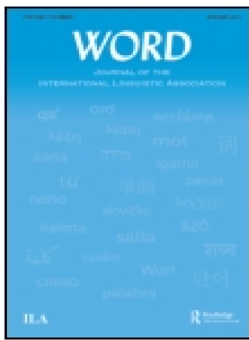

WORD

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Three Methodological Remarks on “Fundamentals of Language”

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DISCUSSIONS

THREE METHODOLOGICAL REMARKS ON "FUNDAMENTALS OF LANGUAGE"

Though the booklet *Fundamentals of Language* by Roman Jakobson and Morris Halle[1] does not contain much material that is new relative to earlier publications by these authors, the fact that this booklet is more or less self-sustaining makes a critical examination of some of its major methodological points a relatively simple and rewarding task.

For the following discussion, I selected three theses; I shall examine them not in the order in which they appear in the booklet but rather in the order of increasing importance (as I see it, not necessarily as the authors would judge them).

1. The authors present a new version, couched in their "code" terminology, of the old thesis of the primacy of speech over writing. I shall not deal here with the history of this thesis nor try to disentangle the different, sometimes radically different, interpretations given to it by various linguists, but concentrate on the formulations given on pp. 16-17 of the *Fundamentals*.

The authors start with the factual statements, "In contradistinction to the universal phenomenon of speech, phonetic or phonemic writing is an occasional, accessory code that normally implies the ability of its users to translate it into its underlying sound code, while the reverse ability, to transpose speech into letters, is a secondary and much less common faculty. Only after having mastered speech does one graduate to reading and writing." (I believe that these statements will only stand under an extremely liberal and benevolent interpretation of the word 'normally', but, for my present purposes, I need not, fortunately, enter into a discussion of these statements at all.) Then they

[1] R. Jakobson and M. Halle, *Fundamentals of Language* (Janua Linguarum Nr. 1), Mouton & Co. 'S-Gravenhage 1956.

go on to make the following methodological statements: "There is a cardinal difference between phonemes and graphic units. Each letter carries a specific denotation—in a phonemic orthography, it usually denotes one of the phonemes or a certain limited series of phonemes, whereas phonemes denote nothing but mere otherness (cf. 2.3). Graphic signs that serve to interpret phonemes or other linguistic units stand for these units, as the logician would say."

It seems to me that the mode of expression chosen here by the authors is in many respects unfortunate and misleading. The terms 'denote' and 'denotation' occurring here as well as on pp. 10-11, to which the authors refer in the last-quoted passage, are used with such a high degree of ambiguity and vagueness, and with such a high degree of deviation from both ordinary and logico-semiotical usage, that the understanding of the sentences in which they appear is highly impeded. The phrase, "phonemes denote nothing but mere otherness," if you take away from it its metaphysical flavor and the associations with some of the worst formulations of Ch. S. Peirce and F. de Saussure, seems to mean no more than what would be expressed in ordinary language by something like "Phonemes do not denote and their functioning is purely differential," with some elaboration needed to clarify the matter of 'differential functioning'. But when the authors contrast the denotation of a phoneme with the denotation of a letter, their usage, "[a letter] usually denotes one of the phonemes," carries the misuse of 'denote' beyond possible justification. A letter does not denote anything, not even in the most stretched sense of 'denote'—in general, of course, because within certain symbolic formulae letters very definitely denote; but the authors clearly do not refer to this exceptional usage. Between a letter in a phonemic orthography and a certain phoneme there exists, of course, by definition a certain relationship, but it is not that of denoting. The status of a letter, in a graphic system, is *analogous* to that of a phoneme, in a phonemic system. They are counterparts in their respective systems. This seems to a logician so obvious that he is often at a loss to understand all those distinguished linguists who claim otherwise. The explanation that the linguists are confusing here historical with logical primacy, the asymmetricity of the relation 'precedes-in-time' with the asymmetricity of 'denotes', is not always sufficient, certainly not in the present case. Could it be that the authors here fell prey to the fallacy, well-known to logicians, of "the confusion of use and mention

of signs"? Could it be that because something that looks like a letter is often used to denote a phoneme—the phoneme /k/ being customarily denoted by '/k/'—that their misleading formulation arose? (Notice that nothing is denoted either by the letter 'k' or by the phoneme /k/, whereas the letter 'k' is denoted, for instance, by "k", though so-called autonymous denotations[2] are also often used by linguists, with certain confusions occasionally arising.

At the end of the passage, the authors try to strengthen their point by invoking a musical analogy: "One could neither state that musical form is manifested in two variables—notes and sounds—nor that linguistic form in manifested in two equipollent substances—graphic and phonic. And just as musical form cannot be abstracted from the sound matter it organizes, so form in phonemics is to be studied in relation to the sound matter which the linguistic code selects, readjusts, dissects, and classifies along its own lines. Like musical scales, phonemic patterning is an intervention in nature, an artifact imposing logical rules upon the sound continuum." This analogy is interesting but totally wrong. The sound *re* is not analogous either to a phoneme or to a letter, not even to a morpheme, but rather to what a morpheme denotes. This sound can be denoted by a certain geometrical pattern of lines and ellipses just as it can be denoted by (the written letter-sequence) 're' or (the spoken sound) [re]. In music, the denotata of the various possible denotational systems *are* sounds—with *other* sounds, in some of the systems, denoting them; but in linguistics, phonemes and letters have no denotata, whereas the denotata of morphemes and graphemes are, in general, non-linguistic entities—'dog' and /dog/ both denote dogs (though they also fulfill other functions).

The word 'logical' in the last sentence of the quoted passage is irksome. In order to interpret the function of this word in the phrase 'an artifact imposing logical rules upon the sound continuum', as well as in many other phrases used by Prof. Jakobson in prior publications, I replace it by—zero. Could I be mistaken?.

2. On the whole, there can be no good arguments against trying to reduce the set of phonemes of any language (or all

[2] I.e., a situation where a sign is denoted by itself, or rather where a sign-type is denoted by one of its sign-tokens. Cf., e.g., R. Carnap, *Logical Syntax of Language*, London-New York 1937, § 42, A. Tarski, *Introduction to Logic*, New York 1946 (2nd ed.) § 18, or W. V. O. Quine, *Mathematical Logic*, New York 1940, § 4.

languages) to a smaller set of other entities. And there are many good reasons for using such more basic units, call them 'features'. The specific treatment, however, which this reduction receives in *Fundamentals* leaves me still not quite convinced, and the claim for exclusiveness and uniqueness of the presented analysis seems to me totally unjustified. But a detailed criticism, though certainly a most wortwhile enterprise, is beyond the frame of these remarks. I intend to comment here on one aspect only of the whole problem, which has some special methodological interest, viz. to the claim of the authors that the distinction they make between distinctive and redundant features is an absolute and intrinsic one. I do not think that this claim is anywhere explicitly formulated in the *Fundamentals* but it is everywhere implicitly involved.

If ten people gather in a room containing six chairs, four people are "redundant," in the sense that even if the seating capacity of the room is fully exploited—assuming that only one person can sit in one chair at a time—four people will remain unseated. So far, it makes no sense to say that John, Bill, Mary and Ann are redundant whereas the other six are not. Only by introducing additional considerations and conventions can an assignment of the redundancy to certain four people be justified. One might, for instance, take the time of arrival into consideration or invoke a "family hold back" principle or take age into account. None of these assignments, however, can make any claim to exclusiveness or naturalness.

When the articulation of a certain sound S in a language L can be characterized by its possession of the features A, B, C, D, and E—no "different" sound (=type or=design) possessing the same features, and it turns out that wherever a sound in L possesses feature D it also possesses feature E, and vice versa, and that whenever a sound in L, if produced under certain circumstances or in a certain environment, possesses feature B, it also possesses feature C, and vice versa, it makes sense to say that the *feature set* [A, B, C, D, E] characterizing S is redundant to a certain degree under any conditions and in any environment, and to an even stronger degree under certain specific conditions or in certain specific environments. So far, it makes no sense to single out the feature D, rather than E, as being an absolutely redundant feature, or to single out B, rather than C, as being a conditionally redundant feature. Such a singling out is extraneous to the situation, though there might be certain purposes for which a conventional assignment of redundancy to some of the features, instead of to the

whole set, might be a worthwhile procedure. But it must be clear that for different purposes different assignements are appropriate, and that for some purposes no such such assignment might turn out to be helpful.

If in French, the phonemes /b/ and /p/ share so many features but differ (only) in that /b/, under normal conditions, is implemented by a lenis and voiced sound, whereas /p/ is implemented by a fortis and voiceless sound, there being no phoneme whose implementation, under normal conditions, shares the common features of /b/ and /p/ with them but is either lenis and voiced or fortis and voiceless, then the feature sets [—, —, —, —, lenis, voiceless] and [—, —, —, —, fortis, voiced] are redundant (the dashes indicating the common features). But so far, there can be no justification for the claim that the lenis/fortis opposition (to change now somewhat the terminology, in accordance with the author's none too consistent usage) is distinctive, whereas the voiceless/voiced opposition is redundant. If the authors still make just this claim, they seem to justify it by considerations of simplicity and elegance of characterization. Characterizing /b/ by [—, —, —, —, lenis] is certainly simpler, more elegant and more economical than characterizing it by [—, —, —, —, lenis, voiceless]. (The assignment of distinctiveness to the lenis feature rather than to the voiceless feature is due to still other considerations that need not be discussed here.) But this gain is a spurious one. The statement that the implementation of /b/ by a normal French speaker under normal conditions is voiced, rather than voiceless or indifferent with respect to these features, has still to be made somewhere. Simplicity is a virtue of a whole system. An increase of simplicity in one part of a system, if accompanied by a decrease in the remainder, is of doubtful value. But the situation in our particular case is even worse. The authors are obliged to state that under abnormal conditions, such as energetic shouting, or whispering, the distinctive function of the distinctive features may be taken over by the (normally) redundant features (pp. 9-10). Here the spurious elegance surely backfires. No such statement has to be made at all for a characterization of phonemes by redundant feature sets. From a comparison with the other feature sets one can read off immediately whether the disappearance ("neutralization") of certain features under certain conditions will or will not lead to a loss of unique identification of the phoneme involved.[3]

[3] I believe that C. F. Hockett intends to make a similar, if not identical, point in

The taking over of the distinctive function by a (normally) redundant feature should by no means be understood as a psychological observation, as if the listener, under normal conditions, were to take account only of the distinctive features, whereas he attends to the redundant features only under abnormal conditions. This interpretation is explicitly repudiated by the authors. To use a certain well-known slogan, they do not claim that the distinctive features have a greater psychological reality than the redundant ones.

Altogether, I believe that there remain no good reasons for the distinction between distinctive and redundant among the features. I believe that all the valuable points incorporated in this terminology can be made as simply, or even more simply, by the 'redundant feature set' terminology which avoids, in addition, the less desirables points of the 'distinctive-redundant feature' terminology.

For those, who are not yet convinced of the pointlessness of the "economy" introduced by the distinctive-redundant division, let me give an analogy from elementary geometry. A rectangle is customarily defined as a quadrangle whose four angles are right angles. One might accuse this definition—and some mathematicians did so—of redundancy: it would suffice to require in the definiens that at least three angles should be right; for Euclidean geometry the rightness of the fourth angle can then be proved. Though this might be a useful thing to do for certain axiomatic purposes, it is regarded as pointless by most mathematicians. The gain in "economy" is offset first by a certain loss of intuitiveness—which has its pedagogical values, to say the least—and second by the fact the statement that every rectangle has four right angles, an immediate consequence of the ordinary definition, has now to be laboriously proved. On the other hand, of course, under the ordinary definition a theorem has to be proved to the effect that any quadrangle with at least three right angles has exactly four right angles. Incidentally, such a theorem seldom occurs in ordinary textbooks, because of its relative unimportance. (The reader will find no difficulty in pointing out the weaknesses of the analogy, but my argument does not depend only on its strength.)

his criticism of the "determining-determined"-terminology; see his *A Manual of Phonology* (Indiana University Publications in Anthropology and Linguistics, Memoir 11), Baltimore, 1955, especially pp. 172-175.

3. The authors dedicate much space to the discussion of the relationship between phonological entities and sound. They distinguish between what they call an *inner*, immanent approach adopted, for instance, by Bloomfield and themselves, and various *outer* approaches. Unfortunately, the mode of expression they use to explain the inner approach is rather metaphorical; locutions like "the distinctive features are present in the sound waves," or "the inner approach locates the distinctive features and their bundles within the speech sounds, be it on their motor, acoustical or auditory level" are certainly not to be taken literally. And they are of little help to someone who, for methodological reasons, is interested in the relationship between phonemes, features and sounds. They even look slightly inconsistent—sound waves have no motor level—though this can easily be remedied.

I shall not go into a discussion of the authors' presentation of the various outer approaches and the validity of their criticisms. None of the views presented, including the authors' own one, is completely convincing, especially because of their heavy reliance on metaphors and the subsequent lack of clarity and preciseness. Nevertheless, almost all views presented seem to contain some grain of truth, and their mutual inconsistency seems occasionally to be due more to unfortunate modes of expsession than to the fact that all except at most one must be wrong.

It is at this point that a certain methodological insight, recently obtained—or, to be more precise, recently clearly formulated—might be of help, both in eliminating inessential differences and in bringing into focus the essential ones. The terminology, in which this insight will be formulated here, is in part already quite customary among psychologists, with the remainder coined in the investigation of Professor Rudolf Carnap on the methodological character of theoretical concepts,[4] in which this insight has found its concise formulation.

Let me give a rough outline of the main ideas of this investigation, insofar as they are of relevance to our present problem. Many methodologists of science, though not all, distinguish between two parts in the language of science, the *observational* part, on the one hand, and the *theoretical* part, on the other. (The actual

[4] R. Carnap, "The methodological character of theoretical concepts" in: Feigl and Scriven (eds.), *The Foundation of Science and the Concepts of Psychology and Psychoanalysis* (Minnesota Studies in the Philosophy of Science, vol. I), Univ. of Minnesota Press, 1956.

terms are, of course, sometimes quite different.) A logician might prefer to speak about two languages of science instead of two parts of the one language. I shall use here an intermediate way of expression and talk of the two *sublanguages* of science, the observational and the theoretical one. This distinction is mostly drawn with respect to physics, for certain well-known historical reasons, but is now occasionally applied to psychology. I intend to show that a similar distinction should prove to be of great methodological usefulness also for linguistics.

The observational sublanguage of physics contains such terms as 'warm', 'loud', 'red', 'warmer than', 'louder than', 'brighter than', referring to observable properties and relations. The theoretical sublanguage of physics, on the other hand, contains such terms as 'atom', 'spin' and 'electromagnetic field'. According to some conceptions, the observational sublanguage contains also such simple quantity terms as 'temperature' and 'weight'. Each sublanguage contains, of course, also all those terms that are explicitly definable on the basis of the primitive descriptive terms of this sublanguage (in addition to the logical terms). This last statement will later be expanded.

Since the terms of the observational sublanguage are ordinary words and phrases (say, of English) or their one-to-one symbolic counterparts, and their combination into sentences follows the rules of ordinary syntax (or, again, their simple symbolic counterparts), no problems arise as to the interpretation of the sentences of this sublanguage.

The situation is different with regard to the theoretical sublanguage. Unfortunately, it is impossible, without presupposing a considerable amount of knowledge in modern logic, to describe in detail the logical structure of this sublanguage. A certain loss of preciseness in the following discussion is the inevitable result. It is hoped, however, that this loss will not seriously impair the value of this discussion.

The theoretical sublanguage will contain a number of primitive descriptive constants, in addition to its logical vocabulary, as well as the terms defined on their basis. Instead of 'theoretical terms', the expressions 'theoretical constructs' and 'hypothetical constructs' are often used. A theory, formulated in this sublanguage, consists of a finite number of *postulates* containing, in addition to logical terms, only the theoretical terms. Finally, *correspondence rules* are given, which connect the theoretical terms with the terms of the observational sublanguage.

The theory by itself, without the correspondence rules, is an *uninterpreted calculus*. Its terms and sentences are so far without meaning, and the theoretical language, so far, is useless as a means of communication. With the adjunction of the correspondence rules, the theoretical terms become interpreted, the theoretical sentences meaningful, the whole theoretical language a means of communication. However—and here comes the decisive new insight—these correspondence rules connect in general only *some* of the theoretical terms with the observational terms, providing them with a direct though still in general incomplete interpretation, whereas the remainder of the theoretical terms receive their interpretation only indirectly and still more incompletely by their being connected with the first terms through the postulates. Let me stress at this point that, without going into a detailed description of the form of the correspondence rules, these rules should by no means be considered as definitions, not even as disguised ones; such a shapeless conception of definitions would, in our case, result in much harm to an understanding of the situation.

I promised before to elaborate on the status of terms definable on the basis of the primitive descriptive terms of the observational sublanguage. The situation is quite complicated, due to the fact that one may be more or less liberal with regard to the structure of the definitions admitted for this purpose. The strictest approach would admit only explicit definitions of an extensional form (i. e. a form that does not involve either logical or causal modalities). Less strict approaches would also admit the introduction of non-primitive terms either by means of explicit definitions in a non-extensional form, i. e. using modal terms such as 'possible' and 'necessary' or subjunctive conditionals, or by means of certain kinds of conditional definitions, so-called reduction sentences. The most important kind of terms introduced by definitions of the latter types are the so-called *disposition terms* like 'elastic' or 'brittle'; such terms, if their introductory phrase makes no use of theoretical terms, will be called *pure disposition terms*.

I already mentioned that not all philosophers of science subscribe to the double-sublanguage view. Operationists, like the physicist Bridgman and the psychologist Skinner, would claim that the language of science need not contain anything besides terms that are either logical or else observable predicates or pure disposition terms; no scientific term needs to be considered as a theoretical

term. Carnap, like many other scientists and philosophers, regards this view as too narrow and as not easily reconcilable with current scientific practice.

I am afraid that the outline given here was none too convincing. The interested reader will be well advised to read Carnap's article in extenso as well as a careful and detailed paper by C. G. Hempel on the same topic, forthcoming in the second volume of the Minnesota Studies in the Philosophy of Science. As to psychology in particular, the paper by MacCorquodale and Meehl[5] should be consulted, whose distinction between *intervening variables* and *theoretical constructs* seems to coincide, more or less, with that between pure disposition terms and theoretical terms, though the observation language to which the authors refer contains also certain quantitative terms.

It must therefore come rather naturally to try to apply this whole view, or rather this whole gamut of views, to the present status of linguistics. I do not think that this has been done before.

It seems, if one is somewhat bold in the interpretation of the various metaphors that mar the authors' discussion of the "inner" and "outer" approaches on pp. 8-17, that this distinction mirrors once more the distinction between the pure-disposition-term view (or the one-language view) and the theoretical-term view (or the double-sublanguage view). The authors, following Bloomfield, insist on defining phoneme (and distinctive feature) on the basis of the observable properties of speech sounds, though these observable properties have again to be interpreted in a liberal way and to include properties whose establishment involves certain measurements. The adherents of the various variants of the outer view, on the other hand, seem to regard the terms 'phoneme' etc., as theoretical terms, whose connection with "concrete sounds" is only indirect and incomplete.

So far, this is, of course, all very vague and perhaps unconvincing. Only a very detailed study could conclusively prove the fruitfulness of showing that the recent methodological quarrels within linguistics fall into a pattern well-known from other sciences. Such a study will not be easy: the ways in which linguists are used to formulate their methodological attitudes is often very idiosyncratic and metaphorical, and a constant danger of misinterpretation lurks

[5] K. MacCorquodale and P. E. Meehl, "Hypothetical Constructs and Intervening Variables," *Psychological Review* 55 (1948), reprinted in Feigl and Brodbeck, *Readings in Philosophy of Science*, New York 1953, pp. 596-611.

behind any attempt to put their formulations into a unified terminological framework.

The following scattered remarks will therefore be of a programmatic and sketchy nature.

a. I already once[6] had the opportunity of calling attention to the fact that insufficient knowledge of recent developments in logic and methodology often causes linguists anxieties over the seemingly unbridgeable disagreements on such issues as the definitions of the most basic concepts. Let me intimate, as an illustration, that the difficulties in coming to agreement on the definition of phoneme might not be due to the ineptitude of the (fellow-) linguist but rather to the fact that such a definition cannct be given, at least not in the form of an explicit definition using observable predicates only. It might perhaps be more appropriate to treat 'phoneme' as a primitive theoretical term, connected by postulates with other theoretical terms like 'feature', 'environment', 'immediately preceding' etc., and connected with the observational terms like 'speech sound', 'phone', 'velum', directly or indirectly, by certain correspondence rules. Instead of being bewildered by the failure of arriving at a generally acceptable definition of the basic terms of linguistics and by the accompanying weird controversies, linguists could now perhaps come to an agreement on the underlying postulates and on the worthwhileness of transferring their disagreement to the exact form of the correspondence rules to be adopted. The old schoolbook recipe of 'define your terms' has now to be recognized as misleading and unjustified, especially when the terms involved are theoretical ones. The new recipe, 'list your postulates and correspondence rules', is less attractive as a slogan but more efficient.

I believe that the double-sublanguage view is already implicit, for instance, in the "fictionalist" view of Twaddell, criticized by the authors on pp. 13-14. It is understandable that scientists who have not quite mastered the methodological niceties of the double-sublanguage approach to their discipline will be worried about the ontic status of their theoretical concepts and, misled by the fact that these concepts are not defined in terms of observable predicates, tend to assign them a kind of second-quality reality, often dubbed as "fictional." This again will irk other scientists who will interpret the fictional character of these theoretical

[6] Y. Bar-Hillel, "On Recursive Definitions in Empirical Sciences", *Proceedings of the XIth International Congress of Philosophy*, volume V, 1953. pp. 160-165.

constructs as being "arbitrary," "having no necessary correlate in concrete experience" and who will feel that the objective value of this discipline is thereby endangered. A clear understanding of the workings of the two sublanguages of science should completely eliminate these quibbles.

b. I hope that I am not using too much force when I interpret the "algebraic" approach of Hjelmslev as an attempt to stress the theoretical character of the basic linguistic notions. That the "expression plane" of language can be studied without any recourse to phonetic premises—as the authors formulate Hjelmslev's thesis—may mean nothing more than that the postulational theory of the linguistic structure of a certain language can be usefully studied as such, without having to invoke at every stage the correspondence rules, a thesis to which I think one cannot but consent. Without the correspondence rules, however, the theory remains an uninterpreted calculus and is, therefore, in a certain important sense, not a linguistic theory at all. I venture to interpret the authors' criticism of Hjelmslev's approach as accusing it of playing down the role of these rules, on the one hand, which might be correct, and as a critique of the double-sublanguage view in general, on the other, which seems to me unsubstantiated.

I hope to have made it clear that I am not interested in cheap peace-making or in an uncritical eclecticism but that it is my firm belief that many of the currently raging methodological controversies in linguistics are due to misunderstandings that are based, in their turn, on an insufficient utilization of the tools that modern general methodology is able to put at the disposal of the scientists. I am, of course, quite aware that an important role in these controversies is also played by all kinds of overt or hidden metaphysical beliefs cherished by the participants in these controversies. I hope that a unified terminology will also help to assess the exact role played by these beliefs, and hence perhaps help to overcome their detrimental effects.

It is my impression that the double-sublanguage view will also solve another puzzle (at least it was a puzzle to me). I refer now to the relation between utterance and sentence. The customary view which regards a sentence as a kind of utterance not only runs into seemingly insuperable difficulties in determining the specific nature of this kind but left me, at least, dissatisfied in principle. I had always the feeling that linguists *treat* these two terms as being on two entirely different levels, whereas they *talk* about them as belonging to the same level. I am quite convinced

now, and I think that not too much deliberation is required in order to come to this conviction, that 'sentence' is best treated as a theoretical term—as used by linguists, not necessarily as used in ordinary life—whereas 'utterance' is rather an observational term. What connects 'sentence' with 'utterance' is not a definition but rather a set of correspondence rules.

Let me stress again that my last remarks were sketchy and occasionally rather dogmatic. A more systematic and better documented treatment will perhaps be undertaken at some other occasion.

Hebrew University, Jerusalem YEHOSHUA BAR-HILLEL.

A MANUAL OF PHONOLOGICAL DESCRIPTION

In the past few years a number of introductory textbooks and basic manuals on phonological analysis have appeared, but, in spite of this, most instructors of elementary courses in the field still seem to prefer their own approaches, and those who have not yet written a book on the subject often like to dream of an "ideal" textbook. The distinguished linguist André Martinet has now produced his entry, in the form of a revised and enlarged version[1] of his phonological study of the Franco-provençal speech of Hauteville, which originally appeared about ten years ago.

Martinet's aim, as indicated in the preface, was to provide a book which would combine a theoretical orientation with a detailed model of the descriptive technique, and which would be able to serve as an introduction to phonology and as a preparation for the understanding of more fundamental works. He succeeded in his aim: the book is an admirable introduction to the field and will undoubtedly be widely used in elementary courses. It is only reasonable to suppose, however, that individual linguists will continue to present the theory and practice of phonological analysis in accordance with their own views and favorite examples, and that this book like its predecessors will fall short of the ideal

[1] André Martinet, *La description phonologique avec application au parler franco-provençal d'Hauteville (Savoie)*, Société de Publications Romanes et Françaises LVI. 108 pp. Geneva: Librairie Droz, Paris: M. J. Minard, 1956.