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Source: *Language in Society*, Vol. 1, No. 1 (Apr., 1972), pp. 97-120
Published by: Cambridge University Press
Stable URL: http://www.jstor.org/stable/4166672

Lang. Soc. **1**, 97–120. Printed in Great Britain

Some principles of linguistic methodology

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ABSTRACT

Current difficulties in achieving intersubjective agreement in linguistics require attention to principles of methodology which consider sources of error and ways to eliminate them. The methodological assumptions and practices of various branches of linguistics are considered from the standpoint of the types of data gathered: texts, elicitations, intuitions and observations. Observations of the vernacular provide the most systematic basis for linguistic theory, but have been the most difficult kinds of data for linguists to obtain; techniques for solving the problems encountered are outlined. Intersubjective agreement is best reached by convergence of several kinds of data with complementary sources of error.

Less than twenty years divide us from the time when the study of methods was the reigning passion of American linguistics; yet the status of *methodology* has fallen so fast and so far that it now lies in that outer, extra-linguistic darkness where we have cast speculation on the origin of language and articles about slang. It cannot be denied that the pursuit of methods was carried on with a certain willful blindness to both theory and practice; there was a time when methods cast in the canonical form of tough-mindedness and symmetry were elevated to the status of ritual texts, though they were seen even then to be hopelessly impractical. But even if methodology is no longer an O.K. word in linguistics, we have no choice but to use it again. It is an open secret that the rough and ready exploitation of grammatical intuitions has run its course from exhilaration to despair. A more reflective approach to the problem of intersubjective knowledge is needed if our arguments are to mirror anything but our own polemical intent.

In our exploration of the use of language in the secular world, we find that methods have steadily grown in theoretical importance: first as a necessity, then as an art, and now as a route towards the development of a theory of speaking. A course in secular linguistics must focus first on the act of speaking and methods for observing it. But it should also include training in the several methods of historical and synchronic linguistics, making use of the texts, elicitations, and intuitions which are the mainstay of the scholarly world. This paper will present some of the principles that govern the gathering of empirical data, in both the secular and scholastic places of linguistics. The principles will be stated with a minimum of supporting argument, referring the reader to the considerable body of evidence in the sociolinguistic investigations cited.

SOME GENERAL NOTIONS ABOUT METHODS

Methodology as conceived here is not a complete program for converting ignorance into knowledge, but rather a set of strategies for handling the rich data from well-known languages. Linguists who are approaching a language for the first time will have to make their way as best they can; in any case, their findings will most likely be rewritten many times by those who come after them. With the pleasure of being the first goes the certainty of being wrong, which is the converse of the CUMULATIVE PRINCIPLE: *the more that is known about a language, the more we can find out about it.*

The luxury of methodology first becomes a necessity when continued investigations produce several competing theories, and we have to find out which one is right. This paper is in fact designed to hold up as the highest goal of linguistics *the possibility of being right.* To be right on a matter of general principle is certainly difficult in the extreme, a slim chance at best, but the reward is that the work may form part of the continued construction of linguistic theory for a long time to come. It is certainly worth trying, perhaps the only thing worth trying for; but it is unfortunately true enough that this goal has been abandoned by at least one major tradition in linguistics. Beginning with the elegant paper of Chao on 'The non-uniqueness of phonemic solutions ...' (1934) and culminating in Harris's argument for the complementarity of string analysis, transformational analysis, and constituent analysis grammars (1965: 365) we observe a quietistic tendency to claim that almost all our theories are notational variants of one another, that each is true in its own way and has its own insight to contribute.[1] But what is commendable in religion is self-defeating in science. My own view is that such equivalent theories are trivial variants, and to confine ourselves to arguing their merits is to engage in an aesthetic pursuit rather than a scientific one.

Among the other innovations which Chomsky brought with him was a note of high seriousness in this respect. He is clearly interested in the structure of human language and the capacities of the mind which learns it, not in different ways of looking at the matter. Since Chomsky believes that linguistic theory is underdetermined by the data (1966), he proposes an internal evaluation measure, hopefully isomorphic with the one that the language learner actually uses. But the simplicity metric has had hard going; it is frequently misused by those trying to prove that they are right and that someone else is wrong, and there is

[1] 'To interrelate these analyses, it is necessary to understand that these are not competing theories, but rather complement each other in the description of sentences. It is not that grammar is one or another of these analyses, but that sentences exhibit simultaneously all of these properties' (Harris 1965: 365).

some question as to whether it has actually resolved any important issues (Lakoff 1970).

Here linguistics is in a position to benefit from the example of the developed sciences. Scientific methodology can be thought of as the reverse procedure: trying to prove to yourself that you are wrong. That is, methodology is careful and conscientious search for error in one's own work, following Karl Popper's principle that the best theories are the easiest to disconfirm (1959). To be right means that you have finally abjectly, hopelessly failed to prove yourself wrong. It is dangerous to assign this responsibility to anyone else, for no one will have the same vested interest in this pursuit as you do.

This kind of methodological self-criticism leads to a continual refinement of our methods, introducing safeguards, reliability tests, cross-checks, typical of the scientific attitude which can profitably be practised even at the pre-scientific stage where we now find ourselves. But such methodological rigor is often justifiably identified with the dead end of a worn-out approach. By the time the methods are perfected, the most important work has often been done, and the methodologists continue oblivious to this fact, proceeding on the hopelessly unrealistic program that everything which can be described should be. At this point, we find cropping up a second kind of methodology, a revolutionary criticism which identifies new problems and fundamental faults in the older methods which cannot be repaired.

A new methodology with new kinds of data is then called for. But if new data has to be introduced, we usually find that it has been barred for ideological reasons, or not even been recognized as data at all, and the new methodology must do more than develop techniques. It must demolish the beliefs and assumptions which ruled its data out of the picture. Since many of these beliefs are held as a matter of deep personal conviction, and spring from the well-established habits of a lifetime, this kind of criticism is seldom accomplished without hard feelings and polemics, until the old guard gradually dissolves into academic security and scientific limbo.

We might approach the various methods available to linguistics by looking at the activity of the linguists themselves, according to where they can be found. In this search, we would find linguists working in the *library*, the *bush*, the *closet*, the *laboratory*, and the *street*, and might so name each sub-division of the discipline. But in this analysis we will take a different approach and examine the raw materials gathered by each variety of linguistics, distinguishing each linguist by his product: *texts*, *elicitations*, *intuitions*, *experiments*, and *observations*. A full discussion of the experimental method would carry us beyond our present scope; some methodological problems in the application of controlled experiments to verbal behavior are given in Labov 1969. This paper will consider principles of methodology centering about the use of tests, elicitations, intuitions, and observations of the vernacular.

TEXTS

The fundamental methodological fact that historical linguists have to face is that they have no control over their data. Texts are produced by a series of historical accidents; amateurs may complain about this predicament, but the sophisticated historian is grateful that anything has survived at all. The great art of the historical linguist is to make the best of this bad data – 'bad'' in the sense that it may be fragmentary, corrupted, or many times removed from the actual productions of native speakers. He relies first of all on the canons of critical scholarship – by-laws and safeguards against human fallibility and corruptibility. The most important of these is *reference* – the act of making the original texts available for the inspection of others who may have other biases and prejudices. In insisting on the checkability of data, historical linguists are considerably ahead of the average descriptive linguist. The historian tries to bring us as close to his data as he can, while the descriptive linguist keeps us many times removed. Between the reader and the native speaker there are interposed the training, skills and theoretical orientation of the linguist; rarely is an effort made to bridge this gap by publishing tapes or protocols.

The chief methodological principle of historical linguistics remains the NEOGRAMMARIAN HYPOTHESIS:

> every sound change, inasmuch as it occurs mechanically, takes place according to laws that admit no exception. That is, the direction of the sound shift is always the same for all the members of a linguistic community except where a split into dialects occurs; and all words in which the sound subjected to the change appears in the same relationship are affected by the change without exception.
>
> (Osthoff & Brugmann 1878)

It is no longer possible to defend this hypothesis as a substantive claim that word classes actually do move intact and as a whole. Although the objections of nineteenth-century realists seemed to have been overwhelmed, decisive disproof has now been provided by Wang and his associates who have demonstrated the existence of lexical diffusion on a massive scale in the history of Chinese dialects (Chen & Hsieh 1971; Cheng & Wang 1970). But as a methodological principle the neogrammarian hypothesis has been more successful: it has provided the basic incentive to search for regularity and underlying conditioning factors in sound change, rather than accept surface variation on face value.

Unfortunately, most historical linguists felt it necessary to defend the neogrammarian hypothesis as a substantive description of the process of sound change, and this preoccupation brought them into conflict with the solid data of dialectologists. When Gauchat (1905) demonstrated that sound changes in Charmey proceeded across three generations by fluctuations and lexical oscillations, the neogrammarians rejected this and other descriptions of sound change in progress as mere 'dialect borrowing' (Goidanich 1926; Bloomfield 1933: 361). Yet given the imperfect character of historical records, it seems inevitable that we must rely on present data to interpret them. In fact, our current research on sound change in progress (Labov 1970*a*) is based upon the UNIFORMITARIAN PRINCIPLE: *the linguistic processes taking place around us are the same as those that have operated to produce the historical record.*[2]

In weighing the limitations of the Uniformitarian Principle, we are forced to ask whether the growth of literacy and mass media are new factors affecting the course of linguistic change that did not operate in the past. But even if this should be so, we can still isolate in the patterns of everyday speech the kind of factors which have always operated on the spoken language and which determine the main stream of linguistic evolution in the present (Labov 1966*a*).

The mutual interpretation of past and present can be seen most clearly in the classical problem of the Great Vowel Shift. Current controversy and the historical evidence is summed up by Wolfe (1969). The traditional view of Jespersen and Wyld accepted the evidence of Hart and other sixteenth-century orthoepists on the route followed by the long vowels of English: in particular, that the diphthongized high vowel of *die* descended from [dɪi] to [dɛi]. But Kökeritz, Dobson, Stockwell and others found it difficult to accept this view: for at the same time, the vowel of *day* rose from [dæi] to [dɛi]. Presumably the two would then have merged, but in fact they did not. The counterclaim was put forward (Stockwell 1966) that *die* was first centralized and then fell from [dɨ] to [dəi], but there is little hard evidence to support this view. Our current instrumental studies of similar changes in progress show that in a wide range of dialects, the new high diphthong /iy/ in *see* falls to /ey/ as a front vowel – but not in the extreme front position typical of tense vowels. The nuclei of these falling diphthongs follow a centralized track which is clearly in the region of front vowels but with more moderate second formant positions. This view of current sound shifts cannot give us certain knowledge of what happened in the sixteenth century; but it can resolve the contradiction between theory and evidence summarized above. We now know that there is no reason to expect a merger of *die* and *day*, even if Middle English *die* followed the pattern of some current /iy/ vowels and fell to [dɛi] and then to [dai].[3] The two vowels can pass each other on the routes

[2] The term '*uniformitarian*' is borrowed from geology, where it signifies the now generally accepted principle of Hutton that processes now taking place around us – weathering, sedimentation, volcanism, etc. – are the same as those that have operated in the past to produce the geological record.

[3] Even if local observers had reported that *die* and *day* were 'the same' at that time, it would not follow that they were in fact the same and would not prevent these word classes from following their opposing paths without disruption. Recent research on sound change in progress shows that native speakers do not perceive consistent second-formant differences which effectively separate word classes in natural speech. See Labov (1970*a*) and below.

indicated in Figure 1. In approaching this historical problem with very different kinds of data than that originally used, we converge upon the problem with different sources of error. Observations of current changes thus have a heightened value for the resolution of older problems, as indicated by the PRINCIPLE OF CONVERGENCE: *the value of new data for confirming and interpreting old data is directly proportional to the differences in the methods used to gather it.*

The problem of interpreting literary texts, letters, puns and rhymes has two aspects: (1) determining the relation between the writing system and the spoken language, and (2) determining the relation between normative responses and the vernacular. The first problem has been explored and argued (Stockwell & Barritt 1961; Kuhn & Quirk 1953) but the second is still largely neglected.

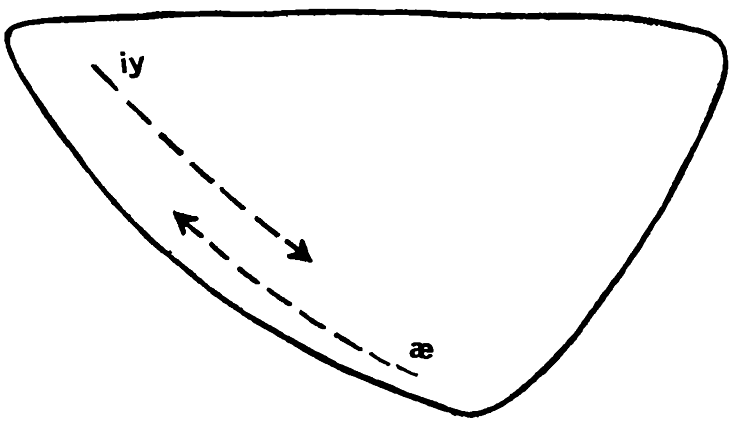

FIGURE 1. Routes followed by falling (lap) nuclei and rising (tense) nuclei in sound changes in progress.

Elegant investigations might be carried out by comparing current novels, letters, puns, poems and dictionaries with the actual state of the language today, and so reflect backwards on the interpretation of earlier documents. This would require a joint effort of historical scholars and linguists working with natural speech – a natural alliance, since these are the two branches of linguistics which are most concerned with controlling their data and searching out error.

ELICITATIONS

For many anthropologists, linguistics as a whole is essentially a methodological necessity. They learn language in order to enter the culture; if they report their knowledge in grammatical sketch, it is to preserve this aspect of the culture before it disappears, not for the sake of the general needs of linguistic theory. The anthropological *linguist* looks at the matter differently, more with an eye to the general problems of linguistics, but he also finds himself approaching the language from outside the grammar and the culture. He makes no claim on native intuition to defend his grammar, nor can he hope to seize the flow of natural conversation until long past the usual stages of language learning and reporting. Few anthropological linguists learn a language well enough to make much use of the conversational data of the street and the marketplace. The normal procedure is to transcribe texts – often traditional folk lore – to elicit translations of sentences from bilingual informants, present minimal pairs and ask for 'sames' and 'differents'. It would be idle to criticize these methods because they are limited; by such techniques, Boas and Sapir enriched tremendously the range of data available to a linguistic theory which had been largely based on texts and normative handbooks of a narrow group of European language families. Yet a calm view of our current field techniques should make it evident that this data is also normative, modified by conscious reflection and governed by cultural norms of right and wrong, good and bad language. Many American linguists assumed at first that such norms existed only in literary cultures, but in one of the most candid and penetrating examples of self-criticism on record, Bloomfield (1927) showed how mistaken he had been in this respect.

To the extent that there is a disjunction between norms and the pattern of everyday speech, the traditional field approach will yield a rough and necessarily imperfect first approach to the language. The minimal pair test or commutation test, long considered the soundest of all behavioral tests, is a dramatic example of this limitation. In our recent work on sound change in progress, we find that minimal pairs can be doubly defective. It has been observed before that native speakers can make distinctions in minimal pairs that they do not make in actual speech; normative responses often preserve fanciful, archaic or mythical distinctions. But it was not realized that native speakers can fail to recognize or register distinctions which they regularly make in natural speech (Labov 1970*a*): their self-reports often reflect the patterns of younger speakers rather than their own, or blur regular phonetic distinctions that are too subtle to withstand the glare of conscious examination.

The chief methodological principle here is that the linguist must be fully aware of the nature of his data. A realistic methodology would not insist that he abandon all description until he can trade insults with the man in the street or dispute local theology in the full flood of an enlightened scepticism. But if the linguist recognizes the existence of these higher levels of competence, he can use his developing grasp of the language on second or third trips to the field to locate the differences between norms and behavior; by doing so, he would deepen the value of his original observations for an increasingly dynamic and secular linguistic theory.

The courses in *field methods* taught in our linguistics departments are of course quite domesticated; they are more garden variety than field. But their well-informed informants provide students with their only serious practice in transcription and analysis. The exercises of our traditional texts are even more removed from the data of a secular linguistics, but the work they demand is honest work. To the everlasting credit of structural linguistics, it took the student seriously, and tried to give him all the help it could. Gleason (1961), Pike (1947) and Nida (1949) assumed that the student they were addressing was going to bring back some important data from the field, and they were anxious to teach him the essential skills of phonetic transcription, segmentation, allophonic grouping, recognition of conditioned variants, minimal pairs and commutation tests. The success of this approach is evident in the best of the California dissertations and SIL reports.

However, the formal methodology which grew out of the structuralist tradition was alarmingly unrealistic. It fully deserved the criticism which Chomsky turned on 'discovery procedures'. Although it is hard for us to believe it today, a number of students took seriously the contention of Bloch and Harris that one could analyze a language by beginning at the phonetic level without reference to the meanings of the words.[4] There was an unstated and informal methodological principle that gained currency and influenced practice which we can call the PRINCIPLE OF PREFERENTIAL IGNORANCE: *the less the linguist knows about a language, the more accurate (objective? scientific?) his description will be.* It is unlikely that the theory of ignorance will ever be fully developed, since the questions we ask today demand deep rather than shallow knowledge. But granted that the linguist could apply his technical skills of segmentation and classification in a reliable way, would the results be valid? What would a test of validity be in such a procedure?

The kind of methodology reflected in the discovery procedures of the 1940s and 50s has little relation to the principles to be presented here. We do not see reflected therein the careful concern with sources of error, the search for new kinds of data, for convergent and confirming perspectives which is our concept of methodology.

At a number of points throughout this discussion the phrase 'sources of error' has been used, and it will be helpful to specify the kinds of error we are talking about. There are of course errors of measurement, of memory, or of calculation, all of which can be avoided by careful attention to procedures. Tests of *reliability* help us to check such errors and eliminate them. But we are more concerned with a different type of error which stems from a misapprehension of the nature of the data. The data which may be cited as evidence of some underlying construct such as a linguistic rule, when it may in fact be largely the product of many factors and represent no single property at all. If the errors of misapprehension or the contextual factors neglected are local (such as interaction with a particular interviewer) we will obtain a loss of reliability when others repeat the work. But

[4] John Street informs me (personal communication) that under the influence of Bloch he once spent many months trying to do a phonetic transcription of Mongolian before he understood the meanings of any of the words. The attempt did not succeed.

if they are general (such as neglecting the tendency to report norms rather than behavior) we may obtain reproducible results which are still erroneous in their application to the theoretical problem. In order to detect and eliminate such results, we need tests of *validity*.

Unfortunately, we do not find any concept of validity in the methodology developed by descriptive linguistics to elicit linguistic data. The seriousness of descriptive intent was offset by a fashionable and aimless relativism in theory. A simple, commonsense adherence to the search for intersubjective agreement will reject such defensive manoeuvers. It seems reasonable for anthropological linguists to be guided by the growing concern within anthropology to reduce the role of the observer and let their methods be as transparent as possible; and so arrange matters that their own training, skills, and limitations be cancelled in the final analysis. This is of course only a goal to be aimed at, not a practice to be achieved, but it reflects a definition of validity which we will explore in a later section: that our descriptions should apply to the language which was spoken before we arrive and will still be spoken after we leave.

INTUITIONS

There is no doubt that Chomsky is responsible for the most important methodological revolution in our field. He directed a withering criticism at the extreme behavioral approach which denied the existence of mental constructs and rejected intuitive evidence out of hand. Chomsky and his students have demonstrated the existence of a vast, seemingly inexhaustible supply of data which the linguist can draw from his own knowledge of language. The richness of the results is beyond dispute. We know much more about English, and about language in general than we knew before and this achievement will stand whether or not the current form of generative theory survives in a recognizable form.

The question remains as to whether generative grammar has any methodology beyond the decision to exploit intuitions of grammaticality to the full. To begin with, we should note that this strategy depends upon the successful exploitation of the SAUSSURIAN PARADOX. Saussure argued (1962: 321) that the linguist must concentrate upon the social aspect of language, *langue*, which is conceived as so general that it is in the possession of every speaker. It follows that one can investigate *langue* by asking anyone about it, even oneself, which is what Chomsky proceeded to do. On the other hand, the individual details of *parole* can be ascertained only through a social survey in the midst of the population. The SAUSSURIAN PARADOX, then, is that *the social aspect of language can be studied through the intuitions of any one individual, while the individual aspect can be studied only by sampling the behavior of an entire population.*

The development of generative grammar has brought about a steady enrichment of this intuitive data. Chomsky's early response to criticisms of the grammatical–ungrammatical opposition was to suggest an ordered scale of grammaticality (1961), but in further developments each writer has followed his own bent. As judgments became more refined, several intermediate designations began to appear: in addition to ungrammatical '*', we observe questionable '?', questionably ungrammatical '?*', and outstandingly ungrammatical '**'. In addition, the kinds of intuitions to be cited as evidence were steadily enlarged. (1) The original judgments of grammaticality (well-formedness) naturally included, (2) judgments of ambiguity, and (3) judgments of correct paraphrase. But even from the outset we also note claims for (4), judgments of sameness or difference of sentence type, and (5) intuitions about immediate constituents (Chomsky 1961). A new emphasis on the theory of markedness has brought the citation of (6), native intuitions on marked and unmarked status. Finally, there are the most powerful of all kinds of intuition, (7) feelings that a given theory is the right one, or that another solution is 'counterintuitive'.

It is unfortunate that this proliferation of the intuitive data has not been accompanied by a methodological concern for the reduction of error, or a search for intersubjective agreement. The weaknesses of intuitive studies in this respect are known to all of us, but not everyone knows what to do about it without abandoning the advances we have made. Originally, Chomsky hoped that the area of agreement on judgments of grammaticality would be so large that the disputed areas could easily be resolved by following the general pattern. But this has not worked out in practice. The search for critical arguments has driven almost everyone to the use of examples which command no agreement at all. As one of countless examples, I cite Jackendoff's article on 'Quantifiers in English' (1968). Among the sentences given as grammatical without question we have *The three of the men that you met yesterday have not left yet* and *Of the men, the three you met yesterday have not left yet*. Chomsky himself has found it impossible to avoid arguments based on admittedly personal judgments. In his paper 'Remarks on nominalizations' (1970) he cites pairs such as *our election of John (to the presidency)* vs. **our election of John (to be) president* and notes: 'Reactions to these sentences vary slightly: [these] represent my judgments.' He then adds, 'Given such data, . . .' and proceeds with the argument. By 'data' he does not mean the disagreements, but rather the evidence of his own decisions. As valuable and insightful as such arguments may be, they cannot alone lead to the sure sense of right and wrong that we have raised as our ultimate goal. To achieve intersubjective knowledge, we will probably have to limit ourselves to intuitions of types (1)–(3) above, and refrain entirely from citing the intuitions of the theorist himself as *evidence*. Any serious consideration of sources of error must hold such data as the most suspect unless it coincides with other sources.

Nevertheless, linguists continue to use uncheckable examples and defend them by asserting that they are only discussing their own dialect. If 'my dialect' means no more than 'people disagree with me', it is certainly an illegitimate and unworthy escape from serious work. Perhaps the most alarming symptom of this retreat into the theorist's introspections is that it is no longer considered proper to doubt such intuitive data. At one meeting after another, such questions have been shrugged off as speakers refer impatiently to their own dialects as the only relevant source of evidence. As a result, one seldom hears questions about data any more.

A number of generative grammarians are now actively investigating the nature of syntactic dialects, most importantly Guy Carden. Carden has discovered implicational relations among dialect differences in the interpretation of negatives and quantifiers (1970). By demonstrating that a given interpretation of quantifiers with negatives implies the form of tag questions and other sentence types, Carden has given meaning to the concept of syntactic *dialect*. He has also given considerable attention to reliability (personal communication). He has examined 125 cases where an informant was asked the same question in a second interview and found

no change	99	
change	20	(9 interview errors, 2 systematic pressure changes, 9 apparently random)
possible change	6	(where identical responses might have been coded differently)

Given this degree of reliability, we must agree that the investigation of other peoples' intuitions is on a sound footing; at the same time, Carden recognizes that there is a problem of explaining and controlling the changes that occur.

Other studies of informant judgments indicate that we are dealing with a statistical phenomenon, at least as usually carried out. The studies of grammatical acceptability by Quirk and his associates confirm our informal observations that it is rare to find 100 per cent agreement or disagreement on any sentence. Investigations of judgments on tag questions and other syntactic issues carried out by Lehiste and by Wedge & Ingemann (1970) showed that such data is variable and shifting in the extreme. We can find implicational relations within the flux of responses (see Elliott, Legum & Thompson 1969), if we are prepared for a certain number of irregularities. There may be emerging a whole new calculus of variations in the study of intuitive judgments. However, there has been no success so far in replicating regular patterns in such variation (Postal 1968; Labov 1970*b*, and Heringer's disagreement with Carden 1968). In general we must observe that it is the nature of language to produce categorical judgments, and we should not forget that it is usually the difficult and disputed areas that have been investigated. But when we enter variable areas, it appears that intuitive judgments are less regular than behavior. We seem to move quickly from regular areas of social agreement (*langue*) to a region of intuitive parole. It seems clear that a great many disputes in generative grammar do revolve about an area of idiosyncratic behavior where the social compact has disappeared. For rare sentence types, it is only natural that each individual should have to solve the problem for himself; in so far as he can do this by extending his current roles in a predictable manner, we are dealing with *langue*; in so far as individuals diverge without any observable pattern, we are dealing with true idiolects. The very concept of *idiolect*, of course, represents a defeat for the Saussurian notion of *langue* as the general possession of the speech community. Our general aim is to write the grammar of that speech community, with all of its internal variation, style shifting, change in progress (Weinreich, Labov & Herzog 1968). When the data begins to fragment into unpatterned idiosyncrasies – for normative judgments for actual behavior – then linguistics comes to a stop. This is not the kind of data that we can rely on for a theory of language that would satisfy the least of Saussure's ambitions.

OBSERVATIONS

There is obviously something odd in placing *observations* last in the types of data used by linguists. But the observation of natural speech is in fact the most difficult of all the methods discussed so far. Texts, elicitations and intuitions are much more accessible, more easily segmented and classified; yet the wealth of linguistic description and theory which has been built upon such data still remains to be interpreted in its relation to language as a vehicle of communication in everyday life. In the gathering of elicitations and intuitions, there is no obvious sense in which the work can be described as *valid*. If another linguist obtains the same judgments from native speakers or from his own introspections, then we can say that the method is *reliable*. But reliability by itself does not help us in developing a sound theory of language in the sense that we intend. Very often the linguist is actually producing his own phenomena.[5] He has therefore created a further problem of relating these artifacts to natural language.

What would it mean for elicitations or intuitions to be valid? One might

[5] As linguists increase their familiarity with the philosophy of ordinary language, there is a growing tendency to use 'thought-experiments' as a means of manufacturing data. Thus Jerry Morgan develops an idea about time machines (credited to McCawley) as a means of producing data about the reflexive. What would an investigator, sitting in a time machine, say if he saw an image of himself slapping himself? According to Morgan, he could report 'I slapped myself'. 'If new me reaches out from the time machine and slaps his younger counterpart, I can report it by "I slapped myself". But if old me reaches into the time machine and slaps new me, I cannot report it by "I slapped myself"' (1969:55). If Morgan should succeed in getting reliable responses to such thought-experiments, then he would have the problem of relating these responses to language as we have conceived it here.

reasonably demand that they match the language of everyday life used when the linguist is not present. This demand follows from the fact that there are very few linguists and there are many speakers, an observation which might be formalized as the PRINCIPLE OF THE VOCAL MAJORITY: *many speak but few elicit.* Therefore if our theories are merely the artifacts of our own analyzing activity, they will have little to tell us about the natural evolution of language. Either our theories are about the language that ordinary people use on the street, arguing with friends, or at home blaming their children, or they are about very little indeed.

Those who gather literary texts are actually observing something which was produced independently: historical linguists are certainly engaged in the observation of language. Some believe that the full structure of language can only be observed in its most literary developments, and that speech is relatively impoverished. However, we retain the conviction of our predecessors in American linguistics that texts can be understood only in their relation to the spoken language – that the main stream of evolution of language is to be found in everyday speech, even in highly literary cultures such as our own.

In order to introduce observations from everyday life, we have to carry out a thorough-going criticism of beliefs and ideology more or less as Chomsky did for the methodology of the Bloomfieldians. One widely propagated belief which is used to discourage the study of ordinary language is that speech is incoherent. Chomsky has often remarked that the child must discard the largest part of what he hears as ungrammatical (1965: 58). This view is a myth based upon no evidence at all, except perhaps a few transcripts of learned conferences. Anyone who works with natural speech realizes this, and it has been shown systematically that the majority of sentences spoken by ordinary people are well-formed without any editing; all but a tiny percentage can be reduced to that form by the use of simple and universal editing rules (Labov 1966b).

Secondly, we find that most investigators describe their own community as exceptional, rife with dialect mixture and chaotic variation as compared to the homogeneous nature of traditional speech communities. But such homogeneous communities are also myths. As Gauchat showed (1905), even the most remote Swiss village shows systematic variation across sex and age group. More recent investigations of speech communities in New England, New York City, Detroit, Hillsboro, Salt Lake City, and Norwich show that this variation follows regular patterns which tell us a great deal about the evolution of language as well as how people use it. We find again and again that the grammar of a speech community is more regular than the behavior of the individual (Labov 1966*a*; Shuy, Wolfram & Riley 1966; Levine & Crockett 1966; Cook 1969; Trudgill 1970).

We find a third ideological barrier in the claim that all such data belong to some other far-away discipline called the study of performance, to be realized when we have mastered the facts of competence. The distinction between competence and performance may have its uses, but as it is now drawn it is almost incoherent. If performance factors are those which facilitate or impede the production of sentences, then almost all of our transformational apparatus would fall under that rubric: rules of extraposition, complementation, particle movement, negative attraction, and so on. We begin with a multi-dimensional deep structure, impossible to perform, and end with a linear organization that is easy to say and to grasp. Instead of the left-embedded *For anyone to do that is a shame*, we extrapose, and say *It's a shame for anyone to do that*; we then perform with greater ease a right-embedded complement as a result of this extraposition.

There are also technical innovations which facilitate the study of everyday speech. The magnetic tape recorder was introduced to this country just after World War II. But most linguists have been slow to admit its importance, continuing to claim that data jotted down in person is more reliable than a tape recording. Most linguistic students in graduate departments have access to an aged Wollensak, if that, and have gotten no grasp of the difficult art of making good recordings. It would be fair to say that a lack of professional orientation towards equipment has been a serious impediment in the development of the study of language in everyday life. The only serious relation to instrumentation is found among phoneticians, and the general impression holds that good recordings are important only in the laboratory. But in actual fact, much better recording techniques are needed for the study of grammar than for phonology, even better equipment is needed for the analysis of discourse in ordinary interaction.[6]

The strongest constraints that prevent linguists from utilizing the wealth of linguistic data with which they are surrounded are the barriers against interaction with strangers in one's own culture. The most common question addressed to me after a lecture involving data drawn from speakers outside the university is: 'What do you say to these people?' This is a legitimate and important question. But before we can answer it, we must recognize the nature of the problem: the unnamed and unspecified fears that these strangers will somehow do us harm. Each person is secretly convinced that he alone is fearful and isolated. To protect themselves against accusations of incapacity, neglect or cowardice, many academic people manufacture a counter-ideology: maintaining that these other people outside the university passionately want to be left alone; that it is immoral

[6] Some of the most recent developments in recording which are important for the study of natural interaction are actually setbacks (possibly temporary) in fidelity. Cassette tape-recorders running at 1⅞ inches per second cannot give us the frequency response to capture all of the relevant acoustic properties of the speech signal. But their small size, low cost, and inconspicuousness can hardly be discounted in the pursuit of naturalistic observations. There have been advances in the insulation of built-in condenser microphones from motor noise which are of some importance for fieldwork of this sort, and it is likely that the combination of stereo separation with equipment of this type will be a further step of consequence for the study of natural groups.

to invade their privacy by talking to them.[7] It is necessary for many academics to established (1) that what they are doing is what linguists should do, and (2) that it is immoral, and not proper linguistics, for others to do otherwise. But no such defense is necessary, since the fears that people feel about contact with strangers are the product of regular rules of social interaction, common to everyone. To overcome such barriers, one must have a general understanding of these rules, how they can be used to advantage, and what their limitations are. With a few exceptions, our society is composed of people who recognize the barriers between themselves and others, who would like to overcome them, and are delighted when someone else takes the step which makes this possible.

There are personality differences among linguists which will inevitably lead to specialization in the library, bush, street or closet. But it seems to me that one must resist the tendency to redefine the limits of linguistics to suit one's own personality. We see this tendency in the historian's rejection of spoken texts, the structuralist's rejection of intuitions, the intuitionist's rejection of everyday speech, and the anthropologist's rejection of his own society.

Our first steps in the study of everyday life allow us to say something about the crucial question of validity for elicitations and intuitions. Under what conditions do normative responses diverge from behavior? Under what conditions can we ask direct questions about grammaticality and obtain responses related to the language which is used in ordinary communication? On the whole, we cannot do this with young children, although Lila Gleitman has demonstrated the extraordinary capacity of certain children as grammarians (1970). The dialect used by children is only one of many non-standard dialects which are in contact with a dominant standard. Given such a condition, we can assert a general PRINCIPLE OF SUBORDINATE SHIFT: *When speakers of a subordinate dialect are asked direct questions about their language, their answers will shift in an irregular manner toward [or away from] the superordinate dialect.* This principle operates whenever we try to study the rules of working-class dialect, Black English, patois or creole using formal elicitation or training native speakers to ask themselves questions. That is not to say that such activities should not or cannot be carried on; but in the absence of any other data, one must expect that the results will be invalid in a number of unspecified and unforeseeable ways.[8]

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[7] Over the past several years, I have collected a number of questions from academic audiences which presuppose or imply this moral stance. E.g. 'Under what pretext do you speak to these people?' One professor asked me, 'How do you insinuate your way into these groups?' and a little later his wife asked, 'How do you accost them?' *Pretext*, *accost* and *insinuate* appear to share a feature of [-propriety] which I have tried to elucidate in the general formulation above.

[8] It is not only subordinate adult dialects which are governed by this principle. Language forms used by children are non-standard dialects which are generally considered incorrect, and one can expect that the direct elicitation of data from children is not easily related to their actual production. It is a general finding that formal experiments and elicitations from children show a linguistic competence which is below that dis-

Granted the ability to pass beyond ideological, technical, and social constraints, and the recognition of the disjunctions between norms and behavior, there remains a crucial methodological paradox in the study of everyday language. It follows from five principles which have been discussed elsewhere (Labov 1970*b*), and will be stated here quite briefly.

First is the PRINCIPLE OF STYLE SHIFTING: *there are no single-style speakers.* Whenever we first encounter a speaker in a face-to-face situation, we must assume that we are observing only a limited part of his entire linguistic repertoire. There may be some linguistic features that do not shift from one style to another, but every speaker will have a configuration of linguistic variables that shift from one context to another.

The PRINCIPLE OF ATTENTION asserts that *Styles can be ordered along a single dimension, measured by the amount of attention paid to speech.* Despite the varied nature of stylistic influences, and the multi-dimensional character of stylistic rules, all of the patterns can be projected on a single ordered dimension which has significance for our methodology. Casual and intimate styles can be stationed at one end of this continuum, and frozen, ritualistic styles at the other. At present we can control some of the factors which cause attention to be paid to speech (see below), but we have not yet quantified the actual behavioral feature: attention to or monitoring of speech.[9]

The third in this series is the VERNACULAR PRINCIPLE: *that the style which is most regular in its structure and in its relation to the evolution of the language is the vernacular, in which the minimum attention is paid to speech.* To justify this principle fully would require a review of a large body of sociolinguistic data from a great many sources (but see in particular Labov 1966: XIV). This principle can also be seen to follow quite naturally from the PRINCIPLE OF THE VOCAL MAJORITY cited above. It is the high frequency and practiced automaticity of everyday language which is responsible for its pervasive and well-formed character. The word 'vernacular' has sometimes led to the misunderstanding that this principle focuses only upon illiterate or lower-class speech. Most of the speakers of any social group have a vernacular style, relative to their careful and literary forms of speech. This most spontaneous, least studied style is the one that we as linguists will find the most useful as we place the speaker in the overall pattern of the speech community.

It can readily be seen that the fourth principle interferes with the third. The

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played in their spontaneous performance (Brown 1971). Observations of language in use were first developed in this field, and recent advances in the semantic interpretations of children's language have utilized close attention to the behavioral context of speech (Bloom 1970).

[9] Methods for quantifying attention have been developed by Broadbent (1962), but such experimental techniques have not yet been applied to measure the amount of attention paid to speech.

PRINCIPLE OF FORMALITY states that *any systematic observation of a speaker defines a formal context in which more than the minimum attention is paid to speech.* By 'systematic observation', we include more than the presence or absence of a human observer. The tape recorder itself has a variable but persistent effect in shifting speech towards the formal end of the spectrum.

We are then left with THE OBSERVER'S PARADOX: *To obtain the data most important for linguistic theory, we have to observe how people speak when they are not being observed.* The various solutions to this paradox define the methodology for the study of language in context.

This methodology can be presented in the form of a brief history of sociolinguistic methods. The point of departure is the traditional practice of dialectology, in which the main concern is to elicit relatively small pieces of lexical or morphological information. This implies a long question from the interviewer and a short answer from the subject – just the opposite of our present practice which limits any question to a maximum of five seconds. But the early interviews in Martha's Vineyard (Labov 1963) and Detroit (Shuy, Wolfram & Riley 1966) contained many such long, short-answer questions.

The sociolinguistic interview form was developed largely in work on the Lower East Side of New York City and in Harlem (Labov 1966*a*; Labov, Cohen, Robins & Lewis 1968). There we explored ways to break through the constraints of the interview situation, as expressed in the FORMALITY PRINCIPLE. These constraints can be sidestepped by setting up the assumption that they are not relevant for a given time, or they may be overridden by more powerful factors. In the Lower East Side study, five contextual situations were located in advance where the vernacular would be most likely to emerge, and in such a context casual speech was identified by the occurrence of one or more independent channel cues: increase in volume pitch, tempo, breathing, or laughter. A question that effectively triggers such responses may take six months to a year to develop – for the theme is not the only important feature. Placement, wording, timing and delivery all contribute to the likelihood of involving the speaker to the extent that formal constraints are overridden. One of the most successful questions of this type is on the Danger of Death: 'Have you ever been in a situation where you were in serious danger of being killed, where you thought to yourself, *This is it?* . . . What happened?' A dramatic shift of style can be seen in the following brief passage, from an interview with an eighteen-year-old Irish–Italian boy in New York City. His careful style breaks up suddenly, with nervous laughter, hard breathing, and rapid bursts of speech. The important linguistic variables shift at the same time: (ing) shifts from [ɪŋ] to [ɪn]; (th) and (dh) shift towards [t] and [d]; consonant cluster simplification rises sharply, and negative concord appears. In this transcript, suppressed laughter is indicated by [hh] or [hhh], usually superimposed on the words, and the switch to casual style and back again is indicated by italics.

(What happened to you?)

The school I go to is – uh – Food and Maritime, That's – uh – maritime training. And I was up in the masthead, and the wind started blowing. I had a rope secured around me to keep me from falling. *But the rope parted* [hh] *an' I was jus' hangin' there by my fingernails* [hhh]. *I never prayed to God so fast* [hh] *and so* [hh] *hard in my life! But I came out all right.*

(What happened?) *Well the guys came up an' 'ey got me.* (How long were you up there?) *About ten minutes* [hhh]. (Jees! I can see you're still sweatin' thinkin' about it.) *Yeh* [hhh]. *I came down, I cou'n' hold a pencil in my han'* [hhh], *I cou'n' touch nuttin'. I was shakin' like a* [hhh] *leaf. Sometimes I get scared t'inkin' about it.* But – uh – well it's training!

In constructing such questions, one must turn back to the basic sociolinguistic conundrum: 'Why does anyone say anything?'[10] There are three content themes which have the greatest force for evoking speech from the broadest range of speakers: (1) death and the danger of death, including any form of physical violence (fights, accidents, sickness, operations); (2) sex and all of the machinery for interaction between the sexes (proposals, dating, household negotiations); (3) moral indignation (e.g. 'Did you ever get whupped for something you didn't do?') Beyond such general considerations, there are a vast array of local issues, humor and gossip which the field worker must seize as a by-product of participant-observation. Questions in specific local areas are constructed by a feedback technique which progressively assumes more as the field worker knows more.[11] An initial question. 'Do you play the numbers?' would thus give way to 'Did you ever hit big?' In talking to deer hunters, an initial question such as 'Where do you aim?' would give way to 'Is it worth trying a rump shot?' As the outsider gradually becomes an insider, the quality of the speech obtained and the speaker's involvement in it rises steadily. A field worker who stays outside his subject, and deals with it as a mere excuse for eliciting language, will get very little for his pains. Almost any question can be answered with no more informa-

[10] That is, the study of methods involves us in basic questions of discourse analysis. Naïve approaches to eliciting speech rely heavily upon questions which superficially force responses. Experimental methods used to assess the verbal competence of children also utilize direct questions and obtain systematically misleading data. Further analysis of the factors which lead children and adults to speak inevitably involve us with a consideration of the actions being performed, and the underlying propositions about the role of the speaker and addressee. Such considerations form the foundation of a study of discourse which distinguishes sequencing rules from rules of interpretation which carry us from 'what is said' to 'what is done'.

[11] Such methods are similar to the techniques used by anthropologists in forming questions which reflect the categories and vocabulary of the native culture (Black & Metzger 1965). But the extreme formality which has been used in the approaches reported so far may give misleading results if there is a significant disjunction between norms and behavior.

tion than was contained in it. When the speaker does give more, it is a gift, drawn from some general fund of good will that is held in trust by himself and the field worker. A deep knowledge implies a deep interest, and in payment for that interest the speaker may give more than anyone has a right to expect. Thus the field worker who can tap the full linguistic competence of his subjects must acquire a detailed understanding of what he is asking about, as well as broad knowledge of the general forms of human behavior.

Beyond the interview. The individual interview will remain as the foundation of our investigations, since only there do we control the large bulk of speech and the complex structures needed for the study of grammar. But the methods just described for overriding the constraints of the formal interview are only substitutes for the real thing, and give us only fragments of the vernacular.

A more systematic approach to recording the vernacular of everyday life is to allow the interaction of natural peer group itself to control the level of language produced. The techniques used here are from the original work done by Gumperz in Hemnes, Norway (1964) where he recorded the interaction of closed and open networks of members of the community. The investigators provide the initial setting, but gradually recede from the situation; the effect of recording is never wholly absent, as our principles would predict, but it is largely overridden by other factors – the same as those which operate in everyday life. Such techniques were further developed in our work with adolescent peer groups in South Central Harlem (Labov, Cohen, Robins & Lewis 1968: 1, 57). Though group sessions give us the basic standard by which we can calibrate our other work, they do not usually provide us with enough linguistic data on each individual. It is possible to feed back some of the same interactive mechanism to interviews with one or two speakers, if the interviewers are members of the same community. There are great differences in the techniques available to insiders, in the quality of interaction, and in the type of information obtained, as compared to the best interviews done by outsiders. Insiders cut deeper; but at the same time, they are more limited in the range of speakers they can deal with. Since they have a fixed location within the community, many members cannot speak to them as freely as they would to a stranger. Narratives told to insiders tend to be more fragmentary, less well-formed than those told to outsiders. The *outsider* can be seen as a blunt instrument, a useful tool for all kinds of rough work, while the insider can penetrate more deeply in a narrower range. No serious study of a speech community should be planned without including both kinds of investigators from the outset.

The kind of long-term observation just described would be most important in studies of the acquisition of language. No such studies have been carried out yet with a natural play group as the focus; the family has been the only target of attention. Within that matrix, there has been considerable progress in the kinds of observations that have been made, with increasing attention paid to the physical behavior of the child which offers the clues we need to semantic interpretation (Brown 1970: 100; Bloom 1970: 15–33).

The family as a whole is the focus of our attention in our recent studies of sound change in progress (Labov 1970*a*). The main purpose is to trace age-grading and the effects of change in progress. But we are also aware that our individual interviews give us only an approximation to the vernacular, and family sessions can give us observations of spontaneous interaction for calibrating the main body of data. A single question, aimed at issues that are argued within the family every day, can trigger an extended discussion in which the interviewer gradually recedes from view. In the following example from a family in East Atlanta, we can observe the beginning of such a lively argument, and the emergence of family interaction that gives us the vernacular style we are looking for. This was an after-dinner conversation at the house of Mr and Mrs Henry G.; gathered around the dining room table were Henry Sr, 59, his wife Mrs Henry, 55, their two daughters Gail and Barbara, and Barbara's husband Bill. Bill works as a machinist in the railroad repair shop where Henry Sr is a foreman.

Interviewer: Do you think there's a natural life span for people, or is it possible there's a way for you to live longer and longer?

Henry Sr: [head of household, 59] Yep. They're gonna do that. They're provin' that every day.

Mrs Henry: [55] [overlapping] They *are* livin' longer and longer. People *are* livin' longer. And that's health, you know. Back then, I don't see how they lived as long as they did . . . back – years ago.

Both husband and wife respond together with the same point of view. The interviewer finds that he has touched a live issue in the family as he is cut off and the conversation proceeds on its own momentum. All five family members join in as the theme moves to the familiar issue of the old ways versus the new.

Interviewer: Yeh, well some –

Henry Sr: What is it? Used to be forty when I was young, and now it's sixty something.

Mrs Henry: Oh – You take a woman back when – even when I was a chil', a woman thirty-five was *old*!

Barbara: [daughter] Probably you took to a rocking chair.

Mrs Henry: Yeah, now that's really the truth, they really were, and they looked old – thirty five.

Mrs Henry: And they were old.

Gail [daughter]: You had to b'eak forty, you were an old man [hh].

Henry Sr: Back then, look what a woman had to do.

Mrs Henry: Tha's why I know; that's what I said.
Bill: [Barbara's husband] Nowaday all they got to do is th'ow it in here, 'n' th'ow it in there, 'n' they got a machine to do it fo' em.

Until Bill entered the picture, sex was not the issue here. But Bill picks up "woman" from Henry Sr and makes it the main point; his wife laughs at his familiar stance. Mrs. Henry gives a straight answer, but when Bill keeps on woman-baiting, she picks up a cue from her husband and carries the day with a crushing reply.

Mrs Henry: Well that's everything – we get a –
Bill: [overlapping] They don't have to exert theyself. Y'got a vacuum cleaner 'n' you push 'n' 'a's all you gotta do.
Barbara: [Laughs]
Mrs Henry: Well that's not healthy. I think it's good for you to do something – work, far as that goes.
Bill: Back then – girls nowaday, back then, if they had to wash clothes –
Henry Sr: [overlapping] Well how about the men? How about the men?
Mrs G.: Men has never worked! [General laughter]

This conversation then continued for another five minutes without any intervention of the interviewer. Bill and Henry Sr engaged in a long argument on which was harder work—using a sledge hammer in the old days, or using a pneumatic hammer today. The family members put themselves on display, showing us their immediate concerns, their mode of argument, their system of values, and the grammatical structure of their everyday language.

As we move away from the individual interview, our data is less complete, but closer to the language of everyday life. Another step in that direction can be taken through RAPID AND ANONYMOUS OBSERVATIONS. Here we may know very little about the speakers, but we can observe a great many of them, and the effect of the interview situation is nil. The survey of (*r*) in New York City department stores (Labov 1966*a*: III) provides one such model, and others have been developed in work with the telephone, asking directions, or street corner observation. More recently we have used such techniques to observe the use of Spanish on Harlem streets and check the birth place of those who use the language there. Such rapid and anonymous studies have built-in sources of error, but the error is complementary with that of interviews. When the two kinds of data converge, we have in effect partialled out the effect of experimental and observational error.

There remains a residual and almost insoluble problem – the rarity of many grammatical forms. It will no doubt always be necessary to extend our observations with intuitions. Yet we are only beginning to learn how to enrich the data of natural conversation by minimal intervention. When we fully understand the use of a given grammatical form, we are then able to elicit it in conversation without using it ourselves, and without seeming to do anything odd or artificial. This has been done only for a small number of items, such as the preterit, the passive, the future, and present perfect, and relative clauses. In general, we can say that the future study of language in context will depend heavily upon the development of means of enriching the data of natural conversation.

CONVERGENCE

This discussion has been limited to the methodology involved in gathering linguistic data. A second paper of equal length is needed to deal with the methods used in analysis. In such a discussion, we would be able to review the more detailed evidence which supports the Vernacular Principle – that the most systematic style is that used when the minimum attention is paid to speech. We would also consider the ways in which different methods can be mutually confirming. In pages 105–108, we noted that the study of intuitions has not achieved this kind of convergence, and it seems unlikely that the current explorations of variation and implications in intuitive judgments will yield that kind of result. If we are right in thinking that most of this variation represents a kind of intuitional *parole*, we can hardly expect uniform and convergent patterns to emerge. The most encouraging aspect of work with observations within the speech community is that we have achieved such convergence on principles of great generality. A number of investigators have reproduced patterns of intricate and regular interaction of stylistic and social stratification for stable linguistic variables such as (ing) (Fischer 1958; Labov 1966*a*; Shuy, Wolfram & Riley 1966; Labov, Cohen, Robins & Lewis 1968; Trudgill 1970). The cross-over pattern associated with the hypercorrect behavior of the lower middle class has been reliably reproduced in many independent studies (Labov 1966*a*; Levine & Crockett 1966; Wolfram 1969). We have independent confirmation of the fact that women may lead men by almost a generation in on-going linguistic changes but also shift more towards the older prestige standard in formal situations (Gauchat 1905; Labov 1966*a*; Shuy, Wolfram & Riley 1968; Wolfram 1969; Trudgill 1970). Variable constraints on the contraction and deletion of the copula have been confirmed in several independent studies (Labov 1969; Wolfram 1969; Mitchell-Kernan 1970). A full presentation of the methodology of analysis would show these convergent results in detail, but enough synthesis has been done so far to justify the claim that with observations of natural speech form an adequate base for intersubjective agreement in linguistics (Labov 1970*b*). If this agreement has been achieved it is because attention was given to possible sources of error in each study. The most effective way in which convergence can be achieved is to approach a single problem with different methods, with complementary sources of error. A number of the studies cited have used observations, intuitions, elicitations, texts and experiments to achieve this result. Here we must refer again to the PRINCIPLE OF CONVERGENCE: the value of new data for confirming and interpreting old data is directly proportional to the differences in the methods used to gather it.

Despite the fact that we have a variety of methodological approaches, the unity of linguistics is not hard to conceive. It is not necessary for everyone to use the same methods – indeed, it is far better if we do not. Otherwise we would not benefit from the complementary principle. The unification of linguistics must necessarily proceed from the understanding of linguists that the field need not be defined to fit their personal style, but can expand to a broad attack on the complexity of the problem. Data from a variety of distinct sources and methods, properly interpreted, can be used to converge on right answers to hard questions.

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