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# Theoretical and Historical Perspectives on Researching the Sociology of Language and Education

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## Abstract

This contribution traces the development of the sociology of language and its key research approaches. Close attention is paid to the contrast between research approaches that focus on *verstehende* (understanding) and those whose primary goal is *erklärende* (explanatory). The piece concludes that the sociology of language and education must be inclusive enough and supportive enough to provide room and recognition for both *erklärende* and *verstehende* approaches to its subject matter.

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## Keywords

Sociolinguistics • Ethnography • Questionnaires • ANOVA

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Sadly, Dr. Joshua Fishman is no longer living; we republish his chapter here, unchanged from the previous edition.

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## Introduction: The Sociology of Language

The designation “sociology of language” is often used in conscious distinction to the designation “sociolinguistics.” The intent of this distinction is commonly relevant both to personal disciplinary orientation and to the level of data aggregation preferred by the researcher. From a disciplinary point of view, the designation “sociology of language,” rather than “sociolinguistics,” implies a greater concern with sociology than with linguistics, on the one hand, and a greater preference for higher levels of behavioral data collection (“higher” in the sense of more abstract, i.e., further removed from directly observed phenomena) and for higher levels of data aggregation on the other hand.

This contribution will trace the development of sociology of language and its key research approaches. It will consider the challenges of different research approaches and the relevance of those that focus on *verstehende* (understanding) and those whose primary goal is *erklärende* (explanatory).

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## Early Developments in the Sociology of Language

The sociology of language has developed alongside of sociolinguistics at least since the summer of 1964, when the modern study of language in social contexts was (re) constituted by a specially convened group of primarily US scholars. The linguists (mostly, anthropological linguists) and sociologists (most of them macrolevel oriented) spent an 8-week faculty seminar at the Summer Linguistic Institute, held that summer at Indiana University in Bloomington (Tucker and Paulston 1997). Since linguists were already focused upon language behavior (whereas sociologists were not, by and large), the perspective of “sociolinguistics” had greater momentum from the outset and could look forward to an academic home in departments of linguistics from the very first days onward. While a few of the Bloomington seminar sociologists immediately began to define themselves as “sociologists of language” (indeed, some not present at the Bloomington seminar had already so defined themselves much earlier, viz., Herzler 1965; Cohen 1956), they did not form a cohesive interest group, either then or afterward, few sociology departments being interested in the new specialty area. Even the designation “sociologists” was somewhat questionable for some of them, since it included the political scientists and the social psychologists among them. Accordingly, although the sociology of language began (and has largely remained) as a recognizable perspective of individual scholars, it never became a well-defined theoretical school nor developed a distinctive research methodology. It has remained a minority position within the total sociolinguistic enterprise, particularly in the USA.

Like sociology itself, the sociology of language has neither well-defined limits nor methods distinctly its own (see Fishman 1965, 1968, 1970, 1972). As a result, whereas sociolinguistics has gravitated toward microanalyses of snippets of “talk” and preselected conversations (Gumperz 1982) or toward particular genres of preselected texts (Hymes 1981) and therefore has no problem incorporating samples of actual speech or recitation in its presentations, the sociology of language has largely been “social problem” oriented (e.g., bilingual education, language maintenance and language shift, reversing language shift, the spread of English, language death, etc.), often utilizing contrasted polities, population groupings, and even the world at large as its universe of study and generalization for inquires into one macro-topic or another. As a result, the data of actual speech is no longer evident in its reports, such data being replaced by language or variety names or categories.

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## Major Contributions

### Contributions from Sociology

“The sociology” of any topic involves the social structure or differentiation of its manifestations in society. A sociological analysis frequently compares individuals in different ethnic groups, racial groups, religious groups, professional groups, age groups, occupational groups, or economic groups with respect to a particular social behavior (attendance at a Yankee game, participation in a general strike, armed forces service, participation in the elections, engaging in recreational reading, etc.). Some subgroups (ethnic group 1, ethnic group 2, ethnic group  $n$ ) may well manifest more of this behavior than others. In that case, the investigator may conclude that ethnicity does play a role in the social behavior being studied (participation in the May Day parade in New York City) or may try to push the analysis further to try to relate the ethnic differences that have been discovered to differences in family income, individual education, immigration status, etc. What may initially have seemed to be ethnic differences per se may, upon further inquiry, be more fully explainable in terms of economic or education differences between the groups involved. Sociologists will derive their hypotheses from the manifold previous studies that have been already been completed on the categorical groups that they are studying in a particular inquiry and on the social dimensions of concern to them (education, income, age, citizenship, etc.). Of course there will be a language variable involved too in the sociology of language and education (e.g., speaking a LOTE [language other than English] at home), but we will turn to such distinctly language variables below.

Sociological interpretations as to “causal factors” typically stop at the societal or group-membership level. This leaves it to other investigators (sometimes from other disciplines, including sociolinguistics) to investigate the role of more individual or psychological factors prompting attendance at Yankee games or participation in May Day marches. Whether or not to extend one’s research to the individual level too will also depend on the investigator’s (inter)disciplinary training and particular focus of

inquiry. Although personality factors may be involved in Yankee game attendance, neither the sociology of language nor the Yankees per se may be interested in sponsoring research on such variables because they would provide little valuable information to them that could easily be incorporated into their own prior *modus operandi*.

## Connections to Education

The sociology of language has been drawn to the study of language in education more by need than by prior intellectual interest. Each of the abovementioned macro-topics has most often been researched within educational settings and institutions (school systems, school districts, school grades, school rooms, etc.). School settings and situations are often selected for sociology of language inquiry because of various assets that they possess and prominently manifest. Schools have research budgets to expend, populations (including minority populations with language problems) that can easily be tapped as data-collection participants, and qualified staffs that can be relied upon to keep order, provide background data, and, in general, assist with the “bookkeeping” that all research entails, shielding investigators from interruptions, interference, or other disturbances. Few of the aforementioned assets amount to theoretical or substantive preferences, and, as a result, the outcomes of such investigations are often both less specifically relevant for education and more relevant for larger-scale societal institutions and social processes more generally than might otherwise have been anticipated.

Although “education” is commonly defined as school-situated or school-related input or output, that need not necessarily always be the case. Education need not be conceptualized in a manner that limits it to either formal settings, curricular emphases, or stereotypic roles (“students,” “teachers,” “administrators,” “school board members,” “parent body,” etc.). Language use during recess in the school yard (playground) is a perfectly reasonable example of simultaneously minimizing formal school influences on informal language use while still easily locating subjects of both sexes and various ages. More generally, therefore, education need not be limited to formal settings or scheduled curricular processes. “Education” may be taken in its broadest generic sense of “to lead, rear, bring up,” whether by example, instruction, or other influences, planned or unplanned, and with or without extrafamilial intervention.

In its most general terms then, education can be seen as a lifelong process of elicited responses, growth, development, and change. The sociology of language and education, therefore, necessarily focuses on only part of the total educational process that pertains to language in society but that is an important part indeed, language being both a major part of the input and a major part of the output of the entire process of education in society, regardless of its localizations. All in all, the sociology of language and education entails a triangulation between societal influences, educational processes, and language input or output. The need to include data collection and theoretically guided interpretation on three different dimensions contributes both to the difficulty and to the stimulation encountered in research on the sociology of language and education.

## Key Research Approaches

The social sciences in general, and the sociology of language and education (SLE) among them, share a small array of research methods and techniques. This array extends from ethnography and observation, at one extreme, through to controlled experiments, at the other, with correlational studies based upon content analyses, questionnaires, and other investigator-constructed “tests” occupying a middle ground between the two extremes. Each of these methods possesses its very own and distinctive advantages and disadvantages.

*Ethnography*, the classical approach of anthropology, based upon the in situ fieldwork observations and recordings of trained and sensitized observers, has gained a considerable following during the past half century in conjunction with the study of language and education.

It typically pursues the formulation, disconfirmation, or confirmation of hypotheses (e.g., “Teachers in X-town public schools reinforce English only speaking students more often and more positively than they do speakers of LOTE”) by means of a large number of extensive observations in various school settings. Ethnographic reports typically include many verbatim excerpts from teacher-pupil interaction, as well as holistic descriptions of persons, places, and events that provide the reader (or viewer of filmed information) with a feel for the “real thing,” second only to being “there” while ongoing life unfolds. This “slice-of-life” realism is obtained at a price, as is the case with every research method bar none.

Ethnography finds it difficult to control certain factors (e.g., pupil social class, ethnicity, age, general attractiveness, etc.) while focusing on others, primarily because life does not present itself naturalistically in terms of neatly controlled but otherwise comparable packages. Of course, given sufficient experimenter time and funds, all of these secondary “causes” or elicitors of teacher reinforcement can be observed in action, and the differences between their rates can be noted and taken into consideration as indications of stronger or weaker co-causes than the major one (pupil’s variable classroom usage) and their separate or combined effect upon or modification thereof. However, researchers seldom if ever have sufficient time and resources, and ethnography is, therefore, not an easy or precise method of unraveling complex interactions between the large number of possibly contributory ongoing aspects of any real-live interaction. An additional concern is that of observer reliability and validity. Wherever there is only a single personally invested observer for any data set, we are left with the problems of observer bias, observer consistency over time, and the entire “issue of degree” of any observed and counted “teacher reinforcement” versus those not counted because they are simply unnoticed or judged to be too weak or ephemeral to count. Investigators also obviously differ from one another in their “ethnographic sensitivity” or “ethnographic aptitude,” and, therefore, although the method provides much direct and immediate researcher gratification, it is so labor intensive and so bound up with the quirks of a single observer that some researchers have concluded that other methods are needed (or needed in addition) for the sociology of language and education if its frequent confounding of method and researcher is soon to be overcome.

*Controlled experiments*, at the other end of the methodological continuum, are the characteristically preferred method of psychological research. Whereas ethnography sacrifices precision and complexity so that it can maximize “holistic realism,” exactly the opposite is true for controlled experiments. Thus, in a study of teacher interpretations of English-Spanish code-switching by pupils, three different pre-filmed scenarios (representing high, medium, and low degrees of code-switching by the same group of student actor-confederates) were viewed by randomly assigned Black, White, and Latino teachers in a large metropolitan high school, each of whom viewed only one scenario. After their viewing the scenario to which they had been assigned, teachers were debriefed as to their knowledge of Spanish, frequency, and types of out-of-school interactions with Latinos, attitudes toward switching, and their interpretations of the overt and the latent meanings of 20 switches that had been built into each scenario. Variables that were excluded from research attention (e.g., teacher age, experience, and attitudes toward race/ethnicity) were “controlled out” of the study by means of random selection and random assignment of teachers to switching groups, so that these variables could not affect any discovered “between teacher and group” differences with respect to levels of switching at more than a “chance” level. Unlike ethnographic researchers, experimental researchers never have the satisfaction of experiencing the reality of “actually having been there.” On the other hand, the latter have the satisfaction of precise answers to precise questions (e.g., does intensity of switching behavior among students effect teacher understanding of latent meaning among teachers who are White and non-Latino?), with the probability of error (false negatives and false positives) being known in connection with answering each such question.

Somewhere near the middle of the continuum of naturalness and precision are the *questionnaire methods* (including most investigator-constructed data elicitation methods, even if they are not of the traditional questionnaire type, e.g., *guided interviews*, *observational checkoffs*, certain *projective techniques*, etc.). Wherever total scores can be derived independently for each member of a studied sample from a summation of that member’s item scores (Fishman and Galguerra 2003), both fully structured and less-structured elicitation methods can be constructed by means of exacting item-analysis methods and can be tried out and improved, item by item, for both item and total instrument reliability and validity.

The only conditions or limitations on the latter claim are (i) that all item scores be independent of each other (i.e., “non-iterative”) and (ii) that a single criterion measurable in “more vs. less” terms be applicable to them all. Thus, for a criterion such as amount of switching during a prior unstructured conversation on “What I do after school,” the predictors of switching can be true-false (or other dichotomized) items, attitudinal or behavioral degree items (fully agree, agree more than disagree, neutral, disagree more than agree, totally disagree), investigator observed checkoff items, projective or other interpretation items with a choice among several enumerated replies, etc. Thus, although formal and semiformal measurements provide neither for the naturalness and holism of ethnographic methods nor for the exact estimation of “error variance” in responses (i.e., variation on factors that the investigator prefers to exclude from a given study) of experimental methods, they do

possess several significant benefits of their own, particularly with respect to demonstrable reliability and validity or the lack thereof.

The major lesson from the brief methodological review in this section is that there is no fool-proof research method for the sociology of language and education. Nor are its researchers methodological factotums, each being most comfortable and productive at a certain point along the entire methodological range. There is no methodological orthodoxy that pervades the entire field, to which all funding agencies, research centers, and journals pay allegiance. A greater or smaller degree of methodological heterodoxy is both the rule and is very much to be recommended as well. Only by increasing the methodological range of one's own competence and comfort can investigators really weigh all of the assets and debits of any choice among them in each particular study that is undertaken. Methods and researchers should never become fully redundant considerations. The trustworthiness of research findings are much enhanced by multi-method and multi-investigator replications, both within and between topical and subtopical areas of the sociology of language and education.

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## Problems and Difficulties: Reality and Complexity

Every bit of research that is reported was conceived of as a means of tapping into both the reality and the richness of "actual life." However, reality is complexly multilayered, and it is very difficult (or perhaps even impossible) to be sure that one has captured enough of the subtle layering of any dimension being studied to be able to draw inferences pertaining to these dimension as a whole. Teacher acceptance of code-switching and code-switching per se both present many obvious and subtle examples of this difficulty.

In our discussion of factors contributing to teacher acceptance of switching, we have recognized at least some of the complexity of common influences on both teachers and bilingual pupil behavior. We have not doubted that there may be other factors at play here, but we have decided to either treat them as "error variance" or to "control them out" via random selection of subjects and random assignment of subjects to differing intensities of switching presentations. Both questionnaire data and experimental data can be subjected to a statistical analysis via a technique known as *analysis of variance (ANOVA)*. This technique essentially compares the variation associated with the data related to any particular dimension of analysis with the total variation exhibited by the data as a whole. Only if the latter is sufficiently great relative to the former can that particular dimension be considered a significant one (i.e., one unlikely to be merely a chance finding due to random sampling factors alone).

While it is impossible in a single brief article to render this technique intuitively transparent, it becomes even more useful if an outside criterion is also available (e.g., the ratings of expert judges [speech therapists] of the switching frequency of each student during several months of interactive observation with a variety of "others" and in a variety of "settings"); then this criterion can be used to gauge the extent to

which any predictive dimension by itself (e.g., race of student or bilinguality of the teacher), or all of them taken together, account(s) for the variance on the criterion. In this manner, the investigator can tell whether the criterion is adequately and significantly accounted for by the research instruments utilized. Obviously, the higher the correlation between the two, the more reliable and valid the explanatory capacity of the particular dimension or set of dimensions. But this crucial determination, available only for experimental and questionnaire data, does not convince ethnographers that these “other” methodologies have studied “the real thing” to any degree similar to that attained by their own studies. Similarly, the quantitative analysts are never convinced by the qualitative findings produced via ethnographic research. Why not?

Ethnography’s implications that its method (and its alone?) can study “the real thing” (and, therefore, “discover the real truth” about it) raise an intricate set of fundamental issues for the sociology of language and education and for social research more generally. How do we know “real” reality and recognize it when we have (or have not) found it? Is finding actual reality (and all of it) the sine qua non of research methodology and of the researcher’s craft? This query touches upon an old and painful dispute that extends far beyond the boundary of the sociology of language and education.

### ***Verstehende Versus Erklärende Sciences and Their Corresponding Methods***

The time has come, as it ultimately does in all social science that maintains a links with the most distinguished thinkers of its own past glories, for a few German words. More than 150 years ago, beginning even before the Bismarckian unification of Germany in the mid-nineteenth century and accelerating significantly thereafter, both the physical and the social sciences were essentially German preserves. It was not until the rise of Nazism, approximately 75 years ago, that this leadership clearly passed to the Anglo-American orbit where it largely remains until this day. Accordingly, it is not merely a silly nuisance that the “human,” “mental,” or “cultural” sciences retain a few particularly apt German terms to this very day. *Gestalt*, *zeitgeist*, *wissenschaft*, *volksprache*, *gemeinschaft/gesellschaft*, and *ausbau/abstand* are among those that it would be a pity to give up, because for several generations, many of our theories and findings have been formulated with them and through them, even if (as happens in all scientific fields) most of these have become substantially modified or even eliminated during this same period. These terms remind us of where we have been intellectually, and unless we know where we have been, we cannot really appreciate how we have gotten where we are (or think we are) today and where we would like to be tomorrow. Among these are the two terms that I will introduce here, *verstehende* and *erklärende*, that represent two kinds of conceptual goals and methodological procedures for scientific research.

*SLE as an Erklärende Science.* One school of German social science thought firmly believed that the rigorous methods and refined quantification of the exact sciences were not only proper and desirable but crucial models and methods for the



social sciences to aspire and work toward. The goal of such sciences was *erklaerung*: explanation. Today, when we think of the “explanation” of any variable in human behavior, it is exactly its variation or variability that challenges us. Why does it wax and wane in the same human subjects on different occasions and why do two different human groups differ with respect to the human behavior being examined such that Group A stands higher than Group B on some occasions while the opposite is true on others? Since this is not the case with respect to the measurement of properties of inanimate objects, the discovery of constant and inescapable variation in human behavior was originally a matter of great anxiety (not to say consternation) among scholars in the latter area.

In the beginning, the variation noted was attributed to errors of measurement, laxity in the training of measurers, or lack of consensus as to the proper units of measurement for research on human behavior. The psychophysicist Gustav T. Fechner (1801–1877) was so distraught by this phenomenon of inescapable human variation, individually or in groups, and by his obvious failures in trying to overcome or remedy it via utilizing different units of measurement, different methods of measurement, or different methods of training measurers that he ultimately went mad due to the aggravation and humiliation that he anticipated in that connection and that he understood to be a result of his own scientific shortcomings. It was almost a century later before the social and behavioral sciences fully understood that it was precisely the study and explanation of this variation that constituted their major responsibility and analytic task. From then onward, a large proportion of social scientists began to differentiate between true variance and error variance and between expected (and, therefore, insignificant) variance and unusual (and therefore significant) variance. The “standard error of measurement” of any measure being employed enabled investigators to distinguish between normal variation and clearly unusual variation and to focus their explanatory efforts on the latter. Such clearly unusual variation from the expected might be attributable to “independent variables” that the experimenter per se either manipulated or that the researcher discovered to exist to different degrees “in the field,” so that their impact on the “dependent variable” could be studied both separately and together.

Briefly put, “explanatory (*erklaerende*)” research is so named because it attempts to *explain* the degree of variation in the dependent variable in terms of degrees of variation (whether experimenter manipulated or field encountered) in the independent variable(s) under study. Such research can also inform its practitioners and advocates of the extent to which the total variance in the dependent variable still remains unexplained (unaccounted for) by the independent variable(s) under study and by the measuring devices employed. This is important because it enables investigators to realize whether explanatory progress is being made, over time and study after study, when focusing upon the same independent variable. Even if this *is* the case, then in the future the recording, observing and stimulus conveying instruments can still be improved and honed. If it is *not* the case, then it might be appropriate to start all over again, not only with instrumentation but with the formulation of underlying hypotheses, predictive (“independent”) variables, and the “unitization” (units of measurement) established for both.

Rigorous though the above-sketched approach may seem, it still has its skeptics and nonbelievers, primarily because it has been overpromised and has, inevitably, underachieved in explanatory power. Therefore, we now turn, in closing, to the *verstehende* approach to research in the sociology of language and education.

## **Verstehende Research to the Rescue**

Even its most adamant defenders must grant that the *erklärende* model in social research has not turned out to be as fruitful as originally expected while, at the same time, natural or physical science research utilizing this very approach has gone on from one success to another, in one substantive area after another. As a result, the social sciences have remained, in the eyes of many of its most prominent investigators, singularly unreformed and unenriched by the adoption of the rigorous *erklärende* research model. Accordingly, many researchers (particularly including many of the young and female among them) have returned to the previously much maligned *verstehende* model (carefully avoiding or sidestepping the Fechnerian error in connection with behavioral variance). The renewed *verstehende* model is anything but “apologetic” concerning any possible errors of the past (certainly not for that part of the past for which its practitioners assume no responsibility whatsoever).

Why, its critics ask, has the *erklärende* model failed to produce satisfying results? Because the complexity of human behavior is so great and so manifold that *erklärung* in neutral and precise measurement terms is essentially impossible with respect to it. Instead of the false god of *erklärung* (explanation), it is claimed that the human sciences should pursue the more limited but also perhaps the more appropriate model of *verstehen* (understanding). *Verstehen* does not assume a physical/natural sciences model. Quite the contrary, it proceeds on the basis of seeking a disciplined and careful human understanding, that is, the understanding of human behavior that only another human being can achieve, derived from observation and empathy. *Verstehen* does not pursue the explanation of variance but, rather, the grasping of holistic and “undessicated” behavioral phenomena, at the very level as do most adults who are native co-members of the same culture. Cultural understanding is and should be the proper goal of *verstehende* science, being the only goal that is distinctly appropriate for research on human subjects. Adult-child interaction that socializes infants into panhuman but also into distinctly Xian culture and teacher-pupil interaction also guides neophytes into panhuman but also into distinctly Xian school-learning culture. It also renders possible the recording and the analysis of the exact language use and behavior of any such interactions, something that *erklärende* research has well-nigh abandoned at the verbatim level.

This is a level of research involvement (problem definition, data collection and processing, and conclusion derivation) which is so different from that of *erklärende* research that the two often have very little to share with each other. When basic methods are very far apart from each other, then research traditions unfortunately become soliloquies rather than confederates in a common venture. Regrettable

though that may be for the pursuit of knowledge within the total enterprise of the SLE, most find it to be preferable to the constant skirmishing and mutually recriminating rejection that would and once did result from forcing incommensurables to interact and collaborate.

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## Future Directions

The total research enterprise of SLE must be inclusive enough and supportive enough to provide room and recognition for both *erklärende* and *verstehende* approaches to its subject matter. The rift between these different approaches is sufficiently recent that few researchers have thus far even had the opportunity to attempt to be trained so as to be equally at home and equally proficient in both approaches, so as to be able to choose between them (or to combine them) on substantive grounds rather than on personal, emotional ones. Perhaps that outcome will be a by-product of the twenty-first century that stretches immediately ahead. *Ojalá!*

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## Cross-References

- ▶ [Censuses and Large-Scale Surveys in Language Research](#)
- ▶ [Ethnography and Language Education](#)
- ▶ [Ethnography of Language Policy](#)
- ▶ [Sociology of Language and Education: Empirical and Global Perspectives](#)

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