception." We often assume that the brain is like a very large computer with maybe a bigger core storage—4,000 K perhaps instead of 32 or 64 K. But is this really credible? Considering all different kinds of memory, surely $10^{64}$ bits is closer. In some sense we remember a vast deal of all we have heard or read in our whole lives, and for certain purposes have extremely rapid access. Given this fact, is a perception algorithm in fact more economical than straight dictionary lookup? It seems hardly likely. The only reason we work away frantically at syntax algorithms for MT purposes, for instance, is the limitations on rapid access storage; if our brain has practically unlimited rapid access storage, it would be very uneconomical to resort exclusively to a listening algorithm under ordinary conditions. I am convinced we do have such a device, but I am also convinced that we rely upon it only sporadically—approximately every time we say "I'm sorry, what's that again?" or "How do you spell it?"

On 429, Halle cites his son's use of the form /aksiz/ as evidence that he "must be able to analyze the noun into its component phonemes." Of
course this proves nothing of the kind, since only the final sound is relevant, and segmentation is unnecessary to locate that. When Halle (432–433) mentions American English *balm* vs. *bomb* and German *reisst* vs. *reist* as examples of overlapping allophones, I am quite puzzled. For speakers who do not distinguish *bomb* from *balm*, what is to be gained by pretending they are phonemically distinct? Unless Halle is prepared to argue that all homophones must have distinct “basic” or “morphophonemic” (to us) or “phonemic” (to him) representations, or perhaps phoneme is to be equated with *diaphone* so that all pairs distinguished in any one dialect must be phonemically different in all. Certainly the German examples can be regarded as morphophonemically or basically *raizt* vs. *raist,* but it is a mere equivocation to speak here of overlapping allophones.

Bertil Malmberg (456–475) gives an interesting discussion of accent, primarily dealing with Swedish and the interactions of word accent and sentence intonation, but also with the differences between subjective perception and instrumental analysis, of which the most striking illustration is the Ibo word *àkwa* (fig. 21) with rising pitch on the first syllable (about 80 to 150 cps) and falling (165 to 75 cps, or so) on the second, and which is perceived as *high* first and *low* second syllable. In the end (474–475) Malmberg plumps for a reduction of “*le nombre de types mélodiques* . . . “ à quelques deux, trois ou quatre invariants, ayant chacun un nombre illimité de variantes contextuelles, stylistiques ou autres” without analyzing them into sequences either of pitch levels or of accents—except in a subphonemic sense.

J. M. Cowan’s experiments with perceived speech (567–570) should throw some light on the way in which pitch movements are perceived by listeners; unfortunately Cowan reports no results, but only procedure and equipment.

Jacques Allières makes an interesting contribution called “Aspects géographiques et diachroniques de la phonétique: le Polymorphisme.” What Allières calls polymorphism is the existence of strikingly different free variants in a local dialect or even in the speech of a single individual. He distinguished two types, one of which represents a phonological change in progress, the other dialect mixture after the change is complete. He criticizes linguists like Dauzat who attribute such variations in dialect atlas records “à l’inexpérience des enquêteurs ou à l’imperfection de leur ouïe.” I should myself be strongly inclined to this type of explanation in some instances of fluctuating recordings of vowels, though I should call it rather “distorted relation between speaker’s basic vowel areas and investigator’s.” Some years ago I and a student of mine (Mr. George Motherwell) checked a field record made by an able and experienced dialect investigator with an **
excellent ear against sound spectrograms of the same material and our own auditory re-check of the sound spectrograms. This linguist had used the official Linguistic Atlas system, with diacritics allowing of about 500 to 1,000 distinct vowels, but in fact he distinguished only about 120. If these 120 were reduced to 20 by appropriate devices, it would be the case that his transcription was in error by more than a single step in any direction only about 25 per cent of the time, and by more than two steps only about 10 per cent. But if you kept the original 120 distinctions, the error rate would be far higher. And many of these errors would be indistinguishable from cases of Allières’s type 1 polymorphism.

Still, not by any means all such variations can be eliminated by this method, certainly not the clearest cases of type 2. And the traditional explanation of these, namely dialect mixture, is surely in some way correct. This is not to suppose that there is ever anywhere a pure unmixed dialect, but on the basis of a smaller number of such postulated pure dialects, a much larger number of real (but impure) dialects can be satisfactorily explained.

Allières’s assumption (530) that scribal fluctuations in spelling are all faithful reflections of the scribe’s own polymorphisms of speech strikes me as simply fantastic. What are the most characteristic variations from any period in any language? As far as we can tell some of them are purely orthographic and could not reflect any phonological difference at all; some are due to similarity of very frequent words, as when we accidentally put “there” for “their” or “the” for “they” or “on” for “of” or “in”; some of them are morphophonemic, and reflect variation between a tendency to fixed spellings and a tendency to distinguish sandhi forms in writing; others are usually (and as a rule correctly, I believe) interpreted as reflecting a variation between a traditional spelling and one based on the scribe’s pronunciation. The truth is that most writing systems are very poorly designed to express phonological developments, even if the scribes had such a wish. So when Allières says “nous prêtons aux scribes médiévaux notre propre psychologie, en supposant qu’ils possèdent en eux un certain arsenal de normes fixes, apprises sur les bancs du collège grâce à un savant rabâchage d’ouvrages normatifs,” I find his irony misplaced. When Shakespeare uses 15 or so different spellings for his name, can we suppose an attempt to reflect accurately the same number of variant pronunciations? And in much of Europe in the middle ages there was indeed “un certain arsenal de normes fixes,” namely the spellings of Latin words. The difficulty comes in writing vernaculars with the same inventory of characters and without violating the pronunciation norms enshrined in the local standard for Latin. Nothing equivalent can be found in Greek epigraphy or papyrology.
or in Latin documents of the archaic or classical periods. When Etruscans first started using Greek characters, the conditions existed for something of this sort, but in general Etruscan seems to have needed fewer characters than Greek, not more. But the picture of a scribe who "transcrit textuellement les variantes polymorphiques telles qu'elles s'offrent à son esprit"—except where the variants are sandhi variants—strikes me as incredible at any period.

A fascinating experiment is described by A. Cohen, J. F. Schonten, and J. 't Hart, "Contribution of the Time Parameter to the Perception of Speech" (555–560), although the brevity of the presentation almost conceals the significance (and some details are even now not clear to me). Essentially their claim is to have synthesized acceptable speech without formant transitions, substituting amplitude transitions instead (expressed in terms of 3 "time parameters": \( t_1 \), which seems to be the time for amplitude to rise from zero to half the maximum; \( t_2 \), apparently the time from zero amplitude to the first decrease in amplitude; and \( t_3 \), the time from that decrease till the amplitude is again at half the maximum). If we think back to Pedro the Voder, on which quite intelligible synthetic speech could be produced, this is perhaps not surprising. I would, however, be surprised if this method would be equally satisfactory on all sequences; particularly I wonder about diphthongs. There is no doubt, however, that the "time parameters" used here (essentially rate of increase, duration of maximum, and rate of decrease) do play an important part, and their effects can often be mistaken for effects of change of spectrum.

Two interesting papers deal with the differences between checked and unchecked vowels (or fester und loser Anschluss), one by D. von Essen (590–595, with comments by Eli Fischer-Jørgensen on 595–597), the other by A. L. Fliflet (610–615). Von Essen's investigations show, interestingly enough, a correlation with parameter \( t_3 \) of the article just discussed: German checked vowels in his sample always have a measurably steeper drop in amplitude (as well as being shorter) than free vowels. Miss Fischer-Jørgensen's comment casts some doubt on von Essen's conclusions, suggesting in particular that other factors may be important also. Fliflet specifies length as the chief factor, after research with Norwegian dialects, Finnish, Hungarian, Japanese, Italian and a variety of other languages. The chief experimental method is stretching and cutting of (a) single stressed vowels, (b) intervocalic and tonal consonants. His conclusions are clean-cut and sweeping (614–615):

Unsere Perzeption der Konsonantenintensität, des Anschlusses und vor allen Dingen der Silbenteilung ist prinzipiell beeinflussbar seitens der Dauer des Konsonanten. Unsere Perzeption der Konsonantendauer wiederum, zugleich mit der der
Silbenteilung, der Konsonantischen Intensität und vor allen Dingen des Anschlusses, ist prinzipiell beeinflussbar seitens der Dauer des vorhergehenden Vokals.

He goes on to say that the perception of checked and free vowels depends solely on the relative length of the stressed vowel and the following consonant. This is only in apparent contradiction with von Essen's results; though the relative duration may be the essential factor, in real live articulation (as opposed to synthetic lengthening or shortening), it may indeed be the case that the amplitude drop is significantly different in the two types of syllable. Fliflet makes a few other interesting points. On 612 he notes that in languages with audibly released final consonant (e.g. French), if the stressed vowel is shortened enough (about 50 per cent in most cases) this release is reinterpreted as a second syllable. On 614: if the first vowel of a two syllable word with short intervocalic consonant (functioning solely as onset of the second syllable) is shortened enough, that unchanged consonant is reinterpreted as a geminate, acting as coda for the first vowel and onset for the second. The cuts in vowels were always made from the beginning (so that amplitude drop and other transition features were left unchanged), those on consonants in the middle (so that neither transition was affected). What is needed now is perception experiments to determine what happens if transition phenomena are varied while vowel and consonant length are kept unchanged.

Among the papers dealing with the phonetics (or at least the phonology) of long-vanished forms of speech, David Greene's on Old Irish 'velarization' (622-624) sounds eminently sensible: what the scribes wrote as part of the nucleus really was part of the nucleus.

In a very interesting brief contribution (625-629) W. Haas distinguishes between two kinds of relevance in phonology (and the same two types may also be found in syntax-lexicon, I make no doubt), namely the relevance of distinctiveness or contrast (which an earlier generation of Americans also called phonemicity) and the relevance of 'determinant power'—roughly the kind of relevance proper to suprasegmentals or prosodies. "Determinant elements are important as markers of syntagmatic regularities, never merely as bearers of syntagmatic contrasts." Interestingly enough, it is apparently only determinant elements that require labeled bracketing or 'grammatical prerequisites.' It's worth thinking about.

Lee S. Hultzen's paper on "Significant and Nonsignificant in Intonation" makes a few simple and obvious points which are too often overlooked (658). "It is only when the intonation denies the straightforward meaning of the words that it can be said to have any function." He means, of course, contrastive or distinctive function, not Haas's determinant power, which is always present in intonation. (661) "But then, much intonation is
quite wrong anyway, especially but not exclusively when the speaker is using somebody else's text."

Jorge Morais-Barbosa's arguments (691-709) for analyzing Portuguese nasal vowels as sequences are extremely telling, in spite of Hammarström's offhand dismissal. Essentially Morais-Barbosa argues on structural and distributional grounds, with speaker's intuition in second place, while Hammarström considers only the intuition argument as relevant. A generative approach might lead one to say that Morais-Barbosa is right at the "systematic phonemic" née morphophonemic level, but Hammarström at the "systematic phonetic" or conventional phonemic level. But the real problem is to verify conflicting statements about speakers' intuitions.

Petrovici's plea (723-727) for his analysis of palatalized consonants rather than rising diphthongs (and triphthongs) in Rumanian closes with a peculiarly apt quotation: "La question se pose si le phonologue doit ou non adopter dans ces interprétations les idées que se font les sujets parlants des phénomènes linguistiques. Ma réponse est négative." This is certainly the ideal post-Bloomfeldian response, but it's going to be a tricky business distinguishing between the native's subconscious intuitions and his conscious linguistic analyses, if such a distinction is indeed relevant.

André Rigault (734-748) gives an interesting study of accent or prominence in French, based mainly on jury reactions to PAT-synthesized words, of which the conclusion is that in French, as in English, Polish, and other languages, the principal acoustic factor in what subjects perceive as stress or loudness is higher pitch. Unfortunately he does not test possible differences in effect of high level, rising, falling, rising-falling, and falling-rising pitches; to judge from one sample he attempted to synthesize only "natural" rising-falling syllables (with the high point before the middle of the vowel). Malmberg's Ibo example cited above suggests that this is important. A paper by G. Faure earlier in this volume (598-609) hints at some of this information for French, but is not explicit enough.

Scardiglì's piece on Umbrian phonemes (another of the studies of extinct languages which we noted above) offers a proposed new phonetic interpretation of the (native) Umbrian letter customarily transcribed with Ḳ, namely [phabet], a voiced apico-dental fricative. This is in many ways an obvious theory; the letter represents a sound which developed in intervocalic position from earlier $d$, and examples of intervocalic $\delta$ from $d$ abound in European languages. The character partially resembles the Umbrian letter for $r$, partially the $d$; a voiced fricative can be thought of as intermediate between a liquid and a voiced stop. Scardiglì, however, does not leave it at this; he assigns the value $\delta$ to early Umbrian, and assumes a later change to $z$, voiced partner of the $s$ (which had earlier passed to $r$, via $z$, precisely in
intervocalic position). Since Umbrian clearly had a voicing opposition in p-b, t-d (even intervocalic d in a few words) and k-g, this has some plausibility. It is true that the other fricative f (the value of h is dubious, but at any rate it seems not to have been a fricative in postvocalic positions) appears to have had voiced and voiceless allophones, just as Umbrian s itself before the rhotacism. The evidence is complicated by the fact that in the later tablets intervocalic -s- appears not only for original -d-, but also for original -rs-, -ss-, -ts-, -ps-, -ks-, s- (after prefixes), -s (before enclitics), and palatalized -k- (native -q-, Latin alphabet -s- or -ś-), and most of these would seem to have been voiceless. In two or three words an original -s- looks as if it escaped rhotacism (and Scardigli believes that such words—e.g., asa “altar”—contained -z-). Maybe Scardigli's theory is right, but the evidence is slim at best. And against it appears some evidence that ŭ was somehow more r-like (see Buck §132a) or else more d-like (the fact that the sign transcribed ŭ is merely the unchanged earlier sign for d, while the sign transcribed d is the newly modified character). On the other hand the sequence -rs- (sometimes reduced to -s-) occurring in both old and new scripts represents original -rs-; later -rs- sequences, coming from -res- or -ris- or from -rss-, appear throughout as -rf-. This certainly implies that original -rs- (and -s- derived from it) must have been somehow different, most plausibly by being voiced. The similar developments of primary and secondary -ns- and -ts- suggest the preposterous hypothesis that -s- generally means [z], whereas [s]—as well as [f]—is sometimes represented by f, in both alphabets. Our verdict must remain not proven.

Kalevi Wiik (795–799) undertakes again the complex and unrewarding task of making sense of the distribution of phonemes in the F₁–F₂ plane (plotted in cycles per second in straight linear fashion, not logarithmically). He ends up with a variety of boundary lines: F₂ − F₁ = 600 or 550, F₂ + F₁ = 2,400, F₁ = 570, and F₁ = 350 or 380. This is nice, but what earthly interpretation can be assigned to any of these except the F₁ = K equation? If F₂ − F₁ expressed in semitones were a constant, that could mean the existence of a fixed interval, but a difference of 600 cps at 2,000 cps is only half as much (roughly) as at 1,000 cps. And the sum of two frequencies has, as far as I can discover, no significance either in cps or in semitones. I have done my share of playing with these charts and lines, and no matter how you handle the thing, the equations of the resulting lines are almost always meaningless (as I remember, I found one boundary where 2F₁ − F₂ was a constant), or at any rate uninterpretable by any theory of hearing known to me. Wiik was also very lucky to come out with such a trivial amount of overlap; the situation described earlier by Shearme and Holmes is much more typical in my experience.
The papers which I have chosen to comment on are only a sample, of course, chosen from those dealing with topics of special interest to me, like vowel quality, accent, phonological theory, change, sound spectrography, and ancient languages. There are many more, some equally important and perhaps even more exciting to those whose special interests are different or less narrow than mine.


Reviewed by HARVEY SOBELMAN

Bar-Hillel’s great versatility is well illustrated by this collection of papers in the field of linguistic analysis and syntactic theory, machine translation, information theory, and information retrieval. Dating from the years 1950 to 1963, and from such diverse publications as Language, The Journal of Symbolic Logic, the Times Literary Supplement, and Digitale Informationswandler, they constitute an interesting intellectual biography of an important interdisciplinary scholar, basically a philosopher, who has been interested in linguistics and applied himself to the investigation of some questions in the field.

In 1950, we learn from the autobiographical introduction (pages 1–16), Bar-Hillel had become familiar with Harris’s Methods in Structural Linguistics and proceeded to duplicate some of Harris’s work in syntactic description using the philosopher Rudolph Carnap’s methods, terminology, and notation (“On Syntactic Categories,” pages 19–37). Specifically, he finds here that the symbolic languages used in logic are significantly unlike ordinary language in that they do not provide for syntactic categories. For instance, if we may translate his argument into quasi-linguistic terminology, in these symbolic languages, any “subject” may occur with any “predicate,” whereas in ordinary English we may find The stone is red, Aluminum is red, This stone weighs five pounds, but not Aluminum weighs five pounds. He shows that a grammar can be constructed
which accounts for this syntactic difference between *this stone* and *aluminum* and concludes with a consideration of the philosophical problems involved in permitting sentences like *Aluminum weighs five pounds* or *Caesar is a prime number*. Of greater linguistic interest is his treatment of the problem of writing a grammar that allows the sentence *James is the black sheep of his family*, but not *James is the green sheep of his family*. His arguments almost exactly parallel those Harris might have used. The possible solutions are (1) to assign *black* and *green* to different syntactic classes, (2) to assign *black* and *green* to the same class, but to consider the *black* in *black sheep* as a different "word" from the normal *black* (thus, two homonyms), or (3) to assign *black* and *green* to the same class, but to consider *black sheep* as a single elementary symbol (what he would later call an "idiom"). An appeal to the principle of simplicity resolves the problem in favor of the last solution.

The basic orientation of this paper is rather difficult to appreciate nowadays. The Carnapian apparatus of Bar-Hillel's metalanguage seems exceptionally cumbersome in relationship to what it accomplishes, especially in comparison with Harris's techniques. Moreover, it is clear that no constructed logical calculus fails to recognize certain syntactic categories, "subject" and "predicate," for instance, so that Bar-Hillel seems to be proposing simply a more accurate or detailed sort of syntactic analysis. Yet nowhere does he make clear the relationship between the usual "rough" and his proposed "fine" syntactic analysis, as Harris's syntactic calculus does so well. We may also object to the lack of empirical relevance displayed in this paper. When we are told that *Aluminum weighs five pounds* is not one of the "meaningful sentences of ordinary English" (page 19) or that *The population of New York is divisible by the population of Chicago* would be recognized as meaningful by "an English grammarian with a logical touch" (page 29), we must simply accept the statements as incontestably true. We rarely find any hint that there might be some basis for linguistic research apart from the philosopher's introspections about his native language or, as in this case, some foreign language; informants are not mentioned. This is of course linguistics in its prescientific stage, linguistics without basis in observation.

The lack of empirical relevance is admittedly unimportant here, since the content of the article is trivial, a mere restatement of results achieved elsewhere through a scientific approach. The case of the paper on "Idioms" (pages 47–55), which can be considered the first structural treatment of the concept, is more serious. Bar-Hillel here points out, to start, the difference between monolingual and bilingual idioms. To cite an example from Hockett, English *marriage of convenience* and French *mariage de convenance*.
are monolingual idioms of English and French respectively because, by Hockett's criteria, their meanings are not deducible from their "structure," i.e. the meanings of the constituent morphemes and the grammatical structure of the two forms. But *mariage de convenance* is not a bilingual idiom of French with respect to English because a "word for word" translation from French to English would produce a satisfactory translation, and similarly for *marriage of convenience*. Instances of bilingual idioms that are not monolingual idioms are much more common, including much of the content of the familiar "idiom lists" of our school foreign-language textbooks; thus, French *avoir x ans* 'to be x years old' is a bilingual idiom of French with respect to English, since a word for word translation would not usually be possible, but is not necessarily a monolingual idiom of French, nor *to be x years old* a monolingual idiom of English. In 1955, when this article was written, there was still considerable interest in the problem of defining various sorts of linguistic "units" such as phonemes or morphemes therefore; Bar-Hillel is at some pains to develop the following definitions of, respectively, bilingual and monolingual idioms:

A given sentence in a language $L_1$ is idiomatic with respect to a language $L_2$, to a given bilingual word dictionary from $L_1$ to $L_2$, and to a given list of grammatical rules if, and only if, none of the sequences of the $L_2$ correspondents of the sequence words of the given $L_1$ sentence is found to be grammatically and semantically a satisfactory translation, after perusal of the applicable grammatical rules (page 50).

An expression in a given language $L$ is idiomatic within $L$, with respect to a given monolingual dictionary and a given set of grammatical rules if, and only if, none of the word sequences correlated to the given expression by the dictionary and the list of rules is (sufficiently) synonymous with it (page 54).

Let us examine these definitions. A "bilingual word dictionary from $L_1$ to $L_2$" is presumably one which lists all the words of $L_1$ and for each word gives one or more $L_2$ correspondents. Any $L_2$ correspondent, we suppose, may be zero, some item less than a single word, a single word, or more than one word. Now suppose that we are trying to determine whether Bar-Hillel's sample sentence, *Truman declared that the whole affair was a red herring*, is idiomatic. Consulting a simple bilingual word dictionary containing the following entries:

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herring  Hering
red      rot
```

and applying the proper "grammatical rules" would produce the semantically unsatisfactory translation *roter Hering* for *red herring*; hence the English sentence would be idiomatic with respect to German, to the given
simple dictionary, and to the given grammatical rules. It would not be idiomatic with respect to the following more elaborate dictionary:

| **herring** | 1. Hering  
|             | 2. Finte in the context red— |
| **red**     | 1. rot  
|             | 2. σ in the context —herring |

This dictionary would provide two correspondents for *red herring*, *roter Hering* and *Finte*, one of which is a semantically satisfactory translation. But, of course, our reasoning in both cases is quite faulty, that is, Bar-Hillel's definition is ill-advised. When we get *roter Hering* as a translation of *red herring* and find it to be semantically unsatisfactory, the best conclusion to draw is simply that the dictionary is incorrect or inadequate. For, if the dictionary had contained, say, the entry

| **red** | blau |

the results of the translation process would have been equally unsatisfactory. Bar-Hillel's mistake stems from studying language by making observations of language descriptions (dictionaries) instead of language behavior itself. Exactly the same problem arises in the definition of the monolingual idiom. If we were to follow Bar-Hillel's lead, we could define a phoneme as any one of the items mentioned in the phoneme list of a linguistic description, a sentence would be anything generated by a sentence-generating grammar, and so forth. There need not be anything "wrong" or circular in this sort of definition, but it is possible to take the contrary position that, say, /p/ is a phoneme of English regardless of any phoneme list that any linguist chooses to publish because native speakers of English exhibit certain characteristic behavior with respect to all instances of what we call the phoneme /p/; more specifically, they consider them all to sound the same. In other words, it is possible to say that linguistic structure is not an arbitrary construct imposed by the linguist on the observed speech data, but rather a direct description of native speakers' reactions to speech data. Bar-Hillel might have attempted to find out whether native speakers of English do exhibit some sort of special characteristic behavior with respect to *red herring*, *hang fire*, *hold water*, and so forth, thus perhaps establishing some justification for talking about idioms as a kind of linguistic "unit," or whether their behavior toward them is predictable in terms of their behavior with respect to the constituent morphemes (e.g., do they feel that a red herring is "really" in some way some sort of herring?). But, apparently, this type of problem cannot be raised within the framework of philosophical linguistics.
Disdain for empirically based linguistics becomes most obvious in the brief remarks entitled “Intertranslatability of Natural Languages” (pages 56–58). The purpose of these remarks is not quite clear, but the point being made by Bar-Hillel is that “the thesis of the intertranslatability of all natural languages” is either false, with respect to closed languages, or trivial, with respect to open languages. Since he gives no explanation of the concept of “translation” and a highly equivocal explanation of the concept of “closed language,” it is hardly worthwhile to try to refute his conclusion. We should note, however, that a proof of his position that translation is not always possible should be very simple. All that would be required is to cite one of the infinite number of possible sentences of any one of the thousands of languages of the world and the name of one language into which that sentence cannot be translated, as determined, perhaps, by a competent translator. It is hardly necessary to report that Bar-Hillel does not do this; instead he presents a “probably fictitious” example (page 57) of a language that has no “indexical expressions equivalent to ‘I’ or ‘the speaker of this sentence’ or ‘your obedient servant,’ etc.” (page 57) into which it would be impossible to translate the English sentence I am hungry.

A fourth article in this section of the collection, “Logical Syntax and Semantics” (pages 38–46), has been discussed at length by Chomsky. In one of his most convincing presentations here, Bar-Hillel argues, among other things, that linguistic analysis should be concerned with the truth value of utterances as well as simple grammaticality. Thus, he says, the fact that oculist and eye-doctor (in certain idiolects) can be interchanged without change of truth value, while oculist and dentist cannot, must be accounted for in any complete description of English, but once we accept this principle, we must include all of logic within the scope of linguistic description, since logic deals with the truth-value relationships of sentences of various grammatical structures. And, in turn, the inclusion of logic in linguistic description calls for the use of transformational techniques of analysis. However, since the publication of this paper in Language in 1954, Bar-Hillel has apparently abandoned this Carnapian approach, and gone over to a more orthodox linguistic position.

The second section of this collection of articles has the title “Algebraic Linguistics” (pages 61–150), Bar-Hillel’s term for studies, using the techniques of symbolic logic, of formal systems with varying degrees of similarity to natural language. However, the first two articles of the group, “A Quasi-Arithmetic Notation for Syntactic Description” (pages 61–74) and “Some Linguistic Obstacles to Machine Translation” (pages 75–86), fall

1 Language XXXI (1955), 36–45.
entirely outside this now familiar genre, and deal simply with a specialized
sort of notation for syntactic description that Bar-Hillel attributes ulti-
morely to the logician Ajdukiewicz. In this notation, as used for the
description of English, s stands for sentences, n for nouns. The symbol n/n
would stand for forms that are annexed to nouns on their right to form new
nouns, n/n · n → n, e.g., “one word adjectives.” The symbol n/s would stand
for forms that are annexed to nouns on their left to form sentences,
n·n\s → s, e.g., verbs. The sentence Poor John sleeps can thus be represented
by the formula

n/n · n · n\s,

and if we treat the symbols to the right of / and to the left of \ as denomina-
tors of a fraction, then a process analogous to algebraic “cancellation,” in
two steps corresponding to the two layers of IC structure, reduces the for-
mal to s, showing that, if we have hit on the correct symbolic representa-
tions of poor, John, and sleeps, then Poor John sleeps is indeed a sentence
rather than some nonsentential string of forms. This type of description is
easily recognizable as a phrase structure or IC grammar, somewhat com-
parable to Harris’s, which he cites (page 62). The two types of grammars are
closely matched in terms of simplicity: Harris uses simple basic elements,
N, A, V, and so forth, with many rules for combining them, such as
AN → N or NV → S, while Bar-Hillel has only one combinatory rule, the
rule of cancellation, but exceedingly complex elements are combined by
this rule, for instance, Modern Hebrew meașer ‘than’, which in ordinary
morphemic analysis is about two morphemes long, but which here (page 72)
has as one possible analysis (n\s/n)\(n\s/n)/n. Apart from this, the Ajdukie-
wicz notation has the defects found in most phrase structure and many
other grammars. It requires a vast proliferation of basic symbols; n and s
are not adequate, obviously, for the description of any language. Thus, Bar-
Hillel considers both adjectives and articles as belonging to the type n/n
(page 71); but adjectives form open constructions with nouns, whereas
articles form closed constructions, e.g. poor, little, tousle-headed, etc., John,
but not the the the boy. The immediate solution is to classify adjectives as
n/n and articles as n'/n, but there is no indication that the total number of
different classes required to account for all possibilities of morpheme co-
ocurrence would not eventually become infinite. However, by the time
this article was published (1953) Harris had provided a partial solution
to the problem of class proliferation in the form of his exponent notation
so that Bar-Hillel’s system of description was obsolete from its inception.
Another fault of the Ajdukiewicz notation is its ineffectiveness with
discontinuous constituents, as in Paul strangely enough refused to talk
(page 73), which has the two ICs strangely enough and Paul . . . refused to
Almost all phrase structure grammars use makeshift procedures (e.g., special types of diagrams) to get around the difficulty, but the Ajdukiewicz notation cannot be easily modified for this purpose. Bar-Hillel’s solution, naturally, is to change the language: he suggests that we put commas around strangely enough and move the comma-enclosed string to the beginning or the end of the sentence, and then proceed with the analysis. Still another problem, found in most phrase structure grammars is the unwarranted assumption that syntactic descriptions must necessarily account for the order of elements in a sentence, rather than, where appropriate, simply the selection of cooccurring elements. Thus, for Bar-Hillel, strangely enough, Paul refused to talk and Paul refused to talk, strangely enough are syntactically distinct, one sentence being left-annexed and the other right-annexed. But it could be maintained that the two sentences are syntactically identical and differ only stylistically, (and this may amount to the same thing) that from the syntactic point of view the difference belongs to parole, not to langue.

The second paper on Ajdukiewicz notation, published in 1960, dwells at greater length on its defects. Bar-Hillel here still gives no indication of understanding that discontinuous constituents in general create problems for IC analysis but does give two more examples of the phenomenon (pages 82–83). He also mentions the pseudo-problem of sentences such as Playing cards is fun, which he incorrectly claims cannot be analysed reasonably in this notation (page 82). On the other hand, he does recognize as such the problem of class proliferation. In this paper, however, the solution presented is to adopt transformational descriptions; it is not claimed that transformation grammars will eliminate any of the problems but simply that they are “more efficient.”

The autobiography (page 15) describes the 1953 paper on Ajdukiewicz notation as “one of the forerunners of algebraic linguistics,” but this is surely an overstatement. The three next papers, written jointly with Eliyahu Shamir, Chaim Gaifman, and Micha Perles, “Finite-State Languages: Formal Representations and Adequacy Problems” (pages 87–98), “On Categorial and Phrase–Structure Grammars” (pages 99–115), and “On Formal Properties of Simple Phrase Structure Grammars” (pages 116–150), fall more clearly into that category. Some of the results achieved in these papers are briefly reviewed in the first lecture (“The Role of Grammatical Models in Machine Translation,” pages 185–194) of Chapter 14, “Four Lectures on Algebraic Linguistics and Machine Translation” (pages 185–218). This lecture is an interesting survey of the field of algebraic linguistics as understood by Bar-Hillel, and can be recommended to those who would like a nontechnical introduction to the field.
Also commendable is the third lecture of the series, “Language and Speech: Theory vs. Observation in Linguistics” (pages 205–211). Bar-Hillel says here:

Today we know that no science worth its salt could possibly stick to observation exclusively. Whoever is out to describe and nothing else will not describe at all . . . All scientific statements must surely be connected with observations, but this connection can, and must, be much more oblique [than] many methodological simplicists believe (page 207).

This is a position in which we can readily concur; Bar-Hillel himself, however, seems to misapply it. On the very next page (208), for instance, he presents the following argument:

I know perfectly well that no competent English speaker will ever in his life be presented with a certain utterance consisting of a few billion words, say of the form “Kennedy is hungry, and Khruschev is thirsty, and De Gaulle is tired, . . . , and Adenauer is old.”, going over the whole present population of the world, but I know, and everybody else knows perfectly well, that were such a speaker, contrary to fact, to be presented with such an utterance, he would understand it as a perfect specimen of an English sentence . . . That John is competent to understand a certain ten-billion-word sentence will not be tested by presenting John with a token of this sentence, but, as we all know, by different, oblique methods. For the above sentence, for instance it would suffice to find out that John understands such sentences as “Paul is hungry.” . . . as well as that he has mastered the rule that whenever α and β are sentences, α followed by ‘and’ followed by β is a sentence.

We might ask here what Bar-Hillel means by “mastering a rule”; in this context it does not seem to mean anything more than “being able to understand a sentence generated from an understandable sentence by as much as a 2.5-billionfold cumulative application of a rule,” which would be begging the question, of course. But even if this rule were in fact a part of some theory-permeated description of English, if in other words the grammar generated certain abstract ten-billion-word “sentences” or even certain hundred-word “sentences,” there would be no reason to expect any direct, unoblique, correspondence between these abstract “sentences” and observable physical speech. Bar-Hillel certainly does transformation grammar and his own previous descriptive theories no benefit by insisting on such a view and does them positive harm when he cites imaginary experimental data in their behalf. The transformational model of language should not be expected to provide a perfect representation of speech behavior—it will not do so in any reasonable way—but it will be a highly satisfactory model even if it proves to provide only a slightly better representation than we have had hitherto.

Bar-Hillel is probably best known for his contributions to the field of machine translation, and these “Four Lectures,” which were delivered in
1962 to a NATO Advanced Summer Institute on Automatic Translation of Languages in Venice, come from a substantial section of the book entitled "Machine Translation." The fourth lecture, "Why Machines Won't Learn to Translate Well" (pages 211–218), recapitulates and amplifies the position developed in the articles of this section: "The State of Machine Translation in 1951" (pages 153–165), "Aims and Methods in Machine Translation" (pages 166–173), "A Demonstration of the Nonfeasibility of Fully Automatic High Quality Translation" (pages 174–179), and "The Future of Machine Translation" (pages 180–184). Some of his conclusions are worth citing here:

I would say that there is no prospect whatsoever that the employment of electronic digital computers in the field of translation will lead to any revolutionary changes. A complete automation of the activity is wholly utopian, since the fact that books and papers are usually written for readers with a certain background knowledge and an ability for logical deduction and plausible reasoning cannot be overridden by even the cleverest utilization of all formal features of a discourse. The hopes to the contrary which many of us had a decade ago just turned out to be by and large unrealizable (page 183).

Autonomous, high-quality machine translation between natural languages according to rigid algorithms may safely be considered as dead. Such translation on the basis of learning ability is still-born. Though machines could doubtless provide a great variety of aids to human translation, so far in no case has economic feasibility of any such aid been proven, though the outlook for the future is not all dark (page 218).

The critical problem responsible for this gloomy picture is the lack of any specifiable procedures ("algorithms") for analysis of texts in a source language, either now or in the foreseeable future. Bar-Hillel's "demonstration" of nonfeasibility involves precisely this problem: the analysis or translation of the sentence The box is in the pen. There is no immediately obvious specifiable procedure available to a computer program for determining whether the pen here is a writing implement or some sort of enclosed area. A human translator, Bar-Hillel argues, has information about the relative average sizes of boxes, writing implements, and, say, playpens or pig-pens, as well as about the meaning of in, and can thus arrive at the "correct" translation, whereas it would be foolhardy to try to supply a computer with the encyclopedic information which is clearly brought into play here. Bar-Hillel is basically right in this, but the argument is not nearly so simple. The crucial difference between the human translator and the translation algorithm lies not in the fact that one is capable of resolving ambiguities and the other not, but rather in the fact that the two translators assign different probabilities to alternative translations. In The box is in the pen, there are at least two possible meanings for pen, regardless of the context in which it
occur: there is no way of eliminating the ambiguity absolutely. The human translator, with the many resources at his disposal, decides that 'enclosure' is the vastly more probable meaning, translates accordingly, and produces a smooth, "high-quality" translation, which is, of course, occasionally liable to be wrong. The explicit translating program, by contrast, may have for resolving the ambiguity only a dictionary which lists two possible translations for pen. The program might give as its output translations all possible renderings of all ambiguities, producing an awkward, "low-quality" translation, which, however, would contain all of the correct possibilities. A more sophisticated program might make use of the linguistic context of ambiguous forms to establish some estimate of the relative probabilities of different translations, but, as Bar-Hillel points out, the use of linguistic context alone is a relatively poor method of attack in comparison with the human translator's many methods. The human method of translation and the explicit method have distinct ranges of utility, and when highest accuracy together with "smoothness" is desired, a combination of the two methods (or rather a combination of the two types of output) may be employed, thus in many Bible translations, e.g., the Revised Standard Version, which puts the most probable translations in the main body of the text, less probable translations in the margin.

A second inaccuracy in Bar-Hillel's demonstration of the unfeasibility of the type of machine translation that he considers to be optimal is brought about by his failure to distinguish properly the different structural levels of language and the contribution of each to translation problems. The example of The box is in the pen, for instance, seems to have nothing to do with translation per se, but rather with a purely intralinguistic question. It happens that the graphemic string <pen> or the phonemic string /pen/ represents at least two distinct morphemes of English, and it is well known that there is no algorithm for converting phonemic or graphemic transcriptions to corresponding morphemic transcriptions. Now it is only by historical accident that English is not written in morphemic transcription; a few languages, such as Chinese and Sumerian, are so written, to a greater or lesser extent, and thus in, say, machine translation from Chinese to Sumerian, a translation problem comparable to that cited by Bar-Hillel could hardly arise, at least insofar as the writing systems are truly morphemic. In general, therefore, he has not demonstrated that fully automatic high-quality translation is unfeasible. And this argument can be extended to all languages, for there is no reason to insist that translation, as a purely intellectual problem, need concern itself with the conversion of phonemic or quasi-phonemic transcriptions in one language to similar transcriptions in another language; conversion between morphemic transcriptions is a
sufficiently challenging problem in itself. To be sure, the practical problem of translation is not solved by redefining the term, but there is surely little to be gained by confusing the separate tasks involved in practical translation and assuming that they should all be attacked by the same methods.

The longest section of this book is taken up with a relatively formal account of Bar-Hillel’s theory of “semantic information” (pages 221–274), and a simplified account of it, “Semantic Information and its Measures” (pages 298–310). This theory was inspired by the familiar “theory of communication” or “information theory,” which deals with communications considered purely as formal tokens. The theory of semantic information, however, deals with the messages conveyed by those communications, i.e., with what they say rather than what they are. Bar-Hillel supplies several explications of the concepts of “(semantic) information” and “amount of (semantic) information,” and it turns out that the amount of information of a message is somehow related to its unexpectedness, just as the amount of information conveyed by a given communication is sometimes considered to reflect its unexpectedness. Thus, the message conveyed by the sentence $17 \times 19 = 323$, a tautology relative to the Whitehead–Russell system of logistic, carries zero semantic information, but the sentence itself has high formal information, since it is a highly “unexpected” sentence. Bar-Hillel develops his theory as an adjunct to Carnap’s theory of inductive logic; it seems to have no relevance to semantics in the linguistic sense, despite his explicit claim that it deals with “the semantic content of a statement” (page 298). For semantics is generally thought to deal with the meanings of utterances and other forms, but the theory of “semantic information” is concerned only with the messages conveyed by utterances, i.e., their reference, a much more restricted topic; it deals with what utterances say rather than with what they mean. The all too frequent confusion of meaning and reference has been discussed by Quine\textsuperscript{2} and requires no additional comment here. Furthermore, Bar-Hillel, in common with many other philosophers, considers the “semantic content” of sentences only and not that of morphemes or idioms (page 299). It is a commonplace observation that the meaning of a morpheme cannot always be completely determined except in the context of some larger form. A highly unwarranted generalization from this leads to Bar-Hillel’s position that, “fundamentally,” only sentences have meaning; unfortunately, the same argument may be applied to sentences in relation to larger forms, or to any form short of a complete discourse. The widespread Bloomfieldian view of this matter, while rather

contrived and paradoxical, seems preferable. Here, morphemes are the 
"units of meaning," by which is meant that any meaningful linguistic 
behavior is so by virtue of the fact that it represents a string of morphemes 
(a form), and furthermore that every morpheme has a unique meaning. 
However, the meaning of a form is not determined by the meaning of the 
individual morphemes of which it is composed but rather by the context of 
the form. This view, in contrast with Bar-Hillel’s, is completely compatible 
with the ideas of the theory of communication, and it is possible that a 
proper linguistic theory of semantic information may eventually arise from 
the background of that theory.

Three essays on the “Mechanization of Information Retrieval” make up 
the last part of this collection (pages 313–372). Bar-Hillel is a critic rather 
than an innovator in this field, and most of his criticisms of the imprecise 
thinking of the IR specialists are well-aimed and illuminating. However, 
even though IR has now become a quite highly ramified technical discipline 
with no relationship as such to linguistics, even linguists should have no 
difficulty in detecting the fallacies in the argument of the last article, “Is 
Information Retrieval Approaching a Crisis?” (pages 365–372). Bar-Hillel 
says here that the flood of scientific information, swelling every year, is not 
creating a crisis which might some day be detrimental to the future of 
scientific research. In spite of the fact that there is a continuing increase 
of publication in every field, he maintains, scientists today spend no more 
time reading than they did in the past because they have specialized their 
interests. This development he considers inevitable and desirable. But he 
fails to explain how, without vastly more efficient IR techniques, the 
scientist is to find the literature in his specialized field, how for instance a 
linguist could know that an article relevant to his interests had appeared 
in *Digitale Informationswandler* or *The Bulletin of the Research Council of 
Israel*. As for the problem of the scientist who finds an increasing number 
of publications in his field in foreign languages inaccessible to him, Bar-
Hillel answers only, in effect, that that is the scientist’s, and the United 
States’, bad luck. He foresees real crisis conditions only in fields where 
information accumulates without becoming outdated, such as the law in 
what he calls “Anglo-Saxon countries,” which makes use of precedents. 
His unfortunate recommendation is that these countries simply abandon 
the principle of justice by precedent.

Misprints are numerous throughout the book, though they are only 
occasionally disturbing. But Bar-Hillel’s many editors, from the *Times 
Literary Supplement* on down, deserve very black marks for the quality of 
English diction they have permitted in their publications. One example from 
the paper “Idioms” (page 51) will suffice:
Too many correspondents for single words are a mixed blessing: a null word among them is a Trojan horse. The *embarras de richesse* caused thereby might prove fatal in some cases, and Buridan's ass should serve as a perpetual warning.

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*Reviewed by HENRY KUCERA*

This extensive book, written by a well-known Czech phonetician, represents, substantially, a summary of Czech phonetic research during most of this century. Bohuslav Hála, Professor of Phonetics at Charles University in Prague and director of its phonetics laboratory, has been active in this field for some fifty years and has published a number of books, monographs and articles, concerned both with general phonetic problems and with phonetic descriptions of the Czech and Slovak languages. His latest book, which is his most comprehensive work, aims at fulfilling a dual purpose. As the title already suggests (*Uvedení* means *Introduction*), it is intended to serve as an introduction to phonetics in general and to the phonetic structure of the Czech language in particular. This the book accomplishes with a reasonable degree of success. Most of the basic phonetic information usually included in phonetic manuals can be found here, such as descriptions and diagrams of the organs of speech, detailed discussion of articulatory processes, information on the principles of acoustic phonetics and on the apparatus used in experimental work, and similar matters. But the book, at the same time, seeks to be more than a phonetic manual; it also contains a summary of many research results obtained by Hála and his collaborators in the phonetic analysis of Czech, for example numerous palatograms and linguograms, measurements of formant structure of vowels, kymographic tracings of continuous speech which give valuable clues to Czech intonation patterns, data on the duration of Czech phones, and similar kinds of information. A student of Czech phonetics can thus find in this volume both the essential information which will introduce him to the subject and to its history and, at the same time, a rich source of data on the phonetic properties of Czech. While much of this information has been published before by Hála and other authors, the present book makes it available, together with some new facts, in a systematic and convenient form.
Although this is thus a work of unquestionable usefulness, it is not a book to which linguists and phoneticians are likely to give their unqualified approval. The acoustically oriented phonetician would probably find the relative sparsity of discussion of the more recent experimental research and apparatus as well as of contemporary acoustic theories of speech to be the main lacunae of the work. From the point of view of linguistics, the principal shortcoming is essentially the lack of a consistent effort for integration of phonetics into a general system of linguistic descriptions. Hála’s book begins at the point where the discrete nature of language on the phonological level is considered as established and the whole problem of digitalization of the acoustical continuum in the decoding of language messages is entirely bypassed. Hála thus never needs to come to terms with such rather difficult problems as the segmentation of speech into its constituent components or the rigorous determination of the invariables of the speech continuum which perform the various functions of linguistic signalling.

Hála’s basic unit of description is the hlásk, i.e., a speech sound, called here, for the sake of simplicity, the phone. Hála’s phones constitute a small finite set and conceptually are quite reminiscent of Daniel Jones’ phonemes. Hála defines phones as “the smallest, further indivisible articulatory-acoustic realities produced by the organs of speech and representing the basic phonetic elements of a particular language which [the phones] are sufficiently differentiated from each other and thus capable of linguistically functional utilization for the purposes of social communication through speech” (p. 107). Because of the emphasis on their communicational and differential function, Hála’s phones overlap, of course, with what most contemporary linguists would also call phonemes. However, they are not identical with them. On the one hand, Hála seems to think of phones as articulatory–acoustic “realities,” admitting, on the other hand, that the “realization” of the same phone will not always be a phonetically identical sound. This rather obvious fact is acknowledged in Hála’s book in a curiously subdued fashion, perhaps because it presents rather serious problems to his conceptual scheme: “The production of phones is not a serial machine production; one can, therefore, not expect that all issues of the same phone will be absolutely identical” (p. 111). It is hard to imagine that this statement can mean anything else than an admission that phones are not “realities” but rather constructs, abstractions or, alternatively, infinite sets of slightly varying sounds. Unfortunately, Hála never attempts to resolve this contradiction or specify rigorous procedures for determining such phone constructs. For example, it is never clear in this book whether complementary distribution is one of the criteria for deciding whether two sound “variants” should be assigned to a single phone or to more than one
phone. To some extent, complementary distribution must be one of Hála's criteria since, otherwise, he could not, as he does, speak of the voiced ζ and ζ as "combinatory variants" of the phones c and c respectively (p. 254). But elsewhere, he argues against the concept of a phoneme as a set of allophones which have been included in the set because, among other reasons, they are in complementary distribution (p. 113). The problems to which this leads can be illustrated by Hála's description of the Czech dental nasal n and the velar nasal η. The latter sound is listed in this book as a separate phone in at least one place (p. 232), while, in other places, it is specifically treated as a "variant" of the phone n. Thus on page 113, Hála states that, from a phonetic point of view, η is not a separate phone in Czech but rather a result of the "assimilation" of the normally alveolar n in certain environments (namely before a following velar stop). The inconsistency of these contradictory positions becomes even more serious when one considers the matter in terms of Hála's own procedure for specifying which articulatory characteristics differentiate one phone from another (such as given, for example, in the table on page 271, where only n and no η are listed). The phone n is there differentiated from other phones of Czech, among other things, by its "pre-alveolar" articulation; it is quite obvious that this differential articulatory property is inapplicable to the velar "variant" so that Hála's table does not represent the true situation. From the methodological point of view, this is not a trivial error because articulatory oppositions (such as those in the table on p. 271) serve for Hála as those invariables which allow him to subsume under a small number of distinct phones the unspecifiable variety of sounds which may occur in actual speech messages.

The problem is further confused by Hála's insistence on treating certain "variants" of a given phone as basic and others as subsidiary, without specifying the conditions which determine such a hierarchy. A good example of this is his treatment of the Czech ʃ which he considers to be basically voiced, secondarily voiceless. This ranking is presumably based on the observation (although this is not explicitly stated) that the voiced variant has a less restricted distribution than the voiceless one. But the situation can certainly be described both more simply and more accurately by stating that the opposition voiced versus voiceless plays no distinctive role in the phone or phoneme /ʃ/, and that the two variants (or allophones) appear in complementary distribution or, in a few cases, in free variation. It is not without interest to note, as Hála himself mentions, that the "secondary" voiceless variant of /ʃ/ is more frequent than the voiced one as speech statistics seem to indicate. It also should be pointed out that the distribution of the voiced and voiceless /ʃ/ in Czech is considerably more complicated than Hála's description suggests.
Another methodological matter which should perhaps be mentioned at this point is the neo-grammian ghost which seems to be haunting quite a few pages of this book. The introduction of historical considerations into a synchronic phonetic description is certainly a rather surprising procedure, especially if it comes from an experimentally oriented phonetician. Nevertheless, Hála seems to suggest in several places that reliance on historical criteria might be a legitimate aid in interpreting the phonetic properties of contemporary Czech. Perhaps the most striking example of this is his discussion of diphthongs *aj, ej, oj*, and so on. He argues at first that these are not diphthongs (since he considers the second phone to be a consonant) but then he retreats a little by saying that one could admit the combination *ej* as a diphthong where it stands (historically) in colloquial language for the original long *ý* or *i* (p. 292). But since there is no discernible phonetic difference in contemporary Czech between the combination *ej* of this origin and of any other origin, the possibility of considering some such combinations diphthongs and other non-diphthongs on the basis of diachronic criteria certainly seems hardly justified as a procedure of phonetic analysis. For completeness, it should be also said that the rather difficult problem resulting from the fact that a vowel + *j* is freely substitutable, in certain positions and in most styles of speech, by the sequence of vowel+ non-syllabic *i* is not taken into consideration at all in the discussion of the diphthongal status of these groups.

I discussed these methodological points in some detail because they illustrate the lack of a consistent effort on Hála's part for an integration of phonetics in the conceptual framework of linguistic science. I will now turn to the individual chapters of the work and discuss briefly their main points and observations.

The book consists of an introduction and four chapters, each carefully divided into a number of sections and subsections. While most of the bibliographical information pertaining to the text is referenced in footnotes, there is also a very short alphabetical bibliography of basic works and periodicals following the last chapter. The appendix contains over 700 palatograms and liguograms, reduced to approximately one-half of actual size and representing studies of a large number of Czech speakers. There are also some photographs and X-ray pictures of articulation, oscillographic recordings, kymographic tracings, a couple of speech spectograms and a few other reproductions of acoustical measurements. The book contains both an index of names and a subject index.

The introduction delimits the subject; it reviews briefly the various definitions of phonetics, starting with Rousselot and ending with the Soviet phonetician Zinder; attention is paid also to Czech scholars in the field.
Hálá's own definition of phonetics is that of a linguistic discipline which investigates, describes and evaluates the auditory means of social communication through human speech and does this both from the point of view of theoretical scientific research and of its practical application in society (p. 10). An outline of a communicational model of language is then given (which, curiously, lacks any discussion of feedback). Although Hála's conviction that phonetics is an integral part of linguistic science is strongly evident already in this introduction, the rather essential question of the relationship between phonetics and phonemics is not tackled at all. The introduction concludes with a concise historical survey of phonetic research, from ancient to modern times.

The first chapter of the body of the text deals with phonetic research of Czech both in the past and in the present. This chapter contains a very informative and useful survey of phonetic observations and research dealing with the Czech language. It cites, for example, some of the opinions of Jan Hus and of a number of other Czech grammarians and writers about the pronunciation of Czech in their time. Much of this represents writings which are not easily accessible to the average reader, and Hála has succeeded well in showing the relevance of some of these observations for a better understanding of the phonological development of the Czech language. Czech phonetic research of the nineteenth and twentieth centuries is then discussed in greater detail. The rest of the first chapter contains fairly standard information, fully annotated and referenced, about the organs of speech and articulatory processes employed in Czech and about the various methods of research, both observational and experimental, which have been used in Czech phonetics. While most of the apparatus described in this last section is of the standard variety, the acoustic phonetician might be interested in the description of some special instruments which have been constructed at the Prague Phonetic Laboratory. Only very sparse information is given about some of the newer methods of acoustic phonetics, such as spectography and speech synthesis; apparently Hála's laboratory has not had much opportunity to utilize this more recent equipment in the investigation of Czech.

The second chapter deals with the basic elements of speech, phones (in Czech hlásky). The theoretical problems which, in my opinion, remain unresolved in this chapter have already been pointed out previously. Phones, in Hálá's view, can be characterized by a set of articulatory or acoustic properties which differentiate them from other phones, such as occlusion or fricative manner of articulation, voice or absence of voice, nasality or lack of it. This method, which Hála uses to specify the phones of the Czech language, is strongly reminiscent of the Jakobsonian distinctive
feature approach, but differs from it in that it does not operate solely with binary oppositions and shows no concern for the economy of description or, for that matter, does not differentiate between distinctive and predictable (redundant) characteristics of phones. Hála states that he had developed his approach independently from Jakobson, although he does borrow some terminology from the Preliminaries. The basic division of phones into consonants and vowels is then discussed and the criteria for such a differentiation summarized. Czech vowels as a group are treated in detail, both from the articulatory and acoustic points of view. After the overall treatment of vowels, each of the Czech vowel phones is described separately; this presentation includes in each case a thorough description of articulation including such information as measurements of jaw angle and lip opening. The discussion is accompanied, for each vowel, by a drawing of a cross-section of the organs of speech in the production of the vowel as well as by a schematic drawing of a labiogram, a palatogram and a linguogram. Acoustically, detailed information is given about the vowel formants including data on upper resonant frequencies. Since Czech utilizes vowel quantity as a distinctive phonological opposition, each of the five Czech vowels occurs in a short and a long version. The short and the corresponding long vowel, however, differ not only in length but in other articulatory and acoustic properties as well. Hála compares all these characteristics carefully and by placing the data side by side in one subsection makes it easy for the reader to perceive the differences. There is also an occasional comparison with vowels of other languages, both in articulatory terms and by means of comparing formant structures. Variant articulations in various styles and dialects of Czech are discussed fairly systematically. A special section is devoted to the problem of vowel quantity and determination of vowel duration; here Hála relies primarily on the measurements published more than thirty-five years ago by Chlumský in his Česká kvantita, melodie a přízvuk.

Essentially the same procedure is followed with regard to consonants which, too, are first classified into basic types and then discussed individually, again with the same kind of illustrations and palatograms and linguograms as was the case with vowels. The emphasis here is, of course, primarily on articulatory phonetics, since only a few preliminary acoustical measurements are available for Czech consonants. One interesting observation among the many made by Hála is his statement that the Czech affricates č and š terminate explosively, that is, that their articulation essentially includes an occlusion plus friction plus a return to complete occlusion followed by an explosion.

The third chapter deals with the structure of connected speech. Hála
considers the basic element of speech to be the syllable which he sees as a natural result of the development of human speech and thus an essential part of every language. The principle of the syllable lies, in his opinion, in the transition in articulation from stricture to aperture. Hála finally concludes that only vowels and certain sonorants \((l, r, m, n)\) can function as syllabic nuclei and are thus a necessary prerequisite to the existence of a phonetic syllable in any language. Hála is consistent in this opinion and rejects such interjections as the Czech \(pst\) ("hush") as marginal cases representing remnants of old expressive signals and therefore not fitting into the structure of speech and not constituting true syllables. Actually, Hála's statements regarding the syllable and its place in the development of human language is a good illustration of the dangers of generalizing about language universals without examining all the available data. There is good evidence, if I am not mistaken, that several languages exist which utilize some fricatives as syllabic nuclei; for example, Lendu (a Central Sudanic language) has a syllabic \(z\) which can even appear as a free form (cf. J. H. Greenberg in *Word* XVIII [1962], p. 79). A similar situation appears to exist in Bella Coola, where whole utterances apparently may consist of consonants only, and possibly also in some other American-Indian languages. The question whether certain fricatives and affricates constitute syllabic peaks in Chinese warrants consideration as well. Thus Hála's theory which is based on the assumption that the syllable of a specific structure (with a vocoid or sonorant nucleus) represents a basic entity in the development of all human speech is certainly subject to serious doubt. Actually, the existence of such interjections as \(pst\) in Czech only seems to support the observation that fricatives have the capacity to function as syllabic peaks.

A substantial portion of this chapter is devoted to the organization of larger segments of speech, particularly the word and the accentual unit, the phonetic measure. Czech stress is discussed in this context. The unit of stress is to Hála the syllable; he operates with four distinct levels of stress, syllables with main stress (hlavní přízvuk), with secondary stress (vedlejší přízvuk), unstressed syllables, and, finally, what can perhaps be called extra-intensity stress (důraz) which serves for contrastive pointing. In addition to these levels, Hála also identifies "emphasis" as extra-intensity stress with strong emotional coloring (p. 307). But there also seem to exist—although this is not entirely clear from the description—significant variations of stress intensity within each stress level. Diagrams reproducing kymographic tracings of speech and showing curves of stress intensity are included. There are two troublesome questions which remain unresolved in Hála's discussion of stress. First is the basic problem of what is the admissible range of variations within one level of stress or, to put it differently, what
minimum differences in stress intensity are necessary to classify two syllables of an utterance as having two distinct levels of stress. Admittedly, this is a difficult problem but it is also an essential one as long as one feels that a finite number of stress levels is actually operative in the language. Unfortunately, Hála does not address himself to this question at all. The other problem, concerning Czech stress specifically, is the place of onset of the main stress which, in Czech, is phonologically distinctive. It is rather clear that this difficulty cannot be simply resolved by the tying of stress to syllabic structure since, as Hála himself admits, there seem to be no generally valid criteria for dividing connected speech into syllables. This, too, is then an area in which more phonetic research of the Czech language would be of considerable value.

Intonation is discussed in great detail with many examples and a number of diagrams reproducing kymographic tracings. Although Hála does not attempt to identify those properties of intonational contours which are functionally distinctive in the identification of various types of Czech utterances (questions vs. non-questions, etc.)—as was attempted, for example, by F. Daneš in his recent book on the intonation of a Czech sentence—he does characterize in detail the intonational patterns found in sentences of various grammatical structure.

While the various types of "modification" of phones in connected speech are discussed in a separate section, this discussion is limited almost entirely to phonic variations observable within syllables or within single words. In this respect, Hála’s information is quite thorough and useful. It is surprising, however, that he pays almost no attention to phonetic phenomena and adjustments effected across word boundaries, where the situation in Czech is rather complex and particularly interesting. Hála states that no systematic research in this area has been done. This is not entirely true; many of the facts concerning such matters as distribution of voiced and voiceless obstruents in final word position, the distribution of the voiced and voiceless variants of the affricates and or r, or the phonetic realization of certain prepositions have been described in the literature. A more systematic corroboration of these observations through phonetic research would certainly be very useful and the lack of information in this area in Hála’s book is particularly disappointing.

There is a great deal of other information in the third chapter, including a summary of a frequency count of Czech phones performed by V. Mázlová and based on the analysis of 360,000 phones.

Chapter IV is only a very concise four-page postscript which defines the theoretical importance of phonetics for linguistics and for some other sciences, as well as some practical applications of phonetic research.
In summary, it is perhaps fair to say that the major strength of Hála's book is exactly in the area which the title emphasizes. It is a thorough introduction to the phonetics of Czech which includes enough basic information to serve well the beginner in the field and, at the same time, contains many research results and data of interest to the specialist interested in the Czech language. The shortcomings of Hála's book are, at least from the linguist's point of view, primarily conceptual and methodological. Although Hála's view of phonetics is clearly linguistic in orientation, the conceptual framework within which the process of selection and utilization of the raw acoustical material for purposes of linguistic communication might become clear is generally neglected. In addition to this, a phonetician might feel that this book is a little out of date and that it neglects or skims over some of the modern research and newer experimental procedures. This is apparent not only in the chapter which describes the apparatus employed in acoustical phonetics but also in the relative poverty of bibliographical references to some of the more recent works published in the West. I find, for example, no reference at all to such an important work as Gunnar Fant's *Acoustic Theory of Speech Production*; Ernst Pulgram's name is not even listed in the index. It is, of course, possible that some of the Western literature was not accessible to Hála. The fact remains, however, that phonetic research has progressed quite a bit beyond the procedures to which most of the space is devoted in this book.

In spite of its shortcomings, Hála's work is a useful and important publication, especially for students of the Czech language. As the jacket of the book rightly says, it is the most systematic and most extensive work in the field of Czech phonetics which has yet been written.

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*Reviewed by Brother David H. Kelly*

In 1962 Katz and Fodor published an article entitled, "What's Wrong with the Philosophy of Language?" (*Inquiry, V*, 197–237). Their heavy critical guns were directed against the linguistic position of two modern philosophies, positivism and ordinary-language philosophy. Positivists, they point out, dismayed by the apparently hopeless complexity, irregularity, and arbitrariness of natural languages, turned to constructing
artificial languages in the hope that the insights gained by careful study of the properties of these relatively simple systems would open the way to a better understanding of the mysteries of natural language. The trouble is that the grammars they construct, most going back to the *Principia Mathematica*, are essentially weaker than the grammars of natural languages; for one thing they do not operate with context-restricted rules, the kind needed to explain natural language phenomena. If then, say Katz and Fodor, one wants to study language seriously, he ought to face its inherent complexity squarely and not retreat to his simpler devices. Besides, modern linguists, not sharing the diffidence of their positivist colleagues, really believe that their techniques and general theories are sufficiently powerful to permit them as never before to penetrate into the inner dynamism of language. Granted that languages may be complex, arbitrary, and irregular; they are not thereby unintelligible. It is precisely the task of the linguist to confront this intelligibility, to let the system manifest itself. Analogies with artificial languages may be helpful, but they lead us only to the threshold; the real work lies within.

On the credit side, the positivistic approach does after all insist that any theory of language must explain the sentences of a language, and no linguist will object to that goal. Ordinary-language philosophers, however, not overly impressed by the positivists’ axiomatic systems for the reason given above, swing much too far in their reaction and begin to treat language as a collection of words. For them the study of sentences and sentence structures is to be excluded from the scope of philosophy; the ideal philosopher narrows his attention to words and their uses. Now such an approach can hardly expect a sympathetic reaction from the representatives of modern linguistics, who might point out half facetiously, half patronizingly that in that case the traditional problems of philosophy have all been solved (assuming they were real problems). For the speaker of English at any rate, the answers are in the third edition of *Webster’s* or, if you prefer, in the *N.E.D.*; just turn to, say, *life, soul, goodness, beauty, God,* or what-have-you, and there you will find in outline form all that philosophy has to say on the matter.

In giving their article the title, “What’s Wrong with the Philosophy of Language?” and then demolishing the linguistic presuppositions of just two philosophical movements, do not Katz and Fodor open themselves to the charge that they unfairly limit the extension of the term, philosophy of language, since there are many philosophers of entirely different persuasions who feel that they have contributed in a significant way to the philosophy of language. Positivism and ordinary-language philosophy may be dominant at present among Anglo-Saxon professional philosophers, but
not even these scholars, committed as they are to a definite philosophical
stance, can afford to dismiss lightly the writings of men oriented to an existen-
tial or phenomenological frame of reference. And, of course, for
students and other amateurs it is existentialism which is in today, not posi-
tivism or ordinary-language philosophy, if only for the (perhaps) deplor-
able reason that it is more immediately meaningful, more pertinent to their
life-situation (as the jargon goes). In response, the M.I.T. philosopher-
linguists could explain that they were justified in passing over in silence the
linguistic positions of existentialism and phenomenology for the simple
reason that they found nothing there worthy of extended discussion and
criticism, that in other words the linguistic theories of these modern philo-
sophies were either so silly or so obvious as to be of no interest (unless for
amusement) to the modern linguist.

The publication of Kwant's *Phenomenology of Language* might seem to
force a reevaluation of the contribution of both phenomenology and exis-
tentialism to our understanding of language. If Katz and Fodor had to redo
their article, would they now find it necessary to broaden their perspective
to account for the insights and theories of this book? My own answer is no.
The work, while readable and fairly interesting, will be a disappointment to
linguists, even those who, like the reviewer, are entirely sympathetic to cer-
tain aspects of existential phenomenology. Kwant, who is a professor of
philosophy at the University of Utrecht, says his book is intended as an in-
troduction “to the fundamental problems regarding language” (XI). He
addresses the book specifically to linguists, many of whom are taken to
task for alleged indifference to linguistic philosophy. “It is regrettable that
many linguists devote little or no attention at all to the philosophy of lan-
guage. For this neglect means that they omit to pay due regard to the funda-
mental questions arising in connection with the object of their studies” (X).
“Let me emphasize once again that this book is written not so much for the
benefit of the few philosophers who specialize in the philosophy of language
as for the many linguists who are interested in that branch of philosophy
and would like to possess a work which raises the problems of the
philosophy of language in such a way that even the non-philosopher can
fruitfully study it” (XIff.). Such an end, in spite of the condescending
phraseology, is indeed laudable; unfortunately, however, linguists will
not read very far before being appalled at the author’s seeming lack of
interest in the results of linguistic scholarship. Meaningful dialogue (again
the jargon) is a two-way street. Linguists want to hear what philosophers
have to say but not when they see the clear-cut advances of their science
either disregarded or cavalierly dismissed.

Perhaps the book’s greatest shocker comes on page fifty-four where
Kwant explicitly disallows the distinction between *langue* and *parole*, between the grammatical system and the spoken utterance—a distinction whose fruitfulness for research has been apparent since de Saussure and which has certainly lost none of its importance in the formulations of the transformationalists. De Saussure's *Cours*, incidentally, is not found in the bibliography. We quote Kwant:

We may not divorce language from the speaking man. . . . When we think of language, we always include also the speaking man. . . . There exists no other language than language which is used. Language comes into existence through use. It exhibits, of course, certain structures which can be studied as objective data. Yet these structures exist in real language, in the language that is used. Such structures are capable of development, but their development takes place in man's living use of the language. For this reason a philosophy of language is of necessity a philosophy of speech. Hence there is nothing strange in the fact that we refer here sometimes to language and then again to speech. Speech and language are one and the same reality. (54)

There is no point in going through a step-by-step refutation or clarification of the tremendous oversimplifications found in this passage. The author's argument flows naturally from his basic frame of reference, a merger of existentialism and phenomenology, succinctly described in chapter two:

The central idea of existential philosophy is the concept "existence," which indicates that our being is essentially and always openness to the other. A central idea of phenomenology is that of "intentionality," by which is meant that our consciousness is always consciousness-of-something . . . Precisely because these fundamental ideas are common to existentialism and phenomenology, these two streams of thought have been able to merge into a single stream as existential phenomenology. (23)

In this amalgamation the problem of meaning becomes all important and involves somehow all other problems. It is understood in the broadest possible sense as the fundamental human response to the other. Meaning originates, as Kwant says, in a dialogue or encounter between one human being and another, between a person and an object (24). The emphasis in this philosophical *Weltanschauung* is on the interplay, the encounter. Now all these human responses can be expressed in language or speech (identical for Kwant of course) although obviously not all with the same ease or fluency. Most of us, for instance, can explain better what happened in the ninth inning of a baseball game than why we feel at ease in the company of certain people.

Kwant's philosophical glance goes out in two directions then: first, to the origin of meaning—the interaction between the existence and the other—and secondly, to its expression in real sentences (*parole*). The area in the middle, the whole problem of precisely *how* these primitive meanings are put into speech, in other words *langue*, disappears from view. Does Kwant therefore imply that the nature of language and its mode of operation do not belong to philosophical inquiry, being left completely to the scientific
linguist? All right, but he did promise to introduce us "to the fundamental problems regarding language" (XI). No one, least of all the linguist, needs to be told that human experience is expressed in language; we take this for granted, I hope. The author himself reveals the bankruptcy of existential phenomenology for the philosophy of language when he admits that ultimately all the philosopher can do is call our attention to the mystery that is language, "whereby sounds are filled with meaning and construct a field of significations" (165). Of course language is mysterious; we don't have to read in existential phenomenology to learn that, but should philosophy be content with pointing to the mystery and telling us to accept it as a gift? (165). Surely it has more to offer the working scientist.

In the last analysis, Kwant's book is a study not of language in se but of its various roles or functions in the human situation. I do not want to be too harsh; in his discussion of such topics as: speaking in the affective mode (212), the living speech of daily life (214), function of speech in radio, television, and film (215), speech as influencing others (216), magic words (221), the religious word (222), Kwant is at times really profound. The reason for this is obvious. Here he feels perfectly at ease; his whole conceptual apparatus is directed to discussions of interpersonal relationships as the typical technical terminology of existential phenomenology makes abundantly clear: dialogue, encounter, openness, communication, confrontation, interplay, interaction. In this area valid and productive insights have been achieved; one wonders though, is it philosophy or psychology?

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Reviewed by B. HUNTER SMEATON

When a forester by background and early training turns to scholarship, as did the author, we have a happy exception to the ever-increasing compartmentalization of specialists. The validity of the study here reviewed, therefore, stems from an intimate knowledge of the subject matter combined with competence in research utilizing older sources.

The word material, derived from all 18th-century German works on forestry available to him (besides some earlier ones, and a few published in the early 19th century), is grouped into eight chapters, each with subtopics
(65 distinct word fields in all), and covers all aspects of tree structure, the characteristics of wood in growth, individual and group designations for trees, types of forests, forest management, cutting procedures, yield-estimating, grading, and the economic and administrative organization of German forestry. A summary treats German forestry vocabulary as a whole and its relationship to adjacent vocabulary fields, while two supplementary chapters deal with transportation to commercial centers (then almost exclusively by water) and the incidental economic functions of the forest and its by-products (loam, charcoal, potash, etc.). The work ends with a 19-page bibliography and a word index.

Due to obvious necessity, forest management was already highly developed in Europe when much of North America’s primeval forestland lay still untouched, and timber, in the frontier regions, was cut indiscriminately. Confronted with the German forestry terminology of two centuries ago, therefore (thus: Kahlschlag for ‘clear-cutting system’), the American or Canadian will find it curiously modern, as though the dimension of time had somehow been suppressed. Indeed, there is hardly a term in, for example, R. S. Troup’s *Silvicultural Systems* (Oxford, 1928) which does not, we find, have an 18th-century German equivalent!

For the structural semanticist there are rich lodes to work here. No linguist concerned with vocabulary can fail to be impressed at the narrowness of his own realm of experience when he views the vast ramifications of this single topical microcosm, and at how the layman’s lone linguistic sign may correspond to scores or hundreds for the expert, to whom so much more is relevant. He will also find dramatic illustration of the eternal shifting of content vis-à-vis expression, even within the limited boundaries in time and space here examined, and may be reminded of Meringer’s celebrated maxim: “Ohne Sachforschung keine Wortforschung mehr.” In the same connection one is struck by the evidence (however unneeded) of the overwhelmingly oral nature of the transmission of occupational vocabulary, which (on a frequency basis) rarely, as here, reaches print, even though a whole cultural domain is involved. Of interest also is the author’s observation on pages 227ff. that *Fachsprache* draws upon, and transcends, various regional dialects. To the reviewer’s knowledge, no all-inclusive theory of vocabulary transmission has ever been developed; and while the diachronics of occupational terminology would be only a small part of such a theory, it might make as good a point of departure as any.

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This volume on Louisiana French, first in a series of two, represents a departure from the usual analytic-descriptive models and seeks to give facts about the dialect both as a self-sufficient structure and from a comparative standpoint with reference to Standard French. After a preliminary historical and socio-cultural introduction, the authors present a full-scale phonology, morphology, and syntax of the dialect within the framework of the two above-mentioned perspectives. The volume contains also an extensive critical bibliography of general and specific works germane to the study.

According to Conwell and Juilland, Louisiana French is the cover term for the Acadian dialect, a term justifiable because this dialect is the dominant variety of non-English speech in Louisiana. Their thesis is that Acadian French is a rural dialect whose divergence from Standard French is mainly due to the English superstratum. The degree of divergence is not uniform, however, due to a variety of factors. The authors propose to describe a conservative variety of Acadian French, i.e., the Lafayette dialect. The LaP dialect is said to be conservative in that it diverges less from SF than other varieties of LF, e.g., less extension of oral vowel nasalization, less palatalization of dental and velar stops; fewer losses in categories of inflection or in form classes; less influence from English because of the geographic isolation of Southwest Louisiana until recent improvements in roads and communication. Further information about the dialect is that most informants for the study are from Lafayette, the largest city within an area representing a radius of about fifteen miles and including parts of St. Landry, St. Martin, and Lafayette Parishes. This area was selected due to what Conwell and Juilland describe as natural dialectal homogeneity. Besides, the area has socio-cultural homogeneity due to its natural isolation. It should be pointed out that communities selected for the study are strung along the Old Spanish Trail (now U.S. route 90), and the Bayou Têche, all following generally a North–South route through the selected area (West of the Mississippi River) with the Bayou Vermilion forming, part of the way, a boundary between Lafayette and St. Martin Parishes.

1 See p. 21.
2 Hereinafter referred to as LaF; Louisiana French will be referred to as LF and Standard French as SF.
3 See also W. von Wartburg, ACLS Bulletin, No. 34 (March 1942), p. 77, where he points out that it is impossible to delimit linguistic boundaries for Louisiana French.
One feature of the book that adds a great deal of interest for the scholar is the system of structural relations which underlies the whole description of Louisiana French and the comparison of this dialect with Standard French. In statements about phonology, the point of departure is the establishment of the dichotomy between prosodic and segmental elements. If linguistic constructions consist of segments such as phonemes, morphemes, logemes (constituents of syntactic constructs), and tagmemes (phrases, clauses, sentences), they are also characterized by supra-segmental features which determine other kinds of segments, e.g., syllable (pulse), phonetic word (stress), clause (pitch), sentences (pause). On the basis of this two-dimensional system, the authors proceed to discuss the correlation between segmental and supra-segmental entities. A phonetic word, defined by substantive (physical) criteria, such as stress (one stress or one combination of stresses per word), may not always coincide with the grammatical word, as determined by commutation or other criteria. Correlations between prosody and segmental entities are particularly relevant with respect to stress and the phonetic word. There are two kinds of stress: word stress (quantitative in SF and LF) and emphatic stress (intensity). In Standard French, word stress occurs automatically on the final syllable of the phonetic word; moreover, as the phonetic word is incorporated into larger utterances, it is the last syllable of the larger utterance which now assumes the word stress and the newly incorporated word becomes more indeterminate. In LaF, on the other hand, word stress tends to be unstable and may shift from the last syllable toward the first; or, upon incorporation into a larger utterance, the phonetic word may retain its stress, which then occurs side by side with the stress of the now last word of the construct. This change of accentual pattern also changes the rhythm pattern of the language. Emphatic stress, characterized by intensity, is a dynamic force that sets into motion a series of qualitative changes in the phonology, e.g., lowering of mid-vowels: chose = /ʃoz/, elle = /al/, etc.; consonantal affrication: chanson = /ʃfas/, acadien = /kadʒ/, rôti = /rotsi/.

In line with this "dynamic" view, the phonemics of the dialect is presented as being characterized by a great deal of instability (free variation),

because of a very unstable settlement history and the fact that "... the language... has undergone a strong influence of English. The intensity of this influence, however, varies not only from one place to the other, but from one person to another."

4 See A. Juilland, Structural Relations (S'-Gravenhage, 1961).

5 Cross-boundary relations (e.g. linking or liaison) between contiguous grammatical words illustrate physical indeterminacy in SF, and these phenomena are still largely preserved in LaF, although the behavior of word stress allows more independent status for the word.
because *parole*, in the Saussurean sense, plays a greater role than *langue* as a result of the absence of unifying factors (lack of a literary tradition or the effect of loan translations from English on an individual basis). The authors present a basic system of eight oral vowels, four simple vowels /i, y, a, o/ and four complex vowels /e, æ, o, ɔ/, the latter presented as archiphonemes showing neutralization of qualitative differences in certain positions. Four nasal vowels are listed for the dialect. Much underdifferentiation, nevertheless, occurs, e.g., (1) unrounding: y → i (dytu ∼ ditu); õ → ą (jarš ∼ faparā); ẽ → ĕ (fakē ∼ fakē); (2) rounding: a → ą (fātā ∼ fātā); (3) lowering: e → a (far ∼ far); u → o (amur ∼ amor); or (4) rising: a → o (fam ∼ fom).

It is suggested that, rather than dynamics, some of these alternations represent bilingual fluctuation between LaF, with certain archaic forms, and SF, as many informants seek to "correct" their speech in the direction of SF, e.g., fer ∼ far (cf. Eng. merchant: French marchand; English sergeant and French sergent), amur ∼ amor, and others. The case of feze ∼ feze or seki ∼ seki, under emphatic stress is, of course, a familiar phenomenon in SF. The example may be analyzed as the phone /ʊ/ being rounded under stress with subsequent loss of the feature of rounding, in some cases, e.g., ʊ → o → e, (cf. SF məʃo for məʃo).

The dialect has the same consonant phonemes as SF with the exception of /h/ and /r/ is said to have the allophones /t/, voiced linguo-alveolar trill, and /r/, voiced dorso-velar fricative, in complementary distribution.

Other types of alternations might more properly be considered to be on the morphological level, e.g., simplification of consonant clusters such as /ptk bdg/ before /r t/ rather than the alternation of phones. The case of /mɔnd ∼ mɔn/ or /lɔg ∼ lɔŋ/ could be regarded as neutralization of the feature of implosion and explosion. Palatalization of dental and velar stops before high front vowels would be analyzed as sub-phonemic, hence predictable, features. An analysis of sequences such as /maji, bije, kruwaje/ as /mai, bije, kruwaje/, under emphatic stress, would also simplify the analysis and phonemic rewrite. This is very typical of present-day French

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6 The use of /e/ for the morpheme *une* is on the level of morphology. Mention should be made of the confusing use by the authors of slashes for both phonemes, phones, and allophones.

7 For the fluctuation of *er* and *ar* in Old French see K. Nyrop, *Grammaire historique de la langue française*, Vol. I (4e édition; Copenhagen 1939, pp. 259–262; amor is common in texts of the Middle Ages.


9 For this feature, *ibid.*, p. 158.
pronunciation of vowels in hiatus, e.g., in utterances such as Kwaï, pays, Rouault, one might hear /kwai/, /pei/, /ruwo/. Each vocalic segment has full sonority and there is a slight and scarcely audible reduction in sonority as one makes the transition from one vocalic segment to another. The point is that the reduction in sonority is a very transitory phase and is followed immediately by the resumption of full sonority. This is not the case for the VS-nucleus in the realizations of the phonetically similar utterances in English, e.g., /kwái/, 'the bridge', /péyi/, as in payee, and /ruwów/ for the French author Rouault.10

These sub-phonemic features plus the occasional occurrence of English /æ/, as phonetic substitutions, are precisely the substance which defines and illustrates the multiple factors which cause the divergence of LaF from SF: utterances which are labeled as SF are really loanwords11 from that language (i.e., on the morphological level), while the bulk of phonological material recorded represents the Acadian dialect with an occasional bilingual interference from English. The very detailed listing of alternations was to some extent an anticipation of the section on comparative phonemics. Here again certain so-called phonemic variations would better be handled on the morphological level, e.g., je vais: /ʒva/, femme: /ʃom/, amener: /amne/ (cf. earlier French /grämer/ for grand'mère and grammaire)12, cela: /sala/, prenait: /prän/, dire: /di/, ici: /isis/.

Statements that LaF /i u/ correspond to SF /j w/ respectively in /bijê/for bien or /kruwaʒe/ for croyez are not necessary if one analyzes /j/ as a non-relevant phenomenon in vowel hiatus, under emphatic stress. There would then be a perfect correspondence in this case on the phonemic level between LaF and SF.

In a section on historical phonemics, the point of departure is that SF has presumably undergone changes and Conwell and Juilland seek then to classify the physiological nature of such changes, although it might seem better to see some variations as remnants of earlier forms of French, e.g., /amɔ/ for amour, /ho/ for haut, /ämne/ for amener, /pɔm/ for pomme, /hɔt/ for honte, /prän/ for prendre (cf. OF tenront).13

10 We believe that one cannot therefore properly speak of diphthongs, etc. in French but rather of successive sequences of full vocalic segments. For the history of the monophthongization of Old French diphthongs, see W. von Wartburg, Evolution et structure de la langue française (Cinquième édition; Berne, 1958), p. 124f.
12 The statement of the authors that LaF Ø often corresponds to SF /d/ as in grand'-mère is surprising, to say the least. A similar error is made in the statement "Pronunciation of the digraph au alternates between /o/ and /ɔ/ . . ." (47).
13 For tenront and the lack of consonant between n and r in Picard, see von Wartburg, op. cit., p. 83.
In addition to certain basic differences, the dialect is characterized primarily by a simplification in both categories and form classes typical of SF.\textsuperscript{14} Gender has been particularly affected by this tendency, e.g., {\textperthousand} ‘indefinite article’\textsuperscript{15} for both masculine and feminine nouns, extension of the invariable adjectives, especially after être, e.g., /eletfʁɔsɛ/ for elle était française but /is5bɛl/ for elles sont belles. The last example shows how gender has been lost in the third plural personal pronoun, where the plural morpheme ils is most frequently actualized as /i/ and may alternate with ça. Here we have a loss of gender plus a tendency toward an impersonal reference. This latter phenomenon is also observed in the case of the third singular pronouns (il and elle), with which ça and ce alternate in the dialect. For the first singular and plural, we note also a similar reinforcement of the impersonal reference, e.g., je or nous are frequently replaced by on. For the second person, it is to be pointed out that tu may be replaced by vous with no change in the verb form (e.g. vous connais). Plurality for the second person may consequently be indicated by vous-autres alone or in combination with ça.

The demonstrative is often replaced by the definite article or -ça is used to indicate the distance from the speaker, e.g., des musiciens-là, la chemise-ça for ces musiciens-là, la chemise-là respectively.

Number is often more consistently marked than gender, e.g. quelles hommes /kelfɔm/, quelles écoles /kelzekol/ and quatre allemands /kat alm5/. The form monde assumes a more collective content than in SF, e.g. le monde pauvre (cf. St. Martin Creole /munzɛmartɛ/).

The verb in the Indicative may be represented as follows:

\begin{center}
\begin{tabular}{l l l l}
\textbf{IMPERFECTIVE} & \textbf{PERFECTIVE} \\
Pres. & Il travaille & Il a travaillé \\
Past & Il travaillait & Il avait travaillé \\
Fut. & Il travaillera & Il aura travaillé \\
\end{tabular}
\end{center}

An additional category is the Imperfective Progressive as in il est après travailler, il était après travailler, il va travailler.

Other verbal forms are as follows:

\begin{center}
\begin{tabular}{llll}
Subjunctive & Conditional & Infinitive & Participle \\
Pres. qu'il soit & il serait malade & dire & pleurant \\
Past qu'il soit venu & il aurait joué & avoir dit & pris \\
\end{tabular}
\end{center}

\textsuperscript{14} Authors discuss invariables under syntax and variables under morphology, according to constituent (form) class. Since both systematic (paradigmatic) and syntagmatic (text) procedures show little variation in basic form classes of SF and LaF, the authors do not give detailed catalog of LaF morphemes and tend to rely on traditional assumptions as a result.

\textsuperscript{15} Cf. footnote 6.
Conditional sentences show two types: *si tu voulais, il aurait joué* (*si-clause in imperfect plus main clause in conditional perfect*) and *s’il faudrait lever les mains... ça aurait tous, les mains levées* (*si-clause in conditional plus main clause in conditional perfect*).

Morphological behavior of some verbs is different from corresponding forms in SF, e.g., *chanter* has \{jänt\} and *vendre* \{vô-n; -d-\}, *offrir* \{of-r-\} instead of \{fît\}, \{vô-d\} and \{ôfr\}.

Statements about syntactic relations are based on the synthetic-expansive (as opposed to analytic-reductive or IC) model. The minimal syntactic constituents are logemes (words). The fundamental syntactic constituents formed by logemes are noun and verb phrases, called nuclei, but whose constituents and their patterns of arrangement often differ markedly from SF, e.g., extension of freedom of occurrence in *j’avais grand, grand des petits garçons* (for *beaucoup de* ...), *après six ans passées* (for *à peu près*), *son manger* (for *repas*); or, intensification in *combiens beaucoup de farine* (for *combiens de*), *cette voiture-ca-là* (for *voiture-là*). Verb nuclei often show loss of function words, e.g. on *jouait* [de] *la musique*, quand vous *avez eu* [la] *musique*; le créole veut dire ... [de] *différents mélanges*; vous [vous] *souvenez* [de] *les mots*, ils [n’] *avaient jamais vu* ça.17

Nominal nuclear expansion may occur by juxtaposition (*nous-autres-on va, vous-autres-ça battent pas*), subordination (*un garde-soleil pareil son linge, la femme à mon grand père*); expansions for verbal nuclei occur by adding secondary nominal or verbal nuclei, syntactically bound to primary nuclei by means divergent from SF, e.g. *elle a un petit-enfant qui joue l’orgue* (for *jouer de l’orgue*), *ils ont demandé pour une poule* (for *demander*), *je préfère de rester ici* (for *préférer*), il est après *travailler* (for *il travaille*).

Nominal and verbal nuclei may be combined to form clauses (simple sentences), which may, in turn, be coordinated to form compound sentences or subordinated to form complex ones. Here again, function words marking such relations may be dropped, e.g., *c’était une femme smart,* [parcequ’] elle avait été au couvent; *mon père est mort* [quand] il était petit garçon; c’était [qu’] il [n’] y avait pas de brique.

The authors have sought to make two contributions: (1) to aid dialectology by establishing a framework for combining both a description of the dialect *per se* and a comparison of the dialect with SF; (2) exemplify the synthetic-expansive approach to syntactic analysis as a basis not only for a structural statement of the dialect but for standard French as well. It is in

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16 Intensification of an adverb was noted for *assez loin là-bas en arrière* (cf. English *go on down into the barn*). Repetition is also a frequent stylistic device with adjectives and adverbs, e.g. *il est supposé être gros, gros; j’ai un antique shop là-là.*

17 There can be also just plain ellipsis, e.g., *quand les œufs [viendront], on va avoir de l’argent.*
the area of phonology that the Conwell-Juilland descriptive and compara-
tive system is most clearly seen. They seek to reduce variants to basic in-
vants (-emes) on the basis of frequency of occurrence, cognateness to SF
or by inventory of sound change vis-à-vis the SF model.

Some shortcomings of such a procedure include (1) excessive reliance on
phonetic phenomena, thereby losing sight of well-documented historical
considerations (e.g. er ~ ar, o ~ u, etc.) and (2) mixing of levels by analyz-
ing as phonetic many alternations on the morphemic level (e.g., et ~ ets,
ve ~ va, apre ~ ape, ply ~ py, etc.). The concern for establishing a basic
form (SF), from which LF forms diverge, obscures certain phonological
facts which separate LF from SF, e.g. /h/ and the existence of more com-
plex sets of allophones, as ts in [trotsi] for t in rôti before high front vowels.

Conwell and Juilland feel that one derives the same form classes in LF
and in SF whether by the syntagmatic (segmentation) or the systematic
(identification and classification) procedures, and morphs are indicated as
divergent in terms of SF forms, e.g., the feminine indefinite article is said to
be une but, because of nasalization of une, one may have /ãn/ or /ẽn/ or
even /e/, forms which the feminine nouns share with the masculine. The
question may be asked whether we are dealing rather with the loss of the
gender category and the LF speaker has a morpheme containing the allo-
morphs /yn, ë õn ěn/ and he may choose at will from this stock.

With reference to the authors' thesis that Louisiana-French continues
the Acadian dialect and that the modern reflex of Colonial French is now moribund, it should be emphasized that the situation needs further appra-
sal by examining more closely the idea of intra-language leveling. Some
idea of this phenomenon may be gained from (1) a study of the lexicon of
Louisiana-French and (2) an evaluation of the grammatical structure of
Louisiana-French.

All statements, deriving from a study of the vocabularies of the several
varieties of non-English speech in Louisiana, point unerringly to a certain
amount of lexical uniformity, regardless of whether the discussion is about
LF or SF. It seems from the contact situation there, leveling of the voca-
bulary took place, with both LF and SF serving as sources. In the Lower
Mississippi Valley, settlement history would put Acadian (LF) forms on
the Western side of the river and standard forms on the East. Standard
innovations, nevertheless, have made some headway on both sides of the
river, moving North toward Baton Rouge. According to W. A. Read's

19 Wartburg, ibid., p. 79.
20 Read, op. cit., pp. 92–95.
detailed report on the geographic distribution of Louisiana words for 'mosquito', maringouin and moustique, one may, on initial inspection, assume that the isogloss separating the two items coincides with the Mississippi River. Further inspection shows, however, that maringouin and moustique are also found to be in competition in several towns West of the river and, what is more, both the LF and the SF words are found integrated side by side in the speech of settlements in and adjacent to Lafayette Parish. Moreover, the Read material shows that, while most of the LF vocabulary items are found West of the river in Southwest Louisiana, a number of Acadian as well as standard words are found to be uniformly used throughout the entire state.

This lexical leveling, resulting from widespread inter-dialectal borrowing, has resulted in a second type of leveling, i.e., phonological and morphological adaptation, through partial or complete phone substitution and blending of morphemes. This may account for the numerous morphophones which vary in shape by one or more phones or morphs yet cause no problem of communication, as already noted in examples given above.

As for loanshifts, English is the source of many, e.g., les samedis et les dimanches 'Saturdays and Sundays', une demi-tante 'a half aunt', le créole veut dire différents mélanges 'Creole means different mixtures', mon bon vieux mari 'my good old husband', il est supposé être gros 'he's supposed to be real big'. In morphology, the English verb may be the source of analogic leveling as, for example, in the loss of finite verb inflections: on sais versus vous connais, vous-autres tue. It is not entirely impossible that /tʃ3s/ for chance, /dʒ3s/ for just, /læŋ/ for langue are variations by analogy with English chance, just, and long respectively.

With these reservations, this reviewer would like to congratulate Conwell and Juilland for a thorough and detailed contribution to a very important chapter in the history of New World French.

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21 See Haugen, op. cit.
22 The use of this term is similar to that of Trager and Smith in some of their recent papers.

Reviewed by ILSE LEHISTE

The purpose of this book, as stated by the author, is to determine the place of quantity in the phonological structure of Swedish, and to provide facts about the physical correlate of the quantity of vowels and consonants in stressed syllables. Although the aims are stated in this order and a theoretical chapter precedes the presentation of a truly impressive amount of experimental data, the relative emphasis is on the side of the facts and their phonetic, rather than phonological, interpretation. The theoretical views of the author appear rather eclectic, and the conclusions somewhat overly cautious. It depends perhaps on the investigator's scholarly temperament more than anything else, whether he prefers cautious conclusions based on meticulous documentation, or bold generalizations that disdain the pedestrian investigation of possible counterexamples. I usually find myself on the side of the facts, and thus should be the last to criticize the author for excessive emphasis of experimental data; but after reading Elert's book, I find myself wishing that the two objectives would have been kept in better balance, and the facts subordinated to their interpretation. With this reservation out of the way, the diligence and thoroughness of the author merit the reader's unreserved admiration.

Of the nine chapters of the book, two are devoted to providing a theoretical background for the phonetic investigation. The Introduction starts from the very beginning, reviewing the methods of phonemic description and the different kinds of functions phonemes may have. The second chapter deals with the place of quantity in the phonological structure of Swedish. At the level of word phonology, the facts of quantity in Swedish can be easily stated. In every syllable with a higher level of stress, either the vowel or the immediately following consonant is long; all other phonemes are short. The author treats long and short vowels as allophones, although he is fully aware of their phonetic quality difference. Main stress (phonetically symbolized as [I] in accent I words and as [x] in accent II words) is considered to be in complementary distribution with secondary stress (symbolized as [j]), and both are treated together as 'higher level of stress'. In isolated words, one syllable has main stress. Secondary stress occurs on one of the following syllables, if the word has tonal accent II. In his phonemic notation of prosodemes (prosodic features with linguistic function), Elert uses the following symbols: /I/ to indicate 'higher level of stress', /< >/
around the domain of accent II, and /ː/ to indicate the length of a phoneme. Length is noted, for example, for /u/ in skalta₁ < skuːta > 'smack' and for /t/ in skutta₁ < skutːa > 'to leap'. The author never commits himself to a decision as to whether the quantity of the consonant is predictable from the quantity of the vowel, or conversely, although the various possible analyses are reported in some detail (pp. 39–43).

In isolated words, a long vowel may be followed by a short consonant or a word boundary, e.g., bot [buːt] 'remedy' and bo [buː] 'to live'. In a syllable with a higher level of stress, short vowels are followed by a long consonant, e.g. bott [butː] 'lived', past participle. In single words, a long vowel is not followed by a long consonant, but in compound words such sequences are possible. Their occurrence serves to indicate the presence of a morpheme boundary. Elert offers the following minimal set of three (pp. 37–38):

\[
\begin{align*}
\text{by-tävlan} & \quad [\times \text{by}:\text{t}:\text{e}:\text{vlan}] \quad \text{‘village contest’} \\
\text{byt-tävlan} & \quad [\times \text{by}:\text{t}:\text{e}:\text{vlan}] \quad \text{‘contest in changing’} \\
\text{bytt-tävlan} & \quad [\times \text{by}:\text{t}:\text{e}:\text{vlan}] \quad \text{‘contest with buckets’}.
\end{align*}
\]

The demarcative function of quantity thus has to be separated from its distinctive function, before a meaningful analysis of the place of quantity in the phonological system of Swedish can be made.

The problem is further complicated by the interrelations between quantity, tonal accent, and stress. The oppositions between long and short phonemes are to a large extent neutralized in unstressed positions in connected speech. The neutralization of prosodic contrasts in unstressed position applies also to the tonal accents. All prosodic features can also have an expressive function. The author does not attempt to provide final answers to all problems connected with the analysis of continuous speech.

Chapters 3–8 constitute a detailed report of the experimental investigation. Chapter 3 describes the selection of the eleven speakers and the preparation of the linguistic material. The speakers read a list of 118 words and a sentence list, containing about 290 sentences with words in which combinations of vowel and following consonant were varied systematically in similar phonetic environments. In addition, two speakers repeated 24 times a list of four sentences, containing two pairs of VC-combinations.

The tape recordings, instrumental analysis, techniques of measurement, and statistical analysis are described in Chapter 4. Oscillograms, pitch curves, and overall intensity curves were made by means of a Mingograph. A part of the recorded material was also analyzed by the 48-channel spectrograph designed at the Royal Institute of Technology in Stockholm (sample spectrograms and mingograph recordings are reproduced on
Detailed results of the measurements of duration are given in a series of tables (pp. 88–108 and interspersed in the text in later chapters). The tables contain also values for the arithmetic means, and for all populations of more than five items, the standard deviation, the standard error of the mean, and the level of significance of the differences of the means. Most of the calculations were done by an electronic computer.

Chapter 5 deals with the duration of the vowels and Chapter 6 with the duration of consonants immediately following stressed vowels. Chapter 7 considers the duration of the various vowel-consonant combinations and the ratio of vowels to consonants. The absolute duration values measured during the study show that the duration of segments, on the whole, corresponds to the phonemic degree of quantity. There was, however, a great dispersion of the measured values, and the ranges of long and short allophones overlapped considerably for both vowels and consonants. Greater conformity between duration and phonemic quantity was found when the ratios of long and short vowels to the immediately following consonants were considered. The best correlation of measured durations of phonetic segments and linguistically defined units was found when \( V/C \) ratios of pairs contrasting minimally in quantity, pronounced by the same subject, were compared (\( V:/C:V/C: \) ratios). If the duration of the long vowel is twice that of the short consonant and the duration of the long consonant is twice that of the short vowel, the ratio of their ratios is 4. Any increase of the \( V:/C \) ratio or decrease of the \( V/C \) ratio yields a higher \( V:/C:V/C: \) ratio. If there is no difference in the durations, the ratio is 1; thus a value larger than 1 should be obtained, if a phonetically manifested phonemic quantity opposition does, in fact, exist in the language.

In about 650 word pairs of the type \( \text{bot} [\text{bu:t}] \) ‘remedy’–\( \text{bott} [\text{but:}] \) ‘lived’, spoken by the same speaker, the \( V:/C:V/C: \) ratio was well over 1 in the majority of pairs; the average of all the pairs was 2.10, and the standard deviation 0.78 (p. 173).

The necessarily brief review cannot do full justice to the contribution made by Elert not only to the description of quantity in Swedish, but also to the body of general phonetic knowledge. The wealth of verifiable data, the meticulousness of observation and care in presentation make these chapters especially valuable.

Chapter 8 treats the durational differences between speakers. At least this reader was interested to find that the duration of sentences in the readings made by women was about 2.3% shorter than in those made by men (p. 179). This confirms the popular belief about differences between men’s and women’s rate of speech, although the measured differences do not quite reach the levels suggested by linguistic folklore.
Chapter 9 contains a brief summary of the results of the investigation, which is followed by a longer summary in Swedish. A bibliography of 314 items concludes the work.¹


Reviewed by Punya Sloka Ray

This is a very readable compendium of information and arguments concerning a number of situations of language contact as especially focussed on the peculiar social institutions which go by the name of national languages, in so far as these have appeared in the newly independent states. It deals with "the whole host of factors" which bedevil the question of "what language to speak and what language to teach." Since many linguists have had to give advice or to decide for themselves on such problems, little would be gained by classifying these as "really" anthropology, sociology or political science.

The first chapter is the statement of a philosophical basis, namely that languages are instruments of both social organization and of individual creativity, which are mutually dependent processes. The second chapter surveys interaction patterns between languages in contexts defined by science, religion and literary appreciation. The third chapter outlines the post-war phase of decolonization in its impact on the languages. The fourth chapter provides two up-to-date case histories, one of India and the other of Malaysia. The fifth chapter gives a summary of the report of a Symposium on Multilingualism held at Brazzaville in 1962 and lists six variables in the choice of a national language. Le Page points out that any decision is likely to involve bilingualism for many—probably most—of the population, so that a properly oriented linguist might be just as helpful in reaching the best decision as in reducing the damage to the educational system possible from a mistaken decision.

The most welcome feature in Le Page's thinking is the constant emphasis on the comparative economics of social decisions affecting languages. He is also quite refreshing on the issue of the vernacular as a medium of instruction by his massing of the considerations against it. And his is the only dependable account that the reviewer has found of the linguistic situation

¹ The reviewer could not help but notice that her paper on juncture (Ilse Lehiste, An Acoustic-Phonetic Study of Internal Open Juncture. Supplement to Phonetica V (1960)) is erroneously quoted and listed as a co-authored paper.
in Malaysia, where 3 million Malays, 2.6 million Chinese and 600,000 Tamils live together in an efficient democratic polity.

The weakness of Le Page's well informed and humanitarian wisdom is that it is not based on any explicit theory. What exactly is the effect of partial privacies within a larger public, of limited privileges accorded to exclusive groups within an inclusive group? It is the greatest problem for every new state to find a massively acceptable reason for existence. It is hard to find a unifying purpose that will dominate without oppression, to lead without jealousy, to rule more by persuasion than by force. It is easy enough to err now towards chaos, now towards tyranny. To the extent that the golden mean has not been achieved, one would also need a canon for good second choices between moderate chaos and moderate tyranny. For these reasons it is not enough to recognize the danger of the chaos that might be promoted by too high a status for all the vernacular languages or of the tyranny that might be imposed by too high a status for only one of the vernacular languages. It so happens that the best solution for both India and Malaysia has been the retention of English, but an explanation of the facts is not found in the gospel of tolerance.

The problem in language contacts is not separable from the larger one faced by any society at any time or in any place. A society cannot survive if it does not maintain a select class especially able to compete and cooperate with the best of the other societies with which it is in contact. This indispensable inequality must be reflected as an economic inequality if the society in question does not happen to be richer than those with which it is in contact. Yet economic inequality within a state is even more intolerable today than economic inequality between states. The essence of democratic politics is the assurance that the various cultural or racial inequalities within the state are not allowed to bunch together to strengthen the economic inequalities but are encouraged to blunt them and each other.

It is just as necessary, however, to maintain the separate identities of the divisions. For liberties suppressed in peacetime will inevitably tend to assert themselves in any crisis. The combination of deliberately vague boundaries and deliberately accorded privacies is not always understood by leaders of countries just emerging from under very undemocratic colonial régimes, or indeed by their well-meaning foreign advisers. But once the basic principle has been understood in the country concerned, the question of whether to have a national language at all and then which it is to be reduces itself to the consideration of proportions and distributions in details.

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