Changes in linguistic fieldwork
William J. Samarin

It would be perilous to attempt a description of linguistic fieldwork without subdividing the discipline and taking into account changes in research over the past twenty or so years. Since the 1940s, when I first became acquainted with scientific linguistics, there have been three different foci of interest, each with its bias toward a certain kind of methodology.

In the period when we did 'descriptive linguistics' we collected data for the writing of grammars--more or less 'whole' grammars that included everything from phonology to syntax, but very little of the latter and practically no semantics at all. In that period linguistic theory was being honed on preliterate languages--exotic, nonwestern languages most of them were. The source of our linguistic data, quite often just one person, was the informant--the native-speaker assistant. The methodology of that period, with its concern with good data from reliable informants, is described in my textbook, Field linguistics (1967).

My field guide arrived at the end of the era when fledgling linguists went to the field to do their research. With the emergence of transformational-generative grammar, pioneered by Noam Chomsky, attention was given to the
development of theory, and for that English or any other language one happened to know was good enough; one could, in fact, be one's own informant. One collected data by introspection, imagining what one could or would say and what one could or would not say. The goal was to describe how one spoke, with emphasis on one's competence to speak grammatically. As one might expect, there were quite a few disagreements between speakers of English, among the linguists, as to what was said in English--as to what English was with respect to this or that feature of grammar (in linguistic jargon--'grammatical rule'). Were there as many kinds of English as there were linguists who spoke it?

The third period, not without its descriptivist or generativist studies, is the sociolinguistic one. The major difference in this period is that one studies language, not with one speaker who knows all the language, but by means of a whole speech community, as represented, naturally, by valid samples of the same. As the name suggests, in this period one uses social data as well as linguistic data; this means sociological methods as well as linguistic. Moreover, one is concerned as much with how language is used and what people believe or feel about language as with the form their language takes.

My Field linguistics is no longer in print. An updated version might possibly incorporate generativist
My study of glossolalia began about thirteen years ago, at the time when the neo-Pentecostal movement began to attract the public's attention in the United States. (Since then the movement has become international and ecumenical. Catholics, both Roman and Orthodox, regularly and in very large numbers meet for religious exercises.) Members of traditionally non-Pentecostal churches--such as Episcopal, Presbyterian, and Baptist--were being converted to an acephalous movement that emphasized the "filling of the (Holy) Spirit." This is a quality of religious experience that is characterized by, among several other things, a heightened sense of the immediacy of God and a more conscious dependence on God in all matters of daily life (Samarin 1973). The movement promised that the transformation would be instantaneous, or almost so, and that it would be evidenced by God-inspired speech: that is, the person would "speak in tongues."

The reason I undertook the study was that I wanted to know what people did when they spoke in tongues. My interest was linguistic, in the formal sense. My goal was
ideas (although generativists have contributed very little
to linguistic methodology); it would certainly concern
itself with sociolinguistic methods.

In what follows I want to show how sociolinguistic
methods became very important to me in my work on
glossolalia. It is not a language in the proper sense,
yet I went about studying it as if it were. The fact that
I studied it at all and that I studied it as I did--
ethnographically and sociolinguistically--reveals how
things have changed.
and record. Several individuals also consented to tape-record their private devotional times for me. (Neo-Pentecostals have been extraordinarily open in their attitude and gracious to non-Pentecostals serious and sympathetic in their research.) In this way, more or less like a field linguist who had set out to describe an as-yet unwritten language, I collected a large amount of data that would be transcribed and then analyzed.

In field linguistics (as described in my book of the same name) the research is concerned with collecting authentic samples of the language from typical native speakers of that language for the most important genres of discourse. One assumes that grammar of the language will be the same for all speakers of the same speech community, and in all samples of the same genre: conversation, narration, folk tales, or whatever. One assumes, on the other hand, that there might be intertextual differences at different levels of linguistic structure. Certain kinds of words might be found only in incantations that accompany divining; the syntax of folk tales might be different from that of conversation, and so forth. In addition to the structural differences there would be statistical ones: the incidence of clauses of a certain type or certain connectives might be different. The field linguist is guided by these assumptions, but he doesn't know where he is going to find them. This is the reason for his striving to get a good
not ambitious: I set out simply to describe the phenomenon within the framework of what had been known as "descriptive linguistics." (I was also investigating the possibility that instantaneous speech—which is what glossolalia is by definition—revealed features that one found in expressive neologisms like arsey-turvey, diggity-dog, and hurdy-gurdy, that one finds in languages throughout the world but in greatest numbers in African languages.) I was not yet a sociolinguist, but this research led from linguistic data to human behavior and more than any other single factor transformed me into what I am today. The following discussion describes this transformation: how the scope of my research was broadened and how my research methods were modified in consequence of the change in the goal of the research.

My research was carried out in the field, in the library, and in the 'laboratory.' For linguistic analysis what I needed, of course, was tape-recorded samples of glossolalia from a cross-section of the adherents of the movement: people who had just acquired the 'gift of tongues' as well as those who had been utilizing it for some time, leaders and ordinary members, women and men, and so forth; I also wanted glossolalia from different contexts (large and small meetings) as used in its two principal functions (prayer and prophecy). I therefore participated in neo-Pentecostal meetings of various sizes to observe
concerned not only with what was said but also with where
and when and for what reason it was said. It was as if I
were a sociolinguist or sociologist of language trying to
describe the phenomenon of language (also called here, code)
switching, as between English and French or Spanish on
the North American continent.

When I addressed myself to the task of describing
glossolalia linguistically, I expected to find a smaller
range of uses. I had heard, for example, only of praying,
prophesying, and singing in tongues. On the other hand,
I expected to find a great deal of variation from speaker
to speaker--people who were all monolingual members of the
same speech community, and certainly between persons with
different histories of language learning (for example,
between a monolingual and bilingual). There is variation
in normal language as well, but never to such a degree
that the grammar is affected. This is so because people
acquire their first language monodialectally, gaining
language-generating competence in a context where their
linguistic environment is uniform. In short, they learn
to speak the same way others around them speak. With
glossolalia, on the other hand, each person is creating
his own language. That's the assumption I was starting with.
It is still true that each person's glossa (the 'tongue'
that a person speaks in a given period of time)\(^1\) is
pseudolinguistic and idiosyncractic, but I learned that
there was much more learning than I had imagined.
range of texts. That is, unless he deliberately restricts his description to a particular range of speech. One can write a grammar of conversational sentences only.

In ordinary field linguistics, where one is studying normal language, that is, natural language spoken by normal individuals, one doesn't characterize the psychological states of the speakers unless there is reason to do so. We don't say that a speaker happened to be sleeping or that he was tired, hysterical, drunk, or that he was in an altered state of consciousness. With glossolalia I was obliged to keep psychological factors in mind. It was not because they were salient features of glossolalic speech acts; they were not self-evident. Rather I had accepted as a hypothesis to be tested something others had taken for granted, namely, that glossolalia was the product or effect of an altered state of consciousness. This meant that I had to look for any clinical manifestations of, for example, trance or hysteria.

The tongue-speaking community is by definition a bilingual one. Each person has a normal first language and a second kind for use in certain religious contexts for certain religious functions. For this reason also the study of glossolalia was different from the usual field linguistic research. The principal of the latter is to describe linguistic form whereas my research on glossolalia was to describe both form and function. I was
Very soon in my research it became clear that I would be unwise to study glossolalia merely as a linguist, treating the recorded texts as if they were merely samples of ordinary language. There was some kind of interaction--some kind of relationship--between what the people believed--personally and idiosyncratically or in consensus--and what they spoke. (This is axiomatic to sociolinguistics, the foundation on which many of us linguists now base all our work.) Therefore contextual data became important to the study, and I interviewed all kinds of neo-Pentecostals, and some old-fashioned ones too, concerning their beliefs and attitudes. This was complemented by a wide reading of Pentecostal devotional, doctrinal, and apologetic literature, something no other nonbelieving social scientist ever seems to have done before. Furthermore, I distributed as widely as possible a mimeographed booklet with 71 questions (reproduced in Samarin 1972a): e.g., "Do you have the feeling that certain words from your tongues have meaning? List the words and their meanings." The questionnaire, since I was familiar with the population being sampled, was meant to be an "open-ended" instrument rather than one that would lead to massive, statisticable data. Finally, I conducted experiments with subjects as varied as university students, a tongue-speaking clergyman, and myself.
References


This paper was presented at the annual meeting of the Oral History Association in a session devoted to methodology, Saturday, October 27, 1978. In correspondence use this mailing address: William J. Samarin, 24 Chudleigh Avenue, Toronto, Ontario, Canada M4R 1T2.