Chapter 1

The State of American Linguistics in the Mid 1950s

1.1. A PERIOD OF OPTIMISM

If American linguistics was in a state of crisis in the mid 1950s, few of its practitioners seemed aware of it. Einar Haugen (1951), in his overview of the field, wrote that "American linguistics is today in a more flourishing state than at any time since the founding of the Republic" (p. 211). Commentators boasted of the "great progress" (Hall 1951:101), "far reaching advances" (Allen 1958:v), and "definitive results" (Gleason 1955:11) achieved by linguistics, which was "compared in method with field physics, quantum mechanics, discrete mathematics, and Gestalt psychology" (Whitehall 1951:v). Even Kenneth Pike (1958), often critical of many of the assumptions of the dominant American linguistic theorists, felt moved to write that "theirs [Harris's and Bloch's] is an attempt to reduce language to a formal analysis of great simplicity, elegance, and mathematical rigor, and they have come astonishingly close to succeeding" (p. 204).

More than self-congratulation was going on. The psychologist John B. Carroll (1953) wrote that linguistics was the most advanced of all the social sciences, with a close resemblance to physics and chemistry. And Claude Lévi-Strauss, probably the world's foremost anthropologist, compared the discovery that language consists of phonemes and morphemes to the Newtonian revolution in physics (Lévi-Strauss 1953:350–351).

There was a widespread feeling among American linguists in the 1950s that the fundamental problems of linguistic analysis had been solved and that all that was left was to fill in the details. The basic theoretical–methodological statements of Bloch's "A Set of Postulates for Phonemic Analysis" (1948) and Harris's *Methods in Structural Linguistics* (1951) seemed to render any more basic theoretical work

unnecessary. In fact, many linguists felt that the procedures had been so well worked out that computers could take over the drudgery of linguistic analysis. The time was near at hand when all one would have to do would be to punch the data into the computer and out would come the grammar!

There was also a feeling that computers could solve another traditional linguistic problem—translation. The idea of machine translation had been first suggested (in a memorandum by Warren Weaver) only in 1949. By 1955, such translation work was going on in three countries at half-a-dozen institutions. These six years were enough to convert the skeptics, as William N. Locke put it in an enthusiastic review article written in that year (Locke 1955:109).

Other postwar scientific developments seemed to be especially promising for linguistics. A new field called "information theory" proposed methods of measuring the efficiency of communication channels in terms of information and redundancy. Shannon and Weaver (1949) in their pioneering study of information theory pointed out the possible linguistic implications of the theory:

> The concept of the information to be associated with a source leads directly, as we have seen, to a study of the statistical structure of language; and this study reveals about the English language, as an example, information which seems surely significant to students of every phase of language and communication. (p. 117)

Shannon and Weaver's ideas were enthusiastically received by a large number of linguists; prominent among these was Charles Hockett (1953, 1955), who set out to apply the results of information theory in the construction of a Markov-process model of human language.

Progress in acoustic phonetics also contributed to the general optimism. The spectrograph, first made public in 1945, had replaced the inconvenient oscilloscope as the most important tool for linguists in the physical recording of speech sounds. There was a general feeling that spectrograms would help decide between competing phonemicizations of a given language, the existence of which had posed a perennial problem.

Finally, the synthesis of linguistics and psychology was adding a new dimension to the study of language. The behaviorist theory of psychology developed by Clark Hull (see Hull 1943) provided the linguists of the period with the theoretical apparatus they needed to link their approach to linguistic description with theories of language acquisition, speech perception, and communication. By the early 1950s, an interdisciplinary field of psycholinguistics had emerged, with important seminars being held at Cornell in 1951 and Indiana in 1953. The progress reports from the latter seminar were published in the first volume of papers dealing with language and psychology, Osgood and Sebeok (1954), a work hailed by one reviewer as "a scientific event of great importance" (Olmsted 1955:59). Among many other topics, the book dealt with issues such as the psychological verification of the phoneme and the psychological criteria that would help decide between competing linguistic analyses. The book itself inspired work conferences on many related subjects, which themselves resulted in influential publications on content analysis (Pool

1959), stylistics (Sebeok 1960), aphasia (Osgood and Miron 1963), and language universals (Greenberg 1963).

Linguistics seemed so successful that it was being consciously imitated by the social sciences.[1] The anthropologist A. L. Kroeber (1952:124) asked "What is the cultural equivalent of the phoneme?" and Kenneth Pike had an answer: the "behavioreme." Pike (1954) was in the process of constructing a comprehensive theory in which "verbal and non-verbal activity is a unified whole, and theory and methodology should be organized to treat it as such" (p. 2). Not only was it a cliché "that what mathematics already is for the physical sciences, linguistics can be for the social sciences" (LaBarre 1958:74; Le Page 1964:1), but many even held the view that "as no science can go beyond mathematics, no criticism can go beyond its linguistics" (Whitehall 1951:v). In this period the "linguistic method" was being applied to the study of kinesics, folkloric texts, the analysis of the political content of agitational leaflets, and much more.

1.2. STRUCTURAL LINGUISTICS

1.2.1. The Saussurean Heritage

The linguistics practiced in the United States in the 1950s, along with that in much of Europe, owed an intellectual debt to the great Swiss linguist Ferdinand de Saussure (1857–1913). Saussure's lecture notes, published posthumously as the *Cours de Linguistique Générale,* represent a turning point in the history of linguistics. The central principle of the *Cours* is that a well-defined subpart of language can be abstracted from the totality of speech. This subpart Saussure called "*langue,*" which he contrasted with "*parole,*" or "speech." *Langue* represents the abstract system of structural relationships inherent in language, relationships that are held in common by all members of a speech community. *Parole,* on the other hand, represents the individual act of speaking, which is never performed exactly the same way twice. Saussure compared language to a symphony. *Langue* represents the unvarying score, *parole* the actual performance, no two of which are alike.

Since in the Saussurean view, *langue* forms a coherent structural system, any approach to language devoted to explicating the internal workings of this system has come to be known as "structural linguistics." A structuralist description of a language has typically taken the form of an inventory of the linguistic elements of the language under analysis along with a statement of the positions in which these elements occur. The point of such a rather taxonomic approach to *langue* was made explicit in the *Cours:*

> It would be interesting from a practical viewpoint to begin with units, to determine what they are and to account for their diversity by classifying them. . . . Next we would have to classify the subunits,

[1]This point is argued at length in Greenberg (1973).

> then the larger units, etc. By determining in this way the elements that it manipulates, synchronic linguistics would completely fulfill its task, for it would relate all synchronic phenomena to their fundamental principle. (Saussure 1959:111)

Structural linguistics grew slowly in the first two decades after Saussure's death. But by the late 1930s, it was flourishing in a variety of Western academic centers, in particular, in Prague,[2] Copenhagen, Paris, Geneva, London, Chicago, and New Haven. While it had never been a unified movement, the fragmentation of structural linguistics accelerated in the war-torn 1940s, with the consequence that the particular form it took largely became a function of the country in which it was practiced. It was in the 1940s that American structuralism took on its distinctive cast and entered the period of its great success.

1.2.2. American Structural Linguistics[3]

The two pioneers of structural linguistics in America were Edward Sapir and Leonard Bloomfield. Sapir, in fact, had worked out the basic principles of structuralism even before Saussure's *Cours* had been published, as is evidenced by his Takelma grammar of 1911 (published as Sapir 1922). Sapir's interests were far-ranging; in addition to grammatical analysis, he concerned himself with the humanistic and cultural aspects of language and published papers on the functioning of language in creative literature, mythology, and religion. Indeed, the scope of his interests extended far beyond language, and he is still regarded as one of the greatest American cultural anthropologists. Sapir could be classified as a "mentalist," that is, he conceived of linguistic structure as having an underlying mental reality that could be probed by studying native speakers' overt judgments about their language as well as their unguarded use of it. He also believed that linguistic structure plays a role in shaping our perception of reality, an idea that was developed further by his student Benjamin Whorf (hence the "Sapir-Whorf Hypothesis.") Finally, Sapir was perhaps the greatest fieldworker in the history of linguistics: he not only published analyses of a number of American Indian languages that are still valuable today, but trained more than a dozen students who themselves would achieve prominence for their work on the indigenous languages of North America.

Leonard Bloomfield also published analyses of particular languages based on his fieldwork, but his greatest impact on the course of American linguistics resulted from those sections of his book *Language* (1933) that outlined his theoretical perspective on language. While he had earlier been a mentalist too, by 1933 Bloomfield had become convinced that it was unscientific to posit mental constructs within linguistic theory. That is, he had become an empiricist—he had adopted a view of

[2]The work done by the Prague School was of such great importance to the development of transformational generative grammar that it merits a section of its own (see Section 2.4.1.).

[3]For a comprehensive overview of the development of structural linguistics in the United States, see Hymes and Fought (1981). Anderson (1985) presents a thorough discussion of its approach to phonology, which he contrasts to approaches prevalent in Europe.

linguistic science that allowed only statements based on direct observation of the phenomena under investigation or generalizations that could be drawn from observables by a set of mechanical procedures. As he put it, "The only useful generalizations about language are inductive generalizations. Features which we think ought to be universal may be absent from the very next language that becomes accessible" (1933:20). Bloomfield's empiricist orientation also affected his approach to the study of meaning. While he recognized that a central function of language was to convey meaning, he nevertheless was skeptical that meaning could be studied scientifically. This is because "[t]he situations which prompt people to utter speech include every object and happening in their universe. In order to give a scientifically accurate definition of meaning for every form of language, we should have to have a scientifically accurate knowledge of everything in the speaker's world" (p. 139). Since this goal was, of course, unattainable, recourse to meaning was to be avoided wherever possible.

The essence of the intellectual differences between Sapir and Bloomfield can be captured nicely by Bloomfield's sobriquet for Sapir, "medicine man," and by Sapir's references to "Bloomfield's sophomoric psychology" (Jakobson, 1979:170).

By the early 1950s, both Sapir and Bloomfield had a considerable following among American structuralists (Sapir had died in 1939, Bloomfield in 1948). But it was Bloomfield's intellectual heirs, the "post-Bloomfieldians",[4] who predominated both in numbers and influence.[5] The leading post-Bloomfieldian linguists, Bernard Bloch, Zellig Harris, Charles Hockett, George Trager, Henry Lee Smith, Archibald Hill, and Robert Hall, were committed to reconstituting linguistic theory along strict empiricist lines. Sapir's students, whose ranks included Morris Swadesh, Stanley Newman, Mary Haas, and C. F. Voegelin, retained the mentalistic outlook of their teacher, though it must be said that they had no comprehensive alternative theory to offer to the post-Bloomfieldians' empiricist one. The Sapirean group is noteworthy for the many careful descriptive studies of native American languages that they published in that period (and continue to publish).

The predominance of the Bloomfieldian wing of American structural linguistics was a function of the wide appeal of empiricist philosophy in the American intellectual community in the 1930s and 1940s. And this appeal in turn was undoubtedly related to the fact that there was no period in American history in which there was greater respect for the methods and results of science. Social scientists and philosophers, envious of the dramatic achievements in nineteenth- and early twentieth-century natural science and (temporarily) innocent of the ethical issues that would be raised by the atomic bomb, counterinsurgency technology, and genetic experi-

[4]Confusingly, some commentators have applied the term "post-Bloomfieldian" to all American structuralists of the 1940s and 1950s. However, in this work its use will be confined to those committed to Bloomfield's empiricist program for linguistics.

[5]There were other structuralists in America at that time who were independent sociologically and intellectually of both the Sapireans and post-Bloomfieldians. Foremost among them were the Christian missionary linguists, the two most prominent of whom were (and are) Kenneth Pike and Eugene Nida.

mentation, asked: "How can we be scientific too? How can we rid ourselves of the fuzzy speculation that has often characterized theorizing about language?" Empiricism appeared to provide an answer. By hitching his fortunes to the then-popular conception of "science," Bloomfield assured that he and his followers would come to dominate structural linguistics in America. Those who resisted the empiricist tide, like Sapir and his students, found themselves increasingly peripheral in the field.

The post-Bloomfieldians' insistence that linguistics could be assimilated to the natural sciences is partly responsible for the fact that American structural linguists prospered in the 1940s and 1950s, while their European counterparts declined in relative importance.[6] Since their empiricist outlook dissuaded them from raising the broad philosophical questions about the underlying nature of language, the Americans kept to a single-minded focus on developing procedures for phonemic and morphemic analysis. Hence a group of specialists arose in this country, whose only professional loyalty was to the field of linguistics and its particular techniques. European structuralists, by contrast, all had interdisciplinary interests, which paradoxically diminished their commitment to building linguistics as an independent discipline.

Nowhere were the practical consequences of the Bloomfieldian empiricist stance more evident than in the treatment of meaning. To the Europeans, understanding the role of language in conveying meaning was paramount; consequently they devoted considerable attention to the semantic function of the units they arrived at in their structural analysis. Their preoccupation with meaning meant that they were constantly abutting on fields like philosophy, psychology, and criticism, which also studied meaning. Some followers of Bloomfield, on the other hand, attempted to expunge the study of meaning altogether from the field of linguistics! They felt uncomfortable even addressing a concept so notoriously difficult to quantify and operationalize. Yet the very limitation of their vision helped them to create a distinct field with clearly defined boundaries.

To those American structuralist theoreticians most committed to an empiricist approach to language, European linguistic scholarship seemed more akin to mysticism than to science. Robert Hall expressed such an idea with a particular intensity of feeling:

> The present-day intellectual atmosphere of Europe is influenced by an essentially reactionary hostility to objective science, and by a return to doctrines of "spiritual activity," "creativity of the human soul," and socially biased value-judgments which European scholarship has inherited from the aristocratic, theological background of mediaeval and Renaissance intellectualism. This reactionary attitude is present in the theorizing of many modern European students of language, who sacrifice positive analysis of concrete data to discussion of purely imaginary, non-demonstrable fictions like "thought" and "spirit" as supposedly reflected in language. In American work on language, the burning question at present is whether this same anti-scientific attitude is to be allowed to block the

[6]Other factors boosted the fortunes of the Americans with respect to the Europeans. In particular, the former benefited from governmental support during the Second World War while the latter did not, and the commitment of the former to the principle of "egalitarianism" gave them an issue around which they could rally to crystalize their professional identity. For more discussion, see Newmeyer (in press).

> further development of linguistics and its contribution to our understanding of human affairs, especially in our teaching. (Hall 1946:33–34)

Leo Spitzer, a European exile then teaching in the United States, in turn accused Hall of desiring to set up an "academic F.B.I." to stifle views departing from those then current in America (1946:499).

By the end of the 1940s, however, as a result of increased cross-Atlantic scholarly contact after World War II, signs of rapprochement appeared on both sides. In 1951, the American Charles Hockett wrote an extremely favorable review of a book by the leading French structuralist André Martinet, and Martinet in turn wrote that terminological differences alone were the major impediments to Americans and Europeans understanding each other's work (see Hockett 1951; Martinet 1953).

1.2.3. Post-Bloomfieldian Methodology

In this section, I attempt to synthesize the approach to linguistic methodology of the post-Bloomfieldian school. While, as one might expect, its members had differences with each other over certain theoretical issues (some of importance), basic agreements, which far outweighed their disagreements, make such a synthesis possible.

The goal of post-Bloomfieldian linguistics was to "discover" a grammar by performing a set of operations on a corpus of data. Each successive operation was one step farther removed from the corpus. Since the physical record of the flow of speech itself was the only data considered objective enough to serve as a starting point, it followed that the levels of a grammatical description had to be arrived at in the following order:

I. Phonemics
II. Morphemics
III. Syntax
IV. Discourse

Since morphemes could be discovered only after the phonemes of which they were composed were extracted from the flow of speech, it followed that morphemic (or syntactic) information could not enter a phonemic description: "There must be no circularity; phonological analysis is assumed for grammatical analysis and so must not assume any part of the latter. The line of demarcation between the two must be sharp" (Hockett 1942:19). This procedural constraint became known as the prohibition against "mixing levels" in a grammatical description.

These conditions were intended to guarantee that a linguistic description be a catalog of observables and statements in principle extractable directly from observables by a set of mechanical procedures: "The over-all purpose of work in descriptive linguistics is to obtain a compact one–one representation of the stock of utterances in the corpus" (Harris 1951:366). The subjectivity of language consultants' judgments necessarily ruled them inadmissible as data, and any description em-

ploying unobservable "process" notions such as deletion, metathesis, and insertion were found to be incompatible with empiricist strictures. As a matter of fact, the descriptive work of Bloomfield himself often had been stated in a process framework, complete with rule ordering statements (see Bloomfield 1939b). But after the discrepancies between Bloomfield's theory and his practice were pointed out, process statements disappeared from the literature. Hockett (1954) wrote that he could not even conceive of any meaning to "ordering" but an historical one.

The first step in the process of grammar construction was to divide the speech flow itself into a series of phones, the basic units of sound. Phones were eligible to be members of the same phoneme if they did not contrast, that is, if they were in complementary distribution or free variation in an environment. In the earliest structuralist work, meaning contrast was used as a criterion for establishing the separate phonemic status of two phones. (For example, [ɪ] and [æ] result in meaning differences in the context /b . . . d/. Hence they were to be assigned to two separate phonemes.) However, many post-Bloomfieldians explored the use of operational tests that did not rely on judgments of meaning differences in order to put the notion of contrast on a more objective footing (see, for example, Harris 1951.)

After the phonemes were discovered, the next step was to group them into "morphs," the minimal recurrent sequences of phonemes. Since the cues for the existence of morph boundaries had to exist in an already-discovered level, this was no easy task. One method was proposed by Harris (1955), who suggested that morph boundaries might be arrived at by a procedure whose first step was the calculation of the number of phonemes that could conceivably follow a sequence of phonemes in a string. Harris used by way of illustration the English sentence *he's clever*, phonemicized as /hiyzklevər/. He estimated that 9 phonemes can follow utterance-initial /h/, 14 utterance-initial /hi/, 29 /hiy/, 29 /hiyz/, 11 /hiyzk/, 7 /hiyzkl/, 8 /hiyzkle/, 1 /hiyzklev/, 1 /hiyzklevə/, and 28 /hiyzklevər/. Harris theorized that morph boundaries followed peaks, that is, that they were to be posited after /y/, /z/, and /r/.

The procedure for classifying morphs into morphemes was similar to that for classifying phones into phonemes. Two morphs were eligible to be members of the same morpheme if (in the simplest case) they were in complementary distribution and were phonemically similar. Hence, a morphemic description consisted of statements like "The morpheme {ed} has members /ed/ (in a particular environment) and /t/ (in a particular environment)." Irregularity, particularly if it was reflected by vowel alternations, was a troublesome point in post-Bloomfieldian morphemics. There were several different morphemic analyses of /tuk/, for example, all of which attempted to be consistent with the classificatory principle of linguistic analysis and to avoid a process description like "/ey/ goes to /u/ in the past tense of /teyk/."

The principle of complementary distribution led to a different set of procedures in syntax, since one wanted to say that two morphemes were of the same syntactic type if they were NOT in complementary distribution. One set of procedures for assigning elements to syntactic categories was Harris's (1946) "bottom-up" morpheme-to-utterance approach; another was Wells's (1947) "top-down" immediate-constitu-

ent analysis. Harris classified individual morphemes into syntactic categories on the basis of their distributions. For example, any morpheme that occurred before the plural {-s} morpheme (itself arrived at via bottom-up distributional analysis) would be classified as a noun. Lower-level syntactic categories were grouped into higher ones by analogous procedures. Wells, on the other hand, started from the sentence as a whole, and by substitution procedures divided it into smaller and smaller constituents. Since in the sentence *the King of England opened Parliament*, the sequences of morphemes *the King of England* and *opened Parliament* had greater substitutive possibilities than any other sequences in the sentence, the major immediate constituent break was drawn between *England* and *opened*. This procedure was followed down to the level of the individual morphemes.

In the 1950s, Harris began to work out procedures for stating syntactic relations between sentences. Harris's work developed out of his attempt to analyze the structure of extended discourse. The problem was that sentences could exist in many different surface forms, and the usual substitution procedures did not seem to be of much help in stating the obvious systematic relatedness that existed between their various types. Hence he worked out procedures (1952a, 1952b; 1957; 1965) for "normalizing" complex sentence types to simpler "kernel" ones. He noted, for example, that corresponding to sentences of the form

N_1VN_2

there often existed sentences of the form

N_2 is V-ed by N_1

or

it is N_2 that N_1V.

Provided that the cooccurrence relations between N_1, V, and N_2 in the three sentence types were the same, Harris set up TRANSFORMATIONS relating them. They might be stated as follows:

$N_1VN_2 \leftrightarrow$ N_2 is V-ed by N_1 (Passive Transformation)
$N_1VN_2 \leftrightarrow$ it is N_2 that N_1V (Cleft Transformation)

Despite the work of Harris and a few others, the post-Bloomfieldians published relatively few syntactic analyses. Robert Hall (1951) explained why: "Descriptive syntactic studies have also been rather rare; but, since they normally come at the end of one's analysis, the tendency is perhaps to hold them for incorporation into a more complete description" (p. 120). In fact, the little syntactic work that was done necessarily bypassed the procedures to a greater or lesser extent, since a complete morphemic analysis had never been worked out even for English (for the most ambitious attempts to present one, see Trager and Smith, 1951, Francis 1958).

Actually, the structuralist procedures were bypassed in many ways. It was therefore necessary to have some checks that could be applied to any description to ensure that it could have been arrived at operationally. The most comprehensive

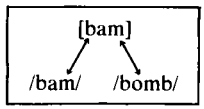

Figure 1.1

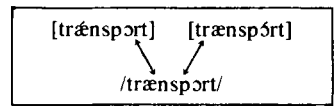

Figure 1.2

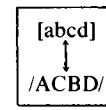

Figure 1.3

was that there be a BIUNIQUE relationship between elements of one level and those of the next higher level (Harris 1951; Hockett 1951). Let us take the relationship between phones and phonemes as an illustration. Biuniqueness entails that any sequence of phones in a description has to be associated with a unique sequence of phonemes and that any sequence of phonemes has to be associated with a unique sequence of phones, ignoring free variation. Hence, descriptions involving neutralization at either the phonetic level (Figure 1.1) or the phonemic level (Figure 1.2) were automatically ruled out.

It is easy to see how both situations are incompatible with an empiricist-based requirement that phonemes be literally extractable from the sequence of phones by mechanical procedures.

Operational considerations also demanded that a condition later termed "linearity" be placed on adjacent levels in a linguistic description. Linearity requires that the linear order of elements at one level match the linear order of the corresponding elements at the next level. Analyses representable by Figure 1.3 were therefore ruled out, assuming that phone [b] belongs to the phoneme /B/ and phone [c] belongs to the phoneme /C/.

Another necessary check was that an invariant relation hold between phones and phonemes: that a phonetic segment in a given environment be invariantly assignable to a given phoneme. The weak form of invariance allowed partial intersection (overlapping) of two phonemes. Biuniqueness was not violated if, say, the [D] of [θDow] *throw* (in the dialect described in Bloch 1941) was assigned to the phoneme /r/ and the [D] of [bɛDiy] *Betty* assigned to /t/, since the environment for the identical phonetic manifestation of the two phonemes could (in principle) be recoverable from the acoustic signal. However, some post-Bloomfieldians (Hockett 1942; Wells 1945) disallowed even partial overlapping, which resulted in the two occurrences of [D] being assigned to the same phoneme. This requirement of "once a phoneme, always a phoneme" seems to have been a consequence of the desire to raise the principle of complementary distribution to an inviolable strategy.

While empiricist assumptions necessitate a biunique relationship between levels within one description, it does not follow that there is necessarily a biunique relationship between the corpus of data and a description of it. The reason is that there were an indefinite number of procedures of segmentation and classification applicable to a given body of data, each consistent with empiricist methodology. This nonuniqueness of phonemic analyses (see Chao 1934) was a constant source of dismay to the post-Bloomfieldians. For example, at least three different phonemicizations of the vowel nucleus in the word *bay* were proposed: /bej/ (Trager and Bloch 1941), /bee/ (Swadesh 1947), and /bei/ (Pike 1947b). The criterion

of "pattern congruity" was often applied when a choice was made between alternatives. That is, the phonemicization was chosen which led to the greatest overall symmetry in the language's phonemic system. The Trager-Bloch analysis was selected as preferable by means of this criterion.

1.3. CLOUDS ON THE HORIZON

So far, the picture painted of the self-perceived state of American linguistics in the mid 1950s has been a uniformly rosy one. Yet, hardly 10 years later, the advocates of post-Bloomfieldian structuralism were hard pressed to find an audience. And 20 years later, that approach was typically encountered by the student in a history of linguistics class, not in a survey of contemporary theory. How did it fall so far so fast? In this section, I begin to answer this question by pointing both to the changing intellectual climate in America and to some of the weaknesses of post-Bloomfieldian theorizing that had already become evident.

1.3.1. Crisis in Empiricist Philosophy

The downfall of post-Bloomfieldian linguistics was hastened by dramatic changes of outlook in the philosophy of science. Little by little, the post-Bloomfieldians found the philosophical props knocked out from their conception of scientific methodology. And since more than anything else they felt they had scientific justification for their theory, when they lost that justification they had very little to appeal to.

For years, philosophers had been grappling with the criteria for determining whether a statement is meaningful, and hence proper in scientific discourse. An extremely strong empiricist position known as "the principle of complete verification" had considerable support in the 1930s. According to this principle, the meaning of a statement is simply a description of the ways in which it might be verified: "Whenever we ask about a sentence 'what does it mean?' . . . we want a description of the conditions under which the sentence will form a true proposition, and of those which will make it false. . . . The meaning of a proposition is the method of its verification" (Schlick 1936:341). Strict verificationism was abandoned very early as untenable by philosophers (though not by behavioral psychologists). Various weaker forms of verificationism were put forth in the late 1930s and in the 1940s. The criteria for significance were weakened still further to the "principle of falsifiability," by which a statement was to be considered meaningful if it was falsifiable in principle. But still there were problems—many meaningful statements (such as any containing an existential quantifier) simply did not meet this condition. Yet it seemed counterintuitive to call them "unscientific."

By the late 1940s it was widely believed in the United States that a sentence could be considered meaningful if its constituent terms could be given what is often

known as an "operational definition." That is, all terms in a scientific statement would have to be linkable directly to observables by an operation (or series of operations) performable by the investigator. Notice that the theoretical terms in structural linguistics (such as "phoneme" and "noun") had just this quality. If anybody wanted to know why the claim had been made that /k/, for example, was a phoneme of English, that person could (in principle) be provided with a list of the operations performed on the raw data to arrive at that conclusion.

In two important papers (partly surveying earlier work, partly original in nature), the philosopher Carl Hempel (1950, 1951) laid to rest any hope for an empiricist criterion for cognitive significance.[7] After reviewing the earlier, more inadequate theories of meaninglessness, he pointed out that even the more permissive empiricist approaches to this question failed to capture the essence of what it takes for a statement to be considered scientific. There is simply no direct connection between a scientific term or statement and the empirical confirmation of a theory containing that term or statement. For example,

> the hypothesis that the agent of tuberculosis is rod-shaped does not by itself entail the consequence that upon looking at a tubercular sputum specimen through a microscope, rod-like shapes will be observed: a large number of subsidiary hypotheses, including the theory of the microscope, have to be used as additional premises in deducing that prediction. (Hempel 1950:58–59)

Moreover, many fundamental scientific notions, such as "gravitational potential," "absolute temperature," and "electric field," have no operational definitions at all.

How then might a statement be judged as meaningful or not? The problem, according to Hempel, lies in attempting to ascribe meaningfulness to statements themselves in isolation. Science is more in the business of comparing theories than in evaluating statements. A theory is simply an axiomatized system which as a whole has an empirical interpretation. We can compare competing theoretical systems in regard to such characteristics as these:

a. the clarity and precision with which the theories are formulated, and with which the logical relationships of their elements to each other and to expressions couched in observational terms have been made explicit;
b. the systematic, i.e., explanatory and predictive, power of the systems in regard to observable phenomena;
c. the formal simplicity of the theoretical system with which a certain systematic power is attained;
d. the extent to which the theories have been confirmed by experimental evidence. (Hempel 1951:74)

Hempel went on to write:

> Many of the speculative philosophical approaches to cosmology, biology, or history, for example, would make a poor showing on practically all of these counts and would thus prove no matches to available rival theories, or would be recognized as so unpromising as not to warrant further study or development. (p. 74)

Statements like these signaled the demise of empiricism as a significant force in the philosophy of science. As its philosophical props gave way, post-Bloomfieldian

[7]These two papers were later merged in Hempel (1965).

structuralism found itself in a distinctly unstable posture. Not surprisingly, it was relatively simple for a new theory, defective by any empiricist standards, yet nevertheless highly valued according to Hempel's four criteria, to topple it completely.

1.3.2. Unresolved Problems in Structural Linguistics

The most penetrating challenges to the post-Bloomfieldians in the early 1950s came from those who accepted the basic principles of structuralism yet had no commitment to an empiricist program. Since these latter linguists had no well-worked-out alternative, the most they could do was point out the failings of the dominant procedural approach. Nevertheless, their doing so helped to create an accelerating atmosphere of uncertainty around the entire post-Bloomfieldian enterprise.

The contemporary critics of the post-Bloomfieldians had no trouble in pointing out the failure of this approach to present a half-convincing account of suprasegmental phenomena: stress, pitch, and juncture. Alternative analyses abounded, and no one was able to argue successfully that their phonemicization was latent in the acoustic signal, as empiricist constraints demanded that it be. James Sledd, in his 1955 review of Trager and Smith's influential *Outline of English Structure* (1951), told the truth more openly than it had ever been told before. Sledd, who had a reputation as an iconoclast, said that he simply did not hear the neat distribution of stress, pitch, and juncture phonemes of the Trager-Smith analysis, strongly implying that the authors had done nothing less than cheat.

Another persistent critic of the post-Bloomfieldians, Kenneth Pike, pointed out many of the same problems (see Pike 1947a, 1952). But Pike went farther than other critics; he said that in order to assign juncture correctly one had to mix levels—to do at least part of the grammatical analysis first. Pike's alternative did not really represent a break from operationalism since he outlined (very vaguely) some mechanical procedures for identifying morphemes before doing a phonemic analysis. But it is noteworthy for its clear statement of the dilemma facing competent linguistics of the period: they knew what to do to get the right grammatical analysis, but their theory would not let them do it. According to Pike (1947a:159): "There must be something wrong with present-day phonemic theory if workers agree on the practical value of a procedure (and of evidence) in the field which they then rule out in theoretical discussion and in presentation."

By 1958, the inability of the post-Bloomfieldians to handle suprasegmentals had become so obvious that even their own followers had to acknowledge it openly. Archibald Hill admitted that

> until a few years ago, it was an assumption almost universal among linguists that a speaker, even without special training, would infallibly and automatically hear the contrasts in his own speech and that the only things he would not hear would be sounds which are not contrastive. Consequently, it would at that time have been necessary to say that any speaker who had trouble in hearing four grades of stress would be one who had only three contrasts. We have taken the position that there are

> speakers who have four contrasts but who still have difficulty in hearing all the distinctions they make. Such difficulties occur not only in the system of stresses, but with other sounds as well. (Hill 1958:17–18)

There are a number of examples in the literature where their empiricist outlook forced the post-Bloomfieldians to adopt analyses that they themselves seemed to recognize were inelegant and uninsightful. Two examples of this sort involve Bernard Bloch. Bloch (1947) worked out detailed procedures for assigning morphs to morphemes. After disposing of the bulk of regular and irregular verbs in English, he noted the variant phonemic shapes of the word *have* in four pronunciations of the sentence *I have seen it:*

/ày hæv síyn it/
/ày v síyn it/
/ày həv síyn it/
/ày əv síyn it/

Since /hæv/, /v/, /həv/, and /əv/ are neither in complementary distribution nor in free variation (they differ in their "social flavor"), Bloch (1947) could not escape the conclusion that they belong to four separate morphemes. He wrote, "Reluctant as we may be to allow the multiplication of elements, we cannot escape the conclusion that the verb forms /həv/ and /əv/ belong to none of the morphemes mentioned so far" (p. 417).

An even better example can be found in Bloch's two phonological studies of Japanese (1946, 1950). The earlier paper gave an extremely elegant (though informal) description of the sound patterning of that language. But in 1950 he revised his analysis in the direction of complexity and lack of generalization. Why? Because earlier he had "confused phonemes with morphophonemes"; the earlier insightful analysis had violated biuniqueness. Hockett (1951) comments on this: "What is deceptively simple in [Bloch's] earlier treatment turns out to be quite complicated in the later—but the more complicated treatment is also obviously more accurate" (p. 341). Bloch and Hockett could not have been undisturbed by having to reject simple treatments for complicated but accurate ones.

By 1955, even Hockett had to agree reluctantly that one could not do an analysis of a language objectively—one had to empathize with the informant (Hockett 1955:147). But if that were the case, then what theoretical validity could operational procedures have which were designed to guarantee the correct description of a language?

Finally, in 1957, at least one observer noted a growing decline in dogmatism and confidence among practicing structural linguists:

> In the intervening years [since the early 1940s], however, it seems to me that the attitudes and behavior-patterns of linguists have changed. Naturally the fervor of that generation has waned, but even among the younger linguists there seem to be a few [*sic*] who are either as chauvinistic, as

> passionate, or as confident that they have discovered the whole truth. . . . Dogmatism also appears to have declined, though, to be sure, it has not vanished (and presumably never will). (Householder 1957:156)

As events in that year were to prove, they had very little to be dogmatic or confident about.