

Essays in the History of Linguistic Anthropology

Dell H. Hymes

JOHN BENJAMINS PUBLISHING COMPANY

ESSAYS

**AMSTERDAM STUDIES IN THE THEORY AND
HISTORY OF LINGUISTIC SCIENCE**

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Volume 25

Dell H. Hymes

Essays in the History of Linguistic Anthropology

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OF
LINGUISTIC ANTHROPOLOGY

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JOHN BENJAMINS PUBLISHING COMPANY
AMSTERDAM/PHILADELPHIA

1983

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ISSN 0304 0720 / ISBN 90 272 4507 X

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TO
CARL VOEGELIN
who provoked it all

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INTRODUCTION

There is not such a thing as sustained study of the history of linguistic anthropology, and indeed, there is hardly a broad conviction that such a topic exists to be studied historically. The history of linguistics has developed substantially as a scholarly enterprise in the past decade or two, not least due to the efforts that have created the series in which this volume appears. The history of anthropology has serious practitioners, qualified historians, now. The intersection of the two has a history, but not historians.

Clearly I am not myself properly a historian of science, but a scholar whose interests in linguistics and anthropology have a historical dimension. The genres represented in this volume show as much. There is the essay on a specific subject, such as the use of linguistic method in ethnography, and lexicostatistics and glottochronology; there is the obituary of a valued predecessor (Kroeber, Swadesh); reflections on a chosen field (the Americanist tradition, American anthropological linguistics). Only the first and last essays may be said to be essays for the sake of history itself, the first to call attention to a missing dimension of a chosen field, the last to address conceptions of history that play a part in current debate.

Parts of these essays may be of use to historians as such, inasmuch as they involve persons and activities that are part of larger currents. Comte de Murville and Broca, for example, in chapter 2; the relation of American scholarship to the American Indian (ch. 3); interdisciplinary influence and alignment, and the methods proper to the human sciences (ch. 4); the distinct perspective that analysis of a personal career brings to one's understanding of the institutionalization of new disciplines (chs. 5, 6); international influence and alignment (ch. 7). I venture to hope that the discussion of Kuhn's concept of paradigm (ch. 8) will be of interest to historians of science and ideas, as well as to linguists affected by its ideological use.

Acknowledgement and Contexts

It seems quite appropriately historiographic to embed the sources of the studies brought together in the present volume in accounts of the contexts within which they arose. For each *chapter*, then, there follows citation of its

reference to its original publication and thanks for permission to reprint it, together with other thanks, recollections of the context of origin, and occasionally comments on the relation of that context to the present day.

Chapter 1, "Notes towards a History of Linguistic Anthropology", was prepared for a conference on the history of anthropology sponsored by the Social Science Research Council (SSRC) in 1962. It was memorable personally for my first meeting with Raymond Firth, who wanted to know first of all how well Boas had known and used Kwakiutl and for George Stocking's rebuttal of the categorical attack on Boas by Leslie White. Overall, the conference reflected the first rumblings of sustained serious work on the history of anthropology in the United States, and, the emerging shift of George W. Stocking, Jr. from the study of American social history to the study of the history of anthropology, American and British. I am glad to have been Stocking's colleague at Berkeley at that time, and to have been able to play some part in facilitating his involvement with anthropology.

The present paper is somewhat revised from the published paper, through additional marking of its divisions and contents, and a few appended comments (marked with asterisks). The published version was itself somewhat revised from an original prepared for the Conference on the History of Anthropology, sponsored by the Social Science Research Council, and held in the offices of the Council on April 13-14, 1962. I am indebted to C. F. Voegelin, one of the organizers of the SSRC Conference, as well as of the symposium in which the paper first appeared, and to Florence M. Voegelin, also a participant in the SSRC conference, for the opportunity first to give these notes and reflections a wider circulation. The entire issue of *Anthropological Linguistics* (5:1 (January 1963) in which the paper first appeared was devoted to "History of Linguistics. A Symposium Presented at the 1962 Meetings of the American Anthropological Association", and contained papers by Hoenigswald, Voegelin and Voegelin, Hodge, and Wonderly and Nida, as well as myself, I am also indebted to W. W. Elmendorff, John Freeman, and especially John H. Rowe, for several improvements on the original draft.

My own observations on the SSRC conference were given as a talk to the Kroeber Anthropological Society, published in the *Kroeber Anthropological Society Papers* (KASP) 25.81-88, (1962) and condensed as a report on the conference for SSRC under the heading, "On Studying Anthropology", *Items* 16:3.25-27 (1962). The longer KASP paper was reprinted by Regna Darnell in her *Readings in the History of Anthropology* (New York: Harper & Row,

1974), a volume we planned together initially with Walter Lippincott, then still at Harper & Row.

"Notes towards a History of Linguistic Anthropology" served also as my pre-conference contribution to a symposium sponsored by the Wenner-Gren Foundation for Anthropological Research at Burg Wartenstein, Austria, in 1964, and entitled "Revolution and continuity in the study of language" (see report in *Current Anthropology* 7.492-93 (1966). That symposium led eventually to the book from which the last paper in this collection is taken.

Clearly the examples in this chapter reflect the horizon of the early 1960s, when few general books on the history of linguistics existed, and none at all on the history of linguistics in relation to anthropology. The great upsurge of excellent research in the history of linguistics in the last decade or so had not yet become visible. The general conception of the paper seems to me still useful, and to sketch a variety of interesting lines of research that remain to be taken up. The dated character of many of the references may be useful too, documenting a moment in the history of linguistics for that history's own history of itself. The recent past of American linguistic anthropology, indeed, sometimes seems so remote from general consciousness as not to have existed. It is commonplace in linguistics, semiotics, and anthropology itself to ignore it or to get it wrong, reading it simplistically and ideologically (cf. Hymes & Fought 1975, 1981; Hymes 1977). Yet issues of current concern were debated and developed in ways that enlarge our understanding of what is conceptually characteristic, likely, possible.

I am grateful to Florence M. Voegelin, editor of *Anthropological Linguistics*, for publishing the paper (vol. 5 no.1 (1963), pp. 59-103) and for agreeing to its republishing here.

Chapter 2, "Lexicostatistics and Glottochronology in the Nineteenth Century (with notes toward a general history)" might be titled "Broca's Discovery of Glottochronology" or "Paul Broca, linguistic anthropologist". The stimulus to the study arose from the chance of browsing in the stacks of the library associated with the Department of Anthropology of the University of California at Berkeley, while I was on its faculty, and finding the address on linguistics and anthropology (1862) by Broca which is the center of the essay. An earlier title was "Lexicostatistics and glottochronology in Paris (1834, 1862)", the two dates calling attention to the reference points in the work of Jules Dumont d'Urville (1790-1842) and Broca, (1824-80) respectively. Around these points grew bits of information accumulated for a history and

updated survey of lexicostatistics and glottochronology (now abandoned). Certain possible recurrent historical patterns emerged, together with additional depth and insight into the sources of what has crystallized in our century into a controversial method. Hence the more general title of the published study.

It seems unlikely that the general history to which the chapter points will be accomplished, so long as the character structure of historical linguistics remains as it is. Whereas in other areas of research one has to make the case for the qualitative aspect of anthropological inquiry, against an entrenched quantitative mode, the opposite is the case in linguistics. Numbers are suspect, if not anathema. And those who choose the subject seems constitutionally incapable of regarding its foundations as an object of empirical study, as broached by Greenberg and Swadesh, and taken up by myself (Hymes 1959). The field is seen as divided between rash exploration and rigorous adherence to an undoubted testament of correct and cautious methodology (even though the fixed testament seems to have to be rewritten from time to time). Methodology is a sphere of faith and catechism, not of inquiry.

Lexicostatistics and glottochronology, of course, are from the beginning approaches which depend on empirical foundations, specifically, the study of known cases of relationship as a basis for extrapolation to cases whose relationship, or detailed history, is not known. It remains of interest to those to whom estimates of dates are meaningful, such as archaeologists and ethnohistorians. American Indian linguistics, and the linguistics of other regions of the world of special concern to archaeologists and ethnohistorians, could readily strengthen their roots in anthropology by pursuing the foundations and application of lexicostatistics and glottochronology, receiving recognition of relevance in return that might be valuable. This is unlikely to happen. Should it happen, the history sketched in this paper may come to be more fully explored.

One can also hope that recurrent attention to Broca as a figure in the history of neurolinguistics and the like (cf. William Orr Dingwall's report in *Historiographia Linguistica* 8(1) (1981)) will someday broaden to recapture his place as a figure in the intellectual and institutional history of anthropology as a whole, and therefore, someone necessarily concerned with the place of the study of language.

I am grateful to Isidore Dyen for his patience and encouragement for the completion of the full study, and for his agreement that it be reprinted here, from his book *Lexicostatistics in Genetic Linguistics* (*Proceedings of the Yale*

Conference, Yale University, April 3-4, 1971), pp. 122-76 (The Hague: Mouton, 1973). I was sorry not to have been able to be present at the conference, but glad to be able to contribute. I am grateful to Mouton & Company also for permission to reprint the paper. It should be considered together with the essay on Morris Swadesh (Chapter 6 below).

Chapter 3, "The Americanist tradition", is reprinted with permission from the volume, *American Indian Languages and American Indian Linguistics (The Second Golden Anniversary Symposium of the Linguistic Society of America)*, ed. by Wallace L. Chafe (Lisse: The Peter de Ridder Press, 1976), pp. 11-33. I am grateful to Chafe and the committee which organized the Golden Anniversary, including Einar Haugen as chair, for inviting me to prepare the paper and to present it at Berkeley, where the symposium was held in November 1974. I remember as well, perhaps irrelevantly, flying from San Francisco to New York on the following Sunday with just enough time to reach the City Opera production of Frederick Delius' "A Village Romeo and Juliet". The analogy that may lie behind the coincidence is that Delius' finest opera is gradually gaining the steady attention it deserves, aided by growing recognition that it is its own kind of opera, and not some other kind, and aided in the magical Corsaro production in New York by technical devices not available to Delius when he composed it early in this century. This chapter seems to me to contain an original argument, distinct from what is usually said about Amerindian linguistics. It argues that American Indian languages have usually been studied for some specific purpose beyond themselves, and that sustained, integrated presentation of the materials of the language has seldom occurred. The fundamental philology of the languages in their own right has barely begun. And one can add now that such study requires technical devices not invented when the bulk of the published work was done, devices of analysis into verse and line, devices of typographic presentation (including the invention of capital letters for the literary presentation of certain sounds in the text (cf. Hymes 1981)).

I have to say that I was greatly disappointed with the discussion following my paper at the Symposium. The original was not even ridiculed, let alone attacked or debated. Rather, attention was immediately turned to the diffusion of American Indian linguistic traditions of work to other places in the world. My hope is that we are readier now to analyze the history of those traditions themselves, and draw the difficult lessons from the history.

In preparation for the symposium, and the writing of the paper, I reread

each instance of a similar paper that I could find, papers on the general study of American Indian languages by Brinton, Boas, Sapir, and Hoijer. I saw (and see) this paper as aligning those papers as its tradition. Immersing myself in a variety of materials from the 18th century onward, I excerpted quotations of special pertinence or curiosity, and strung them together in a mimeographed collection, "Some quotations from the tradition", copies of which were distributed at the symposium and is briefly indicated here in an appendix to the chapter. The string of quotations is without context or analysis, but shows that the history of linguistics, the history of anthropology, the history of American society, could each be written in terms of what was said and could be said about American Indian languages. Such a history would be partial, full of gaps, yet its jagged contours would say much.

Chapter 4, "Linguistic method in ethnography", is somewhat revised here from its original publication in *Method and theory in Linguistics*, ed. by Paul L. Garvin (The Hague: Mouton, 1970), pp. 249-311. I am grateful to Paul Garvin and to Mouton for permission to reprint the essay.

Revision has taken two main forms: some polishing and amplification of the beginning paragraphs; addition of a discussion of the essays in memory of Sapir in much greater detail.

The additional material about the essays in memory of Sapir was written in the same period as the full article, but too late to be included in the published volume. The article, as published, was completed in England, where I had the good fortune to be a fellow of Clare Hall at Cambridge University in 1968-69, and to benefit from a Guggenheim Fellowship in the first half of 1969. On return to Philadelphia my family and I stayed for a few days in the house of our friend and then colleague, J. David Sapir. The opportunity to delve into some of the books of his father, Edward, in David's study, and to ponder the memorial volume more carefully, led to additional insight and writing. As said, those pages of 1969 were too late for the book; they see print for the first time here.

For time I considered a third major revision, namely, omission of the discussion of several writers and publications current at the time (1968-69). I do apologize for any unfairness or arbitrariness in singling out certain names and pieces, but have decided to retain the section. One reason is that it does document the 'structure of feeling' of the time (to use a phrase of Raymond Williams). A second reason is that the kinds of attitudes persist. Readers of this book may well think of analogies to current concern with 'structuralism' and 'semiotics'. A third reason is that the several kinds of attitudes from a neces-

sary background to the argument of the last section of the essay, on "Speaking", and to the epilogue.

If the essay were to be brought up to date, by a review of the decade of the 1970s, a balance would have to be struck between optimism and depression. It ought to make one optimistic that linguistics, and many other disciplines, at least have come to jostle one another under the heading of 'discourse'. Studies of discourse, speech acts, conversation, pragmatics, abound. The natural and necessary arena of an integration of linguistics and anthropology is crowded with investigators. The depressing fact, however, is that very little of the work is grounded in ethnographic inquiry. The need for such grounding is not even recognized in most quarters. As I write, I have just rejected for publication an article addressing the difficulties of children from the newer sectors of Israeli society and finding part of the explanation in differences in the ways in which the children respond to adult questioning. The well-meaning author of the article introduces the data with an exposition of the four maxims of the philosopher Grice, as if these maxims were known to be universally valid, and thereby available for the explanation of differences between children from different cultures in Israel. The fact of the matter, of course, is that no such thing is known. There indeed is evidence to the contrary. It may be that every community must have some orientation to the four aspects of verbal interaction singled out by Grice. It may be that the study of any group should seek to discover how members of that community deal with (a) sincerity and truthfulness (maxim of quality): (b) taciturnity and verboseness (maxim of quantity—saying less of more than required): (c) relevance (maxim of relevance); (d) clarity, ambiguity, obscurity (maxim of manner). What do they expect, value, find necessary, enjoy in these regards in using speech with each other? Practices may vary considerably from place to place, group to group.¹

1) And if a group considers it dangerous to share information, who has given us the right to tell it that it is 'violating' a universal maxim? Nor does it make sense to say that talk of 'violating' a maxim is merely descriptive. The metaphor may influence what one thinks and sees. And to use it is rather like saying that a woodwind quintet is to be analyzed as a violation of a maxim, 'write for strings'. For cultures chary of conveying information, violation of course would be to fail to be chary. Nor is it more than a desperate move to say that one could interpret the maxim to fit, that it is simply a question of what the group in question understand as saying less than is required. For the foundation of the Grice maxims is the idea of cooperation, and the maxim of relevance is founded on the assumption of saying as much as, not more than, the hearer requires. Whereas in a good many peasant societies, the foundation of prudent speech is to say no more than, and only as much as, is good for the *speaker*. One might get from Grice to the peasants technically, descriptively, in some formal account, but the cost would be to make nonsense of the culture being described. A violation in Grice's terms would not be marked but normal: to observe Grice's maxim would be abnormal.

In sum, the maxims may point to dimensions that are universal, just as there are universal dimensions in kinship, phonology, and grammatical categories. Any particular group, may ground its conduct in one or another view of what is to be done with regard to the universal dimensions. Cooperation is not solidarity; it may come in typical degrees. And it takes ethnography to find out. Participant observation, openness to the point of view of those studies, descriptive categories grounded in what counts as an instance in the case in question such essentials are no more than the birth right of a tradition of work that looks back to Boas and Sapir.

Two valuable developments of the past decade have been in the study of language acquisition and of oral literature. The study of language acquisition has recapitulated the development of linguistics proper, from phonology and morphology to syntax, semantics and pragmatics, and it has attracted a number of fine researchers who are attentive to social context and cultural difference. The study of oral literature has begun to find language and culture inseparable in the analysis of American Indian texts. The kind of transformational relationship discovered by Levi-Strauss is indeed a fundamental discovery. It extends to myth the realization that things can be connected by systematic opposition as well as by cumulative similarity. But the work of Levi-Strauss, and of many 'structural' models of narrative, ignores the actual mode of existence of narrative in words and voices. New discoveries of the organization of American Indian texts in lines, and groups of lines, open up the need for reanalysis of all the established materials, and open up insight into a use of language that is the tacit instillation of cultural logic of experience and action (see Tedlock 1972, 1982; Hymes 1981, 1983). The discoveries within texts are associated with a growing attention to the performance of texts, and other folkloristic genres, that contributes much to the ethnography of communication (cf. Gossen 1974, Sherzer 1982).

The use of conversational maxims and postulates, such as those of Grice, Lakoff, Levinson and Brown goes together with the limitation of data, order and intelligibility to interactive events and their transcripts. Both have an understandable source in belief that what has been explained in the past in terms of cultural tradition and social norm should rather be accounted for in terms of rational processing in the course of action. There is an essential truth in this view, though not an entire truth. It fits a climate of modernity and alienation, in which one's identity is experienced as a multiplicity of choices, rather than as something largely ascribed. Its plausibility is greater in middle class academic and professional life in the United States than in "traditional"

societies. Inasmuch as adherents of the view are committed to close analysis of real data, one can hope that reality will educate them. Insofar as they find themselves interested in the data for its own sake, the data is likely to lead them willy-nilly into a rediscovery of the relevance of culture, history, and norm.

What is most discouraging from the standpoint of the argument of this essay, however, is the extent to which anthropology itself appears to be abandoning linguistics. What it expects to have left as a claim on a distinctive place in the academic scheme of things is not clear. Archaeology and physical anthropology, of course. But social and cultural anthropology are likely to go the way of classical studies, to become honored specialists (in small departments) if they let go of language, and have to compete solely on the basis of being cross-cultural. It is reported that 90 % of the cross-cultural research now being done is done by other than anthropologists. The turn of many anthropologists to phenomena of our own society could be beneficial to all concerned, but a major advantage that anthropologists might have over sociologists, psychologists and others would be the ability to transcribe and interpret the meanings embodied in recurrent verbal patterns. Yet many departments of anthropology seem not to provide linguistic training anymore, or to expect of field workers that they be able to use what people say as data. The great growth and success of linguistics appears to have encouraged a tendency to leave language to linguists. Linguists, of course, have often encouraged such a tendency, and temptation must be all the greater now, when linguists discuss speech acts and conversation. Yet anthropologists have reasons of their own for knowing speech and language. To abandon the principle of using the local language, is to abandon the essential standard of humanistic research -- who would credit a specialist in French culture who did not know French? And it is to abandon a competitive advantage over social scientists who work through interpreters. Language and linguistics are the anthropological answer to the methodological hegemony of surveys and statistics. There is no lasting future for a profession of humanists who can't handle the languages of the cultures they interpret, and social scientists whose methodology is a weak version of what sociologists and political scientists do better.

It may be that the linguistic ethnography of the future will have been developed by an odd coupling of adventurous linguists, folklorists and sociologists, cultural anthropology having ceased to be its home. That would be a striking chapter in the story which this study begins to tell.

Such a chapter would be the less surprising, because the world of general

culture, the *New York Review of Books*, and semiotics has already forgotten, it seems, the initial chapter. Structuralism is discussed without reference to the initial concern with the relation of linguistic method to culture in the United States, a concern that involved Levi-Strauss himself. The notion of linguistic structure is imputed exclusively to de Saussure, although it had been reached independently and apparently at a somewhat earlier date in the linguistic anthropology of the United States, most evidently in the dissertation of Sapir (completed in 1908). In the United States the Chomskyan school projected its own ideological myth of previous American linguistics, pretty effectively obliterating continuity and consciousness of what had preceded (cf. Hymes and Fought 1975, 1981). Those who come to these questions from literature and cultural studies lack any acquaintance with the American history. It is unlikely that my own study here will affect the simple-minded picture of de Saussure, 'American structuralism', and Chomsky that prevails, but it does at least make it possible for some to know better. And a curious cultural historian may find the impression of recurrent tensions and cycles in the relations between linguistics and anthropology across the generations of general interest.

Chapter 5, "Alfred Louis Kroeber: Linguistic anthropologist", is reprinted with permission from *Language* 37:1 1-28 (1961). It had been reprinted with omission of its last paragraph in *Language in Culture and Society*, ed. by D. Hymes (New York: Harper & Row, 1964), pp. 697-707, because of the wishes of the publisher's editor, and in the same form in *Portraits of Linguistics*, ed. by T. A. Sebeok (Bloomington: Indiana University Press, 1966), vol. 2, pp.400-437. The descriptive subtitle, 'linguistic anthropologist', has been added here at the suggestion of Konrad Koerner. It identifies the most important reason for a study of Kroeber here. He was a 'linguistic anthropologist' in the fundamental sense, an anthropologist who had anthropological reasons for the study of language.

Kroeber (1876-1960) is perhaps not widely known to those who have entered anthropology since his death some twenty years ago, but in the 1950s he was the living embodiment of American anthropological tradition to many. His own work had embraced aspects of all its established fields, he had studied with the founder of the modern profession, Franz Boas, (1858-1942) and had himself founded a major department at Berkeley. He had intellect, wit and style. In the years in which I was at Harvard (1955-60), as the first linguist to be hired in the Department of Social Relations, I came to know Kroeber partly from having been a student of Harry Hoiyer (the senior active linguistic an-

thropologist at the time), and partly from becoming a sort of a protégé of Clyde Kluckhohn at Harvard (to whom Hoijer had recommended me). Kluckhohn admired Kroeber, and the two joined in an interest in linguistics and conviction as to its importance to anthropology. In the spring 1960, when the question of a future for me at Harvard or at Berkeley was being decided, I sensed something of a friendly competition between the two older men. In the event I came to Berkeley, but both Kluckhohn and Kroeber were dead, Clyde in early summer from a heart attack while canoeing in the south-west, Kroeber in a hotel on the Quai de Voltaire suddenly in Paris, in early September, a few days after the conference on “Anthropological Horizons” that he had organized at Burg Wartenstein, (and to which he had invited me, Levi-Strauss, and others he enjoyed as ‘mavericks’). My world was suddenly devoid of institutional fathers.

This essay builds in part on what I said at a memorial service for Kroeber at Berkeley that fall, but much of it involves subsequent surprise. I had not known the extent or the configuration of Kroeber’s involvement with the study of language. The discovery was engrossing to me, and to the first reader of the result, Bernard Bloch, (1907-1965) who as editor of *Language* had honored me by asking me to write an obituary, and whose series of postcards, as he read his way through the outcome, I treasure still.

The essay is the first in date of the chapters on this volume, and perhaps represents best the respect in which my concern with the history of linguistic anthropology has been to recover a missing tradition. Such a concern is hermeneutic, I know; one constitutes as well as finds. It is a kind of concern that has been central to my own work, and that I cannot help but believe would be valuable to many. Having come to terms with Boas, Kroeber, Sapir, and some others in a certain period, I find my concern directed now not to them but to work that builds on from them. I cannot but believe that anthropology would be healthier if there were more such coming to terms and building. The relevant past of our field is not a known terrain to which one can be given a packaged tour. Each scholar is a personal vantage point, and so the terrain must be explored afresh by each, who must make his or her own map, find his or her own way, inheritance and epiphanies.

Chapter 6, “Morris Swadesh: From First Yale School to World Prehistory”, is reprinted with permission from *The Origin and Diversification of Languages*, composed of an original manuscript and related articles, written by Swadesh, and edited by Joel F. Sherzer (Chicago: Aldine, 1971). My contribution is divided between a foreword (pp.v-x), which discusses evolution of

language, the study of Swadesh's work and career, pp. 228-70, which is reprinted here with slight revisions, and a complete bibliography of Swadesh (pp.324-36).

The study was revised and enlarged from "Mauricio Swadesh (1909-1967)", *Anales de Antropologia* 5.212-25 (April, 1968), which has also been published as "Morris Swadesh" in *Word* 26:1.199-38 (1973 for April 1970). The appearance of the posthumous book was made possible by the cooperation and dedication of Evangelina Arana Swadesh, Alexander Morin of Aldine Press, and Joel Sherzer, friend of both Swadesh and myself.

Swadesh's career sheds light on those distinct and vital periods of linguistics and anthropology in the United States, both in what he did and in the response to him. Response to him can still elicit controversy, as shown in Robert Hall's book (1975), my review of the book (Hymes 1980), and Hall's privately published rejoinder (Hall 1981). A rounded history of the periods, before, during and after the Second World War, will shed additional light on Swadesh, and on a series of complex interplayings of personality, climate of opinion, and patterns of scholarship. The book from which this essay is taken is now out of print, and I hope that its availability here will stimulate the additional work that is needed for such history. Most of all, we still have no full account of the Yale school around Sapir, and of the way in which the fabric of linguistic anthropology in the 1940s and 1950s came to depend upon personal relationships established in the 1930s in and around Sapir at Chicago and Yale.

A recreation of the full climate of opinion will shed a great deal of light on a wide range of things: the central role of the category of aspect in Whorf's writings on Hopi, the origins of the standard Americanist orthography of the present day, the strange eclipse by the Second World War of directions in typology, phonology, social psychology of language, that seemed to take a generation to renew after that war.

Chapter 7, "The Pre-war Prague School and Post-war American Anthropological Linguistics" is reprinted with permission from *The Transformational-Generative Paradigm and Modern Linguistic Theory*, ed. by E.F.K. Koerner (Amsterdam: John Benjamins, 1975), pp.359-80. The essay was written in late 1970 at the invitation of colleagues in Prague, and intended for a volume to be published there, but in the event could not be.

My sense of intellectual affinity with the Prague School has been deepened by the chance to give a seminar there at the Oriental Institute in late August 1964, and the joyful recognition of common concerns between myself and František Daneš during our joint participation in the 'International Days

of Sociolinguistics' in Rome in September 1969. My paper for that conference was translated by Daneš for publication in a journal whose founders included founders of the *Travaux de la Cercle Linguistique de Prague* as well (Hymes 1970a, b).

The essay is a personal reflection as well as a reflection upon the tradition of a school, but I hope with some use in that regard. The prediction implicit in its characterization of Chomsky's place in the development of contemporary linguistics seems increasingly to be borne out.

Chapter 8, "Traditions and Paradigms", was written as the introduction to *Studies in the History of Linguistics: Traditions and Paradigms* (Bloomington: Indiana University Press, 1974), pp.1-38. (The book might itself have borne the title 'Traditions and Paradigms', but Hans Aarsleff refused to let his contribution appear if the title first proposed, 'Studies in the History of Linguistics', were so changed.

The book was the culmination of two successive conferences, one at Burg Wartenstein in 1964 (see remarks above on the first essay), and one in February 1968 at the Newberry Library. The latter conference (see my report (Hymes 1968)) became very much a forum for the vast knowledge and stimulating insight of Roman Jakobson, and it was long hoped that the book would have a comprehensive concluding essay by him. In Thorkild Jacobsen's phrase, it would begin with Jakobsen and end with Jakobson. That did not prove possible, and my own essay was written as an attempt to meet the need for an integrating section. In the event it was about twelve years from the conception of the first Wenner-Gren supported conference to the appearance of the book.

The essay has been edited to fit its new context here, mainly through changing the manner of referring to chapters of the original book, the original volume now being out of print. (All those chapters are now cited in the references of chapter 8 itself). The value of the essay in this new context, I think, is twofold: as an illustration of the temporal depth and scope of the aspects of the history of study of language in which linguistics and anthropology can jointly take interest, and in its attempt to provide a frame of reference for thinking about the history of sciences and disciplines that does rough justice to the original intention of Kuhn's notion of 'paradigm', while avoiding the vulgarization and ideological manipulation to which it has been made subject. The heart of the argument can be said to be that one must not confuse 'paradigm' with 'cynosure' or 'center of the stage', if one is to reach understanding rather than perpetuate illusion. Anthropologists interested in language, and linguists in-

terested in anthropology, have a stake in a conception of the history of linguistics that is adequate to the full field of forces and interests at work. Much of our own history goes on out of the spotlight of the moment, yet its story is a part of anthropology itself, with bearing on the relation of language to culture. It is a part of what one civilization, our own, has made of language.

I am indebted to Konrad Koerner for editorial advice, including helpful comments on this introduction, and indeed for the opportunity to bring together these studies, each written independently, yet having a certain perspective in common. That perspective, I should like to think, is appropriately anthropological (and sociological), and tries to do justice to the multiplicity of factors that interact to shape a subject. There is something here of climate of opinion and long-term intellectual trends, but of personality as well; something of social structure and institutionalization, but also, of national character; and respect for the ineluctable degree of autonomy that specific means (materials, methods) confer on any persistent scholarly tradition. My hope for this volume is that it will be useful to those who do have an interest in the persistent tradition to which it is addressed, and even that it may stimulate sustained attention and research. Such attention does not seem to me to be antiquarian in the depreciating sense, but a contribution to the visibility and legitimacy of the tradition. Without a history, it has no future.

Finally, let me acknowledge the invaluable help of Josie Yanguas in preparing some of the bibliography and all of the indexes, and of Rosemary Klumpp and Donna Webb for the necessary typing of parts of the manuscript.

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NOTES TOWARD A HISTORY OF LINGUISTIC ANTHROPOLOGY

[Prefatory note: Time has permitted me no more than a tentative outline, a series of prolegomenous notes, for the study of the history of linguistic anthropology. The paper should be read as an informal, rather than finished, survey. Although the topics and illustrative examples come chiefly from my own experience, I hope that they have variety and significance sufficient to show the interest and profit of the subject. The focus on American work is due to my inadequate knowledge of the relations between linguistics and anthropology elsewhere. A few references to European work are made, but many gaps remain to be filled.]

I. Introduction

By linguistic anthropology I understand a field coordinate with the other major branches of anthropology in the Anglo-American sense of the term. The field can be briefly defined as the study of language and speech in the context of anthropology. The existence of such a field as a subject for historical study has an empirical, rather than logical, rationale.

For some, all linguistics is logically part of anthropology, while for others the two are coordinate. Similarly one may also view all study of language and speech as logically part of linguistics or as apportioned between linguistics and other fields, such as anthropology. On either view, there is no gap in the table of organization of the history of science, once the history of linguistics and the history of anthropology are registered. Yet if the writer of a history is a linguist not versed in anthropology, or an anthropologist not versed in linguistics, his product is likely to skimp the interests of the other. What writings exist are evidence enough.

In point of fact, there has been a study of language and speech, shaped by anthropological contexts, that has had a partly independent course, seen from the history of anthropology, because of its relations to anthropology. In short, anthropology and linguistics, as historically developing disciplines, have had partly separate roots and traditions. In particular settings and in general, the two disciplines have partly shared, partly differed in the nature of their materials, their favorite types of problem, the personalities of their dominant

figures, their relations with other disciplines and intellectual currents. The two disciplines have also varied in their interrelation with each other and the society about them. Institutional arrangements have reflected the varying degrees of kinship, kithship, and separation. Such relationships themselves form a topic that is central to a history of linguistic anthropology, yet marginal to a self-contained history of linguistics or anthropology as either would be conceived by most authors. Most of the rest of this paper will document further the existence of a subject-matter for a history of linguistic anthropology.

There exists, indeed, not only a subject matter for a history of linguistic anthropology, but also a definite need. To my mind, there is a general need in the current study of language for codification, articulation as well as exploration. From a humanistic viewpoint, such work might be seen as the reconstitution of a general philology. In strictly anthropological terms, such work might be seen as the framing of a provisional general theory of language and culture. In either case, the work of criticism and interpretation would have to draw for perspective equally as much on the history, or development, of the study of language as on a survey of current knowledge and research. History and systematics would be interdependent.

Reasons for this are familiar to students of intellectual history, and the combination seems often to have occurred (thus Butterfield (1957) on the link between interest in general history and the history of historiography). I mention the matter here out of a strong sense of its timeliness and importance for anthropology. To the degree that we have lacked an active knowledge of the history of our field, we have been limited by lack of some of the perspectives that have not been transmitted to us, and by the partialness of some of those that have. A critical history can help us regain the one and transcend the other. In my own work I have sometimes felt that progress in understanding was but the recapture of perspective that had been lost.

Certainly a case can be made for intellectual discontinuity in American linguistic anthropology during this century, such that some important work of preceding generations has become unintelligible, its meaning having to be recaptured by special study.* I say this not

*See discussion of Whorf and genetic relationship below. This 1962 statement now seems to me (in 1982) sadly prophetic of the eclipse of understanding of the actual character of descriptive linguistics and its role in anthropology. (Cf. Hymes 1976 and ch. 4 of this volume.)

out of overestimation of the worth of earlier work. Much of its content has been permanently superseded, and its neglect thus to some extent justified. But historical interpretation and critique of earlier periods has the two-edged value of a regaining and transcending (mentioned above), and I say this, not as an historian, but as a practitioner, of the field in question. I would identify the situation in this way. Our most recent, still continuing, period has been dominated by reaction against an earlier perspective considered too sweeping, too ambitious in scope, too weak in data and method. In outline caricature, the devolution from generalizations of bold scope has been first to drop the generalizations, and then the scope.* Very narrow definitions of linguistics, affecting anthropology, have come to the fore. By enabling us to put in full perspective many of our problems and assumptions, historical study will help change the situation in two respects. In some ways the consequence will be to depart in a much more thoroughgoing way from earlier work, since the departure will be not simply a contraction, but a fresh start. In other ways the consequence will be to renew earlier periods by renewing attention to problems posed in them. Ideally, the fresh start will harness the technical and empirical advances of the latest period to the broad sense of scope and relevance of its predecessors.

I have noted that the actual histories of anthropology and linguistics have been partly joining, partly distinct. That state of affairs is likely to endure, and hence the practical need for historical study of the intersection is likely to continue. A quasi-logical justification for such a state of affairs might be as follows:

- (a) linguistics has the task of coordinating knowledge about language from the viewpoint of language;
- (b) anthropology has the task of coordinating knowledge about language from the viewpoint of man.

*So it seemed in 1962. In 1982 one might say that the cycle has been repeated in the opposite direction. Reaction against an earlier perspective considered too narrow has led to restoration of generalizations and of the scope over which one was willing to generalize. Too often the wish to generalize has been without the sense of diversity accumulated by earlier generations. One or a few cases have been taken as sufficient; hypothetical models informed by our own culture, or a single class within it, have been taken for granted as applying to all peoples; a critical sense, unpacking proposals in the light of the possibilities of historical and situational diversity, has not been exercised.

While both tasks are essential and everything relevant to one may seem in some sense relevant to the other, in practice the hierarchies of relevance are likely to differ, resulting in an interplay that is one of the central topics of a history of the study of language from an anthropological point of view. (Observations similar to the preceding paragraphs may apply to other aspects of anthropology, e.g., John Rowe finds them applicable, mutatis mutandi, to archaeology).

II. Chronological development

Like the other disciplines concerned with humanity, linguistics and anthropology must consider their antecedents and possible continuity from the classical Mediterranean world. If a named, classical father for linguistic anthropology is desired, Herodotus can be claimed, for he paid attention to the apportionment of Ionian towns among four dialect groups, to evidence of linguistic acculturation between the Caunians and Carians, and to the affiliation of the Carian dialects, as part of his broaching of the ethnology of the world known to him.

The best definition of temporal scope is probably the widest, following Hallowell (1960) in his depiction of scientific anthropology as an emergent out of folk-anthropology, as having continuity with man's earliest efforts to obtain reliable knowledge about himself and his place in the universe. Within this broad scope we can distinguish, again broadly, three levels: folk-linguistics, national philology, and general linguistics.

The motives most frequently cited for the emergence of conscious attention to linguistic features can be generalized as recognition of difference or discrepancy. What sort of difference is recognized may vary; most often cited are difference, as between social varieties or levels within one speech community; as between distinct speech communities; as between the current state of a language and retained knowledge of an earlier stage--although these types are not exclusive, but may merge. It seems likely, however, that in any society at any time, simply differential individual competence, including most markedly, that between linguistic adults and acquiring children, may suffice to give rise to some explicit linguistic consciousness. In recent times, of course, one must add the effects of interaction between linguistic adults and inquiring fieldworkers (cf. my comments on Swadesh's paper in Discussion to the Symposium on Translation between Language and Culture, AL 2.2.81-84 (1960)).

The subject of folk-linguistics, as that of folk-anthropology has not been systematically pursued, and both need to be, for their importance as part of scientific ethnography. From either a diachronic or synchronic point of view, they provide the background against which to assess the achievements of the other levels, as well as some

indication of their origins. Contrary to the view sometimes held, that primitive peoples are wholly unconscious of linguistic phenomena, both as regards structure and norms of use, there already exists evidence to show that such consciousness exists. Some form of folk-linguistics is as universal as language. The metalinguistic function is a universal function. (see VII, (G)) It becomes an empirical matter to determine the varying degree in the given case. Gudschinsky (1959) reported a Soyaltepec Mazatec conception of phonemic tones (thick vs. thin), and Elmendorf and Suttles (1960) have put in social context a Halkomelm Salish recognition of the distinctive feature of nasality. Lowie (1935) has documented normative consciousness among the Crow (cf. observations in Hymes 1962c), and Bloomfield (1927) has devoted an article to some of the facts and theoretical consequences of its presence among the Menominee. But we lack systematic description of any folk system of terminology and belief about speech, let alone comparative studies.

Conceptions about the origin of language and languages show continuity from the simplest societies to some of our recent intellectual predecessors (cf. White's chapter on philology (1896) and Borst (1957-63)).

The origin and spread of writing systems should be examined. Invention or adaption of such systems is *prima facie* evidence of some form of structural analysis on the part of the inventors or adapters. (A striking example is the Brahmi script of India, as much in advance of contemporary scripts descriptively as Sanskrit grammar of Mediterranean grammar (John Rowe, p.c.).) Of special interest are the cases of stimulus diffusion to which Kroeber (1940) has called attention. No adequate survey of writing systems among peoples not of the major civilizations, at least from this viewpoint, has been made. (cf. Hymes 1962b)

For the most part, writing systems involve the second level of development of conscious attention to linguistic traits, that of the national philologies, and the two may seem necessarily related. The Greek tradition, however, so far as it was linked with, and motivated by, rhetorical education, might have been carried on without writing; and the Indic tradition, centering around preservation of a sacred language, was so carried on in the first instance, and continued to focus on memorization after writing was introduced into the society. Only in some cases, such as the Babylonian and Chinese (and the cases, to be mentioned again later, in which the national philology is developed by foreigners) can one say perhaps that writing was intrinsic to a philological tradition that could not have developed without it. It may be coincidental, or an indirect and mediated correlation, that we know no fully developed traditions of national philology in societies without writing. National philology represents a focusing and cultivation of a self-consciousness which might repeatedly arise, requiring not special materials (graphic)

but only special interest. It seems fair to say, however, that general linguistics could have arisen only in the context of historical developments beyond the scope of any one society, and to have required the accumulation in single centers in consultable (i.e., written) form of materials of diverse alien origins. (Pedersen (1931) has called attention to the role of printing.)

As to the study of the classic lines of national philology, Kroeber (1944) has a good introduction in his chapter on philology. The materials for the various major, or classic, lines--Indian, Chinese, Japanese, Korean, Arabic, Greek, Roman, and the like--are unfortunately difficult of access for the general student. Dispassionate individual studies would be of great value, especially if conceived so as to facilitate comparative study, either by way of paired comparisons (some such as Indian: Chinese would be fascinating) or across the board. Such points as the relative role played by type of linguistic structure, form of social organization, and cultural values in the formation of each philology could be assessed.

In the lines of national, or individual philology, both in the societies in which they emerge and in the societies where they may later spread and be maintained, the treatment of linguistic structure is closely linked with other fields of interest, such as logic, rhetoric, poetics, philosophy--in short, with the practical uses of language recognized and valued by the society. Modern histories seem to treat together at most some one or two of the fields (e.g., Steinthal joins classical linguistics and logic, Sandys joins classical linguistics and literary work). Mostly we get historical discussion of linguistics proper, or of logic, or of literary criticism, but not of linguistic study as a whole as conceived and conducted by the society itself. The separation is understandable and would be difficult to surmount, but unfortunate, not only for adequate understanding of the development of the philological traditions themselves, but also for the perspective of linguistics and anthropology today. (We need to develop the ethnographic description and comparative study of the complexes of logic, rhetoric, poetics, dialects, and the like, found in societies around the world, as part of a broadly functional approach to language.) Comprehensive historical studies would set an example, and serve as partial 'etic' guides, 'notes and queries' at least, for the ethnographic task.

The classic cases of national philology precede the emergence of general philology, but of course also persist after it, and the interaction, not always to the advantage of general philology, is of some interest. New national philologies continue to emerge after the emergence of general philology, moreover, sometimes motivated by it, sometimes on the model, if any, of other national philologies. One of the distinctive traits of the period after the emergence of general philology, indeed, is the development of many individual, or national, philologies, by strangers to the people and language studied

(e.g. Eskimo philology, as conducted by the Danes). The difference in provenience may reveal itself in difference with regard to normative attitude, sometimes as between presence and absence, sometimes as between the rating given the language studied. At one stage this is elementary philology or anthropological linguistics. At another stage an interesting interplay arises between indigenous and foreign practitioner concerned with the same subject-matter. Noteworthy and interesting in this connection are Whitney's arguments as to the autonomy, equality, and in some respects superiority of Western studies of Indic philology, vis-a-vis those conducted within the Indian scene and tradition (William Dwight Whitney (1867)).

General linguistics, or general philology, or scientific linguistics emerges about the same time as general, or scientific, anthropology, attendant upon certain changes in outlook and knowledge which have been described, although without much specific reference to the study of language, by historians of ideas. The essential criterion can be said to be a concern with all languages as having scientific relevance.

It would be difficult to draw a hard and fast line, demarcating a period as that of general linguistics. Some would point to the late eighteenth and early nineteenth century, and it is certainly true that interests come to a focus, and cumulation and continuity intensify about that time with the rise of Indo-European studies particularly. At the same time, any individual line of interest must be traced much further back (just as the history of the knowledge of any specific line of anthropological concern must be traced further back than the constellation Americans would identify as anthropology); and, as will be observed again later, much depends upon the scope and content one assigns to the concept, 'linguistics'. A good case can be made for beginning the story with the accession of diverse information about languages from the revival of knowledge of the classical Mediterranean world and from the increased contact of Europe and European-derived peoples with each other and the rest of the world. (J. R. Firth has sketched something of the effect of the Age of Discovery in The Tongues of Men (1937), and L. H. Gray (1939) gives an accounting of the growth of descriptive reports of non-European languages through several centuries.)

My colleague, John Rowe, has generously allowed me to quote here some of his observations on this score:

"The first step in the development of a general comparative linguistics, as distinct from philological cultivation of one or two languages, was probably the production of learners' grammars and dictionaries of spoken languages for the convenience of travellers,

missionaries, and administrators. This type of effort began just before 1500 (cf. Nebrija in Spain (1492)). Outside of Europe the production of these grammars and dictionaries was I believe exclusively a missionary concern until the late 18th century. It was an activity of Protestants (Roger Williams, John Eliot) as well as Catholics. The work varied considerably in quality. I do not know of any evidence that missionaries received any previous training in language analysis, other than what they might get from studying Latin, Greek, and sometimes Hebrew, nor do I know of any early interest in using the materials so recorded for any kind of systematic comparison. Furthermore, it does not seem to have occurred to philologists in Europe that the materials recorded by the missionaries might have any value for comparison with the languages they were studying (Greek, Latin, Hebrew, Arabic, and occasionally, for special purposes, Syriac, Armenian, and Ethiopian, in each of which there was some early church literature and translations from Greek), and speculations about "the language of Adam". The only comparison the philologists seem to have been doing was a little wild etymologizing now and then, in the tradition of Isidore of Seville. Nevertheless, materials for comparison were being accumulated.

"A necessary condition for this kind of activity was organized missionary work; at any rate, as noted, only missionaries did it. There had been an earlier wave of such missionary work, leading to the conversion of northern Europe and Christian penetration of much of Asia, but this earlier wave did not produce much in the way of grammars and dictionaries, as far as I know. It did lay the foundations for the development of a written literature in Slavic, German, Old English, and Old Norse, but the missionaries must have learned the languages orally and concerned themselves with writing only when scriptures or sermons needed to be recorded. If any grammatical observations were made, they did not become part of a cumulative stock because they were not written down.

"The Moslems had a missionary tradition too, in a sense, but it was a different sense from the Christian one and did not involve preaching in native languages on any systematic basis. As far as I know, Moslem missionary practice produced little in the way of language records.

"What was new around 1500 was that there was a lot more emphasis on the use of writing for communication among missionaries and in missionary training, and printing was available. A very large part of the missionary philological work found its way into print.

"There were two things going on in the late 18th century that seem to have had something to do with the development of comparative linguistics, in addition to the ideas of Herder (about the worth of each language). One was a general broadening of secular scholarly

interest, particularly in France. Around the middle of the century Barthelemy deciphered the Palmyrene and Phoenician scripts and gave two new languages to Near Eastern studies. There was a growing interest in Coptic as a possible clue to ancient Egyptian writing; and Freret worked out the principles of Chinese writing, thus opening Oriental literature to European scholars. The discovery of Sanskrit was part of this general broadening of horizons; perhaps Rask's interest in Old Norse was too, to some extent.

"The other development was the expulsion of the Jesuits from Bourbon dominions. The Jesuits from vast mission areas were suddenly shipped off to exile in the Papal States where there was nothing left for them to do. Many of them devoted the remainder of their lives to writing up notes on their mission experience, and many first rate studies resulted. The two most important projects of this kind were Gili's comparative work on South American languages, published in 1782, and Hervas' subsequent and more general comparison and classification of all the languages of America and then the world, published first in Italian in 1784 and then in Spanish in 1800-1805. Perhaps the missionary material would have been exploited anyway sooner or later for comparative purposes, but the idleness of exiled Jesuit scholars brought this about sooner rather than later."

III. Scope

Whatever the demarcation in terms of time, there still remain two crucial questions of demarcation in terms of scope, the dimensions of the subject taken as focus or starting point, and what is admitted as part of its content.

I shall discuss briefly an aspect of each.

In some respects the most accessible, interesting and popular foci for the study of the history of linguistics, or linguistic anthropology, would be the history of great men, on the one hand, and of great ideas, on the other. Here I shall plump for a general history of the study of language that begins empirically with the case histories of individual lines of activity. In recent times this starting point would involve consideration of 'schools', although the concept of 'school' must be used carefully, to avoid attempts to partition the scholarly universe with misleading neatness. To take this course would be to ascribe secondary status to the history of particular topics, ideas, and the like, and to accounts of particular persons abstracted from their institutional involvements. While such studies, like other studies of cultural content and biography, have an undeniable autonomy and value, my belief is that an adequate history must ultimately anchor cultural content and individual lives in space and time in terms of a sociocultural matrix, that the fundamental frame of reference must be the on-going activity of inter-communicating groups of scholars.

In these terms, individual and comparative studies of all the lines of national or individual philology are in order, whether of indigenous or foreign provenience, as already noted. As for studies of lines cultivated mainly by outsiders, the characteristics revealed would be most immediately those of anthropologists and linguists, and of the state of those disciplines, secondarily of the cultures from which they came. Studies of the lines cultivated mainly or first by insiders would of course reveal an additional element of the culture studied. The collective study of both types of line would comprise much of the history of general philology, and lead into a large part of the rest. There would have to be added only such comparative study and generalizing activity as did not stem from one or more such line, as well as the interrelations among all of these. The amount to be added would be smaller or larger, depending on one's conception of the goals of linguistics, and of linguistic anthropology.

What can be said of such goals, with some relevance to actual practice?

The description and classification (genetic, areal, typological) of the languages of the world would be a goal of linguistics all might agree upon. Phrased more broadly, the goal could be said to be a theory or set of theories to explain the similarities and differences found among the languages of the world. Perhaps not all could go on to say that linguistics proper has as a goal the formulation of a general theory of the nature of language. Probably some would not include the corresponding formulations for the study of language in anthropology, which would have to expand each of the above statements of goal to read, successively: the description and classification of the languages and patterns of linguistic activity of the societies (or cultures) of the world; a theory or set of theories to explain their similarities and differences; a theory of the nature of language as an aspect of culture, society, and personality, hence, of the nature of these in terms of man as a language-using animal.

If the narrower definition of the goal of linguistics, description and classification, is adopted, the content of the history of the field has the long and broad continuity previously mentioned, but the relevant material is fairly well circumscribed. The more the definition is expanded, the more the content relevant to the history of the field expands, until it is not possible to study the history of linguistics, and of linguistic anthropology, apart from a working knowledge of general ethnology and the history of civilization. To the material relevant in a given society or culture are added values and beliefs pertaining to language and linguistic activity, and the linguistic aspect of social structure and personality. To the material relevant to the course of development is added a fair share of the course of ideas in society at large.

How does the course of ideas in fact fare in the treatment of the history of linguistics? Such treatment tends to focus on advances in empirical knowledge and controversies over method and disciplinary affiliation. General shifts and currents of ideas, and their contexts, receive less attention, apart from their role in dispute as to disciplinary affiliation, and control over resources and benefits. The best known treatment in English, that of Pedersen (1931, 1962) is largely specialized to one line of work and largely uninformed by general intellectual and social history. Even a brief, if comprehensive, guide to the whole terrain of the development and schools of linguistics thus remains an unrequited wish. (As an outline treatment, Kroeber's chapter on philology (1944) is useful, but suffers from closeness to our own time; the lists of great scholars seem partly arbitrary and incomplete. If the method were applied to lines of scholarship defined more precisely and consistently, the results could be quite valuable.)

The classical controversies over the nature of language as physis or nomos, or between the analogists and the anomalists, are often noted, and they are indeed still with us in some form, but these and subsequent aspects of intellectual history on the same level are seldom examined closely. The impact on the West of the discovery of classical Indian linguistics is stressed, from the standpoint of effect on method and empirical insight into structure (and the Indian achievement sometimes explained away as due to a 'transparency' of Sanskrit), but the Romantic movement within which the discovery flowered in the West is likely to be at best alluded to either in its own terms or as a reaction. If however, one does include in the history of general philology some examination of the history of general ideas about language, then, so far as Western civilization is concerned, one might begin with two classical controversies mentioned above. There might follow the rise of speculative grammar in the later Middle Ages; the Renaissance impetus to anthropological-like searching out of neglected texts (Boccaccio's experiences read like an ethnologist's quest for the last survivors of a dying language, and indeed pure Classical Latin was then dead, having to be rescued); The Age of Discovery's impetus to European sampling of the diversity of the languages of the world; English empiricism; the interplay of the Enlightenment and Romantic movement; the interplay of evolutionary thought and historicism; the rise of structuralism. All these would figure as main subsequent themes.

These matters would not only be central to a broad history of linguistics proper, but also enter significantly into the history of anthropology's concerns with language, affecting, as they have and do, the linguistic materials available for its work, the relations between linguistics and other anthropological work, and, through anthropological conceptions of the nature of language, anthropological conceptions of the nature of culture and man.

Thus, the joint chapter in the histories of linguistics and anthropology might be written as a gradual progress in quantity and quality of knowledge, if any could, but when we note the fluctuations and discontinuities in progress, and when we inquire into the motives of its authors, and the ideas which affected the character of their work, we are back in the general and checkered course of intellectual history. For example, Cartesian rationalism, through Port Royal, shaped many of our early grammars of non-Western languages; the perhaps simple-seeming collections of vocabularies and Lord's Prayer translations (Adelung, Vater) cannot be interpreted apart from the Enlightenment and the movement of general grammar in the late eighteenth and early nineteenth centuries; the devotion of Boas to the recording of Amerindian languages is more intelligible against the background of the German intellectual tradition which influenced his interest in grammatical categories. (More will be said on these points in the next sections.) The significance of the interplay between data from other cultures and ideas from our own leaps out from the criterion of anthropology stated by Boas. Of themselves, data on other cultures (and languages) were and remained curiosities. "It was only when their relation to our own civilization became the subject of inquiry that the foundations of anthropology were laid" (Boas, 1940.514).

In short, an outline of the history of linguistic anthropology must be one in which the significant events are intellectual movements as well as travels and expeditions, one in which the dates of certain philosophers mingle with those of ethnographers.

To summarize so far, the development of linguistics in relation to anthropology might be broached in terms of the successive emergence, and subsequent complex co-existence of the levels of Folk-linguistics, National Philologies, and General Philology (or General Linguistics). The appearance of an evolutionary perspective is intentional, insofar as it concerns the broadest levels although of course advance can be imputed only in terms of one's own postulated criterion of the standard or goal. I would defend it as a framework exemplifying Julian Steward's interpretation of "universal" evolution (1953), empirically observed stages for human culture as a whole, without any implication of unilinear evolution, involving stages which each unity must be traverse in turn. (If desired, the individual lines might be interpreted in terms of Steward's concept of "multilinear" evolution.) Indeed, the sequence resembles the course of sociolinguistic development, reflecting, as it does, the successive emergence of different types of linguistic community. It is hard to be concise on this point without seeming to play into the hands of any who may still hold naive or simplistic notions of societal types and evolutionary stages, but we can all recognize the existence, and earlier and later appearance as types, of the sorts of linguistic community illustrated by the aboriginal Crow on the one hand and the contemporary

German speaking world on the other. Change in type might be imaginatively illustrated by the continental Germanic tribes as known to Tacitus, the linguistic stratifications of Roman and Norman England, and the linguistic variegation of modern England. The rough parallel between the development of linguistics and the development of socio-linguistic types is expectable, since linguistics is itself one use of language, motivated by and answerable to social needs. The history of linguistics is one facet of the history of civilization, which in terms of that facet might be viewed as the successive emergence (as well as consequent interaction) of societies predominantly justified, rationalized, in terms of oral tradition; in terms of a central body of revealed or accumulated, venerated text; in terms of the claims and applications of reasoning, science and scholarship.

The distinction of three levels, let me repeat in so many words, does not imply a simple picture of evolutionary supersession. As noted, folk-linguistics exists in all societies. As noted, within the context of general linguistics, or contemporary with it, new lines of national philology continue to appear. Nor need the three levels be seen as natural developments, one from the other; in a given case, any may be a break from, reaction to, in conscious opposition to, any other.

Especially important, from an anthropological viewpoint, is the fact that all three levels are grist for the mill, not only of the student of history of the study of language, but also for the student of the place of language in culture. That place varies from one society to another, and from one time to another within the same society. The level, or complex of levels, of conscious attention to language manifest in historically known cases constitutes subject matter for comparative study together with, and just as much as, the case-histories derived from contemporary ethnography.

IV. Technical development

Approach. By technical development, I mean the development of the field in the sense in which one speaks of 'technical linguistics' or 'technical philosophy'--the development of the field largely as seen from within, or in terms of the activity of its professional practitioners.

In general terms, the development of the field might be seen as dependent on four sorts of things: (1) its resources, including here the prior and continuing development of method, the accumulation of materials and knowledge, and circumstances of support and access to method and materials; (2) its personnel; their numbers might be taken as one of the resources, but one should consider separately their nature, especially the personal style and effect of leading figures; (3) its social structure, regarding its own organization

and the organization of the society around it; (4) its beliefs and values, especially its participation in wider currents of thought. One might examine a particular department or university, or the field at some period and place, or the field as a whole within such a framework.

The framework is one I have loosely adapted from the schema for the study of a social system devised by Talcott Parsons. Whatever its failings, the adaptation points to something of importance, namely that when practitioners of a social science field such as anthropology study its history, they should learn to be historians, but not forget to be social scientists. Having developed ways of studying sociocultural phenomena, their patterning, their diffusion, etc. (not without some common roots, historically, with history itself), anthropologists should be prepared to use and test such ways of study when the sociocultural phenomena in question are constituted by their own profession.

I can not discuss the many topics that might be taken up, but will draw for illustrations here mainly upon questions of method and materials. And I want to say something about the special interest of the technical history of a field to its practitioners in this regard.

Benefit and contribution. The relationship between benefit and contribution is twofold. Many specialists can most easily benefit from a history of their field which concerns the internal record of its ways of doing things, and also they can most easily contribute to such a history.

First, as to benefit. In a field such as the study of language, in which older materials may be of contemporary value as the only documentation of a case, or may supplement modern data, or confirm modern inferences, some knowledge of the history of the field may be necessary simply for its ongoing research. It may be necessary in order (a) to know what materials are extant, (b) for their preservation, and (c) for their adequate use.

Regarding knowledge of extant material, it is important to remember that collection of linguistic materials was often joined with ethnography or other activities, and that scientific activity generally need not flow in the channels obvious to later periods. Materials relevant to the historical study of linguistic topics, such as the concept of basic vocabulary, are likely to be missed by the linguist unfamiliar with the history of ethnographic expeditions. (A vocabulary list from a Russian expedition of the late eighteenth century was recently brought to my attention through the good offices of a chain of archaeologists.)

Regarding preservation, it is worth commenting on the fact that the collection of data does not imply its preservation or availability, and that the drama of disappearing cultures and languages is sometimes matched by the in some ways more tragic loss of valuable materials on such cultures and languages, materials that the profession, so to speak, once held in its hand and let slip away. Active effort and study is needed to prevent this. (Those who think all earlier data not worth attention will of course not be moved, but such people usually lack historical perspective on their profession and their own problems.) Some of Sapir's materials are said to have been lost subsequent to dispersal at his death. (There is of course also Dixon's burning of his own notebooks before his death, and Sapir's of his correspondence.) Many and varied are the individual bits of knowledge as to the location of such and such field notes, or manuscripts, or even unrecorded experiences still filed only in someone's neurons. Of such knowledge gained in any given generation of anthropologists, distributed across it, only a portion survives as public knowledge. On the other hand, reports of loss may be false. (I had been told that Pliny Earle Goddard's Athapaskan notes disappeared after his death, but John Freeman informed me that Kroeber had deposited them in the Library of the American Philosophical Society.) Not only anthropologists are sources on this score. A Wasco Chinook informant reported with convincing detail the collection of now unobtainable mythology from his father years ago, material that never reached print. (Unfortunately, despite this lead, no trace of the collector (a woman) or the material has appeared.) Indeed, the field investigation of informant's perception of the study of their group by various and successive field workers would be in itself a fascinating type of study in ethnohistory. Combined with library research, it would be an invaluable kind of lead both for the ethnohistory of the group concerned, and as a special vantage point on the history of anthropology.

Regarding use of materials, their correct interpretation, regarding orthography, organization, and technical terms, requires a knowledge of the general development of method at the time, and of what was available to the source of the materials. (Thus to determine the phonemic system of Kathlamet Chinook, now extinct and recorded substantially only by Boas, it was necessary for me to trace the development of Boas' orthographic practice and knowledge of the phonetics of North Pacific Coast languages ("The Lateral Order in Northwest Coast Languages and Linguistics" (ms.).)

At present the information which is useful in the three purposes cited must become by ad hoc and often accidentally. Often it is subject to the vagaries of oral transmission, and much personal knowledge dies unrecorded. Two things are greatly to be desired: treatments of the history of field work, including the methods used, and a habit of making depositions as to personal knowledge of matters

which conventionally do not reach print, but which will illuminate the efforts of later scholars to find and interpret older materials.

Other purposes are served as well. Knowledge of the details of a field's past, or compendia in which such knowledge can be found, are sometimes helpful in planning and conducting research, pointing to opportunities, or enabling one to avoid unnecessary repetition, or mistake, if nothing else. The last point is worth stressing in a field which, properly suspicious of conjectural history when offered by informants, continues to indulge it in regard to itself. Out-right errors of ignorance, failure to check the record, occasionally occur in the historical references and interpretations made by notable figures in the field. Sometimes it is a matter of detail, sometimes error is on a broader scale. A major source of discontinuity, and of superficial revolution, is failure of historical knowledge and consciousness. Let me discuss two examples of some general interest, pertinent to recent research.

(a) Regarding Whorf: the gradual remission of amnesia as to the past anthropological history of the problems dramatized by Whorf should be a source of some embarrassment to those who saw in his work, as Whorf did not, a revolutionary discovery. John Carroll's publication of an early manuscript, "A Linguistic Consideration of Thinking in Primitive Communities," (1956.65-86) makes clear that Whorf saw his work as deriving from Sapir and Boas, to whom he credited renewal of the essential insight he was concerned to dramatize and develop. The record of continuity is in fact much longer, going back, of course, to Wilhelm von Humboldt, through Daniel G. Brinton in American anthropology, for Brinton translated Humboldt's essay on the character of the American verb, and posed the dual nature of the problem exactly; Brinton included as a third part of linguistics, besides phonology and grammar, "The determination of the psychical character of the tribe through the forms instinctively adopted for the expression of its thoughts and reciprocally the reaction exerted by those forms on the later intellectual growth of those who are taught them as their only means of articulate expression" (1980.37). And early in this century the interest was in fact widespread across the social science disciplines, e.g., in writings of Durkheim and Mauss in France, of Hocart and Marett and others in England. Had there been a continuing historical awareness of this interest, the level of recent discussion and of accomplishment might have been much higher.

Thereby hangs the tale of the loss from recent historical consciousness in recent American scholarship of the second major aspect of nineteenth century linguistics, the development of typological concepts in connection with social-psychological interpretations of language in relation to culture, often with a developmental

slant. Pedersen's standard work, Linguistic Science in the Nineteenth Century, celebrates the triumph of the comparative method but largely neglects the typological-social psychological current, the rival trend of general linguistics of the period. Glimpses of it appear in the first book of Jespersen's Language, but aside from the occasional outcropping of Humboldt's concept and label 'inner form', it seems to have pretty much been lost to sight. Its reflections in Boas' concern for grammatical categories, for grammars written as if a native were to make conscious the form of his own thoughts, and in Sapir's typology, which was fundamentally conceptual in base, have seemed unintelligible to some. Why nineteenth century writers concerned themselves with typology seems forgotten by participants in the current revival of interest in the subject.

(b) Regarding the genetic classification of North American languages: Most anthropologists have recently operated with a folk-historical conception of Amerindian linguistic classification, and of Sapir's role in it. The error is compounded of inadequate systematics, regarding the logical bases of genetic classification, and of inadequate historical knowledge. Sapir's deserved present-day reputation, preeminence, is projected backwards, and the history of classification commonly assimilated to a confrontation between Powell's 58 units of 1891 and Sapir's 6 units of 1929 (Sapir 1929a). In point of fact, the 'Powell' classification was almost wholly accomplished under the direction of Henshaw (my "Powell and Henshaw," paper presented to the May 1960 meeting of the Kroeber Anthropological Society, Berkeley, demonstrates this from a comparison of the 1891 Powell classification to one prepared by Henshaw in 1885 and privately circulated within the Bureau of Ethnology for the use of its members; cf. Hymes 1961b).

The proper confrontation of Powell is with Sapir's 'other' classification, that into 23 units which is presented before his adumbration of 6, admittedly partly conjectural, broader groups. The 23-unit classification is no more than a codified report on the joint accomplishments of American anthropologists during the decade from 1910 to 1920. Almost all of it, and of the combinations in the 6-unit classification, can be traced to earlier work, to which Sapir was to be sure a major but not the sole contributor. Dixon, Kroeber, Radin, Swanton, Harrington, Frachtenberg, as well as Sapir, contributed, sometimes independently arriving at the same new results, sometimes corroborating and accepting each other's conclusions.

(The development of Sapir's conceptions, their emergence vis-à-vis the timing of their publication, and their sources in the evidence, is itself a fascinating subject, for which some of the essential evidence is in the unpublished correspondence in the Boas Collection in the Library of the American Philosophical Society (vide John Freeman).) A detailed analysis of the literature of the period reveals

some interesting details that depart from a conventional picture - e.g., on Penutian, technically, Frachtenberg was first to publish the concept of a 'Penutian' outside California, and Kroeber first to publish data in support of it ("The Concept of Penutian: A Chapter in the History of Amerindian Linguistics" (ms., 1961)).

Their evidence was better than that of Powell's, resulting from more extensive, qualitatively superior, studies of the vocabulary and grammar of many Amerindian languages, especially those in areas of greatest diversity, the West and the South, where the new syntheses were chiefly made. Moreover, their immediate purpose was purely linguistic, as Powell's may not have been. Powell had been concerned to provide a linguistic classification as a service to ethnology, as a means of ordering the confused situation with regard to the nomenclature and identity of Amerindian peoples. Perhaps in this goal lies a partial explanation of the conservatism he showed, eschewing, for example, the connection between Shoshonean and Nahuatl accepted by Buschmann and Brinton. A low order classification would be the most useful to ethnology, and Powell's intermittent remarks that X or Y was first to separate a family perhaps reflects a need still felt to discriminate authentic units, as against the vagueness and inconsistency inherent in earlier work based on less adequate information. Certainly Powell's view of classification was weighted toward separation, and continued so, as shown by the later separation of Seri from Yuman on Hewitt's authority, but failure to promulgate Hewitt's inference of a relationship between Sahaptin-Nez Perce, Molale-Cayuse and Klamath-Modoc (later incorporated in Sapir's classifications) (- John R. Swanton, p.c.).

Their (Sapir, etc.) methodology was better grounded, weighting both grammatical features and lexical items as equally historical products, hence as equally involved in proof of genetic relationship (equally subject, of course to criteria of quantitative and qualitative adequacy). Powell, on the other hand, clove to lexical elements (including affixes, which proved decisive on Boas' urging for the linking of Nootka and Kwakiutl in Wakashan), and eschewed features of grammatical form. The latter, he thought were partly adventitious, partly the signs of the stage to which a language had attained -- in short, partly evolutionary rather than historical in character. He thought also that grammatic methods change perhaps even more rapidly than words, concluding "grammatic structure is but a phase or accident of growth, and not a primordial element of language" (1891.12).

Finally, their work was but a continuation that Powell himself had foreseen, evidenced in explicit statement ("It is by no means a final paper on the subject, but is intended rather to give an account of the present status of the subject, and to place before the workers in this field of scholarship the data now existing and

the conclusions already reached, so as to constitute a departure for new work" (1891.xxxvi), and in comments to individual groupings ("From a comparison of the vocabularies of the Haida language with others of the neighboring Koluschan (=Tlingit) family, Dr. Franz Boas is inclined to consider that the two are genetically related. The two languages possess a considerable number of words in common, but a more thorough investigation is requisite for the settlement of the question than has yet been given. Pending this the two families are here treated separately" (1891.120). Thus, had the classification been published a few years later, it might likely have united some groups kept separate in 1891 (thus, Boas definitely proposed the Haida-Tlingit connection (taking into account structures as well as vocabulary), and Swanton's work in the same period supported it). Indeed, it was revised within the Bureau of Ethnology itself, witness the reduction to 55 stocks in its revised map of 1915.

What happened in American anthropology was that a classification intended primarily for ethnological order, conceived as tentative, ignoring grammatical form as evolutionary in nature, based on rough vocabularies, which evidence was never published, became accepted not only as foundation of further work, which it was, but effectively also as gospel. A classification intended to represent purely linguistic history, resolving some of the uncertain points in Powell, based on superior evidence and knowledge concerning the languages, and for which the evidence had been largely published, or independently verified through the cooperative work of the leading scholars in the field, themselves the successors and peers of those who had formed Powell's classification from the evidence of an earlier day -- that classification was effectively forgotten.

Powell's 1891 classification had been superseded in terms of its own kind and presumable quality of evidence by 1920. The new groupings were not just the result of seeing (or imagining) more, or requiring less, in the same data; they were largely the result of seeing about the same degree of evidence, requiring about the same, in a much greater body of data, although occasionally the standards of evidence were simply raised. (I doubt that anyone working for Powell then saw as much evidence of Eskimo-Aleut connection as Sapir later published for Ritwan-Algonquian or Na-Dené.) Sapir's 6-unit classification, which became the focus of attention, was published as a guide to further work, not as a definite result. Yet subsequent picturing of the history of North America genetic classification has largely been conceived in terms of these two poles. The record of the intervening work has been effectively neglected, together with Sapir's 23-unit classification, which summed it up. Reflecting the existing level of evidence and hypothesis, that classification would have provided a framework within which work and discussion could have been fruitful and continued cumulatively, for both anthropologists and linguists. As it was, the projection onto the consciousness of

the field of the 6-unit classification, one out of reach, capable of being accepted on faith or rejected, but not tested as such, tended to inhibit work and discussion, and to produce an unfortunate image of polarization into opposing camps, a conservative camp holding to Powell's 55 units as firm footing and sceptical of anything subsequent, and a radical camp flinging 'macro-phyla' (a term associated with the classificatory level of Sapir's 6 groups) about with abandon. In fact, of course, an adherent of the 6-unit classification should have been concerned about how much it might include that was unverified or even false, but an adherent of the Powell classification should have been equally concerned how much it might omit that was verifiable and true. (A more extended explication of the problem is given in Hymes (1959).)

It is interesting to note that the cause, probably, was in part a visual one -- the adoption about a generation ago of the six-unit classification as the master, and colored, key to the only widely distributed modern linguistic map of North America (C.F. and E. W. Voegelin (1945)), a map via its key, repeated in a number of other books. What would have been the case if the 23-unit classification had been used as the key?

To be sure, Sapir's intermediate classification had to be extracted by the reader. It was not summed up as a unit by Sapir himself, but distributed across three sections. Sapir introduced Powell's classification in two parts, first discussing 9 language groups, and then listing 46 languages in alphabetical order. (The distinctness of Siuslaw from Alsea (Yakonan) is noted at the end of the list, so that there in fact are 47 languages in the second part.) He then indicated 12 reductions, groupings together of separate Powell stocks, that seemed probable or very possible. To arrive at the overall result, one must notice that the reductions leave untouched 3 groups in the first set of 9, and 8 languages in the alphabetical listing, and add these 11 units to the 12 new groupings.

The resulting 23 groups, enumerated so far as I know for the first time now, are as follows (newly numbered here and given in the order of Sapir's discussion):

(1) Eskimoan, (2) Muskogian, (3) Siouan (from the first set of 9 groups);

(4) Chinook, (5) Keres, (6) Kootenay, (7) Timuqua, (8) Tsimshian, (9) Yuchi, (10) Yuki, (11) Zuni (from the alphabetical list);

(12) Wiyot-Yurok, Algonquian, Beothuk (?); (13) Iroquoian, Caddoan; (14) Uto-Aztekan (Shoshonean, Piman, Nahuatl); (15) Athapaskan, Tlingit, Haida; (16) Salishan, Chimakuan, Wakashan (=Mosan); (17) Atakapa, Tunica, Chitimacha; (18) Coahuiltecan, Tonkawa, Karankawa; (19) Kiowan, Tanoan; (20) Takelma, Kalapuya, Coos-Siuslaw-Yakonan

(=Oregon Penutian); (21) Sahaptin-Wailatpuan-Lutuami (Klamath-Modoc); (22) Karok, Chimariko, Shastan, Yana, Pomo, Washo, Esselen, Yuman, Salinan, Chumash, Seri, Chontal (=Hokan); (23) Miwok-Costanoan, Yokuts, Maidu, Wintun (=California Penutian).

It should be noted that S. M. Lamb (1959) independently reconstituted pretty much the Sapir 23-unit classification, through his own assessment of the evidence. Lamb's results have been adapted in the excellent chapter on language of the best modern book on the North American Indian, (Driver, (1961)). Independent efforts (by Hymes, Shipley and perhaps others) indicate that the number of units is indeed the level at which cooperative research can be effectively secured at the present time.

To take up briefly now the contribution of the practicing anthropologist to the history of his field: while he or she may not easily be able to contribute analyses of general movements of ideas, or comparative studies only partly involving his own chief interests, he is often uniquely the person fitted to contribute within the area of his interests. If we conceive the history of anthropology anthropologically, as a whole to which all is germane, not only the sector of major overt ideas and values, but also the institutional structure, the personalities, the ins and outs of accomplishment in methods and materials, as a whole which resides in its parts as well; if we should like a history of anthropology not only to move at the level of the broadest trends and major ideas but also to be able to move back and forth intelligently between the broad and the particular; then we see that most anthropologists have some contribution of value to make. Only the specialist in a subject knows well how broad trends have been interwoven with it. The intellectual historian can recognize the reflection or appearance of ideas in anthropological writings, but he can not easily trace them into the nooks and crannies of specialized literature, nor easily tap what oral tradition has to add. Nor often can he properly assess a technical report which may, seen in proper perspective, be a 'concrete universal', whose recognition illuminates some aspect of the field's development. At the very least there are the multitude of details whose verification would exhaust an individual lifetime several times over, so that accuracy of the record must be continuing collective effort, as each scholar encounters some portion of it from his own vantage point. And of course each new vantage point opens an opportunity for a new perspective on the history of the field. In sum, most anthropologists can make a contribution, and should do so, for the history of anthropology should face two ways, both outward toward the larger intellectual world and inward toward the profession itself. An active concern with its history is essential to its cumulative progress and self-understanding.

V. Some Perennial Questions and Tensions

(A) The relation between terminology for a hybrid field, and the role of linguistics in anthropology: One of the best indications of the existence of a hybrid study, part linguistics, part anthropology, is the long history of attempts to name it. The attempts show recognition of a need; their lack of permanent success shows the fluctuating fortunes of the hybrid, never quite either wholly independent, or wholly assimilated to one or the other of its parents.

Scattered examples show 'ethnological philology' in England as early as the 1850s (Latham, Cull), and 'linguistic ethnology' somewhat later in William Dwight Whitney's writings. Coordinate terms, 'ethnology and philology', later, 'linguistics and ethnology', dominate until recently (although Broca did use 'linguistic anthropology' for a major division, and Horatio Hale (1892) said "Linguistic anthropology is the only true science of man". (And cf. Whitney on 'linguistic ethnology', cited at the end of ch. 2). Although Bloomfield referred to the 'ethnologic-linguistic school' (1925) and Malinowski had spoken of urgent need for an "Ethnolinguistic theory" to guide field work (1920.69)), such syntactically more intimate terms have become prevalent only since the Second World War. Since then, 'ethnolinguistics', 'metalinguistics', and 'anthropological linguistics' have all received attention in several senses, alongside a great currency for 'psycholinguistics' in the last decade, and some frequency being achieved by 'sociolinguistics'. The variety and currency of these changes rung on the base '-linguistics' attest both the prestige of contemporary linguistic method and the desire to renew its connections with a broad range of problems. A full collation and analysis of the history of use of such terms as those cited could shed some light on the relations of linguistics and anthropology, and could be the preliminary to standardization of current use.

With decline in the use of 'ethnology', present anthropological usage most frequently employs a combination of 'linguistics' and 'anthropology' in either order, 'anthropological linguist/ics' and 'linguistic anthropology/ist'. Both orders have checkered precedent through ad hoc coinage, e.g., Lowie, Kroeber, Emeneau, and probably others have used 'linguistic anthropologist' in some context during the past generation, Voegelin and others have made 'anthropological linguist' well known, and Herskovits uses both in the first part of his textbook Man and His Works (New York, 1949), as do Danehy, Hockett, Pittenger in The First Five Minutes (Ithaca: Paul Martineau, 1960).

(B) The relation between the state of linguistic methodology, real or supposed, and the place of linguistics in anthropology:

(1) The history of instrumental aids to linguistics, among other branches of anthropology, has been discussed by John H. Rowe (1953), showing that there are a number of worthwhile questions and relationships to be pursued. (A later survey, although without special reference to anthropology, is that of Trendelenburg (1957); cf. also the essay in the same volume by G. Panconcelli-Calzia (1957).) The interaction of such instrumental aids, their actual results, and the expectations they have aroused, not only with the planning of work but also with general linguistic conceptions, badly needs further study. With regard to American anthropologists, we may note here the interlude five decades ago of instrumental research into the phonetics of Amerindian languages by Goddard and Kroeber, using instruments imported from France; the sporadic appearance of instrumental reports on Amerindian languages over the years in IJAL; the flurry of expectations that have successively followed the development of the sound spectrograph, the tape recorder and the electronic computer, each with some impact on concepts and method. This topic leads into

(2) the history of conceptions of the phonic level of languages. There are of course the notions of 'alternating sounds' in primitive languages, of rapid phonetic change in the absence of writing, etc. Such notions are properly varieties of a general trait, that of imputing variance, irregularity, where the linguistic or social regularities depart from one's own norm. Instances can be found for all aspects of language, as well as for the other attributes of men. So far as such notions may have affected the serious study of the languages spoken by peoples of interest to anthropologists, they are of a piece with other stereotypes, early discredited within most of the profession, and often the subject of admonition to others. Regarding Boas' role, see Wells (1974).

More notable for recent anthropology, so far as theoretical ideas are concerned, is the connection between the development of experimental and comparative phonetics, and Sapir's celebrated essay, "Sound Patterns in Language" (1925), wherein the concept of configurative pattern is highlighted and phonetic data assimilated to cultural, or sociopsychological system to 'Geistwissenschaft' from 'Naturwissenschaft', between phonetics and Bloomfield's belief in the reality of the distinctive feature, as conceived by him; between recent acoustic phonetics, and Jakobson's conceptions, postulated much earlier, of the distinctive feature and binary opposition. With Sapir it was a case of reaction against an existing brand of phonetics, a new interpretation of phonetic data on the basis of personal insight, experience, and general intellectual approach; with Jakobson it was a case of new work in phonetics lending support to, and a chance to develop, theoretical notions presented long before, again originally on the basis of personal insight, experience, and general intellectual approach. In both cases there has been a

connection with broader areas of anthropological thought. Sapir's conception of pattern in language, according to Aberle (1960), was the model for much of American culture and personality theory. Kenneth Pike (1955) attributes to Sapir's conception, first explicitly developed in regard to phonetic data, the inspiration for his own conception of 'emic' vs. 'etic' approaches. Jakobson's influence on Clyde Kluckhohn's approach to the analysis of values in his latest writings is patent.

These cases are of some interest, for the diffusion of conception or terminology into the rest of anthropology bespeaks a special appeal for linguistics, or language, during the period to one current of anthropological thought; yet the conceptions themselves (at least in Sapir's case) probably do not derive simply from linguistic results, but from theoretical conceptions and personal bents brought to language data, the results with language data being later employed to support application elsewhere. Sapir's gift for sensing configuration, and concern for the subjective or actor's point of view in interpreting social phenomena, his commitment to both configuration and the actor's point of view intellectually, are evident in his 1921 Language (cf. the discussion of drift, of diffusion, of the 'inaudible flow of ideal phonetic elements' perceived by the native speaker) and in earlier work. The explicit development later of the concept of the phoneme, which became the kernel of subsequent American conceptions of linguistic patterning, may of course have reinforced Sapir in the mode of thought. But here he probably was already indebted to Boas for the analogy between language patterning and other aspects of culture. His essay on "The Unconscious Patterning of Behavior" (1927) draws heavily on language for illustration, but Boas had already argued the importance of language to ethnology precisely because linguistic phenomena, being less subject to secondary (conscious) rationalization, were more purely representative of the nature of cultural processes (1911). In developing the configurational, or systematic, implications of the concept of pattern, however, Sapir, and Pike, do go beyond Boas, who was a forerunner of structural linguistics, but not a structuralist himself; see my review of The Anthropology of Franz Boas (1961a).

Boas was the great American champion for the principle of approaching a language empirically, with but the minimum necessary preconceptions. In organizing the data of description, however, he hugged closely the surface level of detail and observables (witness his phonetics, and his positional analysis in grammar), and did not much seek internal relations or synthetic rules of much systemic scope. Probably he would have regarded that as involving too much construction on the analyst's part. Sapir combined Boas' empiricism with a search for internal (paradigmatic, generative) relations characteristic of modern structuralism. Clearly enough, there are

examples of the other two possible combinations as well: both structural, and surface-level, approaches which are toward the a priori rather than empirical pole. The historical development is not straight-line. Indeed, if someone familiar with modern linguistics were to plot the various approaches within these four co-ordinates, the result probably would show a good deal of scatter.

Returning to Aberle's critique, he accepts Sapir's particular pattern approach in linguistics, arguing only for its inadequacy as a model for the rest of culture. Aberle objects that viewpoint confronts the individual with the heritage of shared culture, especially symbolic resources, without the intervention of the necessary third term, the social system ('cultural system' in Aberle's phrase), in which persons participate as in a network, but which they do not strictly share. He is partly right in combating the influence of linguistics on such views of culture, since on the American scene linguistics has lent such a viewpoint support; but he is partly wrong, since he should challenge the viewpoint within linguistics itself as well. The linguistic and cultural theories are of a piece, historically and in practice, in American anthropology, both focusing more on the patterning of symbolic products than on the patterning of interaction. Considerations which find the viewpoint inadequate for non-linguistic culture should find it inadequate for language as well. (The argument is detailed in Hymes (1962).) In Aberle's historical critique we have an illustration of the current prestige of linguistics, and of the frequent failure to recognize that major ideas in linguistics and anthropology, especially where the two professions have been joined in the work of the same scholars, have often the same sources, virtues, and limitations.

Here also should be mentioned the influence of Prague School linguistic theory on Lévi-Strauss, most notably the appeal to the precedent of Trubetskoyan conception of necessary relations in linguistic phenomena at the outset of Lévi-Strauss' extended analysis of the relations between family structure, authority and attitudes (1958).

This topic involves:

(C) the relative closeness of linguistics and anthropology at various times, as related to the state of linguistic method. The intensive development of descriptive method that began a generation ago seemed to result for a while in both increased prestige and increased distance for linguistics. Probably it was partly that the innovations seemed to leave behind those whose linguistic training had been acquired earlier, partly the 'young Turk' attitude of some of the younger linguists, but also probably because there simply were few linguists, and fewer anthropological jobs for them. Now that the new linguistic methods are encountered by students as just

another part of the curriculum, we begin to witness a younger generation of anthropologists who again take linguistics for granted, as Boas' students took his methods, and we witness also the beginning of a new integration of linguistics into the practice and theory of ethnography (cf. Goodenough (1957), an article which has had great impact on some young ethnographers, and cf. of course the surge of interest in componential analysis).

The sequence of distance and closeness seems to recapitulate a sequence in Boas' own conception of the relations between linguistics and anthropology early in the century. In 1904 he noted a breaking up of the field of anthropological research through specialization, and, although he demanded that the field anthropologists be familiar with the principles of biology and linguistics, foresaw their final separation from ethnology-archaeology: "I think the time is not far distant when anthropology pure and simple will deal with the customs and beliefs of the less civilized people only, and when linguistics and biology will continue and develop the work that we are doing now because no one else cares for it" (1904). A few years later Boas devoted a major section of his famous "Introduction" to the Handbook of American Indian Languages (1911) to the thesis that linguistics must be an integral part of ethnology, for practical, and, more importantly, for theoretical reasons: Ethnology is the science of mental phenomena, and language shows such phenomena in purer form (the thesis we noted above).

(Whether this theoretical belief, or simply a desire to record exotic languages before their disappearance, or some combination of factors, underlay Boas' frequent urging of a linguistic thesis on Ph.D. candidates in anthropology, I do not know.)

(D) The relations between linguistics and major subfields of anthropology:

(1) In the nineteenth century the relation between linguistics and physical anthropology, or biological anthropology, seems most saliently at times to have been one of rivalry for precedence in tracing human history and origins (see ch. 2) and in explaining human capacity. The professionalization of a discipline in the twentieth century, being accompanied by a sharp demarcation between the cultural and the biological and between language, race, and culture, did not encourage interconnections, except common concern with the history of particular peoples and areas, such as the American Indian. In the latter part of the present century the rise of primate studies and ethology, and the revival of interest in the origin of language have led to a great deal of attention to the biological bases of language, the emergence of levels of capacity for language in human history, and comparison of modes of communication across species

(what Wells and Sebeok have called 'zoosemiotics'). Racial and cultural explanations for apparent differences in linguistic ability in schooling have continued to engage concern. So far as I know, there is no systematic examination of these relationships, either historically or in contemporary terms.

(2) The relations between linguistics and archaeology have been little examined. Some few have combined linguistic and archaeological work in the field situation (Frederica de Laguna, for example, explored Eskimo linguistic terms for the objects and places she was excavating in Greenland (about 1930) as aspects of the same problem. So far as methodology is concerned, there would appear to have been a peak of interest in linguistic results in the nineteenth century, associated with sanguine expectations from the methods of linguistic reconstruction, followed by diminution of interest with critiques of the results and correlations to be hoped for, and theoretical and methodological emphasis on the historical discontinuities of race, language, and culture (cf. Boas, "Introduction", cited above and Sapir's Language (1921), or his 1912 essay, "Language and Environment". The recent advances in historical evidence and method in Amerindian and other branches of linguistics associated with anthropology have stimulated archaeologists and ethnologists again to a higher level of interest (e.g., papers in the last decade by Rouse and D. Taylor on Island Carib, Willey on Algonkian-Gulf relationships, W. Taylor on Shoshonean prehistory, Morris Opler's forthcoming discussion of implications of results of linguistic classification of Apachean tribes; Romney's discussion of a genetic model, involving linguistic classification, in relation to Uto-Aztekan (stemming, with several other pieces of work by others, from a particular seminar of D. B. Shimkin at Harvard some years ago), and spread of interest in such an approach. The prospect of new historical gains from linguistic evidence was in large part responsible for Kroeber's return to linguistic activity in the last decade of his life. The attraction of linguistic method as a model has recently also been felt in American archaeology, although little of the result has reached print. (I have in mind work of Irving Rouse and John Rowe.) To what extent such influences may have been exerted in earlier periods, I do not know, but cf. Hymes (1958) and see now Hymes (1970a, b).

(3) One aspect of the relations between linguistics and ethnology is of special importance: the use of a classification into linguistic units for the ordering of ethnological data. The earliest instance of which I know is Schlözer (late 18th century). In American ethnology, we know that Powell approved Henshaw's suggestion for use of linguistic classification in organizing the chaotic information on North American Indians, that the Powell classification was accepted as a first-order framework for Amerindian ethnography, not only in the Handbook of American Indians North of Mexico for

which is was devised, but generally (cf. Kroeber: "The close ultimate relation which exists between linguistics and ethnology, with the latter as the chief gainer from the efforts of the former, is illustrated by the extent and depth of influence which the Powell classification of linguistic stocks has from the day of its promulgation exercised on every aspect of American ethnology" (1918)); and that linguistic units became relied upon by many as the framework within which to tackle also problems of comparative ethnology and culture history (e.g., Swanton and Dixon (1914), and Wissler (1917)).

What is not clear is the extent to which various factors contributed to the adoption of a linguistic framework. Relative ease in obtaining it, and the neat qualitative pigeon-holes it provides, certainly loomed large. But was ease a function of linguistic method? or of the nature of linguistic data? Or to what extent were the seeming ease, and the ready acceptance of the resulting pigeon-holes, dependent upon a particular view of the relation between language and culture? I suspect that the Romantic movement's identification between a particular language and a particular culture, and its stress upon the language as a symbol of the culture, contributed in an important way. Moreover, I believe it can be shown that the continued influence of the equation, one language -- one culture, has hampered American anthropological thought in a number of noticeable ways over several decades.

(E) An afterthought, really, regarding (C): The history and systematic analysis of the relations between historical methodology in the various branches of anthropology would be a greatly rewarding study. Part of the study would be to determine the extent to which the methodologies have influenced each part; the extent to which they have been similar or different in assumptions, procedures, and the like; and other historical questions. Part would be to determine the extent to which the methodologies can rest on the same assumptions, or employ like procedures, and to determine the points at which the nature of their materials force them to depart from one another. Here should figure the history of the degree of understanding of each other's methodology and results that has obtained, for that degree of understanding has variously affected the course of the field.

Here too might enter consideration of the profile of relative prestige for linguistic method during the course of anthropology, and its sources, legitimate and imputed. The singling out of linguistics as most precise Geistwissenschaft goes back at least to Hermann Paul, but the motives vary. It plays a significant part in the intellectual careers of Kroeber, Kluckhohn, and Levi-Strauss, as indicated. The theme is part of the history of anthropology's relation to other disciplines, and its conception of itself as a humanistic or scientific enterprise, or both. Insofar as anthropology's vocation, historically, has been that of treating more scientifically

the materials of the humanities, linguistic methodology is central, for, I believe, it has been the example most commonly appealed to as warrant for such a vocation (cf. Levi-Strauss' conclusion to the article cited above; Sapir's conclusion to "The Status of Linguistics as a Science" (1929b); Kroeber's references to his own writings under the index item 'Language as an example for culture' in The Nature of Culture (Chicago: University of Chicago Press, 1952)).

(F) Fieldwork. One final topic of importance is the relation between linguistic methodology, and anthropological conceptions of language, on the one hand, and the fact that 'anthropological linguistics', as Voegelin states, has predominantly been concerned with discerning the structures of exotic languages through first-hand work in the field.

It is well known that a decisive experience in Sapir's life was coming to Boas as a student, and finding that for every generalization from his philological training, Boas could cite an American Indian exception. The set of mind in which one was prepared to find an exception or uniqueness at any turn was strong in linguistic work, as in other branches of anthropology, for some time, and legitimately so. Early work of Boas, Kroeber, and Sapir had to rescue Amerindian languages from the sweeping generalizations of premature typology, and from uninformed prejudices about what languages could and could not be like (Boas had an early paper on Bella Coola refused by a German editor who said no language could have words without vowels in them). Moreover, the intensive research that began with Boas simply did continue to uncover surprises, patterns that fit uneasily into existing frameworks. Yet it is noteworthy that two leaders saw need for a universally applicable framework early in the period and it is equally noteworthy that the perception was without effect for a generation. (Kroeber's statement of the need is little known and was apparently soon forgotten (see Hymes 1961.21-2 (this volume, ch. 5) for discussion), while Sapir's typological scheme in his 1921 Language seemed almost an aberration perhaps to some.)

In this period, the effect of intensive field experience, then, would seem to have been to destroy the claims of inadequate general frames; but whether one rested content with that, as Boas, and for a while some others, seem to have done, or whether one wishes for an adequate general framework in replacement (as Sapir and Kroeber) seems to have depended, not on field experience, but on pre-existing intellectual bents and aspirations.

In this connection it is worth asking why such intensive field experience did not give rise to an adequate descriptive methodology long before the present century. In the 18th century Marsden adapted

general grammar skillfully to the needs of the Malay, and in the 19th century Kleinschmidt consciously depicted the structure of Eskimo in its own terms. In each case the success seems to have been limited to the scope of a single language, and to have been predicated upon the interaction of years of intimate knowledge with first-rate ability. Perhaps this is simply a question of when the field became sufficiently professionalized for accumulation and transmission of skills. But cf. also Castrén's 'remarkable mid-century (19th) description of Siberian languages' (W. W. Elmendorf, p.c.) and Boghtlink's description, partly Panini-inspired, of Yakut. (The history of work in Soviet Asia badly needs telling.) Further examination of early descriptive grammars, back to those of Rask or beyond, certainly is in order.

Many of the characteristics of linguistic anthropology can be traced in part to its involvement in field work, but characteristics may be mistakenly so traced. Indeed, a sizeable study could be made of the number of results whose source has been claimed as field experience, but which could be shown to stem from theoretical preconceptions. The rise of field experience as the validating criterion in anthropology has been accompanied by a rise in mythical accounts of its role in intellectual history. Quite diverse approaches have been equally ascribed, just as the distinctive characters of British social anthropology and American cultural anthropology have each been claimed to have arisen from field work. The neo-Bloomfieldian temper in linguistics has been ascribed by some to the impact of field work, and plausibly so, with its stress upon the viewpoint of the decoder beginning with the phonological level. Yet some of the most ardent advocates of such a position have engaged in relatively little anthropological field work, whereas the principal anthropological linguistic field workers of the period have been those trained by Sapir. In point of fact, the salient attitudes of the neo-Bloomfieldian outlook stemmed largely from interpretations of his adherence to behaviourism in the latter part of his career. The attitudes were in rather sharp conflict with some of the basic perspective of Boas and Sapir, the two men with the greatest personal field experience with languages. In turn, both Boas and Sapir were excited and enriched by their vast experience with the diversity and details of the languages with which they worked, but Boas' general ideas about language come as much from his general intellectual background and ethnological views, and Sapir, although his 1921 Language is packed with illuminating examples from Amerindian languages, attributes the book's fundamental insight into the nature of language to a reading of Croce.¹

An adequate account of the relation of field work and field experience to the development of linguistic knowledge and theory, within and without anthropology, would be a very broad undertaking indeed. Some of the subjects that would have to be included would

be: the early and sustained tradition of field work in the Finno-Ugric field, and, generally, the European, including Russian, expeditions to the east, parallel to those in the New World to the west; the work of the missionary orders and missionizing societies (including the consequences of differences between orders, as between Jesuits and Franciscans); the rich history of jointly ethnographic-linguistic field work in Europe, associated with study of regional and peasant culture; the history of short word lists, field guides, instructions to travelers, these having affected so much of the information now available, and somewhat methodology, especially in the area of genetic classification; the history of the compendia of linguistic information, such as Adelung's Mithridates. One theme of such a study, as already indicated, would be the diversity of motives that have led to field work; for example, the celebrated Indo-Europeanist, August Schleicher, undertook pioneering field work in Lithuanian about a century ago, because of Lithuanian's importance to comparative work. (He complained about the miserable conditions, but reported them compensated for by the chance to hear the beautiful forms of the language.)

Another facet of the history of linguistic anthropology is:

(G) the relation between the state of linguistic method in anthropology and its state outside anthropology. There have been relative lag and advance in both directions.

(1) When 19th century anthropologists, including Boas, wrestled with the glottalized consonants and voiceless laterals of the North Pacific Coast languages, it took until about 1900 for some of the sounds to be accurately recognized and orthographically provided for. Was sufficient knowledge of their nature available elsewhere? Was the difficulty felt by Americanists working with Amerindian phonetics at the time unavoidable, given the general state of phonetic knowledge at the time, or a result of lag, lack of communication and diffusion of training? (I suspect the latter, although I have not had the chance to check thoroughly.) Or, does skill and adequacy in phonetic recording depend not only on thoroughness of general training, but also on training or accumulation of experience with respect to the particular detail and gross phonetic effect of the type of language investigated, given absence of a phonemic perspective? The actual progress in phonetic recording (=scientific observation, at one level) of Amerindian languages seems to have reflected both factors.

(2) With the achievement of a genetic framework for Indo-European, satisfactory apart from thorny problems of subgrouping, most Indo-Europeanists have not been interested in asking further genetic questions. Although Pedersen accepted the relationship of Indo-European to Finno-Ugric ('Nostratian'), and Cuny and Müller

devoted themselves to the connection of Indo-European with Semitic (as Sapir had planned to do before his death), most research has been concerned with matters internal to Indo-European. Insofar as import for other branches of human history is a spur to historical linguistics, presumably the rich content of Indo-European materials and their involvement with the national histories of Europe has been satisfying and complex enough. One typical involvement of anthropologists with culture history in Europe has been through regional and dialect studies, often tracing connected details of linguistic and material culture ('wörter-und-sachen' approach). In parts of the world where the salient opportunities have seemed to lie at a deeper level of linguistic relationship, there has been more effective impetus to develop the methodology of remote genetic connections.

The net result has been that the methodology for initial discovery of genetic relationship although developed in Indo-European and some other families in the 18th and nineteenth centuries, has lain largely fallow there. Further development has been carried forward within anthropology, especially in the second and fifth decades of the present century. Ninetenths of the controversial subjects of lexicostatistics and glottochronology, for example, are but the making explicit and precise of techniques and assumptions that have a long history within older comparative philology. With the continuity, however, one must also record progress in clarification. (Note, again, that I am speaking here of the stage of initial discovery, of heuristics and elementary proof, not of what I would term the establishing of relations, the refining and detailing of networks of etymologies and correspondences.)

(3) In one sense the two examples just cited are matters of lag as between linguistics within anthropology and linguistics outside it, as noted at the outset. Such an out-of-phase relationship is sometimes a mere matter of chronology and the ordinary rate of diffusion, but sometimes it has more interesting implications. One wonders what real barriers to diffusion can account, for instance, for the writing and publishing in an English linguistic journal a few years back of an article on 'Pre-Grammar?', in which it is once more suggested that there may be languages (associated with stages of 'savagery' and 'barbarism') which lack true grammatical systems. Or for the recurrence of assumptions about rates of change and concreteness of vocabulary in primitive languages at an international congress of linguistics as recently as 1952.

With regard to discovery of genetic relationships, the 'out-of-phase' relationship, of course, has to do mainly with the nature of the problems being tackled. There does result, however, some confusion. Some linguists within and without anthropology identify the comparative method totally with the imposing edifice of Indo-European as presently worked out by a century and a half of collective

talent, advantaged by native knowledge of some of the languages and access to early documents. Such scholars may evaluate work of discovery and preliminary establishment in terms of the completed or well developed ideal image appropriate for present-day Indo-European research. To see matters thus is to inhibit work that is necessary, and must be necessarily rough. It is also to forget how the corresponding stages in well established families were handled. Anthropologists are often concerned with a stage that corresponds to that which Indo-European work experienced in the first part of the nineteenth century, and Whitney's chapter on genetic relationship, coming at the end of that period, speaks to the condition of present day anthropological work in a way that nothing can in Brugmann or Hirt (authors of synthetic comparative grammars of Indo-European). Yet some would set strictures on initial proof that would have prevented the acceptance of Indo-European itself. (And Indo-Europeanists today are often quick to point out the many uncertainties of detail in their knowledge of the family.)

One aspect of the out-of-phase relationship is that sound correspondences, 'phonetic laws', one of the great achievements of a mature Indo-European scholarship, have sometimes been taken as prerequisite to heuristics and initial discovery, whereas they were not so in the history of Indo-European itself. Thus Sapir has been attacked within the anthropological field for the weight he gave to grammatic evidence of relationship, yet in doing so he was but following Indo-European tradition (undoubtedly consciously so). Jespersen, Meillet, and many others stressed the vital role of grammatical evidence; Pedersen's history of linguistics in the nineteenth century brings out its significance. The vocabulary evidence of Celtic connection with Indo-European, for example, was suspect until analysis revealed traces of the Indo-European inflections as well.

A history of conceptions of genetic relationship is badly needed, both in anthropology and as part of the general history of linguistics. It is clear enough that the eighteenth century was rich with genetic insights and suggestions, and that some such can be traced as early as the 12th century. It is conventional to date comparative linguistics and Indo-European from Sir William Jones' famous announcement, but while Sir William provides a convenient origin myth, the myth itself provides no explanation of why and how comparative Indo-European developed when and as it did. For instance, Gyarmathai apparently discovered the Finno-Ugric connection earlier (cf. ch. 8). Similar problems are posed within anthropology, as the earlier discussion of Sapir's work has indicated. The linking of the histories of genetic relationship in anthropological work and in linguistics outside anthropology will reveal a series of fascinating instances of the interaction of ideas, materials, and personalities. By clarifying the background of the present situation it will also contribute to further advance.

In so linking these histories it will be important to trace the contribution of anthropological work to the general validation of the principles developed within a field such as Indo-European, and to consider the extent to which such validation has figured, if at all, in the European controversies over comparative method. (The notable American example here, of course, is the demonstration by Bloomfield and Sapir that the principle of regularity of phonemic change held for unwritten languages (Algonquian and Athapaskan, respectively).)

In this connection it will be important to trace in contemporary contexts vis-a-vis other fields of the social, cultural and physical sciences, the assumptions consciously or unconsciously present in the views held by linguists on the processes of linguistic change. Which aspects of change were to be assigned to the natural and which to the 'social' or 'cultural' sciences; whether change should be interpreted fundamentally in terms of individual or social psychology; what range of factors could be admitted or excluded -- these and other clashes involved linguists in views which may or may not have had substantial support elsewhere in the knowledge and views of their times. (Either case is equally interesting.) Here history and systematics meet with special force, for anthropological, sociological, and psychological knowledge should contribute not only to the understanding of past views, but to the clarification, and testing, and systematization of views held now and in the future, since any work in historical linguistics implies assumptions as to the processes of linguistic change, and such assumptions are of a piece with, subsumed by, general propositions of the behavioral sciences.

VI. Genres

One way of approaching the history of a scholarly field is to view it in terms of the cultivation of various scholarly genres. Thus, one could study: the relation between the cultivation of linguistic genres in anthropology and the cultivation of such genres in other branches of linguistics or anthropology.

(A) C. F. Voegelin has been sensitive to the development of grammatical genres in American linguistic anthropology, e.g., in his study of the "Boas model" (1952), and his attention to "structural restatements". (On the former, see now Stocking (1974), noted in ch. 8 of this volume.) Yakov Malkiel has carried forward in pioneering fashion the typological and distinctive feature approach to historical perspective on linguistic writings, especially within the fields of Romance linguistics, etymology, and lexicography (1957, commented on further in 1962a, 1962b). His work should be emulated generally in the study of the history of linguistics. The approaches make possible a more specific tracing of influences, and a sharper profile of the rise and fall of interests and lines of work.

In American anthropological linguistics there should be recognized, besides the Boas model, a Sapir model, exemplified in his Takelma grammar, and terminologically, having roots as well in the typological chapters of his book Language. An early Kroeber model for succinct characterizing sketches of languages might also be discerned, as must a later Voegelin model using index numbers for distributional statements. The latter feature carried over into the Pike tagmemic model now flourishing as a framework for much of our best knowledge of Latin American and Philippine languages. Among points of historical and theoretical interest are the rise and fall of discussions of grammatical processes and grammatical categories; the continuity between the Boas and Voegelin models in positional analysis of grammatical categories, a feature not found consistently elsewhere, except as incorporated by Pike in relation to the tagmemic concept. Hockett's depiction of items-and-arrangements, and items-and-process models (with mention of words-and-paradigm) is well known to linguists.

I have mentioned examples recent enough to elicit interest because of obvious relevance to current work, but someone should have the hardihood or inspiration to read the grammars before Boas, to analyze their features and trace their connections. Here can legitimately enter the evaluation of grammars, in the sense that such and such a grammar is the best of its kind, or for a certain type or group of languages, rising above methodological preoccupation to assess actual accomplishments, and to inquire into the reason: therefore. The same considerations apply to other genres and sub-genres, the text analysis, the short phonemic sketch, the analysis of a paradigm or domain, in descriptive linguistics, just as Malkiel has exemplified it for forms of etymological study. Partly it is a matter of tracing stemma amidst the seeming congeries of literature, partly a matter of asking, as we do too little, what have in fact been the best pieces of work, each of its kind, that our field has produced.

While the histories of models, genres, and sub-genres, can be discussed as matters of style, loyalty, or method, they also embody, and can be made to reveal, quite important assumptions about the nature of language and of the nature of our knowledge of it. So analyzed, the intellectual characteristics of models can be stated in abstraction from linguistic content, and one can seek parallels with ways of working in other subject matters, perhaps influence or exemplifications of cognitive trends, or personal style. Something has been indicated of this with regard to Sapir's handling of the concept of patterning. Elsewhere I have suggested a similar connection between Boas' handling of grammatical processes and his earlier conclusions as to cultural processes in regard to the elements of folktales (1961a). In Kroeber's work there is a clear affinity between his modes of working with language and with other materials

(John Rowe concurs in this conclusion: my study of Kroeber's linguistic work and his of Kroeber's archaeological work have independently brought out much the same traits). There is also such an affinity, although very different in kind, between the linguistic and cultural work of Paul Radin now (cf. Hymes 1968). In each case it is clear that the grammars were written by the men who wrote the ethnographies.

(B) Of course models for linguistic description affect or reflect conceptions of the relation of linguistic work to other aspects of anthropology. This is true particularly with regard to the scope of the models. To the extent that vocabulary and semantics are considered central, integral or marginal to description; to the extent that representative texts are considered fundamental (as in the Firth school, which adopts Malinowski's slogan of context of situation), necessary, or optional; to that extent the possibilities of relating linguistics and other anthropological work are expanded or narrowed. In the last three decades we have seen a contraction and now the beginning of an expansion.

Another shift of focus of linguistic description occurred at the beginning of the century. It is well known that Boas stressed the intensive grammatical study of Amerindian languages in contrast to the emphasis (not exclusive, of course) on vocabulary collection under Powell, and that the stress was motivated by Boas' desire for a comparative analysis of Amerindian grammatical categories.

The nature of models affects also the utility of linguistic work for historical anthropology, and time perspective has been variously incorporated into the grammars themselves, e.g., Radin's Wappo (an extreme case), Sapir's Takelma and Newman's Yokuts (more in the form of annotation). The treatment of morphophonemics in Amerindian grammars is an excellent clue to the kind of historical interest held by the author.

(C) Most of the details so far have concerned the content of grammars. The content of texts and dictionaries, as genres in linguistic anthropology, also would repay study. The traditional trinity of description, of course, is grammar, texts, and dictionary. Conceptions of the appropriate content, and mode of arrangement, have varied as has interest in obtaining or publishing them.

The salience of texts in Boas' conception of linguistic work reflects his humanistic interests, the extent to which he held the conception of anthropological linguistics as responsible for the philologies of unwritten languages. In his work and that of his first generation of students, the high place of authentic texts as documents of native culture ('memorials', 'literary monuments' in earlier phraseology) was unquestioned.

A second aspect of text collection has had to do with validity of linguistic information itself. Attitudes toward texts as a sole source of data, as opposed to supplementary eliciting of paradigms, have reflected differing conceptions of the validation of scientific knowledge. One school of thought has stressed purity of the source of data, while another has stressed the adequate testing of the generalizations to which the data lead.

As for dictionaries, there is, of course, no one dictionary of a language. From a theoretical standpoint, a dictionary is simply and generally a collection of lexical material, ordered by some principle, and, usually, accompanied by some additional information. What lexical material, what ordering principle, what additional information are the three chief vectors in terms of which dictionary differences can be defined. Any one dictionary represents current or traditional choices as to needs and goals. The history and nature of one field of linguistics, such as the Amerindian, compared to another, show clearly through the history and nature of the dictionaries in each.

(D) One genre of linguistic writing has a particularly clear relation to its corresponding genres in other branches of anthropology, and that is historical writing, in the sense of chronological and reconstructive work. It is perhaps not always remembered that there was almost no such historical writing in any branch of American anthropology at the turn of the century. There had been, of course, Dorsey's pioneering Siouan statements, and such efforts as Buschmann's in Germany on Athapaskan and Uto-Aztekan, but most 'historical' work was concerned with classification on the flat, so to speak. Relationship was more yes or no, than a matter of closer or more distant within a hierarchical framework of different levels of time depth. Nor, as Kroeber has reminded us, was archaeology much concerned with time perspective. Sapir wrote his famous paper on time perspective (1916) in something of a pioneering spirit, and criticized the work of the Bureau of American Ethnology for its lack of true historical sense. Sapir's truly epoch-making comparative studies in Uto-Aztekan, Na-Dene, Ritwan-Algonquian, and Hokan coincide in time with Kroeber's discovery of surface seriation as a technique of relative chronology for potsherds at Zuni, with the development of stratigraphy in archaeology by others, and with the onset of the heyday of reconstruction from ethnographic distributions, as exemplified in the work of Wissler, Spier, and others. We must remember, despite such labels as the 'Boas Historical School', that Boas himself was interested more in unravelling than in reconstructing history. In stressing more and more the historical, rather than comparative, evolutionary, approach to culture, he was focusing upon cultures as products of historical, rather than evolutionary processes. The processes and the products preoccupied him, not the starting points that might be inferred, although his interest in process may have

stimulated people with more diachronic minds. He almost never ventured into comparative linguistics, apparently lacking faith in its validity at any great time-depth and seems to have distrusted the efforts of the Finnish school to reconstruct proto-types of folk-tales. Historical reconstruction, as a major activity of American anthropology generally, is severely delimited in time. There are many questions to be asked about its flourishing when it did. One must also investigate the degree to which it has continued in linguistic anthropology, independently of other branches, and the reasons for its flourishing there. (Such flourishing cannot be taken for granted. There have been, and are, schools of linguistics, climates of linguistic opinion, in which reconstruction by the comparative method comes under attack.) I suspect that much of the explanation lies in a dominant Germanic inheritance in American linguistics parallel to the dominance of such an inheritance in ethnology.

(E) The genres of ethnographic writing should be mentioned here. The extent and kind of linguistic information included in ethnographic writings has varied in ways that may reflect something of the relations between linguistics and the rest of anthropology over time, as well as the viewpoints from which ethnography itself has been undertaken.

In this connection, the linguistic content of guides to travellers, explorers, and ethnographers proper, should be examined. For example, the massive kinship lists in Powell's Introduction to the Study of American Indian Languages (literally hundreds of glosses) and the section 'New Words' are of interest in their own right; the one seems to reflect Morgan's schedules, the other to contain the first use of 'acculturation' in its modern meaning. Such lists of words to be collected, together with early studies in genetic classification (the latter often determined in scope by the former) are important for the study of the history of the concept of basic vocabulary (cf. discussion of it in Hymes 1960a).

A general historical review of the primary sources of our knowledge of languages of direct concern to anthropology must someday be made. Such a review may resemble the ethnohistorical critique of ethnographic knowledge now underway, although the results are not likely to be as severe. Post-contact influences may produce the semblance of an aboriginal culture area or economic pattern, and the semblance go undetected until documents are examined and historical perspective restored, but the influences of language contact under such conditions are both more superficial and easier to detect. Still language is not immune to acculturation, and linguistic work not necessarily free from critics of its validity. Perhaps the validity and value of anthropological linguistic data which cannot be checked, the languages being extinct, will not be disputed, but

one cannot be sure. Anthropology seems to be increasingly resented by embattled humanists who identify humanism as parochial rather than human in scope. In any event, the quality of our knowledge of Amerindian languages, and of aboriginal languages in other parts of the world has been partly determined by the historical conditions in which the particular pieces of work with knowledge of the local situation and period. Such a critical study of ultimate sources would reveal much of interest to the student of the history of the field, contributing to the pursuit of many of the topics already raised, and, by providing a knowledgeable assessment, perhaps preclude an ill-formed one.

VII. Language and Culture

Of interest to the history of linguistic anthropology is the history of conceptions of the relation between language and culture. I shall mention briefly several interconnected themes.

(A) General modes of conceiving the relation between language and culture (or other aspects of culture, or society) can perhaps be placed in terms of two dimensions. One dimension is the polarity signalled by W. von Humboldt in arguing that language is not an ergon, but an energeia. In the course of twentieth-century anthropology we have seen the conception of language as an activity, hence of the unity of language with the rest of social life as residing in the unity of the act, put forward vigorously by Malinowski and adopted by Firth and the major school of modern British linguists. Malinowski explicitly rejected the distinction of la langue and la parole, so central in most European structural linguistics. De Saussure, who formulated the distinction, shares in a tradition in French thought that links Durkheim, Mauss, Meillet, himself, and today, Levi-Strauss, a tradition which sees the unit of language and the rest of social life as residing in their unity as products of the human mind, either on the general plane or in the context of a particular society. (See Mauss (1923); for an early enunciation; Levi-Strauss (1953a) for a recent statement.)

A second dimension is a continuum between the two poles of conceiving the relation between language and culture as one of correlation, or as one of integration, or subsumption. Regarding unity as products, for example, in Whorf's famous paper on "The Relation of Habitual Thought and Behavior to Language" (1941), he treats the connections between Hopi language patterns and cultural patterns primarily as correlations between two forms of activity that have developed side by side over time. In "An American Indian Model of the Universe" he writes more as if he viewed the language and cultural patterns as both expressions of a fundamental and general aspect of the culture, its world view. Regarding unity as action, Malinowski's views might seem to absorb language activity into

the act as a whole, whereas recent American sociolinguistics has typically sought for correlations and parallels between features of language and features of culture, the two being independently determined.

Together, these cross-cutting dimensions, unity as product: unity as activity, and correlation: integration, may prove adequate for placing different approaches and historical developments in relation.

(B) Conceptions of the unit or object of attention, so far as language is concerned, have been of great importance. Some aspects of the topic have been already mentioned. In one sense it is a question of what Kenneth Burke has termed 'representative anecdote': what part or aspect of language have anthropologists had in mind when speaking of 'language' in relation to culture, of two sets of data as being the 'same language', of a 'language' as being of a certain type? Sapir's remark about not confusing a language with its dictionary points to this topic. It is intimately involved in all theoretical discussions of the historical relations between language and culture, as well as in all problems arising from language classification.

(C) It is well known that languages may be classified together genetically, areally, or typologically, each mode of classification implying a different basis in historical process, and different connections therefore with culture. An important part of the history of linguistics, and of linguistic anthropology, would be to trace the relative fortunes of each mode of classification in time and space, and to examine the underlying reasons.

(D) The problem of the functions of language has been re-opened for American linguistics and anthropology recently by Roman Jakobson. No issue is more crucial for the relations of language and culture, both in theoretical perspective and professional practice. Many arguments in the history of the study of language can be identified in terms of differing views as to the nature, number, and hierarchy of linguistic functions. (For Jakobson's brilliant statement, see his "Concluding Statement: Linguistics and Poetics" (1960).) For argument as to cross-cultural variation in the functions of language, and the need for a descriptive science of same (its absence saying much about the history of linguistic anthropology), see Hymes (1962). The evolutionary origin of language, and the subsequent evolutionary development of languages, are two topics to which the topic of language functions is most obviously pertinent. In their history we see reflected much of the history of anthropological conceptions of the nature of language.

(E) One facet of the history of language-and-culture has to do with relative emphasis on the similarities or differences of languages.

One reason for dissatisfaction with Pedersen's book is that he not only says little of nineteenth century work other than that concerned with the comparative method, but that he says little of earlier work except as it serves as preparation. From a present day standpoint, the nineteenth century is not so preeminently the century of the history of linguistics (although we would do well to know more about it). So far as historical and comparative work itself is concerned, we see more of what preceded Sir William Jones' announcement, noticing the work of Gyarmathai in Finno-Ugric, early eighteenth-century comparisons by Lhuydd, even earlier predecessors such as Tolomei who stated regular sound correspondences from Latin to Italian, and Dante, who pronounced the mutability of all languages. The record of reasonable genetic hypotheses and etymologies goes back at least as far as the 12th century, and is bound up with assumptions about the unity or diversity of language stemming from theological sources.

More broadly, our interests are shifting and hence so is the history we want. First, as structuralists, we want to trace the concept of structural description, and description sui generis, honoring such pioneers as Kleinschmidt, Marsden, and the Icelandic First Grammarian; and we begin to wonder why structural description did not come into its own earlier. We would like to trace the factors working for and against adequate, inductive description of languages. Here, indeed, our habit of blaming the 'Latin model' as the primitive bondage out of which Boas led us is too simple in conception. It would seem that the thrust of work with Latin during the Renaissance and after was toward an accurate account of classical Latin, sloughing off subsequent accretions, establishing the texts, the proper dictionary, the grammar. The offense of casting exotic languages in alien (Latin) garb probably derives not so much from Latin grammars as such, as it does from the impact of Cartesian rationalism, via the Port-Royal grammars and logics, and the subsequent work in general grammar, such as that of the Englishman James Harris in his Hermes. The explicit identification of the categories of language with categories of logic and thought, imbedded in a powerful philosophic movement, probably was at least as much a force as Latin grammar per se. It was perhaps not the prestige of Latin itself, so much as the prestige of a philosophical approach whose conception of language was derived in large part from its knowledge of Latin. (Whether or not transformational grammar will unconsciously impose English rules on other languages remains to be seen.) In short, it was not Latin, but a conception of language universals, of the essential similarity of languages, that was responsible.

Second, the waves of historicism, Romantic relativism, and radical empiricism (to use some convenient tags) seem largely to have spent their force in American linguistics and linguistic anthropology, and a new tide of general linguistics seems waxing. This makes the

eighteenth century, the major preceding century of a largely ahistorical general linguistics, of renewed interest. And combined with the resurgence of psychological study of language, it makes the submerged psychological side of nineteenth century general linguistics again significant. Greenberg's concern with typology and linguistic universals, and Chomsky's grammatical theory, especially suggest the return of eighteenth century interests. (Regarding the latter, a comparison suggests itself. In the earlier period, a writer such as James Harris operated within a framework of general principles of logic and thought, equivalent to a general theory of semantic structure, while the modern school operates within a framework of general principles of formal structure, eschewing semantics. Yet in dealing with the details of a particular language, English, the eighteenth century writer, Harris, phrased in semantic terms what were actually adjustments to the language's formal structure, while modern transformationalists seem sometimes to phrase in formal terms what are actually adjustments to the language's semantic structure (to be in effect explicating Whorf's concept of cryptotype). If correct, the comparison may point to one of the implicit boundaries within which grammatical writing moves historically.)

It must be remembered that all schools of linguistics have held some assumptions about universal features of language, or language change, and that during the heyday of emphasis upon structural diversity in American linguistics and anthropology, the Prague school of Trubetskoy and Jakobson was stressing the search for necessary laws and universal dimensions.

Central to an historical understanding of the matter is the way in which different schools of thought have conceived the relation between structure and function. Put in simplified fashion, the nineteenth century typologies, interpreting linguistic form in cognitive, sometimes evolutionary terms, might be said to have focused upon both structural and functional diversity, the two implying each other. The position of many American linguists and anthropologists in this century might be taken as emphasizing structural diversity and functional similarity (all languages are equal, serve equally well the needs of their cultures, all can express anything, are probably equally complex, etc.). General grammar, and Chomsky's conception of language, might both be said to emphasize structural similarity, certainly as a basic supposition. I am not sure of the connections with views of functions, but suspect it may prove that the assumption of uniform structural framework in general grammar was compatible with emphasis on functional diversity, conceived as differing degrees of development of, or from, the common base. The present trend seems to link cross-language similarity both in basic structure and in function. The import of a view of course depends much upon the ways in which structure and function are specified.

*These four possibilities can be put schematically as follows:

	A	B	C	D
Structural	-	-	+	+
Functional	-	+	-	+

similarity = +, diversity = -.

Such orientations are as much a matter of taken-for-granted climates of opinion, perhaps, as of explicit findings, and may reflect social and political conviction easily enough, as well as methodological convenience.

(F) One further way in which the relations of language and culture have figured in the history of anthropology concerns conceptions of the general relations of the sciences, and/or their subject-matters. An 'integrative levels' point of view has recurred in anthropological thought, seeing linguistics, or language, as that branch of the sociocultural realm closest to the biological or natural sciences. Some have seen linguistics as a medium for the extension (or encroachment, depending on viewpoint) of natural science methods into the sociocultural realm, while others have seen linguistics as warrant for the development in the sociocultural realm of scientific methods which are not those of the natural sciences. One's sense of continuity or contrast of course depends upon one's picture of each side, and such images have varied, both in accuracy and in objective historical content.

*(G) Finally, reflection on linguistics is in effect an exercise of one of the elementary functions of human language, the metalinguistic function. The various forms and stages of linguistics are instances of the shape taken by the metalinguistic function in various societies and types of culture. To reflect on linguistics in relation to anthropology is to reflect on the highest form of the problem of the relation between language and culture. Comparative and historical understanding of the relations between linguistics and anthropology in different national cultures, regions and language traditions, and different eras of human history, is not a parochial concern, whose significance is limited to a handful of scholars. It is an anthropological problem of the highest order, if one takes an evolutionary perspective on human culture.

VIII. Other Disciplines

All aspects of the relations of linguistics and anthropology are of course affected by the historically varying relations between linguistics, anthropology, and other disciplines. I shall try to illustrate some of these.

(A) Folklore: The close connection of linguistics, ethnology, and folklore in the nineteenth century is well known. One can mention Max Müller's theory of 'etymological disease' as an explanation of mythology, Brinton's library of aboriginal American literature, and the interrelations of historical theory in linguistics and folklore, e.g., vis-a-vis the Stammbaum and Wellentheorie interpretations. The roots of these connections in the Romantic movement and Herder's work are well known. In American anthropology the source has especially been in the collection and interpretation of texts as authentic documents. It is worth noting that Boas worked out his conception of historical process, as it affects cultural materials, in his studies of folktales, and that the expression of his views on language classification follow the folklore work in time, perhaps also in principle. If so, Boas would have been assuming a greater unity of language and culture with respect to regularity and rate of change, than Sapir and most American anthropologists in this century. The connection between folklore and linguistics has been weak in recent years, in American scholarship, following the breakup of the old philological mold, but is being renewed through interest in the application of linguistics-like structural principles to the analysis of myths, *(and now to the analysis of performance).

(B) Philosophy: Cassirer's writings are of great value here, especially "Structuralism in Modern Linguistics," (1945), and Philosophie der Symbolischen Formen: Die Sprache (1921); translated by Ralph Manheim, the Philosophy of Symbolic Forms, vol. 1: Language (New Haven: Yale University Press, 1953). For the most part, American linguistics and anthropology have eschewed much public interest in philosophy, although occasional citations occur, recording influence or moral support. The actual mutual influence, indebtedness, has undoubtedly been great, but will not be easy to trace. My impression is that the leading American linguistic anthropologists of recent times have been conscious of relations to philosophical positions.*

(C) Biology: The controversy over Schleicher's effort to 'Darwinize' linguistics in the nineteenth century, and Darwin's citing of comparative philology as a precedent for family-tree descent, are notable earlier examples. Van Ginneken and recently Darlington and Brosnahan, have tried to link biological traits and sound-change. Some have seen linguistics as a bridge between the biological and (other) cultural realms in the hierarchy of sciences. An evolutionary perspective seems prerequisite to mutual involvement of linguistics and biology; the renewed interest in the origin of language, and the comparative study of animal communication, are part of the general renewal of evolutionary perspective in

*cf. the appeals to logic, philosophy of science, phenomenology, hermeneutics, in more recent years.

anthropology. The relation of biological properties to evolution of languages is stressed in a causal way by Martinet, analogically by Gerard, Kluckhohn and Rapoport (1950) to cite contemporary examples of the two main types of link.

(D) Psychology: Steinthal's efforts to link linguistics and psychology; including the founding of a journal with Lazarus, one regarded by some as the birth of social psychology; Wundt's folk-psychology and its influence on the early Bloomfield; behaviourism and its influence through Weiss on the later Bloomfield may be cited here. (A paper of interest here is Alkon (1959), and also Delacroix's earlier historical review (1933).)

There seems little to trace in relation to psychoanalytic thought. The interest of Devereux (1949), and such occasional papers at Sapir's and Frachtenberg's on abnormal types of speech have been exceptional. Despite Freud's own great interest in linguistic phenomena, the impact of his ideas on other parts of anthropology has not been matched in linguistic work. Indeed, Boas early cited language, from the viewpoint of his interest in grammatical categories as unconscious habits, specifically in opposition to the relevance of psychoanalytic thought (1920.320):

"If, however, we try to apply the whole theory of the influence of suppressed desires to the activities of man living under different social forms, I think we extend beyond their legitimate limits the inferences that may be drawn from the observation of normal and abnormal individual psychology. Many other factors are of greater importance. To give an example: The phenomena of language show clearly that conditions quite different from those to which psychoanalysts direct their attention determine the mental behavior of man. The general concepts underlying language are entirely unknown to most people. They do not rise into consciousness until the scientific study of grammar begins. Nevertheless, the categories of language compel us to see the world arranged in certain definite conceptual groups which, on account of our lack of knowledge of linguistic processes, are taken as objective categories and which, therefore, impose themselves upon the form of our thoughts. It is not known what the origin of these categories may be, but it seems quite certain that they have nothing to do with the phenomena which are the subject of psychoanalytic study."

(E) Sociology: The greatest importance of sociology for linguistic anthropology would seem to reside in the impact of Durkheim on de Saussure and Meillet. Meillet expressly aligned himself as a follower of Durkheim, language being the social fact par excellence, and the celebrated cornerstone concept of European

structural linguistics, la langue: la parole, is at least partially indebted to Durkheim's sociology. (Doroszewski argues that de Saussure's conception was an attempt to reconcile the theoretical emphases of Durkheim and Tarde (1933).) The closest link between sociology and language would seem to have continued to be in France during this century.

A narrative and critic history of the varying relations of sociological and linguistic thought would be of great interest to the current emergence of new interest in the sociology of language. Durkheim's Elementary Forms of the Religious Life (1912), and A. R. Radcliffe-Brown's A Natural Science of Society (1957), must be placed, for example, in the genealogy of Whorfian interests in this century.

The parallel in time and thrust between the movements for autonomous study of culture (Lowie, Culture and Ethnology (1917)); Kroeber, "The Superorganic" (1917); of society (Durkheim), and of language (de Saussure, Sapir) is noteworthy.*

(G) History: Explicit connections between linguistic anthropology in the United States, and history as a discipline, are hard to find, although early common roots of linguistics and history in Europe are apparent (Vico, Herder, Schläger, Mommsen, for example). Clearly however, one must take account of Kroeber's interest in Rickert and Windelband, his express denomination of the Handbook of the Indians of California as a history, etc.; and try to assess the place of historicism as a contributor to Boas' conception of the tasks of ethnology and linguistics. As time perspective on the development of linguistics and anthropology deepens, of course, the web of relations with the development of history thickens.

(H) Theology: The influence of theology on early conceptions of the origin and history of languages; on the missionary activities which are responsible for a good deal of our knowledge of languages; and, occasionally, on conceptions of the structure and function of language, may be noted.

IX. Afterword

No adequate general history of anthropology, or of linguistics, exists, and there is none at all for linguistic anthropology. Almost every thing remains to be done. I would argue that the writing of

*The relevance of these views earlier in the century to contemporary critical thought is indicated in Hymes (1979). Obviously this section was written before the flowering of sociolinguistics/ sociology of language since 1964.

the history of linguistic anthropology should be done, on three counts: (1) its utility, among other such histories, as a series of cases for comparative study of the development of scientific and scholarly disciplines; (2) its significance for the general history of anthropology; (3) its importance to ongoing work in linguistic anthropology itself. Hopefully, the study of the history of linguistic anthropology can benefit and contribute with respect to each.

For extensive bibliography on all aspects of linguistic anthropology, with considerable attention to historical perspective, see my forthcoming Language, Culture, and Society: An Anthropological Reader (New York: Harper and Row, 1964). The following notes and references, which include works not otherwise fully identified in the paper, may be useful.

On the history of linguistics generally, no really satisfactory study is available, but a number of relatively adequate and accessible accounts can be consulted, such as Arens (1955); Benfey (1869); Bloomfield (1914), Ch. 10; Carroll (1953), pp. 15-23, 246-268; Cassirer (1923), Ch. 1 (117-176 in the translation of 1953), (1944), Ch. 8; Gray (1939); Jespersen (1922), Book I; Kroeber (1944), Ch. 4; Meillet and Cohen (1952) xvii-xlii, esp. xl-xlii; Pedersen (1931); Sandys (1903, 1908); Steinthal (1863); Thomsen (1927 (various editions)); White (1896), Ch. 17. Accounts more limited in scope are also found in Allen (1949, 1953); Brough (1951); Bloomfield (1933), Ch. 1; Cassirer (1945); Emeneau (1955); Grundriss ... (1916); Meiller (1934), Appendix; Robins (1951, 1958); Verburg (1949, 1952); T. A. Sebeok, ed. Profiles of Linguists (tentative title -- in preparation) (see now Sebeok (1966)) will be a valuable resource. In general, one can consult obituary articles in journals, and the section on the history of linguistics in the volumes of the Linguistic Bibliography.

On the history of linguistic work in anthropology, there are discussions of varying scope and value in Boas (1904); Kroeber (1950); Levi-Strauss (1949, 1953, 1960); Penniman (1952) 195-204, 435; and with particular reference to American anthropology, also in De Laguna, ed. (1960) 380-383; Greene (1960); Hallowell (1960) 23-34; Kroeber (1939); and Wissler (1942).

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LEXICOSTATISTICS AND GLOTTOCHRONOLOGY IN THE NINETEENTH CENTURY (WITH NOTES TOWARD A GENERAL HISTORY)

0. INTRODUCTION

The terms 'lexicostatistics' and 'glottochronology' are both less than a generation old, and one commonly thinks of the subjects as being equally new. It is therefore surprising to discover that a form of glottochronology was devised more than a hundred years ago by the great French anthropologist, Paul Broca, and that Broca built upon a form of lexicostatistics devised by the noted French explorer, Dumont d'Urville. The latter had capitalized upon an idea of Constantin Rafinesque, poet, supposed lost Dauphin, finder or forger of the Delaware Indian *Walam Olum*, a man of both genuine and spurious claims to scientific repute. Broca invented his glottochronology for an address to the Société d'Anthropologie de Paris;¹ behind it lay two Pacific expeditions, vocabularies from French exploration under Dumont d'Urville and genealogies by Horatio Hale from a U.S. Exploring Expedition under Wilkes. Behind Dumont d'Urville's lexicostatistics lay a prize essay sent across the Atlantic to Paris from Rafinesque's residence, Philadelphia.

Dumont d'Urville's lexicostatistics, and Broca's glottochronology, may be especially appropriate to this volume, since the languages of concern are those of Oceania, languages which have been central to the main line of advance in the subject in recent years, through the work of Isidore Dyen. The circumstances involve the general history of linguistics and anthropology in the nineteenth century, specifically the 'ethnological question', which, Stocking (1969) has stressed, was central to the period, yet has been badly slighted by modern scholarship. A sketch of one unsuspected aspect of pursuit of the 'ethnological question' may add something to our understanding of it (see especially 4.1). The sketch may also enhance our understanding of lexicostatistics and glottochronology, by helping us to see them in a longer perspective of time.

¹ I discovered the Broca address in the Art-Anthropology Library, University of California, Berkeley, in 1962, and want to take this opportunity to express my appreciation to Robert Pfeiffer for making that library so useful to scholarship. The material relating to Dumont d'Urville and Rafinesque was found while a Fellow of Clare Hall, Cambridge, in the libraries of the British Museum, Cambridge University, and the Scott Polar Institute, whose director I should especially like to thank for discussion of Dumont d'Urville. Regna Darnell made an initial translation of Broca's address; I am responsible for the present form of the portions quoted here.

A full history of lexicostatistics and glottochronology is not now possible, preparatory studies having hardly begun. Yet even an initial glance discloses that the history of these two subjects comprises the histories of several contributory notions and practices, and that these histories, partly independent, partly interconnected, extend back into the early periods of serious concern with the relationships of languages. I have taken advantage of this occasion to begin with some account of these histories, in order to suggest their interest. Ideas as to linguistic change and language relationship have had a significant part in the general history of ideas more than once since the Renaissance, and may yet again; the historiography of lexicostatistics and glottochronology is relevant in this respect as part of the general history of the study of man. Observations on the present standing of these subjects are relevant, because their history is not over. The work of Dumont d'Urville and Broca is a part of a history that has a future as well as a past. Lexicostatistics and glottochronology, in the nineteenth or twentieth century, are subjects with implications that reach to the foundations of linguistic theory, but their cultivation has mostly been a matter of convenience and practical need. The several histories that come together in what we now understand as these subjects lead into the future, partly just because their theoretical and methodological implications remain to be fully explored.

1. TOWARD A HISTORY OF LEXICOSTATISTICS AND GLOTTOCHRONOLOGY

The story of a nineteenth century French glottochronology is told here not only for its own interest, but also with an eye to the present. Such an approach frequently is called 'the Whig interpretation of history', but that term more precisely means history written from the standpoint of victors, and lexicostatistics and glottochronology can hardly be said to have conquered; indeed, they barely survive. The history of linguistics is mostly written from a Whig-like vantage point, but that entails that the nineteenth century is told essentially as the triumph of the comparative method, neglecting any sympathetic or insightful treatment of general ideas about language or of some of the reasons why the study of language was pursued. Every period perhaps may be characterized by its way of considering the unity of language and mankind; in the nineteenth century as in the eighteenth, such consideration included the origin of language, with the addition of a typological approach, and of an ethnological approach, to the relations of languages among themselves.

Aarsleff (1969) has described the origin of language tradition, so important in the eighteenth century, and several writers have discussed the typological tradition, so important in the nineteenth century; both traditions, while having a temporal aspect, essentially have been concerned with the inner nature of language. The ethnological tradition has been concerned essentially with languages as evidence of human history, and it is to this perennial concern that our present subject most belongs, although ultimately it may come to involve questions of the nature and types of language, as disclosed through explanation of rate of change, as well.

Morris Swadesh – to all intents and purposes the inventor of lexicostatistics and glottochronology in our time, and the one to launch them on their present course – recognized that several of their elements have had a prior history. The fundamental notion is that diversity among dialects and languages is a function of time. The history of this notion and of the varying views associated with it, ties linguistics closely to the general history of ideas. Perhaps as old is the notion of basic vocabulary, and it is of course the coming together of these two notions, the use of basic vocabulary as an index of diversity due to time, that constitutes the core of what we understand as lexicostatistics and glottochronology today. Lexicostatistics and glottochronology have also become associated with techniques of grouping dialects and languages, through the use of (tables of) percentages, and with efforts to demonstrate relationship when, due to remoteness in time or scarcity of data for other reasons, lexical resemblances have to be assessed without benefit of sound correspondences or grammatical comparison. All of these have a nineteenth century history, and at least the first two a history older than that.

1.1 *Diversity a Function of Time*

Swadesh cited Sapir's time perspective monograph of 1916 for the idea that diversity in language is a function of time (1949 [1916]: 452): "The greater the degree of linguistic differentiation within a stock, the greater is the period of time that must be assumed for the development of such differentiation."

The idea, however obvious in its general form, had a strategic significance in the anthropology of Sapir's day (cf. Sapir 1917:190). The issue was to gain acceptance for the idea as not only a truism, but also an explanation, in cases of less than obvious relationship. There was prevalent a tendency to assess linguistic relationships 'on the flat', as it were, without an articulated time sense, and to think of new relationships as 'reducing' the absolute number of units. In point of fact, of course, discovery of a relationship does not reduce, but adds; the previously known units remain distinct, and to them is added an inferred earlier point of reference, the source of certain of their resemblances. Albanian did not disappear upon the recognition of its Indo-European heritage. Yet in the period in which Sapir wrote his time perspective monograph, some anthropologists published lists of words apparently NOT related between languages, as if a claim of relationship required accounting for all of vocabulary, for what was not related genetically as well as what was. From a broader perspective, to be sure, non-inherited differences are of interest, and one can wish to explain the history of languages, or vocabularies, in full, as Boas (1929:1) maintained, not just the portion, often quite small, that attests a genetic connection. Genetic connection is nevertheless prerequisite to study of the full history of languages, and the explanatory power of the accumulation of diversity over time has had to be established in anthropology in order to permit the contribution of genetic classification to the larger study to go forward.²

² See Swadesh (1951), a milestone in this regard. The general need to calibrate differentiation with

That languages differ in proportion to the distance of time that separates them had been stated by William Dwight Whitney (1896 [1875]: 33) well before Sapir, and the idea is perhaps as early as historical comparison of languages. Herodotus had something of the sort to say in passing with regard to a particular case, and the notion played a significant part in the reasoning of scholars from the sixteenth century on. What distinguishes the present period is not the stated notion, but unstated presuppositions. In the earlier history of comparison one may indeed find sober, trenchant use of the notion, notably by Johannes De Laet (1582-1649) in his refutation in 1642 of the theory propounded by the eminent Hugo Grotius, that most North American Indians were descendants of Norwegians (see Metcalf 1969). De Laet showed that the relationship of the language of the Anglo-Saxons to that of the Norwegians was still evident in many clear cognates, although the Anglo-Saxons had migrated to Britain before the presumed migration of the Norwegians to the new world; the great diversity of American Indian languages could be explained only by a common origin so early in time as to exclude the Norwegian hypothesis. Indeed, De Laet concluded, America must have been inhabited since shortly after the confusion of tongues and the dispersion of peoples, to account for the number of its inhabitants and their diversity.

More commonly, however, diversity was associated with notions of change as degeneration and of the exemption of certain languages from such corruption. In his *Mithridates* (1555) Konrad Gesner (1516-1565) was shrewd enough to suggest that Psammetichus' experiment to determine the original language, by having two infants raised in isolation, probably resulted in their uttering, not the Phrygian word for 'bread' (*beccus*), but an imitation of the bleating goats who had nursed them. He was also very clear that Hebrew was the first and most ancient of languages, and that change and corruption were essentially synonymous (Metcalf 1963b; cf. Metcalf 1963a). Dispute tended to concern, not the assumption of differential antiquity of languages, but the identity of the original or most ancient language, as when Johannes Goropius Becanus (1518-72) maintained that the original language had indeed survived Babel, but was Dutch, not Hebrew. Abraham Mylius (1563-1637) understood a number of basic points about linguistic relationship in his *Lingua Belgica* of 1612, such as the need to exclude chance and onomatopoeic convergence, and to distinguish common

time depth is the principal way in which lexicostatistics and glottochronology implicate the foundations of linguistic theory. The explanation of resemblances at remote time depths of course cannot be exclusively in terms of genetic retention. Even when the relationship is detected at a stage beyond which any grammatical evidence remains, the resemblances found in basic vocabulary are not necessarily specific to the particular comparison, but may be present due to borrowing, perhaps from a pool of areal persistence of a shape, or even due to separate retentions from a stage parental to all or most known languages. (See Swadesh 1960a for examples of forms whose presence in a language may have such an explanation). From the very possibility of indefinite retention of certain shapes within the field of basic vocabulary meanings, it follows that very low percentages of resemblance in basic vocabulary and a fortiori, very low differences in low percentages, cannot easily be used to distinguish a special relationship for the languages compared. An ancient shape may recognizably occur in the basic vocabulary of a language by way of a variety of routes.

inheritance from borrowing. Change, however, was relative to language family, Hebrew perhaps being excepted from change, as the original language, together with his own Belgian, which he considered essentially like the ancient Teutonic tongue. (Mylius thought to demonstrate the point by noting how little change there had been between a Brabant charter of 1312 and contemporary speech). Justus-Georgius Schottel (1612-72) shared Mylius' views that Hebrew was the original language and German a language of unique antiquity and purity of preservation. Not all change was degradation for Schottel, only that which involved departure from fundamental structure, which he defined in terms of the morphological structure of the word. Like the others noted here, however, Schottel was far from the uniformitarian view of change that a later age would demand.

One might regard the early nineteenth century as a sort of intermediary stage. A founder of comparative linguistics such as Rask saw languages as undergoing disruption when they moved from one stable state of structure to another; Bopp and Schleicher were not strangers to the idea that an earlier state of Indo-European (which they sought to reconstruct) had been as it were perfect, and the loss of inflections in daughter languages was a form of decay. A conception of change as different for different broad categories of languages was expressed in various ways by other writers, e.g., Johnes (1846:88), who observed that the Gothic and Celtic languages of today differ almost totally, and that their branches could come to differ by causes of the same change at the same rate, so as eventually to obscure their former unity almost entirely; but on the other hand,

it would be highly erroneous to infer that the rate of change previous to the commencement of the Historical period was the same as it has been since; it must have been much more rapid! Changes of this nature are prompted by the dictates of convenience, which suggest the extinction of superfluous words, and the appropriation of the remainder to distinct though kindred purposes; names for 'Water, Rivers, the Sea', for example, were doubtless in the first instance applied indifferently to all those objects. Now, inasmuch as languages are more redundant in their earlier than they are in their later stages, it is apparent that these changes, of which this redundant character is the source, must be more rapid.

Pre- and non-historical languages, in effect, are here considered 'polymorphous', as they were judged 'polysynthetic' by other writers.

In one form or another, then, the nineteenth century accepts change as universal and normal,³ and poses the issue of relativity in the rate of change, not as between one or a few Old World languages and the rest, but as between two classes of language, the 'historic' or written languages, especially of Europe, and the unwritten languages of the American Indians, Oceania, and elsewhere.

The nineteenth century saw the triumph of a uniformitarian approach to change in geology and other fields, and although ethnocentric notions about 'primitive languages' have persisted well into the twentieth century, even among scholars (cf. Hymes 1964:82), the latter nineteenth century saw the establishment at least

³ In the words of one observer, "For nature is constantly changing" (Gilbert 1875).

within anthropology of the principle that unwritten languages did not change more rapidly than other languages. The principle is implicit in the view on diversity cited from Whitney above, and the views of Latham (1850) on the rate of change, to be discussed below. Various pieces of empirical evidence were presented by a variety of writers within a period of about twenty years; cf. Hale (1883), Brinton (1885; 1890 [1887a]; 1887b), Powell (1891: 141), E.E. Hale (1903: x), Codrington (1903). Brinton (1885: 95-97) for example, compared three writers, Campanius (Swedish), Zeisberger (German), Whipple (English), who had recorded Delaware at about equal intervals over a two hundred year period (1645, 1778, 1855) in the orthographies of their own tongues; he used a basic word list of some 32 items. Elsewhere he maintained that "anyone who has carefully compared the earliest grammars of an American tongue with its present condition will acknowledge that the changes are surprisingly few." Continuing immediately, "To me the exceeding diversity of languages in America and the many dialects into which these have split, are cogent proofs of the vast antiquity of the race, an antiquity stretching back tens of thousands of years" (1890 [1887a]: 35; cf. 1887b). In his introduction to Trumbull's working up of Eliot's 16th century Natick (Algonquian) dictionary, E.E. Hale reported that in 1899 he had presented a thirty word vocabulary to a Chippewa boy, who had recognized fifteen of the words and puzzled out most of the rest, from which Hale concluded that "In the course of two and a half centuries the uses of words differ as much among Indians as among white men, but it would seem that they do not differ more" (1903: x).

The ready acceptance of such tests suggests that the conclusion was needed and desired, as a stepping stone, and once accepted by the community of scholars, not in itself of further interest. A uniform rate of change, with an emphasis on gradualness of change, was deemed essential to use of American Indian languages as keys to the history of the American Indian. A few informal instances and some reasoning by analogy would suffice.

The situation is not much different today. Efforts to group languages according to differential degree of resemblance, of shared features, necessarily assume that the languages have diverged with regard to the features in question at a uniform rate. Otherwise, of three languages equally closely related, two showing a degree of sharedness quite different from a third, might imply a special grouping that was false. Yet there has been little concern to establish the principle empirically, or to go beyond analogy (e.g., "about as different as Italian and French") in its application.

It is because of this situation that 'glottochronology' appears to stand apart, as a not quite reputable aspect of linguistic practice and theory. That diversity is proportional to time, and that the relation is uniform in all languages, a linguistic universal, are commonly assumed in practice, but not professed or assessed.⁴ The effort to test and measure the relation is so marked as to be seen, not as a development, overdue,

⁴ When an interesting case is reported, as in Hall (1959), it is presented to impugn glottochronology, not as a contribution to a more adequate general understanding of rate of change, and, in the case in question, as possibly a vital criterion of a type of language change (completed pidginization).

out of a long history of linguistic concern with change, but as an intrusion with a separate name. Where explicit analysis of rate of change is concerned, the implicit motto would seem to be 'Let sleeping dogs lie'. The concept around which modern efforts to test and measure rate of change have centered, basic vocabulary, has wide acceptance, that it has a history is more widely known, but investigation of its history and nature remains almost as dormant.

1.2 *Basic Vocabulary*

The notion of a sector of vocabulary, common to all languages, and especially criterial of relationship, is at least as old as the first half of the seventeenth century. The origin and vicissitudes of the concept have yet to be adequately traced. It was used in a concrete form by De Laet in his refutation of Grotius in 1642. De Laet specified that comparison must involve not any or a few words, but above all "the names of those things which are domestic and most common to that nation: for otherwise it is not difficult to find words in all languages agreeing to some extent with other languages" (quoted from Metcalf 1969: 36). He specified parts of the body, numerals, and the method of counting, close family relationships, and certain geographical terms (*hill*, etc.). The notion of basic vocabulary has been noted in Rudbeck (in 1675) and Ludolf (in 1702) (see Diderichsen 1972), and Leibniz in 1698 (see Aarsleff 1969: 188). The notion was utilized by Rasmus Rask early in the nineteenth century (cf. Wells in this volume) and recurs throughout the century, e.g., Warden (1825, 1834), Dumont d'Urville (1833), Johnes (1846), Latham (1859), Koelle (1854), Brinton (1891). Statements of principle vary, but the contents of the lists remain strikingly akin. Johnes (1846: 8-9), for example, writes:

In determining the mutual relations of different languages, it is obviously not necessary to compare the whole of their component parts. [In addition to the general composition and structure of languages, one needs the words:] "fire, sun, day, eye, moon, heaven, human being, man, woman, hand, arm, foot, leg, ear, tongue, head, water"... "Terms for the objects above enumerated will be found to include the greatest portion of the primary elements of all languages".

The remarkable missionary-linguist, Koelle, (whose contributions are now being honored a century later by a series of articles in the *African Language Review* through the initiative of its editor, David Dalby), wrote:

In the selection of the words which were to compose the Vocabulary I was guided by the desire to take those only which are most easily understood by the natives – names of the visible objects at which I could point – avoiding all abstract words. I think that in most of the words I have chosen the affinity of languages is especially likely to exhibit itself, so that any two given languages appear from this Polyglot to be rather more closely allied than if we could compare their whole glossarial treasure, but never *vice versa* (iv).

Koelle's extensive list of 281 glosses contains 44 verbs, 41 names of animals, 39 body parts, 23 vegetable terms, 22 adjectival terms, 21 terms for materials and objects,

20 terms for kinds of person (kinship, social role), 20 numerals, 15 terms for material equipment, 10 terms for natural phenomena ('sun, smoke, rain', etc.), 7 religious terms, 6 terms for clothing, 3 terms of time, 3 medical terms, 2 terms for places, and 1 for the activity of war. Sixty of these terms are found on the Swadesh 100-item list. (Those on Swadesh's list and not used by Koelle are 'person', 'name', the 7 pronominal forms, 7 body part terms [flesh, grease, horn, tail, feather, heart, liver], 6 natural terms [star, earth, cloud, ash, pitch, mountain], 'seed', 'bark', 'louse', 8 adjectival terms [red, green, yellow, full, round, all, many, long], and 7 verbs [bite, know, swim, fly, walk, stand, burn]).

As part of a case AGAINST a relationship, that of Beothuk to Algonkin, Gatschet (1886) mentioned differences of phonetic system, a difference in case system, complete difference of numerals (which he considered decisive), and "The terms for the parts of the human and animal body, for colours (except *white*), for animals and plants, for natural phenomena, or the celestial bodies and other objects of nature, as well as the radicals of adjectives and verbs differ completely" (quoted from Hewson 1969: 86). Insofar as Gatschet did not intend to expatiate upon the two vocabularies as wholes, his specifications of domains, and the order in which they are given, as "The facts which most strongly militate against an assumed kinship", indicate that he reckoned with all aspects of language, but especially case inflection and basic vocabulary, of which numerals, body parts, colors, and "natural phenomena", such as 'sun, moon, star' had precedence. In short, he had some working notion of basic vocabulary as criterial. (It is worth noting that Hewson so interprets this fourth objection, as taking into account in each subheading "a basic area of vocabulary" (1969: 91); Hewson is able to contravene Gatschet's several arguments, including that as to absence of resemblance in the basic vocabulary, citing comparisons for the numerals 'one' [two forms], 'five, six', and 'nine', and for 'nose, back, eye, nail, hand/finger, eagle, fish, tree, gooseberry, sun/moon').

In his general classification of American Indian languages, published in the same year as the famed Powell classification of Indian languages north of Mexico, Brinton indicated his use of a selected word list most likely to reveal relationships; he employed it especially for South American languages for which the morphological date he preferred in establishing genetic relationship was lacking. The list is essentially the same as the 32 item list used in his observations on the stability of Delaware, but shorter by some eleven items. Whereas the 1885 list comprised: 'man, woman, father, mother, head, hair, ear, eye, nose, mouth, tongue, tooth, hand, foot, heart, house, pipe, sun, star, fire, water, snow', and the numerals 1-10, the 1891 list omits 'father, mother, head, tongue, heart, pipe, snow', and the numerals 6-10, and substitutes 'moon' for 'star'. The omissions, if not due entirely to practical reasons of the availability of data, suggest some of the same factors as were to be involved in Swadesh's revisions of his test list ('head', 'hair' not always distinct, 'snow' not universally found, kinship terms not as stable as body part terms, etc.).

One clue to the full history of the notion of basic vocabulary, its exemplification in

lists of terms, and the purposes and outlooks with which it has been associated, lies in the comparative vocabularies and guides to the collection of vocabulary devised in the eighteenth and nineteenth centuries (some points are noted in Hymes 1960a, 1960b). In any case, the concept of basic vocabulary, as specially indicative of relationship, had become part of general intellectual culture in the United States by the end of the nineteenth century. Such at least is the implication of the occasion and audience of remarks by one Erasmus Darwin Preston (1900)⁵, concerned, indeed, with the languages that will figure with regard to Dumont d'Urville and Broca. Hailing Müller as discoverer of the Oceanic family, Preston goes on to argue against the theory that each dialect had an indigenous base, that which is common to all having been introduced through commercial intercourse, in these words:

In the first place, the connection was far too slight to produce the effect mentioned, and in the next place the common words are not those ingrafted and absorbed by intercourse, but such as are in every known language – the oldest and the commonest. When the Normans came to England they introduced many words; but they could not displace those simple names of natural objects as sun, moon, etc., nor those indicating family relationships, such as father, mother, brother, sister – terms always dear to the heart of humanity and jealously guarded against foreign intrusion and corruption. Just so in the Pacific. The words common to all branches of the group are those which from the very nature of things must have been in use from time immemorial. The fact alone explodes the theory that the dispersion of terms was the result of intercourse.

In the twentieth century the role of basic vocabulary was acknowledged even in the course of emphasis on grammatical criteria of relationship by the great French comparativist, Meillet, and is accepted as criterial and fundamental by a leading contemporary scholar, such as Hoenigswald (1971). Indeed, the notion and its use for the particular purpose, seem to have become so ingrained in American anthropology in this century that a leading ethnologist, writing before 'glottochronology', felt constrained to justify departure from common practice (Spier 1946: 5):

Comparative vocabularies of primitive languages are customarily secured only in order to establish genetic connection. Having this aim, they are usually restricted to words for parts of the body, natural phenomena, and the like, on the quite reasonable assumption that these would be least affected by differences in culture. But it is obvious that if the aim is to reveal more precisely the degree of resemblance, with the dissimilarities weighed equally with the identities, such restricted lists are inadequate. For this purpose it is essential that all parts of the vocabulary should be represented to much the same extent; for where words for body parts and the like may be expected to show a high degree of resemblance, it may well be that names of things (implements, rituals, etc.) will show great divergence though the items themselves be common to the culture of the two groups. Hence it is desirable to obtain random lists, sampling all phases of activity and culture, in which the several specialized parts of the vocabularies will be effectively balanced.

⁵ Preston's lecture is not technical, but a series of mostly personal observations, including one on the use of Hawaiian Pidgin in official business. I have no information about Preston and his background.

Spier's focus on random lists reflects problems of whole-culture classification prominent in the ethnology of his day, but the question of degree of resemblance, and of relations within an area, suggest a general approach. Lists sensitive to differences might be used to study cultural adaptation.⁶ Kind of difference, as well as degree, might be studied, initially with lists specialized to a region, activity or problem (ecological niche, social field, etc.). Such research would be complementary to the investigation of basic vocabulary, as part of a general study of lexical adaptation, persistence and change. Indeed, it might be said that the concept of basic vocabulary cannot be fully clarified without research to clarify persistence and change in 'non-basic' vocabulary. Neither side of this distinction has been much investigated, however, apart from work directly stimulated by lexicostatistics and glottochronology (e.g., Kroeber 1963).

In sum, the concept of a sector of basic vocabulary is of long standing in linguistics, widely used, with considerable concurrence over time in the kinds of meanings involved. The concept has been empirically validated many times in linguistic practice. Proposals to clarify the theoretical basis of such empirical validity, to refine the implementation of the concept through test-lists, or in general to go beyond tacit acceptance to explicit testing and measurement have had little general acceptance. In some instances clichés of criticism have been repeated in ignorance of subsequent work that has answered them. The value of the lists devised and revised by Swadesh has been demonstrated by their frequent employment, but as a received instrument, to be used and/or criticized as given. Apart from a very few scholars (e.g., Douglas Taylor, and notably now, Oswalt 1971), use of the lists has not been regarded as part of a continuing concern on the part of the community of linguists to refine and amplify their use, and to understand better their nature.

1.3 *Tables of Percentages*

Lexicostatistics is often identified with the use of tables of percentages. Strictly speaking, such tables are not glottochronology. They can be, and have been used to group dialects and languages without explicit reference to chronology. Chronology, as we have seen, is implied in the interpretations usually put upon some tables, as to closeness and distance, or even relationship and non-relationship; but in principle such tables can be a measure of lexical agreement of whatever source, genetic, diffusional, or other. If the lexicon involved is basic vocabulary, and especially if it is a standardized test list of basic vocabulary, then of course one has the use of lexicostatistics to explore and even to demonstrate genetic groupings, best known today through Swadesh's work. The history of the uses of lexicostatistics is poorly known, but something of its outlines can be indicated.

The use of comparative lexical tables dates from at least the mid-eighteenth century. The first uses known to me are reported by Dumont d'Urville (1834a: 263-64) in his

⁶ Cf. now Mayers (1961), who has used the Swadesh 100-item list to show minimum difference between informants and a 150 word list intended to show maximum difference between informants.

review of the prior history of the question of the relations of the languages of Oceania, Malaysia and Madagascar:

Forster, le premier,⁷ dressa un tableau où il plaça en regard quarante-sept mots pris dans onze des dialectes de l'Océanie, et leur expression en malaïo, mexicain, péruvien et chilien [Presumably Aztec, Quechua and Araucanian]. De ce tableau, il résultait déjà que les dialectes océaniens offraient de nombreuses analogies avec le malaïo, tandis qu'ils ne présentaient aucun point de contact avec les langues américaines.

Après lui, Anderson, dans un tableau comparatif qui fut publié à la suite du troisième Voyage de Cook [1776-1780; official report, 1784] mit en évidence l'analogie des idiomes océaniens avec le malaïo et le madekass [Malagasy], à l'égard des noms de nombre recueillis en quarante-deux dialectes différents.

The first use of lexicostatistic tables known to me is that of Dumont d'Urville himself, as discussed in the next part of this paper. The use was without conscious successor, so far as I know. Latham (1859: 503) makes use of such a table without comment as to the source of the notion, whether through knowledge of Dumont d'Urville's monograph (a possibility), through the stimulus of his own previous conception of an average rate of change in language (1850: 565), both or neither. The nature of Latham's use of percentages is clear from his own words:

The Burmese are the dominant members of the class to which they belong; the directions in which they have intruded and conquered being from north to south. The annexed table illustrates the extent to which the numerous vocabularies, collected by the Rev. Mr. Brown, and published in the Transactions of the Asiatic Society of Bengal, represent like or unlike languages. The numbers give the percentage of words that agree. And it is clear that they tell a long tale briefly. They give, indeed, not only the nucleus of the Burmese group, but most of its details. They show, too, that the Annamese, on the one side, and the Siamese and Khamti on the other were strangers to it.

Between Latham in 1859 and anthropology at Berkeley a half-century later, no instance of the technique is known to me, but the existence of such is likely, the technique being obvious enough to be reinvented easily, as perhaps it was by Latham, as apparently it was at Berkeley (and as it clearly has been, subsequent to Swadesh's work, by Hoffman (1959)!). The first California use of a table of lexical percentages, so far as I know, was in a monograph by Samuel A. Barrett (1908). The celebrated use by Dixon and Kroeber (1919) served to unravel their California data and disclose the outlines of the families they christened Penutian and Hokan. Swadesh referred to his own subsequent use of the technique as reinvention of theirs (1956: 17-18):

comparative linguists should restudy the modest but epoch-making monograph, *Linguistic Families of California*, by Dixon and Kroeber, and ... note the quantitative method of

⁷ That is, Johann Reinhold Forster (1729-1798), who accompanied Capt. Cook to the South Pacific, not his son, Johann Georg Adam Forster (1754-1794), well known as a writer on travel. According to Dumont d'Urville (1834a: 275), Forster considered the languages to be derived from an ancient common ancestor, now lost. Forster's views, writes Dumont d'Urville, are of astonishing wisdom and lucidity, but of course he was not alone in arriving at a hypothesis of this form before Sir William Jones' celebrated statement of 1786; cf. Metcalf 1972.

counting similarities in a standardized test vocabulary. So little appreciated at the time that it had to be reinvented thirty years later, 'lexicostatistics' is now found to be an invaluable tool...

The subsequent history of the use of such tables in Amerindian linguistics and ethnology has been discussed several times, notably by Driver (1964), and Kroeber (1963). (See the references in these two sources, as well as Hymes 1963: iv, and references in Hymes 1960a, b).

Such tables represent a degree of explicitness in assessing degree of relationship; their validity of course depends upon the validity of the data they summarize and methods of interpreting them. Such methods have ranged from inspection to mathematical manipulation. They may be constructed in ways quite independent of the commonly known way, as in Gleason (1959). Their usefulness, at least for first approximations, is fairly well established; their elaboration and interpretation is part of a more general trend in ethnology (as Driver 1964 indicates), and is not specific to linguistics or lexicostatistics, the underlying mathematical form in ethnology and linguistics being frequently the same. The general problem of the interpretation of percentages of relationship among languages in terms of trees, and in relation to a variety of criteria and implications, continues to be extensively investigated, by Dyen, Sankoff and others. In ethnology Driver, Aberle, Jorgensen and others continue to use and develop the approach. This sector is indeed the most popular and well sustained of those associated with the general subject.

Since lexicostatistics is sometimes taken to be in competition with other methods, something should be said on this point. Where vocabulary is all the evidence there is, perforce lexicostatistics is central. From a general standpoint, lexicostatistics (and glottochronology) have a valid but partial contribution to make to the variety of questions for which one wishes a determination of boundaries within or between groups. It is clear, for example, that lexicostatistical indices cannot be accepted as universal criteria of boundaries of communication (Hymes 1968), that the relation between agreement in basic vocabulary and communication is not univocal. That is a difficulty for some attempts to use lexicostatistic criteria, but an interesting problem from a sociolinguistic standpoint. Sometimes it is suggested that one should choose one line of evidence as the single correct one. The truth, I think, is that when one has only one line of evidence, one uses it as well as one can, and that when one has several, one should use them all. The lexical relationship of languages, by definition, is an aspect of their relationship. Discrepancies shed additional light on actual relationships and histories, and point as well to the need for more general theories, deeper analyses, of the interrelations of sectors of language to each other and to social forces. It may be that the development of a more thoroughgoing sociolinguistic approach to change may find new uses for lexicostatistics (cf. Mayers 1961).

1.4 *Proof of Relationship*

Probative use is not intrinsic to lexicostatistics, much less to glottochronology. Lexicostatistic grouping and glottochronologic dating are indeed partly independent of each other (in that grouping may not entail dating, and dating may be of a group as a whole without specification of internal relationships), and neither inherently involves questions of proof. Both can be pursued entirely within the confines of accepted genetic relationships. As noted, however, the grouping of dialects and languages with regard to relationships internal to a family is not distinct itself from the grouping of families, once themselves dialects and languages in degree of difference; and from a standpoint that considers all languages ultimately related, all grouping is subgrouping, differences being matters of scale. Such grouping may of course be done by inspection (as by Dumont d'Urville 1833 and Latham 1859). In addition, lexicostatistics has sometimes been associated with specific proof of relationship, through analysis of lexical resemblances according to some criterion of significance, apart from a theory of sound correspondences (or a showing of grammatical connection).

The history of lexicostatistic proof is independent both of chronological inference and basic vocabulary, apparently, until the work of Swadesh. (Our considerable ignorance as to the history makes this statement highly vulnerable). The earliest known instance has been pointed out by Rulon Wells (see his paper in this volume) in the work of Dr. Young in the second decade of the nineteenth century. The grouping of languages by percentages, by Dumont d'Urville for Austronesian and Latham for Burmese, does not involve explicit statistical criteria or tests either for the relationship of words or of languages. An interesting and forgotten case slightly later is that of Pliny Earle Chase (1869a, b; papers read 1863). Chase cites Dr. Young as precedent (1869a: 27) and considers that he has mathematically demonstrated the practical impossibility of accidental resemblance among languages on general mathematical grounds, formally presented (1869a). He goes on to apply his approach to a recently published lexicon of Yoruba (1869b). (Chase 1869c is a mathematical note on linguistic resemblances by his friend J.E. Oliver, who concludes by also citing Dr. Young. The mathematics of Chase and Oliver should be technically assessed in relation to contemporary work: Swadesh 1954, 1960b; Cowan 1962).

Chase's reasoning is interesting in its detail and in its indication of the contemporary 'ethnological problem' and of the role of language in its solution. He justifies lexical evidence in relation to what appears as the almost exclusive dominance of a grammatical approach (1869a: 25; cf. Haas 1969). Chase states the issue in terms that have a resonance for lexicostatistic investigations a century later (1869a: 25):

The process of grouping languages into families, has already been extended nearly to its utmost practicable limits, and the question of connection between the families themselves, where no grammatical analogy can be discovered, must be solved, if it can be solved at all, by a comparison of radical words or syllables.

Such comparisons had fallen into disrepute, he notes, and he attempts to answer the principal objections in a way that goes beyond the defects of past practice. The failing of Chase's reasoning, even for one sympathetic to some of his points, which anticipate significant points made by Swadesh, is that he has no standard measure of relevant vocabulary, despite mention of "the most common objects" (1869b: 45), and no explicit criterion for assessing equivalence of lexical items. (Swadesh 1960, it may be recalled, devised a method for assessing chance resemblances with regard to the particularities of the languages compared).

Much of the historiographic interest of Chase's papers lies in his references to Horne Tooke, von Humboldt, Prichard, Bunsen, and others, and his analysis of the current climate of opinion as to genetic proof. A facet of the 'ethnological problem' shines through in his avowal of a feeling of universal brotherhood as the goal of his efforts (1869b: 61). In its concern with the unity of origin of man, with the role of language in the debate between monogenesis and polygenesis, the work is very much a part of the nineteenth century with Prichard, Müller, Broca and others.

There is a hiatus of ninety years between Chase and the major effort to develop lexicostatistical procedures of proof in the work of Morris Swadesh (1954, 1956, 1960b). The best known statistical work in the interval, that of Kroeber and of Chrétien, is concerned with internal relationships, not proof of new relationships. Swadesh himself cited work of Collinder (1948) along lines similar to his own, but other such work of the period is unknown to me.

I have argued my own views on proof of relationship elsewhere (1956, 1959, 1960a, 1964). Suffice it to say that I continue to share the view of Mylius, Meillet and others that the first step in proof of historical connection is to eliminate explanation by chance or independent convergence; that I consider a theory of sound correspondences one such way, but not the only one, certain kinds of grammatical evidence being another (as Meillet, Sapir and others have maintained), and whose parallel to lexical cognation in some instances I have attempted to show (1956), and a certain level of agreement in basic vocabulary being yet another. At the same time none of these lines of evidence is unequivocally a talisman of genetic explanation. Proof of historical connection does not of itself discriminate between genetic and diffusional connection; sound correspondences, grammatical traits, and basic items of vocabulary may all on occasion and to some degree be diffused. The explanation of an historical connection is best assessed in the light of all three lines of evidence concurrently. Still, lexicostatistic criteria of relationship can either supplement other criteria, or be used in advance or default of other lines of evidence.

Since Swadesh's untimely death in 1967, there has almost begun another hiatus in the history of this subject, except for a valuable paper by Bender (1969). Where Swadesh (1954) had concluded that four or more cognates in a 100-item list of basic vocabulary imply genetic relationship at the 95% confidence level, and Cowan (1962), using different mathematics, estimated 3 or more to be required, Bender compares 100-item lists in 21 languages empirically. He finds that (1969: 530):

For languages from unrelated families, the occurrence of 'cognates' based on establishing CVC correspondences is indeed random and follows the Poisson approximation to the normal distribution quite closely. At least three levels of establishing 'cognates' can be determined according to more or less conservative criteria for accepting correspondences. After eliminating obvious loanwords, the results for these three levels are as follows.

STRICT [Swadesh]: the existence of two or more cognates implies that more than chance is involved in the relationship of the two languages in question at a 95% confidence level; 3 or more cognates assure this at the 99% level.

EXTENDED: three or more cognates refute the nil hypothesis at 95% level, four or more at 99% level.

WEAKENED: seven or more cognates refute the nil hypothesis at 95% level, but this figure must be used with caution.

The level of extended criteria leads to a result agreeing very well with Cowan's computations. Swadesh's earlier result appears to be a bit on the conservative side.

While Bender's results are of great interest, he appears to be unaware of Swadesh's later treatment of the problem (1960b) in a way that measures chance as a function of the shapes of the items compared, that Swadesh had worked on a technique of computer calculation (cf. Bender 1969: 525), and of other discussions of Swadesh's work and the issue of chance (e.g., Hymes 1959, 1962).

Sustained interest in this subject would seem to depend almost entirely on interest in remote relationships and perhaps in linguistic evidence of the original unity of mankind, the latter of which I think influenced Swadesh as it did Chase. Effective interest in the history of the subject may depend on the strength of the same two concerns.

1.5 Dating

It is the chronological aspect of the contemporary subject, glottochronology specifically, that least has exact precedent. The precedents that are presently known all have to do with just the idea of measuring change. They do not propose methods of using such a measure to reconstruct history. The earliest known precedent, Latham (1850: 565), does specify that such a measure is possible. In the course of a set of apothegms, the brilliant English physician and scholar states:⁸

XXXIII. Antiquity of the human species. – The problem is most likely to be worked through the phenomena of language. When determined it will give precision to the *recent* period of the geologist, converting it from a *relative* into a conventionally *absolute* epoch.

XXXIV. The average rate at which languages change is capable of being approximated.

XXXV. The maximum difference, at a given period, between two or more languages is also capable of being approximated.

...

XXXVII. The minimum amount of time necessary for the *maximum* amount of difference is the measure of the shortest admissible *recent period*.

⁸ For an account of Latham as ethnologist and philologist, see Quirk (1961). Latham's apothegm was pointed out by Gordon Hewes in his contribution to the discussion with Hymes (1960b).

The same concern with estimating the antiquity of man also motivated Paul Broca (see section 4). Without mentioning Latham, Broca felt he had given something of an answer to the question of human antiquity, at least by showing through language that it was more remote than linguistics could penetrate.

The third precedent looks to estimates of the chronology of particular lines of linguistic history, rather than to the history of mankind as a whole. In his time perspective monograph Sapir (1949 [1916]: 452) wrote:

linguistic changes proceed more slowly and what is more important, at a generally more even rate than cultural ones. This means that, particularly where there is abundant comparative linguistic material available, we are enabled to penetrate farther back into the past and to obtain a more reliable feeling of relative durations of such linguistic time sequences as are available.

There is at least a suggestion here of a uniform measure of differentiation, and Harris (1951: 308, n. 26) remarks, "An echo of this appears in the work of Sapir's student Morris Swadesh on rate of vocabulary change."

Both Latham and Sapir speak of language in general; there is no selection of one sector of language as the basis for approximation of rate or degree of differentiation. This is indeed appropriate to the term, 'glottochronology'. Measures of rates of grammatical and phonological change are important to the full development of the subject, both in a general theory of linguistic change and in its applications.

There was continuous interest in relative chronology within Amerindian linguistics and anthropology from at least the time of Sapir's 1916 monograph. The recurrent use of the amount of similarity in vocabulary to group dialects and languages implies an underlying belief in a tendency toward a regular rate of lexical change. Still, it was Swadesh who made this implication the basis of an explicit method, whose research provided it with a systematic foundation, and who determined actual estimate of rate of change.

With regard to basic vocabulary and lexical percentages, Swadesh made precise and scientifically vulnerable a traditional notion and a neglected procedure. The idea of measuring a general rate of change seems to have emerged unexpectedly out of work with the first two notions (see Swadesh's 1952 own account), in the course of recognition of the need to determine, not merely assume such a rate, and to use it in estimating the time-depths of relationships. The recently discovered technique of dating by radio-carbon also contributed to a climate of opinion in which dating by lexicon could be conceived and taken seriously, a fact reflected in use of the term 'morpheme decay'. (There were also similarities in the receptions given the two kinds of dating [cf. McGhee 1964]).

While dating may appear to be a matter of purely practical interest, not one of general scientific concern (see the conclusion to Sankoff's paper in this volume), the importance of even approximate dating to prehistory is manifest. If prehistory is granted status as a serious discipline, then the contribution of linguistic chronology has serious import. Moreover, there are cases within historical linguistics that simply

require an assessment of rates of change for their solution. Michael Krauss, for example (p. c.), has applied glottochronology to the lexical relationship of Athapaskan, Eyak, Tlingit and Haida (Sapir's Na-Dene) with the constant result that the degree of possible lexical relationship appears too small for the degree of grammatical relationship (for which cf. Hymes 1956). In effect, Krauss suspects that the explanation of the grammatical connections among the languages may be other than genetic. Setting aside the possibility that additional cognates may be found, as historical reconstruction of the component languages advances, we are confronted with a case in which a rate of grammatical change must be assumed. No general measure of rate of grammatical change is presently available. There are other cases in which the relation between degree of grammatical and of lexical connection, i.e., between rate of grammatical and of lexical change, presents a problem. The investigation of such matters touches the foundations of historical linguistics, is of consequence and yet of great difficulty, and can not be attributed to a schoolboy desire to attach a date to things.

The mathematical and the empirical bases of glottochronology have been challenged, and the mathematical bases, at least, reconstructed in the work of Dyen and his collaborators, van der Merwe, Whitman, and especially now, Sankoff. The empirical bases, specifically the reworking of the original control cases together with new ones, in the light of the mathematical work, remain to be reconstructed. The work should include close attention to the effects of different choices with regard to lexical items, size of test list, and the like.

1.6 *Reprise*

The histories of the several aspects of lexicostatistics and glottochronology present themselves somewhat as follows. With regard to the relation between diversity and time (1.1), we have an assumption or premise, sometimes stated, often implicit, and carrying with it conditions on its scope that have changed over time. It is indeed a nice question as to what particular part of language is in view when general statements about stability and change, and rate of change, are made, as well as which languages. With regard to basic vocabulary (1.2), we have what amounts to a tradition, recently codified, most of whose transmission as yet remains obscure. With regard to use of percentages (1.3), we have an apparently easily reinventable technique that has indeed been reinvented several times. With regard to proof of relationship (1.4), we have an approach which seems a function of concern for comparison and relationship that pushes beyond what is accessible by conventional methods. Such concern has perhaps never been wholly absent from the scholarly scene, but conjunction of such concern with the mounting of a mathematically explicit argument seems to have happened just twice, at an interval of almost a century. Finally, with regard to dating (1.5), we have something which presupposes an idea that also has precedent about a century earlier (the idea of measuring linguistic change), but whose application in the dating of

prehistoric relationships is wholly new. Direct antecedent seems most likely stimulus diffusion from an adjacent field.

There is a thread that runs through such precedents as have been noted for use of percentages, lexicostatistic proof, and measurement of rate of change. As noted just above, it is a thread of concern with relationships that go beyond what is most accessible, a concern with pushing back the frontiers of knowledge of relationships, whether the frontiers be those of relationships in Oceania, Southeast Asia, the New World, or the world as a whole. The Pacific was such a frontier in the early nineteenth century, when Dumont d'Urville commanded his expeditions there, just as was the antiquity of the varieties of man, intellectually, in the period in which Broca dominated the science of anthropology in France.

2. DUMONT D'URVILLE'S LEXICOSTATISTICS (1834)

2.1 *Dumont d'Urville*

Captain J. Dumont d'Urville (1790-1842) was perhaps the most celebrated French explorer of his day, a man who to some extent personified exploration to the French public, to judge from one of his publications (Dumont d'Urville 1834b). In addition to his major expeditions, in search of the lost La Pérouse, through Oceania, and to the South Pole, he had an active role in French circles concerned with the organization and intellectual implications of exploration. In particular, he held office in the Société de Géographie, contributed to its discussions and its Bulletin, was once chosen for one of its gold medals, and received an extensive eulogy, printed in its Bulletin, upon his death (Berthelot 1843).

Dumont d'Urville undertook his linguistic work, as he himself makes clear in his reports (1833, 1834), because the relations among the Oceanic (i.e., Malayo-Polynesian, or Austronesian) languages were still uncertain, both as to the fact of relationship as well as to the internal grouping. It may be assumed that he worked with lexical material because it could most readily be obtained and was the major line of evidence available (as is still true for a fair part of the Malayo-Polynesian group).

2.2 *Rafinesque's Gold Medal*

Lexicostatistics came to the hand of Dumont d'Urville, just when he was prepared to use it – while writing up his philological materials from the voyage of the *Astrolabe* (1826-1829; published 1830-1835) – through circumstances that could not have been anticipated. In March 1830 the Société de Géographie announced a prize of a gold medal, 1000 francs in value, to be awarded in 1832, for a competition on the topic of the origin of the Asiatic Negroes. The committee to judge the competition was composed of Douville, d'Urville, Eyries, Roger and Warden.⁹ Dumont d'Urville

⁹ The story of the competition, and its aftermath, is found in the unusually informative pages of

reported on behalf of the committee that only one entry had been received, bearing the devise, "languages do not lie" (no. 107: 175-86). Its author went far beyond the assigned question, seeking to prove that black nations had been indigenous to all parts of the globe, had intimate, mutual relations in their languages and dialects, and were of a common origin (176-77, 178). The attempt to prove Negro population indigenous to the New World had been refuted before, and nothing in the present work changed the committee's mind. But in the sixth section, "Fundamental principles of the composition of languages and dialects", the committee found something on which to fix its attention: a procedure by which to appreciate at a glance the diverse degrees of identity and affinity that can be established between two languages, of which a certain number of words common to both are known.

On his own behalf, Dumont d'Urville stated that he had applied the procedure to some of the languages of Oceania, and most often with true success. He does however consider the author too facile in the value that he gives to diverse nuances of identity, likeness, resemblance, analogy and deviation. Above all, one would wish to employ in the proof only words truly radical and of which the signification was always well established. It would seem moreover that one was not authorized to deduce a satisfying conclusion by the method except insofar as the number of comparable words approached at least thirty or forty, and it would be desirable that these words should always be the same. The method would of course fail in some circumstances, such as with the well known languages of Europe, where, as one colleague had well observed, the procedure would show French *jour* entirely separate from Latin *dies*. But "in savage languages, where analysis, that sort of Ariadne's ball of thread, is entirely lacking, it is necessary to have recourse to a more empirical means [n.b.]; that which the author of the Memoir has proposed and used appears at once ingenious, simple, and convenient [commode]."

In consequence of this and other considerations, such as agreement of some of the conclusions about Oceania with ones independently arrived at by direct observation by Dumont d'Urville (185), the special committee proposed to award a "medal of encouragement" worth 100 francs to the author, and to invite him to make it public, while developing and revising it in the light of the discoveries even then being constantly made. The committee moreover regarded it as very essential that the author give himself the trouble to cite exactly all his authorities. And it proposed that competition on the topic be withdrawn.

The author of the memoir, and recipient of the gold medal of encouragement, was C. S. Rafinesque (1783-1840), "professor of historical and natural sciences in Philadelphia". Unknown to the committee and society, Rafinesque's professorship was

the *Bulletin de la Société de Géographie*. See no. 107 (March 1832, vol. 16, second series), 175-86, "Rapport à la société de géographie, sur le concours relatif à l'origine des nègres asiatiques"; idem, 194-95; no. 108, (April 1832, vol. 17, second series), 248, 249; no. 113 (September 1832, vol. 18, second series), 184, for the response of Rafinesque; no. 120 (April 1833, vol. 19, second series), 228, for a further communication of Rafinesque.

self-bestowed. The medal, however, would seem to have been earned on merit, for the procedure of the committee was to read a submission without knowing the author's name (no. 108: 249). Rafinesque's controversial, partly disreputable, fascinating career cannot be gone into here (see Rafinesque 1836a; Call 1895). For Americanists, suffice it to recall that he is the source of the disputed *Walam Olum*. The rest of his relation to the present story is quickly told. He accepted the medal with thanks, expressing his wish that the memoir be published among the memoirs of the society (no. 113: 184). Subsequently, he announced that he had just retouched his work and drawn up a supplementary memoir containing other important notions. It would be directed immediately to the society, as well as an example of each of his works. He offered his services to the Society for the solution of questions of geography and statistics that it judged appropriate to address to him, concerning North America, and he announced that he planned to explore during the year the southwest part of the Allegheny mountains. The central committee (it is reported) listened with interest to the reading of his letter, sent it to the committee of the bulletin with an invitation to insert an extract, and charged the president to transmit to M. Rafinesque the Society's thanks. (The extract may be that published the following year: Rafinesque 1833). This report is the last indication of communication from him on the subject.

Rafinesque refers briefly to the memoir in his autobiographical work (1836):

At last I have reached this year 1833, when I write these outlines of my travels, out of my journals, memoirs and mpts [sic]. I have sent many plants...I have sent a third memoir or supplement on the primitive Negro of Asia and America to the Society of Geography of Paris, as well as other tracts relating to geography.

Whether the statement refers to the original submission of the memoir (in which case the year is misremembered from 1832), or to submission of a revised memoir in hopes of publication, I do not know. In any case, the French society did not publish the memoir.¹⁰

2.3 *The Role of Basic Vocabulary*

The exact nature of the comparisons made by Rafinesque can not be determined without access to his memoir, but it is clear from Dumont d'Urville's report that the comparisons did not meet his own standards. His comments imply a standard test list, of a certain minimal size, directed toward elementary words. From his own work, it is clear that he also made use of a notion of basic vocabulary.

The notion of basic vocabulary does not appear in Broca's comments on Dumont d'Urville – quite the opposite, for he refers to Dumont d'Urville's Tahitian-Hawaiian comparisons as “words taken at random”. In point of fact, Dumont d'Urville did have a criterion for the words to be employed in lexicostatistic and other comparative

¹⁰ It did acknowledge receipt of a copy of his *Atlantic Journal* (Rafinesque 1832-33) and publish three further articles by him, that mentioned of 1833 and one each in 1834 and 1835.

work. Indeed, a notion of basic vocabulary seems to have been present early in the century in the circles represented by the Société de Géographie. In 1825 Warden (who was to serve on the committee that read Rafinesque's essay) published an account of the antiquities of North America.¹¹ Warden, who was for a time consul-general of the United States, was also a correspondent of the Institute of France, member of the Royal Society of the Antiquaries of France, etc., at least by 1834 (see title-page of Warden 1834). In a concluding chapter, "Remarques et recherches sur les antiquités américaines", Warden included a section on the "Prétendue affinité des langues indiennes avec celles des divers peuples" (481-89). Having passed in review proposals of Grotius, Lafiteau, Barton and Reland on affinities of Amerindian languages to languages of Asia and Europe, Warden writes:

For there to be agreement [conformité] between two languages, it is necessary that it be found among the common words, such as those of number, the sun, moon, land, water, wind, thunder, to eat, to drink, good, bad, the names of parts of the body and of animals, etc.; for in supposing that these words should have undergone great change with regard to pronunciation, there would exist nevertheless enough resemblances to discover their origin. (483)

[In this connection Warden rejects Reland's assertion that Indian pronunciation of words undergoes frequent change, finding instead great care to conserve correct pronunciation (484) – the stereotype was to need rejection as late as the Seventh International Congress of Linguists in London in 1952. (Bloch 1956: 394-96)].

Warden and Dumont d'Urville were associated in the Société de Géographie, as has been indicated. And Dumont d'Urville was himself explicit about the role of some notion of basic vocabulary. Indeed, as part of his major linguistic work, he seems to have intended to publish an exemplification of the notion. In the first part of the philological volume from his expedition of the 1820's (prepared by himself, and issued under his name, as not all the separate volumes were), he writes (1833: vii-viii):

¹¹ Warden's study, and its successor of 1834, expanded to the whole of the New World, are of considerable interest as to knowledge and ideas regarding American Indians and their languages in the period. It is striking that in 1834 Warden omits the section quoted above from his study of 1825, the omission being the only change in the contents of the part in question. A certain part of the 1834 volume is devoted to two questions proposed to the European historical congress convened in Paris in 1835, "Discuter et établir le valeur de documents relatifs à l'histoire de l'Amérique avant la conquête des européens" and "Déterminer s'il existe des rapports entre les langues des différentes tribus de l'Amérique et celles des tribus de l'Afrique et de l'Asie". (Not the relations of Amerindian languages among themselves, but their relations to the Old World seem to have been of primary interest in the period. Scholars were blocking out the world as a whole). Warden is very skeptical about the first topic, as reference in the title of a long section of his work to "Pretended knowledge of the ancients..." makes clear. On the second topic he seems to have changed his mind, due to the work of a Mexican scholar, communicated to Warden by Duponceau, connecting Otomi with Chinese. Warden devotes several pages (125-29) to the Otomi-Chinese material, and has later a chapter, based on a contribution of Dunbar to the American Philosophical Society, comparing Plains Indian sign language and Chinese, initially on the basis of the shared function of communication between people whose speech was mutually unintelligible. Comments by others in no. 9, part 1 of the 1834 volume indicate that the Otomi material was convincing to several, including as it did evidence of likeness in vocabulary, grammar and phonological type.

We have intended to add to this part the Comparative Vocabulary of 115 *commonest* [*plus usuels*] words collected among 50 Oceanic languages. [Emphasis supplied] This work, for which we had prepared the materials, seemed to us to be such as to be of interest to those concerned with philological research, and who would like to follow step by step the diverse modifications that can be undergone by conventional sounds intended to express the needs, the passions, and the thoughts of man more or less near to a state of nature. But we enter the last year accorded us by the Minister of the Navy to complete the publication of the Voyage of the Astrolabe. The printing of the Comparative Vocabulary of which we have just spoken would be long and full of painstaking detail; moreover, all our time is absorbed by the other parts, historical, hydrographic, and physical, that we must finish. We thus will probably be forced to postpone to a more favorable time the publication of the Comparative Vocabulary of Oceanic idioms. *Si quandò fortuna faveat!*...

Unfortunately the 115-item list does not seem to have ever found its way to print. The vocabulary in the second part (1834) of the philological volume is always in excess of 115 words for a given language, and the comparative vocabulary deals at various length with six Oceanic idioms. The list is not among communications from Dumont d'Urville to the Bulletin of the Société de Géographie (which at least once did publish a vocabulary). In the report of the subsequent voyage, there is no linguistic volume.¹²

Publication of the 115-item list and the associated comparisons might well have shown Dumont d'Urville to have developed a lexicostatistic approach to an extent not matched or surpassed for more than a century. The work he did publish suggests as much. It shows him to have seized the germ of a usable idea from quite loose work, to have refined and tightened it, and to have had in mind further refinement and tightening. He had obtained some useful results with the application published in his report on philology (see below); it is likely that he had further useful results in mind from the sequel.

The vocabularies actually published and used by Dumont d'Urville vary considerably in size. The first of the two parts of the volume on philology is devoted entirely to a *Dictionnaire Français-Madekass et la Vocabulaire Madekass-Français* (with an Essay d'une grammaire Madekass par M. Chapelais, 65-98). About two-thirds of the second part presents vocabularies of seventeen other languages; some rather extensive and others brief.¹³

Each vocabulary has an introductory paragraph or two, explaining its source. Some incorporate materials of others, often as checked by the expedition itself (cf. 265-66). Thus, of the first vocabulary (from Port du Roi-Georges, Australie), Dumont d'Urville writes:

¹² See section 3 below on this point.

¹³ The vocabularies are: "Vocabulaire de la langue des Habitans du port du Roi-Georges" (1); du golfe Saint-Vincent (6), du port Dalrymple (9), la baie Jervis (11), "V. françois-mawi (maori)" (14), V. mawi-français (56), V. français-tonga (56), V. tonga-français (98), V. de la Langue des Habitans du havre Carteret (143), des Papous du Port Doree (146), des Papous de Waigiou (152), des Habitans de l'île buebe (156), de Tikopia (161), de Vanikoro (165), V. français-ualan (182), V. de la Langue des Habitans de l'île Satawal (182), de Gouaham (190), des Harfours de Manado (193-94).

Durant notre séjour au Port du Roi-Georges, MM. Gaimard, Faraguet et moi, nous interrogeâmes fréquemment les naturels pour former un Vocabulaire des mots de leur langage. Je tenais beaucoup à me procurer ce document, afin d'avoir un moyen de comparer directement entre eux les idiômes des Australiens séparés par la largeur entière de la Nouvelle-Hollande.

Depuis notre retour, à la suite d'autres renseignements curieux sur les naturels du Port du Roi-Georges, un Vocabulaire de leur langage, recueilli par M. Scott Nind, a été publié en 1831 dans le premier volume des Mémoires de la Société géographique de Londres. Nous en avons profité pour rendre notre petit travail plus complet, et nous avons fait suivre de la lettre N tous les mots extraits de la liste de M. Nind pour les distinguer de ceux qui proviennent du Voyage de l'Astrolabe (1834a: 1).

The introductions are interesting in themselves as indications of the spirit of the inquiry and the times. An apparent error is further shown to be such by the consideration that an Australian would not have known how to count beyond 5, or at least have had simple terms to express numbers beyond 5. A Tasmanian woman stated positively that expressions corresponding to 'chef' and 'dieu' did not exist in her language. A vocabulary from Gouaham in the Mariannas is important, because while the dialect no doubt has altered since Magellan discovered the archipelago, the language is fundamentally the same, and it matters much to philology to conserve carefully the remains of a language that each day tends to be denatured and even to be lost completely. Even an incomplete list from the northern Celebes (Manado) suffices to dash the hope, suggested by external and physical relationships, of finding in the Celebes a branch of the Polynesian family. The Polynesian branch must have constituted a separate nation with its own language, manners and religious opinions already at a very distant epoch.

The remainder of part two of the volume comprises a "Vocabulaire comparatif de sept Dialectes de la Langue océannienne" (195) and "Considérations sur les Dialectes de la Langue polynésienne" (263-306). (The number of Oceanian languages compared is actually six; the title of the part suggests that French has somehow inadvertently been counted into the total). Some 799 items of meaning (in French) are employed in the comparative vocabulary, but the six languages are far from equally represented, the number of entries decreasing from left to right among the columns: French, Malagasy, Malay, Maori ('mawi'), Tongan, Tahitian, Hawaiian.

Dumont d'Urville consistently shows concern to obtain the fullest and most accurate vocabularies possible, and concern with the general advance of the subject. He regrets the limited resources for Hawaiian, noting that one Naguère had shown him a rather extensive Hawaiian vocabulary, which would have come near to completing the comparative list, but Naguère neither wished to allow him to communicate it nor appeared disposed to publish it himself. One can thus only wait for the English missionaries to provide at last what they have long since promised (266, n. 1). He applauds the recently published work of Marsden, brought to his attention as he was sending his own work to press, a work most of whose opinions he shares; but he regrets that Marsden thought he should limit comparisons to only 24 words, apart

from numerals, and is astonished that he had preferred to use words from Cook's Voyages for Tonga and New Zealand, rather than those one finds in the much more exact vocabularies of Dr. Martin and the missionaries (264, n. 1). At the same time Dumont d'Urville's own lists and reasoning keep close to the notion of basic vocabulary. His general list includes all but 3 of Swadesh's revised 100-item test list, and the rest is generally of similar semantic character, expanded in scope. And if Marsden's 24 words plus numerals is too small a number, a total closer to a hundred is apparently justifiable, for he praised Martin for having presented a list of 55 Malay words found more or less altered in Tonga: "Suivant nous, c'était là le meilleur moyen de faire apprécier à toute leur valeur les rapports qui pouvaient rapprocher les langues océaniques des langues malaio et madekass" (264; it is to this statement that the note regarding Marsden is appended).

Indeed, Dumont d'Urville becomes quite explicit about the methodological bases for such comparison (1834a: 267-68):

Cette méthode qui ne paraît qu'empirique au premier abord, nous a cependant offert des résultats satisfaisants, surtout quand le nombre des mots dépasse au moins cinquante, et lorsqu'on a déjà l'idée des mutations dont ils sont susceptibles en passant d'une langue dans l'autre; seulement il faut toujours faire attention que les rapports établis par ce procédé ne sont jamais que relatifs au nombre des mots comparés. Pour être absolus, il faudrait comparer deux à deux tous les mots des deux langues, ce qui serait impraticable, et ce qui heureusement est inutile au but qu'on se propose. Il suffit des mots les plus essentiels, de ceux que l'homme dut employer dès qu'il usa du don de la parole.

The points as to standardized comparison are reiterated in a footnote correcting the misleading impression of figures between Hawaiian and Tongan, and Malagasy (1834a: 271, n. 1): "That is why we believe that the method can lead to truly satisfying results only to the extent that the words compared are the same, that their number is pretty nearly equal across the several comparisons, and that they at least exceed 50." In sum, lists of basic vocabulary, of at least 50 items, when compared in the light of sound correspondences and interpreted in terms of the proportion of equivalences to number of items compared, can suffice.

2.4 *Lexicostatistic Analysis*

It is Dumont d'Urville's use of his comparative vocabulary with respect to Malayo-Polynesian as a whole, and the Tahitian-Hawaiian part of the comparison, used later by Broca, that are of particular concern here.

2.4.1 *Comparison of Six Oceanic Languages*

As noted, Dumont d'Urville undertook his comparative vocabulary to clarify the relations between the languages spoken by the peoples of Oceania and those of the Malays and the natives of Madagascar. Everyone who has concerned himself with the languages of Oceania, he remarks, has been struck by the resemblances to a greater

or lesser degree. Maori (dubbed 'Mawi' from the last part of the name of the large northern island, and the name of the legendary first man of the race), Tongan, Tahitian and Hawaiian have been chosen for comparison with Malayan and Malagasy for the double reason that they are found more or less at the four extremities of the Polynesian region, and above all are those best known at present.

The criteria advanced by Dumont d'Urville, as quoted at the end of the preceding section (2.3), already are in advance of anything known to me in such comparison at the time, and indeed in advance of much of the practice exploring relationships for some time to come. It is particularly important to note his awareness of 'mutations'. The expression itself might mean much or little. Broca was to refer to Dumont d'Urville as working before the laws of consonantal alternation were known, suggesting naïveté on the explorer's part. In point of fact, Dumont d'Urville devotes a section to the correspondences between the very pair of languages on which Broca was to base his glottochronology: "If one compares Hawaiian to Tahitian, one sees at first a host of words perfectly identical in pronunciation; then, in those which differ, uniform alterations that have accompanied the transition from one dialect to another." He indicates, for example, that it is Hawaiian that has innovated with regard to the *k* it shows where Tahitian has *t* (1834a: 292-93).

2.4.2 *Calculation of Resemblance*

Dumont d'Urville's lexicostatistics anticipated work a century later; it also makes use of a way of arriving at lexical percentages that has yet to find a successor. The general method of calculation is presented by Dumont d'Urville as taken directly from Rafinesque's memoir on the question of the origin of Asiatic Negroes. Between two forms expressing the same meaning in two languages, six degrees of relationship are established: 0, for words wholly disparate; $\frac{1}{5}$, $\frac{2}{5}$, $\frac{3}{5}$, $\frac{4}{5}$ for words presenting more or less marked resemblances; $\frac{5}{5}$ or 1, for words perfectly or nearly identical. Thus if 45 words are compared, and the sum of relations (the sum of the numerators of the values assigned each comparison over 5) is $\frac{135}{5}$, or 27, then the degree of identity between the two languages in question is $27 \div 5$, or 0.60. If the sum of agreements is only $\frac{35}{5}$, or 7, then the degree of identity is $7 \div 45$, or 0.15.

Dumont d'Urville considers that this method, which appears only "empirical" at first, can give satisfying results, as we have seen, if systematic criteria for the kind and amount of vocabulary compared are met. In addition, more exacting assignment of values than Rafinesque has appeared to use is required. Thus Dumont d'Urville calls identical (value of 1.0) only cases of perfect agreement, i.e., without difference whatever. He assigns a value of 0.8 where there is only a slight difference, "comme une lettre altérée, supprimée, ou remplacée par une autre de même nature" (268). He assigns 0.6 where "la différence devient sensible, mais où cependant l'identité d'origine est encore évidente"; 0.4, where "il y a différence notable, mais pour lesquels cependant il y a encore lieu de présumer une origine commune"; and finally,

0.2 to those "qui n'offrent qu'une consonne radicale commune, une analogie générale de son, ou même une certaine conformité de composition, quand l'expression de l'idée est complexe" (268).

Dumont d'Urville gives some examples of the several kinds of case, as between Malagasy and Malay (26809), but the entries in the vocabulary are not marked in any way to indicate the values assigned to them. Question arises particularly with regard to the lower degrees of identity, where more than a single change is found. The relation of the percentages he arrives at to the true relations of the vocabularies compared is thus quite uncertain. The sphere of application, what comes between the statement of method and the figures printed in his tables, is blank. Having no special knowledge of comparative Malayo-Polynesian, I cannot independently assess the comparisons in terms of present knowledge of cognation and borrowing; but I have undertaken to examine one of the comparisons, that between Tahiti and Hawaiian, in terms of Dumont d'Urville's own data and expressed criteria. The result indicates that he applied his criteria in an explicit, consistent manner, one capable of being replicated, and that the bulk of his assigned values of relation (those in the three categories of greatest resemblance [1.0, 0.8, 0.6]) probably do reflect historical connection. Indeed, these three categories rather closely fit the extension of 'strict Swadesh criteria' explicated by Bender (1969: 527), while Dumont d'Urville's 0.4 category would seem to match Bender's third level, weakened criteria. Given that he employed basic vocabulary to a considerable extent (cf. 4.3.1) and indications that he was alert to the possibility of loan words, in the light of what was known of other vocabularies and historical movements of peoples (cf. 1834a: 298-306), it appears that his conclusions as to the family as a whole, at least, agree with modern knowledge as result, not of accident, but shrewdness.

Dumont d'Urville's own table for the comparison of Tahitian and Hawaiian is as follows:

Words compared	221
Identity [coefficient of]	0.74
Words identical at 0.2	11
at 0.4	32
at 0.6	32
at 0.8	51
at 1	93

The sum of the number of words is 219, from which one would infer that the number of words scored as 0.0 is 2. (Values for the 0.0 category are never given in the tables). Actually, there appears to be an error in the calculations, or in their printing. This can be seen by comparing the coefficient of identity given for Tahitian and Hawaiian, 0.74, with an independent computation in terms of the printed figures. The sum of the values assigned in the several categories of degree of identity is

840/5 ($11 + 64 + 96 + 104 + 465/5$) or 168. If 168 is divided by 221, the number printed as number of words compared, the result is not 0.74, but 0.76.

The most likely correction appears to be in the number of words compared. If the figures for number of words in the several categories are accepted, the criteria for assigning words to the categories can be explicated fairly rigorously, as will be seen directly below. While some room exists for differing reconstructions of what the criteria may have been, the higher degrees of identity (1.00, 0.8) match perfectly the figures given, following the quite explicit criteria stated by Dumont d'Urville, and the lesser degrees of identity also match in ways quite consistent with each other and the rest. On the other hand, an independent count of the number of meanings with both Tahitian and Hawaiian entries gives a total greater than 221. The full total is 230. Four of the meanings show forms identical with forms under other meanings, and a fifth almost does, giving a total of independent entries of 226 or 225. Now, if 168 is divided by 225, the result is a coefficient of 0.7466. While Dumont d'Urville never gives coefficients past the second place, he appears to have rounded upwards in a case such as 0.7466 (cf. Malagasy, Maori [270], where 0.16 is given for a coefficient calculated as 15.8).

The other possibility (and the more likely one) gives, dividing 168 by 226, a result of 0.7433. In short, a consistent interpretation of the table, apart from the one figure for number of words compared, can be obtained by assuming that the numbers for the various categories are based on the Tahitian-Hawaiian entries as published in the report, and that the coefficient of identity for Tahitian-Hawaiian is obtained by dividing the result by the number of non-redundant comparisons published in the report.

I stress the phrase "published in the report", because there is a difficulty here. At the end of the series of tables (there is a table for each two-way comparison among the six languages), the Tahitian-Hawaiian table being the last, Dumont d'Urville remarks (1834a: 274, n. 1) that the figures expressing the number of words compared are nearly always less than the number of words in the vocabulary, especially for Tahiti and Hawaii. The difference is due, he says, to the fact that the forms for several terms in these idioms were obtained only after having made the comparisons. He believes that the additions would slightly increase, rather than diminish, the coefficients of identity that have been calculated. Given the discrepancy that results from the figures in the published table, and the analysis just presented, I am inclined to believe that the published table does represent a late calculation, despite the footnote; the numbers printed total 219, allowing only for an accession of 11 forms, none showing any resemblance, given the published total of 230 entries for Tahitian-Hawaiian – a possibility quite contrary to the expectation of increase in the coefficient of identity if the added forms were considered.

One interpretation of the footnote is that it refers to the discrepancy between the number of words in a given comparison, and the number of possible comparisons, were all the entries in the whole vocabulary available (some 799). This interpretation

of the footnote is perhaps more reasonable in the light of the language of the note:

Nous devons faire observer que les chiffres exprimant le nombre des mots comparés sont presque tous inférieurs à ceux des mots de notre Vocabulaire, surtout pour les idiomes de Taiti et d'Hawaii. Cette différence résulte de ce qu'il a plusieurs termes dont nous n'avons obtenus les valeurs, dans ces idiomes, qu'après avoir opéré nos comparaisons; mais nous croyons pouvoir affirmer que, loin de diminuer par ces additions, les chiffres indiquant les identités recevraient plutôt de légers accroissements.

The language is at least compatible with the rankings of the six languages according to the number of words available for comparison at the time of the printed calculations:

Malagasy-Malay 728, Malagasy-Tonga 725, Malay-Maori 716, Maori-Tonga 713, Malagasy-Maori 709, Malay-Tonga 649, Tonga-Tahiti 447, Malay-Tahiti 443, Malagasy-Tahiti 434, Maori-Tahiti 431, Tonga-Hawaii 247, Malay-Hawaii 242, Tahiti-Hawaii 221, Maori-Hawaii 208, Malagasy-Hawaii 182. The figures involving Malagasy, Malay, Maori, Tonga, among themselves are close to the total number of words in the vocabulary as a whole, ranging from 728 to 649; as soon as Tahitian enters, there is a marked drop to the 400 range, and as soon as Hawaiian enters, there is a further marked drop to a range between 247 and 182.

This interpretation perhaps is further supported by the observation that Dumont d'Urville's own figures would be entirely consistent, if the figure for number of words compared were "221" as a misprint for "227", since $168/227$ gives precisely 0.74. And 227 would differ from the 226 obtained by myself as the number of independent comparisons by the smallest difference possible.

I shall proceed on the interpretation just advanced. As will be seen, it is consistent with an independent analysis of Dumont d'Urville's vocabularies; the inferred criteria give results that depart from his figures at only one point (the 0.6 category), by the amount of 1 or 2.

2.4.2.1 To determine the assignment of values, one must start with the cases of required identity and work toward those regarded as without resemblance. Even with the criterion of perfect equivalence, some supplementary considerations appear as soon as one examines the basic data in the vocabularies. There appear to be five types of case. Of entries identical without further question, as between Tahitian and Hawaiian, there are 55; these are: *aimer, ami, bambou, boire, cacher, chose, cinq, cou, coupe/tasse, couteau, dans/dedans, défaut, deux, donner, donne-moi, dormir, feu, feuille, frère, front, hache/herminette, igraine, lézard, libre, mari, méchant/mauvais, neuf (nombre), nez, nom, nombril, oiseau, ombre, os, où?, oui, pagaie, particule répondant à c'est, peau, pénis, peut-être, pirogue/navire, pisser, planche, pluie, poitrine, poule, quatre, racine d'arum, racine de Dracoena, rat/sourris, rire, sable, sept, six, trois.*

Of entries identical but for a division within one, not in the other, there are 2; these are: *arracher, assez.*

Of entries identical but for complete reduplication of one, not the other, there are 1; this is *barbe*.

Of entries with one or more forms on each side identical (order not considered), there are 33; these are: *à, asseoir(s'), attendre, bon, ceinturé, dessous, dix, eau, enfant, hameçon, jour, lever (s'), long/étendu, lui, lune, main, manger, mer, montagne/mont, mort, mûr/mûrir, natte, noir, nous (duel), œil/yeux, petit/mince, rivage/plage, rompre/brise, rouge, sal/malpropre, soleil (lumière du), toi/tu, un*.

There remain five entries, three simply identical, two with one or more entries on each side identical, which appear not to have been counted as independent, because the same Tahitian/Hawaiian form appears. The entries for *arbre* and for *bois* are both *rahau*, and so count together as 1 within the first subcategory above. The forms for *sacré/inviolable* are simply *tabou*; the entries for *défendu/non permis* and *prohibé/interdit* each show *tabou* on each side, and all three count as 1 within the general category.

The total number of entries in this category (perfect identity-1) thus is 93 (plus 3 redundant entries).

2.4.2.2 Dumont d'Urville's second category appears to be readily identifiable, given a division into two subcategories parallel to two just given.

Of entries identical without further question, with one sound change, there are 29; these are: *accoucher, action, bouche, corde, debout (être), demain, dieu, éclair, étoffe en écorce d'arbre, faire/agir, fronde, huit, île, instrument pour la culture, joue, mamelle, nuit/ténèbres, obscur, peureux/lache, plume, poisson, pou, prier, saluer, sang, soir, ventre, vous (duel), vrille/forêt*.

Of entries with one or more forms on each side identical (order not considered) with one sound change, there are 22; these are: *aller, amer, chef/grand, ciel, cœur, couper, danse, femelle, femme, forteresse/fort, homme, le/un, lit/natte pour dormir, maison, mère, mien/mon, nous (pluriel), prêtre/qui sait, requin, route, son/sien, terre/terrain, vent*.

The correspondence between forms poses no problems, so far as consistency in inspection is concerned, apart from two considerations. Almost all the differences are differences in a single letter.¹⁴ From an analysis of all the categories of values, it appears that two appearances of the same change within a word are counted as a single change; thus, among the forms for *mien, mon*: *noou, naau*, and as forms for *sang*: *toto/koko* (cf. *fafan/papan*, counted as 0.8 as between Malagasy and Malay [268-69]). It also appears that *ng* may have been reckoned as a single unit, as suggested by the forms for *requin*: *mao/mango, mano*; on this basis, the forms for *joue* have been counted here as showing a single change: *Papa riha: Papa ringa*. (cf. 293: "Dans le

¹⁴ The term 'letters' is used advisedly. Dumont d'Urville advises his readers (1833: 4): "Dans tous les vocabulaires que nous donnons dans ce volume, il faut faire attention que toutes les lettres, consonnes ou voyelles, même à la fin des mots, conservent constamment leur prononciation propre, à peu près, comme nous le pratiquons pour la lecture du latin".

hawaii le *ng* s'était simplement converti en *n*."). Finally, while *ou* must normally be taken as a sequence, there is a unique case in which its reduplication (in Tahitian) corresponds to the stem with a single change in Hawaiian; this is the case with *mamelle*: *ou ou*: *hou*. Recall that complete reduplication was not taken as distinctive in the case of *barbe*. This case has been counted here as showing a single change within the scope of Dumont d'Urville's criteria. The total number of entries in the category is 51, plus one possibly redundant entry, *air* (matahi/makani) = *vent* (matai/matani, makani, aho).

2.4.2.3 Dumont d'Urville's third category is pretty readily identifiable, given a careful specification of the kinds of changes apparently recognized. The distinction between entries directly equivalent, and entries with one or more forms on each side equivalent, of course remains, and it appears that Dumont d'Urville always counted an entry in the highest category for which it was eligible. The essential distinctions for our purpose now have to do with the number and kind of changes reckoned as such that "l'identité d'origine est encore évidente". It appears that the essential criterion for this category is that the forms be identical except for two changes. This is in keeping with Dumont d'Urville's own examples, but what may count as an instance of two changes is not entirely certain from the examples alone.

In order to show the kinds of changes considered, it is convenient to separate the cases according to whether the changes are contiguous or not, and as to whether consonants, vowels, or both, are involved. Actually, Dumont d'Urville appears to have respected vowels just as much as consonants, despite the saying attributed to Voltaire that in etymology the consonants count for little, and the vowels for nothing. Recall that two occurrences of the same change have been counted as a single change; the cases here involve each a different change in each position.

Two non-contiguous changes (consonants): *air* (cf. *vent* under 2.4.2.2): matahi/makani; *fils*: tama idi/kama iti; *mouche*: lao/naro; *noble*, *seigneur*: raha tira/rana kira; *pied*, *patte*: avae/wawae; *trou*: roua/pouta, bouka.

Two non-contiguous changes (vowels): *dent*: niho/nihieu; *langue*: rero/errou; *visage*, *figure*: moti/mate.

Two non-contiguous changes (consonant, vowel): *courir*: oro/rere (*o*: *e* counting as a single change); *je*, *moi*: vaou/au; *patate douce*: oumaru/ouhara; *vous* (pl.): o outou/oukou.

Two contiguous changes (consonants): none.

Two contiguous changes (vowels, including VV: V, VV: Ø): *blanc*: tea tea/ teou teou; *cuisine*: oumou/oumai; *cuisse*: awha/ouwha; *nager*: ao/ou; *santé*, *salut*: ou ora/ora; *tête*: oupoho/poho.

Two contiguous changes (consonant, vowel): *coco* (*noix*): niahou/ niau; *douloureux* (*pénible*) = *mal*, *maladie*: mamahi/mahi; *étoile*: vetou/hitou; *houle*: rou/narou; *ici*: ihonei/iounei (not perfectly contiguous); *jambe*: wai/wawai; [alternately here (cf. a above) *je*, *moi*: ahou/au]; *manquer*: hou apa/apa, see below; *sel*: taitai/patai (again,

complete reduplication not distinctive); *tonnerre*: patiri/hetiri. (These cases may involve morphology, but Dumont d'Urville nowhere speaks of it).

Two contiguous changes (vowel, consonant): *cheveux*: ourou/oho; *prix, valeur*: otou/oukou; *sec*: oumaro/naro (taking ou/o as a single change);

The entry for *manquer*: hou apa/apa involves 3 contiguous changes, but the degree of equivalence of the whole seems to set it off from the next category. Part of the entry is identical on each side (*apa*), and that part both contiguous and at one boundary; moreover, there are a number of cases in which forms clearly differ only by an initial portion present in one and absent in the other – recall *mamelle*: ou ou/hou, and cf. several entries under “Two contiguous changes” above (*santé, tête, douloureux, houle, jambe, sel, sec*).

Three other entries appear to belong in this subcategory of evident identity of origin. All involve on one side what appears to be a phrase or complex form, containing as a part of the form on the other side with one or two changes: *guérir*: wa ora/ola, oura; *guerrier*: tahata toa/tawa (the latter part would be “two contiguous changes (vowel, consonant)”; *père*: medoua/modoua tane.

The total number of entries in the 0.6 category is 34, plus one redundancy, (*mal, maladie* = *douleur, pénible*), if *air* is counted as independent of *vent*; otherwise, the total is 33.

2.4.2.4 Whereas the preceding categories have been stated in terms of degree of difference, essentially, none (1.0), but one point of difference (0.8) or but two (0.6), we now enter a sector in which the consideration appears to be the degree of similarity that can be observed. There appears to be a sharp dividing line between the preceding three categories (which contribute most to the coefficient of identity) and the remaining two. With 0.4, it is a question of finding some two points of similarity, and with 0.2, any similarity at all.¹⁵

There appear to be two subcategories within 0.4. Forms may be considered equivalent at this level if they share an identical syllable, or if they share two analogous, non-identical syllables. If two syllables are identical (and contiguous), the forms would be counted within 0.6. If two syllables are analogous, but only by an implausible change and/or an assumption of metathesis, the forms appear to be relegated to 0.2. In short, phonetic plausibility and the overall shape of forms appear to play a part in the assignment of forms to this category.

Of entries sharing an identical syllable, there are 13; these are: *affamé*: pohia/porori; *auge*: poakou/koumete; *battre*: toupahi/pepehi; *beaucoup*: rohi; *manou*: noui; *calme*: manino/marie; *crabe*: papa/opai; *écouter*: faharoo/rohe; *épaule*: taponno/poivi; *mince*: rairai/rahoi, dehoi; *nuage*: ahou/hopoua; *ongles*: aiou/maiao; *parfum*: noanoa, naiou/nawai; *tranquille*: mamahou/maria. Some of these entries also involve the considerations for two analogous syllables.

¹⁵ Dumont d'Urville's illustrative examples for these two categories several times show greater degrees of similarity than called for and than can be found to the extent required to reach his totals for each.

In interpreting forms as containing two analogous syllables, certain plausible changes must be allowed for. Such changes appear to be change within a consonantal order (e.g., *p*: *b*, *v*: *w*) not crossing the line between stopped and non-stopped consonants; change within a series (e.g., *m*: *n*); and often, change within the set of resonants (*r*, *n*, *l*). Also, *a*: *o* and *e*: *i* appear to count as analogous vowels. Two identical vowels of course count as two points of resemblance. Two syllables appear to count as analogous in a few cases (*dessus*, *grand*) when the two points of comparison are the consonant of the first and the vowel of the second.

Of entries showing two analogous syllables, there are 18; these are: *attendri*: nanti, tounjou/tatadi; *aujourd'hui*: iniari, ari sini/ahinei; *déchirer*: hahai/wawahi; *dessus*: niha/nouna, rouna; *doux*: mona/nolou nolou; *esprit*, *naturel*: varoua/wano; *grand*: rahi/noui; *hier*: nanahai/ineinei; *maintenant*: ariana, tenana/ahenei; *pierre*, *rocher*: ouai/poure; *pointe*, *pointu*: hoi, ohoi/koira; *pleurer*, *gémir*: oto, tai/awe; *prendre*, *saisir*: mamao/hao; *tempête*, *ouragon*: vero, taroua/ino; *tombe*: tou, pappou/bouoa; *vrai*, *exact*: mou, parou/pono.

Where the entirety of one side is analogous to a form of the other side, it is also counted here. This involves two cases of reduplication: *étranger*: hehe/ho ori; and *lancer*, *pique*: tao, oioi/aie. Finally, *chien*: ouri/ilio is counted here, as qualitatively distinct from 0.2 to follow. The total number of entries in the 0.4. category is 32.

2.4.2.5 The best way to describe this category is as entries whose forms are not devoid of any point of resemblance whatever. In two or three cases it is not certain that the forms entered here might not have been counted by Dumont d'Urville in 0.4, in exchange for less obvious forms there. The candidates for this exchange are *aveugle*, *chapeaux* and perhaps *chaud*. The difference to be noted is that in 0.4 above, a comparison such as *pierre rocher* involves identical vowels, and that each vowel is counted as a syllable.

Of entries showing a minimal degree of resemblance, there are 11; these are *aveugle*: matapo/paha; *chapeaux*: tapoho/papale; *chaud*: mahan, hana/vera; *désirer*: ina aro/ake, make; *enfuir* (*s'*): mahoue/rere; *entendre*: ite/rohe; *là*: ihona/terera; *malade*: hoa hoa/mahi; *mouvement*: ou ata/ono; *non*, *negation*: aipa, aita, aima/aoe, aore; *palissade*: awa/ pahaou; *parler*: paraau/ orero.

The total number of entries in the category, as said, is 11.

2.4.2.6 There are four entries for whose forms no trace of resemblance, by any criteria implicit in the data, can be detected. Of such entries there are 4; these are *chanter*: hiva, oupə, oupa/meri; *froid*: mah vidi/anou, hokeka, toe; *oreille*: tarika/ pepe ido; *quelque*, *quelqu'un*: fanou/te tahi.

2.4.3 Use of Percentages for Historical Inference

Dumont d'Urville's comparisons of the six selected languages, two by two, can be taken to imply a table such as the following:

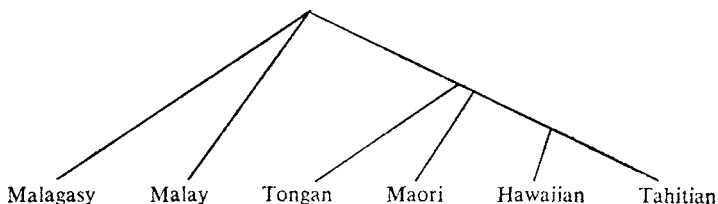
	Malagasy	Malay	Maori	Tongan	Tahitian	Hawaiian
Mlg	—	21	16	17	17	21
Mly			14	16	14	16
Maori				45	53	65
Tongan					41	46
Tahitian						74
Hawaiian						—

The order of the table indeed is the order in which the pair-wise comparisons are presented by Dumont d'Urville: Malagasy-Malay/Maori/Tongan/Tahitian/Hawaiian; Tongan: Tahitian/Hawaiian; Tahitian: Hawaiian. The table brings out clearly the major alignments within the data, selected, as will be recalled, to test the relationship of representative Polynesian languages to Malay and to Malagasy. And Dumont d'Urville proceeds immediately to interpret the percentages as if he had informally drawn up such a table (1834a: 275):

A glance at the preceding tables suffices to demonstrate that the principal Polynesian dialects participate equally in the two languages, Malay and Malagasy, i.e., in relations that vary only from 0.14 to 0.18, while the degree of identity of Malagasy and Malay is 0.21. The figures indicating the coefficients of identity of the Polynesian dialects, taken two at a time, rise to 0.41, 0.45, 0.46, 0.53, 0.65, and even to 0.74.

Dumont d'Urville had previously warned that the figures must be interpreted in the light of the number of words entering into comparison, specifically with regard to the figure for Malagasy and Hawaii: (271, n. 1): "it is not necessary to conclude that Malagasy is nearer to Hawaiian than Tongan, for this result would be contrary to the truth, as will be seen later. The raising of the coefficient of identity is due to the great decrease of that which expresses the number of words compared".

On the basis of the apparent closer connection of Hawaiian and Tahitian with Maori than with Tongan, of Tongan and Maori with Hawaiian than with Tahitian, and of Tahitian with Hawaiian, one could rearrange the columns and rows of the table into the order: Malagasy, Malay, Tongan, Maori, Hawaiian, Tahitian. And one could devise a tree diagram to show the relationships implied by the table:



Dumont d'Urville, however, correctly observed that the varying numbers of words compared could confound interpretation of the figures, and it was no doubt to remedy that deficiency that he intended to analyze a standard 115-item vocabulary in a

projected third part of his report on the philology of the expedition. Cf. the concluding remarks in the footnote just cited, that truly satisfying results require that the words compared be the same, almost equal in number (and at least fifty in number).

The broad results of the tables are used directly by Dumont d'Urville to come to a conclusion as to the nature of the relationships among Polynesian, Malay and Malagasy. The first of the results (quoted above), the equal relationship of the Polynesian dialects to both Malay and Malagasy, "destroys the rather natural supposition that the Polynesian languages owe their analogy with Malagasy to the mediation of Malay; for in that case, their identity with Malay should be more pronounced than with Malagasy". (Recall Erasmus Darwin Preston's remarks on diffusion as an explanation of such resemblances a half-century later!) Dumont d'Urville continues,

Moreover, one must not lose sight of the fact that the words common to Malagasy and the Polynesian dialects are not found in Malay, or are represented there only in very altered form. These two considerations seem to confirm, do they not, the hypothesis that all these languages should derive from a very ancient language, today lost, and of which the traces have remained more or less pure and numerous in the various idioms of Oceania?

Further inferences are drawn. Their great closeness bespeaks the recent common origin of Tahitian and Hawaiian, very probably one from the other (276). While there is great resemblance between Tahitian and Maori, the coefficient of identity is much diminished, and is much lower than that between Maori and Hawaii, in spite of the great distance separating New Zealand from the Sandwich Islands; this fact suggests that the communications between the two peoples have taken place without the mediation of the Tahitians, or perhaps that mankind has been able to travel from one to the other without passing, or at least without staying in, Tahiti. As to Tonga, Dumont d'Urville remarks that in the third part of the work, one will see that the lower relations of Tonga with the other three Polynesian peoples diminish still for the islands nearest Tonga, that is, Viti and Rotuman. It is thus probable that the western regions of Polynesia have departed from its primitive type, through communications with peoples more recently arrived from the west.

Dumont d'Urville goes on to consider the most likely hypothesis as to the lands the human species must first have occupied in Polynesia and the direction that must have been followed in their migrations. These considerations come to an end with a reference once again to the third part of this work, in which, at a more favorable time, more complete development can be given. Dumont d'Urville does indicate that he has used his vocabulary studies, not only to trace the relationships internal to the Oceanic languages, but also to assess their place with regard to the languages of adjacent territories. He limits himself here to declaring that he has found no satisfactory relation between "le grand polynésien" and any of the known languages of the two neighboring continents.

Not one of those of America offers the least point of contact with Polynesian. It is the same with the languages of the riverain peoples of the Asiatic continent toward the East, such as the Anamese, the Ava, the Pegou, the Siamese, the Chinese and the Japanese. The names

of numbers themselves, which more than any other part of the languages resist the alterations of time, cease to offer the least analogy (298).¹⁶

Some similarities in Chinese are noted, but apart from a small number, these are rather vague, when one considers the monosyllabic character of Chinese, and the number of diverse meanings which correspond to the same articulation. In any case, these agreements could indicate only chance contact.

Here we see Dumont d'Urville using an implicit criterion of amount of similarity required to indicate any relationship, and of amount required to indicate a relationship that is genetic, rather than diffusional, together with the basic vocabulary (including numerals) as a qualitative criterion. We see further that he had a sense of a criterion for assessing resemblances as due to change in terms of the morphophonological character of a particular language.

The work concludes with some suggestions as to the original culture and homeland of the Polynesians. Dumont d'Urville notes the evidence of an original three-fold system of social classes, and he wonders, in the absence of any satisfactory indication of a homeland within their presently known territory, whether the simplest solution as to the cradle of the Polynesian people might not be to suppose a sunken continent or great island (such as Australia)? But, he adds, we do not cling to this idea; it is very simply a hypothesis to explain the recent isolation of the great Polynesian language on the surface of the waves of the Pacific Ocean (306): Let someone present another more simple, more satisfying, and above all based on better established facts, and we are entirely ready to adopt it. And so closes, under place and date of "Paris, 30 juin 1834", this remarkable work, unprecedented, and for many decades, unmatched.

3. FROM LEXICOSTATISTICS TO GLOTTOCHRONOLOGY

Dumont d'Urville spoke of a third part of his work, in which the analysis of the Polynesian relationships would be more thoroughly developed. Presumably in this third part would appear the 115-item basic vocabulary of which he spoke. From such an extension of his work, one might have had the elements of modern use of a standard test-list of basic vocabulary, and of tables of lexicostatistic percentages, almost full-blown. The third part did not appear, however, so far as I know. Indeed, the reports of the second expedition under Dumont d'Urville's command, published beginning with the year of his death (1842), contained no volume on language whatever. Since the second expedition comprised Oceania in important part, and the series of volumes was to contain ultimately (1854) a remarkable polygenist interpretation of the physical anthropological data, written up by some one not on the expedition, it would seem that a place might have been found for a treatment of linguistic data as well, as another line of ethnological evidence. The volume on physical anthropology, written by

¹⁶ Cf. Gatschet (1886): "The numerals differ entirely in both which would not be the case if there was the least affinity between the two". Quoted from Hewson (1968: 86).

Emile Blanchard and published under the name of Dr. Dumoutier, who collected the data, indicates the possibility. Active interest in linguistic relationships and methods would seem to have depended upon Dumont d'Urville himself, and not to have survived his death.

The only use of Dumont d'Urville's work of which I know was by his compatriot Broca twenty years after his death. Broca did not develop the methodological bases of Dumont d'Urville's work; indeed, as will be clear from Broca's account of them, he did not accurately understand them. He remarks on the vocabulary as accidentally chosen; as we have seen, it was not. Broca summarizes the categories of degree of identity as three, not five (identical words, similar words, words completely different); he judges that Dumont d'Urville had to count as completely different a great many words which differed only by pronunciation, such as later work on sound changes in consonants would have disclosed to be related, whereas we have seen that Dumont d'Urville was quite cognisant of some sound shifts, and of the general principle, and had a category for any apparent degree of resemblance, such that almost nothing actually cognate in Polynesian would be likely to be relegated to the category of no resemblance whatever. Broca here appears to be reasoning after the fact from his own general knowledge of the significance of sound correspondences. Moreover, Broca seems to imply that Dumont d'Urville arrived at his coefficient of identity by taking only identical or very similar words, as against the sum of all words similar at all (thus Broca's discussion of the method, *vis-à-vis* Tahitian and Maori, and his treatment of Dumont d'Urville's analysis of the Tahitian-Hawaiian relationship). As we have seen Dumont d'Urville obtained his coefficients of identity by means, not of a simple sum, of words in just some categories, but by means of the WEIGHTED sum of the words in all categories. (And in neither of the two cases mentioned can the use of the most similar and the identical words give the reported coefficient.)

Broca's purpose, however, was not to develop lexicostatistics, or the lexicostatistical bases of linguistic prehistory, much less to consider the relationships of the Malayo-Polynesian languages as such. Even in inventing a glottochronology, his purpose was not to invent it, but to use it to help establish a certain course for the study of prehistory as a whole. Given the centrality of the 'ethnological question', his purpose was to set a certain course for the entire nascent discipline of anthropology.

4. BROCA'S GLOTTOCHRONOLOGY (1862)

Paul Broca (1824-1880) was Secretary and principal organizer of the Société d'Anthropologie de Paris. Its history, and some of the difficulties Broca encountered in organizing anthropological activity in France, due to the liberal implications of his work, are a separate story. Suffice it to say that Broca's address at the meeting of the 5th of June, 1862, must have been intended as a major statement of policy. The address remains fascinating as an expression of the concerns and circumstances of a fledgling field, and in particular, as a documentation of the crucial role played by linguistics.

The need still to reject Biblically derived explanations, to debate nomenclature for major ethnic groups, to define the relations of the newly cooperating disciplines, as well as the announcement of a French translation of the *Origin of Species*,¹⁷ with its rejection in advance as conjectural history – all these elements show the address aimed at the heart of the issues of the time. And it has as well the merit of a clear, vigorous style.

4.1 *Context: the Ethnological Problem*

Broca addressed himself to the relation between linguistics and anthropology at a time when relevance was assessed in terms of bringing order into diversity through tracing the relationships among the varieties of mankind. He expresses this theme in his opening remarks, describing as a goal of the Paris society (1862: 264):

to seek, by the multiple routes of anatomy, physiology, history, archeology, linguistics, and even paleontology, what have been in historic times and in the ages which preceded the most ancient remains of humanity, the origins, the affiliations, the migrations, the mixtures of the numerous and diverse groups which make up the human species.

The place of linguistics (or philology) was one of the major issues of the time. (As we shall see, Broca's address was aimed against a possible claim or priority for it.) The interest of ethnologists and others in linguistic evidence is well known, and has been touched upon in the first portion of this paper. The success of work on Indo-European in the first part of the nineteenth century was to give an impetus to linguistic investigation, including 'linguistic paleontology' (reconstruction of original cultures and homelands), that was to reverberate throughout the century. Insofar as scholars were working within a limited time-depth for mankind (the 6000 years of one calculation, or estimates not very different in magnitude), linguistics was also attractive because it was possible to assume that the pace of physical change might be much greater than the rate of change in language, and a commitment to monogenesis, for religious, or humanistic reasons, or both, was prevalent (cf. Curtin 1964: 365; 364ff. and 397ff. generally on this period and topic).

It may be worthwhile to call attention to the degree of concern of linguistics of the time with ethnology. In 1854 Max Müller (1823-1900) had contributed an extensive treatment of the latest results in the investigation of Turanian to one of two volumes published by Christian Karl Josias Bunsen (1791-1860), Prussian Ambassador to the Court of St. James, sponsor of the conference on orthography which involved Lepsius, Koelle and others (reported also in the volumes), a friend of von Humboldt and Bekker (Hare 1879: 141), and an author interested in languages as evidence for the unity

¹⁷ Broca says, "The English have been reading furiously for some years a singular but charming and most remarkable work which will soon appear in French and which is entitled 'On the Origin of Species by Natural Selection'. The author, M. Darwin, studying the causes which can modify wild species, has signalled an order of influences scarcely entertained before him, and called by him under the name of natural selection – with this element, which is not entirely hypothetical, he has attempted to demonstrate the hypothesis of Lamarck on the mutation of species..." (313).

and mental characteristics of mankind within a religious perspective. Müller (1854: 349-52) stressed the independence of 'physical ethnology' and 'linguistic ethnology' (which he also called 'phonology'). With Bunsen, he believed in the common origin of mankind, which he considered to have been shown physiologically; one probably cannot connect up the diversity of human languages, but should not attempt again to disprove the unity of the human race by arguments from the apparent diversity of human speech. As for ethnology short of a common origin of mankind, the study of crania does not support the natural groups of the nations; the researches of ethnologists tend to show, with more and more certainty, that these alliances are to be discovered by linguistic investigations alone.

The Société Linguistique de Paris was founded three years after Broca's address and of its founding Vendryes (1955: 13) has noted:

The principal modification (in the constitution) consisted in limiting the activity of the Society to questions pertaining to language. The first article of the statutes of 1866 expressly states: 'The Linguistic Society has as its goal the study of languages, that of legends, traditions, customs, documents able to elucidate ethnographic science'. We have seen above that some of the first founders would have liked to unite philology and ethnology in their program. It was only after discussion, and not without hesitation, that the name of 'Linguistic Society' was decided upon.

On the American scene, one can note the extended attention given to the ethnological significance of linguistics by the leading American scholar, William Dwight Whitney (1827-1894); see esp. "Difficulties of the ethnological problem" (Lecture X in his *Language and the Study of Language* [1867] and 1875: 271ff.).

A major issue of the period was the priority of one or another line of evidence, and, by implication, line of study, in the common enterprise of classifying and tracing human groups. We have seen Müller's view of 1854, held throughout his life, in agreement with Horatio Hale among American anthropologists (Müller 1890:52), as to the priority of language and linguistics.

For Broca, as for many others, the prime consideration was the relative stability, or permanence, of traits. It is striking that Lewis Henry Morgan justified his *Systems of consanguinity and affinity of the human family* (1870) in such terms. The work is now hailed as the founding of the comparative study of kinship, but Morgan conceived it as an answer to the ethnological problem of tracing the origin of the American Indian to Asia, and argued that that type of kinship system was the best guide to the history of the human race as a whole, because it was the most persistent human trait. Kinship, Morgan maintained, was more persistent specifically than linguistic affiliation. Philologists might reduce humanity to a dozen or more unrelated families only, whereas study of kinship could reduce the number to two or three, and could explain the developmental relationship among these.

Material culture and archaeological evidence generally did not much figure. Archaeological documentation of a great antiquity for man played a part and expressly motivated Tylor (1871), Morgan (1881) and others to attempt to explain the relation

of modern civilization to its ancient beginnings, but ethnological inference rather than archaeological data filled the gap. Not until the twentieth century about the time of the First World War did seriation, stratification, detailed chronologies and the like begin to make archaeology central to anthropological reconstruction of a continuous past. As late as Sapir's 1916 essay on time perspective, ethnology and linguistics hold the center of the stage. And at the time of Broca's address (1862), systemic ethnology was peripheral. Indeed, one way to define the period of Tylor and Morgan, which we have tended to think of as the beginning of modern anthropology, is at the period in which ethnological reconstruction took the center of the stage in dealing with the 'ethnological problem', somewhat displacing physical anthropology and a linguistics which by the last third of the century had been found by many to have feet of clay (cf. Isaac Taylor 1864), so far as being an infallible index of ethnic history was concerned.

We tend to think of evolutionary and historical, especially diffusional explanation, as antithetical, but to think of them as antithetical in the latter nineteenth century is to impose a frame of reference developed within twentieth century anthropology. Tylor, like Morgan, started on the path that led to evolutionary theory from an interest in the Asiatic connections of American Indians. We forget that an evolutionary approach, when applied to the case of their own origins, was commonly seen by Europeans as a matter of empirical ethnology. Gomme was to write on the village community as the history of a human institution in England (1891), and theoretical or universal significance was wholly disclaimed by Hearne (1879: 2), who proposed:

to describe the rise and the progress of the principle institutions that are common to the nations of the Aryan race. ...My subject is confined to the institutions of the Aryan race. I do not offer these pages as a contribution to the history of culture. I do not seek to propose or to support any system as to the origin or the evolution of man. With the theories that have been advocated on these subjects, I am not now concerned, and I express no opinion upon them and neither affirm nor deny their truth. I seek to investigate the early history of the institutions of one family of the human race, and to follow that inquiry so far only as there is positive evidence for our guidance.

In short, while to a superficial view the nineteenth century falls into two parts, an earlier ethnological period superseded by an evolutionary period, a closer view finds both the ethnological and evolutionary interests to be continuous. A general evolutionary interpretation of mankind is present among those who pursue ethnological classification and history, in the first part of the century, e.g., among Americans considering the American Indian (cf. Pearce 1954), and ethnological inference and reconstruction are of concern to those who take part in the recrudescence of evolutionary theories in the second part of the century. Indeed, the general evolutionary interpretation was never to be displaced, persisting in Boas (1911) whose entire book rests on the distinction between primitive culture and civilization, and his students. Controversy then, as now, was not about the general notion of evolutionary interpretation, but about the use of specific evolutionary models as keys to the explanation of particular histories.

The preeminence of linguistics and physical anthropology in pursuit of ethnology in the middle third of the nineteenth century may be due, one might guess, to a higher development of the two fields. Physical anthropology could draw on human biology, especially, in Broca's case, anatomy, for detailed analyses, and the linguistics of other languages could draw on Indo-European philology. Archaeology, as has been mentioned, had not developed systematic methods for study of continuous history, and ethnology itself, as the study of cultural phenomena, had proceeded slowly and irregularly. Our sense that Tylor and Morgan are founders may reflect their having provided a decade or two after Broca's address the first systematics for ethnology continuous enough with our own. [Cf. Lowie's (1937) praise of Tylor in this regard.]

Stability, or permanence, had two aspects. One aspect was the internal rate of change, the persistence of features within the particular sphere of evidence; how far back could one trace a particular type of language, physical makeup, kinship, etc.? The other aspect was external stability, persistence of the connection between the particular sphere of evidence and an ethnic group ('race'); how likely was a group to have undergone a change of language, physical type, kinship type, etc.? And what were the calculable consequences of such a change for discovering its original identity?

4.2 *Broca's Address*

Both aspects of the question are clearly delineated by Broca (more clearly perhaps than in any other work of the period – it is a trenchant piece). How he approached the question, and how he brought the rate of linguistic change into it, can best be shown by a brief sketch of the address.

The published essay has three parts (1) "Preliminary Remarks" (264-67), (2) "An Examination of Some Questions of Anthropological Nomenclature" (267-82), and (3) "On the Anthropological Value of Traits Drawn from Language" (282-319). The last two pages or so (317-19) are a résumé of 6 main points made in the lecture, essentially in its third part.

4.2.1 *Preliminary Remarks*

Part 1 states the utility, indeed necessity, of such a society, where scholars of different training can join in pursuit of common problems. Only through such an organization of diverse, cooperating talents can the immense complexity of the study of man progress. Language and linguistics deserve special attention. Among "the primitive products of human initiative" (Broca's expression for human culture, as distinct from human physical makeup), language is first in importance, being the earliest, most essential, and most durable. In another period anthropologists could misconceive the importance of linguistics, which had not yet attained its rank among the positive sciences. Now it has established principles, and many general facts, some virtually incontestable, others extremely probable, and has become one of the most valuable

and productive sources for scholars who study the history of human races. But without seeking to diminish legitimate admiration, one must ask what the exact rank is that is to be assigned in our science to the information philologists provide.

4.2.2 *An Examination of Some Questions of Anthropological Nomenclature*

Before proceeding to the heart of his argument, Broca takes up in Part 2 a preliminary matter affecting the unity of the anthropological sciences, the nomenclature of ethnic units – specifically, the dangers of superfluous and/or inaccurate terms proposed for major groups by some linguists. (The standpoint throughout the discussion is that of a careful scholar taking the long view.) In the early stage of a science collective names cannot help but be more or less arbitrary. Indeed, a wholly arbitrary name would be best, but would run the risk of being unintelligible and ignored. One must be resigned to accepting more or less conventional, appropriate names, while waiting for the more exact names that scientific progress will make possible. Neologisms, in any event, should positively not mislead. But in this regard, names created by linguists for three of the most important segments of mankind (Mongolian, Indo-European, and Semitic) are deceptive and cannot be admitted without disturbance into anthropology.

One case is the coinage by Chevalier Bunsen of “Turanian” for a linguistic family coincidental with the racial groups anthropologists call Mongolian. That which is useless in the sciences is very near to becoming detrimental, as has happened in this case; the name has led some even to invent an ethnogenic ancestor Tur (with whom Broca has considerable fun). If one wishes to distinguish the Mongols proper from the type as a whole, one can, like Latham, use the term Mongoloid for the latter. In any case (Broca makes manifest his conviction): “Linguistics will have obtained the finest triumph on the day when it will have demonstrated that the connections and distinctions which it has established coincide with the natural divisions which are inherent in the physical study of the human races.” Bunsen and his adherents should have sought to secure their own linguistic conclusions, while conserving the name introduced in science by their predecessors, the anthropologists, from classical times.

Broca is delighted with “Indo-European”, the finest claim to glory of linguistics so far. Anthropologists have adopted with alacrity the very apt term offered by the philologists, one which makes known the distribution of the peoples designated while leaving open the question of their ultimate origin. The discovery is confirmed by ethnology and anatomy. When one knows definitely their first origin, a more precise, decisive, and probably shorter term may be substituted; but the time is not yet, and the frequently used terms, Aryan and Iranian, must be rejected. Applied to the same thing, they are less than synonyms, for each announces a different disputed theory. (Broca passes over the use of Japhetic [from the son of Noah, or father of Prometheus], but notes that a recent editor (1860) had to frequently put ‘Aryans’, ‘Iranians’ and ‘Japhetics’ together in parentheses to indicate their common reference).

Broca protests on behalf of anthropologists against the misleading use of 'Semitic' for the language family and race, both of whose unity he takes as accepted. He cites Renan, Maury and Pritchard in support of Syro-Arab, a term formed on the same principle as Indo-European. (The source of discomfort in the cited authorities seems essentially the lack of fit between the linguistic family [first dubbed 'Semitic' by Eichorn]^{17a} and the notion of peoples descended from Sem in the Bible.) Broca dares to wish that the anthropological society will maintain its preference for the classical name, Syro-Arabic race. If a simpler term is wanted, Hebrewoid, on the parallel of Mongoloid, can serve. He does not propose it; he wishes only to show that one can have a simpler term, if one wishes, without adopting 'Semitic'.

4.2.3 *On the Anthropological Value of Traits Drawn from Language*

Part 3 plunges directly into the main issue. We possess two orders of traits with which to classify human races, those of language and those drawn from the physical organization of the races. The natural method obliges us to consider all traits, but also to give priority to those which present the most fixity (this is "according to the principle of *subordination of traits*").

What then of physical organization in this regard? The principal human types have not changed appreciably since the beginning of history, in either the Old or New World. Whether one is a polygenist or monogenist, the "persistence of the organization of man, compared to the mobility of his institutions and to all which emanates from initiative, constitutes if not an absolute permanence at least a relative permanence" (286).

What then as to language? Broca puts by with some unpressed objections (but later confutes sharply) the view of a colleague that specific language type is physically based; for now the matter is an instance of the insolubility of questions of first origins. Type of language is certainly to be ranged among the distinctive traits of a people or race, and can even provide more precise delineation than physical organization in some cases (289-90). But what is the degree of permanence? To be sure, never in the period accessible to our investigations has a language spontaneously (i.e., without imposition) lost its type to the point of becoming unrecognizable to the practiced eye of our scholars. But why do we admire their perspicacity and say that linguistics is one of the glories of our century? Because it has revealed unknown things, things which would have stupefied our fathers, and which scarcely twenty years ago were still found incredible, even among the philologists. It required half a century of toil and struggle to establish that Greek, German, Russian, Low-Breton are languages from the same stock as our own – so profound are the modifications, so hidden the connections.

In sum

for physical traits, spontaneous variation is so slow even after a great number of centuries that its existence is even doubtful; for the traits of language, in contrast, it is so extensive

^{17a} In fact, the term was first coined by Schlözer in 1781.

that it almost entirely masks the affiliations of languages. To demonstrate alteration of physical type and to demonstrate persistence of linguistic type are two equally difficult tasks. This suffices, I think, to establish that traits drawn from organization (physical) are infinitely more permanent than traits drawn from language (291-92).

The argument so far has assumed change in isolation. Broca now turns to the consequences of contact. While eschewing the oft-cited Haitian Creole as atypical, he notes at once that many peoples have changed language. The outcome of contact depends on many things, but especially on two: the numerical proportions and the relative degree of civilization of conquerors vanquished. On more or less the same level of civilization, numerical preponderance will tell; the case is then like that of mixture in physical organization. But with language a numerically inferior group can impose its language if it has a much superior civilization. In contrast, a blended race tends more and more to return to the physical type of the more numerous race even if that more numerous race was the conquered one (295-96).

In sum, linguistic traits are not permanent; to the spontaneous modifications which can occur through the action of time alone, especially among peoples without literature, and to which it is difficult to assign limits, are joined accidental or occasional modifications, radical substitutions which can take place rapidly at contact with a foreign race, even when the mixture of bloods is insufficient to impress enduring changes on the autochthonous race (297).

Linguistics thus does not provide anthropology with traits of the first order. When there is a contradiction between its evidence and that of anatomy, we need not hesitate. Linguists do have a great advantage over us; they can dispense with us although we cannot dispense with them. They need not consider questions of race in their work, whereas in anthropological labors, where dealing with similar groups, physical traits must be recognized as insufficient, and linguistics is especially invaluable. We are tributaries of linguistics but cannot be its slaves. We ask of it information, not commands. We take its facts as certain, but it falls to us to apply them to anthropology, and we can do this without being linguists. We confront them with facts of a different order, and base conclusions on the ensemble of all.

The address might stop at the point just noted (297-99). The theme of the proper relationship between linguistics and anthropology has been summed up with great force. And if one notices a systematic ambiguity between 'anthropology' as a general science, considering all facts pertaining to the history of human varieties, and 'anthropology' as the province par excellence of what we would today term the physical anthropologist, the situation is no different from that common today when the systematic ambiguity is between 'anthropology' as a general science, assigning special disciplines their places, and 'anthropology' as the province par excellence of the cultural/social anthropologist (the terms being understood as exclusive of archaeology and linguistics).

Broca goes on, however, to say that it is not surprising that linguistic and anthropological conclusions do not always agree. This would not happen, if all linguists knew

how, like Renan, to appreciate the limits on the legitimacy of their inductions. When they outreach themselves, their hypotheses contradict themselves, and we must wait for them to reach agreement before accepting their results. His chief targets are Bunsen, Max Müller, and the many other scholars of great merit who "do not recoil at the idea of reconciling the diversity of linguistic types with the dogma of the original unity of the human species" (301).

Broca stresses the same limitation of philology as Morgan was to stress (see above), but to a different end. Being a polygenist, he welcomes the indication that major types, such as Indo-European and Syro-Arabic (Semitic) could not be reduced to a common origin. Indeed, it is in this context that he undertakes glottochronology. He seeks to show that the rate of linguistic change is so slow (apart from contact) that a very great antiquity must be assumed to account for the existing number of diverse major groups. The conclusion has the immediate purpose of scuttling a hypothesis by M. de Bunsen, who sought to reconcile the Biblical account of origins with known diversity by extending the Biblical scale to some 18,000 years. It has the wider effect of rejecting the monogenetic thesis of Müller and others, by removing any possible common origin to a period so remote as to be unreachable by science.

In general, Broca's approach in the essay, and especially on this point, appears a defensive one, a protecting of the sphere of (physical) anthropology, and of the polygenist position, by critical analysis of linguists for going beyond established evidence and failing to agree, and by putting ultimate origins in any case beyond possible reach.

4.2.4 *Passages of Broca's Address*

Let me now give the pertinent passages of Broca's address before commenting on them further.

The spontaneous alterations of languages undoubtedly take place according to certain laws; but is the evolution of a language comparable to those algebraic curves that one can construct in their whole extent when one knows certain points? Has linguistics arrived at or will it ever arrive at this degree of precision? I am not qualified to say, but I very much fear that it is as difficult to divine what a language was like 22 thousand years ago as to predict what it will be like at the end of a similar lapse of time (303). ...

To appreciate the slowness with which languages modify themselves through the action of time alone, when sheltered from foreign influences, it suffices to consider and compare the dialects of the principle archipelagoes of Polynesia. Everyone knows that Captain Cook took on board a Tahitian named Tupaia whom he thought to use as an interpreter when he reached New Zealand. Dumont d'Urville, in the *Partie philologique du Voyage de l'Astrolabe* (1834) (vol. 1, p. 273, Paris, grand en 8°), published extended vocabularies of most of the Polynesian dialects and compared these vocabularies two by two, counting identical words, words simply similar, and finally words which seemed to him completely different. Not knowing yet the laws which presided over the alteration of consonants in each archipelago, laws which were discovered by his successors, Dumont d'Urville had to necessarily arrange in the category of words completely different a great number of words which differed only by pronunciation. In spite of this he found that the number of very similar

or identical words varied, following the terms of comparison, from 41 to 74 per cent. For example, among 431 words considered in Tahitian and Maori (New Zealand), he found 100 identical words, 257 simply (.8, .6, .4, .2) similar words, in all 357 words more or less similar, which makes more than 80 per cent; but in taking only *identical* or *very similar* words to establish the comparison, he expressed the *coefficient of identity* by the figure of 53 per cent. In proceeding thus, he summarized in the following table the four principal Polynesian languages:

	# Words compared	Coefficient of identity
Tonga and Tahiti	447	41 (per cent)
Tonga and Maori	713	45
Tonga and Hawaii	247	46
Maori and Tahiti	431	53
Maori and Hawaii	208	65
Tahiti and Hawaii	221	74

These coefficients confirm and explain the facility with which the Tahitian Tupaia could use the language of New Zealand.

It is a question now of determining approximately how many centuries since these islanders, who still understand each other, have ceased to be in communication. That the period of that separation is very remote, one cannot doubt; but in this regard, one had nothing certain or even probable until the words of Horatio Hale, author of the volume entitled *On Ethnography and Philology of the United States Expedition under Charles Wilkes* (Philadelphia, Lea and Blanchard, 1846). This eminent philologist has not only, as Dumont d'Urville, compared the words of the Polynesian languages in their existing forms; he has studied the modifications of phonetic elements, discovered the laws of successive alterations of pronunciation, and traced, by philology, the itinerary of successive migrations which peopled the diverse archipelagos of Polynesia. These migrations, according to him, initially led the first Polynesians into two neighboring archipelagos of Melanesia, the archipelago of Samoa or the Navigators, and the archipelago of Tonga or the Friends. These first two waves of Polynesian immigration came separately from a region much further west, probably one of the most western islands of Malasia. But I will not follow the author to this point; this part of his work was almost entirely conjectural. Be that as it may, the islands of Samoa and Tonga, peopled by men who spoke two dialects of the same language, were the first two homes from which the other migrations ultimately radiated. The archipelagos were peopled successively and the author thought to follow these hardy adventurers from Samoa to Tahiti, from there to the Marquesas, from the Marquesas to Hawaii, etc.

These facts, deduced from linguistics, have found valuable confirmation in the local traditions, and I add that the itinerary traced by M. Hale accords rather well with the tables of Dumont d'Urville on the coefficients of identity of the Polynesian languages. *The languages which, according to Dumont d'Urville, are most different are also those which, according to M. Hale are the most separated by time* [Emphasis supplied].

Up till now we have only spoken of the succession of migrations, of the order according to which they were effected, without troubling ourselves with duration of time. But for many points in Polynesia, M. Hale has collected fragments of chant or recitation in which are enumerated from generation to generation the names of the leaders who had succeeded one another since the arrival of the first inhabitants. Thus, the list of the Marquesas covers 88 generations; that of Hawaii, 67; that of Mangareva, only 27, etc. It is indeed the generations and not the leaders that are indicated on the lists, in view of the fact that when many brothers ruled one after the other they were enumerated within the same verse and formed only one unit on the general list. At 30 years per generation (Hale's estimate), the population of the Marquesas would date from 2640 years ago, that of Hawaii would date from

2010 years before Tameamea, the chief of the present dynasty, which would make until our day nearly 2400 years. M. Hale thinks, it is true, that the first generations were purely mythological, and he thus reduces in a somewhat arbitrary fashion the duration of the occupation of Hawaii up to Tameamea to 1400 years, or, until our day, almost 15 centuries. I am not as convinced as he of the legitimacy of this suppression. Revered personages of a mythological character are far from always being imaginary; most often they have had a real existence, and it is perhaps excessively rigorous to show oneself more skeptical with regard to the heroic times of Polynesia than with regard to Romulus, who, while not the son of Mars, not nourished by a wolf, and not raised to the sky, nonetheless existed. I remark first that the different lists collected by M. Hale accord very well with the general conclusions of this author. Thus, Hawaii, according to him, was peopled from the Marquesas and consequently after the Marquesas. Now, the list of the Marquesas contains 88 generations; that of Hawaii contains only 67. The difference of 21 generations or about 600 years seems to me in perfect harmony with the succession of migrations. Because, for a horde of adventurers to entrust themselves to the hazards of the sea in search of an unknown and distant residence, it is necessary to have an excess of population, that the whole archipelago of the Marquesas be already occupied, and such a result could not be produced in less than 5 or 6 centuries.

But let us accept the reduced figures of M. Hale. Hawaii would thus have been peopled about 1500 years ago from the Marquesas islands; but these people came from Tahiti, and the undetermined epoch at which they were separated from the Tahitians was undoubtedly well before that at which the Hawaiians in their turn separated from the Marquesans. If one says that the Hawaiians ceased to communicate with the Tahitians about 2000 years ago, one no doubt remains short of the truth, even given that the reductions made by M. Hale are correct.

Now, according to Dumont d'Urville, the coefficient of identity of the language of Tahiti and of that of Hawaii is 74 per cent. But if in place of taking the very similar (*très semblables*) words one takes all words which are not essentially different one finds 219 out of 221. Only two words in the 221 words taken at random that Dumont d'Urville has examined have become unrecognizable! That is all the alteration that two languages entirely separated for more than 1500 years have been able to undergo naturally!

The New Zealanders of the Polynesian race (one knows there are two races superimposed on New Zealand) came, like the Tahitians, from the Samoan islands. The separation of the Tahitian languages and the Maori, the language of New Zealand, is thus anterior to the arrival of the first immigrants on the archipelago of Tahiti. One assumes that this separation ought to be set at 3000 years at least. The table of Dumont d'Urville effectively shows that the coefficients of identity between Maori and Tahitians are less than those between Tahitian and Hawaiian, which accords well with the preceding evidence. Even though reduced by a third, that is to say by 2000 years, the antiquity of the separation of Tahitian and Maori, the already cited history of Tupaia would show that the *spontaneous* alteration of words and grammatical forms could only disfigure a language at the end of a very great number of centuries...

Imagine, after such examples, how many centuries would be necessary for a single language, left to itself to beget (descendants); I do not speak of languages such as Greek, German and French of which the parentage, so long effaced, can be rediscovered at least by linguistics, but such as the Chinese, Sanscrit and Hebrew, which have resisted up to now all attempts at scientific connection. The period of 18,000 years, postulated by M. Bunsen (see above), would be completely insufficient. With the rate at which things have proceeded during the time accessible to researches, it is not 18,000 years but perhaps 100,000 years that would be necessary to make plausible the hypothesis of M. Bunsen.

The idea of such an antiquity does not surprise me; those who saw Europe covered with glaciers, those who, in an epoch incomparably more distant yet, fought in our clime with weapons of stone the rhinoceros and the mammoth – they undoubtedly spoke a human language at a date far distant enough from us for the five or six thousand years of our history to be no more than a moment in the life of humanity, and for one to be able to assume anything about the changes that languages could have undergone during that incalculable duration. Envisioned thus, the conjectures of M. de Bunsen no longer seem impossibilities. But what use can we make of them? None. To prove that a thing is not impossible, is to anticipate an objection, nothing more; it is to clear away an obstacle (*déblayer la place*) in order to give full play to one's imagination; it is not even to make the first step toward the demonstrations that science requires (312).

4.3 *Broca's Use of Glottochronology*

It is striking to find an anatomist ranging across disciplines, establishing a framework for the general study of the history of mankind, and in passing, joining separate lines of evidence to calculate a rate of linguistic change. The case reflects the only successfully institutionalized intellectual unity that anthropology has even enjoyed, a unity around questions of history. Given Broca's own conclusions, that linguistic change was too slow to be of ethnological value, it is not surprising that he did not develop his invention, or ever bother to draw explicitly all the implications of what he had presented. Indeed, as has been indicated earlier (section 3), his account of the work of his predecessor, Dumont d'Urville, can most charitably be described as cavalier. Nevertheless, some comment on the details of the first known control case for glottochronology are in order, before considering its still-born status.

4.3.1 *Technical Comments*

Broca was the first and only person to make use of Dumont d'Urville's lexicostatistics, so far as I know. His use of Hale's lists of generations to obtain an approximate absolute time depth is a stroke also entirely original with him. In accepting Hale's reduced figures for time depth, Broca furthermore chooses against, not in favor of, his own views. A longer time period for the same 'coefficient of identity' would have made his point that much more impressive. On the other hand, the choice of the Tahitian-Hawaiian case (the one with the highest percentage by far) considerably facilitates his argument, although, to be fair, it may have been the only possible choice, in the absence of generational data for other cases.

Broca does choose in his favor when he includes the 'similar' words as well as the 'very similar' so as to find only two pairs not recognizably related, as between Tahitian and Hawaiian. The result is an implicit coefficient of similarity of 99% and an implicit rate per millenium of the same. As has been seen in analysis of Dumont d'Urville's comparisons, and might have been seen by Broca, who elsewhere spoke of sound correspondences, the lesser categories of resemblance are not of the same order as the first three, and not at all convincingly of common origin.

One may wonder if Broca made some private calculations as to the relation between rate and time-depth. The estimate given in the address for time-depth necessary for the supposed common origin of languages, 100,000 years, may be merely a round figure and order of magnitude. Still, 100 millenia and the rate of 99% implied by his argument from Tahitian-Hawaiian would bring the 'coefficient of identity' to 14%, a figure about as low perhaps as he might admit as having significance for relationship.

Had Broca calculated a rate per millenium from the figures for "very-similar" (identical) pairs for Tahitian-Hawaiian, he could have found the 74% coefficient for 2000 years (his estimated minimum date of separation) to yield a rate of about 86% – precisely the rate arrived at by Swadesh for his revised 100-item glottochronologic list (1955). It is not likely that there is much more than coincidence in this fact. Basic vocabulary indeed is involved in both cases. 71 of Swadesh's 100 basic items are included in the 230-item Tahitian-Hawaiian comparisons, and 30 of Swadesh's supplementary 100. Moreover, the items are concentrated toward the categories of perfect and near perfect identity, i.e., the categories whose historical connection is not in doubt, as can be seen in the following table:

	100-item list	Supplementary 100-item list
1.	35	15
0.8	15	5
0.6	9	5
0.4	7	3
0.2	3	1
0.0	1	1

A modern assessment of the chronological import of Hale's genealogies, and of the linguistic relationship between Tahitian and Hawaiian, is of course needed before one can accurately judge the extent to which the one case Broca deemed sufficient would have accidentally, or justifiably, given an approximately accurate indication of a rate that was to be found nearly a century later.

4.3.2 *Glottochronology and Polygenesis*

As for Broca himself, clearly his purpose is to establish the priority of physical organization in anthropology, and to protect the polygenist hypothesis. He does not discuss his glottochronology as a method at all. Having used it to show great slowness of spontaneous change, and having further argued that rapid alteration in language can be due only to foreign contact (of a type implicitly taken as restricted to relatively recent times), he passes on to the use for which the glottochronology was intended. Recall that after having argued the primacy of physical traits as most stable, he then argued the primacy among the rest (those due to man's initiative)¹⁸ of linguistic

¹⁸ An instance of a pervasive theme in the human sciences, that of defining as invariant and as prior object of study something considered to be beyond human conscious intervention or creation.

traits, as most stable of these; and for great slowness of spontaneous change in both, such that, given the existing diversity of mankind, the question of the ultimate formation of the main types is beyond scientific reconstruction. Glottochronology appears here as an empirical weapon because of what it makes impossible to know.

Broca concludes his main argument by passing briefly in review the two main threats to his position, Darwin, and a series of linguists. Both maintain both original monogenesis (Broca finds the thesis to be logically implicit in Darwin) and rapid spontaneous alteration of type (Broca finds the thesis to be logically implied by the linguists). The response in both cases is the same. Is Darwin right or wrong?

"I know nothing of it, I do not wish to know. I find in the things accessible to science, sufficient nourishment for my curiosity without losing myself in the darkness of origins" (314). Darwin does not know, and those who refute him know no more than he. Of both Biblical and secular theories of monogenesis in language, we can, he says, choose among the contradictory systems which embrace 'the insoluble question of origins'. (Here as elsewhere the lack of agreement among linguists is taken as evidence of lack of success, and a sign that claims need not be yet taken into account in general anthropology.) On the theme of origins, Broca sums up: "Let each of us in this regard feel and think in his own way in his moments of leisure; but let us note well all these systems are outside of science, that they have been up to now strangers to linguistics properly speaking, as well as to anthropology; that they will perhaps always be so and that we can today make no application of them in our studies" (315-16).

As we know, Broca was to lose the argument for which he invented a glottochronology, at least for most of the field most of the time,¹⁹ although linguistic and glottochronologic arguments have been introduced (misleadingly) into one recent effort to argue, like Broca, for persistence of major racial divisions since the threshold of human status (Coon 1961). The discussion of the relative utility of lines of evidence and of disciplines continued. For those for whom cultural phenomena became the central concern, language was to become, especially in American anthropology, almost dogmatically the principal guide to classification and history, for much the reasons considered by Broca – as being the most stable and intimate of things due to man's initiative. In important part for the latter reason – what language as an institution reveals – Whitney was to argue that the role of language was the larger and more important, although in summing up his views he rejected discord or questions

¹⁹ But cf. Hovelacque (1877): "Thus philology furnishes a new and formidable argument to the polygenists, who were already supplied with so many before" – his English translator feels compelled to insert a bracketed paragraph to disagree – present diversity, Keane writes, does not exclude original unity. And Sayce (1880: 317-18), starting from Whitney's view of language as a social creation and institution (cf. 301), and sharply distinguishing language from race, philology from ethnology (316, 317), concludes with an agnosticism rather like that for which Broca argued. Monogenesis of one (race, or language) would be compatible with plurality of origins of the other, and conversely; comparative philology agrees with geology, prehistoric archaeology and ethnology in showing that man as a speaker has existed for an enormous period (317-18, 322).

as to relative rank among "those two branches of ethnological study" (1867: 382). One quotation from this still interesting discussion must suffice to illustrate the view that was to dominate:

And if, in certain circumstances, a race may change its tongue, while yet retaining in its physical structure evidence of its descent, a race may also undergo a modification of physical type, and still offer in its speech plain indications of its real kindred... This is the grand drawback to the cogency of physical evidence of race, and it fully counterbalances those which effect the cogency of linguistic evidence, rendering the aid of the linguist as necessary to the physical ethnologist as is the latter's to the linguistic ethnologist. (376)

Whitney's argument reads almost like a refutation of Broca's argument; in any case his discussion of rate of change in his book makes no mention of establishing a representative rate or of its use to estimate time-depth.

5. CONCLUSION

The idea of a glottochronology would seem to have died stillborn in the context in which it appeared. Its inventor had no particular further use for it, and others did not extract and develop the idea. Development would have been possible, perhaps by application (and refinement) of Broca's Polynesian data, or by seeking additional control cases as well. The notions of basic vocabulary and lexicostatistics were available, and would have fitted well the concern for persistence, with which the idea was introduced. Perhaps, to be sure, it would have been remarkable had someone both seen Broca's address and been prepared to extract and develop the idea. The independent occurrence of Latham's Apothegm a little earlier suggests that the climate was such as to prepare the seed but not the ground.

One parallel between glottochronology in Paris in 1862, and modern glottochronology, is worth noting. Once his methods had been developed to a certain stage, Swadesh, like Broca, was concerned to establish a general conception of prehistory (concerning its linguistic dimension), albeit a conception the opposite of Broca's polygenism and definition of what was accessible to science. Technical concern with the foundations of the method was left behind, the purpose of estimation being sufficiently well served. It remains to be seen whether the recent contributions of Dyen, van der Merwe, Sankoff and others will crystallize into a third stage of development, concerned again with the foundations of glottochronology.

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THE AMERICANIST TRADITION IN LINGUISTICS

Berkeley is an obviously appropriate site for this symposium. For a generation it has been the most faithful and active center of Americanist research. One has only to point to the many scholars who have been trained here, and to the many monographs they have provided (in a series which we must hope is not to be stifled fatally by a false economy, insensitive to the unpurchasable respect and good will built for a Press and its University by such a series.). And if one looks for precedent for this symposium, one finds it at Berkeley, again in connection with the Linguistic Society not quite a generation ago. The general essay in that earlier symposium by Hoijer, indeed, says much that is valid today, and on the saying of which one could not improve (Hoijer 1954:3-12).

With such precedent, and with so many leading Americanists in the audience (including Mary Haas, and my own first mentor in the field, Carl Voegelin), I cannot but feel presumptuous in speaking on 'the Americanist tradition'. Others here have shaped it more than I. Let me speak not so much with authority, then, as with surmise. Let me consider the sense in which an Americanist tradition can be said to exist, and project a conception of what the tradition ideally might be in relation to the three communities – general linguistics, Americanist studies, Native Americans – that constitute its public and shape its evolution.

In devoting one of its three Golden Anniversary symposia to American Indian languages and American linguistics, our Society may have intended simply to recognize the great part that Americanist work has played in its past. Loyalty to the sources of one's being – 'piety', as Santayana called it – is indeed essential to health, in a profession as in a person. The Americanist tradition has relevance as well to what Santayana (1905:276) called 'spirituality' – concern with the future and with ideal ends, and I shall try to bring out something of that.¹

Work with American Indian languages of course has a history (by now a considerable history); but a *tradition* implies something more, a continuity not only of object, but also of approach. For some scholars, Americanist linguistics may seem to lurch through time, disconnected, diverse, uninformed by tradition. Yet an irreducible minimum of continuity in approach, of values and understanding, stems from the very nature of the materials with which Americanists work. These data are not replaceable, as are laboratory substances and college sophomores. New data adds, and may illuminate, but cannot wholly replace. There must be continuity across generations, simply because some of the work requires informed knowledge of what predecessors did and why and how they did it.

Often this is so because a given document is the only witness, or only extensive witness, to a language, or state or aspect of a language – hence, precious and to be preserved, as all testimony of the working of the human spirit in the shaping of distinctive symbolic form. The maintenance of such material, its renewal in usable form, is an essential contribution to general linguistics, Americanist studies, Native American communities, all three. Americanist linguistics began as philology of people without philologies of their own, intellectually as part of the stream of interest that founded classical, then Oriental, philology, politically in a context of colony and conquest; it cannot escape the responsibility of this history.

The past interacts with the present for reasons internal to technical work as well. Although each new generation in methodology tends to show contempt for data obtained before its advent, scholars may provide old data with new contexts, such that it may help answer new questions. The more we come to know of a language, or group of languages, the more we can learn from what we have. Even a scanty early word list, if carefully interpreted, can play a part (as work of Madison Beeler, for example, demonstrates). Even if a language is recently well documented, early data may uniquely attest etymological connections, semantic change, or bases of present variation and dialect mixture.² Languages are in history, and for American Indian languages early data is precious for showing trajectory over time.

Permanent need for philology is not limited to materials that are old. Ample, generally reliable modern works may require checking against original documents.³ Thus the original materials, even if published, must be preserved and consulted.⁴

These points bring out two respects in which there must be continuity over generations, given the nature of our data as something for which we

care (in a dual sense). The safety and state of materials, even in institutional collections, let alone private hands, cannot be taken for granted. Only personal concern can ensure that manuscript material is not irretrievably misfiled, mislaid, or loaned, or that untranscribed wire recordings and tapes do not become useless through lack of use. An active knowledge of the tradition, as to the activities of predecessors, and the disposition of their work, is essential. Further, to use data from an American Indian language commonly is to depend on work of a few scholars, sometimes fundamentally just one. Documentation of Kathlamet Chinook is essentially from work of Boas, of Clackamas Chinook from work of Jacobs, of Takelma from Sapir, of Chitimacha from Swadesh, of Tunica from Haas, of Tonkawa from Hoijer, etc. Knowledge of a language becomes interwoven with knowledge of a scholar, of the pertinent stage of his or her development, and of that of the field. To interpret the materials, one must interpret the context in which they came to exist. The study of predecessors – of their training, models, habits of transcription, relationships with communities and individuals – thus contributes to both the historiography of linguistics and the analysis of languages.⁵

Leading scholars are aware of the points rehearsed here. They are aware as well that what has been said falls short of actuality. In speaking of a continuity intrinsic to materials, I have reflected not actual history, but an ideal. And in stressing the nature of materials as basis for tradition, I have perhaps played into the hands of those who stereotype Americanist work. Let me try to clarify the relation between an Americanist tradition and Americanist materials, adding to the perduring need to use our materials the perduring need to obtain them.

Yakov Malkiel (1964) has said of Romance linguistics that its most distinctive traits may be deduced from an inventory of its characteristic resources. Whereas the ambit and tone of Romance linguistics can be seen as greatly predetermined by *abundance* of material, preserved or accessible, Americanist linguistics might be seen as greatly determined by initial *absence* of material, and difficulty of access to it.

Much of the Americanist tradition does follow from this situation: the languages were unwritten, exotic, many, and diverse. Ways had to be found to record them, to comprehend them, to relate them. There arose a continuing emphasis on accuracy and salvage of data, within a national culture largely indifferent. The emphasis has been sustained, given the continuing disproportion in number between workers and languages, and recurrent advances in knowledge and method that have motivated

return to the same languages, so long as possible. (I have alluded to the frequent disdain of one generation for work of its predecessors.⁶ The complement, in part the source, of such disdain has been the need to obtain new materials to help to answer new questions.)

Americanist linguistics, in sum, has looked to the field and the present, more than it has looked to archives and the past. Energies have had to be concentrated on providing data, a task shadowed from the outset by ceaseless decimation of speakers of the languages. Here is the element of truth in the common stereotype of the tradition (and of the anthropological and structural linguistics associated with it).

There follows from all this the importance of field work. What does *not* follow is any single relation between the obtaining of data and the purposes the data is to answer, or the uses to which it is put. There follows no single relation between need to obtain data and kind of data preferred, the sector of language given precedence, the mode of analysis practiced, or academic or intellectual context of interpretation. It is worth remembering that the sanest, most constructive nineteenth-century scholar to interest himself in Indian languages, Whitney, had no field work with them, and that first-hand collection of data did not prevent Powell and Brinton from misinterpreting languages in terms of received evolutionary and typological ideas far more grossly. If Barton, Jefferson, and Powell sought word lists, while missionaries and Boas sought grammars; if Boas analyzed grammatical categories as central, while some successors focussed on phonology as fundamental; if Sapir developed analysis of aspect, grading, and other semantic features about 1930, while Bloomfield developed immediate constituents; if Swadesh and Pike insisted on use of meaning in description, while Harris sought to avoid it; then the fact that all had done first-hand work with Native American languages cannot explain their differences.

To interpret the course and variety of Americanist work, one must have recourse to what Malkiel called "other powerful determining factors" (1964:671): the specific state reached by a subdiscipline, the matrix of culture that gave it birth and sheltered it, the impact of major leaders. Had descriptive techniques, recording devices, analytic theory developed at a more rapid pace, or earlier in time, in relation to the exploration and conquest of North America and the resulting linguicide, Americanist linguistics would be far different. Had the nineteenth century United States been more French or even British in cultural tradition; had anthropology and linguistics remained so much the province of Western naturalists and Eastern Wasps as to exclude the infusion of German in-

tellectual tradition and liberalism brought by Boas, Sapir, Lowie, Kroeber, Radin, and others; had Indo-European training and Americanist field work come together in a sustained way before Sapir and Bloomfield; had Boas been disinterested in languages as a key to cultural theory; had Sapir or Bloomfield, or both, remained Germanists; had Sapir been able to train students at Berkeley from 1917; had any Native American community been able to support, or insist on support for, study of its own language before recent years; had these and any of a variety of other factors been otherwise, the field would be significantly otherwise as well. What we have, what we know, and what we need to obtain and to know are functions of the interaction of cultural, institutional, and personal factors.

The point is important, because Americanist (and American anthropological and structural) linguistics often has been explained away in terms of materials alone. The importance of the materials gives such explanation enough truth to make it especially dangerous.⁷ The evolution of the Americanist tradition, past and prospective, can be better understood if we consider the extent to which the reverse of the stereotype is true. *To a great extent, the materials have not determined the scholarship; rather, the scholarship has determined the materials.*

We need to know far more than we do about the history of this scholarship. Fortunately, the number of serious and useful studies has grown considerably in recent years. Let me simply mention, alphabetically, work of Darnell (1967, 1969, 1971a, 1971b, 1974), Darnell and Sherzer (1971), Haas (1969a, 1969b), Hanzeli (1969), Hoijer (1973), Hymes (1963, 1970, 1971, 1973), Landar (1975), Rowe (1974), Stocking (1974a, 1974b), and Wells (1974). The main task of positive history has been, and continues to be, *to describe and explain the selective use, and creation, of the resources for the study of American Indian languages.* Much remains to be done. Perhaps some linguists will be attracted to the work by the sheer shock of discovering how false is the current cliché that the nature of the work can be deduced from the nature of the language situation alone. The sheer discovery that the relations between materials and scholars are diverse, and significantly so, may attract research. In any case, I should like now to generalize from the diversity of this relationship, and to state an ideal conception of the Americanist tradition. The reference point of the ideal is indeed the material of the subject, but its comprehensiveness allows for the varied factors that have shaped and will continue to shape its development. An ideal conception might be formulated in these terms: *to know all that can be known about, and by means of, Native American languages.*

With regard to the past, the definition provides scope that is natural to understanding of materials themselves, as an aspect of linguistics, and to explanation of the work which the materials reflect, as an aspect of historiography. What one wants to know about Native American languages, what one wants to know by means of the languages, what information one uses – these parameters have defined a history of considerable change and variation. In particular, the relation of Americanist linguistics to its major constituencies has shown notable change. Study of Native American languages has sometimes been central to general linguistics, sometimes marginal; it has been central to American ethnology for some, not so for others; it has never yet been central to Native American communities themselves, but that time may be coming. Certainly there is a notable change from nineteenth-century writers (Barton, Pickering, Whitney, Brinton) in all of whose pages the Indians are by convention collectively extinct or about to become so, to Boas, for whom Charles Cultee, George Hunt, and Henry Tate are title-page collaborators, to those linguists today who are retained, even paid, by Indian communities.

With regard to the present, the definition has most of all an important corollary: *use all there is to use*. That is, use, therefore get, fresh data whenever possible; marshal data already extant wherever relevant; employ ideas from whatever discipline and source if helpful. The study of Native American languages is ideally an integration of a plurality of modes of work, disciplines, and orientations.⁸

With regard to the future (and as a perspective on the whole of the tradition), the ideal definition implies a comprehensive continuity that is far from being realized. The definition suggests that the materials of the field, far from having wholly determined the scholarship, *have not determined it enough*. The scholarship has seldom been responsive to the materials for their own sake. So much is this the case that today there is perhaps not a single language for which the extant information has been assembled, critically assessed, and made available in an integrated form. Not one. New data continue to be added, and that is essential; old materials are used by specialists, for specific studies; but critical editions of texts, comprehensive dictionaries integrating all vocabularies, lucid grammatical handbooks, systematic guides to languages, language families, language areas – these expectable, indispensable instruments hardly exist. Only a small part of what can be known about Native American languages, and by means of them, can be known now. The basic tools are not in working order.

The prompt, proper answer to this account is that there has not been the support necessary. That is indeed true, but not in itself the whole of the matter. More scholars and dollars are required, but are not in themselves enough. Institutional bases of continuity are essential, and few exist.⁹ It ought to be embarrassing, shocking, that there is not a minimum of one chair in Native American languages at each of the state universities. (I am not sure that there is one such chair in the entire country.) There ought to be Institutes of Native American Languages, venerable Institutes that would now be celebrating or approaching their centennials. The scandal of the lack of institutionalization for the Americanist tradition does reflect public attitudes and policies; but it perhaps reflects also a limitation of our own. The continuity of the tradition has depended far too much, far too long, on feats of individual dedication. Most of us have not thought we could aspire to establish the subject in its own right, as a matter of right. Apart from great individual accomplishments, then, such as those of Franz Boas and Mary Haas, attention to the work has depended too much on the waxing and waning of interest in it in the three primary constituencies (general linguistics, ethnology, Native American communities). Until recently the first two, the scholarly communities, have had by far the major role. It may be that only now, when Native American communities become actively involved in shaping their own cultural policies and provide a political constituency for the work, that public support of institutionalization will grow.

To base the work of the tradition on work of Native American scholars and communities as much as possible is the fundamental challenge now and for the future. Many problems are entailed, but it may be only on this basis that Americanist linguistics can realize its potentiality as an object of study in its own right.

In sum, determination of the work by the nature of the materials is not a shackle to be escaped, but an aspiration to be realized. Success may depend on acceptance of a responsibility to the materials because they represent languages that are 'American', not simply in the sense of pertaining to a hemisphere or country, but in the sense of pertaining to members of our own communities, both local and scholarly, in which the languages and the materials retain social function – if not that of personal or religious communication, then that of ancestral heritage and source of identity. The Americanist tradition, having begun as the study of languages of a fading past and far west, will find fruition as the study of the languages of citizens.

Let me anticipate the consequences of such a fruition by way of conclusion.

When one considers the study of language from the standpoint of Native American *communities*, topics come into view that might otherwise seem peripheral. The first, most far-reaching consequence of looking at matters from this standpoint is to extend the tradition to encompass the full range of sociolinguistic inquiry. Let me just mention salient topics: Indian norms for English; norms and etiquette of speaking (independent of the language in question); norms of language use originating outside Indian communities, but which affect Indian lives; verbal repertoire and the specialization of languages to particular functions; language obsolescence, maintenance, standardization, and revival; the relation of a community to an ancestral language no longer in use; the role of members of a community in research on their languages, used and ancestral.¹⁰

From this standpoint, Americanist linguistic research is not only a concern for something with roots in the past, but a concern for something that broaches all the major issues as to the place of language in the society at large. Quite in contrast to common stereotype, Americanist linguistic research has the opportunity to be something of a prototype for a broad-based linguistics of the future. It can become a prototype of a linguistics that is a community science as well as an academic discipline.

It may be easier to extend the topical scope of the field than to meet the challenge of changed relationships to the communities whose languages and language problems define the field. There is first of all the residue of colonial relations. Native American communities typically find that knowledge about their languages, as well as about their cultures, is stored in archives, libraries, and heads distant from them and sometimes closed to them. A year or two ago Deni Leonard posed to me the question: what would one see if one took a map of Native American communities, and superposed on it a map showing the locations of scholars who know about the languages of those communities? I could only reflect on the fact that such a demonstration would show Wasco in central Oregon and Washington on the first map, and in Chicago and Philadelphia on the second. A good many linguists are working to overcome these synchronic discontinuities, but we all know that much much more remains to be done.

A sense of responsibility to communities entails a revitalization of the Bloomfieldian concern for the state of linguistic knowledge in the general society. Members of Indian communities often suffer from the misin-

formation and misconceptions about language provided them by the schools and persons to which they have access. (They may have been led to believe, for example, that to write an Indian language is to 'put it into English'.) The kind of crusade regarding naive American 'folk-linguistics', which Bloomfield waged, is more than ever needed now. The costs to Native Americans themselves for lack of it are sometimes high. Let us put an end to the irony of scholars travelling great distances, even across oceans, to learn about a language from speakers some of whom have been persuaded to think of it as inferior or worthless.

Responsibility to communities entails an active sense of audiences who are not linguists. It is difficult not to write for the professional reference group to which one belongs, and from which one needs recognition and support, but it is increasingly necessary. To do so goes against the deepset grain of terse elegance so admired in linguistics, a stylistic norm that is part of what has attracted many people to the field; and it goes against the widespread assumption in the academy that something ordinary people can understand cannot be good. Nevertheless, new styles and genres of communication must be developed. Here again, Bloomfield is perhaps the great example to emulate. It is of course not possible to avoid all technical concepts and terms, but it may be surprising to find how much can be made clear, and stated accurately, with sparing use of them. Americanists who master modes of exposition that can be useful to Native Americans may encounter neglect or active disdain from their colleagues, especially from linguists, among whom elitist attitudes have been pervasive. Nevertheless, it is a price one must be prepared to pay.

Practical work has specific demands and costs, but it will in fact be prerequisite to most future field work, and some younger scholars would not want it otherwise. This union of theory and practice, enforced by Native American communities, may indeed provide new depth and richness.

The relation of Americanist linguistics to ethnology will mirror the situation just described. The kinds of contribution that linguistic work can make to sociocultural understanding will be enlarged and enhanced, but at the same time mediated by the relation to Native American communities. If we think of this relation as 'ethnolinguistic', then the established modes of ethnolinguistic inquiry have an opportunity to flourish as never before. Broadly speaking, ethnological interest in linguistic work has had two main concerns, classification and interpretation. Of the four modes of classification of languages, and means of speech –

genetic, areal, typological, functional – only the genetic has had sustained progress in recent years. Much remains to be learned by means of it, but much awaits revival of areal work as well.¹¹ Typological study, both linguistic and ethnolinguistic,¹² and functional, sociolinguistic classification, have hardly begun to be pursued in a thoroughgoing way, but are likely to have special interest in the future, the one having to do with the ways in which the languages, as symbolic forms, have shaped and been shaped by Indian cultures, and the other having to do with the role of the languages still used in Indian life. By classification, let it be added, I understand a step that leads to explanation, the two being interdependent.

The interpretation of culture through linguistic data has far from reached its potential contribution. Much of the kind of work that can be done involves vocabulary – place names, personal names and titles, the terminologies of various activities – and such work has been shunted aside during the development of formal models of language structure. It has seemed the concern of the antiquarian or ethnologist, but not of the linguist. Much of what Native Americans wish from scholarship, however, as to their own traditions and sometimes rights (as attested for fishing places by sitemames, for example) involves analysis of vocabulary. This is one of the respects in which Native American concerns may enrich the effective scope of the field. The mode of cultural interpretation most actively pursued today is interpretation of texts. Lévi-Strauss has made the world aware of riches hidden in the old unwieldy volumes of the Jesup North Pacific Expedition, and BAE. Much more remains to be disclosed: structure and meaning that can be found only through close control of the language of the texts. Although much of the poetics is lost, much remains to be recaptured. Such work is a prime example of service both to scholarship and to Native American communities themselves. Often the narrative tradition has been disrupted, and scholarship is necessary to bring to life again the oral artistry hidden in old print.¹³ Not that it should be assumed that oral artistry disappears with disuse of an aboriginal language. Traditional style may remain vital, and be appreciated and studied, in English. Such study may contribute to contemporary schooling and education, and may illuminate older materials as well. The norms implicit in native language texts are commonly both aesthetic and moral,¹⁴ and current judgments of the propriety of form and conduct may shed unexpected light on texts generations old.

From a community-oriented standpoint, it would be reasonable to attend to the creative work in verbal art going on now.

If one includes the literary uses of language in our field of concern, as did Boas, then what has been done so far is likely to seem only prologue to a vast expansion of attention in the decade ahead.

A base in Native American communities is likely to strengthen the relation of Americanist linguistics to general linguistics. The relation has waxed and waned, as the perceived significance of structural diversity in language has waxed and waned. A virtue of the perspective sketched here is that it makes inescapable the two-sided nature of explanation of language. From the standpoint of a Native American community, one must be concerned to explain not only what Hopi, say, is like in virtue of being a language, but also with what it is in virtue of being the language of the Hopi.

Some may think that the Americanist tradition seized one pole of this duality, the particularistic pole, leaving the universalistic pole to be grasped and developed by Chomskyan generative grammar. Nothing could be further from the truth. The first great mission of Americanist linguistics under Boas was to establish universality. Sapir gave the egalitarian thrust of the work its most memorable expression, from a typological standpoint, when he wrote "When it comes to linguistic form, Plato walks with the Macedonian swineherd, Confucius with the head-hunting savage of Assam (1921:235).¹⁵ The Americanist tradition did, and does, stress the need for an inductive, cross-cultural basis for validity of universals, and cultivates respect for the contours and spirit of the particular language itself; but there are signs that languages themselves are imposing some of this respect anew.

We touch here on the one philosophical orientation which may be said to be inherent in the Americanist tradition. The orientation does not derive automatically from concern with the object of study, and it has not been present always and everywhere. It derives from the German intellectual tradition in which so many major scholars have been rooted. Nor has the orientation been always what we would make of it today. Throughout much of the nineteenth century it regarded American Indian languages as a whole as one type; respect for specific individuality was as gross as a hemisphere and an evolutionary stage. What is noteworthy is that the error was purged toward the end of the century and beginning of the next by men rooted in the same tradition, who applied its concern for inner form at the proper level, that of specific languages, and who did so informed by far closer knowledge of specific languages. Universality of structural skeleton and functional power was insisted upon, as against cultural prejudice and evolutionary stereotype, but the ac-

tive mode of investigation of general properties, was typological.

This preference for a typologically oriented general linguistics derives, I believe, from a certain view of the relation between particular and general in the human sphere. The philosopher Ernst Cassirer has explained the view by contrast to that of Spinoza. For Spinoza, particularity was limitation, and uniqueness of personality the polar opposite of the infinity of God and nature. To remain bound in uniqueness was necessarily to be in danger of anthropocentrism and anthropomorphism. In contrast, Cassirer (1961: 24-25) says:

The neo-humanism of Goethe, Herder, and Humboldt calls for a different union. To them, the Spinozistic thesis, that definition is limitation, is valid only where it applies to external limitation, such as the form given to an object by a force not its own. But within the free sphere of one's personality such checking heightens personality; it truly acquires form only by forming itself. Consequently, though we are obliged to recognize in the definition of personality a limitation, when compared to the infinite being of God and nature, we are also obliged to acknowledge, and to come to know intimately, that this very shaping of one's personality is a genuine and underived power.

Every universal in the sphere of culture, whether discovered in language, art, religion, or philosophy, is as individual as it is universal. For in this sphere we perceive the universal only within the actuality of the particular; only in it can the cultural universal find its actualization, its realization as a cultural universal.

Cassirer goes on to discuss the way in which Wilhelm von Humboldt developed this basic thesis with particular reference to language in the famous preface to his study of the Kawi language (1836). Cassirer states (25):

As historian and philosopher of history Humboldt held firmly to this fundamental insight and found new support and proof of it. All historical life is nationally conditioned and limited; but in this very conditioning, indeed, by virtue of it, it exemplifies the universality, the unbroken oneness, of the human race.

And he adds (27): "None of the systems of historical determinism which we have studied has conceived the principle of individuality with such depth and clarity."

One might paraphrase this orientation by saying that in the sphere of human cultural life (including language), universality is fundamentally a matter of function, rather than of specific form. The creativity and resultant diversity of cultural and personal ways of life rests in the work-

ing of a universal power, but a power using particular means to particular ends.

It was the accomplishment of Boasian analytic grammar and of the generation of Kroeber, Sapir, Lowie, Radin, and others, to establish such an orientation toward American Indian cultures and languages; to demonstrate that they embodied powers universal to humanity, and at the same time experience and creativity specific to particular peoples, as against a priori prejudice and rash generalization of whatever kind. If I had to state one philosophical position as 'the' position of the Americanist tradition, I would state this. And I would add that it seems a position consonant with the orientation of Native American cultures themselves.

From this standpoint, the contribution of Americanist linguistics to general linguistics will continue to depend on the importance of testing and discovering generalizations through knowledge of languages originally unwritten and exotic, and still manifold and diverse. There is a further contribution as well. Like Romance linguistics, Americanist linguistics can serve as witness to the claims for relative autonomy of a distinctive subfield. It can serve particularly well within our own society as a reminder of the just claims of a valued body of material for continuous devotion, as against the recurrent claims of a particular approach to be temporary 'king of the mountain' of linguistics as a whole. There is much interest now in the notion of scientific paradigm, as a model for understanding disciplines. Americanist linguistics shows the importance of being able to think, and work, in terms of a scholarly tradition.¹⁶

In sum, the Americanist tradition has value far beyond that of an object of proper piety. Its implicit aspirations have yet to be fully realized, and its greatest and most comprehensive accomplishments may lie ahead: the collective, chosen work of present and coming generations.

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NOTES

¹ For Santayana (1905), 'piety' and 'spirituality' are the two complementary motives of rational religion. An anniversary, as a ceremonial occasion, does call for a bit of the 'public religion' of a field, especially when one has been asked (as I have been) for something 'philosophical', so I hope the reference will not seem inappropriate.

² Thus the Powell schedule vocabulary of Wasco, obtained in 1885 by Jeremiah Curtin, attests as common Chinookan a Penutian cognate for 'pelican'; records forms and metaphors for features of aboriginal dwellings now forgotten; indicates that the only word for 'horse' was once an extension from 'dog'; etc. The 1884 vocabularies in the Alsea and Yakona dialects, obtained by J. Owen Dorsey, will make possible some control of dialect levelling in Alsean material subsequently collected by Sapir, Frachtenberg, Jacobs, and Harrington. In the published lexicon Frachtenberg (1920) transposed Dorsey's material into his own orthography without indicating which forms came from Dorsey, so it is necessary to go back to Dorsey's own manuscript (preserved in the Smithsonian).

³ In comparing Sapir's *Wishram Texts* (1909) with his field notebooks, I have found two cases in which, because the narrative apparently had seemed slightly garbled, Sapir reordered forms for the published record. An analysis of narrative structure has found motivation for the originally recorded order. See Hymes (1974a, 1976).

⁴ Cf. Goddard (1973) for telling examples and an excellent general treatment of philological assessment of data.

⁵ The need to study individual scholars in this way is suggested at the end of Hymes (1966).

⁶ There have been at least two generations in the regard. A linguist exploiting Hoijer's Tonkawa material for the sake of an issue raised within a Chomskyan framework recently complained that the data did not answer all his questions. That the original work was done in the 1920s, forty years before, in the infancy of phonology and morphology; that a reasonable lexicon of some scope, from the last few speakers,

had been a considerable task; that without Hoijer's labors in central Texas there would have been nothing for later armchair use at all – such thoughts seem not to have occurred. (For different opinion cf. Haas 1967:318, Troike 1967:323, 330, and Hymes 1967:276.) At a 1961 conference Chomsky referred to early materials in American Indian languages as 'false' (not as limited or incomplete, as I had characterized them).

Such an attitude is not unique to a generation of analysts that works with the principle of transformational derivations. A preceding generation could feel the same way about material obtained without benefit of the principle of phonemicization. I have heard such remarks from several of them, and recently, having sent a copy of Kroeber's Yokuts dialect survey to one senior scholar, because of published attention on his part to Yokuts, was thanked, but nevertheless with serious question as to the utility of the word lists: "Kroeber, however brilliant, was doing this work without benefit of the phonemic principle, which renders his transcriptions virtually unusable." Both linguists and anthropologists have sometimes scorned data from non-professionals (cf. the observations of Morris Swadesh 1954 on this problem). The treatment of Wishram Chinook calendrical names in the one published ethnography (Spier and Sapir 1930) is almost incredible, given the data available in Curtis (1911). (The author of the treatment must be Spier; Sapir would have seen that the Clackamas names (p. 209) are not 'abstract', but analyzable in large part, and in one or two cases merely orthographically disguised variants of Wasco names.)

⁷ The stereotype of a tradition determined by the exigencies of field work with little known languages is so widespread in textbooks and historiographic writings as to constitute a cliché. Americanist work, American structuralism, and American linguistic anthropology obviously have been closely connected, but none of them can be reduced to a determination by field work. The discovered *content* of Native American languages has had a great impact, but the process of inquiry has greatly varied. Current stereotypes about the process do not reflect direct acquaintance with the tradition and the interplay of field work and other factors within it, but rather reflect a reaction to the ideology dominant among some American linguists in the 1940s. In that decade an image of the anthropological tradition under Boas, Sapir, and Bloomfield was joined with a methodological doctrine derived from certain aspects of Bloomfield's work by the first generation to develop an autonomous academic discipline, associated with wholly general models of linguistic structure. The crystallization of outlook among some leaders of the 1940s has been projected onto the past as a whole. The actual history of American linguistics as related to 'American structuralism' is considered in some detail by Hymes and Fought (1975).

⁸ Notice that the definition provides no permanent pride of place for field work. Indeed, field work steadily decreases in importance in two respects: (a) from the standpoint of the field as a whole, as the proportion of languages for which field work is possible decreases; (b) from the standpoint of individual languages, as the proportion of what can be newly learned in the field to what can be learned from data already in hand decreases (or falls to zero). In general, field work is one step, or moment, in research, and over time has become less often the first step. Fieldwork is more and more often to be justified as a step following upon analysis, or recognition of a new problem, such that new data is seen to be needed. None of this diminishes the importance of field work where new data *is* needed, or where there is need for salvage or community service. The true situation does make the statement of purpose inside the cover of the *International Journal of American Linguistics* seem curious: "The functions of the Conference are, first, to discuss matters of common interest to linguists and anthropologists who do field work with American Indian languages; and secondly to pool information on opportunities for publication, and on the sources of subsidy for field work and publication" (see, e.g., IJAL 49(3) (1974)). If taken seriously, the statement implies that an American Indian language ceases

to be of interest to the Conference on the day that it ceases to be available for field work. Scholars who do comparative reconstruction, historical analysis, philological interpretation of texts and meanings, or who work with American Indian languages through data obtained by others, are excluded. Field work is implied to be an end in itself; once published, the use to which it is put is not of interest. Such a statement, officially made in the only journal of the subject, capitulates to those who would reduce a major chapter in the intellectual history of linguistics and anthropology to the gathering of data. It disgraces the purposes to which the great figures of the tradition have worked, and should be changed.

⁹ The Survey of California Indian Languages, made possible by Murray B. Emeneau and Mary Haas, is one of the important few. The contribution of the American Philosophical Society, through its Library and its Phillips Fund, and of the Smithsonian Institution, should be specially noted too.

¹⁰ As instances of the study of such topics, let me mention Leap (1973, 1974), Philips (1972), Basso (1970), Barber (1973), Darnell (1971c), Swadesh (1948), Hill (1973), Golla (1973), Spolsky and Kari (1974), Albo (1973), Hale (1973), V. Hymes (1974). The genres, styles, and norms of non-Indian languages that affect Indian lives have been little studied, whether in regard to Indian communities or otherwise. In general, attention has been given to minority communities in terms of the ways in which in which they are different, and so present a problem for the institutions of the established order. There has been little study of the way in which the established order is a problem for the minorities, and indeed for people generally, except with regard to schooling (on which see Philips, cited above). On the general problem see Nader (1973).

¹¹ Cf. Darnell and Sherzer (1971) and Sherzer and Bauman (1972).

¹² Cf. Hymes (1961).

¹³ Cf. Hymes (1965, 1968).

¹⁴ This point is stressed by Gossen (1974).

¹⁵ Cf. Whitney (1875). Both statements are at the end of chapters. The attention to universals and theory in the Americanist tradition is discussed in Hymes and Fought (1975). Let me observe that not only does Sapir's sentence contain several well-placed internal parallelisms of assonance and consonance (*come : form, -donian : -hunting, swine : savage, herd : head*, and the near rhyme of the stressed, enclosing phrase endings (*form : -sam*), but also that the two long, parallel phrases count almost as alexandrines.

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LINGUISTIC METHOD IN ETHNOGRAPHY: ITS DEVELOPMENT IN THE UNITED STATES

To understand the development of anthropology, and of ethnography within it, one must consider many things, from Kant to colonialism. For anthropology in the United States, one of the most important considerations is the study of language.

I. THE NINETEENTH CENTURY

In the nineteenth century the contribution of linguistic research to a general science of man was highly valued by many. The contribution that loomed largest was classification of languages. Classification of languages according to common origin and provenience was seen as a primary avenue into the history of peoples such as the American Indians, around whose study anthropology in the United States first found unity. Classification of languages according to structural type was seen as an indication of mental character and development.

Let me cite two leaders of the field. Horatio Hale was a pioneer in field linguistics and ethnology, well known for his role in the Wilkes Expedition of the early 1840s, and his book on the Iroquois Condolence Rite. Late in the century he was asked to supervise Boas in his initial work on the Northwest Coast (sponsored by a committee of the British Association for the Advancement of Science). Hale went so far as to urge (1891:414) that "It is language alone which entitles anthropology to its claim to be deemed a distinct department of science" and to state (1892, quoted in Dieserud 1908):

'Solely by their languages can the tribes of men be scientifically classified, their affiliations discovered, and their mental qualities discerned...Linguistic anthropology is the only true science of man.'

Daniel Garrison Brinton, the "fearless critic of Philadelphia," a dominant figure, ranking with Morgan and Powell, took a similar view in his Vice-presidential address to the Section of Anthropology

of the American Association for the Advancement of Science (1887:34):

"This individuality of the race is still more strongly expressed in their languages. You are all aware that it is upon linguistic data almost exclusively that American ethnology has been and must be based. The study of the native tongues becomes therefore of transcendent importance in the pre-historic chronology of the Continent."

Indeed, Brinton advocated a study of language which, in his words, would go beyond lexical externalities to the forms of a language and beyond this again, to (p.37):

The determination of the psychical character of the tribe through the forms instinctively adopted for the expression of its thoughts, and reciprocally the reaction exerted by those forms on the later intellectual growth of those who were taught them as their only means of articulate expression.

Hale and Brinton expressed positions taken on a pervasive concern. The respective claims of language, of race, and of cultural materials, as evidence of historical relationship and mental ability were in dispute. There was in effect a debate in the nineteenth century as to the role of a linguistic method of ethnology. (The context of the debate is discussed by Stocking 1973:xc-cx and in Hymes 1973d: 158-161, 170-1 (ch. 2, this volume)).

This question of linguistic method was a question of the role of linguistics, not a question of the use of the methods of linguistics outside the sphere of language.

In the United States the work of the Bureau of (American) Ethnology, under John Wesley Powell, settled the immediate question, so far as the American Indians north of Mexico were concerned. The Bureau's linguistic classification (Powell 1891) was fundamental to its own ethnological synthesis (the Handbook of American Indians, edited by F. W. Hodge (1907-11)), and to the study of the American Indians, and hence to American anthropology, for some time to come. It gave a sense of order out of chaos, so much so, indeed, that American anthropology has been ahunted since, both by the classification's particular results (there has been no agreement on revision or successor until the present decade), and by the implicit image of a world made up of stable, language-bounded, one language cultural units. (On the history of the Powell classification and the work by Sapir and others that should have replaced it, see Hymes ms. a; on recent consequences of its implicit ethnolinguistic model, cf. Hymes 1966, 1968b, 1970a).

The success of the Bureau classification may be seen as the triumph of one sort of linguistic method. The impact was one of

establishing boundaries and of providing a frame of reference, however, not one of affecting the conduct of ethnography, except inasmuch as collection of data bearing on linguistic classification was considered a worthwhile part of ethnography. The Bureau's method, vocabulary elicitation, seems to have had no consequences for the rest of ethnographic work. (On the characteristics of ethnography in the period, see the University of Pennsylvania dissertation 1969 by Regna Darnell.)¹

Linguistics does not seem to have been a specific source of conceptions of method in the period. To be sure, Sir Henry Maine did appeal to comparative philology as warrant for his use of comparative method in ethnology. Indeed, he considered ethnological comparison valid only within the scope of what philology had shown to be a single group (cf. Burrow 1966:232, 148-9, concerning writings of Maine in 1886 especially). Hearne (1879:2-3) took a similar view. But this was a question of the results of the comparative method within linguistics, and of an appeal to linguistics as primus inter pares at a time when comparative reconstruction and inference was widespread. The specific findings of linguistic comparison might be debated or treated with caution by its admirers, and the impression that comparative philology might provide the solution to ethnological problems was not long entertained (cf. Taylor 1864). When an anonymous reviewer criticizes the posthumous writings of the famous ethnologist McLennan, he appeals to comparative philology as a standard of method, but takes it as the great achievement of a generally shared "Comparative Method of Enquiry" (Anon. 1886:197-8). (Maine may have been the anonymous reviewer). The instances of evolutionary use of the comparative method with references made to linguistics are common enough (cf. Buck 1906, cited by Bock 1956:23, n.20), but I am not aware of appeals to comparative and historical linguistics as a source of new method by anthropologists,² outside perhaps of folklore.³ It would seem to be a striking but isolated fact that one linguist did assert the superiority of linguistic method in much the same terms (mutatis mutandi, given the historical context) as would be used later in the context of synchronic study (Paul 1886:5):

Es gibt keinen Zweig der Kultur, bei dem sich die Bedingungen der Entwicklung mit solcher Exaktheit erkennen lassen als bei der Sprache, und daher keine Kulturwissenschaft, deren Methode zu solchem Grade der Vollkommenheit gebracht werden kann wie die der Sprachwissenschaft.

(It is worth noting that Paul, like Whitney and de Saussure, placed linguistics among the social sciences: Die Kulturwissenschaft ist immer Gesellschaftswissenschaft. Erst Gesellschaft ermöglicht die Kultur, erst Gesellschaft macht den Menschen zu einem geschichtlichen Wesen.)

It is in the twentieth century that linguistics comes to be a recurrent methodological model for anthropologists in the United States. The beginning is with Boas, just before the First World War. Boas did not put the issue in its full form, but present understanding of it is the outcome of a development that stems from him. He and his students shaped modern professional anthropology in the United States, and shaped also its conception of the relationship between study of language and study of culture.

I shall first sketch some distinctions regarding the notion of "linguistic method of ethnography" (II), then trace the development of the notion. Boas creates a stage in which the unity of linguistics and ethnography is explicit with regard to subject matter, implicit with regard to method (III). With Sapir comes the second stage, explicit recognition of a methodological role for linguistics (IV). In Sapir this comes about in the course of a subtle development of thought, from a separation of language from culture toward a substantive unity of language and culture, then toward a changed conception of the nature of the unity. After the Second World War, one finds a third stage, containing a rapid, yet clear, succession of phases: concern with a strained relation between linguistics and anthropology, and with the suddenly discovered importance of Whorf (V); methodological impact of structural linguistics (VI); and, most recently, changes set in motion by new models and extensions of grammar, and by anthropological critiques of linguistics and linguistics-related methods (VII).

The discovery of these clearly delimitable, connected stages was unexpected, when I began this study. They are a striking fact that this paper, focussed as it is only on one chain of ideas can only partly explain; a larger context, intellectual, social, personal, will be required.

II. GENERAL CONSIDERATIONS

Anthropologists have been concerned with both the use of language and the use of linguistics. The phrase "linguistic method of ethnography" is thus ambiguous; it could refer to either. (No one says "language method of ethnography" for the first.) The development traced in this essay can be seen as the development of yet a third, deeper meaning for the phrase, a development in ethnography from (1) use of language, and (2) use of linguistics with regard to language, to (3) use of linguistic method beyond language.

The history of the first two meanings cannot be traced here in detail, but something must be said by way of context. This can be done by considering the use of language, and linguistics, as ranging along something of a scale, from least involvement of technical linguistics to most. Roughly, one can distinguish uses of a

linguistic method of ethnography as (1) facilitating, (2) generating, (3) validating, (4) penetrating, and (5) foundational. These uses are not mutually exclusive; a thoroughgoing conception of ethnography would incorporate them all. At the same time opinions and interests with regard to them differ.

By a FACILITATING role is meant use of a language for access to a community, for survival and rapport within it. By a GENERATING role is meant use of a language for the production of data. By a VALIDATING role is meant use of a language as a criterion of adequacy and validity. These three roles have in common the possibility of no use of technical linguistics, being first of all matters of what one does or has IN a language, rather than of what one does TO a language as an object of study. A language may be known in advance, an existing analysis may be relied upon, or, as must have happened again and again in history, a language may be acquired on the ground without recourse to any technical framework. If a language is made an object of study, it may be only as a means of entree and preface to actual work--as an initial facilitating step. In supplementing observation by interview, taking down conversations, eliciting names and stories, the focus may be on the translatable content, not on interviews, conversations, names or stories as having interesting systematic properties of their own. One language is then a means of generating data that is handled essentially in another. And it may be the fact of use of a language, not of its analysis, that confers validity, as in ability to check upon an interpreter or to work independently of one. Sustained texts may confer validity by letting members of a community "tell it like it is" in their own words, largely independent of one's own purposes and preconceived queries. None of these purposes need lead to a grammar, dictionary, or linguistics-based analysis of discourse.

It remains that the leaders of the first generation of academic anthropology in the United States (Boas, Kroeber, Lowie, Radin, Sapir) believed in control of the native language as a matter of scholarly standards, if ethnography was to stand comparison with the disciplines studying classical, oriental and contemporary European cultures. For some studies, such as of poetry, which Boas (1911) classed among "deeper problems of ethnology," only native language data would be acceptable. Radin (1933) carried concern with authentic native language documents to the point of making them the defining goal of ethnology. These men and their students did produce grammars, dictionaries, and analyzed texts, and occasional special studies in onomastics (toponymy, personal names, botanical terms, etc.). If linguistic analysis was sometimes judged for its contribution to ethnography rather than to linguistics, it was a recognized and valued contribution. Many ethnographic situations, indeed, make the roles so far discussed difficult, if not impossible, without some use of linguistic analysis.

The three roles just discussed shade into a fourth. Nadel's interest in native terms as "cognitive summaries" (Nadel 1951) and Bohannon's insistence (1958) on use of native terms show concern for language as a source of validity and cultural insight; but serious analysis of native terminology is analysis of language itself. Here one begins to deal with language itself as a cultural system. The predominant interest of this sort in the United States has of course been with kinship terminology and grammatical categories, the latter taken as indication of world view (recall the initial quotation from Hale)--viz., Boas' concern to write grammars in terms of the "psychological groupings," the "inner form" of each language (1911:81) and Whorf's "Linguistic consideration of thinking in primitive communities" (1956a). The grammatical analyses undertaken by Boas and Whorf were in effect uses (and developments) of technical linguistics to pursue a problem set by cultural anthropology. The sense of reaching deeper, commonly tacit, levels of thought and pattern suggests PENETRATING for this fourth role.

There is finally a concern with language as a cultural system, such that linguistic methods are found relevant beyond language, by direct application, or as part of the methodological foundations of the study of culture as a whole. Such a FOUNDATIONAL role may arise from personal contact and intellectual affinity, as in the effect of Boas' linguistic teaching on Kroeber, and of Sapir's on Kluckhohn. And such factors presumably played a part in the genesis of the article in which Levi-Strauss first treated linguistics as an inspiration to social anthropology (1945). It appeared in a New York-based but European oriented journal, the linguistic model was that of Prague structuralism, and Levi-Strauss was at the time a colleague at the Ecole Libre des Hautes Etudes in New York of the great Prague structuralist, Roman Jakobson (Levi-Strauss 1949b:xxvi). I take the emergence of a foundational role for linguistics to depend first of all, however, upon inclusion of linguistics in anthropological training.

A comparison with British anthropology may make clear the importance of this claim. British anthropologists have stressed use of the native language, as a heritage from Malinowski, and have been known to inquire in this respect about the proficiency of eminent Americans (cf. the remarks of the British-influenced Bohannon[1958]). Moreover, the structural orientation of the group might be thought to have provided a congenial climate for a structural orientation toward language. Malinowski himself had done empirical linguistic work, and had called for an ethnographic theory of language. The pioneer of British structural linguistics, J. R. Firth, did in fact gain inspiration from Malinowski, and I have been told of a scholar who might have served as a continuing personal link between the two, had he not prematurely died (Peter Strevens, personal communication). But if Malinowski persuaded British anthropologists to think function-

ally and contextually about language, he did not lead them to learn linguistics. Apart from the possibility of training in a particular language, or in phonetics, for field work, training of a linguistic sort did not form part of training in anthropology. In particular the techniques and operational concepts of linguistics did not. (I have known a British anthropologist to respond with surprise to the suggestion that such training might have helped one field worker who had to give up for failure to master a local language.) Apart from Evans-Pritchard (see references in Hymes 1964:225), British social anthropologists until recently showed little interest in linguistic matters, and linguistics played no part in theory. In a 1950 lecture, Evans-Pritchard (1962:23) made clear and sound use of linguistic inquiry as an illustration of inquiry in social anthropology, but only in general terms and in terms of a parallel. (Cf. now Hensen 1974, and, for change, Leach 1964, Whiteley 1966, Ardener 1971, Bloch 1975, Crick 1977. Leach [1974] tries to refute the above picture by scattering of names and details, but Henson's subtitle remains accurate as to the whole.)

The difference between British and American anthropology may have to do with the period of neglect of culture in favor of social structure among the former. That might explain relative inattention to language, which, as such, would seem quite epiphenomenal (cf. Radcliffe-Brown 1957:142-3). Yet Levi-Strauss has shown that analysis of social structure can give importance to linguistics as a model. Much depends on underlying assumptions and orientations, but the crucial factor, I think, is a climate of opinion in which the techniques and operational concepts of linguistic work are familiar, as opposed to one in which they are not.

To prove this point would require comparative study of several national traditions in anthropology. The presence or absence of linguistic training, even if found to be diagnostic, might prove symptomatic rather than fundamental. For anthropology in the United States, at least, it does appear to be crucial. The traditional inclusion of linguistics in anthropological training is, I think, the institutional basis for the intellectual development traced in this paper. I would predict that the surge of a potentially distinctive British interest in language will be short-lived and unfruitful if such an institutional basis is not secured.

It is not that American anthropologists are often skilled linguists. The number of ethnographers able to write a grammar or to handle text analyses and lexicography has always been small. The fact of obligatory linguistic training, however, has ensured a certain familiarity with linguistics on the part of most anthropologists, and a certain number of ethnographers who contribute to linguistics. The fact of training has meant a continuing opportunity for some anthropological students to find a personal or theoretical

significance in linguistics, in relation to their other training; and it has meant the presence within anthropology departments of linguists to provide the training, and thus the opportunity for informal mutual influence. Working among anthropologists, and often enough considering themselves anthropologists, some linguists thus have come to relate their work to anthropology as a whole. The result has been an attention to linguistics, and a legitimacy of concern for it, and, indeed, an influence of linguistically oriented thought out of all proportion to the number of linguists in anthropology.

The influence of linguistics has not been equally important for all departments of anthropology, nor uniform among any of them throughout their history. The development treated in this essay is to a marked degree a story of a few centers at certain times: Columbia, Yale, especially, together with Chicago; Berkeley; Indiana; Harvard. (Comparative analysis of departments within the United States would be valuable.) Personal affinities and abilities, and purely institutional factors, have played roles that are only partly known. And the influence of linguistics has been controversial (as will be discussed in VII). Kluckhohn perhaps overstated its significance for the field (although not for his own view of the field) when he wrote (1959:262):

But the distinctive aspects of the anthropological outlook derive primarily from the second historical factor: among behavioral scientists, only cultural anthropologists have been in sustained contact with the extraordinary developments in structural linguistics over the past generation.

Not all anthropologists have seen, as did Kluckhohn, the intellectual importance of linguists; many have thought of linguistics as at most a practical tool. The fact remains that the practical role has been commonly granted, and training provided toward that end; and that out of this pervasively institutionalized ground have recurrently grown contributions to method and theory. Use of language has implied use of linguistics, and linguistics has given rise to reconsideration of the nature of culture itself.

III. FRANZ BOAS

The beginning, as was said, is with Boas. Boas had engaged in linguistic work as a museum employee in Berlin with visiting Bella Coola informants, before his first trip to the field. He continued linguistic work in the Arctic (1884-5) with Eskimo, and on the North Pacific Coast (from the late 1880s on), some of the time under the official direction of Hale (as representative of a committee of the British Association for the Advancement of Science). In this work Boas was concerned with linguistic evidence of ethnological relations and with linguistic documentation of native culture; he

was sensitive as well to the need to recognize the form and meaning given to languages and verbal art from the point of view of the individual cultures. It is known that he had met the psychologist and interpreter of W. von Humboldt, Steinthal, and later described his linguistic work as having been based on Steinthal's principles (Stocking 1968:347, n. 33; cf. Stocking 1974a:482, n. 7).

Boas early arrived at a recognition of the role of cultural factors in perception, and stated it most seminally in an analysis of a linguistic question (the question of supposedly "alternating" sounds (1889)). The leading student of Boas' role in the development of anthropology comments (Stocking 1968:159):

"It is impossible to exaggerate the significance of this article for the history of anthropological thought."

Still, what Boas arrived at was a principle, not a method. His analysis of "alternating sounds" is a fascinating anticipation of phonemic analysis, but not more.

The main theme of Boas' concern with language was often the importance, indeed necessity, of the comparative perspective provided by anthropological work. He went so far as to refer to a "linguistic method of anthropology," with stress on "anthropology" (1889:623-4):

"this subject, as well as its methods, must have a stimulating effect upon the teaching of philology, because its conclusions are based upon the broad grounds of human language; not on the studies of a single family of languages. The science of linguistics is growing slowly on account of its intrinsic difficulties. These difficulties are based on the lack of satisfactory material as well as on the amount of labor involved in the acquisition of knowledge in its particular line of research. Work (sic) in this field is disappearing rapidly, thus depriving us of valuable material for comparative study."

The view is an application to language of Boas' general view of the invigorating effect of the breadth of an anthropological approach to the problems of understanding the general development of mankind (cf. Boas 1904 at end). Indeed at the time he wrote it seemed reasonable to him to identify philologists with the study of special linguistic stocks, and to say by way of contrast (1899:622):

"the anthropological problem is a wider one--it deals with the general question of human language."

Language had ethnological importance because Boas (following Steinthal) looked toward a "truly comparative psychology," for which the results of "comparative linguistics" (in the sense of the comparative study of types of language) would form (1899:624):

an important portion of this material, because the forms of thought find their clearest expression in the forms of language.

Ethnology (and the languages it studies) have the advantage that the forms of thought have grown up entirely outside the conditions which govern our own thought.

All this is very close to a special role for language in ethnology, as part of a special role for ethnology in the human sciences, but to say that forms of thought find clearest expression in forms of language falls short of the explicit theoretical grounding for the role of language at which Boas was to arrive a decade later.

For a few years Boas seemed to become less hopeful for linguistic work in anthropology. He saw as not far distant the time when specialization of method would require anthropology proper to deal only with customs and beliefs, and when a separate linguistics would continue and develop "the work that we are doing now because no one else cares for it" (1904:524). He appealed to philologists for help (1906:644-5):

"I must say with regret that the anthropologist of the present day is not the man to solve these problems (of the study of the many American Indian languages): that we require not only the stimulating example of philologists, but also their assistance."

Allowing for the possibility of false humility in an address to philologists, these two papers, published within three years of each other in Science, do seem to strike a different note. Yet a positive note soon returns. In a lecture of 1907 Boas first (to the best of my knowledge) asserted with regard to general anthropology that (1908:23):

the study of language promises to point the way in which many of our problems may find solution.

This theme is repeated and forcefully developed in the section on "linguistics and ethnology" which Boas included in his well known Introduction to the Handbook of American Indian Languages (1911). There he treated first the "practical need of linguistic studies for 'ethnological purposes,'" and went on to stress as well the "theoretical importance of linguistic studies."

Boas here seems explicitly to reject the anticipation of a separate linguistics of the articles of 1904 and 1906. In this analysis (written in 1908 [cf. Levi-Strauss 1958:26]), it is noted that other subjects important to ethnology also require specialization of method. Boas writes now in the tone of someone who has known past objections to what he will say, and can anticipate others, but has made up his mind:

the purely linguistic inquiry is part and parcel of a thorough investigation of the psychology of the peoples of the world. If ethnology is understood as the science dealing with the mental phenomena of the life of the peoples of the world, human language, one of the most important manifestations of mental life, would seem to belong naturally to the field of work of ethnology, unless special reasons can be adduced why it should not be so considered.

In particular, linguistic processes and categories are argued to be of the same kind as those of ethnology, and to be peculiarly illuminating. While the origin is unconscious in both cases, ethnological phenomena often rise into consciousness and become subject to secondary reasoning and reinterpretation that obscure their fundamental nature. Linguistic phenomena, in contrast, tend to remain unconscious, hence not secondarily reinterpreted, hence privileged for the understanding of the nature of ethnological phenomena in general.

Boas' mature formulation thus seems to emerge within a few years around 1908. (To judge from the contents and dates of these published works; unpublished correspondence might modify the inference.) There seems here the first instance of a cycle of practical separation, then theoretical reintegration, of linguistics and ethnology. It was to recur in Sapir's thought in the late 1920s (IV), somewhat in Kluckhohn's in the 1950s (V), and more generally within anthropology in the 1940s and 1950s (V-VI), and to begin to occur between anthropology and theoretical linguistics in the 1960s and 1970s (VI-VII).

(Notice that for Boas "ethnology" is equivalent to current "cultural anthropology," comprising field investigation, theory, and comparative analysis [as in his "Linguistics and ethnology"]. The "mental sciences" are equivalent to the Geisteswissenschaften. Although influenced by psychologists [Galton, Wundt and others], Boas is not here placing anthropology within professional psychology. He can be said to have been a mentalist, however, in the sense in which the term is now used in American linguistics, given his assumptions about the goals of linguistic and ethnological study.)

What was the outcome of the Boasian formulation of a principle of unity for language and culture, especially with regard to a linguistic method of ethnography?

An implicit unity in method

The only concrete result in the Handbook of American Indian Languages is the principle just noted. This universalistic claim is argued at some length, examples being developed to show the unconscious origins of both linguistic and ethnological traits. The Boasian thesis was upheld by Lowie (1920:12), elaborated in a seminal paper by Sapir (1927), and made a requirement of structural analysis in linguistics and anthropology by Levi-Strauss (with acknowledgment to Boas), as a guarantee of validity, objectivity, and universality (see Levi-Strauss 1958 [1949]:26, 1950:xxx, 1953:526-7, and cf. Jakobson 1944, and Whorf 1956). Boas himself seems to have done no more with it (except to state that the unconscious categories of language had nothing to do with Freud's unconscious [1920:321]).

Although Boas argued the identity of linguistic and ethnological phenomena in kind, he did not specify identity in any particular features, whether of content or form. Nor did he suggest that identity in kind (unconscious origin) might support a unity in method. He does take a stand, regarding both, against imposition of a priori frameworks, and demands the discovery of authentic native categories. Boas' work has a more specific methodological unity, however, and appropriately enough, it is a unity that did not rise to conscious expression, nor undergo secondary elaboration. The unity can be found in his frame of reference in dealing with folklore, grammar, and cultural phenomena generally.

For Boas, the context of explanation was commonly a developmental one. As an "historical school," Boas and his students focussed, not on the tracing of origins, but on the understanding of outcomes. Insofar as phenomena were traced to earlier origins, it was not to reconstruct (a possibility in which Boas placed little confidence, whether in regard to proto-languages, tale types, or culture complexes) but to disclose the diverse provenience of the elements in question, the processes by which they had come to be given a contemporary coherence and common significance, and the patterns or orientations in terms of which coherence, significance, was assigned. The terms of analysis, in short, were ELEMENTS, PROCESSES, and PATTERNS. One can see the same mode of thought in the Boasian analysis of folk tales in the 1890s and in the Boasian analysis of grammars in the 1900s. With folk tales one compared texts within and between communities, showing them to be composed of elements of diverse histories secondarily combined. With language one compared sentences and words within and between texts, showing them to be composed of elements ("phonetic groups"), also of diverse origin and secondarily combined. Given analysis into fundamental elements, one could trace the particular mode of formation of the products embodying each, and consider the underlying ideas so expressed. With folk tales, the products were texts, the mode of formation historical, and the underlying ideas

had technical names: grammatical processes, grammatical categories. (On Boas and folklore in this respect, see Hymes 1961e, 1962b; on the unity of approach, Hymes 1963; on its subsequent role in American ethnology, Hymes 1967c. The linguistic approach continues in Sapir--cf. his chapters on "Form in language: grammatical processes" and "Form in language: grammatical concepts" [1921, Chapters IV, V], and has been revived by Jakobson [1957]. On Boas' analytic approach, see now the important studies by Stocking [1974a], and cf. 1974b.)

An implicit unity of approach can also be found in work of Sapir, of Kroeber, and of Radin, with regard both to language and to culture. With each the grammars can be seen to have been written by the men who wrote the ethnographies. The unity is in both the climate of opinion and the style of the individual scholar, and is brought to language, rather than extrapolated from its study.

In sum, for Boas linguistic method in ethnography comprised the facilitating, generating, and, especially, the validating and penetrating roles suggested in II above. He stated no foundational role. There was a deep foundational unity of his linguistics with his ethnology, but it remained implicit. There was no development in detail of methodology, in linguistics or in ethnology, let alone in a linking of the two.

Outcome and context of the Boasian stage

Development of linguistic methodology might have led Boas further in the development of its relation to ethnography; but Boas was not a structural linguist. He was a pre-structuralist, a man who had some of the essential understandings of structural linguistics, and who cleared the way for it.

Certain kinds of generalization and inference, especially as to underlying relations, certain kinds of systematization, were alien to Boas' spirit, perhaps more so in his later years. He was not in principle a "taxonomic" linguist, but was so in practice. Boas would have accepted the earlier statements of Whitney (1867:10, 54) that the goal of linguistics was not to describe, but to explain. It is fair to say, however, that his own work laid foundations for explanation, but did not move very far toward the goal. Indeed, a generation later, some linguists would be puzzled as to why he was interested in such things as grammatical categories. His deeper theoretical motivation would be temporarily forgotten, and only his contribution to clearing the way for adequate description remembered. (cf. Hymes and Fought 1975:960, 970, 981-4, esp. 984).

Boas' status as a "pre-structuralist" is seen by comparing his treatment of phonetic sounds with Sapir's concept of sound pattern. Sapir's paper (1925) is fundamental because it marks and inaugurates

the change from a Boasian "analytic" approach to a structural approach proper. In the Boasian approach one observed carefully and pruned one's observations to arrive at a consistent orthography in keeping with the principle that "every single language has a definite and limited group of sounds" (1911:16). One did not employ the principle of defining the elements of the language's group of sounds in terms of internal relations. In particular, one did not explicitly and consistently employ the principle of functional contrast. Sapir's paper is a brilliant demonstration of this principle. He shows that two definite and limited groups of sounds that seem very much alike may be shown to be quite different in pattern, once the relevance of the sounds to differentiation of words and utterances is analyzed. Conversely, two languages with quite different inventories of sounds, as sounds, may be found to be very much alike in pattern. In other words, an analysis of the relations among sounds in terms of their contrastive function in the language may disclose a structure that is at least one step more abstract than the sounds themselves.

Boas' general thesis as to the role of language in ethnology did become established in the systematic books of the new profession of anthropology, written by major students such as Lowie and Kroeber. Thus, Lowie (1920:12):

"It is not necessary to multiply instances of this type because an inexhaustible supply of relevant data is furnished by a single department of culture, to wit, LANGUAGE. Linguistic processes belong to the same category psychologically as the processes by which the non-intellectualistic part of culture come into being, and what applies to them has accordingly a wider application."

And Kroeber (1948:245, in a chapter unchanged from the first edition of 1923):

"The quality of unconsciousness seems to be a trait not specifically limited to linguistic causes and processes but to hold in principle of culture generally. It is only that the unconsciousness pervades speech farther."

And (1948:248-9):

"Linguistic phenomena and processes are on the whole less conscious than cultural ones, without however differing in principle.... Organization or structure in both cases takes place according to unconscious or covert or implicit patterns, such as grammatical categories, social standards, political or economic points of view, religious or intellectual assumptions."

It remains that both Lowie and Kroeber were interested in language primarily as a source of understanding of cultural stability and change. Lowie draws on language in his book to demonstrate points about independent convergent development and diffusion (pp. 12, 433). Kroeber, assimilating his most deeply held reflections on the nature of culture to his chapter on language, is most of all concerned with the unconscious and relatively unchangeable nature of both.

To this should be added establishment of the Boasian thesis that cultural patterning was historically specific, that is, not inherent in the elements patterned, but deriving from a cultural tradition and outlook. This view, developed by Benedict (1923), by Lowie (1920), and others, came to be stigmatized as finding in culture no coherence, only "shreds and patches." The truth is otherwise. Coherence of cultures was asserted, though qualified in that cultures were seen as complex in origin and continuously in change. The point was where coherence was to be found. To use terms that Pike was to establish thirty years later, Benedict and Lowie were developing the Boasian view that coherence, valid pattern, was not to be found in "etic" frameworks, but to be discovered only in "emic" relationships within specific cultures (cf. Benedict 1923:84).

These outlooks were to continue among many American anthropologists as part of the context in which the implications for anthropology of structural linguistics would be assessed. The subsequent development of notions of patterning in culture would be influenced and changed by developments in linguistics, but the notion of cultural patterning itself, and the notion that cultural patterning was well, or even best, illustrated by language, were already to hand in the legacy of Boas, and the work of his students.

Boas' program of linguistic inquiry into the psychology of the peoples of the world was not itself completed. A comparative study of the grammatical categories of American Indian languages had been planned for the first volume of the Handbook (1911). In the Introduction it was announced as postponed until completion of further work, and it never appeared.

Boas, of course, had other problems to investigate and many roles to play--in physical anthropology, in art, in organization, in building a fledgling profession and combating racism (see Stocking 1968, Chapters 7-11). In the present context he can be fairly judged as a figure of his times, as a man who saw a role to play--his "psychological" approach to language was cause for comment--and who played it decisively. In effect he built and set a stage which Sapir, taking up themes announced by him, was to animate and transform.

IV. EDWARD SAPIR

The thrust of Sapir's first writing on language in relation to culture (implicitly, on linguistics in relation to anthropology) is to separate the two (1949 [1921]:89-103). The issue is the notion that language is a reflection of culture. Words of course always reflect detached cultural elements, but, Sapir maintains, the relation between the form of language (grammatical categories and processes) and the forms of cultural thought and activity is practically impossible to detect (he remarks that he does not maintain that there is NO relation). One can imagine an initial stage in which both words and grammar symbolized culture, but change inevitably occurs in the form and the content of both language and culture and the relation must cease to hold. Cultural elements, as more definitely serving the immediate needs of society and entering more clearly into consciousness, will change more rapidly than those of language; and the form itself of culture, giving each element its relative significance,⁴ will be continually shaping itself anew. In sum,

changes in culture are the result, to at least a considerable extent, of conscious processes or of processes more easily made conscious, whereas those of language are to be explained, if explained at all, as due to the more minute action of psychological factors beyond the control of will or reason (1941[1912]:100).

A necessary consequence is that "the forms of language will in course of time cease to symbolize those of culture, and THIS IS OUR MAIN THESIS" (1949[1912]:102; emphasis mine).

Sapir reiterated this view in his celebrated monograph on time perspective (1949 [1916]:433, 1921:iii-iv, 232-4).

There is an irony here. Sapir takes the Boasian theses of the unconscious nature of linguistic form, and the crosscutting of cultural form by secondary rationalization, and pushes the two as far apart as possible. Culture is not wholly equated with the conscious, nor denied structural form; but form in language is equated with the subconscious. In effect Sapir takes the Boasian view of the value of language because of "the unconscious and unrationalized nature of linguistic structure" (1921:iv), and runs with it, leaving culture to shift for itself. The variability and dynamics implied in the view of culture do not, in this stage of his life, at all attract him (cf. the concluding statement in "The grammarian and his language" (1949 1924 :159). What attracts the impersonal stability, the "strange, cumulative drift in the life of the human spirit" [1921:iii], of language as "the most massive and inclusive art we know, a mountainous and anonymous work of unconscious generations" [1921:235].)

It is the independence of language (and linguistics) that is at stake, and was noted. The parallel to the views of de Saussure in the same period is striking. Sapir's work in this period can be seen as a hymn to the autonomy of linguistic form. His historical conceptions are the projection of his view of the essence of language (this is not to deny their merit; the projective relation is a common one). He criticizes the Bureau of American Ethnology, and Boas anonymously, for neglecting true historical work, and refers slightly to the attempt to relate the study of grammar to psychology (1917).⁵ He writes in letters to Ruth Benedict of the lonely vision that is his of the remote unities of American Indian languages (Mead 1959:53). He speaks of linguistics as having for a certain type of mind the profoundly serene and satisfying quality inherent in mathematics and music, the creation out of simple elements of a self-contained universe of forms, within it lies hidden the same classical spirit, the same freedom in restraint, a spirit antagonistic to the romanticism rampant in America, debauching so much of science with its frenetic desire (1949[1924]:159).

By 1921, however, a new theme has been broached. On the one hand, language is even more saliently the realm of form. The parallelism to culture (recall that in 1912 both had had form, though differing in its source and rate of change), almost disappears. That culture and language are in any true sense causally related, Sapir says, he cannot believe (here the issue is still culture as cause of language). The latent content of all languages is the same, the intuitive SCIENCE of experience, but language itself is a collective ART of thought [Sapir's emphases]. Culture is WHAT a society does and thinks, language a particular HOW of thought [Sapir's emphases again]. It is difficult to see causal relations between an inventory of experience and a manner of expressing it. The drift of language is not properly concerned with changes of content at all, only changes in formal expression. So far, then, one has the issues of 1912 even more sharply posed, Sapir insisting that language does not reflect culture, just as he insists, here and later, on the formal adequacy, completeness, and equality of all languages, primitive as well as "cultured." (The second argument is of course in a sense the continuation of the first.)

On the other hand, Sapir remarks as the one possibility (1921:233-234):

If it can be shown that culture has an innate-form, a series of contours, quite apart from subject-matter of any description whatsoever, we have a something in culture that may serve as a term of comparison with and possibly a means of relating it to language. But until such purely formal patterns of culture are discovered and laid bare, we shall do well to hold the drifts of language and culture to be non-comparable and unrelated processes (1921:233-234).

At the very point of questioning the presence of form in culture, Sapir begins to describe cultural form as something to be discovered, and as something that, to be form, would have to be like form in language. By implication, the form that had been attributed to culture in 1912 was no longer satisfactory in kind. Perhaps the parallel presence of form in language and culture had been taken for granted then (cf. above). In any case, the stress now, almost as an after-thought, on an innate form to be discovered and laid bare, which MIGHT again link the drifts of language and culture--the 1912 conclusion is suddenly qualified--would seem to signal a turning point.

In the next decade two changes occur. The "how" comes to have an increasingly active connection with the "what," and the "what," culture, is assimilated to, reunited with the "how," language--on language's own terms, so to speak. The concern for the autonomy of language as self-contained form continues to be expressed, but it is as if autonomy having been won, relations to culture can be explored again from the newly established position. The temptation latent in the view of language as form, culture as content--namely, to see language as giving form to content--begins to emerge. The relation that was eschewed in 1912, between language and culture, begins to be taken up again, but from the opposite end, and with a change in character. The relation is not one of reflection (which seemed to have been primarily associated with content), but one of moulding, explicitly associated with form.

The source of the notion of "form-feeling" is clear. It is Croce. That Sapir speaks in this period of "intuition," "feeling," "aesthetic" and the like, where a linguist today would refer to much the same matters in cognitive terms, seems due to the Italian scholar. Having stated his concern that linguistics escape sterility through knowledge of its wider relations, Sapir continues (1921:iii):

Among contemporary writers of influence on liberal thought Croce is one of the very few who have gained an understanding of the fundamental significance of language. He has pointed out its close relation to the problem of art. I am deeply indebted to him for this insight.⁶

One sentence in the 1921 Language sums up the position of this period (58, concluding a chapter): "Both the phonetic and conceptual structures show the intuitive feeling of language for form." There one has the unconscious knowledge, aesthetically phrased ("intuitive feeling"), the superorganic autonomy (it is language to which feeling is attributed), and the leitmotif, form.

The inner dynamic of Sapir's subsequent thought can be glimpsed in "The grammarian and his language" (1949[1924]:150-159). Like other articles, it is highly expressive on the autonomy of linguistic form, and most of the article is about the implications of the idea.

The one qualification is joined to the notion's strongest statement: "To say in so many words that the noblest task of linguistics is to understand languages as form rather than function or as historical process is not to say that it can be understood as form alone" (1949 [1924]:152). What happens that is new is two fold. STRESS IS LAID ON FORM-FEELING AS A POSITIVE FORCE. In a passage that anticipates later criticism of behaviorism by Chomsky (1959), Sapir speaks of "a certain innate striving for formal elaboration and expression and...an unconscious patterning of sets of related elements of experience" (1949[1924]:156). And THIS NOTION IS JOINED TO THE "OUTSTANDING FACT ABOUT ANY LANGUAGE,: ITS "FORMAL COMPLETENESS" (1949[1924]:153). The ineluctable adequacy of any language to express whatever its speakers need to express (recall the Time perspective essay (1949[1916]:433, n.18)) is held to

establish a definite relational feeling or attitude towards all possible contents of experience, in so far, of course, as experience is capable of expression in linguistic terms.... The world of linguistic forms...is a complete system of reference, very much...as a set of geometrical axes of coordinates is a complete system of reference to all points of a given space.

And Sapir continues in a passage that must be quoted in full (1949 [1924]:153):

The mathematical analogy is by no means as fanciful as it appears to be. To pass from one language to another is psychologically parallel to passing from one geometrical system of reference to another. The environing world which is referred to is the same for either language; the world of points is the same in either frame of reference. But the formal method of approach to the expressed item of experience, as to the given point of space, is so different that the resulting feeling of orientation can be the same neither in the two languages, nor in the two frames of reference. Entirely distinct, or at least measurably distinct, formal adjustments have to be made and these differences have their psychological correlates.

Philosophers are warned against the likelihood of becoming dupes of their own speech forms, since the mould of their thought, typically a linguistic mould, is apt to be projected into their conceptions of the world (157). And Sapir goes on to say that examples of incommensurable analyses of experience in different languages would make very real to us (1949[1924]:159):

a kind of RELATIVITY [my emphasis] that is generally hidden from us by our naive acceptance of fixed habits of speech as guides to an objective understanding of the nature of

experience. This is the relativity of concepts or, as it might be called, the relativity of the form of thought.

It is difficult not to see in these passages the source of Whorf's reference to the need for speakers of different languages to calibrate their linguistic backgrounds, to the value of linguistics for transcending the influence of a single language, and to relativity.⁷

In his fundamental paper on linguistic method the next year, there is but one sentence, the last, that bears on these questions; it indicates the generalization of perspective that is taking place (1949[1925]:45):

The present discussion [of sound patterns] is really a special illustration of the necessity of getting behind the sense data of any type of expression in order to grasp the intuitively felt and communicated forms which alone give significance to such expression.

The generalization is developed explicitly two years later in "The unconscious patterning of behavior in society" (1949 [1927]:544-559). The aesthetic nature and autonomy of language again are stressed, and the formal completeness, and adequacy (1949[1927]:550-1); but this discussion takes language as prime example of something true of all socialized behavior. It is enclosed between the claim that "All cultural behavior is patterned" and the conclusion that, while not all forms of cultural behavior so well illustrate unconscious patterning as does linguistic behavior, "the more carefully we study cultural behavior, the more thoroughly we become convinced that the differences are but differences of degree" (1949[1927]:546, 556, cf. Kroeber 1923).

One striking aspect of the article is the way in which Sapir hesitates at the edge of a concept of tacit knowledge. He refers to the 'intuitive repertoire' of the native speaker (551), and interprets the notion of cultural patterning of behavior in terms of crucial turning points in the course of action that give formal significance to the whole in the minds of those who possess the key to its understanding (546-7). Because the connection between forms and functions (practical purposes) is unstable, a functional approach does not suffice; one can not stop with asking why something is done, one must also know "what is the precise manner and articulation of the doing" (547). Normally, an individual cannot, however, accurately articulate the forms of behavior he follows, or give a general rule of which specific behaviors are examples,

although all the while he acts as though the rule were perfectly well known to him. IN A SENSE IT IS WELL KNOWN TO HIM (Sapir's emphasis). But this knowledge is not capable of

conscious manipulation in terms of word symbols. It is, rather, a very delicately nuanced feeling of subtle relations, both experienced and possible. To this kind of knowledge may be applied the term 'intuition'...(1949[1927]:548).

It is because the term 'knowledge' in itself implied conscious knowing, that Sapir used 'intuition' (cf. 1949[1927]:548, in regard to unconscious status: "these patterns are not so much known as felt"). Clearly the references to 'form-feeling' and the like are concerned with what today would be called 'tacit knowledge', a tacit knowledge of formal relations. Indeed, inasmuch as the knowledge is of possible, not experienced, relations, it is implicitly generative.

Notice that Sapir here revised the 1912 principle of the dependence of cultural form on consciousness. It is now not the form so much as the function of cultural patterns that is readily reshaped. It is just because of "the readiness with which forms of human conduct lose or modify their original functions or take on entirely new ones that it becomes necessary to see social behavior from a formal as well as from a functional point of view" (1949[1927]: 547), to know "what is the precise manner and articulation of the doing". It is a commonplace, Sapir continues, that "the reasoning intelligence seeks to attach itself rather to the functions than to the forms of conduct" (1949[1927]:547). Insofar as rapidity of change is still linked in Sapir's mind with conscious awareness, one now has in culture a kind of form whose slower drift may be more in keeping with that of language. Indeed, a viewpoint rather like that of romantic historicism comes to the fore with a vengeance at the end of this essay, when "a healthy unconsciousness of the forms of socialized behavior to which we are subject" is said to be as necessary to society as unawareness of the workings of the viscera to the health of the body. Awareness should be left to the student whose business it is to understand such patterns. "Complete analysis and the conscious control that comes with a complete analysis are at best but the medicine of society, not its food" (1949[1927]:558-559).

One sees the union of the ideas developed in the articles of 1924 and 1927 in "The status of linguistics as a science" (1949[1929]: 160-166): (1) the form-feeling and formal completeness, adequacy of language, as tending to give form for individuals to cultural behavior (1924), and (2) all cultural behavior as sharing with language a determination by unconscious patterning--cultural behavior as a realm of pervasive, tacitly known form (1927). This union may be seen to underlie the often-quoted paragraph on language as a guide to social reality. What had been an admonition about habits of speech becomes a flatly stated general doctrine: Language powerfully conditions all our thinking; human beings are very much at the mercy of their particular language; indeed,

The 'real world' is to a large extent unconsciously built up on the language habits of the group. No two languages are ever sufficiently similar to be considered as representing the same social reality. The worlds in which different societies live are distinct worlds, not merely the same world with different labels attached (162).

The wheel has come full circle in a sense, but inverted itself. Where in 1912 one could not accept language as a reflection of culture, one can now speak of "language as the SYMBOLIC GUIDE TO CULTURE" (1949 [1929]:162; emphasis Sapir's).

The logic of ideas whose unfolding has just been discussed is essential, but nevertheless I do not think it the whole story. There is the question of Sapir's receptiveness to the inspiration he received from Croce, and also of the constancy and force with which he repeats in almost every article the autonomy, the completeness and adequacy, of language; the force of his statement of the positive feeling for form; what seems the increased power with which he states the unconscious pattern of behavior in 1927, not simply as a proposition, but as a tyrannical hold, that should not be lightly broken. These are matters on which biographical information may shed more light. The renewal of a view of culture as patterned must be studied in the context of the development of ideas of pattern in the Boasian tradition (Boas 1911 [cf. Hymes 1967c:37-9], Lowie 1914:20).

From the thesis of language as a guide to social reality was to follow, after Whorf's rediscovery in the late nineteen-forties, one kind of linguistic method of ethnography, language as a guide to world view. From a second thesis of the 1929 essay was to follow the development that is the main concern of this paper: "linguistics is of strategic importance for the methodology of social science" (1949 [1929]:166). This thesis also depended on the development just traced: the rediscovered unity of language with all of cultural behavior as unconsciously patterned (166) and the special nature of language as the most self-contained sector of tacitly known from (164-5). A fuller context for the statement just quoted is as follows (1949[1929]:166):

"It is precisely because language is as strictly socialized a type of human behavior as anything else in culture and yet betrays in its outlines and tendencies such regularities as only the natural scientist is in the habit of formulating, that linguistics is of strategic importance for the methodology of social science. Behind the apparent lawlessness of social phenomena there is a regularity of configuration and tendency which is just as real as the regularity of physical processes in a mechanical world, though it is a regularity of infinitely less apparent rigidity and of another mode of apprehension on our part. Language is primarily a cultural or social

product and must be understood as such. Its regularity and formal development rest on considerations of a biological and psychological nature, to be sure. But this regularity and our underlying unconsciousness of its typical forms do not make of linguistics a mere adjunct to either biology or psychology. Better than any other social science, linguistics shows by its data and methods, necessarily more easily defined than the data and methods of any other type of discipline dealing with socialized behavior, the possibility of a truly scientific study of society which does not ape the methods nor attempt to adopt unrevised the concepts of the natural sciences. (Emphases mine).

The centrality of language and the rigor of linguistics make of linguistics a model for a social science whose rigor is not quantitative, but qualitative. (cf. Hymes 1977).

One can see the implication already in the essay on unconscious patterning. There Sapir mentions gesture and posture (1949[1927]:557) and breathing (545-6) in a way that anticipates the development of kinesics, proxemics, and paralinguistics a generation later. (cf. Birdwhistell 1961, Hall 1964:155, 1968). That same article spoke of unconsciously known rules with regard to terms of address and permitted social relationships. One can see in the concluding sentence of "Sound patterns in language" a further hint of a linguistics-based methodology, applied to the discovery of the tacit knowledge governing social interaction and communication (1949[1925]:45) and, in that same article, the starting point for the methodological foundations of such an approach--the initial statement of structural contrast, of the distinction between 'emic' and 'etic' analysis of behavior, and of the relations among levels in language that, when carried through to its conclusion, comes to implicate a level of speech acts, and nonverbal acts, together with language proper.

None of this is set forth by Sapir himself. He does move to a view of culture in terms of social interaction; he reiterates in telling ways the view of a "more intimate structure of culture", of underlying relationships missed by the 'table of contents' approach that divides cultures up into a priori categories; he repudiates the "desire to lose oneself safely in the historically determined patterns of behavior" that he sees underlying the study of culture as such (1949[1934]:592) in favor of an approach that brings every cultural pattern back to the living context from which it has been abstracted in the first place (592). And he sketches the consequences for language (1949[1934]:592-3):

The social psychology into which the conventional cultural and psychological disciplines must eventually be resolved is related to these paradigmatic studies as an investigation

into living speech is related to grammar. I think few cultural disciplines are as exact, as rigorously configured, as self-contained as grammar, but if it is desired to have grammar contribute a significant share to our understanding of human behavior, its definitions, meanings, and classifications must be capable of a significant restatement in terms of a social psychology which transcends the best that we have yet been able to offer in this perilous field of investigation. What applies to grammar applies no less significantly, of course, to the study of...any segment...which convenience or tradition leads us to carve out of the actual contexts of human behavior. (Emphasis mine).

Set in the context of the rest of Sapir's latest views on the study of man, this passage suggests very strongly the kind of work now being begun under the rubrics of the 'ethnography of speaking' and 'sociolinguistics'. Sapir's very argument as to the failure of conventional approaches, which deal with supposedly self-contained patterns, because behavior is not a recomposition of abstracted patterns, but the very matrix out of which the abstractions have been made in the first place (1949[1934]:594)--that argument is the same as was to be given, more than a generation later, for the ethnography of speaking as an approach that sought the relation of language and culture, not as disparate abstractions, but where it actually existed, in speech. Sapir would seem to have transcended, not only the 'obscure opposition in spirit' of the cultural and personal (1949 [1934]:592), but also the opposition of language to culture with which this intellectual development had begun. The specific linguistic implications, however, were never detailed by him, but only, as I have said, suggested (cf. Silverstein 1973:160).

Aberle's critique

Culture and personality:

It has been argued that linguistics did in fact have an immediate major role in shaping cultural anthropology in the period just discussed. Writing on "the influence of linguistics on early culture and personality theory", Aberle (1960) maintains that methodological assumptions in the linguistic work of Boas and Sapir shaped the limited success, and the eventual cul-de-sac, of the culture-and-personality work of Benedict and others. A mechanical transfer or imposition of linguistics is not suggested. Rather,

Given a striking early success in one field of cultural analysis, it would appear natural that the analogy adopted for an approach to culture would be that of language. I am not arguing that the approach to language developed in independence of thinking about culture and was then applied to culture. Rather I am saying that certain general assumptions made about culture, including language, proved particularly

fruitful for work in linguistics, and that success and more detailed theoretical development in linguistics made the extension of assumptions derived from linguistics to total culture exceedingly tempting.

Linguistics is seen to have been influential through the drawing of parallels, explicitly and implicitly, between language and culture as a whole.

Aberle locates the ultimate failure of the approach in its restriction of its object of study to just two terms: a cognitively shared culture, on the one hand, and an isomorphic personality, on the other. Missing was a necessary third term, that of 'cultural system' (equivalent to social structure, or network of social interaction). This third term was not derivable from the shared value-orientations and knowledge on which the culture-and-personality approach (a dominant approach in the American anthropology of the time) came to focus. The 'cultural system' is one in which persons participate and through the relationships of persons to values and knowledge can be made intelligible. Its study entails an analysis of the on-going adult system of technological, economic and political interaction within an ecological context.

Aberle's critique would apply to the main influence of linguistics (especially generative grammar) on the study of culture and persons today; it faces a cul-de-sac because it cannot handle the matrix of social interaction. I agree with Aberle, and indeed wish he would not, like so many social anthropologists, excuse linguistics by saying that what is untenable anthropologically may of course be quite all right for linguists. The difficulty with Aberle's analysis, as a historical account, is that Sapir pretty much agreed with him. More precisely, the account does not catch the development of Sapir's own thought. If culture-and-personality took the direction it did partly because of an appeal to linguistics, it did so after 1934 in contradiction to Sapir's own views.⁸

There was indeed a prevalent approach which saw but two terms of analysis, general cultural patterns and individuals; for which cultural patterns were uniform, and the problem of personality study that of explaining the replication of the uniformity in successive generations (cf. the related critiques by Schneider 1965 and Wallace 1961). It would be widely agreed that personality must be studied in terms of what Wallace calls 'organization of diversity'-- differential acquisition and possession of values, knowledge, expectations, within a shared social system. Sapir himself came to criticize cultural anthropology, and, in passing, linguistics itself, in such terms. His celebrated articles on the study of personality have just this point, summed up in the often-quoted comment on an idiosyncratic Omaha Indian: "In some sense, Two Crows is never

wrong" (Sapir 1949[1933]:572; for an application of the perspective to linguistics, adapting Sapir's language, see Hymes 1967a).

Sapir's point was that the essence of the cultural is not that it is shared (consensus) but that it is capable of communication to other individuals (symbolic). An idiosyncratic divergence has from the very beginning the essential possibility of culturalized behavior. And in fact from the standpoint of the individual, cultural behavior is not a matter of abstract patterns (Sapir came to speak of them as 'cold' and 'indifferent'), but of participation in various subcultures. He came to conclude that "it is only through an analysis of variation that the reality and meaning of a norm can be established at all" (1949[1938]:576), and that "In spite of all that has been claimed to the contrary, we cannot thoroughly understand the dynamics of culture, of society, of history, without sooner or later taking account of the actual interrelationships of human beings" (575).

Sapir did not develop his perspective in terms of a third concept, that of social or 'cultural' system. He rejected one term, cultural pattern, as starting point, in favor of the other, the person. Instead of asking how culture was transmitted, he wished to ask how culture was acquired, created, used and shaped, how meaningful. He saw his approach as one of "explicit stress on the total personality as the central point of reference in all problems of 'culture' (analysis of socialized patterns)" (1949[1939]:479, n.1; the quotes are Sapir's). But he did also reject the conventional contrast between individual and society (1949 [1932]:519) to recognize subgroups and subcultures as the common locus of patterns, and he repeatedly insisted on systems of interrelationship, social matrix, experienced realities of communication, specific interactions, as "the true locus of culture" (1949 [1932]:515).⁹ If his focus remained the differential participation of persons, his concern for personal meaningfulness and adaptation complements, if it does not indeed lead to, analysis of the social, economic and ecological dimensions.

Language and linguistics become caught up in this 'psychiatric' perspective ('psychiatric' for Sapir explicitly means focus on the total personality as central point of reference (1949 [1939]:579, n. 1); cf. the admonition to set ourselves at the vantage point, not of a passively inquiring, but of a culture-acquiring child (1949 [1934]: 596). At the end of his life Sapir thus broached a perspective radically different from that with which he began. Indeed, there is no point or publication in his career that can be safely taken as representing 'Sapir's view'. Sapir's view of the relation between language and culture, between linguistics and anthropology, was a continuously changing one. Beginning with ideas and problems set by Boas, he continued to develop their implications, partly in terms of an internal logic, partly in response to factors around him, until his death. It is in the context of this continuing development that

one can understand his enunciation of linguistic method as a model for social science.

It remains that Sapir's appeal to linguistics as a source of method was not developed by him. (This point is also made by Silverstein 1973:160.) In its immediate context, as we have seen, it is an argument for the autonomy of the human sciences as a whole (1949 [1929]:166):

Better than any other social science, linguistics shows by its data and methods, necessarily more easily defined than the data and methods of any other type of discipline dealing with socialized behavior, the possibility of a truly scientific study of society which does not ape the methods nor attempt to adopt unrevised the concepts of the natural sciences.

And a call for a wide view of the field of linguistics, for it continues:

It is peculiarly important that linguists, who are often accused, and accused justly, of failure to look beyond the pretty patterns of their subject matter, should become aware of what their science may mean for the interpretation of human conduct in general.

The years after Sapir's death in 1939 were to see, not a broadening, but a narrowing of the effective scope of linguistics in the United States. There were a few studies by students of Sapir in his spirit (notably Newman 1939, 1941), but generally the history of the relation of language to culture after Sapir's death in 1939 can be said almost to have recapitulated, twice, the outlines of his own personal development. An emphasis on the separateness of language and linguistics, a sense of achievement with regard to linguistic form, a beginning to seek out a broader relevance to social life on the basis of that achievement--such can be said to be the course, very roughly, of linguistics, first under Bloomfieldian, then under Chomskian auspices.

V. BEFORE AND AFTER THE SECOND WORLD WAR

Before

Sapir enunciated the significance of linguistic method for the cultural sciences in 1929; significant professional attention to the notion came in the 1950s. This intervening period--roughly, the 1930s and 1940s--saw a great deal of work that was to have importance in cultural anthropology: the development of the structural concepts that were to provide specific exemplars of linguistic method, and the writings

on the relation of language to world view that were to provide the first great excitement as to the importance of linguistics after the Second World War. This work, like Sapir's move in the 1930s to a somewhat "phenomenological," "existential" approach, did not have apparent consequences at the time.

The mid-point of the period--the brink of the Second World War--seems in fact to have been something of a low point for the relation of linguistics to cultural anthropology, so far as published statements by anthropologists are concerned. Lowie elaborated on the Boasian theme of the validating role of practical use of linguistics, but said nothing of Boas' concern for the theoretical importance of linguistics for ethnology (1937:70, 89, 95, 130, 132-3). Mead (1939) was perceived as a challenge to the view that mastery of the native language (and perhaps of linguistics) was essential (Lowie 1940). An anthropologist was chosen president of the Linguistic Society of America, but not for current work. His presidential address (Kroeber 1941) was brief; its five pages reviewed four ways in which linguistics and ethnology might contribute one to the other with historical findings.

What might have happened in these years, had Sapir lived and been an active force, can be conjectured from the linguistic contributions to a volume in his memory (Spier et al 1941). One finds in the volume the sense of a linguistics rooted in anthropology, and expressing continuities, tensions, and seeds of change that could be expected from such roots. A large proportion of the contributors address the relation of linguistics to anthropology. To be sure, the first paper (Hoijer 1941) honors the enduring concern with the "ethnological question" in a cautious up-dating of the Powell classification of American Indian languages that manages to conceal the ferment that Sapir's discoveries and hypotheses as to remote relationships had stirred (and in which some of the other contributors [Swadesh, Trager, Whorf] were caught up). But in a paper similarly conceived as an informative service, a survey of the still-existing Indian languages, Voegelin (1941) reaches out for a connection between language and culture against the background of "one of the famous negative generalizations of American anthropology . . . to the effect that there are no expectable correlations between race, language and culture" (31). Voegelin negates that well known Boasian negation, pointing out that the lack of correlation holds at the level of comparison between language families and culture areas. As smaller units are considered, correlations emerge. Each separate language is in only one culture area, even closer correlations appear with dialects, and a single individual may be said by definition to represent a complete correlation. By a sort of logical tour de force, then, Voegelin proposes a reversal of vantage point for language and culture relationships. The Boasian concern had been to disprove necessary relationships between cultural accomplishment and linguistic or racial identity, because of misleading, sometimes imperialist and racist inference. The new vantage point is

analogous to the personalistic approach proposed by Sapir in his later papers (though no citation to Sapir appears), in its mention of the individual, and its finer mesh for the relation between languages and culture areas might have given rise to a new direction of research in that regard. Such directions, however, were to have to wait for a sociolinguistic climate (cf. Sherzer and Bauman 1972).

In general it is a vision of breadth in relation to known subject-matter and method that informs most of these papers, when they reflect on Sapir and his legacy. Swadesh (1941:50) defends his observations on the effect of one of a bilingual's languages on the other against a narrower behaviorist orientation:

This is Sapir's method, the critical use of every bit of evidence. It refuses to be restricted to using only half the observable facts, and is not frightened at such stigmas as "mentalist."

Herzog (1941) also studies a situation of language contact and change, providing indeed what is still one of the richest sources on lexical acculturation, with sensitive suggestions as to attitudes and the conscious or unconscious status of the processes of change. As with Swadesh, the orientation is to a single linguistic line, and the perspective is integration of cultural and linguistic content, not extension or change of method in either discipline.

Two of the authors do express some disciplinary "self-criticism" (Emeneau, Newman). Emeneau (1941) calls upon the linguist to contribute to anthropology by accepting a broad scope of subject-matter, and as in the case in hand, especially a focus on verbal art-forms, stories, songs, oratory, and the like which are seen as giving the most overt and self-conscious expression to the patterns which members of the culture follow or profess. Sapir's statement that language is a guide to the scientific study of a given culture (1929:209) is aptly elaborated in this vein (a vein of course continuous from Boas). Emeneau admonishes:

Linguistic study, if it is confined to its primary object of recording and analyzing the forms of utterances and never goes beyond that, can offer little to ethnology. It must, with that analysis as a basis, go on to a strict philological analysis of well-recorded and fully guaranteed texts in the forms most highly esteemed by the culture, and the results of such study should provide then "the linguistic symbolism which makes these outlines (of a culture) significant and intelligible" (a quotation from Sapir).

(Such a culturally-guided textual linguistics raises its own questions of method, of course, but not here the question of a method common to linguistics and ethnology.)

Newman's comments on the scope and method of linguistics stem from research of a kind called for by Sapir and little developed in method or findings (this is a decade before the emergence of "psycholinguistics"). The comments are sharp, noting tension between the autonomous linguistics that is emerging and other interests in language. The usual linguistic description is said to be a highly selective and abstract part-picture, omitting a great deal of the language process as it takes place in the speech and writing of human beings; the linguist seems to regard language as consisting of huge automatic mechanisms and the language-using person as passive. Newman explores an overlapping of linguistics and individual psychology by a description of the language behavior of a specific person. A relation between linguistics and other fields is proposed in terms of general recognition of overlapping subject matter, thus (1941:95):

The sharp separation which exists between linguistics and other disciplines is, at best, an arbitrary one, based upon the development of an isolated methodology and not upon any empirical division of subject matter.

There is concern, then, at growth of a gap between linguistics and cultural anthropology. There is critique of linguistics in terms of scope: linguistic problems should be conceived broadly, not narrowly. There is not a critique of methods or methodological assumptions in linguistics, except insofar as concern with breadth militates against a narrow conception of method. But three other papers stand out as harbingers of developments to come after the war, seeking to close the gap between linguistics and cultural by attention to the methods of linguistics itself.

Whorf (1941) extends the importance of linguistics, as subject-matter, through his well known thesis, following Sapir, that where linguistic patterns interact over time with cultural attitudes, they may come to shape a world-outlook, and that language patterns, because of their rigor, centrality, and slowness to change, may come to play a fundamental role in maintaining such an outlook. In all this Whorf was essentially developing the implications of a consensus to be found in Boas, Lowie, and Kroeber. The assumption of the unconscious patterning of cultural behavior and language, and of the greater clarity of patterning in language, so that language could be privileged as an area revealing the nature of cultural patterning in general, were shared. The novelty of the view stated by Sapir and developed by Whorf was to propose a role for language that was not only exemplary, but causal.

Just the standard methods of linguistics would take on extreme importance, if the grammars they produced should be keys to world views. (In the context of focus on individual cultural traditions, the implication was equivalent to the significance of general grammar in the context of focus on universal mind.) But like Sapir in

concern with "living speech," Whorf did not accept the adequacy of ordinary grammar for discovery of diagnostic connections with culture of sort he found among the Hopi and suggested could be found more generally. Specific cultural types and categories, and specific linguistic types and categories, are too gross. One must look for "fashions of speaking" that do not depend upon any one system within the grammar, but cut across the typical grammatical classifications, so that such a "fashion" may include lexical, morphological, syntactic, and otherwise systemically diverse means coordinated in a certain frame of consistency (1941:91-2). In summ (93):

These connections are to be found not so much by focusing attention on the typical rubrics of linguistic, ethnographic, or sociological description as by examining the culture and the language (always and only when the two have been together historically for a considerable time) as a whole in which concatenations that run across these departmental lines may be expected to exist, and if they do exist, eventually to be discoverable by study.

Here is in effect a call for a method that is truly ethnolinguistic. Language and culture are to be studied, not apart, but together. The connections between them are to be found, not by analogy or correlation between conventional categories or categories kept apart, but by an integrating pursuit of "ways of analyzing and reporting experience" ("fashions of speaking") which, one may suppose, could extend to ways of conduct as well. Such an approach would have changed both linguistic and ethnological method from a standpoint anthropological in origin.

Whorf's conception of fashions of speaking was not developed, and perhaps had to wait for a sociolinguistic climate of opinion a generation later for its relevance to be perceived (cf. Hymes 1966, 1974a: 174ff.).

It is the papers by Trager (1941) and Kluckhohn (1941) that announce questions of method that are to have sustained development after the war. One deals with extension of linguistic method, the other with appeal to linguistic analogy.

Trager has traced the development of paralinguistics, kinesics, and his general scheme of language in relation to other cultural systems back to his study of accentual systems in this volume, and properly so (Trager 1958:1). The study extends the principles of linguistic structure and method into a sector of speech not previously encompassed. (Trager refers the principles to Sapir's "epoch-making 'Sound patterns in language'" (p. 131). Phenomena of relative loudness, pitch, quantity, that had been treated in terms of physical measurement are to be brought within the "realm of configurational linguistics" (143) as prosodemes and prosodic systems. The paper is thus a

substantive step in the realization of the program implicit in the last sentence of "Sound patterns of language":

(sound patterns are) really a special illustration of the necessity of getting behind the sense data of any type of expression in order to grasp the intuitively felt and communicated forms which alone give significance to such expression.

Kluckhohn was to be an advocate of linguistic method in anthropology until his death in 1960. In this volume he publishes the first explicit appeal by a cultural anthropologist to the example of linguistics on behalf of a new method of work in cultural anthropology itself, particularly in a section, "Linguistic concepts in anthropology" (pp. 110-112).

There is continuity in Kluckhohn's appear with the interest of Lowie and Kroeber in the historical regularities and stabilities of language, with the view of language as exemplifying unconscious patterning that is general to culture, and with the view of pattern as specific to a cultural tradition. Indeed, at one point Kluckhohn remarks that "Conceptions of patterning used in anthropology seem to have been much influenced by linguistics" (110), although the examples that precede the statement have to do with folk speech, not with linguistic structure as understood at the time. Kluckhohn does appreciate the principle of phonemic configuration as a principal of structural regularity, as opposed to randomness, and has the notion of interrelatedness as key. But it is regularity of pattern as such that is elaborated upon, not any principle of method or structure on which it may depend. It is the presence of determinate organization (as against randomness), and the fact that the determinate organization, the pattern points, differ between cultures and languages. There is sense of the interdependence of pattern parts, a sense of differentiation and contrast, of items as entering into different relations in different patterns, but the overall thrust is toward patterns as cumulations of what is alike within a culture, and of contrast as entering between cultures. The stress on regularity seems consonant with the view of pattern (found notably in Benedict's Patterns of Culture) as summative within a culture, as a matter of leitmotifs or themes or overarching wholes.

The main purpose of the paper is indeed to establish conceptions and terminology (especially to establish interrelated uses of structure, pattern and configuration), and to establish the kind of regularity that "patterning" implies as central to the development of anthropology. The debt to Sapir, as to "how the concept of 'pattern' might be transferred from linguistics to anthropology" (p. 111) is to the "Unconscious patterning of behavior in society" and the paper on "The concept of phonetic law," which assert and demonstrate patterning.

There is no mention of the Sapir paper of 1929, in which the claim of the general relevance of linguistic method for establishing pattern is made, nor of the 1925 paper on sound patterns, which has proven seminal for methodological principle. Contrast within a pattern, or culture, as the basis for determining pattern to exist, was not to be noted in general anthropology until after the Second World War. In 1959 Kluckhohn would cite Sapir's example of a Frenchman not distinguishing between English /s/ and /th/, as he had in 1941; but whereas in 1941 the example served only to show the presence of pattern, in 1959 it would also manifest the method. The change would depend, I think, on the spread of actual experience, training, in the methods of linguistic analysis, especially of phonology during the 1940s.

In sum, the volume in memory of Sapir might have provided a catalyst for stock-taking as to the place of linguistics within anthropology, or stood as symbol and point of departure for the maintenance of close, indeed closer, ties on the basis of a Sapir tradition. There seems to have been no such effect, nor was there a chance of one. The Second World War intervened, and a focusing of activity around materials for the teaching of languages and elaboration of descriptive methodology. (cf. Hymes and Fought 1975: 955, 955-1022, 1018-1029, 1035ff.).

After

The years just after the Second World War show some tension in the relation of linguistics to anthropology. The two men who had most powerfully united the two fields in their own persons, Boas and Sapir, were dead (in 1942 and 1939, respectively). Kroeber's interest in linguistics was at a low ebb (see ch. 5). And two activities that had sustained a common world of reference--the study of the American Indian, and problems of historical ethnology--became rapidly peripheral. Most leading linguists in anthropology were identified with Amerindian work, but anthropologists as a whole increasingly were not. Amerindian examples of linguistic structure remained popular, but the historical relationships, and even the names of American Indian languages passed from common knowledge to esoteric lore.

The situation was conditioned by a change of generations. For most linguists, especially students of Sapir and Bloomfield, the relationship to anthropology was important. The approach to language teaching that had been the major focus of the profession during the war, and that had brought linguistics considerable attention, was often called an "anthropological" approach. Conceptions of method were linked to (although not uniquely determined by) an ethnographic style of work. Anthropology was the main alternative to the otherwise dominant language and literature departments as an intellectual framework and as a source of employment. (The close connections of linguistics to psychology and logic from the late 1950s on were not yet in sight, and

separate departments of linguistics had hardly begun to emerge.) Even linguists not close to anthropology often considered their discipline in principle a branch of anthropology.

On the side of cultural anthropology, a change was marked organizationally by revamping of the American Anthropological Association in 1946, a move spearheaded by those trained circa 1930. It is my impression that such men had less interest in linguistics, and that two factors were important in shaping such an attitude. On the one hand, this generation was at a second remove from the German cultural tradition, evident in Boas, Kroeber, Lowie, Sapir, and Radin, that linked anthropology with cultural studies generally, and made language an essential instrument as well as object of interest. On the other hand, this generation did not have much opportunity (or necessity) to learn the methods of linguistic analysis newly developed during the 1930s and early 1940s. Its conception of linguistics harked back to the Boasian style. (The difference extended to orthographic loyalties. Sapir and his students [Herzog, et al, 1934] initiated a symbolic change in two senses of the word.) After the war, then, many anthropologists found linguistics officially a branch of their profession, yet a novelty, if not an enigma or a mistake, in its approach.

Linton (1945b) talked of configuration, but not of language, seemingly pointedly ignoring the linking of the two by Kluckhohn (1941), and the relation of speech to personality and patterning shown earlier by Sapir. As testimony to the climate of opinion, note Trager (1966:25): "Good ethnographers have always paid attention to 'minute details' of meaning. The 'new phase in descriptive ethnography' that Colby notes is not new--it is merely a long-neglected continuation of the old, tried and true methods of Boas and all founding fathers of our field. True, they were followed by a generation of much lesser stature, with a kind of inbred fear of language and its uses, and the 'great thinkers' of that generation went in for theory, or what they thought was theory, without bothering with troublesome data. There are now once again anthropologists who insist on recording data, including language and remarks about other data..." In the period itself, one linguist dispassionately noted the anthropologist's viewpoint (Greenberg 1948 [1964]:28): "Present-day descriptive linguists strive towards formulations in which elements are defined by a purely formal procedure without reference to meaning. While it is in syntactics [i.e., semiotically, in the kind of study just indicated] that recent linguistics has made its most significant methodologic progress, the remoteness of this aspect of language has led to the recurrent complaints of the cultural scientist against the irrelevance to his problem of a large portion of contemporary linguistics." (Cf. Newman 1941:94).

Shifts in educational backgrounds of students entering colleges and universities may have had some part; cf. the complaint of Lowie (1944). A further factor may have been the growing prominence, after the war, of British social anthropology, and American anthropology derived from it. As has been noted earlier, British social anthropology (especially under the influence of Radcliffe-Brown, who has pre-eminent at the time), had no place for linguistics as such. In the lectures in the 1930s responsible for his great influence at Chicago he considered language the one subject that might be excluded from a 'natural science of society' (1957:142-143).

Some bearers of the new linguistics, proud of their subject and lacking proper recognition, were for their part aggressive in what the Voegelins have called an 'eclipsing stance' (1963:12) toward previous work. Some seemed to make a dogma of exclusion of the study of meaning, and to derogate vocabulary and texts, as against formal grammar and phonology. It came to seem to some anthropologists that no substantive interest mediated between them and linguistics. The new work in linguistics, and the new linguists, might acquire prestige, but their subject-matter and method appeared disjunct. (A noted postwar anthology [Linton 1945] explicitly excluded linguistics on the grounds of its irrelevance. Cf. also the sense of disjunction signalled in the title of Voegelin 1949a, "Linguistics without meaning and culture without words".)

A sense of a traditional unity, yet a patent gap, evoked a series of writings on the relations between linguistics and anthropology, and between language and culture, such as had not been seen since the preceding century. Then the context had been controversy as to the proper basis for classifying ethnic groups as to their history and origin; now the context was the analysis and explanation of culture.

In this respect, the years from the end of the Second World War to 1952-1953 form something of a natural unit. They begin with the resumption of normal academic activity (anticipated, to be sure, before the war was actually over, as in the resumption of the International Journal of American Linguistics at the end of 1944, and the launching of the new international journal *Word* in New York in 1945), and end with several, conferences that together express the end of one set of relations for linguistic method and the beginning of a new. The flurry of discussion in these years has three phases: general discussion of the scope and relevance of the two disciplines with regard to each other; discussion of relation between the patterns of language and those of culture, focussed especially on Whorf; and the beginnings of general anthropological discussion of extensions and transfers of the methods of linguistics to the rest of culture.

Scope and relevance.-- A number of anthropologically-oriented linguists joined in attempts to define the place and relevance of

the new work in linguistics within a larger cultural frame of reference (Voegelin and Harris 1945, 1947; Greenberg 1948; Hockett 1948a, 1948b; Nida 1948; Trager 1949; Olmsted 1950). The principal controversialist was Voegelin (perhaps sensitive to the situation, as chairman of a new department of anthropology). He challenged the traditional unity of the two disciplines (1949a), and of language and culture as well (1950), provoking vigorous objection from an ethnologist (Opler 1949), a fellow linguist and a student of Hoijer (Bittle 1952), but persisting in exploring the differences further (1949b, 1951). The thrust of Voegelin's writings can be seen as provoked by, and intentionally provocative to, the neglect of linguistics by the cultural anthropologists of his generation. They could not both have credit for linguistics in principle and ignore it in practice. Voegelin's tactic was to seize upon practice as the true test.

Method entered into discussions only as an evidence of difference (e.g. Olmsted 1950:7-8). References to Boas and Sapir did not elaborate on the ideas treated earlier in this paper. When Greenberg concluded a comprehensive analysis with the view that

a mature science of culture is unlikely to emerge without
the linguistic approach to culture having played a significant
role

he included questions of method in the "rewarding field which awaits the linguistically oriented ethnologist" only in terms of practical skills and understanding of the linguist's technical processes for the sake of informed use of their product. He did not include use of those technical processes by the ethnologist himself, let alone for the sake of other, non-linguistic products (Greenberg 1948:147). Sturtevant has pointed out (1964:124, n. 3):

It is significant that Olmsted in a general survey of the
relations between linguistics and ethnology made in 1950
envisaged nothing like the present adaptation of linguistic
methods to ethnography.

Lévi-Strauss had indeed published "L'analyse structurale en linguistique et anthropologie" in New York in 1945. There he had appealed, as had Sapir, to the special place and methodological example of linguistics among the human sciences, specifying the work of Trubetzkoy and Jakobson for its concern with unconscious, rather than conscious, structure; with relations, rather than terms; with, indeed, systems of relations; and with the discovery of necessary relations, general laws. Although Lévi-Strauss and Jakobson both were in the United States from early in the 1940s, and Jakobson remained (having stayed initially in fact with Boas), their influence on relations between linguistics and anthropology in the United States was to become prominent only with conferences of 1952 and in the period following.

Levi-Strauss' 1945 paper was not taken up, partly perhaps because in French, but no doubt partly because of the prevalent suspicion of things 'European' and 'metaphysical' in the American linguistics of the time.¹⁰

The one explicit discussion of relations in method, among the papers here considered, was in one of the pair that initiated the American discussion. Observing that the data of linguistics and ethnology were in general the same, their methods different, Voegelin and Harris noted specifically (1947:593; cf. Voegelin 1949a: 36, Olmsted 1950:7-8):

Cultural anthropology is dependent upon comparative consideration for finding its elements; linguistics is not.

That is, units of culture were in practice determined by considerations of cross-cultural comparison and classification, those of linguistics through internal analysis of structural relationships. (The issue is independent of the question of the place of general theory, or universals, in analysis of a particular culture of language. Either way of determining units makes assumptions as to theory and universals; either might be a priori or inductive in emphasis.) The Voegelin and Harris statement neglects some study of social structure and cultural patterning, but does reflect a prevalent practice of treating ethnography as not a theoretical activity in its own right, but rather as the filling in of a set of chapter-headings for major departments of culture, the checking off of a list of traits diagnostic within an area or the identification of one of a set of cross-culturally significant alternative types.¹¹

It was precisely as warrant for an alternative to such practices that linguistics was to attract some ethnographers a few years later. Voegelin and Harris correctly reflect the sense of disjunction in method at the time they wrote. The same sense of disjunction is to be found with regard to two specific subject matters, wherein a congruence of method might be expected, and where, indeed, it was later to emerge, semantics, and social interaction. Writing when semantics was not on the agenda of linguistics, Nida argued the necessity of an ethnological approach (especially from the standpoint of his concern for translation), concluding that "A combination of analytical social anthropology and descriptive linguistics provides the key to the study of semantics" (1945:208). Greenberg shared Nida's view (1948 [1964:28]), and went on to argue the importance of semantic description to the ethnologist as well. Both men, the one to a linguistic audience, the other to an anthropological, write in terms of a gap to be overcome by cooperation. Where sharing of training and method are mentioned, it is the linguist who is thought of as acquiring social anthropology, not the converse (Nida 1945:194, Greenberg 1948 [1964:28]). In their first article, Voegelin and

Harris (1945:356-7) define 'ethnolinguistic situation' as one in which both verbal and non-verbal behavior are integral parts, and again speak in terms of disjunction:

The result of this practical divorce of linguistic work from cultural investigation often means that the final linguistic statements and the final cultural statements are incomplete; or statements covering the ethnolinguistic situation as a whole are neglected.

Greenberg (1948 [1964:28-29]) pointed to study of the use of language under the category of 'pragmatics', including there all that related language to its users, but singling out patterns of (and in) speaking. At the same time he correctly observed that the interest of the linguist was merely marginal, and that though verbal behavior should be an object of study in its own right to the ethnologist, description of its patterning as a whole was never attempted.

In retrospect, one can see the rest of the story of linguistic method in ethnography (and of ethnographic method in linguistics) as largely a response to these three problems: the implicit challenge to reconsider the nature of the cultural, in relation to language; the challenge to take up again the ethnographic study of meaning; the challenge to recast the scope of linguistics and ethnography to encompass patterns of speaking. For the moment, attention was to be focussed elsewhere. Following on discussion of the possible connections between linguistics and ethnography, there was to be concentrated discussion of a connection of the greatest import, language as a key to world view.

Language and world view.-- Whorf had died in 1941. In 1952 and 1953 it seemed as if the 'Whorf hypothesis' was the question of the relation between language and culture, and between linguistics and anthropology. "Language and culture" seemed for the moment almost to mean Whorf. The timing of the focus on Whorf, and the focus itself, need explanation.

Whorf's most comprehensive, and ethnologically judicious, paper on language and culture had appeared in the Sapir Festschrift of 1941. Nor was he the only source for such an interest. Papers by Dorothy Lee had begun to appear 1938 (on Wintu) and 1940 (on Trobriands), and a comprehensive account of her Wintu analysis was published in 1944 in the first issue of the new IJAL. Kluckhohn had cited her 1938 and 1940 articles (and none of Whorf's) in an essay on the nature of culture in a prominent postwar anthropological collection, quoting: "Grammar contains in crystallized form the accumulated and accumulating experience, the Weltanschauung, of a people" (Lee 1938:89, cited in Kluckhohn and Kelly 1945 [1962:60]). As the Kluckhohn essay shows, these ideas fitted readily into the concern

of American anthropology with cultural pattern (within which, indeed, they had arisen), and particularly fitted the concern with culture and personality, then at its height.

The surge of interest at the outset of the 1950s depended, no doubt, on the issuing of four of Whorf's papers in a single pamphlet in 1949. They might, however, have been ignored. The interest they excited was due, I think, to their identification with linguistics, and to the situation in the relation of linguistics to anthropology that has just been described. First, the four papers in important part were on the nature and promise of linguistics ("Science and linguistics", "Linguistics as an exact science", along with "Languages and logic" and "The relation of habitual thought and behavior to language" the paper from the Sapir Festschrift). Second, the papers were reissued, and rechristened (as 'metalinguistics') by a colleague and friend then near the height of his career as a leader of the new linguistics, who with the christening fitted them into a scheme of the relations between the new linguistics and culture, from a linguistic standpoint (Trager 1949). Third, Whorf himself had standing as a linguist, in the line of succession from Sapir, and where Lee treated linguistic facts, Whorf himself wrote grammars (as well as also contributing to historical linguistics). When reissued in 1949, then, Whorf's ideas benefited from the new prestige of linguistics in anthropology. In the minds of many they were associated with its claims of new insight and rigor.

At the time there was little knowledge or use of the new linguistics in anthropology. It was as if the concern over the relationship between linguistics and anthropology brought a principle of compensation into play, day-to-day neglect of linguistics in ethnography being matched by interest in language as a major determinant of what ethnographers study. It was as if grammar-writing might again, with Boas, be a method ethnography, although one that now would not be part of the ethnographer's repertoire, but provided by linguists such as Whorf. There was also excitement at Whorf's sometimes extreme statements, the appeal for the unmasking of unconscious forces (the silver cord of one's mother tongue), and for a new weapon against ethnocentrism. Both Whorf and Lee appealed to interest in inferring cultural pattern, but Whorf appealed as well to the anthropologist's willingness to use relativism in critique of his own civilization. Lee sympathetically interpreted the Wintu, Whorf hailed the Hopi.

The sense of excitement was abetted by an initial ignorance of the history of the problem (cf. Hymes 1963:72-73). In the immediate period the name of the temporary cultural focus proliferated rapidly, from 'Whorf hypothesis' to 'Whorf-Lee hypothesis' (I believe that Kluckhohn so put it to Voegelin in 1952), to the 'Sapir-Whorf hypothesis', as more of its history and precedent became familiar (Hoijer 1953, 1954; cf. Hoijer 1951), and anonymously, with some

recognition of W. von Humboldt and perhaps of Lee, the 'Weltanschauung problem' (Osgood and Sebeok 1952). The initial impact of the 1949 republication would seem to have prevailed, aided perhaps by the collection of much more of Whorf's work (Carroll 1956); later essays refer again simply to the 'Whorf(ian) hypothesis' (Trager 1959, Fishman 1960), or to his phrase, adopted from Sapir, but made famous by him, 'linguistic relativity'.

If the four papers on 'metalinguistics' (1949) came something like a spark in a charged atmosphere, the fire swirled and died out in very few years. It was Whorf's lot to be cast (mistakenly) as an apostle of radical relativism, as the pendulum swung away from relativism. Mixed results from two summers of experimental studies in the American Southwest in 1955-1956 discouraged some, and were interpreted by others as grounds for ignoring effects of language (cf. Hymes 1961b). In general, recognition that the problem was complex and not amenable to ready experiment, criticism of Whorf's faults, both real and imagined, and a reluctance among many linguists themselves to entertain notions of world view, effectively ended serious investigation. Many had been hostile from the start, to any suggestion of linguistic determinism or relativity, unwittingly perpetuating idealism in the name of materialism, by assigning language to the other camp. A consistent and comprehensive materialism, or naturalism, would see in languages sets of verbal tools, and recognize that they, like other tools, condition what they produce, without wholly determining it. But as will be seen with regard to linguistic method in ethnography generally, linguistic approaches tend to be treated in all or nothing terms. (I have tried to assess Whorf's work more fully elsewhere [cf. Hymes 1961c, 1966, 1970a, 1974a: 174, 1974c:1448-53, Hymes and Fought 1975:000-1002].)

It is important to note what of Whorf's work was not taken up at the time. His bold statements were discussed, sometimes out of context, but his efforts to develop semantic description and semantic typology were neglected. His key notion of 'fashions of speaking' (in the 1941 paper) was not taken up, though it might easily have led in a sociolinguistic direction. In some of his writings (particularly 'Linguistics as an exact science'), Whorf carried on the theme of Sapir's 1929 claim for the importance of linguistic method. That was to be noted later (Kluckhohn 1959:265), but not now. In all these respects, the response to Whorf was within the limits of the discussions of scope and relevance in the same years.

Turning point. -- Conferences in 1952 and 1953 round out the period in focus here, and for the first time bring to the fore some ingredients of the next. The major anthropological conference, held in 1952 at the Wenner-Gren Foundation, was conceived as 'an encyclopedic inventory', international in scope if American in weighting, meeting to assess the postwar situation of the discipline (Published as

Anthropology Today [Kroeber et al. 1953]). Four linguistics papers were scheduled, on field work, historical linguistics, structural schools, and "The relation of language to culture" (Hoijer 1953). The general relations were discussed as an introduction, Hoijer rebuffing Voegelin's suggestion of the separation of the two. The body of the essay treated Whorf and Lee (and some of Hoijer's own work). "The central problem of this report is, then, a thesis suggested by Sapir in many of his writings and later developed in more detail by Whorf and others" (Hoijer 1953:558). At a conference of anthropologists and linguists just following, organized by Voegelin, the same topic was on the agenda (urged on the organizer as the most essential one by Kluckhohn), among several other substantive questions. Yet again in the same summer (and same town) the 'Weltanschauung problem' was included in the report of a seminar that was to herald the emergence of a new major partner for linguistics among the social sciences, psychology. Semantics was salient here, as it had not been at the other; but methodology was not a focus at either. Finally, a special conference was held in Chicago in 1953, following on the main theme of Hoijer's 1952 report, and making Whorf again the focus of the general question of the relations between language and culture, linguistics and anthropology.

All this might have seemed a case of putting all the eggs in one basket, so far as the role of linguistics in cultural anthropology was concerned. In the event, even though the place of linguistics was indeed closely interwoven with the fate of Whorfian interests (cf. Kluckhohn 1956a), the two were to prove independent. In two of the conferences themselves, the question of linguistic method had come to public attention, although not in the set topics.¹²

In his prepared paper ("Universal categories of culture") Kluckhohn took note of the achievements of structural linguistics, but with doubt that they could be duplicated elsewhere (1953:507, 517):

...linguistics alone of the branches of anthropology has discovered elemental units (Phonemes, morphemes) which are universal, objective, and theoretically meaningful...It is arguable whether such units are, in principle, discoverable in sectors of culture less automatic than speech and less closely tied (in some ways) to biological fact (Wiener vs. Lévi-Strauss 1951).

In the same year Kluckhohn had published with Kroeber a comprehensive study of the nature of culture, which included a review of much of the literature on the mutual scope and relevance of linguistics and ethnography cited above. The review reached no definite conclusion and summed up, with regard to method (Kroeber and Kluckhohn 1952:124):

It is also clear that language is the most easily separable part or aspect of total culture, that its processes are the most distinctive as well as the best defined in the social sciences. What the 'cultural' equivalent of phonemes or the linguistic equivalent of 'cultural traits' may be has not yet become apparent; it may be unanswerable until the question is reformulated.¹³

A few years later Kluckhohn was to state vigorously a quite different conclusion. Tracing first the distinctive character of anthropology to its ties with linguistics, he would refer to papers by Sapir such as "Sound patterns in language" and "The unconscious patterning of behavior in society" as having had a profound impact upon cultural anthropologists, especially such 'configuralists' as Ruth Benedict (recall here Kluckhohn's own paper, "Patterning as exemplified in Navaho culture" in the Sapir Festschrift), and then take from a phonological example of Sapir, not the concept of the phoneme as a unit, but the notions of configuration and contrast as the basis of a method:

Sapir pointed out that "...the naive Frenchman confounds the two sounds 's' of 'sick' and 'th' of 'thick' in a single pattern point--not because he is really unable to hear the difference, but because the setting up of such a difference disturbs his feeling for the necessary configuration of linguistic sounds." THIS STATEMENT CONSTITUTES A MINISCULE PARADIGM FOR A WHOLE THEORY (my emphasis--DH). What is often decisive in the realms of culture (of which language is one) is not so and so much of something or whether or not phenomena have random distribution (cultural phenomena, by definition, have other than a chance distribution), but rather the question of order and arrangement. ...In my opinion, the study of cultural phenomena will progress mainly along linguistic lines by distinguishing contrastive categories--rather than by measurement as such (1959:263-264, 267).

What had happened in the interval between 1952 and 1959 was that the focus of concern for the relation of linguistics to ethnography had changed drastically. The question of 'cultural' equivalent of the phoneme had come to seem answerable, because in fact it had been reformulated as the question of relevant contrast in all of culture (including language).¹⁴ The reformulation indeed had already begun when the conferences of 1952 and 1953 were planned. It emerged first in the discussion at the 'Anthropology Today' meeting. An ethnologist query as to the cultural equivalent of the phoneme led to a discussion by Lounsbury; in response to another request, Lounsbury presented what was in fact the first published sketch of a componential analysis of kinship systems; and, under the published heading of

"Minimum units in various aspects of culture", Levi-Strauss presented examples of structural analysis in art history, technology, social organization, religion and mythology, analysis offered as being of the same type as those in linguistics (i.e., in terms of contrastive distinctive features), requiring only that the materials be approached in terms of function, or use (Tax *et al.* 1953:283-7, 278-9, 293-6). At the 1953 'Language in Culture' conference McQuown interjected the question of relations in method into his prepared paper (1954:24-29) and Hockett led off with it (1954:106-111), presenting as one subheading: "IIB. Development of methods for use in one field on the basis of methods already in use in the other" (107). Theoretically, he wrote, there are two sets of methods in linguistics, those that work in language because language is culture, and those that work in language because of the special characteristics which distinguish language from all other phases of culture. Which was which, was not yet known (112). Interest in the topic (phrased by Hockett as "linguistics may have methodological lessons for other phases of ethnography" [110]), was strong in the conference discussions, together with interest in semantic description, then regarded as outside linguistics (and hence, a province of ethnography), and tended at times to take attention away from the prescribed theme (see pp. 162-166, 147-250 of the discussions finely edited by Hoijer).

The papers and discussions thus commingle several kinds of use of linguistics as a method of ethnography: a 'penetrating' role of the most important sort, that of disclosing world view; something of a 'validating' role, by example, if not actual transfer, stimulating search for cultural units as universally valid and meaningful as phonemes and morphemes were taken to be; and, least saliently, a 'foundational' role, signalled in discussion of structural analysis and distinctive features, and of the combination of these that was to be known as 'componential analysis'. Semantics being considered on the side of the rest of culture, rather than of language, and kinship terms being at the core of social anthropology, one had here specific, telling extension of linguistic method beyond language. Almost immediately there were to appear programmatic statements, generalizing, or 'centralizing', linguistic method for the analysis of culture in its entirety. In the next section I shall trace these developments.

VI. LINGUISTICS AS CULTURAL METHODOLOGY

Some of the development to be discussed in this section is part of current controversy. That is perhaps the most important fact about it. Several of the lines of work to be noticed have waned, others are waxing, and part of my task will be to suggest reasons for the differences in fate. From the standpoint of the whole, the point is that since the start of the 1950s, it has not been possible to discuss seriously ethnographic method, and the nature of culture, without

discussing linguistics and the nature of language. Those who reject a foundational role for linguistics are compelled to argue against it. They cannot ignore it.

One sees in the field as a whole the cycle of movement, observed in the work of Boas and Sapir, from a separation to a reintegration of language and culture. In the preceding periods, one could indeed focus on the work of individuals (Boas, Sapir, and to some extent, Whorf). Now it is a question of development along several lines at once. Certain names stand out--e.g., Levi-Strauss, Goodenough--but there are many names, a considerable literature, and influences that are yet spreading.

Phases can be discerned: a burst of programmatic publications, drawing on different streams within structural linguistics; the rise of 'structural anthropology', and 'structuralism', with the work of Levi-Strauss, this having an initial personal footing in the United States in the period in question; the rise of 'ethnoscience', and 'componential analysis', associated with work of Goodenough, Lounsbury, Conklin, Frake and others; a falling away of interest on the part of linguists with the rise of transformational generative grammar. By the mid-sixties there is an attempt at a reconstruction of ethnoscience in the name of transformational generative grammar and considerable anthropological criticism of linguistic approaches in ethnography, and at the same time a growing ethnographic critique of some approaches in linguistics itself. I shall leave these last three matters for the next section (VII).

The work to be discussed comes to a peak in the United States approximately in 1949-1965, and constitutes a certain stage as well. The stage, of course, has continued to be represented by new instances (cf. Deetz 1967, Schneider 1968:32). What gives the stage unity is the question it poses and the answer it seeks. The disparity stated by Voegelin and Harris (1947:593--see V above)--that language and culture differ as to units and methods of study--is taken, not as an observation, but as a challenge. Linguistics is taken as enjoying a kind of structure that anthropology otherwise lacks. One seeks to overcome the disparity by using conceptions of structure drawn from linguistics.

Cultural anthropologists might have felt no challenge (many, indeed, did not), or might have reasoned oppositely, and challenged linguistics to learn in method from cultural anthropology (cf. Hymes 1961:43-45, Vidich in Radin 1966 1933). Cultural anthropology, however, was itself much concerned with pattern and structure (the culture and personality movement [Benedict, Mead and others], the social anthropology of Radcliffe-Brown's adherents at Chicago and elsewhere, the debate in general theory as to the nature of American anthropology's regulative concept, 'culture' [cf. Kluckhohn and Kelly 1945, Kroeber and Kluckhohn 1952]). And, thanks to the teaching

of linguistics in anthropology, many anthropologists verified in their own experience that linguists had ways of dealing with pattern and structure that were more precise, demonstrable, comprehensive, explicit. Linguists might assert this out of pride, but many anthropologists had reason to believe them. A good many anthropologists, to be sure, found linguistics a forbidding mixture of a physical skill (phonetics) and a logical apparatus (phonemics) beyond their acquisition (so they thought). Mostly this attitude enhanced the prestige of linguistic method, and a certain number of anthropologists assimilated it, and put it to use. (A few centers predominate--Yale, Harvard, Chicago, Berkeley--for reasons I cannot fully explain.) Moreover, the two figures dominant in anthropology in the period, Kroeber and Kluckhohn, shared a tradition of scientific humanism that welcomed linguistics for its distinctive rigor. With Cassirer (1961 [1942]:120), they saw the methodological problems of the human sciences as being "particularly clear in linguistics", and shared the expectation of Boas that "the scientific understanding of man will in all likelihood grow from our understanding of language" (in a report of 1939, quoted in Hymes 1954a:9). Their shared interest in linguistics was an important factor in the climate of opinion attending the stage here discussed.

(A) Conceptions of structure

Formulations of the desired conception of structure varied, as did orientations in linguistics itself. All shared (reduced to barest minimum) a focus on UNITS, and their DISTRIBUTIONS. The methodological key is the notion of CONTRASTIVE RELEVANCE. It recurs in the ethnographic applications, and is the essential diacritic of the state (cf. Garvin 1952:217-219; Goodenough 1964 [1951]:188, 1957; Harris 1951:297; Levi-Strauss [Tax et al. 1953:293], Pike 1954; Conklin 1964 [1955]:190, Levi-Strauss 1958:1974, Frake 1964[1961]:195, 1962a:76, 84, 1964 [1961]:199; Bock 1964:220, Conklin 1964:25-26; Hockett 1964:129, Sturtevant 1964:107, Colby 1966:6-10, Deetz 1967:85-90). The gist of the notion, as a specifically ethnographic instrument, is well expressed by Frake (1962a:76):

The basis methodological concept advocated here--the determination of the set of contrasting responses appropriate to a given, culturally valid, eliciting context--should ultimately be applicable to the 'semantic' analysis of any culturally meaningful behavior.

Here in effect is the point on which Sapir had concluded his pioneering article on sound patterning (1949[1925]:45)--"the necessity of getting behind the sense data of any type of expression in order to grasp the intuitively felt and communicated forms which alone give significance to such expression"--developed by structural phonology into a teaching method, and generalized to all of ethnography and

culture. (It was the teaching of structural phonology in anthropology departments that inspired the initial ethnographic movement.)¹⁵

The principle is at once symbolic and functional. It is SYMBOLIC, as against attempts to study cultural phenomena in mechanistic or narrowly 'naturalistic' ways, i.e., by sheer observation and measurement. Physical properties by themselves, although seemingly objective, lead into a deceptive subjectivity and a morass of uncertainly relevant discriminations, whether one's senses or instruments are relied upon. True objectivity lies in discovering the intersubjective objectivity of the symbolic forms, the cultural systems, participated in by those one is studying. The principle is equally FUNCTIONAL, in that it is not objects or units in themselves, but their place in a system of relations, that is fundamental, and the system of relations presupposes the function in terms of which is organized--in the case of structural phonology, the function of differentiating lexical and grammatical form.

The principle, and the results to which it leads, answered to theoretical needs in cultural anthropology. It justified the intuitive recognition that cultural behavior showed regularities not discoverable by quantitative or statistical methods alone, as against a widespread misidentification of science with just such methods (cf. Sapir 1925, 1929, Levi-Strauss 1945, Kluckhohn 1959); and it mediated between concrete data and universal significance. The units of structural phonology, and those sought in ethnography, were concrete universals (although no one called them that): universal, as concepts in a general methodology answering to the nature of all language, and concrete, as elements specifically determined for each language. The relativistic thrust of the 1930s was ending, and indeed, being reversed, with 'one world' concepts coming to the fore (cf. discussion of universals in Kroeber et al. 1953, and Tax et al. 1953; on methodological grounds the prospect of universality was important to Levi-Strauss from the outset [1945, etc.]). The lesson of relativism, however, was not unlearned; Boas' warning against distorting a priori frameworks remained a canon. Moreover, the crucial place of field work in American anthropology made discovery of structure through the handling of new, personally obtained data a requirement for every aspirant to professional recognition. Indeed whereas Levi-Strauss elaborated the structural inspiration of linguistics along ethnological lines, interpreting and comparing data already reported, most American attention was given to the process of field work itself.

Linguists had elaborated named units from the time of Bloomfield's Language (1933), most notably phonemes, morphemes, tagmemes, sememes. From about 1950 anthropologists did the same. There was the general analogy of 'phonemes of culture' (Tax et al. 1953:283-287; cf. Colby 1966:26, wherein Wescott attributes the origin of the analogy to

Kluckhohn's *Mirror for Man* 1949) and individuated concepts such as kinemes (Birdwhistell 1952), behavioremes (Pike 1954), mythemes (Levi-Strauss 1955), gustemes (Levi-Strauss 1958:99-100). None of these terms became a standard part of a productive method. What became general was the underlying principle, christened EMIC by Pike (1954), as distinct from ETIC. (Maybury-Lewis 1965:211-21, 227, shows that it became unnecessary to identify Pike as the source.) The emic term of the distinction has been equated with 'native point of view', purely verbal, and 'good'. All these equations are misleading. As formulated by Pike on the basis of PHONEMIC, the notion does not imply that those whose behavior manifests an emic system are conscious of its nature or can formulate it for the investigator. It is so far from being identical with the verbal that Pike uses the example of a car motor, beside stressing study of nonverbal behavior. Emic analysis is not good, etic analysis bad, but rather, there is an interdependence. Emic analysis is simply analysis of the units and relations of a system, where the functional relevance of the units and relations within the system is validated. It means the difference between discovering the principles that actually underlie the choices of residence of members of a community (Goodenough 1956a, Frake 1962b) and classifying the observed facts of residence according to a preconceived scheme. It is not that a preconceived scheme cannot be adequate, but that its adequacy cannot be taken for granted, and must be demonstrated. Crudely put, the difference is between thinking of description as, or as not, a theoretical task. For some, linguistic and ethnographic description have been tasks of observation, recording, and classification. For those who would adopt the term 'emic', description is the discovery of a theory for a particular case, and in principle predictive of new instances of the language or culture concerned. Etic analysis (following Pike 1954) is of two kinds: (1) the initial framework that gives one a purchase on the system (as in phonetic transcription), and (2) the systematic comparison of the results of emic analyses. Quite obviously, (2) feeds back into (1). (Cf. Sturtevant 1964:101-104, Hymes 1964b:15-21.)

The notion of contrastive relevance (or functional relevance) is basic to 'emic' analysis in ethnography. It implicates more than units as objects of analysis. Equally essential are dimensions, features, and frames (equivalent to domains [Sturtevant 1964]). Units contrast in terms of underlying dimensions, and dimensions are defined in terms of distinctive features. (For example, 'uncle': 'aunt' contrast on the dimension of sex, defined in terms of the features 'male': 'female'.) Distinctive features have been to the forefront in anthropological use of linguistics, because of the initial and continuing influence of Prague School phonological and case-system analysis. That contrastive relevance must be ascertained within a determinate context or frame was clear from the linguistic models of phonological orders and grammatical paradigms. The necessity of determining the validity of the frame itself has been

as important as the question of units and features, for those anthropologists concerned with ethnographic method (cf. the quotation from Frake above, and Conklin 1955:341, 1964:25, Sturtevant 1964:103-105, Hymes 1962a:18-19, 1964d). This concern in fact distinguishes ethnographic method proper from the use of contrast and distinctive features in comparative ethnology and general theory independently of ethnography (see discussion of Levi-Strauss, and of Kluckhohn 1959:247; in Hymes 1964b:15-21, and Sturtevant 1964:103-105).

These interdependent concepts--units, contrasting on dimensions defined by features, within a valid frame--constitute the initial lesson learned by ethnography from linguistics. Formulations varied, as has been mentioned. The particular techniques of American descriptive linguistics played an important part. Thus, Goodenough (1951) used 'complementary distribution' as a way of arriving at contrastive invariance in a cultural system. Romney and others adopted the strategy of much American phonology, requiring an etic transcription as the basis for procedures of emic analysis (but cf. Frake 1961, where the necessity is rejected, but distinctive feature analysis is mistakenly identified with use of an etic starting point). Much discussion was in terms of 'segmenting the stream of behavior', on the analogy of phonological segmentation, rather more perhaps than in terms of paradigmatic contrast as the basis of segmentation (cf. Hymes 1964b:16, and 46,n.5).¹⁶ I have given a generalized, somewhat idealized account, based on the consensus that had emerged by 1964 in the main line of ethnographic work ('ethnoscience').

Strategies and tactics varied too. Sometimes the foundational role of linguistic method was stated in essentially inspirational terms, without specific methodological content. Culture was seen as structured, as was language, but not necessarily homologously structured (thus Goodenough 1951, Levi-Strauss 1958:98; Trager 1959:32). A specifically American interest was elaboration of etic grids, so as not to miss data and distinctions potentially relevant to a domain (cf. Trager 1962:114). This concern has been especially notable in the analysis of kinship. The danger of an etic grid is that one may rely upon it, rather than test it, thus unwittingly perpetuating the sort of imposition of inadequate categories against which linguistic method (emic analysis) was supposed to guard.¹⁷ To avoid mechanical use of etic grids, Metzger and Williams have been concerned to develop a self-correcting discovery procedure, one which arrives at questions and frames warranted as valid within the culture studied (Williams 1966, Siverts 1966-67). This approach combines the generating and validating functions of linguistic method in an explicit way. Others have simply noted a lack of adequate etic grids for most of cultural behavior, and pursued linguistic analysis as the best route to the cognitive structures of a society (Frake 1962)--an example of the penetrating function

(what might be called a 'prelinguistic' approach). It is important to notice that for some anthropologists a foundational use of linguistic method did not entail an empirical use of linguistic data (Levi-Strauss 1955, Bock 1964), even where the cultural system in question (myth, ritual in the two cases cited) finds verbal expression. This is a major difference between 'structural anthropology' and ethnoscience.

At the heart of the ethnographic approach has been the criterion that analyses be PRODUCTIVE, that is, capable of correctly anticipating and interpreting novel instances of the behavior with which they are concerned as appropriate or inappropriate. It is in virtue of this criterion that the use of linguistic method in ethnography is intended to be what I have dubbed 'validating' and 'penetrating', corresponding to what Chomsky has called achievement of 'descriptive adequacy' (1965:27). Goodenough (1957) is commonly taken as the starting point of this 'generative' perspective in ethnography (cf. Frake 1962a:85, 1962b:54, 1964:132, Conklin 1964:25, Sturtevant 1964:100, Hymes 1964a:13, Keesing 1972 [initial paragraph]), although this paper, written in 1954, may be seen as a development of the conception of ethnography argued in his first monograph (1951).

The general perspective is stated cogently by Conklin (1964: 25-26), from whom I quote the first sentences:

An adequate ethnography is here considered to include the culturally significant arrangement of productive statements about the relevant relationships obtaining among locally defined categories and contexts (of objects and events) within a given social matrix. These non-arbitrarily ordered statements should comprise, essentially, a cultural grammar

No common, or indeed very explicit, way of handling the productive statements and relationships that should constitute a cultural grammar emerged (see Sturtevant 1964:123-124, Keesing). Frake's general statement as to contrastive sets, quoted above, occurs in the context, "The BEGINNING of an ethnographic task ..." (my emphasis), but it was this beginning that was most systematically developed. The interrelations of contrastive sets within taxonomic hierarchies, perfect paradigms, and trees (see Conklin 1962, and Kay's comments on Colby 1966) were explored, and some studies of the place of an analyzed institution within a whole culture provided (see especially Frake 1964a, Conklin 1964). The particular methodology was understood as a means to that more general end (cf. Conklin 1964, where the componential analysis proper is relegated to a footnote). There was no clear model for such larger (or deeper) relationships in the linguistics on which the ethnography drew, and none was independently discovered.

Nothing is more revealing of the 'taxonomic' character of the level at which American structural linguistics had so far arrived, indeed, than the projection of this linguistics onto cultural behavior. The one task, beyond establishment of units and features, is conceived consistently as the statement of distribution (Garvin 1952, Pike 1954, Hall 1959, Bock 1964, Hockett 1964, Deetz 1967). A late, flat statement is that of Hockett (1964:130-1):

The ethnographer's report of the inside view should appear as a tabulation of the traits of the community, of the distinctive properties of each, and of their schedulings [= distributions]. There is nothing to be said of a community's way of life that cannot be said in this fashion, and no format can yield greater accuracy and clarity.

Just these three concepts, units, features, and distributions, recur in all the generalized formulations by American linguists, from the first statement (Garvin 1952), through the two major general schemes of Trager, Smith and Joos (see Hall 1959, for 'sets, isolates, patterns'; and Pike 1954: 'manifestation, feature, distribution modes'), to, as quoted, Hockett. (See also Braggar 1966, adapting Hockett's model.)

While distribution was applied within language, and applied also within the rest of culture, it was not, inconsistently enough, applied to the relation of language to culture. Such application was to emerge only later, on a rather different basis, as part of a sociolinguistic critique of the image in 'Herderian linguistics' of one language, one culture unit? (cf. Hymes 1962a, 1964d, 1966, 1973b, c). The relation of language to culture, structurally conceived, was treated in one of three ways: in terms of homologies, parallels, analogies, whether in general form or specific structures (Levi-Strauss 1951, 1953b, 1965; Pike 1960; Bock 1964); in terms of complex relations, allowing for homologies, contradictions, and dialectical relations of transformation (Levi-Strauss 1958:98), or simply leaving the relations as problematic (Goodenough 1951); and by integration, for this is the proper term for the ethnographic semantics that developed, most notably with regard to kinship, where a subject matter central to culture was analyzed in linguistic terms. What at the time of 'linguistics without meaning and culture without words' would have fallen between the two disciplines, and have required a special name, such as 'ethnolinguistics', became equally a part of both, as linguistics enlarged its subject matter, and ethnography its means.

Having characterized the content of this stage, I must now sketch the several lines along which the actual development took place, assessing their outcomes, and evaluating further the development as a whole. The four phases indicated at the outset of this section--initial linguistics-based programs, structural anthropology,

ethnoscience, and the consequences of transformational generative grammar--are interwoven chronologically. I shall separate them somewhat artificially, mentioning chronological interconnections.

The first date pertaining to the present stage is 1945, when Levi-Strauss published his first article taking linguistics as a methodological inspiration. The second date, 1949, belongs also to him in this regard, through publication of his major monograph on social structure, where the example of linguistics, specifically phonology, is acknowledged (1969[1949]:492-493), and presentation of a paper at the International Congress of Americanists in New York City, wherein a search for structural correlations between languages and cultures is developed on the basis of his general perspective on both as manifestations of the activity of the unconscious mind. More will be said of his work below. The same Congress was also the occasion for the first specification of structural linguistic concepts as a potential ethnographic method by Garvin.

(B) Linguistics-based approaches

Garvin--Like Levi-Strauss, to whose work (1945) he refers, Garvin was indebted to the Prague School and the work of Trubetzkoy and Jakobson, the latter of whom, indeed, had much earlier envisioned common cause between linguistics and cultural anthropology (Bogatyrev and Jakobson 1929). Like Levi-Strauss, Garvin spoke of CORRELATIONS. Whereas Emeneau (1950) proposed simply that there did exist some parallels in structure between language and the non-linguistic (in a presidential address to the Linguistic Society of America in 1949), Garvin spoke of an essential parallelism between verbal and non-verbal behavior. The key concept is structure, "stated in terms of relations between units defined in terms of the function of the structure" (217). It is stressed that the parallelism lies in the relevant relations, not the specific contents ('units' [of structure] being distinguished from 'items' [of content]). Garvin singled out, besides units ('actualizable ranges'), the definition of units in terms of distinctive contrast, and distribution (which he preferred to treat as integration of units in units next larger, or of higher level)--in short, the trio of concepts that was to dominate this stage. Features and distribution were illustrated from Garvin's own analysis of the Kutenai language, but not from any analysis of Kutenai culture--presumably because the ethnological source on which he drew did not permit.

In the year of publication of the preceding paper, Garvin published another paper (Garvin and Riesenberg 1952) in which a complementary ethnographic analysis could be provided. He did not follow this with other ethnolinguistic studies. Garvin thus has priority for conceiving an ethnographic method utilizing specific linguistic concepts, but its realization, as integral part of unitary investigation, was to be the work of others.

Uldall--A conception of linguistics as part of a general discipline, subsuming all of culture, was of course not an idea first put forward after the Second World War. De Saussure had conceived of semiology, of which linguistics would be a part, in his lectures posthumously published in 1916, and Cassirer had envisioned a unity of culture through the analysis of the symbolic function in its several forms (1923, 1944). Neither seems to have influenced American anthropology directly. A behaviorist formulation of semiotics by Morris (1946) is indeed reflected in the general outlook of Greenberg (1948) and Lounsbury (1956); but it only apportioned the field of semiotics. Morris did not provide specifications as to cultural method.

One general formulation, linguistically inspired and implemented, did develop in the years before and during the Second World War. It came from the version of structural linguistics formulated in Copenhagen by Louis Hjelmslev and Hans Uldall, glossematics, and was brought to print by Uldall. Uldall argued the case for a common methodology for the humanities (the human, or cultural, sciences), and exemplified a glossematic approach in terms of the analysis of text. The monograph was delayed, not appearing until 1957, as only one of two intended parts (cf. its preface, and Uldall and Shipley 1966:6-8).

Uldall had had contact with American anthropology, through field work out of Berkeley in the 1930s, and illustrated some of his points in the monograph with a California Indian language, Maidu. Glossematics itself was taught at successive Linguistic Institutes at Indiana University by Hjelmslev (1952) and Uldall (1953). It had been the subject of a presidential address to the Linguistic Society of America (Haugen 1951), and Hjelmslev's Prolegomena appeared in English translation in 1953, being reviewed at length (Garvin 1954). Both leading textbooks of the decade borrowed from its terminology (Gleason 1955, Hockett 1958) without acknowledgement,¹⁸ and Uldall's monograph did stimulate at least one ethnographer who read it (Maner Thorpe, whose excellent Korean ethnographic study remains unpublished); but both the linguistics and the general program remained marginal in the United States. The American currents were already in motion on the basis of other modes of linguistic statement, and the novel glossematic terminology and methodology seemed at best equivalent. Glossematics remained a purely linguistic school, neither having nor acquiring anthropological practitioners. With the deaths of Uldall and Hjelmslev, it now seems a finished chapter.

The great impact in the early fifties came from two sources in American linguistics, George Trager (and his associates), and Kenneth Pike. Behind both is the figure of Sapir. Pike dedicated his book (1954-60) to Sapir, reproducing a photograph taken by himself as frontispiece. In the Sapir Festschrift article that he later cited

as a starting point for paralinguistics (1958:1), Trager used Sapir's 'epoch-making' "Sound Patterns in Language" (1925) as a precedent for extension of 'configurational' linguistics to prosodic phenomena. In collecting "Four articles on metalinguistics" by Sapir's informal student Whorf, he chose the one from the Sapir Festschrift and others on Sapir's theme of the importance of linguistics as a model for sciences (cf. Whorf 1956a in Hymes 1964f, 136). (The theme of the special importance of linguistics was more widely shared [Boas, quoted above, Hockett 1948b, reflecting Bloomfield, and others].)

1949 appears to be a year in which the initial impetus toward connecting language with culture, in terms of linguistic structure, gathered and broke on every side. Besides Levi-Strauss's monograph and paper, Garvin's paper, Emeneau's presidential address, and Kluckhohn's reflection of linguistics in his Mirror for Man, there was a paper by Zellig Harris, also at the Americanist Congress (1952), and the publication of Trager's first general program for the field of linguistics. (Cf. also Olmsted 1950.) At the time a unity of content (Whorf) rather than of method dominates anthropological attention, as we have seen. But within four or five years, the thrust of the discussion at the major anthropological conferences, discussed in V, was to be matched by specific programs and a fair amount of implementation. The most extensive empirical implementation was to be associated with Trager, the greater conceptual success with Pike.

Trager--In 1949 Trager defined the field of linguistics (in a pamphlet of that title) as having three parts: 'pre-linguistics' (physical and biological bases) on one side, 'micro-linguistics'--the usual analysis of language--in the center, and 'meta-linguistics' beyond, concerned with the 'relations between language and any of the other cultural systems' (1949:7). In the same year he had Whorf's four papers republished. These anticipations were quickly followed by attention to 'metalinguistic phonology', 'metalinguistic morphology' (Trager and Smith 1951:81-88), and style; a mimeographed "An outline of metalinguistic analysis" (Smith 1952) extended the analysis of vocal phenomena. Birdwhistell took up gesture and body motion, under the name KINESICS, and a general, language-centered approach to all of culture, under the aegis of communication, was developed by Trager and Hall (Hall and Trager 1953, Trager and Hall 1954), communication itself being placed in a larger setting called 'symbolics'. In Hall's best known book (1959) he included a ten-by-ten typology of the compartments of culture, developed with Trager, and the trio of descriptive concepts mentioned above (set, isolate, pattern), as general to culture. Trager himself later elaborated a scheme of 3 cultural processes (setting, content, functioning), which, successively applied, yield 9 foci, 27 fields, and 81 systems of culture (Trager 1962).

Trager's own purpose would seem to have had two main parts, one generally foundational, the other the development of etic grids as 'generating' devices. Fundamental is the assumption of structure:

If language can be structurally analyzed, and if the rest of culture has been largely conditioned by language, then all of culture must be structurally analyzable also

The realization of this view is seen as vindications of Whorf, but specific structural parallels are not required:

... this does not mean that parts of culture other than language are necessarily to be analyzed homologously or even analogously with language, but they can be analyzed in their own terms... and we can compare them for points of relationship (Trager 1959:32).

As to method,

I have for some years been concerned with the analysis of culture, with the goal of finding a guiding methodology that would ensure getting all the data (Trager 1962:114, citing Hall and Trager 1953).

In the mid-fifties the group associated with Trager and Smith (Bloch, Hill, Joos, McQuown and others) was central to linguistics in the United States, and its extensions into other communicative modalities attracted considerable attention. We should note here the Georgetown Round Table papers by Hall, Hockett, and LaBarre (all 1960). Today it is moribund. The explanation would seem to be that the linguistic base was lost, and the anthropological offshoots failed to take root.

The attention to semantics and language varieties was somewhat marginal, Joos (1958) making an interesting finding as to relations among lexical meanings, and proposing (1959, 1962) a suggestive conception of English styles, as the leading contributions of the group to the growing interest in the two subjects. The strength and promise of the approach, for anthropology, and other fields as well, especially psychiatry, was the work on communicative codes interwoven with language in social interaction. After a promising start, the work on paralinguistics, kinesics and the like bogged down. It contributed to pioneering work of Alan Lomax in ethnomusicology, and helped give rise to Hall's suggestive work on spatial and other nonvocal relationships in communication ('proxemics') (1959, 1963). A good deal of insight and documentation accumulated (see discussions in Sebeok, Hayes, Bateson 1964 by Birdwhistell, Hall, McQuown, Mead and others, and Hockett 1960). Much of it remained unpublished, however, including the initial study (known in manuscript as "The

natural history of an interview"). The one major publication (Pittenger, Hockett, Danehy 1960) showed the intricacy possible to the subject but not how to establish social interpretations or to develop an ethnographically usable method. To a considerable extent, the work was prisoner of its analog of the phonetic grid and discovery of phonemes by segmentation and classification. It specified and classified etic factors, yet failed to develop a workable transition to emic analysis. It lavished precision on dissection of corpora in the laboratory and left ethnographic interpretation informal. Communicative forms and their functions never quite united in an adequate way. While acknowledging Sapir as its forefather (both Birdwhistell and Hall did so explicitly), the work did not absorb his lesson, the principle Levi-Strauss shared with the Prague School, of seizing upon the function, or purpose, that informs and organizes structure and observed detail.

Similarly, the general approach to culture remained, in Hall's and Trager's hands, a classification, becoming neither a theory nor a usable method. To say that culture has 100 (Hall 1959) or 81 (Trager 1962) divisions, deductively, is about as useful as insisting that all languages have eight parts of speech, and goes against the concern of ethnographers to validate analysis in terms proper to the culture studied.

Some of those attracted to the work in what might be loosely called 'circumlinguistic' analysis drifted away, or were pulled back into questions within linguistics proper by the rise of transformational generative grammar from 1957 onwards, and an adequate cadre of researchers did not arise. This approach most of all felt the impact of Chomsky's work, establishing, as it did, new problems within the heart of linguists from other aspects of communication. The initial expansion outward from linguistics had presumed that the linguistic foundations were secure. The confidence centered in 1951 around Trager and Smith (1951), and Harris (1951), as having provided the necessary tools for language structure proper, such that new domains could be explored--that confidence vanished. Trager and others did not meet the challenge to reconstruct a linguistic basis adequate to the new conception of linguistic structure itself. Their linguistics became isolated, and 'circumlinguistic' work drifted

Pike--Pike's linguistically-based general program began to appear almost simultaneously with that of Trager and Hall (1954-1960). Pike argued the necessity of a unified methodology, one capable of 'moving without jar' between the verbal and nonverbal aspects of unitary cultural behavior (e.g., conduct in which verbal and nonverbal elements may substitute for each other within the same frame.) As has been noted, Pike coined the widely adopted terms 'emic' and 'etic' (from 'phonemic' and 'phonetic'), and developed a trimodal conception of structure (manifestation, feature, and distribution). He linked

his general approach to a new linguistic concept, appropriating a term used by Bloomfield, tagmeme. (The concept unites a contrastive set with a position in which it occurs, thus integrating the paradigmatic and syntagmatic axes of grammatical structure.) Where the Trager approach had conceived of parts of culture as something like separate slices of a cake, and its 'circumlinguistic work' rather like tracing the marbling within a single slice, Pike maintained no view of culture as a whole, but for the analysis of specific behavior argued the necessity of multiple perspectives ('particle, wave, field'--see Pike's 1956 article in Hymes 1964f), and developed analyses of integral events and behavioral sequences. He introduced the notion of 'behavioreme' as a possible basic unit of culture, and informally analyzed a breakfast, a church service, and a football game to illustrate his approach.

Where the Trager *et al.* approach became isolated from general linguistic practice and anthropological thought, the linguistic practice introduced by Pike in these volumes ('tagmemics') became the mode of description of many languages, through the work of missionary linguists of the Summer Institute of Linguistics, of whose training Pike was in large part in charge. (The initial 'power base' of Trager and Smith, in the Foreign Service Institute of the State Department, had ended with the advent of the Eisenhower administration in January 1953.) For those concerned with what was to be called the 'ethnoscience' approach, the first chapters of Part I (1954) of Pike's book were exciting. Not only did he articulate the same attitude and give it a name ('emic analysis'), but also he stood apart from the antipathy to meaning and intuition characteristic of the then dominant behaviorist outlook. Pike insisted on theoretical recognition of what, through his own extensive field work, he knew practice to require--use of meaning, native reaction, recognition of the substantive (identificational) as well as contrastive role of units, interplay of structural levels. He was the first to renew actively the methodological outlook of Sapir, then under a cloud.

Notions analogous to those of Pike were used effectively by K. French (1955), Mayers (1959) and Kemnitzer (1975) in studies of ritual behavior. Bock (1964) adapted Pike to study of roles and social structure, and Hymes (1958) and Dundes (1962) borrowed from him in study of folk tale and myth (though see Sturtevant 1964:103 on the latter). There has been no major development of Pike's approach, however, only piecemeal utilization. Pike himself has devoted most of his energies subsequently to developing tagmemics as a specifically linguistic approach, and to related analytic devices (such as matrices) and materials (such as texts, approached in terms of rhetorical analysis). The promised concluding chapter of his major work (1960, ch. 17) is a disappointment. The initial promise had been an open-ended exploration of cultural behavior, leading to examples of integrated analysis. Whether because of diversion

of energies due to the unanticipated challenge of Chomsky's linguistic theory, or other responsibilities, the work concludes by falling back upon language and society as separate, parallel structures, the very approach that the opening chapters had sought to transcend (cf. Hymes 1969).

To sum up this part: the linguists discussed here envisioned a general analysis of culture, but failed to sustain it. They stimulated anthropologists and others, but did not transcend the practice of linguistics proper, neither becoming nor training ethnographers. The anthropological component necessary to analysis of culture was left to anthropologists to supply.

(C) Anthropologically-based approaches

Levi-Strauss and 'Structural anthropology'.--Levi-Strauss, as we have seen, was first of anthropologists after the Second World War to see and use the implications of structural linguistics for the study of culture. As did all others then, he appealed to the example of phonology, then the major accomplishment and theoretical focus. He was indebted particularly to the Prague school, especially Roman Jakobson (1945, 1969[1949]:xxvi). (For early reorganization of the significance to anthropology of linguistics-based structuralism, see Sebeok 1946.)

From the beginning, Levi-Strauss was concerned with more than particular empirical parallels between language and culture, although he regarded each finding as itself an empirical problem (cf. 1958 [1953]:90, and his reply to the criticism that he reduced society to language [1958:93-110]). He explicitly sought an underlying unity. If phonology can attain objective realities, consisting of systems of relations, produced by the unconscious activity of the mind, can an identical method lead to the same results for other types of social phenomena? If so, can we admit that diverse forms of social life are in nature substantially the same--systems of conduct that are projections, on the plane of conscious, socialized thought, of universal laws regulating the unconscious activity of the mind? (1958[1951]:67; cf. 1968[1949b]:492-3, 1958[1945]:62). For this foundational role of linguistics, rooted in the unconscious, Levi-Strauss had first cited Trubetzkoy as exemplar (1958[1945]:40), in linguistics proper; for the anthropological application, he came to cite Boas as originator (1958[1949a]:26, 1950:xxx, 1953a:526-527). To this underlying warrant of structure, Levi-Strauss adds a second, 'external' and 'functional' foundation: the various structures in society (kinship and marriage, economics, language) can be treated as types of communication, as diverse modes of laws of exchange (1958[1949b]:69; 1949c; 1958[1953]:326-327).

Of all those contributing to the subject, Levi-Strauss provided the most profound conception of an integration of linguistic method in anthropology, combining a sense of the history of the disciplines, of their relation to other disciplines, of the need as well as the possibility of new methods, of the need as well the possibility of theoretical justification and empirical demonstration. On the most general question, he saw at once that linguistics presents anthropology with not just an opportunity, but an obligation. Anthropological theory must account for language. There cannot be no relation between language and culture, nor can there be a total correlation (if there were, it would have been already recognized). The task is to discover the aspects and levels of the actual relationships (1958 [1953b]:90-91; cf. 1958:98). At the Conference of anthropologists and linguists (Bloomington, 1952), Levi-Strauss put directly and concretely the answer to the crux stated by Voegelin and Harris, that the methods and units of linguistics and ethnology were in general distinct. He discussed Whorf in terms of an attempt at correlations between language and culture (cf. Whorf 1956a, 1941b), correlations which do not always carry conviction. Is not the reason that Whorf's methodological requirements for culture are less exacting than those for language? The error is to compare results of a methodic, quite abstract analysis of language with ethnographic observations that are merely empirical, or the result of an arbitrary segmentation of social reality (1958[1953b]:84, 1958:97; cf. Olmsted 1950:7-8).

Levi-Strauss suggests examples of what is wanted, proposing first that among the Hopi, Zuni and Acoma of the American Southwest, mythology and kinship are parallel in form within each society (and mutually in contrast between societies); linguistic correlations might also be found. As a more complex example of how anthropologists must proceed, in order to meet linguists on common ground, he elaborates part of his Americanist Congress paper (1958[1951]). There he had compared the languages and kinship systems of several areas of the world as to structure (simple/complex) and elements (few/many). Here he contrasts the Indo-European and Sino-Tibetan areas, concluding that in the former the social structure is simple, the elements numerous and complex, while in the latter, the reverse is the case. Once social structure is thus analyzed, can one not engage in dialogue with the linguist? (1958[1953b]:87-90).

Levi-Strauss is a great figure in anthropology, but not because of the attempts to bring together the results of linguistics and social anthropology. The suggestions he put forward were not taken up, except in a critical vein (Olmsted and Moore 1952) or with reservations (Kluckhohn 1961b). His most recent direct analogy between linguistic and cultural systems (1958:99-100, 1965) met with reservations (Sturtevant 1964:103) and outright rejection, both as to the empirical implications and the theoretical basis of the analogy (Shankmann 1969). Much of the American response to

Levi-Strauss stems from a difference in fundamental outlook (Scholte 1966), reflecting different intellectual histories in the United States and on the continent, but it is not the criticism alone that requires explanation. It is the success as well.

Levi-Strauss' work in social structure (1949 is the major contribution) has become a central reference point and created something of a school. His analyses of totemism (1962) and of 'primitive' thought (1962) have restored to prominence what were central concerns of anthropology in the first part of the century, and together are THE anthropological interpretation of the human mind for our day. His work on myth (1955, 1960, and the series of four volumes, Mythologiques) has revived materials and a subject also long neglected by social anthropologists, and has inspired a host of related studies by others. In all these respects Levi-Strauss has stimulated interest in linguistics among anthropologists, by his own reference to its example,¹⁹ and, in his work on totemism and myth, by combining the logic of binary oppositions with ethnographic detail to shed considerable light on folk taxonomies, a subject central to ethnographic semantics.²⁰

Other anthropologists have not taken up the specific linguistic analogies drawn by Levi-Strauss, and criticism of them (e.g., Shankman 1969) is in fact justified. The example set by Levi-Strauss in the analysis of social structure, folk taxonomy, and myth, has been taken up, because he has shown how to obtain new understanding of materials that were (in the case of social structure) or could now become (in the case of taxonomies and myth) integral parts of the development of social anthropology itself. He enlarged the scope of structural understanding in the first case, and showed its possibility in the second. Other anthropologists did not need to share Levi-Strauss' personal history of inspiration by structural phonology, of faith in the unified activity of the human mind, to be challenged by, and profit from, his substantive work. It was possible for them to test his insights in materials of their own.

A related case is instructive. Kluckhohn, like Levi-Strauss, was stimulated by Jakobson's conception of distinctive features. He was indeed stimulated by Levi-Strauss as well, returning from the 'Anthropology Today' conference of 1952 with the report that Levi-Strauss was the one person who had had something new to say (David Schneider, personal communication). In a manner somewhat like that of Levi-Strauss, Kluckhohn sought to achieve results like those of linguistics in another cultural domain, values, rather than social structure. His three papers (1956b, 1958a, 1958b) and a chapter on Greek values in a posthumously published set of lectures (1961a) were built upon after his death only by a student, Benjamin Colby, and that only temporarily. The reason for the neglect, I think, is that the connection between the formal structure and the content to be

illuminated appeared arbitrary and remote. Kinship and marriage, totemic classifications, myths were palpable anthropological subject-matter; Levi-Strauss explained something already there. Value-emphases were nebulous; the distinctive features seemed to create, but not to explain, an object (cf. Levi-Strauss 1962:327-31).

Levi-Strauss did much to shape a climate of opinion receptive to linguistics, but did not much shape the form linguistic method took in ethnography itself. After the initial neglect of his ideas, and lack of sympathy, there came a dramatic turn, so that by 1958, or 1960 certainly, a commonality of interest between his work and that of ethnoscience was mutually recognized. As has been shown, however, Levi-Strauss dealt with linguistic concepts at the level of ethnological comparison and general theory, rather than of ethnographic discovery. In interpreting specific cases, he used the general notion of qualitative structure, often in revealing ways. The logic of analysis did not in fact depend upon linguistic formats, nor even upon linguistically analyzed data. (He maintained that myth structure was independent of its verbal form--against this, cf. Hymes 1981.) The relationships arrived at might in principle have been found without either. Levi-Strauss thus stimulated ethnographers to seek evidence of structures of the sort he analyzed, but assigned linguistics no clear role in the search. Explicit integration of linguistics in ethnography was primarily a development indigenous to the United States.

"Ethnoscience".--The term "ethnoscience" is not entirely happy (cf. Sturtevant 1964, Williams 1966), but has come to be conventionally assigned to the work to be discussed here. In origin it is a generalization of such terms as "ethnobotany", "ethnozoology", etc., and indicates the goal of analyzing indigenous systems of classification and knowledge. "Ethno" indicates the concern with analysis of cultural systems in their own terms; "science" refers ambiguously both to the goals of the work itself, and to the assumption that native cultural systems are not imperfect versions of Western science, but products of human rationality and experience in their own right (cf. Levi-Strauss 1962 on the universality of the "savage" mind).

The essence of the approach, as it developed in this stage, has been characterized in large part at the outset of this section, and a major survey is available (Sturtevant 1964; cf. also Colby 1966, and comments thereto). Here I shall sketch the history of the approach, before relating it (and structural anthropology) to the emergence of transformational generative grammar.

The main contributors to the approach have generally considered Goodenough (1951, 1957) its starting point. Goodenough had in fact come to see linguistics as a model for cultural anthropology while a graduate student at Yale in 1940. Problems raised in his work in social psychology as to the concept of self found an answer in insights

gained from the principles of linguistic analysis taught that year at Yale by Trager (personal communication from Goodenough; Trager was not himself discussing cultural analysis). Lounsbury independently worked along similar lines, regarding kinship (recall the discussions at the Anthropology Today symposium Tax *et al.* 1953), and Conklin (1955) published a pioneer article, based on field work in the Philippines that had begun at the end of the Second World War.

The new approach first gained considerable public attention with the publication of articles on kinship semantics by Goodenough and Lounsbury in the issue of Language dedicated to Kroeber (1956). In the same year David French (1956) published a pioneering sketch of a general approach. Kinship remained the exclusive focus of Lounsbury's work (1964a, 1964b, 1965), and the principal concern of many (cf. studies in Romney and D'Andrade 1964, Hammel 1965, and references therein). Frake (1961, 1962a, 1962b, 1964a, 1964b) extended the scope of analysis to other cultural domains, and articulated the goals and principles of the approach. Meanwhile Metzger and Williams (1963a, 1963b, 1966) developed explicit methods in relation to several cultural domains (see also Black and Metzger 1965). The years 1964-1965 comprise a crest of the initial impetus, reflected in several volumes (Goodenough 1964, Romney and D'Andrade 1964, Gumperz and Hymes 1964, Nader 1965, Hammel 1965).

The growth of the 'ethnoscience' approach, like that of structural anthropology, is due to three concurrent factors. First, the fate of the work has not been tied to the fate of a particular formulation of linguistic method. More must be said about this point immediately below, but it is generally true that the two successful anthropological approaches have worked with concepts GENERALIZED from linguistics, and have done so in ways responsive to the nature of the particular materials to be analyzed. Particular linguistic formulations have indeed colored particular pieces of work, but the central notion of contrastive relevance has transcended such differences, and proven an enduring basis of analysis. Granted this notion, and the related notions of domain, dimension, feature, unit, and relations among such, changed linguistic formulations can be absorbed.

Second, the two successful approaches have addressed themselves to problems important to anthropology, and have become part of sustained developments within anthropology. They have not remained programmatic proposals and classifications. In this respect, it has been generally true that anthropologists have absorbed linguistics, but that linguists have not absorbed anthropology. There are indeed linguists who are good ethnographers, but it is a plain fact that none of the general programs originating in a particular linguistic school have found ethnographic realization. (This need not always be true. One may yet see departments of linguistics hire anthropologists, as departments of anthropology now hire linguists; but a change in the usual conception of the basis of linguistic description

will be required.)

Third, the two approaches have in fact been phases of larger movement of thought, of the general growth of structural and formal analysis in this century, and of the return of interest in cognition in psychology and other fields since the early fifties. The leading figures in the two approaches have not at all restricted themselves to linguistics as a source of method and insight. Levi-Strauss was early interested in communication theory (1951, 1953a; cf. Davenport 1964:216, who stresses the significance of this). Goodenough has developed applications of Guttman scaling to social structure; Conklin has employed graph theory; Hammel, Metzger, Romney and others have explored the mathematics appropriate to computer simulation and processing; various adaptations of algebra have been employed in semantic analysis itself (cf. Romney and D'Andrade 1964; Hammel 1965, e.g. Lamb 1965; and Buchler and Selby 1968, ch. 9). A long-run view may well find that linguistics has played a transitional role in the development of a full-fledged formal methodology in anthropology. Because it studies material of interest and familiarity, and because it is in close proximity within a shared tradition, linguistics may well be an indispensable efficient cause of formalization within anthropology; but the full story of the trend toward explicitness, precision, and mathematical models will not reduce to its role. (The textbook by Buchler and Selby [1968] is an indication of this general prospect; cf. Kay 1971, Keesing 1975:400).

VII

The story that I have tried to sketch is almost complete--from a signalling of the centrality of language and an unconscious unity of methodological outlook early in this century to the establishment of methods of analysis inspired by linguistics as significant movements in anthropology. Yet there remain strands of the story that require comment--criticisms which, if correct, would belie the apparent success of the development so far sketched, on the one hand, and criticisms which, on the other hand, point to further advances. I shall consider the role of transformational generative grammar first, then take up critiques on anthropological grounds, and other critiques from within the movement toward a linguistic method of ethnography itself.

Transformational generative grammar

Early in the preceding section it was noted that the question of 'cultural grammars'--of statements of the relations among the different structural domains--had not been resolved. The rise of transformational generative grammar posed this question in sharp form. The challenge posed by this approach to conventional structural linguistics was transferred to ethnography.

Broadly speaking, the conception of linguistic structure on which the first stage of modern linguistic ethnography was based worked well enough in the discovery of phonemic and morphemic units, and of some semantic properties. Contrast and co-occurrence, segmentation and classification, enabled one to recognize relevant sets of units, together with their positions and privileges of occurrence within sentences. Where it first clearly was seen to fail was in handling systematic relations BETWEEN sentences. It depended on the occurring order of relations among elements. If the goal of structural linguistics is defined as explicating the relation between sound and meaning (as was commonly held by structuralists), then the relations obtained by classification and distributional statements do not exhaust the relations that are relevant.

Lyons (1968:247) gets to the heart of the matter. There are "deeper connections" between sentences which "cut across the surface grammar", and that "have much to do with how we speak and understand", though "still largely unexplored, in any systematic way, by grammarians" (Lyons is quoting Hockett 1958). To deal with these connections, one must recognize, in addition to the surface grammar observable in occurring sentences, one or more levels of deep grammar, and seek to understand the complex, but systematic ways in which they are related. (The terms "deep" and "surface", which have become general in linguistics, are due to Hockett.) Lyons defines TRANSFORMATIONAL relationships as these 'deeper connections', and generalizes the concept of transformational grammar to comprise "any grammar that claims to assign to each sentence that it generates (i.e., accounts for) both a deep-structure and a surface-structure analysis and systematically to relate the two analyses" (1968:248).

Recognition of transformational relationships has brought radical reformulation of structural linguistics, and change within the initial reformulation itself. For present purposes, some informal examples of what is involved will serve. In

- (a) Kennedy forced Khrushchev to withdraw the missiles, and
- (b) Khrushchev was forced by Kennedy to withdraw the missiles,

the subjects and objects of the two sentences are exactly opposite, so far as surface structure is concerned; yet, underlying the surface relationships, Kennedy and Khrushchev have the same relationship in both ('logical subject' and 'logical object' in traditional terminology). Here different surface structures manifest a single underlying relationship. Conversely, a single surface structure may manifest different underlying relationships. Thus,

- (c) Kicking horses can be dangerous

may be equivalent either to

- (d) It can be dangerous to kick horses, or to
- (e) Horses that kick can be dangerous.

That is, horses is the surface subject, but may be either 'object' or 'subject' in underlying relationship. And of course, as with the first example,

- (f) It can be dangerous to kick horses
- (g) To kick horses can be dangerous
- (h) Kicking horses can be dangerous

have all the same underlying relationship between horses and kick. Finally, the relationships of a single surface element and position can be quite complex. In

- (i) What made Eric mad was to be dismissed as naive,

Eric is at once object of made mad, object of dismissed, and subject of the predication be naive (cf. Chomsky 1965:70).

The logic here is essentially the same as that used by Sapir (1925) to show that an implicit level of phonology exists behind the observed level of phonetics: differing sets of sounds may have the same underlying pattern, and the same sets of sounds may have differing underlying patterns. Recognition of this logic brought to the fore a new functional relation. CONTRASTIVE RELEVANCE could warrant the status, and disclose something of the organization of elements; TRANSFORMATIONS (or operations of equivalent role) were required to give an adequate account of their organization.

It is important to note that the level of deep structure, which is entailed by transformational analysis, is not simply an abstraction from the particularities of surface structure. The deep structure of a system may bear no obvious resemblance to its surface structure, being much more different in form than the examples given above suggest. It is equally important to note that transformations do not merely handle distributional relationships of individual elements (e.g., assigning kick its proper place in (f-g-h) above). Transformations operate on the underlying structural relationships themselves, and, as the name implies, can change one structure into another.

One could say that the first lesson learned from linguistics by ethnography has had to do with units and their distributions, and the second with levels and their relations. This second lesson can indeed be taken to mark a new phase in the development of a linguistic method of ethnography. That it marks a new path to success after previous failure, however, as has been claimed (Werner 1966, Durbin 1966; see especially Vidbeck and Pia 1966:71), is not the case.

Within linguistics there could be a sharp sense of antithesis between the goals of transformational generative grammar and the descriptive linguistics that immediately preceded it. Even this antithesis was far from complete, and concerned the particular 'neo-Bloomfieldian' behaviorism that was temporarily dominant after the Second World War. The 'mentalism' of the new approach was not in any way in conflict with the outlooks of Pike, students of Sapir, and Jakobson and those following him. (Anthropologists like Goodenough, indeed, heartily welcomed the Chomskian critique of behaviorism.)²¹ The goal of accounting for productivity, the ability of users of languages to produce and interpret an indefinitely large number of novel sentences on the basis of a finite system of rules, was made central to linguistics by Chomsky (cf. Hymes 1964b:30, n.9). In cultural anthropology Goodenough (1957) had independently stated the goal of ethnography in such a way as to make the generative goal of linguistics a special case (cf. Conklin 1962a:119, Frake 1962a:85, Sturtevant 1964:99, Hymes 1964b:30-31). The goal of accounting for the underlying connections among specific structures (Durbin 1966) was in view from the outset (cf. Goodenough 1951), (Keesing 1970). The goal of relating internal knowledge to plans and strategies for its use (Werner 1966:44, Vidbeck and Pia 1966) was already a part of ethnoscience (cf. Goodenough 1956a, Frake 1962b, Wallace 1961a, 1965b), as it was already a part of cognitive psychology (with which the ethnoscience approach grew up in close connection--see Romney and D'Andrade 1964, Parts IV, V). The goal of analyzing a level of underlying (deep) structure, one possibly quite different from occurring structures, was present from the start in the work of Levi-Strauss (cf. 1958:364, 1962:328), as both a lesson from linguistics and a goal for anthropology. He expresses it most tellingly perhaps in a famous paragraph in response to criticism (1960b:52):

He [the critic] claims to be a structuralist, he even claims to defend structuralism against my reckless manner of handling it. But he is still a structuralist in Radcliffe-Brown's terms, namely, he believes the structure to lie at the level of empirical reality, and to be a part of it. Therefore, when he is presented a structural model which departs from empirical reality, he feels cheated in some devious way. To him, social structure is like a kind of jig-saw puzzle, and everything is achieved when one has discovered how the pieces fit together. But, if the pieces have been arbitrarily cut, there is no structure at all. On the other hand, if, as is sometimes done, the pieces were automatically modified by a cam-shaft, the structure of the puzzle exists, not at the empirical level (since there are many ways of recognizing the pieces which fit together: its key lies in the mathematical formula expressing the shape of the cams and their speed of rotation; something very remote from the puzzle as it appears to the player, although it 'explains' the puzzle in the one and only intelligible way.

The linguistic critique of this view, indeed, would be that the empirical level of structure is seemingly abandoned, whereas a goal of transformational generative grammar is to account for the systematic relations between the two levels.

Some linguists and anthropologists indeed did not allow for deep structure in their formulations of cultural analysis (Hockett 1964, Bock 1964) but that is not the case with the main body of ethnoscientific work, a point recognized recently by Buchler and Selby (1968): "We...tried to indicate our agreement with Goodenough (1956b) that we are more interested in strategies and motives, and underlying structures, than in topological descriptions." They there succinctly christen the two points just discussed as ground rules and sets of strategies, the two jointly defining the goal of analysis.

The technical handling of relations between deep and surface structure has itself undergone major revision within linguistics (cf. Chomsky 1965), and there are a number of distinct approaches, both within and without the transformational school. Use of generative rules was introduced into analysis of kinship ('componential analysis') early on by Lounsbury (1964b, but presented at the annual meeting of the American Anthropological Association in Philadelphia in 1961). Levi-Strauss had dealt with transformations between systems (1956, 1958:98, 365-6), and in his work in mythology has developed a concept of transformation quite analogous to that of recent grammar. Indeed, the underlying structure is taken as like a Boolean algebra, just as in Chomsky's syntax (Chomsky 1965; Levi-Strauss 1960). Some experiments have been made in representing social interaction by transformational rules of Chomsky's type, but not published. In general, there are a variety of formal models available in linguistics for representing the relations between structures; linguistics itself is exploring new models drawn from logic; and the choice of model for a particular cultural system must be empirically validated. Moreover, the adequacy of transformational grammar as a basis for ethnography must itself be critically assessed (cf. Hymes 1964b:24-33, 1972, 1973, 1974).

The contributions of Werner (1966) and Durbin (1966) do not mark a new advance. Werner reduces the scope of ethnoscience to explicit verbal statements (43), indeed, to a subset of a subset of grammatical sentences (44), conceiving a hierarchy such that not all grammatical sentences are semantically interpretable, and not all semantically interpretable sentences culturally appropriate, these last being the object of study. The difficulty is that not all culturally appropriate statements are in fact grammatical, given the limitations of the usual grammars; the relation of non-equivalence works both ways. And to restrict ethnoscience to verbal propositions is to mistake a route for a destination. Speech is an indispensable

and often privileged means of access to native knowledge and cultural systems, but has never been considered the exclusive means or goal (cf. Frake 1962a:75-6 [= 1968:436], Sturtevant 1964:107, Keesing 1966:23). Werner, moreover, proposes to assimilate the pragmatic component ('know-how') of cultural competence to entries in a dictionary, accepting a suggestion that most of cultural competence (the knowledge and ability to use it of members of a culture) is simply a list (43)! The complexities of structure already in view (cf. Wallace 1961, Frake 1964c, Conklin 1964, Wallace 1965b) are ignored, for what, from a generative view of grammar, is a very 'taxonomic' view of culture. (Chomsky has used 'taxonomic' to designate linguistic approaches confined to analysis of surface structure.)

In short, the goals of generative grammar and ethnoscience are largely shared (a point recognized by Göhring 1967:808); the new techniques of generative statement have begun to be utilized, and such novel technical suggestions as Werner and Durbin actually make have been absorbed within the general development of structural anthropology and ethnoscience (cf. references to both in Buchler and Selby 1968). The attempt at defining a new stage was inspired by the enthusiasm within linguistics proper and the sense of revolution there, but made in relative ignorance of the anthropological context. The example indicates again the difficulty, if not impossibility, of basing an ethnographic approach on the specific form of a linguistic model.

If the critics were wrong in thinking to hold the goals of generative grammar up against those of ethnoscience, they were right in saying that progress toward the goals had been but partial. If they were wrong in overlooking how much needed yet to be done in analysis just of 'surface structure' systems, they were right in saying that relations between systems, and to a deeper structure, remained obscure. Much of the reason might fairly have been found in the very small number of anthropologists engaged in such research, and more might have been made of the progress within the particular areas, especially kinship, where attention was concentrated. More serious, however, than these criticisms on linguistic grounds are the critiques on anthropological grounds. Some are misguided, but some are well taken, and together they may significantly affect the participation in the approaches by ethnographers, a factor which we have seen to be crucial.

Anthropological critiques

Harris.--Some critics misunderstand the work in question. Most important of these is Marvin Harris. His standpoint is that of 'cultural materialism' (Harris 1968). It must be added that the standpoint is closer to what may technically be called mechanical materialism than it is to the Marx whom Levi-Strauss (1958:110,

366, 369; 1962:325) and Sartre (1963) acknowledge as a point of departure for anthropology. For Harris, a scientific method must be objective and a scientific theory deterministic. To work through language is to rely on what people say, therefore to be subjective and indeterministic. Taking over the 'emic': 'etic' distinction, and equating 'emic' with the verbal, Harris argues for the necessity of an 'etic', i.e., nonverbal approach. Only an observational analysis, wholly external to the subject, can be objective.

Harris is right as to the need for development of observational description, and devotes commendable energy to it, building on the work of Barker and Wright. The disagreement arises from the inadequacy, IN PRINCIPLE, of an observational method. Lacking any way of determining contrastive relevance, whether by interview, learning the system partially oneself, or testing in other ways, the observational method cannot discover qualitatively discrete units. To establish significant units, to pass from a mass of observed details to some sort of structure, it must fall back on a criterion of quantitative cumulation. In one or another way, frequency becomes decisive for the threshold dividing the significant from the non-significant. Such a criterion cannot distinguish what is rare, or unobserved, from what is systemically impossible. It cannot tell when two observationally identical occurrences are systemically distinct, and two observationally different occurrences systemically the same. (Recall the syntactic examples given earlier, and the development from phonetics to phonology [on which cf. Lamb 1964:75].) In short, such an approach cannot at all recognize, let alone make explicit and precise, what are elementary properties of cultural behavior.

There is a special irony in equating an 'emic' approach with the verbal, subjective, and, because subject to conscious intent, indeterministic. The origin of the term in 'phonemic' should itself show the equation to be false. Phonology is a celebrated case of unconscious patterning. Users of a language cannot normally articulate their phonological system, and lack terminology with which to do so. Their answers to questions provide indispensable evidence of their system, not the system itself. As defined by Harris, 'emic' analysis is a method that phonemics could not itself employ. (The example shows also the error in equating 'emic' with analysis of terminology.)

In short, an ethnography requires observation, but the purely observational approach advocated by Harris (1962) is one that has not absorbed long-standing lessons of structural linguistics, indeed, that rebels against the main tradition of American anthropology (cf. Sapir 1949[1927]:546-547).

Harris has recently (1968) granted emic analysis a place, when the problem is to crack the code employed by native communicators.

That is all that those who advocate it propose. Harris persists, however, in defining 'emic' categories by what he terms their "logico-empirical relationship to cognitive processes", rather than by the use to which they are put. The result is to wholly confuse the relationship of emic to etic analysis, throwing what are moments, or steps, of a single research strategy into opposition on the basis of an ontological dichotomy. Because it deals with semantic phenomena, Kroeber's early (1909) list of dimensions recurrent in kinship terminologies is taken to be 'emic', whereas it has clearly the status of a contribution to an etic grid. Types of kinship relation, like types of speech sound, may have reference to (potential) ideas and logical relationships, as Harris says, but their emic status depends on their being shown to be parts of a given system. Harris is led to consider such etic grids 'emic', when used comparatively, but this could be true only if one were willing to assume them to be parts of a system in a generalized mind of mankind (or of God). There are, to be sure, structural relations among emic systems, as Levi-Strauss has been at pains to show (e.g., 1958: 98 within societies, 265-6 between societies; on distinctive features in this respect, cf. Hymes 1964b:15); but these again are the result of analysis, not an input to it.

Emic analysis is criticized for lack of attention to predictability, which Harris associates with probability: "An ethnography carried out according to etic principles is thus a corpus of probabilistic predictions about the behavior of classes of people. Predictive failures in that corpus require the reformulation of the probabilities of the description as a whole." The criterion of predictability, in the sense of anticipation and interpretation of newly occurring cultural behavior, has been part of the ethnoscientific approach from the outset, as we have seen. Analyses are indeed revised in the light of failure in this regard (as is of course a daily occurrence in linguistic analysis). Indeed, continuing feedback, self-correction of this sort is intrinsic to the ethnographic practice of Goodenough, Metzger and Williams, etc.

Quantitative relationships have not been common in linguistic and ethnoscientific work, but have been employed by Romney and D'Andrade, Wallace, and others, to test validity. Labov (1966) has employed quantitative relationships to discover social structuring hitherto invisible, relating phonological variation to social mobility, social position and situational context. Harris is right to criticize any who would exclude quantitative and probabilistic relationships--the members of a culture have themselves a knowledge of them--but he perhaps misses the point as to their role. To predict what will happen is not the same thing as to explain it. The same level of predictive success may be obtainable for several different analyses. To choose among them, one of course seeks predictions or cases covered by one, not the others; often the generality, consistency, or other theoretical fit of one analysis is the

basis for preferring it. Often, indeed, the fact itself of possible alternative analyses is what is of interest (Wallace 1961b). In all these respects, it is not predictiveness alone, but the way in which the analysis explains the structure underlying the predicted behavior, that counts.

Harris raises questions about the social reference of an emic analysis, the degree of sharing of structures, situational and personal variation, and the like. Much of what he says should indeed be counted as criticism of some studies, in terms of their own goals; it is part of a necessary ethnographic critique of some conventional linguistic assumptions. Harris misses, however, the attention already given to these matters within ethnoscience, and the fact that the range of possible variation, situational and personal, and indeed, of change, is a function of the available structures (cf. Wallace 1961a, 1965a). Linguistic models themselves incorporate ways of dealing with the selection of alternatives according to situations. It is not the case, as Harris would seem to think, that variation can be expressed only statistically. Such instances as a Trukese father reversing the respect due his daughter-in-law do not invalidate a structural analysis of status relationships. They show the presence of transformational, as well as contrastive, relations. An ethnography must deal with culturally appropriate ways to insult, flatter, etc., as well as to show normal respect. The reported comment, "A good hard jolt was just what she deserved", shows the cultural appropriateness of the transformational result in the Trukese case that Harris singles out. Variation often requires statistics, but they must be integrated with the kinds of structural relations that linguists dub 'context-sensitive rules', 'marked and unmarked categories', and transformational (here, concerned with operations on the relations between behaviors, persons, situations).

Harris charges that "In the new ethnography [= ethnoscience] ... culture is a timeless system of logical categories. Hegel's historicism, his only redeeming feature, has been dropped in favor of a synchronic idealist dialect known as distinctive feature analysis." It is fair to criticize any who separate structural analysis from history and from the motives and needs without which cognitive structures would not exist, or function. The need to study developmental process has been urged within ethnoscience (Brown 1964:251-222). The general point is the same here as with regard to variation. History and psychodynamics are in important part functions of, changes in and uses of, cultural structures. It is through structural analysis that one can discover what DOES change, and what is organized for personal meaning and expression (for an example with regard to myth structure, cf. Hymes 1968a; Barth 1966:33, Wolf 1964:44-5, 1967:459 who integrate structure in larger interactional and historical processes).

Harris submits "that it is only with respect to statistically insignificant or scientifically trivial performances that behavior stream events can be predicted from a small set of emic rules". He is right to stress the emergent properties of interaction among persons, properties which go beyond what can be said to be part of the cognitive structures of the individuals. It is possible, and necessary, to study personal strategies as well as cultural ground rules. No one would maintain, however, that the number of relevant rules is small, or that discovery of their interrelations is easy (cf. Sapir 1949[1929]:165). Again, the path lies through, not around, the analysis of structure. And it might be thought no small thing to explain, on 'emic' terms, so much of the working of man's most essential tool, speech, to lay bare much of what enables men to adapt to and transcend situations.

Harris takes it as significant that other sciences had not had to search out subjective worlds of form, which is hardly an argument, if one's subject matter, culture, is such a world. As a counter-illustration, he discussed the Bathonga, noting density and spacing of the animal and human populations, under technico-environmental conditions of southern Mozambique. The example brings to the fore some essential factors of a cultural analysis. Of course Harris is right that emic rules must confront etic reality. Some ethnoscience analyses have done so, showing for example cultural rules underlying the spacing of a human population (Frake 1962b). Harris' discussion of the Bathonga leaves the two (emic rules, etic reality) separate. Material conditions and constraints are left unrelated to purposes and patterns of behavior of the Bathonga. A charge of 'idealism' and 'timeless system' often implies irrelevance to human problems and social change. The etic analysis sketched by Harris appears irrelevant, except from the standpoint of a disinterested observer or a totalitarian control precisely designed not to take indigenous ways and desires into account. Harris asserts that the superiority of the etic approach is that it cannot fail to make discoveries about history. If men make history, it cannot succeed (cf. Diamond 1964: 40-1, 45).

Harris has provided the most extended, sustained criticism of the ethnoscience approach. He articulates views that are probably widely held (cf. those to be immediately mentioned). He has come to grant the possibility of an emic approach, but sets it against, and subordinate to, its opposite. For ethnoscience, the two approaches are interdependent, and 'emic analysis' is not an option, but an indispensable means.

Hammer.--Harris' mistaken equation of 'emic' with overt verbalization is repeated by Hammer (1966:370). Hammer accepts formal analysis, contrasting it with 'empirical analysis', which deals with "what people say and do, where they live, how many of them there are,

what parts of their environment they use and how they use it" (371). Formal analysis, by contrast, deals with systems, such as grammars, and semantic domains, in such a way that only an internal, formal criterion of evaluation is possible. Explanation requires both to be joined, but no suggestion is made as to how the joining might be done.

Hammer misconceives the two writers she discusses (Chomsky, Wallace), basing a 1966 article on Chomsky's (1957) study, and being apparently ignorant of his discussions of just the problem of the empirical nature of linguistics. She in fact mistakes his disavowal of accounting for the processes of actual speaking and hearing as a disavowal of account for anything actually internal to users of a language, thus ignoring what Chomsky takes to be the purpose of the entire analysis (Chomsky 1965, and many other writings). She also mistakenly equates completeness, as a matter of enumerating the contents of a language, with the technical use of finiteness in transformational generative grammar, and is unaware of the studies of change on a transformational basis (for whose absence she reproaches Chomsky). That such errors could be reprinted in a book on theory in cultural anthropology, as a principal part of the book's attention to language, is disheartening.

Hammer does recognize real problems when she stresses the difference between the structuring of reality by a "native" and of data by an analyst; the need to study change; and the empirical factors which need to be united with formal analyses for full explanation of a system. What she does not recognize are the efforts to test for cognitive validity (Conklin 1955, Frake 1961, Wallace 1961, 1965a, 1965b, Romney and D'Andrade 1964, Sandy 1968). Her essay leaves an impression of two disjunct approaches. In point of fact, many of those pursuing formal analysis at the same time seek to integrate it with "empirical analysis" as defined by Hammer (cf. Frake 1962b, Goodenough 1956). What is not clear is that there is a corresponding effort by "empirical analysts."

Burling.--The problem of the "psychological reality" of structural analyses has been raised by Burling (1964), who stresses the vast number of theoretically possible analyses for a given set. Burling, like Hammer, omits to mention the many ways in which ethnographic and psychological research deal with the problem (cf. Buchler and Selby 205-207).

Berreman.--Berreman (1966) has attacked the new approach as a threat to the scope that anthropology must have. Insofar as there is any tendency to make a cult of the approach, such an objection is fair. And it is perhaps legitimate fun to single out methodological studies, such as that of Metzger and Williams on "firewood" (1966), as if they symbolized a narrowness and irrelevance intrinsic to the approach.

It is a strange irony, however, when the critique is made in the name of a call for an anthropology that is both rigorous and scientific, insightful and humanistic, verifiable and perceptive (Berreman 1966: 347). That call is a restatement of the reasons for which Boas, Sapir, Kroeber, Kluckhohn and others encouraged linguistic methods within anthropology, and why Goodenough, Conklin, Frake, Lounsbury and others have developed them.

Vidich.--A similar attack is made by Vidich (1966), as if to seek rigor through language and linguistics was incompatible with humanistic insight, although the men he attacks, Kluckhohn and Levi-Strauss, have been major spokesmen for an anthropological humanism. Vidich betrays the same sense of necessary opposition when he interprets recent work in sociolinguistics (Gumperz and Hymes 1964) as a 'retreat' from a scientific linguistics to readdress oneself to communication and understanding (1966: lxxxii, n.56, cxi, n.73), whereas 'scientific' linguistics is an indispensable part of the program in question.

These are criticisms of what linguistic method in ethnography tries to do. They mostly misunderstand its nature, and the necessity for it. While they call attention to real difficulties and problems that are not resolved, they perpetuate a division between study of language and study of culture. They do this, indeed, on possibly conflicting grounds, charging a linguistic approach variously with being unscientific and with being 'scientific'. Some of the criticisms (e.g., Burling, Hammer) in fact attach to ANY cultural analysis, not just to those conducted under the aegis of linguistics and ethnoscience. The relation between an analysis and reality is a GENERAL problem, and it is curious that an approach that explicitly faces it is criticized, as if other approaches did not face the problem at all, as if failure to examine it meant it did not exist.

Schneider.--Such critiques are at best cautionary, not serious theoretical ventures, so long as they show no way of incorporating what is already known as true about linguistic and related structures in a more comprehensive framework. A telling criticism is one that shows the approach not to satisfy its own accepted criteria. Schneider (1965, 1968) has made just such a critique of some of the work in componential analysis of kinship. Schneider himself makes some use of linguistic concepts (1968:32, lll, n.3). Schneider understands the point of an intensional analysis of kin terms as contrastive sets, defined by a finite set of implicit features. His criticism is that some leading analyses of American kinship have taken for granted, rather than validated, the domain of terms analyzed. From his own ethnographic research, Schneider finds that 'relative' may mean either a person related at all, or a person significantly related, when an informant answers questions containing the term. Moreover, the terminology used in referring to kin is not necessarily either identical with, or privileged with regard to, terminology

used in addressing kin or introducing them. In short, Schneider finds that the domain to be analyzed has not been properly validated.

Schneider's point has been made on a formal basis by Tyler (1969). Having shown the relation of parentage (P) to be the one genuine primitive relation in the usual kinship typologies, such that "The a priori etic grid utilized in the genealogical method presupposes P", Tyler concludes that it is just on this basis (but not necessarily any other) that Lounsbury is right in interpreting kinship terminologies as extensions from a core set of terms for the immediate family relationships. Tyler suggests the need to transcend the grid so defined, and himself has earlier indicated how to do so (1966).

The essential thing here is the danger, ever present, of substituting a mechanical for a dialectical use of 'etic grids', or analytic frames of reference. (Cf. Levi-Strauss 1962:325-326 on analytical and dialectical reason, the latter the exploratory, adapting arm of the former; and Conklin 1955 for an ethnographic example.)

One tendency noted above is to criticize a new method in terms that would invalidate old methods too--this is one of the risks a new method, especially a new method claiming novel rigor, must run. A subtler risk is a failure to carry valid criticism through, out of respect or hesitation before an established discipline. In the present case, this danger may be summed up in the phrase, "It may be good linguistics, but it's bad anthropology." Some anthropologists make such statements. But bad anthropology cannot be good linguistics--that at least is a fundamental assumption of this study. In the particular case of ethnographic semantics, if an analysis is false on anthropological grounds, because of error in defining its domain, no linguistic excellence can save it. It is just as much the goal of linguistics as of ethnography to analyze correctly the tacit knowledge of participants in a culture. The impossibility of a separation as to goals in this respect is part of what I have meant by referring to ethnographic semantics as having resolved the relation of linguistics to anthropology, language to culture, by integration. Linguistics has the greater concern to place semantic analysis in the context of grammar, anthropology to place it in the context of culture, but adequacy of analysis is the concern of both.

Speaking

In section IV it was said that three considerations determined the outcome of the history recounted here: reconsideration of the nature of the cultural on structural terms, and the extension of linguistic analysis into semantics and into social interaction. The first two have come about, and their development so far has been our focus. The third development has also begun, although less advanced. It is a prime example of the need for a constructive anthropological critique of linguistics, as opposed to passive acceptance or

rejection (cf. ch. 3).

The absence of semantic description from linguistics and anthropology in the years just after the Second World War could not but be noticed. It has been present before, and, for some approaches, still was. First mediating between linguistics and ethnography, semantics became integrated into both. The fundamental conception of language, of the object of linguistic description, as linking sound and meaning, prevailed, and at the same time, remained unchanged. What was not much noticed was the absence of description of the use of language. Structural linguistics, indeed, had taken as its charter the separation of language, as structure, from speech, as variation, and this charter had not been successfully amended. Discussions of the relation between language and culture were reduced in practice to discussions of the normal practice of the two disciplines of linguistics and anthropology, and the patterning of speech, the structure of speech acts, was not normally described by either.

To trace the history of this third consideration would be to start a new story, the development of stylistics and sociolinguistics. Three points can be made here. First, the premise of the work in question is that speech is itself structured. In this respect, the ETHNOGRAPHY OF SPEAKING includes ethnographic semantics, as an extension of the notion of structure radiating out from linguistics (cf. Chapters 1, 2, 3). Second, the goal is to discover underlying knowledge, as in contemporary linguistics, but the term employed in linguistics for this, COMPETENCE, is redefined. Whereas it had been restricted to grammatical knowledge (Chomsky 1965), it is extended to the gamut of knowledge, and the abilities as well, that underlie speaking, and more generally, communicative competence in the normal sense of the word. Third, the sociolinguistic, or ethnography-of-speaking, approach departs sharply from some of the methodological assumptions of ordinary linguistics. In place of assuming homogeneity of knowledge and use, organized solely around the function of reference, the new approach assumes heterogeneity, and a plurality of functions, including the expressive. It takes as its starting point, not a single language, from which connections to social life may later be sought, but a community's speech economy, of which a particular language is but one component, together with other varieties of language, and modes of communication, within a system of scenes, participants, channels, and acts and genres of speech. When this change of starting point is systematically pursued, it leads to a general critique of long-standing assumptions as to the relation of language to culture. Many of the misgivings and doubts expressed by critics of a linguistic approach are taken into account, by a reconstitution of the linguistic approach itself. In particular, phenomena of variation and use are taken into account (cf. Sankoff, 1974, 1980).

In brief, one may say that the carrying-out of this third line of development, that of social interaction, leads to complementing a linguistic method of ethnography by an ETHNOGRAPHIC METHOD OF LINGUISTICS (cf. Labov 1966, Hymes 1974a, Chapter 3, and 1980a). It provides for an integration of linguistics and ethnography in the study of the structure of speech, including explication of texts as to both verbal form and context of performance. In doing so, the new approach transcends the basis for an integration of language and culture possible previously. In the initial proposals of Levi-Strauss, and typically in anthropological thinking, the question of a relation between language and culture has been taken as a question of correlations between separate analyses. The guarantee, or promise, of correlation was the unconscious activity of the human mind. Such an approach takes the traditional "departments" of human life for granted (just as did Cassirer 1923). In the new approach the definition of "departments" (religion, art, language, etc.) in a culture is taken as itself a problem (cf. Frake 1964a); a unity is sought in the integration of speech in social action; and the ethnographic method itself is required to guarantee the structure and relations, so that conscious and purposeful activity need not be set apart (contrast Levi-Strauss 1950:xxx). It is thus in principle possible for the new approach to deal with social reality as practical activity, and to conceive and rationally understand the coincidence of changing of circumstances and of human activity as revolutionary practice (Marx 1968:660-664).

From this vantage point, the assumptions of both Marvin Harris and Levi-Strauss can be seen as complementary products of a certain stage in the human sciences. The two are united in a desire to discover structure in human life, to achieve a determinate, scientific status for the human sciences. In reaction against idealism, one approach saw any reference to factors internal to man as concessions to subjectivity and indeterminism. It saw structure as the product of forces external to man, to individuals, independent of their wills. Men as diverse as Lenin and Leslie White shared this view. In seeking to account for intuitively known structures, a second approach discounted external factors (sometimes, as with Cassirer 1923, ignoring them altogether), seeing structure as the product of the human mind. Yet it too placed structure beyond the intervention of human consciousness or will, either in principle (Boas, the early Sapir, Levi-Strauss) or by speaking of MAN in abstraction from historical MEN (Cassirer). BOTH approaches make a science of man and conscious control of human history incompatible. Each approach perpetuates the other, because of what it neglects, the first perhaps more so than the second. The structuring of social life is a product of the interaction of men and their circumstances, of the human mind and human ecology, so to speak. (Here is the crux to which Hammer validly calls attention.) There is no longer even tactical motivation for denying that some of the determinants of human action and social structure are within

human beings--that thesis was perhaps understandable, as an ideological necessity, in a period when the social sciences had to define their autonomy from biology and psychology, as well as from philosophy and religion. Our present understanding of biology and ethnology, permits a NATURALISTIC understanding of human nature that is neither 'materialistic' nor 'idealistic' in the simple terms in which the two have been felt to be opposed. Likewise, it cannot be satisfactory for long to pursue a science of man that perhaps reintegrates culture with nature, contemplatively (Levi-Strauss 1962:320), and satisfies the scientific thirst for uniformity and generality, but that has nothing to say to man's present prospect and social change (Wolf 1968; 1964:96).

The practice of linguistic ethnography can participate in transcending this opposition. It finds structure in the activity of the human mind, conscious as well as unconscious. It does not justify structure by some one source of data (verbal or nonverbal) and a monopolistic epistemological claim. It does not postulate structure as end in itself, but as indispensable means. If it places structure at the center of its work, that is because it believes structure--meaningful structure--to be at the center of human life. If the genesis, maintenance, and change of social and cultural order--their acquisition and meaningfulness in personal life--are to be explained, the nature of the order must be known. Those who honor Marx should grant no less. True, some stop short of dynamics and change, from the side of structural analysis, just as some fall short of structure in the study of its conditions. For the tradition singled out as central to this study, from Boas and Sapir through Goodenough, Conklin, Wallace, Frake and others, either is a fault, not a goal.

I speak here of promise, as much as, or more than, of accomplishment. If LINGUISTIC ETHNOGRAPHY may serve to designate the dual focus of a 'linguistic method of ethnography', comprised in an 'ethnographic method of linguistics', then the prospect that it holds out for the next stage in the study of the relation of language to culture, in the relation of linguistics to anthropology, may be indicated in two diagrams, which together summarize the discussion just above. The realization of the prospect would be in large part a realization of the final outlook of Sapir.

I. RELATION OF LANGUAGE TO CULTURE

- (a) Language opposed to culture (Voegelin and Harris, 1947, M. Harris, 1962)
- (b) Language parallel to culture (Levi-Strauss, others)
- (c) Language integral to culture (ethnographic semantics, ethnography of speaking)

II. WARRANT FOR STRUCTURE

- (a) external (economic, ecological, behavioral observation, etc.)
- (b) internal (unconscious activity of human mind)
- (c) ethnographic practice

EPILOGUE

With so many false starts and unfulfilled beginnings; with specifically linguistic methodology incorporated within logical and mathematical methods generally; with the initial inspiration of linguistics to 'structural anthropology' ending with the analogies between language and culture unproven, and language as necessary integument to myth renounced; with realization that there is needed an ethnographic critique, as well as use, of linguistics in 'ethnoscience'; much of the subject of this study must seem a local episode, based on the enthusiasm of a linguistics flushed with discovery and success, and the anxiety and parochialism of an anthropology, uncertain of its proper domain, and needing a familiar field, language, to mediate to it the movement of our century toward qualitative, mathematical analysis in all fields.

In two respects, however, the question raised by the development we have traced are part of the permanent agenda of anthropology, and indeed, of the human sciences. This is so because:

(1) There are structures and meanings accessible only through language and the methods of linguistics, either as language itself, or as adjacent to, or interwoven with, it. There is much yet to be discovered here, as to kinds of structures and meanings, not just as to determination of known kinds in new milieux.

(2) The theory of culture (or, of cultural order) must account for language. Some nonverbal forms show linguistic kinds of structure (e.g., Nubian body painting, Buriat Mongol ongon representations), others apparently do not. The common basis of all codes in the human mind conduces to unity of structure. Differences in the means, both within the body (vocal facial, manual, etc.) and without, and differences in purpose and use, dictate diversity. If the central mode of human experience is communication, the nature of the repertoire of modes of communication, and the meanings of their use, as modes, is central to an understanding of men, generally and in given groups. Work directed to this second problem might be termed the ethnography, and the ethnology, of symbolic forms (honoring in the terms Cassirer as the great expositor of such a vision).

The future of these matters depends upon linguistic ethnographers. It is not only that language and linguistics are indispensable to ethnography, for the reasons dubbed 'facilitating', 'generating',

'validating', and 'penetrating'. Study of semantics and of speaking raise questions of analysis, and of mode of statement, that lead to integration of ethnographic and linguistic formats. Such work can extend the scope of linguistic analysis beyond the particular boundaries of a given time, and lead to recasting of linguistic analysis in the light of what is found.²³ It is only by this kind of INTEGRATION, not by ad hoc analogies and comparison of separately arrived at products, that the true relation of language to culture, the true place of language in social life, can be discerned.

Such integration points to disciplines of linguistics and ethnography whose adjacent sectors are almost indistinguishable. So far, the training of ethnographers in linguistics has proven indispensable. It may indeed be the best way to introduce formal rigor, joined with empirical insight, in anthropological training. Certainly it is necessary if an adequate theory of symbolic forms, and of culture, is to be attained. There are, to be sure, capable ethnographically oriented linguists, and linguistics as a discipline may come to recognize a need for ethnographic training, if the empirical foundations of its own work are to be secure. There is hardly a cadre of such people now. For the present, as in the past, the key is the teaching of linguistics in anthropology, the training of linguistic ethnographers.

Chapter 4

FOOTNOTES

- * This chapter is revised from "Linguistic method in ethnography: Its development in the United States," in Method and Theory in Linguistics, ed. Paul L. Garvin, pp. 249-325. I am grateful to Garvin for inviting me to the conference at 1966 Linguistic Institute at which the paper was presented, and for his courtesies and patience as editor. Harold Conklin made valuable comments on the first draft, and Bob Scholte and Joan Davlin made possible its completion. I dedicate the study to Carl Voegelin, who appointed me a linguist when I came to Indiana University to study anthropology and folklore.
- 1 Note the blunt restriction of "linguistic anthropology" stated by Mason (1881:397): "The mere acquisition of a language (i.e., learning of it for fieldwork), or even the accurate study of its phonology, its etymology, and its syntax, is not a part of anthropology. Linguistic anthropology has reference, first to the origin and life history of languages as a whole, and, second, to the comparative study of the languages of the globe as a means of grouping its peoples." (I owe this quotation to Regna Darnell.)
- 2 American anthropologists seem to have been innocent of the methods of comparative philology and historical linguistics, until Sapir and the students he trained. Comparative philology might be appealed to as an example, but I know no instance of its method being applied outside its own domain. Radin's chastisement of Boas for using comparative philology as representative of ethnological methods of securing time perspective, and the statement of Boas he quotes, show only that neither understood the subject (Radin 1933:14-15); "the comparison of static phenomena combined with the study of their distribution" hardly describes linguistic reconstruction. Sapir's precision in delineating methods of ethnological reconstruction no doubt owes something to his linguistic training, as Radin (1933:55, 60) maintains. Significant influence in this period, however, was the other way. The pioneering areal-typological studies of linguistic traits by Dixon and Kroeber, and Boas' later arguments, against Sapir and others, as to the nature of language classification, invoke a methodological perspective developed in ethnology (cf. Hymes 1961d, n. 25 [=1964a:764, n.21]).

- 3 Cox (1883:8) expresses an appeal to the authority, and the foundation, of comparative philology:

"The work before us is therefore one strictly of comparison and analysis, and the conditions under which this work must be done are laid down by the Science of Comparative Mythology, the science which compares the stories, as the science of comparative philology compares the speech, of tribes or nations which exhibit any tokens of affinity. The science proposes, in short, to examine popular traditions generally, beginning with the only basis which may be surely trusted, the basis of language."

But by basis of language he appears to mean the results of philology, especially as they bear on the names of figures in myths (cf. 8ff.). The mode of explanation involves the unlikelihood of chance of the resemblances, and normal processes of change after separation of a once common group, but again, this reflects a shared perspective rather than diffusion of a technical method.

- 4 Notice that Sapir already had the notion of form as defining the status of elements in terms of their relations to one another. He here applies it to culture, but his development of it in phonology (1921, 1925) is thus not surprising. What is unknown is its source in his thinking in 1912. As we shall see, as of 1921, the notion became assimilated to notions of the aesthetic derived from Croce.
- 5 One must realize that it was Sapir, and other students of Boas, who developed an American method of historical ethnology, and who did so at about this time (First World War and after). Boas had used historical methods more as a means of analysis and criticism than as a means of reconstruction.
- 6 Professor A. Momigliano points out to me that the English translation of Croce is a wretched one, and that Croce himself cannot be considered a liberal in the period in question. My concern of course must be with what Sapir made of Croce. Let me here thank Prof. Momigliano and Prof. E. Gombrich for the opportunity to discuss Sapir in a seminar at the Warburg Institute, London, in the spring of 1969.
- 7 From this discussion it should be clear how misleading is any attempt to erect a fundamental opposition between Sapir and Whorf on these points (as in Landar 1966).

- 8 Harris (1951:315) suggests that Sapir's formulations in the study of personality "have hardly been understood or used by any professionals, because they are so incisive and lead so readily to social criticism."

- 9 Cf. Sapir's critique of fragmentary sciences of man (1949 [1939]: 578-589), and the first published statement of this perspective, in which he says that society is often spoken of as if it were a static structure defined by tradition, but in the more intimate sense ("intimate" here has suggestions of depth of structure as well as of the personally meaningful) is a highly intricate network of partial or complete understandings between the members of organizational units of every degree of size and complexity. Though apparently a static sum of social institutions, it actually is reanimated or creatively reaffirmed from day to day by particular acts of a communicative nature among the individuals participating in it (1949 [1931]: 104).

- 10 The Levi-Strauss paper was indeed noted in another journal (under 'Miscellaneous Papers') by a linguist sensitive to European structuralism (Sebeok 1946). His remarks on 'structuralism' were vague, interpreting it in terms of the difference between mere description of a simple combination of isolated elements and study of the unified system in its totality, without reference to any specific feature of linguistic method. Structuralism is simply said to be a scientific approach long in use in geometry, physics, biology, and psychology, but among the social sciences, first of all in linguistics. (That would have disagreeably surprised Marx). But the note's prophecy as to the role of linguistics was presciently correct: 'Its diffusion has begun' (49). And the observations on linguistics in relation to anthropology are revealing of a tension felt at the time (48):

'When this part of the study of linguistics [the structuralism] is not known or not appreciated by ethnologists, the possible contributions which linguistics may make to general anthropology are lost,--and nothing is gained by a denial of a constituent discipline within a larger field. Anthropology may be regarded as a kind of holding company having actual constituents A, B, C, D...Now it sometimes happens that a member of A makes disparaging remarks about C, or excludes C, or regrets that the members of C have not contributed to A, or the company as a whole, in any significant way. For a rather unhappy example of this, see Ralph Linton, The Science of Man in the World Crisis (New York, 1945), pp. 7-8.)'

- 11 An interesting, though jaundiced, view of the background can be gained from Radin (1933, chs, 4-5).

- 12 The immediately preceding background in linguistics, beginning about 1949, is discussed in VI following.
- 13 Kroeber and Kluckhohn knew enough of linguistics not to find satisfactory the straightforward equation of phonemes with culture traits in a leading text (Gillin and Gillin 1948: 154-5): If one looks at a culture as a total functioning system, one may break it down into its simplest units, which are called traits (citing Linton 1936). A trait is either a single custom or a custom plus equipment. At all events, it is a unit which cannot be further broken down from a functional point of view...In language the unit sounds, such as long a, the nasal n, or soft th, are culture traits, called by the technical name phonemes. In football, the act of kicking the ball is a culture trait; the single blast of the referee's whistle is another; and so on." True, phonemes (or distinctive features) are culture traits; but the common sense of it conceals all the disparities of method and systemic context that were at issue.
- 14 Cf. at about the same time LaBarre (1960: 75), who citing Kroeber and Kluckhohn 1952: 124, quoted just above), decried its literalism, saying 'I do not think we should mis-spend a moment's time trying to discover cultural 'phonemes', and suggesting that the proper analogy was that linguistics could be for the social sciences what mathematics is for the physical sciences (74).
- 15 The accomplishment of structural phonology was also a brilliant realization of the perspective and program for the cultural sciences of Cassirer (1923--cf. 47-48, 51-52 of the 1953 introduction by Hempel, and 79, 91-93, 155, 159 of the text).
- 16 Much American linguistic work at the time was expressed in behaviorist terms, discouraging adequate attention to function and meaning, and native speaker responses. Much of it attempted to restrict attention to overt features of a corpus. The inability of such an approach to justify its own practice has generally been conceded. The approach belied the "mentalistic" perspective that made the initial, as well as later, achievements of structural linguistics possible. (Transformational generative grammar is here taken as an extension of structural linguistics.)
- 17 On etic grids, and other points discussed here, cf. discussion of Colby 1966 by Kay and Keesing. Schneider's criticism of some componential analysis on the score of etic grid (1965, 1968) is discussed in VII below.

- 18 The crosscutting distinctions, expression and content, and form and substance, as the design of language, in Gleason; "cenematic" and "plerematic" as general types of unit in Hockett.
- 19 Thus, Davenport (1963:216) writes: "It was the fusion of the formal method of linguistics with the French *Année Sociologique* school, founded by Durkheim, that has yielded this new and influential approach to social structure." Cf. Orenstein 1965.
- 20 Levi-Strauss pointed out neglect of the problem posed "by the concrete attitude of a culture toward its language" (1958 [1953b]:77-8), but the problem came to the fore only with the growth of sociolinguistics (cf. Hymes 1966:124, 158).
- 21 In his analysis of Iroquois kinship, presented orally at the Vth International Congress of Anthropological and Ethnological Sciences, Philadelphia 1956, and in lectures at Harvard and Yale that spring, Lounsbury set as his aim to explain how an Iroquois (here, specifically a Seneca) could apply an implicit knowledge of his kinship terminology in designating any and all possible kin correctly. By extending analysis to all possible kin, rather than limiting it to the most salient cases, as had commonly been the case in anthropological analyses of kinship type, Lounsbury showed earlier interpretations of Iroquois kinship to have been mistaken. The two modes of social classification kinship terminology and clan membership, had been regarded as equivalent. When analysis of terminology beyond immediate kin was carried through, the two classifications were found to be in fact different (cf. the published account, Lounsbury 1964: 1079, n.4). Lounsbury treated kinship terms as a set of intensively, not extensively, defined categories, capable of productive use in new situations.
- 22 Indeed, transformational generative grammar appears a revolutionary change more against the partial background of American behaviorism, than in the larger context of Sapir, Pike, Jakobson and others. In the long run it may well appear an extension of structuralism, defined by discovery of the functional principle necessary for adequate analysis of syntax, and by the recasting of structural phonology consequent on the working out of the principle. See Hymes 1975b and Hymes and Fought 1975, 1981. Keesing (1972, 1975) overreacts to the temporary ascendancy of Chomsky's overgeneralized and ideological attack on all preceding linguistics, and perhaps reacts as well to the literal, rather than narrow interpretation of some American descriptivist statements in the work of a few anthropological colleagues. His reaction really has little bearing on the approach he explicitly discusses, that of Goodenough, as shown. The essential

foundations of the earlier approach can never be 'pulled down' (Keesing 1975: 295), partly because Chomsky engaged in important part in pulling down straw pillars of his own erecting, but most importantly because the principle of contrast within a frame is fundamental to all linguistics, and to as much of cultural behavior as involves situated human choice.

- 23 Cf. Lefebvre (1966:331): "Analysis of language thus will allow one to tighten analysis of praxis. Conversely, analysis of praxis, by clarifying a function of language -- the situational function -- permits one to tighten analysis of language." The point holds for the gamut of functions of language in speaking. The standpoint of the ethnography of speaking is that the functions of speech in a community have PRIORITY (are constitutive of what is to be analyzed), are PROBLEMATIC (must be discovered and validated in the particular ethnographic situation), and, of course, PLURAL (so that a language is organized in terms of stylistic [including expressive], as well as 'referential' units and relations). Cf. now the perspective of Bourdieu (1972) and Silverstein (1976, 1977). And in relation to current focus on the openness of interaction and negotiated meaning, cf. the new of Sapir summarized in n. 9 above.

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ALFRED LOUIS KROEBER: LINGUISTIC ANTHROPOLOGIST

Alfred Louis Kroeber died in Paris in the early morning of Wednesday, 5 October 1960, in his eighty-fifth year. He was a member of the Linguistic Society of America from its beginning, a Signer of the Call that led to its founding, and its president in 1940.

Kroeber was born in Hoboken, New Jersey on 11 June 1876, the son of Florence and Johanna Mueller Kroeber. His first language was German. He and his younger brother, Edward, began their education with three years of tutoring from the principal of the Ethical Culture School, then were sent to a college preparatory school (Sachs), and later to a boarding school in Connecticut. Alfred Kroeber entered Columbia College at sixteen. The reminiscences of a childhood and college friend suggest some of the qualities of the young man. At the preparatory school the Kroeber boys helped form a society whose main objective was collecting expeditions, and it lasted well into Alfred Kroeber's second year at college. At Columbia,¹

Kroeber was perhaps the shyest and most diffident one of us, yet at heart the boldest and least hampered by convention or fear of consequences; so he got rather deeper than usual into freshman escapades.

In his sophomore year Kroeber was much influenced by lectures on English literature, and he led in launching a successful undergraduate literary magazine. But the most important influence at Columbia came in the person of Franz Boas.²

After receiving his A.B. degree in 1896, Kroeber entered graduate work at Columbia with Boas. His family met severe financial reverses, but he chose to persevere, rather than to enter business. He received his A.M. degree in 1897, and made his first field trips, sponsored through Boas by the American Museum of Natural History, in 1899–1901. The trips took him to western North America in parts of the Oklahoma territory and the new states of Wyoming, Utah, Idaho, and Montana, for work mainly with the Arapaho, on whose decorative art he wrote his dissertation.

The year of his doctorate (1901) brought Kroeber to the university that was to be the lifelong setting of his career. He came as first faculty member of the Department of Anthropology, just founded at California through the generosity of Phoebe Apperson Hearst.³ His subsequent career can be briefly outlined: at the University of California (Berkeley), instructor in anthropology (1901), assistant

¹Carl L. Alsberg, Personal reminiscences, in *Essays in anthropology presented to A. L. Kroeber* xv (Berkeley, 1936).

²Alsberg (xvii) places Boas' first influence in Kroeber's junior and senior years (1894–6). Kroeber, however, has recorded Boas as coming to the American Museum of Natural History (New York) in 1895 or 1896, 'quite possibly at the turn of the year', and as receiving his first appointment at Columbia in the summer of 1896. The timing is uncertain, but the influence is not. See Kroeber, Franz Boas: The man, *AA* 45:3.15 (1943).

³See the Historical introduction, in Kroeber (ed.), *Phoebe Apperson Hearst Memorial Volume*, *UCPAAE* 20.ix–xiv (1923).

professor (1906), associate professor (1911), professor (1919); curator in anthropology (1900, 1903–11) of the California Academy of Sciences, curator (1908–25) and director (1925–46) of the Anthropological Museum of the University of California, and research associate of the Chicago Natural History Museum (1925–60). Upon retirement as Emeritus Professor and Emeritus Museum Director at seventy (1946), he did not cease teaching, but went as visiting professor to Harvard (1947–48), Columbia (1948–52), Brandeis (1954), and Yale (1958). He also went as a Fellow to the Center for Advanced Study in the Behavioral Sciences (1955–56), and returned there in the spring of 1958 for a series of seminars on the comparative study of civilizations. Such stays at other institutions helped him to know and encourage younger scholars, to whom his interest increasingly turned. With characteristic discretion, he also thus left the department at Berkeley as free as possible of the shadow of virtual identification with himself.

Many honors came to him. A founder of the American Anthropological Association, he was its president in 1917–19; president of the American Folklore Society (1906); vice-president, Section H, of the American Association for the Advancement of Science (1906); vice-chairman of the division of anthropology and psychology of the National Research Council (1921–22); as stated, a Signer of the Call, and president of the Linguistic Society (1940); Honorary Fellow of the Royal Anthropological Institute of Great Britain, and recipient of its Huxley Medal (1945); Honorary Fellow of the Academies of Science of Peru and Denmark; recipient of the Viking Medal of the Wenner-Gren Foundation for Anthropological Research (1946), and chairman of its international symposium (1952); elected to membership in the American Philosophical Society and the American Academy of Arts and Sciences. He was also a member of other professional groups, such as the American Ethnological Society, and the Modern Language Association, which he joined at eighty-one in order to publish an article on statistical study of parts of speech in poetry in *PMLA* 73.309–14 (1958), after rejections from other journals he could not interest in it. His sixtieth birthday was celebrated with a festschrift and his eightieth birthday honored by the dedication of an issue of *LANGUAGE*.⁴

Kroeber's contributions to knowledge, sustained over some sixty-four years, are remarkable not merely in number, but for scope and quality. He was probably the greatest general anthropologist that American anthropology has known. His contributions to linguistics, archaeology, ethnography, and ethnology could each have earned him an enviable reputation as a major figure, and he made noteworthy contributions to biological anthropology and folklore as well. He was a prolific fieldworker, a master systematizer, an independent and provocative theorist and critic. Something of his scope is reflected in the monographs and books he has occasionally listed as major publications: *The Arapaho* (1902), *The Yokuts language of South Central California* (1907), *Zuni kin and clan* (1916), *Peoples of the Philippines* (1919, 1928); *Anthropology* (1923, 1948), *Handbook of the Indians of California* (1925), *Cultural and natural areas of native North America*

⁴*Essays in anthropology presented to A. L. Kroeber* (Berkeley, 1936); *Lg.* 32:1 (January–March 1956).

(1939), *Peruvian archaeology* (1944), *Configurations of culture growth* (1944), *The nature of culture* (1952), *Style and civilization* (1957).

Kroeber delighted in quantitative estimate as a heuristic device: of eight paragraphs of preface to *The nature of culture*, three set forth fractions that characterize the papers collected therein; and so it is worth noting that his publications ultimately will total 460 or more, and that of this number, some 70, slightly less than a sixth, are wholly or in important part contributions to linguistics. (This does not count reviews or comments in published records of conferences.) It is appropriate, too, to put the figures in temporal perspective: the linguistic contributions are not evenly spaced throughout his career, but cluster at its beginning and end. Somewhat more than half come in the first two decades in California, a dozen or so are distributed among the next three decades, and then, beginning with 1952, there are about half again as many in his last ten years.⁵

The roots of Kroeber's linguistic interests go deep. His first remembered purely intellectual pleasure, as a boy of ten, was the demonstration of pattern in the classes of English strong verbs. As to his professional work, he observed: 'I came from humanistic literature, entered anthropology by the gate of linguistics' (*Nature of culture* 173). When Kroeber began graduate study at Columbia, Boas had announced courses in statistical theory and American Indian languages. The two courses were to remain fundamental to Boas' program for forty years, and the two subjects, sometimes in conjunction, were to be lifelong interests of his great student. Indeed, the course in American Indian languages seems to have been Kroeber's first graduate course, and he frequently mentioned the experience, speaking of Swanton as having 'cut his teeth first on Chinook like so many of us'⁶, elsewhere describing himself⁷ as 'an anthropologist who found his way into his profession by being shown how to analyze Boas' Chinook Texts into grammar', and describing with pleasure

Boas' first linguistic class, which met Tuesday evenings at his home around the cleared family dining table, [and] consisted of an archaeologist from the Museum, a teaching assistant of English, and an adventurous nondescript who soon after rolled himself out of anthropology as suddenly as he had rolled in, and who required some quarts of beer in a can from the nearest saloon to overcome the tension of a two hours' session with Chinook or Eskimo.⁸

The effect on the teaching assistant of English, Kroeber, is implicit in his praise

⁵A full bibliography of Kroeber's writings will appear in the *American anthropologist*. At the request of the Editor of *LANGUAGE* I have not tried to duplicate part of that bibliography, but cite particular writings as they are discussed. The abbreviations used include AA: *American anthropologist*; SJA: *Southwestern journal of anthropology*; UCPAAE: *University of California publications in American archaeology and ethnology*; UCPAR: *University of California publications, anthropological records*; and, as short titles for some of the books cited above, *Handbook*, *Cultural and natural areas*, *Configurations*.

⁶The work of John R. Swanton, *Essays in historical anthropology of North America, Smithsonian miscellaneous collections* 100.2 (1940); a graduate student at Harvard, Swanton had come to Columbia to learn linguistics, and wrote his dissertation, Harvard's first in anthropology, on the morphology of the Chinook verb.

⁷Foreword, in D. H. Hymes (ed.), *Reader in linguistic anthropology* (to appear).

⁸Franz Boas: *The man*, AA 45:3.7 (1943).

of Boas for instituting the first productive teaching of American Indian linguistics by

setting his students to discover the structure of a language by analysis of texts. This equivalent of laboratory method introduced the student to an attitude of independent research. It also served as specific training for new field investigation, which was subsequently provided whenever possible. This method naturally proved to be intensely stimulating to capable students.⁹

That the study of language should have played an important part at the outset of Kroeber's career is not surprising: in the school of Boas it almost could not have been otherwise. With Kroeber the linguistic interest was to prove abiding and individual. Partly this was because of a personal attraction to linguistic data, but it was also because of Kroeber's conception of anthropology. He sets forth this conception and his commitment to it in the sections of introduction written for *The nature of culture* and in the appendix to the report of his week-long symposium in the summer of 1960 on 'Anthropological horizons' (the report is to be published by the Wenner-Gren Foundation). In *The nature of culture* Kroeber presents his anthropology almost as a matter of personal style, involving both a way of working and a perspective. To sketch its character, one can perhaps best say that it joined a capacity for a broad view with love of concrete data; desire to discover patterns intrinsic to data with belief that patterns must be understood in context; a giving of primacy to cultural data and cultural frames of reference with continuous effort to extend cultural patterns and contexts to the limits of their relevance in space and time; that much of his work had its roots in a natural-history approach to the materials of the humanities; that his preference was for characterization rather than dissection; for ordering rather than manipulating data; for theory sweated out of empirical studies rather than proclaimed in advance. There was almost no side of culture but subtended his angle of vision; to change the metaphor, there was little in cultural phenomena that could not come as grist to his mill. For such an approach there was a natural place for the study of language in its own right as part of culture, and for the sake of its contribution to other, sometimes general problems; and there was of course much in language that lent itself to his way of working.

These qualities of Kroeber's anthropology could perhaps be inferred from the body of his linguistic work alone, for it is a microcosm of the whole. It has, indeed, a certain unity. Problems of the earliest years reappear in the writings of the last decade, appropriately recast; and several of the themes that run through his linguistic work are often enough combined in one publication, so that any point of entry to his work is likely to lead, chronologically and bibliographically, into much of the rest. To a considerable degree, *tout se tient*. Much of the work has been forgotten, yet it is worth while to survey it with some care, not only in tribute to a great scholar, but also because some of the problems still wait to be carried beyond the point to which Kroeber brought them, and because the significance of the man and work as an example for the future relations between linguistics and anthropology lies so much in the details seen as part of the whole.

⁹An outline of the history of American Indian linguistics, *ACLS Bull.* 29.119 (1939).

The matrix is the early period in California. There the first two decades of the century saw the bulk of Kroeber's linguistic field work, and the appearance of his characteristic interests and mode of interpretation.

A pattern of combined ethnographic and linguistic investigation had begun with Kroeber's first major field work, among the Arapaho. His dissertation related their decorative art to a theoretical controversy, but he also obtained material for a valuable monograph on Arapaho dialects, *UCPAAE* 12.71-138 (1916). This pattern continued in his almost single-handed labors in California in the first part of the century. The tribes of California were many, diverse, and little known. With the support of Mrs. Hearst, Kroeber undertook an 'ethnological and archaeological survey' of the state (ethnology included linguistics). He was joined in the linguistic work by Goddard (Athabaskan), and, for varying periods, by Dixon (Maidu, Wintu, Shasta, Achomawi), Sapir (Yana), Waterman (Yurok), Barrett (Pomo, Miwok), Radin (Wappo), and others, but the sustained bulk of the work was his, culminating, ethnographically, in the monumental *Handbook* (completed in 1918, but not published until 1925).

Much of the work was in response to the obvious need to fill gaps in knowledge before too late. Already some of the languages and dialects were extinct, and for them philology of a sort with early materials was all that could be done,¹⁰ if that. Since the description of disappearing languages has often not seemed an obvious need at all, despite being the only contribution to the future of linguistics that future linguistics cannot make for itself, Kroeber's extensive service in this task deserves full praise. Sometimes his reports mention absence of other knowledge of a dialect as a reason for publishing imperfect data obtained in the course of an ethnographic field trip.¹¹ Opportunities for obtaining linguistic data that would remedy a lack were seized.¹² Sometimes native speakers were brought to Berkeley to be worked with or trained to write their own language.¹³ As much as could be done was done, given the opportunity. Voegelin tells the story of Kroeber waiting for a train, noticing an Indian, and promptly taking a vocabulary in the time available. Kroeber's descriptive experience during this period embraced Arapaho, Zuni, Marshallese, and, of the languages of California and the adjacent west, Atsugewi, Bannock, Chumash, Costanoan, Diegueno, Esselen, Karok, Luiseno, Miwok, Mohave, Pomo, Salinan, Shoshonean, Ute, Washo, Wiyot, Yokuts, Yuki, Yurok; altogether some 33 languages.

¹⁰E.g. in The Chumash and Costanoan languages, *UCPAAE* 9:2.237-71 (1910).

¹¹Thus, 'Since there is practically no Nisenan linguistic material accessible beyond old word lists, the vocabulary obtained is given in full'—The Valley Nisenan, *UCPAAE* 24.289 (1929); 'There was no intention of presenting the imperfect lexical material thus obtained, until it was realized that no vocabulary of Washo has ever been published, and that the determination of the language by Powell as constituting an independent family, however correct it may be, has never been rendered verifiable by the general availability of the information used for the determination'—The Washo Language of east central California and Nevada, *UCPAAE* 4.308 (1907), and Notes on the Ute language, *AA* 10.74 (1908).

¹²E.g. 'Consequently an occasion for obtaining information as to these two languages, presented by the visit to San Francisco ... of a number of Shoshoni and Bannock was made use of'—The Bannock and Shoshoni languages, *AA* 11.266 (1909).

¹³E.g. Juan Dolores (a Papago) and Gilbert Natchez (a Paiute); see *UCPAAE* 20 (1923).

Throughout Kroeber's work with language as with other phenomena, there runs a remarkable capacity to observe and to seize opportunities for doing so. At Zuni in 1916 he encountered what was observable to all, surface finds of archaeological materials, but it was Kroeber who first ordered the stylistic variations of surface finds into a series, and thus invented a method of chronology. This by-product of his Zuni work was matched by another, apparently the only observations on the speech development of an American Indian child ever published. He had occasion to hear the daily speech of the youngest son of the family with whom he lived, and recorded the changes over a two-month interval; the record is still worth noting, for its details and general summary.¹⁴

A little-known series of phonetic studies shows Kroeber's attentiveness to empirical detail,¹⁵ and his eagerness to make use of aids to precision. He himself had moderate phonetic gifts, and he worked when what to expect in the native languages of western America was still uncertain. The identification of segmental sounds was being sweated out mostly by men without much phonetic training,¹⁶ and Kroeber welcomed application to Amerindian languages of

principles and methods of phonetic research established by European scholars ... largely through the entrance into this field [Amerindian] of several students trained in the study of Indo-European philology.¹⁷

Kroeber's empirical work was never data-gathering for its own sake. There was always in mind an immediate question or a larger frame of reference, and his phonetic studies show this. They were undertaken to provide orientation and grounding for further descriptive work, to answer questions current at the time about the phonetic characteristics of American languages, and also because phonetic data were cultural data whose distribution and historical interpretation were of interest in themselves. One of the questions of the day concerned the so-called 'intermediates', consonants variously perceived as voiced and voiceless by fieldworkers. As part of the phonetic instability considered characteristic of 'primitive' language, the matter had been analyzed by Boas in a brilliant paper.¹⁸

¹⁴The speech of a Zuni child, *AA* 18.529-39 (1916).

¹⁵Phonetic constituents of the native languages of California *UCPAAE* 10.1-12 (1911); Phonetic elements of the Mohave language, *UCPAAE* 10.45-96 (1911); Phonetics of the Micronesian language of the Marshall islands, *AA* 13.380-93 (1911); Phonetic elements of the Diegueno language, *UCPAAE* 17.177-88 (1914); cf. also, Visible speech, *Scientific American* 112.471 (1915).

¹⁶A good example is found in the history of gradual recognition of the members of the voiceless lateral order; the matter is mentioned by Kroeber (*UCPAAE* 10.11 [1911]) and is salient in Boas' work on the Northwest Coast.

¹⁷He had particularly in mind his first colleague at Berkeley, Pliny Earle Goddard, who, fresh from a degree in philology with Benjamin Ide Wheeler, combined Athabaskan field work and laboratory phonetics, using equipment modelled on that current in French research. Goddard's study of Hupa (*UCPAAE* 5.1-20 [1907]) was probably the first instrumental phonetics done with an American Indian language, and Kroeber, wishing to extend the use of such methods with American languages, chose Mohave because of familiarity with it through earlier fieldwork (*UCPAAE* 10.45-6 [1911]).

¹⁸On alternating sounds, *AA* 2.47-53 (1889), reprinted in F. de Laguna (ed.), *Selected papers from the American Anthropologist, 1888-1920* (Evanston, 1960), partly at Kroeber's recommendation. Boas used psychophysical data to explain the supposed alternation of

In the case of 'intermediates', Kroeber suspected a specific phonetic cause in the sounds themselves, voiceless onset of otherwise voiced stops (*UCPAAE* 10.8 [1911]); and when a German trading schooner with a crew of Marshall Islanders docked at San Francisco in April of 1911, he seized the opportunity. His earlier recordings of Caroline Islands dialects had showed such inconsistency in writing surd and sonant as to make him suspect that 'intermediates', widespread in western America, might be found in Polynesia too. About 409 tracings revealed surds in final position, sonants intervocallically, and initial 'intermediates' that began surd and invariably became voiced approaching the following vowel. Kroeber goes on to summarize the study as a whole in characteristic fashion, placing the phenomena as a type in a broader geographical and genetic context.¹⁹

The California linguistic field work had indeed an overall problem orientation from its beginning. Against the background of the number and distribution of linguistic stocks in the rest of North America, California stood out as a great exception. Nearly half the stocks (according to the Powell classification) in the country were represented in the one state (22 out of 52). Explaining this extreme diversity was seen as 'the fundamental problem of California linguistics' (*AA* 5.2 [1903]). When similarity in grammatical structure was glimpsed between a number of languages whose vocabularies seemed unrelated, there seemed an obvious bearing on the problem of diversity.²⁰

An attempt was therefore made by the writers [Dixon and Kroeber] to secure, through field investigation, information concerning the grammatical structure of all Californian languages. This task was rendered necessary by the fact that with one or two exceptions the grammar of these languages was wholly unknown.

The background of the problem orientation was a natural-history approach to the data of ethnology. Languages, like cultures, were units to be ordered, their connections traced and explained, and in this period of American anthropology the value of linguistics for ordering the ethnological data was at a peak. With historical documents lacking, archaeological time perspective not yet available, and no uniform way of determining political or cultural units amidst the mass of data, the qualitative units of linguistic classification were seized upon by many, and became a primary framework for description and interpretation. Subsequent work has modified the role of genetic classification to that of one line of evidence to be integrated with others, but its value remains a main source of anthropology's vested interest in linguistics.

When Kroeber began his California work, the Powell classification was so well established and so rightly valued that the temper of the times was not inclined

pronunciation as due to alternating apperception of a fixed sound partly resembling each of two different sounds in the observer's speech. The paper, although falling short of a phonemic conception, is a remarkable anticipation of modern work on phonic interference.

¹⁹*AA* 13.393 (1911): in all essentials the Marshallese phonetic traits 'are duplicated in the Pima-Papago language of Arizona, and several individual features recur in a number of American languages; but as regards the allied tongues of Malayo-Polynesian stock, the Marshall dialect seems to be phonetically greatly specialized.'

²⁰Native languages of California, *AA* 5.2 (1903).

toward more than incidental tinkering with it.²¹ Also, Kroeber was well aware of the necessity of lexical correspondences for proof of genetic connection,²² and the California vocabularies did not manifest connections not already considered by Powell. With the data and analyses available, the apparent resemblances among languages were in structural outlines and subsystems, and these were the lines pursued in search of further ordering. Since the connections that appeared were traced typologically in space rather than genetically in time, the correlative principle of explanation was diffusion, convergence through areal contiguity.

The first result of the field work inspired by the notion of typological resemblances was the first areal classification of a set of New World languages according to grammatical type. The four chief diagnostic features (pronominal incorporation, syntactical [pure-relational] cases, material [mixed-relational] cases, morphophonemics [Sapir's typological feature of technique, here contrasting agglutinative vs. fusional combination]) would not today seem sufficient, even with the additional minor features, such as marking of number and reduplication, that were noted; but a classification according to these features was one that the data could support, and the results made sense: the California languages fell into three broad types correlated with geographical location, cultural groupings seemed more or less to coincide, and comparison with languages outside California showed the types to be indeed distinctive. Interpretation of the typology was carefully restrained, and its application to languages outside California on which it had not been based was not assumed. Moreover, it was a remarkably original step in the study of New World languages, one that has never been adequately followed up.

Kroeber collaborated with Dixon in another pioneering typological survey, one of California numeral systems. A major point was to note correlations between features and to show the inadequacy of some existing conceptions: decimal and quinary-vigesimal could not be set up as absolute types for whole systems, since the character of a system might change at 10, and, indeed, the California material showed more cases of shift at 10 (from quinary to decimal or from decimal to vigesimal) than of continuity (quinary becoming vigesimal or decimal remaining decimal). The great diversity of radicals below 10 within even the most closely related languages was shown, the contrast with the uniformity within Indo-European made, and the moral for comparative method drawn:

Altogether it would appear that numerals occupy a very different place in California languages from their philological position in Indo-European and other great linguistic families of the old world, and that on the whole they cannot be given the importance in comparison and in questions of determination of genetic relationship that they occupy in these languages.²³

²¹'The convenience of the first exhaustive and entirely definite classification was so great that it was soon looked upon as fundamental, and the incentive to tamper with it was lost'—Kroeber, *UCPAAE* 11.288 (1915); cf. *UCPAAE* 16.49 (1919).

²²The determination of linguistic relationship, *Anthropos* 8.389–401 (1913), and statements in other writings of the period, e.g. *UCPAAE* 9.415 (1911).

²³The numeral systems of California, *AA* 9.690 (1907).

The California diversity was explained in terms of the use of arithmetical operations and compounding in the numerals below 10, and the systems related to the corresponding counting practices. This study has stood almost alone in the Americanist literature since its publication more than a half-century ago.²⁴

The thrust of the first of the two papers (Native languages of California) is such as to define what are to all intents and purposes grammatically based linguistic areas, and the principle of areal relationship is clearly stated in both.²⁵

The phonetic studies of this period show the same interest in tracing types of phenomena in the contexts of geographical distribution and genetic affiliation. The Marshallese paper concludes with the summary:

In all essentials, these phonetic traits are duplicated in the Pima-Papago language of Arizona, and several individual features recur in a number of American languages; but as regards the allied tongues of Malayo-Polynesian stock, the Marshall dialect seems to be phonetically greatly specialized.²⁶

With the California phonetic studies, the Marshallese study raises the prospect of a typological and areal survey of phonetic traits; but like the German trading ship that supplied Kroeber's informants, the prospect was soon lost from sight. (The ship sank 24 hours after sailing; the crew was saved, and Kroeber's informants had deserted before the vessel left San Francisco.) For many years, Kroeber's California paper²⁷ remained the only detailed survey for any part of the New World. Areal groupings and 'the fundamental problem whether the linguistic families of America possess any underlying or general features peculiar to themselves as a class' (*UCPAAE* 10.2 [1911]) remained untackled.

The typological interest informed many of Kroeber's reports on individual languages at this time, and his study of Washo was expressly to determine its place and areal connections as a type.²⁸ But about 1910 the focus of linguistic interpretation shifted to genetic relationships, as a decade began that was not to be paralleled until that of the nineteen-fifties for rapid unfolding of new connections and disruption of established perspectives. Kroeber has himself recorded the

²⁴Its only successor as a systematic study was inspired by two former students of Kroeber; see V. D. Hymes, Athapaskan numeral systems, *IJAL* 21.26-45 (1955).

²⁵'A principle that appears prominent in the facts that have been presented is that of territorial continuity of characteristics. A feature is rarely found in only one language. When it does occur in several stocks, as is usually the case, these are not scattered at random and more or less detached from each other, but generally form a continuous or nearly continuous area, however irregular its outline may be. This principle applies as well to types of languages as to single characteristics'—*AA* 5.21 (1903); 'The accompanying maps showing the geographical distribution by linguistic families of the various methods of numeral formation, sum up the material collected and the generalizations stated. They are in no need of a commentary beyond a notice of the extent to which the principle of territorial continuity of characteristics obtains. While diversity and irregularity seem the chief features of the maps, yet the areas in which similar numeral methods occur are not randomly scattered, but with few exceptions are geographically continuous. This makes it clear that, with but little borrowing of specific words distinct families have considerably influenced each other as regards their processes of numeral formation'—*AA* 9.671 (1907).

²⁶*AA* 13.319 (1911). (The passage was quoted in fn. 19 above.)

²⁷*UCPAAE* 10:1.1-12 (1911).

²⁸Introduction, *UCPAAE* 4.252-3 (1907).

excitement of the period.²⁹ Swanton showed Natchez to be Muskogean, and compared Athabaskan, Haida, and Tlingit; Kroeber linked Miwok and Costanoan again (Powell having separated them) and with Dixon joined Maidu, Wintu, and Yokuts with them to form Penutian; by a quick series of steps, aided by Harrington and Sapir, Hokan came into being, comprising first Karok, Chimariko, Shasta, Pomo, Yana, Esselen, (with Chumash and Salinan separate as 'Iskoman'), then joined in to Hokan through Sapir's comparisons and Harrington's affirmation of the unity of Chumash and Yuman), and incorporating Seri and Tequistlatecan through Kroeber's work, and Washo through that of Harrington and Sapir. Yurok and Wiyot were joined in Ritwan by Dixon and Kroeber, and then connected with Algonquian by Sapir. In addition, Sapir demonstrated the unity of Uto-Aztecan to the satisfaction of all and of Na-Dené to the satisfaction of some, and traced Penutian northward into Oregon and Canada. In passing, Kroeber had also forecast the linking of Salishan, Wakashan, and Chemakuan. No wonder that in the midst of this period Kroeber asserted:³⁰ 'We may accordingly be confident that the language map of North America will be thoroughly recolored in a few years.' One could not then foresee that the breaking of the Powell log-jam was to release a tide of discovery and controversy that shows no sign yet of subsiding. But the genetic connections discovered by Kroeber and Dixon have stood the test of subsequent work, and today the necessarily slow establishment in detail of comparative Hokan, Penutian, and Ritwan is one of the healthiest parts of Amerindian linguistics.

Of particular interest is the way in which the two men came to perceive the relationships. Structural similarities had suggested connections such as that of Miwok and Costanoan, and of Yurok and Wiyot, but, adhering to the Powell framework, they wrote off all lexical connections among recognized families as due to diffusion. Attempting to interpret the accumulating lexical evidence, they made a mass comparison of the equivalents in 67 dialects (of the 21 California stocks) for a list of 225 meanings appropriately 'basic' for the area. The considerable number of resemblances that appeared made no sense on any hypothesis of diffusion.

Finally, in a mood rather of baffled impotence, an interpretation of the cases of most abundant resemblance as due to genetic relationship was applied. At once difficulties yielded, and arrangement emerged from the chaos.³¹

The method (involving a table of interrelationships) anticipated the statistical

²⁹*UCPAAE* 11.287-9 (1915) are pages of special value for understanding this period; see also *SMC* 100.7 (1940).

³⁰*UCPAAE* 11.288 (1915). Kroeber's contributions to this work are found in *The Chumash and Costanoan languages*, *UCPAAE* 9.237-71 (1910), on Miwok and Costanoan; *The languages of the coast of California north of San Francisco*, *UCPAAE* 9.273-435 (1911), on presumption of Yurok-Wiyot connection; *Relationship of the Indian languages of California*, *AA* 14.691 (1912; with Dixon); *The relationship of the Indian languages of California*, *Science* 37.225; *New linguistic families in California*, *AA* 15.647-655 (with Dixon); *Chontal, Seri, and Yuman*, *Science* 40.448 (1914); *Serian, Tequistlatecan, and Hokan*, *UCPAAE* 11.279-90 (1915); and the principal statement, *Linguistic families of California*, *UCPAAE* 16.47-118 (1919; with Dixon).

³¹*UCPAAE* 16.50 (1919).

analyses of cultural and linguistic similarities that were to be one of Kroeber's main interests in the nineteen-thirties and later, and it has been credited by Swadesh as an independent earlier invention of lexicostatistics.³² Perhaps the most important legacy of the work is in its tactic of careful progress, its attitude of scrupulous boldness. Too narrow a concern with purity of method was rejected as sterile, but essential methodological safeguards were observed. There was no clinging to conventional classifications, but new findings were built up step by step, and not projected beyond the accessible horizon.

The 1919 monograph culminates the first period of Kroeber's linguistic work, and marks its end. On the theoretical plane the period is outlined by the shift in the dominant mode of interpreting historical connections, genetic retention replacing convergence through diffusion. The 1919 monograph is the chief product of this shift; following it, there is a sharp drop in linguistic publication for three decades, and also little or no conceptual change with regard to historical interpretation. Indeed, insofar as periods of Kroeber's linguistic work can be defined, it is jointly by amount of activity (great or small) and state of historical perspective (stable or in development). In the first and last periods there is both extensive activity and development in historical perspective; in the middle period, as stated, there is little of either. His textbook chapter on language, mostly concerned with historical perspective, hardly changes from the first edition to the last (1923-1948). That is a tribute to its soundness (it is still worth reading), but also a symbol of Kroeber's lack of involvement in the main linguistic developments of the period.

From about 1920 to 1950, Kroeber's linguistic publications consist of a few reviews of linguistic books; some ethnological monographs containing linguistic material; treatment of linguistic topics in general books (*Anthropology, Handbook, Cultural and natural areas, Configurations*); subgroupings of known language families (with development of statistical techniques); and a scattering, topically and chronologically, of papers. Most of the topics are not confined to the period, and indeed, except for instrumental phonetics and child language, which seem wholly part of the first period, it is hard to find one of Kroeber's linguistic interests that does not persist throughout his career. Mode of historical interpretation becomes vital again in the last period, so it seems best to consider it there, while considering now the range of other topics not yet discussed.

Ethnology led Kroeber into dialectology almost at the start. An early paper treats the degree of dialect differentiation within California languages.³³ Kroeber noted conflicting assumptions about the nature of the unusual Californian diversity, as projected at the dialect level, and made an empirical test. The paper also takes up the relations between dialects and political, social, and cultural units, showing how the relation differs as between such groups as the Maidu and Yokuts. As elsewhere, he states that collection of uniform materials by a single investigator is needed to resolve problems. Much dialect material was obtained adventitiously, but he twice made special field trips, one for the Moquelumnan

³²*Lg.* 32.17-8 (1956).

³³The dialectic divisions of the Moquelumnan family in relation to the internal differentiation of other linguistic families of California, *AA* 8.652-63 (1906).

(Miwok) study just noted, another for systematic coverage of the many Yokuts languages and dialects. The Yokuts material was first dealt with in part of his major linguistic monograph, *The Yokuts language* (1907), and the full data, promised there, form part of a monograph completed late in the summer of 1960. Dialect work involved interest in the historical information gleaned from place-names, as shown in the Moquelumnan paper and throughout the *Handbook*. In addition one paper is devoted solely to toponymy, California Place Names of Indian Origin.³⁴

As a concomitant of ethnography, Kroeber noted social variation in speech. Information is scattered through the *Handbook*, and that for the Yurok is collected in a special article.³⁵ Such study was never intensive on Kroeber's part, and the early ethnographic work that is most important for linguistics today is that on kinship. In *Classificatory Systems of Relationships*,³⁶ he defined eight principles (or categories) as basic to the classification of kin by relationship terms, and in so doing, showed the way for the semantic (componential) analysis of kinship that has only now come into its own.³⁷ In this paper Kroeber showed himself not only a brilliant analyst, finding principles that could order a mass of data, but also a polemical theorist. Here and in subsequent writings on kinship, he insisted (as against Rivers, Radcliffe-Brown, and their followers) on the linguistic dimension of kinship. For Kroeber this meant that kinship systems could not be explained entirely by fit to social institutions and practices, but must also be understood as systems of classificatory logic with a partly independent history of their own. That kin terms are linguistic facts was taken as warrant for this view,³⁸ and as showing historical linguistics to be essential to kinship study. Belief in the latter point led him to make a trial reconstruction of Athapaskan kin terms and later to urge Hoijer to undertake a more nearly definitive study. The conclusion of his Athapaskan paper puts the historical matter clearly.^{39a}

Since kinship systems are, first of all, systems of classificatory logic expressed in words which are parts of languages, the analysis and comparison of such systems without reference to their linguistic history, so far as this may be available, is an arbitrary limitation on understanding.

The point is especially pertinent, for the best effort so far to reconstruct the

³⁴UCPAAE 12.31-69 (1916).

³⁵Yurok speech usages, in S. Diamond (ed.), *Culture in history: Essays in honor of Paul Radin* 993-9 (New York, 1960).

³⁶J. *Royal Anthr. Inst.* 39.77-84 (1909).

³⁷See the papers by Lounsbury and Goodenough in the issue of *LANGUAGE* (32:1, 1956) dedicated to Kroeber. Kroeber's other important discussions of the problem are in his California kinship systems, UCPAAE 12.339-96 (1916); Kinship and history, AA 38.338-41 (1936); Yurok and neighboring kin term systems, UCPAAE 35.15-22 (1934); Athapaskan kin term systems, AA 39.602-8 (1937). The 1909, 1934, 1936, and 1937 papers are reprinted in *The nature of culture* (the 1937 paper only in part), with introductory comment (172-3).

³⁸'... the patterns have had each a history of its own as a pattern, just as the languages in which they occur have had each a history of its own'—*Nature of culture* 200.

^{39a}AA 39.608 (1937); *The nature of culture* 209.

evolution of kinship terms on a purely sociological base seems to have gone wrong in two cases where it has been linguistically checked.³⁹

Like other American anthropologists, Kroeber was concerned from the first to destroy misconceptions about the 'primitive' languages he studied. Part of his early work in typology (and of his praise of Sapir's *Language*⁴⁰) was directed against overgeneralizations and prejudices current among scholars (and still remarkably alive, even among linguists.⁴¹) For the *Popular Science Monthly* (then an intellectual rather than mechanical journal), Kroeber wrote a special article exploding notions of Amerindian languages as rapidly changing, barbarous in sound, and the like, and many of the points were incorporated into his textbook.⁴² Here he showed his persistent concern not only for objectivity in science, but also for its communication to the general society, as part of a desire to maintain the public relevance of scientific work. Although never a reformer or political activist, Kroeber more than once expressed such concern. It is reported from his student days,⁴³ and it enters as an argument against the esoteric consequences of Powell's principles of priority in the nomenclature of linguistic families,⁴⁴ in the note struck in the introduction to his monumental *Handbook*, in his praise of Sapir's book as uniquely a successful popularization,⁴⁵ in the organization of his classic textbook, whose first eighty-six pages, preceding the chapter on language, concern the lack of objective evidence for belief in racial inferiority, particularly as regards the Negro.⁴⁶

Kroeber's substantial interest in the phenomena and science of biology impinged upon his interest in language. In the first years of the century he was mainly concerned, like most American anthropologists, to separate the biological

³⁹The sociological reconstructions are in G. P. Murdock, *Social structure* (New York, 1949); the linguistic checks are H. Hoiyer, Athapaskan kinship systems, *AA* 58.309-33 (1956), which gives bifurcate collateral terms in the first ascending generation as against the generation type of terminology proposed by a follower of Murdock (see discussion in D. H. Hymes and H. E. Driver, On reconstructing Proto-Athapaskan kinship terms, *AA* 59.151-5 [1957]), and G. H. Matthews, Proto-Siouan kin terms, *AA* 61.252-78 (1959), which gives an Omaha system where Murdock inferred a Crow.

⁴⁰*The Dial* 72:3.314-7 (March 1922).

⁴¹E.g. W. J. Entwistle, Pre-grammar?, *Archivum linguisticum* 1.117-25 (1949), and *Proceedings VIIth International Congress of Linguists* 96, 392, 411 (London, 1956); see 394-6 of the latter for a statement in refutation by Bernard Bloch.

⁴²The languages of the American Indians, *Popular Science Monthly* 78.500-15 (1911); *Anthropology* (1923) 112-9.

⁴³*Essays* xvii.

⁴⁴*AA* 7.579-93 (1905).

⁴⁵'The technique of modern philology has something superb about it. It is as austere as anything in the world. The work of an accepted leader like Brugmann is of an order unsurpassed in any branch of learning. But it cannot be popularized ... [Here is where Sapir's book is new] ... It is unique in its field, and is likely to become and long remain standard'—*The Dial* 72:3.314, 317 (1922).

⁴⁶This in 1923. Kroeber's distaste for antiquarianism and insistence on public relevance appear most strongly here: 'obviously the heterogeneous leavings of several sciences will never weld into an organized and useful body of knowledge. ... As a co-laborer on the edifice of fuller understanding, anthropology must find more of a task than filling with rubble the temporarily vacant spaces in the masonry that the sciences are rearing'—*Anthropology* 2.

from the cultural realm, but, this accomplished, Kroeber took up, unlike most anthropologists, problems of comparison and continuity. In the first edition of his textbook he defined anthropology as concerned with the interplay of biological and cultural factors,⁴⁷ and gave special attention to the emergence of language in the course of human evolution and its comparison to animal communication.⁴⁸

The latter interest shows in his response to the theoretical implications of von Frisch's work.⁴⁹ Most important are his articles *Sub-human Cultural Beginnings* and *On Human Nature*.⁵⁰ With renewal of interest in such questions, Kroeber has been singled out as a pioneer.⁵¹

It is not certain when Kroeber first became interested in systems of communication other than speech. The development of writing and the alphabet was long a subject of special interest, both in its own right and as an example of processes of cultural change and growth. (Language phenomena always appealed to Kroeber in this regard.) The alphabet has a prominent place in both editions of his textbook, and the spread of writing systems provides several case histories for his concept of stimulus diffusion.⁵² Linguistic phenomena indeed provide most of the examples in this article, for several other cases deal with the development of grammatical traditions (in Japan, in China, and in Greece vis-a-vis India), and the diffusion of patterns of quantitative meter and rime. His interest in sign language appears first in a review, later in a discussion of the theoretical import of the first results from research on sign language that he helped sponsor.⁵³

Kroeber's interest in literature came early and continued throughout his life. His first publication was a short story, and one activity of his last years was to experiment with translating Heine into English, Housman into German. He was a conscious stylist in all his writing. Of poetry, he once said, 'I soon learned that I had nothing to say—in verse', but he returned to creative prose in 'Earth-maker', a fictional (Mohave) biography written for Elsie Clews Parson's collection of such, *American Indian life* (1922), and he included it in his collected essays, as an experiment that was not repeated, to be sure, but yet one with perhaps untried possibilities.⁵⁴ His field work resulted in publication of many myths and tales from American Indians, of which the most significant for comparative

⁴⁷'Here, then, is a specific task and place in the sun for anthropology: the interpretation of these phenomena into which both organic and social causes enter. The untangling and determination and reconciling of these two sets of forces are anthropology's own. They constitute, whatever else it may undertake, the focus of its attention and an ultimate goal.'—*Anthropology* 3-4.

⁴⁸Sections on 'The biological and historical nature of language', 'Problems of the relation of language and culture', 'Period of the origin of language', *Anthropology* 106-10 (1923).

⁴⁹Sign and symbol in bee communication, *Proc. Nat. Acad. Sci.* 38.753-7 (1952).

⁵⁰*Quarterly review of biology* 3.325-42 (1928); *SJA* 11.195-294 (1955).

⁵¹*The evolution of man's capacity for culture*, ed. J. N. Spuhler (Detroit, 1959), is inscribed as bringing up to date Kroeber's 1928 paper, and within the volume, Hockett's chapter on animal communication vis-a-vis language is dedicated to him.

⁵²*AA* 42.1-20 (1940).

⁵³Review of W. Tomkins, *Indian sign language*, in *AA* 29.127 (1927); Sign language inquiry, *IJAL* 24.1-9 (1958).

⁵⁴*The nature of culture* 263 ff.

literature (and linguistics in relation to it) are probably the later Mohave pieces.⁵⁵ In the second Mohave work he states his conception of stylistic analysis, a critical comment on the linguistic inadequacy of most of what has passed for stylistic study of Amerindian materials:

Of course, in any strict sense of the word, style is choice of language and can therefore be fully conveyed only in the original idiom. Even considered translation from a text in the original by one who knows the language well will successfully seize only part of the style. ... However, the majority of American Indian narrative material has been recorded in English or some other European language. And even the smaller fraction written and published in text in the original idiom has practically never been subject to genuinely stylistic word-by-word analysis. ... The one outstanding exception is the description of Yokuts linguistic style by Stanley Newman. ... Occasional other references to 'style' in Indian narrative or song usually boil down essentially to matters of form or content ... and not with linguistically selective form, which, it seems to me, is what literary style above all means.—*UCPAR* 11.133 (1951).

It is unfortunate that such comment should be necessary, but Radin's Winnebago work is almost the sole exception, half a century after Boas had insisted that stylistic study had to be undertaken with linguistic tools. Kroeber's other late publications reflect his interest in long-range perspective,⁵⁶ sometimes linked with statistics, especially as stimulated by the research of Josephine Miles.⁵⁷ She tells of his delight at discovering the possibility of tracing long-range temporal patterns in use of words through the concordances of the major English poets; a study in which he found such a pattern for the frequencies of *death*, *dead*, *die*, *dying* is unpublished.

As to the history of linguistics, Kroeber of course contributed partly as a participant observer. Passages in his writings on predecessors and contemporaries such as Brinton, Powell, Boas, Swanton, and Sapir are indispensable to the historian of American linguistics; and his personal correspondence with Sapir should be edited (as he himself wished) for the material of scientific relevance. His participation engaged him eventually in full-scale discussion of the authorship of the Powell classification, after allusions throughout his career to the role of the ornithologist Henshaw.⁵⁸ He contributed an historical sketch to the final report of the Committee on Research in Native American Languages,⁵⁹ and has helped

⁵⁵Seven Mohave myths, *UCPAR* 11.1-70 (1948); A Mohave historical epic, *UCPAR* 11.76-171 (1951).

⁵⁶The novel in Asia and Europe, *UCP in Semitic and Oriental studies* 11.233-41 (1951).

⁵⁷Parts of speech in periods of English poetry, *PMLA* 73.309-14 (1958), a discussion of Miss Miles's work.

⁵⁸Kroeber, Systematic nomenclature in ethnology, *AA* 7.580 (1905); Some relations of linguistics and ethnology, *Lg.* 17.288 (1940); Concluding review, in S. Tax and others (eds.), *An appraisal of anthropology today* 369 (Chicago, 1955); Powell and Henshaw: An episode in the history of ethnolinguistics, *Anthr. ling.* 2:4.1-5 (1960). The full-scale discussion in the last paper was prompted by W. C. Sturtevant, Authorship of the Powell linguistic classification, *IJAL* 25.196-9 (1959). The basis of the matter is a visit to Kroeber by Henshaw early in the century. In date and content Kroeber's own 1905 paper (cited above) corroborates his memory of the event 55 years later.

⁵⁹An outline of the history of American Indian linguistics, *ACLS Bull.* 29.116-20 (1939).

place the discovery of Indo-European relationships in the context of general science.⁶⁰ His chief contribution to the history of linguistics is his treatment of it as part of the general problem of the clustering of peaks in human achievement.⁶¹ Most of the chapter, dealing with single lines of national philology, is successful, forming one of the best starting points for the student of history of linguistics. The end of the chapter, dealing with the recent period under the two main headings of 'linguistics' and 'comparative philology' suffers from closeness to our own time; the lists of great scholars seem partly arbitrary and incomplete. But if the general method were applied to lines of scholarship defined more precisely and consistently, the results would be quite valuable.

Two characteristic interests were statistics and style, as ways of ordering and grasping significance in phenomena. The 1919 monograph with Dixon sorted counts of cognates in tables. In the 1930's Kroeber's ethnology and linguistics took a decidedly statistical turn as he collaborated with H. E. Driver and later S. Klimek on the ethnological side, and with C. D. Chrétien on the linguistic side. The general mode of approach was the same, seeking statistical techniques for grouping historically related data, whether California Indian ethnological traits or Indo-European dialect features. The statistics in the middle period was part of its most solid single line of linguistic work, subgroupings within known language families.⁶² And statistics played a large part in his later linguistic activity, as he encouraged its use by others,⁶³ and employed it in critical and constructive work himself.⁶⁴

The word and concept of style came increasingly to the fore as Kroeber's work unfolded. It played a great part in his contributions to archaeology, entered into his treatment of other cultural problems (e.g. the stage of development of New World civilizations at time of conquest in *Cultural and natural areas* [1939]), and reached its fullest statement in his Messenger lectures at Cornell (1956). Much of the attention is to art; his doctoral dissertation had been on Arapaho decora-

⁶⁰Evolution, history, and culture, in S. Tax. (ed.), *Evolution after Darwin* 2.1-6 (Chicago, 1960); the section is 'An exception: Philology', 8-9.

⁶¹Ch. IV, Philology, *Configurations of culture growth* 215-38 (Berkeley and Los Angeles, 1944).

⁶²Relationships of the Australian languages, *Proc. Royal Soc. New S. Wales* 57.101-117 (1923); Uto-Aztec languages of Mexico, *Ibero-Americana* 8 (1934); Quantitative classification of Indo-European languages, *Lg.* 13.83-103 (with Chrétien); of Mayan languages, in *Cultural and natural areas* 112-4 (1939); The statistical technique and Hittite, *Lg.* 15.69-71 (1939; with Chrétien); Classification of the Yuman languages, *UCPL* 1.21-40 (1943).

⁶³E.g. '[there are] two new developments to chronicle, both of interest to cultural anthropologists in their results, and both using quantitative expression' (referring to lexicostatistics and Greenberg's typological indices)—History of anthropological thought, in W. L. Thomas (ed.), *Current anthropology* 296-7 (Chicago, 1955).

⁶⁴In lexicostatistics: Linguistic time depth results so far and their meaning, *IJAL* 21.91-104 (1955); Romance history and glottochronology, *Lg.* 34.454-7 (1958); Reflections and tests on Athabascan glottochronology, *UCPAAE* 47.241-58 (1959); Semantic contribution of lexicostatistics, *IJAL* 27.1-8 (1961). On Greenberg's quantitative typology, besides encouragement in Critical summary and comment, in R. E. Spencer (ed.), *Method and perspective in anthropology: Papers in honor of Wilson D. Wallis* 273-99 (Minneapolis, 1954), and in *Lg.* 36.20-21 (1960), the first paper of an intended series, Typological indices I: Ranking of languages, *IJAL* 26.171-7 (1960).

tive art, and a concern for art history found expression throughout his life. But the stylistic interest goes deeply into his linguistic work as well, and language figures in his major application of the concept of style to civilizations. In the central chapter among the published lectures, Kroeber concluded:⁶⁵

I have faith that a greatly enlarged understanding of civilizations as macrophenomena is attainable, and that it will include comprehension of the part played in their constitution by style.

And in reaching that conclusion, he stated:⁶⁶

That the members of our civilization and of others are very little aware of total style need not discourage us much. Every human language has such a patterned style—we call it its grammar—of which the speakers are unaware while speaking, but which can be discovered by analysis and can be formulated. The coherence of a grammar is never total or ideal, but is always considerable; it certainly much exceeds a catalogue of random items. Cultures are larger, more varied and complicated sets of phenomena than languages, as well as more substantive and less autonomous. But the two are interrelated—in fact, language is obviously a part of culture, and probably its precondition. So the structure of cultures, like that of languages, also seems potentially describable in terms of an over-all patterning.

For the most part, style is a humanistic concept, statistics a scientific tool, but for Kroeber there was no clash between the two. Statistics sometimes served in the description of style, and both were means to the main end of discovering the order in phenomena. Indeed, Kroeber's work stands as an example of how the clash between the two cultures of science and the humanities, of which Sir Charles Snow has written, can be resolved in the pursuit of linguistics and anthropology, the two disciplines which have both humanistic and scientific roots. Unlike narrow partisans of science, Kroeber never rejected significant data on the grounds of maintaining the purity of certain methods; unlike narrow partisans of the humanities, he never rejected useful methods on the grounds of maintaining the purity of certain data. He often commented on the importance of linguistics as an example for anthropology and the study of culture, as providing a model and hope for the scientific treatment of humanistic materials,⁶⁷ sometimes with regard to historical, sometimes descriptive work. It is notable that the 'Index of principal topical cross-references' to his book, *The nature of culture*, contains as its only linguistic item: 'Language as an example for culture'. The citations are mostly to the argument that the study of language exemplifies the study of cultural phenomena in purely cultural terms, and their understanding through pattern and historical context.

As the dean of American anthropology, Kroeber became involved in its discussions of linguistic questions at conferences and in various volumes. Two such questions were the general relation of language to culture, and the special relation proposed by Whorf. Kroeber was senior author of the chief American treatment of the concept of culture, wherein literally hundreds of authors are cited; but strangely, the section on the general relation of culture to language does not

⁶⁵*Style and civilizations* 107.

⁶⁶*Style and civilizations* 106.

⁶⁷E.g. 'Linguistics is a genuine natural science dealing with intangible phenomena. That it grew out of culture-bound contexts augurs well for the study of culture'—concluding review, in S. Tax and others (eds.), *An appraisal of anthropology today* 368 (Chicago, 1953).

mention Kroeber's own statements.⁶⁸ His view is manifest, however, in his textbook and many other writings: though often distinct in practice, language is part of culture, sometimes an especially significant part. In his textbook chapter on language emerge his views on the unconscious nature of cultural patterns, the role of the individual in history, and the sane attitude toward cultural relativism (1923, 125-33). And the concluding review of the *Culture* monograph has an important section that begins: 'The clearest case is furnished by linguistics'.⁶⁹

On the hypothesis associated with Whorf, Kroeber took always a cautious view, as shown in his direct comments at the Wenner-Gren conference of 1952, and at the special conference in Chicago in 1953.⁷⁰ His last comments were:⁷¹

As soon as we learn how to approach the problem with varying depth of focus ... it will probably prove both 'true' and 'false' at different levels ... I do not believe that at the present time the Whorfian problem can be solved by tests or experiments any more than by analysis: both evidence and arguments simply do not meet counterevidence or argument ... a new basic approach will be needed for a pertinent answer to this intriguing and important problem.

Whorf's proposed language-culture correlations called to Kroeber's mind such attempts at supersummative patterns as those of Spengler, and his book on style and civilization suggests that his own new basic approach to the problem would have been to trace linguistic and cultural patterns historically, looking for congruences but not determinisms. The gist of his thought seems to have been to regard language as an example of culture, and for the study of culture, but not as its matrix. Certainly his bent was toward the working out of linguistic patterns in their own terms with larger correlations or summations to follow. This is especially clear in his treatment of two problems. One is parallel: reviewing an attempt at cultural and psychological interpretation of music, he wrote:⁷²

The author appears to have had a feeling that a song could best be studied in relation to its place in the culture. Ultimately, this feeling is correct. But in its first aspect a song presents a musical problem and must be brought into relation with other musical material. It is probably only after the music and the religion of the Sioux have been separately worked out with some care that endeavors to determine the relation between the two can be seriously fruitful. ... In other words, a piece of music associated with a certain cultural activity is first of all music, secondly a piece of culture, and only lastly and indirectly an expression of personal emotion.

The parallel (*mutatis mutandis*) to G. L. Trager's formulation of the Whorfian

⁶⁸Kroeber and Clyde Kluckhohn, *Culture: A critical review of concepts and definitions, Papers of the Peabody museum of American archaeology and ethnology* (Harvard University) 47:1.115-24 (1952).

⁶⁹*Culture* 188.

⁷⁰Concluding review, in S. Tax and others (eds.), op.cit. 370 (the Whorf correlations are not proved); comments passim in H. Hoijer (ed.), *Language in culture* (Chicago, 1954), such as that the Whorf insights are very interesting but hard to prove (231-2), and need certain kinds of testing (274).

⁷¹Prepared comments on Clyde Kluckhohn, Notes on some anthropological aspects of communication, *Wenner-Gren Foundation Symposium* 7 (1960). I am much indebted to the director of the Foundation, Paul Fejos, for copies of the paper and the comments.

⁷²Review of Frances Densmore, Teton Sioux music, *AA* 20.446-50 (1918).

problem is almost exact.⁷³ And Kroeber dealt just this way with the cognitive aspect of kinship, which is perhaps the language-and-culture problem par excellence of American anthropology. He insisted on the cognitive import of kin terms as part of language, but also on their degree of autonomy of other parts of culture, such that the problem was always, having worked out the pattern of each in its own terms, to discover the degree of fit, rather than to take one as determinant of the other. Again, the concept of style and historical context enter: kin terms, as unconscious systems of classificatory thought, are 'styles of logic in a limited field of universal occurrence'.⁷⁴

The several conferences and volumes during the early part of the nineteen-fifties involved Kroeber in fresh currents of anthropological discussion about language, and therein is the prelude or turning point for his increased linguistic activity. In the first half of the decade his published linguistic work consisted mostly of comments and discussions for such occasions, although he had begun to work again on early field data. (In 1951 he obtained a grant to aid completion of linguistic and ethnologic researches on California Indians.) The second five years of the decade saw a spurt of linguistic publication. Some of it was the bringing out of California materials.⁷⁵ Most striking were the papers on new results and approaches in lexicostatistics and typology.⁷⁶

Kroeber was responding to, and helping encourage, an emerging trend. What he perceived is best summarized in his own words:⁷⁷ 'Linguistics has begun the return to (1) typology and classification, (2) semantics'. The interest in semantics, or meaning, was a serious one; in his own contribution to the question of the differential stability of semantic classes of stems, he praised lexicostatistics for helping bring meaning back into linguistics more definitely,⁷⁸ and in a discussion of relations between linguistics and anthropology, singled out meaning for consideration as 'one kind of content, one body of phenomena, which language and culture indubitably share'.⁷⁹ But fresh ideas on classification and historical interpretation were what engaged him most, and the changing content of his engagement illuminates his life's work. What began as a continued concern with statistical methods of subgrouping and achieving time depth within a genetic

⁷³G. L. Trager, The systematization of the Sapir-Whorf hypothesis, *Anthr. ling.* 1:1.31-8 (1959).

⁷⁴*AA* 38.340 (1936).

⁷⁵An Atsugewi word list, *IJAL* 24.203-4 (1958); Northern Yokuts, *Anthr. ling.* 1:8.1-19 (1959); *The Sparkman grammar of Luiseño*, *UCPL* 16 (1960; with George Grace); Yurok speech usages (see fn. 35); and two Yokuts monographs now in press. Kroeber also resumed work on Yuki, and one note reached print: Possible Athapaskan influences on Yuki, *IJAL* 25.59 (1959).

⁷⁶Linguistic time depth results so far and their meaning, *IJAL* 21.91-104 (1955); Romance history and glottochronology, *Lg.* 34.454-7 (1958); Reflections and tests on Athabaskan glottochronology, *Ethnographic interpretations* 8, *UCPAAE* 47.241-58 (1959); Statistics, Indo-European, and taxonomy, *Lg.* 36.1-21 (1960); Typological indices I: Ranking of languages, *IJAL* 26.171-7 (1959); Semantic contribution of lexicostatistics, *IJAL* 27.1-8 (1961).

⁷⁷Addendum, *Report on anthropological horizons* (preliminary version) 70 (Wenner-Gren Foundation for Anthropological Research, 1960).

⁷⁸*IJAL* 27.8 (1961).

⁷⁹Foreword, *Reader in linguistic anthropology* (Ms.).

perspective led into a new typological perspective (with increased weight given to diffusion). This was a development that had come full circle, for the California work had begun with typological and diffusional interpretation.

The narrative sequence is somewhat misleading, however, and to understand fully Kroeber's historical work (his chief linguistic love) and to appreciate its value as a legacy, one must realize that each of the three main modes of historical interpretation for linguistic resemblances, genetic, areal, typological, had for him deep and lasting roots, the typological in his love of extrication of pattern, the areal in his ethnology, the genetic in his regard for its ordering power.⁸⁰ All had a part to play in his concern for understanding through classification and context. The key to the shifts in priority of attention is that Kroeber, never a partisan of one mode of interpretation against another, worked and recommended according to his sense of the weight of evidence as to the most productive direction of effort at a given time. He was quick to sense diminishing returns; at the same time he seldom abandoned an interest, but kept it at hand (or let it lie fallow).

This capacity for a mixed strategy is one enduring significance of his historical work, and there are other values for us now in his use of each mode of interpretation. In typological work he had a skill for concise characterization of a language that is worth emulating, and he demonstrated an approach still waiting further development in Amerindian linguistics, when he aligned Yokuts and Yuki, point-for-point, putting differences of structure into relief and also showing a commonalty of type within the wider North American context.⁸¹ His last ethnological monograph applied the same approach, and, although he had known Yurok culture for half a century, he found that the controlled comparison gave him a deeper insight into it. He hoped the approach would be developed by others, so organizing in a new way the accumulated rich data on North American cultures, a hope that was part of his general concern in his last writings for the extension of taxonomy, in culture and language, as the indispensable task.⁸² And in relating

⁸⁰E.g. 'The situation is one of those not infrequently arising in which the philologist, and only he, can come to the ethnologist's or historian's rescue. A dozen randomly preserved facts from the history of civilization of a nation are almost certain to be so disconnected as to allow only of the most general or doubtful inferences; the same number of words, if only they and their meanings are carefully written down, may, if there are more fully known cognate tongues, suffice to determine with reasonable assurance the provenience and the main outlines of the national existence of a lost people. The student of history who permits the difference of material and technique of the sister science philology to lead him into the lax convenience of disregarding it as something alien and useless, withdraws his hand from one of the most productive tools within his reach—on occasion his only serviceable instrument'—*Handbook* 281; Kroeber considered the *Handbook* a history.

⁸¹The Yokuts and Yuki languages, *Boas anniversary volume* 64–79 (New York, 1906). The conclusion states in part: 'the degree to which their similarities are fundamental is quickly and convincingly apparent when they are even superficially compared with such languages as Iroquois, Algonquin, Shoshonean, Eskimo, Nahuatl, Wakashan, Chinook, Salish, or Siouan' (78).

⁸²E.g. 'I feel that the study of both culture and language is in crying need, in its own right, of far more systematic classification of their multifarious phenomena. Perhaps we have had a surplus of bright ideas and a shortage of consistent ordering and comparison of our data'—*Lg.* 36.17 (1960); 'The situation is made more difficult by the fact that

typology to diversity, Kroeber maintained a balance between extremes that it would be well to emulate. In much of the present century there has been a tide toward uniqueness and incomparability; now there is a swelling of emphasis on the essential sameness of languages. Kroeber consistently related his descriptions of individual languages to the concepts and terms of the general linguistics of the time, and where the facts required, participated vigorously in the trend (set by Boas in the Introduction to his *Handbook of American Indian languages*) to highlight relativity and explode biased generalizations;⁸³ and one of his last statements cautioned against premature statement of universals, and against stretching recognition of their importance into a tacit claim that they are of sole or even primary concern.⁸⁴ Yet he made no fetish of diversity or exceptions; inadequate general concepts and terms had to be attacked, so that more adequate ones could replace them. Indeed, he maintained that the goal of an individual description was to place the language in the context of general linguistics. The attitude is clear in early critiques of typological terminology. He rejected biased use of a general term such as 'incorporation'.⁸⁵

It is thoroughly misleading to designate the same process respectively 'composition' and 'incorporation' according as one has in mind his own or other forms of speech. Some day philologists will approach their profession not with the assumption that language must differ in kind or in being relatively better or worse, but with the assumption that exactly the same fundamental processes run through them all, and with the realization that it is only by starting from the conception of their essential unity of type and method that their interesting and important diversities can be understood.

But he likewise rejected ad hoc machinery to fit each case, and in doing so went beyond Boas (or beyond Boas as commonly interpreted and followed) to a position just beginning to be occupied by the advance guard of American linguistics at the present time. In a discussion of the structure of the Algonkin verb, Kroeber maintained that to describe each language *sui generis* was not enough, that the repudiation of frameworks misleadingly extended from other languages was only

anthropologists still tend to value personal expertise, technical virtuosity, cleverness in novelty, and do not yet clearly recognize the fundamental value of the humble but indispensable task of classifying—that is, structuring—our body of knowledge, as biologists did begin to recognize it two hundred years ago'—Evolution, history, and culture, in S. Tax (ed.), *Evolution after Darwin* 2.14 (Chicago, 1960).

⁸³His repeated discussions of the use of suppletive stems for number in verbs, of the relation of objective and subjective forms to each other and to verbs, of the presence or absence of pronominal incorporation, are all with an eye toward then current typological generalizations about Amerindian languages.

⁸⁴Prepared comment on Clyde Kluckhohn, Notes on some anthropological aspects of communication (1960).

⁸⁵Noun incorporation in American languages, *Verh. der XVI. Internationalen Amerikanisten-Kongress* 569–76 (Wien, 1909); Noun composition in American languages, *Anthropos* 5.204–18 (1910). When Sapir then showed that 'incorporation' could be given precise descriptive content (The problem of noun incorporation in American languages, *AA* 13.250–82 (1911)), Kroeber, noting that Sapir's explication related it to stem-compounding, offered a fourfold typology of stem-compounding in terms of parts of speech that did away with need for the term 'incorporation' altogether: Incorporation as a linguistic process, *AA* 13.577–84 (1911).

a first step. If traditional Indo-European categories did not fit Algonkin, it would be meaningless to invent a novel set for Algonkin alone.⁸⁶

The determination of what they [Indo-European and Algonkin] have in common, involving as it does the recognition of that in which they are different, is an essential purpose of the study of both; for whether our interest lies in the problem of the nature or that of the origin of human speech, a classification is involved. In its widest ultimate aspect philology is concerned not with Algonkin as such nor with Indo-European as such but with all languages. Only when speech in general, its scope and its methods, are better understood will both Algonkin and Indo-European, or for that matter any particular group of languages be more truly understandable. The real aim of the study of any American tongue, as well as the aim of any deeper research in Indo-European philology, must therefore be the more precise and fundamental determination of their relations to all other languages; and this necessitates concepts and terms which are applicable in common. It is impossible to characterize the wolf in terms of his skeleton, the elephant of his embryology, the whale of his habits, and then to construct a classification which will help to reveal the inherent nature, the development, or the origin of the animal kingdom.

The point of view of the concluding metaphor is of a piece with that of his last article in *LANGUAGE*. The typological perspective was there, if largely latent, through the intervening period. Similarly the areal perspective persisted, in the broad sense of tying linguistic phenomena to the map and inferring significance from their geographical relationships. As a matter of principle, Kroeber insisted on language as one criterion in the areal classification of cultures, as against primarily ecological approaches.⁸⁷ And areal perspective led him to an original contribution to lexicostatistic theory.⁸⁸ Salishan had been recognized as a family very early, Hokan late and with dispute, yet glottochronology showed similar time depths for both. Pointing out the contiguity of most Salishan languages, the isolation of most Hokan languages, Kroeber argued that the percentages of retention could be the same, but the sources of the replacement different. For a Salishan language most innovations would be shared with other Salishan languages, most borrowings Salishan in origin, keeping relationship apparent; while for a Hokan language, most borrowings would be non-Hokan, innovations unique, obscuring relationship. Geographical distribution thus explained the disparity between the traditional classification and impression of internal diversity, based on inspection of vocabulary lists, and the new glottochronologic time depths. A later article made use of another connection between time perspective and areal distribution, using the principle that 'close uniformity of speech throughout wide

⁸⁶Arapaho dialects, *UCPAAE* 12.71-138 (1916), esp. 93. In the monograph Kroeber accepts rehabilitation of terms such as 'incorporation' and 'polysynthetic', as convenient designations for particular applications of general processes (91-2).

⁸⁷'Language itself is a natural part of culture from one point of view, though it can also be separated off for other purposes of study. I have therefore not hesitated to put Paiute and Walapai into separate subareas and even main areas in my maps. It is true that Yuman speech (Walapai) would be as practicable north of the Grand Canyon as south of it or for that matter in the Colorado River bottomlands; any historically particularized language is in its nature impervious to such interadaptation with environment. Consideration of speech may therefore tend to blur the sharpness of classificatory conceptualization of culture. But as long as speech is in culture, and cultures are what we are classifying, speech obviously belongs in the picture'—Comments to P. Kirchoff, *Gatherers and farmers*, *AA* 56.556-9 (1954).

⁸⁸Linguistic time depth results so far and their meaning, *IJAL* 21.91-105 (1955).

areas must be due to recency of spread' to interpret a number of North American cases (Algonkin, Teton within Dakota, Navaho within Apachean, Chemehuevi and Kawaiisu within Southern Paiute, Northern Paiute and Mono, Yokuts, Wintu).⁸⁹ Nor was the diffusional aspect of areal resemblances lost from sight. He pointed up the problem of grammatical diffusion as the final topic of his presidential address.⁹⁰ He was sensible of Boas' contention against Sapir that grammar might be significantly diffused, but took it as a methodological counter-argument for which Boas had never developed sufficient evidence. He saw the problem as an empirical one that warranted systematic investigation to decide among the alternative interpretations of Ray's work (that IE was exceptional as a group, that Melanesian was exceptional, or that Ray's analysis was wrong). Thus it is no surprise that in his last article in *LANGUAGE* he responded to increasing evidence from his Berkeley colleagues of the importance of diffusion with reappraisal of Boas as possibly right.

It must be noted here that Kroeber's role with regard to genetic and areal interpretation in Amerindian linguistics has been largely ignored or misinterpreted. The usual picture is one of Boas holding out almost in isolation for the importance of diffusion and areal interpretation against Sapir and others hell-bent for long-range genetic connections, later being consoled by news of the areal perspective of many European scholars.⁹¹ Because Kroeber helped discover new genetic relationships beyond those of Powell, accepted many of Sapir's results, and criticized Boas' refusal to consider new evidence or value historical reconstruction,⁹² it has been overlooked that he stood as a third party to the dispute. Not only was his approach pragmatic rather than partisan, but the principle of areal relationships in language was stated clearly in his work far earlier than the writings of the Prague school on Sprachbünde, and earlier, so far as I can ascertain, than in any writing of Boas.⁹³ And, unlike Boas, who acted mainly as a methodological critic, Kroeber helped make substantive contributions to areal interpretation in language. Indeed, the kind of work that he did with Dixon in the first decade of the century still largely waits to be taken up again; the mapping of Amerindian linguistic traits is almost all still to be done.

In genetic classification, Kroeber's middle view is a healthy legacy, as has been noted; he was open to new findings, yet distinguished carefully between the proven and the prophetic,⁹⁴ and sought to work within the range where consolidation and integration with other lines of historical evidence were possible.

In reappraising Boas' views on diffusion more sympathetically, Kroeber implied that the new genetic results of Haas and others led only to anarchy, not in

⁸⁹Recent ethnic spreads, *UCPAAE* 47.235-310 (1959).

⁹⁰*Lg.* 17.290-1 (1940), regarding Ray's work on Melanesian; Kroeber had reviewed Ray's book a quarter-century before, *AA* 29.705 (1927).

⁹¹R. Jakobson, Franz Boas' approach to language, *IJAL* 10.188-95 (1944). The disagreement between Boas and Sapir, and the theoretical issue, have been explicated by Morris Swadesh, Diffusional cumulation and archaic residue as historical explanations, *SJA* 7.1-21 (1951).

⁹²E.g. in *IJAL* 21.92-3 (1955), and *SMC* 100.7 (1940).

⁹³See fn. 25.

time to an ordered, if novel, overall picture. He withdrew the implication,⁹⁵ but the heart of his view would seem still to be in the passage beginning:⁹⁶

The exit from this confounding of the long established order seems to be more comparison and more taxonomy, and let the genetic and the influencing chips lie where they fall. ... When genetic similarity is strong enough to be certain, its findings should of course continue to be accepted. But when the similarity dilutes into mere possibilities which are so scant and scattering that they might be due to remnants of original unity, or to contact influences or borrowings, or to both sets of causes, some broader strategy of attack is indicated.

Kroeber's developing views, as presented in the conclusion of this article, need some clarification. The conclusion, for example, seems directed primarily to persuasion of those whose main interest is genetic connection, by suggesting that other taxonomic methods may probe historical connections even further into the past, whereas the earlier portions of §8 (15-8) are general, implying a systematic taxonomy in both language and culture that would apply at all time levels, remote and near. But it is a tribute to the man that his latest work has the vigor of growth. What beside Greenberg's typological indices he would have accepted into a program for 'achieving a sound linguistic taxonomy of breadth and depth ... by operating with mechanisms that transcend the concept of genetic unity' (21), we cannot now know. Clearly, however, the core of the program is that with which he began: to extract all possible historical significance from linguistic phenomena, operating within known genetic relationships, transcending them with typology, always interpreting in reference to the place of things in time and space, always seeking the largest accessible context in time.

Kroeber's lasting contribution is almost wholly through his own substantive work and example, not, as with Boas and Sapir, also through an impact on descriptive method and on students trained by himself. Kroeber seemed always somewhat shy of the technical core of 'philology' or linguistics, as containing methods whose rigor he admired but with which he did not feel wholly conversant, or free, certainly not to the point of modifying them (his use of statistics is the one exception). He referred to himself as 'something of a philologist (= linguist)' in his review of Sapir's *Language*, and at most claimed no hesitancy with regard to a content he thoroughly knew, that of California Indian language (*Handbook* vi). In this hesitancy to claim the mantle of full-fledged linguist, he was but honest. His training in linguistic analysis came from a self-taught pioneer well before the codification of descriptive methods, and his student contact with the comparative method was not, like Sapir's, first-hand. Whether his bilingualism in German and English helped or hurt, he had no special phonetic gift, and told stories on himself in this regard; nor was extensive phonetic training adequate to the western Amerindian languages available to him at the time. Moreover, his task in his years of field work was never solely linguistics, for had it been, he undoubtedly would have set himself to master and develop descriptive linguistic methods, rather than use those at hand; but he was

⁹⁵'I did not think the "overall anarchy" will be permanent—more like a turn of the tide: still flowing out and the new flood coming in. I'm not in the least pessimistic over it; stimulated rather'—(personal communication, 12 July 1960).

⁹⁶*Lg.* 36.19-20 (1960).

responsible for a broad range of data, and his personal sweep of interest reinforced this commitment. Here again is the lesson that something of a double standard must be invoked in judging the linguistic contributions made by field workers in the course of other duties, the value of the information being set against the imperfections of the record or the lack of excitement in the method.

Kroeber's massive contributions of data and interpretation show how greatly he felt the fascination of linguistics, despite his hesitancy. If any serious criticism can be fairly made of his California career, it is that he did not see to the technical training of others during so much of the period in which he dominated the Berkeley department. In later years he insisted that no anthropology department could claim to be first-rate without an active linguistic specialist, but not so in practice during his middle years.⁹⁷ In the first years of the Berkeley department, linguistics was a major part of the instruction offered, reflecting concern with Amerindian languages and Goddard's interest in instrumental phonetics. After Goddard left in 1909, no other linguist was hired, except for Sapir's presence as Lecturer in the summer of 1915. Native speakers were given encouragement and training, and many students did incidental linguistic work, but the only specialists were J. Alden Mason (University Fellow in 1910-11, Research Fellow in 1916-17), L. S. Freeland at the turn of the 1920's, and C. F. Voegelin in the early 1930's. The Hearst memorial volume (1923) and Kroeber's festschrift (1923) contain but a sprinkling of linguistic contributions, a sharp contrast to the quantity and, in some measure, the quality of the special number of *LANGUAGE* (1956). In the first years there were few or no graduate students, but the influx of the middle years coincided with a period in which anthropology was one of the few settings in which linguistics could be carried on at the university (philology was under a cloud there for some time after the First World War). The influx unfortunately coincided also with the dip in Kroeber's own linguistic activity.

In the first half-century of work with Amerindian languages at the University of California, then, after the extensive activity in the first two decades by Kroeber, supplemented by that of Dixon, Goddard, Sapir, and Harrington, there came decades that saw field work by Harrington of the BAE, by a Boas student (Reichard on Wiyot), Sapir students (Li on Mattole, Newman on Yokuts), Sapir himself (a summer with Hupa), and other friends and associates of the department (especially Paul Radin, Jaime de Angulo and his wife Nancy Freeland, and Hans Uldall); but, except for Freeland and Voegelin, none were the University's own products. A fresh, sustained impetus to the increasingly critical rescue work, and to the training of linguists to do it, had to wait until the formation of

⁹⁷Sapir spent a year before his degree as Research Fellow at Berkeley, but: 'Sapir's stay fell in the terminal year of a second period of affluence and research activity provided for the Department and Museum of Anthropology by Regent Phoebe Apperson Hearst. In the summer of 1908 came a renewed and deeper cut in resources, with the University assuming responsibility for all staff salaries; this circumstance rendered a continuation of Sapir's connection with the University impossible. In fact the staff of Anthropology—Museum and Department—was reduced to the two original academic appointees: Goddard and myself. A year later, Goddard, depressed by the contracted prospects at Berkeley, accepted an appointment with the American Museum in New York.'—Kroeber's preface, E. Sapir and M. Swadesh, *Yana dictionary* (ed. by M. R. Haas), *UCPL* 22.v (1960).

the Survey of California Indian Languages by Mary Haas and Murray Emeneau early in the 1950's. In the intervening period Kroeber and his department concentrated on ethnology and culture history. Kroeber still listed himself as actively instructing in Indian languages (*ACLS Bull.* 29.119 [1939]), but it was the Committee on Research in American Native Languages, sparked by Boas, that supported and fought for the urgently needed descriptive work in California and elsewhere, whereas Kroeber conceived and carried through a massive and effective ethnographic salvage program, that of the Culture Element Surveys. It would be jejune to note all this, were it not that students are part of a scholar's record, and that the record here involves Kroeber's role in American linguistics. When the study of language in future years shows the fruit of seeds sown by him, it will be through the work of men, trained by others, who have responded to his insights and perspective. So far as this will be due to the dip in linguistic activity of his middle years, it is compensated by his encouragement of younger men in his last years, and his attraction of them by his own youthful freshness of mind, for which there are countless anecdotes and illustrations. And if with him a whole historical period has passed,⁹⁸ much of its value he consciously transmitted in his person. He knew well how much a value must change to remain the same.

The dip in linguistic activity must be seen in proportion. The middle period was scant only relative to the standards of productivity of someone like Kroeber—not like an artist's period of silence, but like a period with few portraits from a painter who had turned most of his attention to murals. Like Boas, Kroeber ranged so widely and individually that any conventional framework is too narrow, and to estimate him within one is an error. Perhaps he will not suffer like Boas, who has often been judged anachronistically by social anthropologists who forget that his major chosen fields were physical anthropology, folklore, and linguistics. Yet to evaluate Kroeber one must bring to mind so many contributions in method, theory, and data in so many lines of work that the imagination can hardly hold them all together, although the character and greatness of the man lie in the whole. He was in his own right a cultural world of values, pattern, and distinctive style, a world that teaches that value and meaning may sometimes emerge more from the whole of a dedicated career than from any one striking event. This is perhaps the core of his significance for the future relations of linguistics and anthropology. Kroeber's work embodies the view that linguistic research is intrinsic to (and a responsibility of) anthropology. His work carries implicit definitions of linguistic anthropology as, simply and broadly, study of language within an anthropological context, and of the linguistic anthropologist as one who uses linguistic techniques and data to answer anthropological questions. Sometimes these questions are straightforwardly descriptive ('What is that language like?'), or classificatory ('Where does that language belong?'), sometimes more complex; but language being part of culture, language data are cultural data, and there is no necessary chasm between the study of the two. Almost any general anthropological question can be asked of language, some can be best

⁹⁸C. Lévi-Strauss, *L'Express* 32-3 (Paris, October 20, 1960).

asked of language, and some cannot be answered without the aid of language. The implicit definition of linguistic anthropology, the 'figure in the carpet' of Kroeber's linguistic work, accounts for the diversity of his studies, varying across phonetic detail, grammatical typology, semantic components, speech development of children, statistical subgrouping, and more. All were germane to anthropological questions. Kroeber is the best example to set against the attitude of some anthropologists that linguistics is something apart, reserved for those with a miraculous ear or the mind of a mathematical genius. He respected the rigor of linguistics, but he also practiced it wherever he could, and showed that the main thing is a sense of problem. The need in anthropology is not so much to give anthropologists a training in linguistic techniques, although that is important, practically and intellectually. The great need is for anthropologists to have a sense of anthropological problems in the data of linguistics. Where this sense exists, the rest can follow.

Kroeber's life was touched by sorrow, but not his death. His young brother had died while he was west on his first field trip, the news having to be brought him by pony rider, and his first wife died in 1913 after but seven years of marriage. But his second marriage in 1926 to Theodora Kracaw Brown was a long and happy one, and she and their four children (two adopted from her previous marriage) survive him. His last professional activity was partly a vacation trip for the two of them. The director of the Wenner-Gren Foundation, Paul Fejos, had urged Kroeber to lead one of the series of symposia that the Foundation had inaugurated at its summer headquarters, Burg Wartenstein, in southern Austria near the Hungarian border. Free to choose any theme and plan, Kroeber required no prepared papers, but under the heading 'Anthropological Horizons' brought together some very diverse individuals, essentially for the pleasure, and possible surprises, of having them discuss matters close to his heart. The setting, an old frontier castle, was toward the road part of a village countryside, while toward the valley it commanded a distant prospect of mountains, sometimes breathtaking with mist like that of a Chinese scroll, sometimes brilliantly clear in sunlight. No one enjoyed the discussions more than he, and an evening given over to local Austrian dances and songs found no one livelier in appreciation. At the final dinner he thanked the company, saying that he tended to judge intellectual activities by the pleasure they gave, and that he had enjoyed the conference greatly. His greeting on arrival had been, 'What's new at Berkeley?' and his words at departure were, 'Don't start anything until I get back.'

From Burg Wartenstein the Kroebers made their first trip to Paris. Tuesday, October 4th, was a day of reading and writing in the morning, a walk and drive to museums in the afternoon, with dinner in a favorite restaurant, then talk, and reading in a travel favorite, *War and Peace*. About 11:30 there came difficulty in breathing and forty-five minutes later he was gone. At his University the memorial service followed suggestions he had prepared with his wife, beginning with remembrances from friends, and ending with the playing of a movement from a favorite Mozart trio. He had chosen for reading poems from the *Book of Numbers* (6,26), Lao Tze, the *Bhagavad Gita*, Housman, and the California

Indians, the last ending:

Think of me now and again / As I was in life / At some moment which it is pleasant to recall. / But not for long. / Leave me in peace / As I shall leave you, too, in peace. / While you live, / Let your thoughts be with the living.

It was a life of the full-hearted savoring of life, that in its fulness and discreet boldness extended beyond life to the needs of others. Few men of eighty-four are cut off before their time, but Alfred Kroeber was, not only because of the work he would yet have done, but also because of the symbol and inspiration of the work and person to younger men. Young men who knew him in his later years thought of him as someone all young men should have the good fortune to know.

DELL HYMES

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**MORRIS SWADESH:
FROM THE FIRST YALE SCHOOL TO
WORLD PREHISTORY**

Morris Swadesh was born January 22, 1909, in Holyoke, Massachusetts. Like many great linguists, he was a "linguist" in the popular sense as well. His parents had come from the south of Russia, and from them he learned Yiddish and a little Russian. As an undergraduate at the University of Chicago, he concentrated on languages (German, French and some Russian). Later he was to work in Spanish and Tarascan, and to write guides to the learning of Russian, Chinese and Burmese.

Swadesh's progress through degrees was rapid. He received the Bachelor of Arts, and election to Phi Beta Kappa, at twenty-one (1930). Continuing at Chicago, he took the Master of Arts in a year (1931) working with Sapir on a project of comparative semantic analysis for the International Auxiliary Language Association and using Sapir's materials for his thesis on aspect in Nootka. When Sapir then went to Yale, Swadesh went with him. (Provision for Swadesh and Stanley Newman, also then at Chicago, is said to have been one of Sapir's conditions for accepting the call as Sterling Professor). As research assistant at Yale for the next two years Swadesh devoted himself to "a searching study of Nootka structure" (Sapir, in Sapir and Swadesh 1939:10), completing his doctorate at twenty-four (1933). He had already been published as junior author of a pioneer semantic monograph (Sapir and Swadesh 1932). A year after receiving his degree he had achieved intensive field experience with four languages in five successive summers; had joined in the orthographic changes (1934c) that were to symbolize the generation of Americanists emerging around Sapir and Bloomfield¹; had published the first systematic description of an American Indian language based on phonemic principles (1934b), and the first

1. Boas found the changes "quite unnecessary" (1947: 208, n. 5).

systematic American statement of phonemic method (1934a; cf. Joos 1957:37), the principle that was to be central to the development of linguistics as an autonomous discipline in the United States in the next decade. And he had presented a paper to the annual meeting of the Linguistic Society of America (December, 1934) concerning his work on Sapir's project in English grammar. This energy, enthusiasm for languages and the capacity for hard and detailed work on them, and the sense of pioneering in a cooperative enterprise, were to be characteristic throughout his life.

The First "Yale School"

Swadesh's career must be seen against the background of these years as student of and collaborator with Sapir. One hears of the "Yale School" of linguistics in reference to Leonard Bloomfield, who went to Yale as Sterling Professor in 1940, and more particularly in reference to the "neo-Bloomfieldian" scholars who dominated American linguistic discussion in the nineteen-forties and early nineteen-fifties. They published through *Language*, edited at Yale, and regarded Bloomfield as the founder of a scientific approach to language. From 1931 until his death in 1939, there existed an earlier "Yale School" centered around Sapir.

The two "Yale Schools" of linguistics differed in several respects. The concerns of Sapir's circle were more similar to those of linguists today than those of the intervening neo-Bloomfield era. Some possible misapprehensions must be forestalled, however. The two "schools" were not sharply opposed. In neither case was there homogeneity of view, but rather a great deal of close collaboration and discussion among persons diverse in background, outlook, and subsequent career. In neither case were all adherents present together at Yale. Elective affinity could be involved, in Bloomfield's case especially—his influence was far more through his book *Language* (1933) than through his teaching. Both Sapir and Bloomfield taught at the Linguistic Institute (in the summer of 1937 for Sapir, the summers of 1938–1940 for Bloomfield).² Certainly Sapir and Bloomfield held each other in high respect (cf. Sapir's choice of title for his paper, "The concept of phonetic law as tested in primitive languages by Leonard Bloomfield," and Bloomfield's remarks about Sapir in the dedication in Hoijer et al. 1946).³ A number of linguists studied with or were influenced by both men, and some admirers of Sapir shared in the outlook and concerns that dominated

2. For some indication of the impact of Sapir at the 1937 Institute, see Joos 1967: 9 (regarding effect on Bloch) and the dedication (p. 3) and pp. 53–54 in Pike 1967; for the 1937 and 1938 Institutes, Sturtevant 1940: 304.

3. I am indebted to R. A. Hall, Jr. and C. F. Hockett for comments on this point.

the “neo-Bloomfieldian” era. In particular, it would be anachronistic to read into the period before the Second World War the atmosphere that prevailed for a time after it, when the elevation of Bloomfield into a scientific savior was accompanied by a down-grading of Sapir as “intuitive” and “mentalistic” (derogatory terms then)—a genius, to be sure, but only that (cf. Joos, 1957:25, 31, 115; also 96, and 80b at end).

It remains to identify the group particularly associated with Sapir. The contributors to the volume in his memory (Spier, Newman, Hollowell 1942)—Hoijer, Voegelin, Haas, Swadesh, Whorf, Trager, Newman, Herzog, Emeneau—and the contributors to Hoijer et al. 1946—Hoijer, Swadesh, Voegelin, Haas, Li, Newman, Trager, Whorf, and Halpern, a student of Hoijer—serve as indication, as does the multiple authorship of “Some Orthographic Recommendations . . .” (Herzog, Newman, Sapir, Haas, Swadesh, Voegelin). Five had followed Sapir from Chicago to Yale (Walter Dyk, Mary Haas, Stanley Newman, Morris Swadesh in 1931, and George Herzog in 1932). Two had already obtained degrees at Chicago (Hoijer, Li). Li was later to visit Yale as Visiting Professor of Chinese Linguistics in 1937–39. Others were post-doctoral students at Yale (Emeneau, a Sterling Fellow in 1931–32 after his doctorate there in 1931; Voegelin in about 1933–35; Trager in 1936–38); one was non-doctoral (Whorf). Two men, Zellig Harris and Kenneth Pike, were never at Yale, but knew Sapir from professional meetings, the 1937 Linguistic Institute, and through association with others of the group.

The founding of the Linguistic Society of America (1924) and the launching of its journal *Language* (1925), Sapir’s “Sound Patterns in Language”, and Bloomfield’s “A Set of Postulates for the Science of Language” (*Lg.* 1:37–51 [1925] and *Lg.* 2:153–164 [1926]—both in Joos 1957:19–25, 26–31) can be taken as starting points for the development of structural linguistics and its successors (or transformations) in the United States. In the second quarter of the century, then, the prospect for young linguists, especially those inspired by Sapir, included these tasks:

- (1) to develop the methods of the nascent structural linguistics and to test their application in the analysis of both exotic and well known languages;
- (2) to sustain the profession of linguistics, where almost no recognition existed so far as departments, chairs, specific courses, and autonomy of the discipline were concerned;⁴
- (3) to continue to rescue disappearing languages;⁵

4. Bloomfield “always, on principle, advised inquiring students to specialize in other subjects than linguistics, on the ground that paying positions in the latter field were very few” (Sturtevant 1940: 304).

5. The mission was unequivocally supported by Bloomfield. In a statement for the conference and bulletin discussed below under (3), Bloomfield spoke of “future work: a national duty. If we fail, we shall be shamed before the judgement of posterity: we may be certain that it is by this kind of thing that future generations will judge us” (Letter in Freeman 1977; quoted in Voegelin 1949: 138).

(4) to pursue proof and establishment of genetic relationships among languages;

(5) to relate the results and methods of linguistic inquiry to other things—to other disciplines in the humanities and social sciences (Sapir once remarked that “Linguistics is the ‘chemistry’ of the humanities and the social sciences” [see p. 315 below; cf. Sapir 1929a]); to particular problems within these disciplines — such as cultural symbolism and patterning or personality and verbal art; and to practical affairs, such as education.

Other possibilities existed, especially the application of dialect geography to American English and the relating of the new discipline to the traditional philologies of both language and literature. The five possibilities sketched above pretty much define the framework within which those who looked to Sapir were to work. The focus of each might differ and change over time, but each was likely to see his or her own path as carrying out the heritage of Sapir. Certainly it was so with Swadesh. While he is best known now for his work on the relationships among languages, he contributed to all the other tasks sketched above, sometimes centrally.

STRUCTURAL METHOD

Swadesh was a leader in the first generation to develop modern linguistic analysis in the United States—the generation that came to the analysis of linguistic structure as something with a distinct methodology to be developed and learned. Boas’ general methodological stance and his interest in grammatical categories as mental phenomena had led him to clear away a priori approaches in favor of inductive analysis. Most immediately in the United States, as one scholar has put it, there was a man and a disembodied book, Sapir and Bloomfield’s *Language* (1933), twin foci for a sense that something new was happening.⁶ And there was the small number of young scholars who chose linguistics, rather than languages, literatures, philology or some other aspect of anthropology, as their uncertain career.

Phonology. It is said that young linguists today read nothing from the years before 1957 (the date of Chomsky’s *Syntactic Structures*). Certainly the rejection by many of an autonomous sphere of phonology, and even of a distinct phonemic zone for the representation of utterances, may make it difficult to grasp the idea of phonemic pattern as a “breakthrough” and to appreciate a major contribution to institutionalization of the idea as significant. Yet in the earlier period (as in part later), theoretical controversy,

6. There was also knowledge of Trubetzkoy and Jakobson, already at work in Prague to develop the new approach systematically. Young Americans perhaps did not then read de Saussure, whose posthumous book had set the goal of synchronic analysis of structural relations, but Sapir and Bloomfield had read him, and Trubetzkoy and Jakobson were obviously a development from such a base.

alternative conceptions of science and the goals of linguistics, and the future of linguistics itself centered on the concept of the phoneme. These existed in Europe an international group to promote the phonological description of languages (with Roman Jakobson as secretary), and to promote the same goal and cooperate with the international group, there was formed a Group for Phonemics within the Linguistic Society of America (headed by Sapir as Chairman, with Kurath, Twaddell, and Kent). As late as the 1940's the notion of phonemic (phonological) analysis was controversial. It is not that its opponents had in mind a superior conception of phonology. The point is rather that phonological analysis constituted a revolution against a study of language without it. In important part it was, as Sapir's "Sound patterns in language" makes clear, a revolt against dichotomization of the synchronic study of language into two disjunct realms, one a natural science (*Naturwissenschaft*) of phonetics, the other a social or human science (*Geisteswissenschaft*) of grammar. Phonology was the sphere in which structural analysis achieved its first recognized success, a success the consequences of which are still spreading (cf. Lévi-Strauss 1945).

Swadesh's central role may be indicated by a few citations:

"I am indebted to Edward Sapir and Morris Swadesh for instruction in phonemic theory and its application to the Shawnee language" (Voegelin 1937:23, n.1).

"Swadesh published *The Phonemic Principle*, which became fundamental to American work in this field" (Haugen 1958: 498).

"The early work of Sapir, showing how two different systems could be created from a single set of phonetic elements . . . ; the careful discussions of Bloomfield . . . , and the methodological work of Swadesh showing how phonemic principles were to be applied and how interpretation of long long consonants grew out of such principles . . . largely comprise the basis upon which my own understanding of phonemics grew." (Pike 1967: 345-6; cf. 54.)

In the study that was to be the starting point of epistemological debate, Twaddell, (1935) despite their methodological differences, conceded that Swadesh's (1934a) was: "the most comprehensive and methodologically lucid treatment of the phoneme with which I am acquainted. Swadesh has recognized and avoided many of the palpable weaknesses of earlier writings on the subject" (in Joos 1957:66). Other papers that were to be seen as fundamental to the later development of the "neo-Bloomfieldian" position acknowledge his influence. In "Phonemic Overlapping" (1941), Bloch cited Bloomfield, Swadesh, Twaddell and Trubetzkoy for general principles, and Swadesh (1934a, 1937b) on pattern congruity (n. 1, n. 7). In "A System of Descriptive Phonology" (1942), Hockett cites Trager and Bloch for chief stimulus, Mary Haas for specific criticism, and also "Morris Swadesh for many suggestions in the past" (n. 1). A paper to which Swadesh contributed the methodological insight (1939c: 2) has been cited as one of "the two germinal papers in American morphophonemic theory" (the

other being Bloomfield's "Menominee Morphophonemics" of the same year in Hockett 1967: 208, n. 2).⁷

In these years the phonemic interpretation of the syllabic nuclei of English was a particular focus of discussion, and to a great extent a symbol of the new linguistics. In a constructive critique of Bloomfield's treatment, Swadesh (1935a) analyzed the syllabics as all unit phonemes, as Trubetzkoy was to do shortly afterward. This short paper, arising out of work on Sapir's English grammar project (cf. n. 19), for some years defined one position (cf. Pike 1947a). Swadesh continued to be concerned with this question, and an incident reported from the 1941 meeting of the LSA in Indianapolis reveals something of the man and of his part in the climate of the time.

"While the room was being prepared for lantern-slide projection, it was agreed at the suggestion of Mr. Swadesh to devote half an hour to a special discussion of the syllabic phonemes of English. Mr. Bloch was requested to lead the discussion and to present in condensed form the interpretation set forth by Mr. Trager and himself in a recent article in *Language* 17: 227-46. Mr. Ward . . . [Sturtevant, Allen, Jr., Swadesh, Bloch, Hockett, Trager, Harris] and Mr. Kent participated in a lively discussion, which it was necessary to interrupt in order to continue the meeting, but which it was agreed should be continued at the close of the session if time permitted" (*LSA Bulletin* 15: 24(1942)).

Swadesh was persuaded by some of the evidence put forward by Bloch and Trager to accept analysis of the syllabics, but not to analyze the non-short vowels into vowel plus consonant, nor to assume a vowel variant of *h* in syllable final. In the course of this argument, Swadesh presented the first published version of what was to become famous as the "overall pattern" of nine elementary vowels for English:

7. Hockett's note begins: "Although not published until 1944, Newman's description of Yokuts was essentially completed by 1936. This antedates the two germinal papers." This generous tribute to Newman for priority of merit, if not of influence, misses the fact that the Swadesh and Voegelin paper was itself "written some years earlier" (Harris 1951: 293, n. 10) and was presented at the Philadelphia meeting of the LSA in December, 1937. Swadesh and Voegelin themselves remark (1930: 2-3) that "Swadesh, having learned the use of formulae in synchronic phonology from Sapir in his work on Nootka...". (And Voegelin & Voegelin (1963: 18) remark that the paper "was written by two authors under Sapir who found time to discuss every problem of the paper as it arose..." and go on to "speak of Sapir as a collaborator (the silent partner)". [Swadesh (1934a: 129) had already used the notion and term "morphophoneme"]. Newman (1944: 6) notes that his first published account, written shortly after the field trip of 1930, is brief and schematic (*IJAL* 7:75-89 (1932)) — it indicates vowel formulae for stem forms (88), but is paradigmatic in format like Hockett's 1967 restatement, not abstractly morphophonemic; that in 1932 a short grammar was submitted as a dissertation at Yale; and that the Yokuts grammar in its present form was largely completed by 1936, as a revised and expanded version of the doctoral dissertation. In short, his abstract morphophonemic treatment was arrived at while working at Yale with Sapir, where he was a Research Fellow of the Institute of Human Relations 1932-37, in the same period that the formulaic morphophonemic treatment of Nootka was arrived at by Swadesh also working at Yale with Sapir. Indeed, in 1934-36 Newman and Swadesh were working together with Sapir on the English language project, and in a wellknown review article of Newman's Yokuts, Harris (1944: 196) described the grammar as an example of Sapir's mature methods. In sum, American abstract morphophonemics would seem to have its origin in the "First Yale School" in the post-doctoral collaboration about 1933-36 of Newman and Swadesh with Sapir. Newman deserves the credit that Hockett gives him, but as part of a larger scene.

"The number and nature of the short-bound [bound, as occurring always before another phoneme] syllabics vary from dialect to dialect, but it is nevertheless possible to give a composite maximum pattern that covers all the types ordinarily found in English. This is a nine-vowel scheme of three levels (high, mid, low) and three front-to-back series (front unrounded, central, or back unrounded, back rounded):

i	ə	u
e	ʌ	o
ɛ	a	ɔ

"In our general survey of English syllabics, we found . . . an overall maximum system of nine short-bound elements. However, it is unlikely that any one dialect will be found to illustrate the full range of types" (1947c: 142, 143).

The origin of the nine-vowel maximum pattern may have been with Trager, since Swadesh, surveying patterns of Scottish, Greenville, S. Carolina, Chicago, New Jersey, and Southern British, makes use of information from Trager for the New Jersey variety (and from Raven McDavid for Greenville). Indeed, the first published mention of Trager's changed view appears in a footnote by Swadesh.⁸ Referring to Trager and Bloch (1941), Swadesh writes: "The description published there has been supplemented by additional information received in correspondence and discussion with Trager. (Note that although Trager and Bloch originally posited only six vowel phonemes, they now recognize a maximum of nine." (1947c: 141, n. 6.)

Swadesh himself proposed an analysis of complex English syllabics as consisting not of vowel plus semi-vowel (w y h) but of vowel plus vowel.

Grammatical semantics. To focus on phonology, however, is too much the sort of history victors write, and only part of the story. Influence on what was seen as important after the war, continuity and cumulation, are important; but there was discontinuity as well. During and after the war the dominant spirit of American linguistic analysis was to reject formulations of linguistic relations in terms of process, and to reject "mentalism" and "intuition" in one's work. Process formulations were a spirit driven out by Sapir's disciple Harris, as Joos (1957: 115) would have it, only to return with a vengeance with Harris' disciple, Chomsky, along with trust in the

8. Swadesh's paper is cited anonymously in the 1950 presidential address of the Linguistic Society (Haugen 1951, n. 21). The Trager and Swadesh analyses of English are given as prime examples of the complaint that "the difficulty of nailing a purely distributional analysis down to anything concrete has been evidenced in the constant change of conclusions concerning given language systems by practicing analysts". The complaint ignores the fact that Trager and Swadesh had encountered new facts, that Swadesh explicitly states the facts and considerations that convinced him he had been wrong, and that a criterion of phonological identity (Haugen's main concern) as well as distribution was in fact used by Trager and Bloch and was a basis for Swadesh's alternative.

judgments of native speakers and in intuition. This turn of the wheel, associated with phonology and Sapir, is well known. Process formulations, intuition, and trust in native speaker judgments were the controversial hall marks of Sapir's work. It is little known that Sapir's mentalism in phonology was matched by a serious "mentalistic" attempt to deal with *semantic* description, in terms both of categories specific to languages and of features and logical relationships that are universal. Both concerns, the specific and the universal, were part of the Boasian approach. Boas developed the first in his own work; Sapir and members of the "First Yale School" developed the second.

An inspection of grammatical sketches by Newman, Swadesh, Haas, and Whorf in a volume planned by Sapir (Hojjer et al. 1946), as well as of their separate treatments of Yokuts, Nootka, Tunica, and Hopi, will show attention to semantic definition of formal categories, such as the semantic features inherent in the unmarked verbal stem, and particular attention to aspect. (Cf. Swadesh 1931, Whorf 1936b, 1938, and the flurry of discussion of a 1935 LSA paper on verbal aspect joined in by Haas, Newman, Swadesh, and Whorf [*LSA Bulletin* 9: 4(1936)].) Whorf's famed treatment of Hopi arose not only out of his experience as an insurance inspector and his analysis of Hopi, but also out of a collaborative ambience at Yale. Swadesh's prior work on aspect, and a general interest in the category of aspect on the part of Sapir, may have influenced Whorf, who made much of the prevalence of aspect and absence of tense in Hopi. In his revised Nootka study Swadesh states: "The semantic interrelation of the Nootka aspects may be summarized in terms of the three processes, namely, linearization, punctualization, pluralization." (1931-32.) All three were to be important dimensions for Whorf in his study of Hopi. Whorf's "A Linguistic Consideration of Thinking in Primitive Communities," written in late 1936 (Carroll 1956, Hymes 1964), acknowledges the tradition of Boas and Sapir, and in its notes cites Haas, Newman, Swadesh and Trager, the first three from then unpublished materials (Carroll 1956: 86). His "Grammatical Categories," written at Boas' request late in 1937 and published posthumously in 1945, states (n. 1): "The author wishes to acknowledge his indebtedness to his colleagues, Dr. George L. Trager and Dr. Morris Swadesh, with whom some of these questions of category have been discussed" (Carroll 1956: 87). Besides well known languages (Sanskrit, English, Latin, French), the paper cites the American Indian languages analyzed by Sapir and his students: Algonquian (Voegelin), Southern Paiute (Sapir), Tubatulabal (Voegelin), Taos (Trager), Navaho (Sapir, Hoijer), Nitinat (Haas and Swadesh), Yana (Sapir), Aztec (Whorf) and of course Hopi (Whorf). A collective body of knowledge seems to have been drawn upon.

The impression has sometimes been given that Whorf was an extreme relativist, or at least interested only in the distinctive differences of languages. A reading of his work (Carroll 1956) shows that impression to be false. Whorf, like Sapir and Boas, was concerned to do justice both to what was

distinctive and to what was universal in a language. With Boas, Sapir, and Swadesh, he was concerned to develop more systematic and precise ways of describing the semantic structure of particular languages, and to do so in ways that would make comparison and universal statement possible as well.⁹

Swadesh's principal personal contribution to this work, *The Nootka Aspect System* (1931), was never published. This study, the comparative part of his paper on Chitimacha derogatory verbs (1933b), and his work in 1930–31 on the ending point relation in English, French and German, are part of a major impulse given by Sapir to "detailed study of the variety of ways of expressing logical relations in language" (Sapir and Swadesh 1932 p. 3), with an understanding of universal characteristics as the practical and scientific goal. But the rise of a self-conscious, autonomous, linguistic science around phonology, morphology, and syntax was to make semantics for a generation appear part of some other science, or at best, part of an interstitial field such as "ethnolinguistics". Anti-mentalism and divorce from meaning may have been popular in part as convenient ideology to shelter the working out of the formal descriptive problems of immediate interest. The post-war climate of opinion, however, did not eradicate its predecessor, so that Chomsky was both to inherit a conception of meaning as secondary and to look to the seventeenth and eighteenth centuries for precedent for his mentalistic interests. There is a precedent just as reasonable not more than a generation back, in the views of Sapir and the work of the nineteen-thirties at Yale.¹⁰

A LINGUISTIC PROFESSION

Swadesh joined the Linguistic Society of America when he began graduate school in 1931, becoming a Life Member in 1937. He was an active member,

9. Cf. "Language: Plan and Conception of Arrangement," circulated among colleagues in 1938 and intended as a supplement to the *Outline of Cultural Materials* prepared by Murdock and others at Yale (Carroll 1956: 125–133), and the conclusion of the 1937 paper on "Grammatical Categories" written for Boas, referring to "generalizations of the largest systemic formations and outlines found in language when language is considered and described in terms of the whole human species." (Carroll 1956: 101).

10. Note that Sapir carried on a tradition that distinguished between "surface" and "underlying" grammatical relations, just as he distinguished between observed and "underlying" phonetic relations. In his Takelma grammar—the fullest and finest published Amerindian grammar before the rise of selfconscious structural linguistics—he noted, for example (1922: 181): "It is the logical unexpressed subject of a passive sentence, not the grammatical subject (logical and formal object), that is referred to by the reflexive possessive in -gwa." Just these sorts of observations "provide the primary motivation and empirical justification for the theory of transformational grammar" (Chomsky 1965: 70). Sapir did not know or develop the formal machinery for handling such relations on other than a case-by-case basis, but the fundamental insight continued into his work on the logical relations underlying the overt expression of notions such as "ending-point." His Takelma grammar, indeed, served as model for at least one of his students, Harry Hoijer (personal communication). It would seem to be the concern with distributional relations of the 1940's that drove out the "spirit" of underlying relations, not as a necessary consequence but because of the empiricist ideology accompanying it.

presenting papers¹¹ and participating frequently in the discussion of other papers, as shown in the accounts of meetings in *Language* and the *Bulletin* of the LSA.¹² At the age of thirty he was elected to the Executive Committee. (Because of his work in Mexico, he was represented at the 1939 December meeting by Trager and McQuown.) He was a member of both special interest groups formed within the LSA in 1937, the "Group for Phonemics" and the "Group for American Indian Linguistics".¹³ He served on the Managing Committee of the latter with Franz Boas as Chairman, Leonard Bloomfield, Alfred L. Kroeber, and Edward Sapir. His belief in cooperative effort to meet the needs of linguistics and his willingness to work hard to this end are evident in these early years. The proceedings of the 1935 meeting of the LSA (*Bulletin* 9: 16 [1936]) report of this young Ph.D. that: "Mr. Swadesh called attention to the need for cooperation in bibliographies,

11. Papers given: "The Phonemic principle," Washington, D. C., December, 1933 (*Lg.* 10: 81 [1934]); "English Parts of Speech," December, 1934 (*Lg.* 11: 65 [1935]); "Proverb Displacement in Chitimacha," Philadelphia, December, 1937 (*LSA Bulletin* 11: 4 [1938]); (C. F. Voegelin and ———). "Morpho-phonemics and Variable Vowel-length in Tubatulabal," Philadelphia, December, 1937, (*LSA Bulletin* 11: 4 [1938]); "Composite Words in Chitimacha and English," December, 1936 (*LSA Bulletin* 10: 17 [1937]); "Problems of Mohican Phonology," December, 1938 (*LSA Bulletin* 12: 25 [1939]); "Foreign Influence: The Bifurcation of Phonemes, as illustrated by Recent Tarascan and Middle English", December, 1941 (*LSA Bulletin* 15: 25 [1942]); "On the Analysis of English Vowels," Chicago, December, 1946 (*LSA Bulletin* 20: 16 [1947]); "Synthetic Trend in Nootka," New Haven, December, 1947 (*LSA Bulletin* 21: 19 [1948]); "An Experiment in Remote Comparative Linguistics," New York, December, 1951 (*LSA Bulletin* 25: 6 [1952]); "Mosan as a Problem in Remote Common Origin," Bloomington, Indiana, August, 1952 (*LSA Bulletin* 26: 5 [1953]); "Time Depth Problems in the Classification of American Languages," Cambridge, December, 1952 (*LSA Bulletin* 26: 20 [1953]).

12. Interventions by Swadesh occurred on many topics—phonemic method, American Indian languages, Indo-European and Hittite, child language. Recall the incident quoted above from the 1941 meeting regarding English syllabics, and see the Proceedings of the annual meetings of the LSA in *Language* and the *Bulletin* of the LSA:

- 1932 *Lg.* 9: 104, 105 (2 papers), 113, 115 (1933);
- 1933 *Lg.* 10: 70 (2), 72, 81 (2), 82 (1934);
- 1934 *Lg.* 11: 54, 63, 64 (3), 65 (1935);
- 1935 *Bull.* 9: 4, 5, 17 (2), 18 (1936);
- 1936 *Bull.* 10: 16 (3), 17 (1937);
- 1937 *Bull.* 11: 4, 6 (2), 20 (2) (1938);
- 1938 *Bull.* 12: 21, 22, 23 (2), 24, 25 (3) (1939);
- 1941 *Bull.* 15: 24, 25 (4), (1942);
- 1946 *Bull.* 20: 16 (5), 17 (2) (1947);
- 1947 *Bull.* 21: 18 (3), 19 (2) (1948);
- 1948 *Bull.* 22: 16 (2), 17 (1949);
- 1949 *Bull.* 23: 21 (1950);
- 1950 *Bull.* 24: 16 (4), 17 (1951);
- 1951 *Bull.* 25: 16 (2), 17 (4), 18 (1952).

13. The careers of the two groups are outlined in the reports of their establishment (*Lg.* 13: 257, 258 [1937]) and in a few comments in the Report of the Secretary of the LSA, until the first decided to discontinue for the time (*Bulletin* of the LSA 15: 16 [1942]), and the second was discharged with thanks (*Bulletin* 22: 11 [1949]). The membership of each was published at the end of the general membership list of the Society: *Bulletins* 11: 53–54, 12: 61–62, 13: 67, 14: 62, 15: 57–58 (1937–41) for the Group for Phonemics; *Bulletins* 11: 54, 12: 62–63, 13: 68, 14: 63, 15: 58, 16: 44–45, 17: 42–43, 18: 45–46, 19: 43–44, 20: 45–46, 21: 49 (1937–1948) for the Group for American Indian Linguistics.

especially in linguistic bibliographies, so as to cover the field completely, and also to eliminate needless bibliographies and those of poor quality." As part of the surge of effort to support work on American Indian languages, Swadesh sought to implement his own advice, launching in *Language* a program that was intended "to furnish complete annual lists of books and articles on American Indian languages, with brief indication of contents" (*Lg.* 14: 319 [1938]); cf. *LSA Bulletin* 12: 10 [1939]). Readers were asked to supply omissions, authors to send offprints, especially such as might be overlooked, and all cooperation was appreciated. When funds for the *International Journal of American Linguistics* seemed lacking, he conceived a plan for a dues-paying society to continue it. At the Chicago meeting of the LSA in 1936, at the request of Chairman Sapir, he acted as secretary to the group that discussed a proposed Society for American Indian Linguistics (*Bulletin* 10: 17 [1937]). From this grew a major though little known effort in the following year.

After ten years of existence and in the midst of a depression, the Committee on Research in American Native Languages found supporting funds running out. Since launching it under ACLS sponsorship in 1927, Boas had been able to sponsor a considerable amount of work by a considerable number of new recruits to linguistics and the Amerindian field (mostly students of Boas and Sapir). In 1935, 1936 and again in 1937, however, there were no funds for field work. Prompt working up and publication of materials was also frustrated by lack of funds.¹⁴ The ACLS appropriated \$1000 for a conference on April 25–26, 1937, to consider the state and needs of the study of native American languages and to formulate plans for further work (*LSA Bulletin* 10: 17 [1937]). The American Anthropological Association and the Linguistic Society both selected representatives. From this meeting came a request for discharge of the old Committee, organization of a new Continuing Committee (listed above as a group of the LSA) (*ACLS Bulletin* 27: 64–65 [1938]), and a series of recommendations and decisions, including the preparation of a comprehensive statement for possible publication by the ACLS as a bulletin (*AA* 39: 733 [1937], *Lg.* 13: 257–8 [1937]). Swadesh was Secretary of the new Committee, and most of the work, including the bulletin, fell to him. He devised an outline for a general statement of the history, character, importance and needs of the scientific study of aboriginal languages of America, together with appendices on several topics to be contributed by Sapir, Bloomfield, Herzog, Reichard, Cassirer, and others (Boas to Swadesh, June 14, 1937). Swadesh wrote for contributions and information (to Kroeber, Michelson, and others),

14. In his report for 1935 he listed urgent needs for 1936–7, all six of them projects of completing analysis for publication (*ACLS Bulletin* 25: 88 [1936]). "In regard to Nos. 4 and 5, until October 1936, Newman and Swadesh will be engaged in the project of English Grammar of Sapir. I am exceedingly anxious that they should go back to their field notes and work these out."

integrated the replies,¹⁵ and prepared the final manuscript. On November 22, 1937, Boas wrote to Swadesh in his usual terse style: "Will you be good enough to let me know about the status of the work you did for the committee on Indian Languages, whether the MSS are completed, in process of completion, or ready for the printer, and what they are?" On February 21, 1938, Boas wrote again: "At the meeting of the Council of Learned Societies it was decided not to print the report submitted by our Committee, but to prepare another report especially designed to interest foundations in our work and Professor Bloomfield was requested to prepare such a report. Yours very truly, Franz Boas." In the end, the *ACLS Bulletin* carried a trenchant statement by Boas and a succinct history by Kroeber.

The *Bulletin of Information on the Scientific Study of Aboriginal Languages* (Swadesh 1937a) remained in Boas' possession and came with his papers in 1943 to the Library of the American Philosophical Society. Two of the appendixes prepared for it were separately published, Bloomfield's "Philosophical Aspects of Language" (1942), and posthumously, Sapir's "The Relation of American Indian linguistics to General Linguistics" (*South-western Journal of Anthropology* 3: 1-4 [1947]). Swadesh's own text remains unpublished, testimony to a young man's energy and devotion in an effort to sustain part of the tradition of Boas, Kroeber, Bloomfield, and Sapir, and to the role assigned him at age twenty-eight in recognition of those qualities.¹⁶

FIELD WORK

From the time he became a graduate student in linguistics in 1930, every year of the ensuing decade saw Swadesh engaged in fresh field work, during almost every summer season and at other times as well. In the summer

15. E.g., "RFD a, St. Helena, Calif, 6/28/37. Dear Swadesh: Your outline looks fine. I enclose the data you ask for. Will you have them typed and send me a carbon for my record? I shall be up here till August 20, then back at Berkely. Sincerely, A. L. Kroeber."

16. The *Bulletin* and associated materials are to be found as Freeman 1977, 497.3/ B63c/Anc, and in Boas Papers, Miscellaneous B/B61 (the relevance of the latter was noticed by Dr. Alexander Lesser, to whom I am indebted for calling it to my attention). I have used also the reports by Boas, "Reports of Progress, 1931" *ACLS Bull.* 18: 74-77 (1932), and correspondingly, 20: 88-89 (552-553) (1933), 22: 112-114 (242-244) (1934), 23: 88-90 (455-457) (1935), 25: 85-88 (743-746) (1936), 26: 74-77 (74-77) (sic) (1937), and 27: 633-635 (219-221) (1938) for years 1932, 1933, 1934, 1935, 1936, 1937. (The pagination: in parentheses is the continuous pagination of the volume, that before parentheses of the issue.) See also Boas 1939 and Kroeber 1939.

17. C. F. Voegelin was also in the group; c.f. Jacobs 1931: 87, 97. Materials from the Nez Percé (and some Sahaptin) work are deposited in the Library of the American Philosophical Society; see entries 567, 568, 2397 in Freeman and Smith 1966. (The "Cayuse" of the heading and main entry for 567, 568 is Nez Percé, as the note to 567, "Nez Percé Language as Used by Cayuse Indians of Oregon" but not to 568, explains. Despite Jacob's efforts to locate Cayuse speakers, the language was by then extinct.) Materials in the Library of the APS from subsequent field work of Swadesh are identified by the short form "Freeman" and the entry number in brackets. I have identified entries, partly because under "Swadesh . . . Field Notes," the Freeman index indicates only a portion of the entries that represent his field work, and partly to call attention to this important source of information.

of 1930 he worked on Nez Percé at Umatilla Reservation, Oregon, under the auspices of the Laboratory of Anthropology (Santa Fé) and the direction of a leading Boas graduate, Melville Jacobs.¹⁷ In the summer of 1931 he and Mary Haas Swadesh worked in southern British Columbia on Nitinat [Freeman 2401], a “remarkably deviating dialect [actually, a separate language] of the Nootka” (Boas, *ACLS Bulletin* 18: 74 [1932]). The summers of 1932 and 1933 were spent near the Gulf of Mexico in Louisiana, salvaging Chitimacha [Freeman 730–736] from its last two capable speakers, the best of whom died in 1934 [Freeman 730–736].¹⁸ Some Penobscot and Malecite material was obtained in 1933 with C. F. Voegelin [Freeman 2102, 2946]. The summer of 1934 saw work in New Haven with Alex Thomas, Sapir’s Nootka informant, producing a “large amount of new material—lexical and grammatical” (Sapir, in Sapir and Swadesh 1939a: 10). In September, 1934, Swadesh and Mary Haas Swadesh collected Atakapa, Alabama-Koasati, Creek, and Biloxi materials in a survey conducted through southern Louisiana, East Texas, and eastern Oklahoma (Haas 1968; Freeman 423, 1890, 463; Haas had worked on Tunica in 1933 while Swadesh worked on Chitimacha, and had recorded some Chitimacha herself). The years 1934–36 saw work on Sapir’s project, “A Descriptive Grammar of English,” to which Swadesh contributed at least analyses of parts of speech and of vowel nuclei.¹⁹

At some point in 1936 an Eskimo came to New Haven and Swadesh promptly obtained material that was to become the basis—together with his “Structural Restatement” (Voegelin 1954: 441) of Kleinschmidt’s nineteenth century Greenlandic material (1946a)—of an important series of papers on Eskimo (1951c, 1952c, d, e, f; Freeman 1355–1358). The first of these papers reveals something of his method of work:

The Unaaliq material was obtained from James Andrews, who visited New Haven, Connecticut in 1936 as a participant in the Sportsmen’s Show. Since Mr. Andrews had only a few days to spend, it was not possible to obtain more than a vocabulary of about 500 words, a few paradigms, and a short text. Following my usual work procedures, I recorded rapidly, reserving the verification of subtle

18. Freeman 731 refers to texts from 1931, and 736 to cylinder records from 1931, while 735 identifies field notes as 1930–34. All other information available to me gives the summers of 1932 and 1933 and some portion of 1934 as the time of Swadesh’s work on Chitimacha. (*ACLS Bull.* 20: 88 [December, 1933]; *ACLS Bull.* 22: 112 [October, 1934]; Swadesh 1933b: 192; Swadesh 1934b: 345, n. 2; Swadesh 1937g: 76). The notebooks now in the APS themselves contain no indication of date. The typed sheet and external writing appear to be in the same form as other materials indexed by Swadesh about 1950 for the Library and in part in his own hand. I cannot explain this discrepancy.

19. Swadesh’s part is reflected in his paper at the December 1934 meeting of the LSA, “English Parts of Speech”, (*Lg.* 11: 65 [1935]). Cf. Sapir’s Report of Progress, 1934 (*ACLS Bull.* 23: 125–127 [June 1935]), in which stress and parts of speech are noted. See also “The Vowels of Chicago English”, *Lg.* 11: 148–151 (cf. Sapir’s Report of Progress, 1935 *ACLS Bull.* 25: 117–120 [1936]). The other published result of the project known to me is Stanley Newman’s “On the Stress System in English”, *Word* 2: 171–187 (1946).

phonetic details for later check and recheck. However, the brief period of work with Mr. Andrews prevented a final check of all points. Some errors remain, particularly in the distinction of palatal . . . (1951c: 67).

The pattern is typical of Swadesh—to push ahead while frankly confessing limitations, regarding perfection vs. silence as a false choice. His work on Eskimo was in fact to make clear for the first time the existence of a language boundary between *two* Eskimo languages, and to substantiate the classification, maintained on authority alone, since Powell's time (1891) of Eskimo-Aleut (1951c).

During this period Swadesh had of course been engaged in intensive analysis of Nootka, with an eye on Nitinat (Haas 1969: 109, n. 8) as well, and had been analyzing and preparing for publication his material on Chitimacha. After six years at Yale,²⁰ he accepted a position as Assistant Professor of Anthropology and Linguistics at Wisconsin. There he worked for two years on Mohican, an Algonquian language, collecting his own materials and making interlinear translations of earlier liturgical literature [Freeman 2080–2083]. He also obtained field notes on the Siouan language, Catawba, on Algonquian Potawatomi (1937—Freeman 551, 3020), and on Algonquian Chippewa and Menominee (1938—Freeman 2546, 2166–67);²¹ Bloomfield prepared a summary of Menominee inflections for his use [Freeman 2161]. At some point he found time to record Chinese from the anthropologist Li An-che (celebrated for his view through non-Western eyes (1937) of the Zuñi made famous by Ruth Benedict's *Patterns of Culture*), and to work up a phonemic sketch as his contribution to the memorial volume for Trubetzkoy (1939e). He suggested that his mode of presentation would be useful for the proposed phonemic survey of the languages of the world.

In 1939 Swadesh began work on an Iroquoian language, Oneida, but his work was cut short by a sudden opportunity to go to Mexico. There, while plunged into a new and demanding role (see below), he managed to collect vocabularies in a variety of languages: Huichol (Freeman 1601), Kickapoo (Freeman 1888), Mixteco (with Norman McQuown [Freeman 2255]), Nahuatl (Freeman 2349 and 2350, the latter with Adrian León), Papago (Freeman 2647), Tzeltal (Freeman 3826), and Zapotec (Freeman 3975–3977).

RELATIONSHIPS AMONG LANGUAGES

The first published indication of comparative inquiry by Swadesh came in re-

20. In 1931–33 as research assistant to Sapir; 1933–34 as ACLS Fellow; 1935–36 as associate on Sapir's grammar of English (an ACLS project); 1936–37 as Instructor.

21. Swadesh apparently was of some assistance to Hockett in the latter's field work on Potawatomi during the summers of 1937 and 1938 (Hockett 1948: 1, n. 1). He also heard Menominee with Bloomfield before beginning his own work; a letter from Bloomfield to Bloch (28, December 1940) states in an answer to questions about the language: "Swadesh, recording more accurately as to ear than I, and not knowing the language, confirmed the above in main outline" (Hockett 1970: 368).

views of 1935 and 1937 (cf. n. 27). His first detailed comparison was undertaken at the time of his Mexican work as part of the preparation in 1941 of a chapter published later (1949a), collecting himself many of the vocabularies and paradigms used (1947d: 220, n. 1; cf. Freeman 3974). His major historical work followed the Second World War, and will be discussed as a whole below. It followed Sapir's death, but was a continuation or revitalization of one of Sapir's major interests and contributions. Whorf and Trager had made some advance in the 1930's along lines sketched by Sapir in Uto-Aztecan and Tanoan and gone on to link that group with Penutian, but the work lay dormant. The two great periods of intense progress in Amerindian linguistic relationships had been at the Bureau of Ethnology under Powell and Henshaw in the 1880's, and around Kroeber at Berkeley, Sapir at Ottawa, and others of the newly emerged academic generation of anthropologists in the 1910's. In the post-war period, with neither government nor academic post, Swadesh was to be the controversial center of a third.

OTHER RELATIONSHIPS

Like Boas and Sapir and their other students, Swadesh was from the first attentive to social and cultural data that emerged in the course of linguistic work and interested in the relation of linguistic features to cultural patterns of expression (cf. 1933b). His article on verbs of derogation viewing linguistic form from the standpoint of an expressive function is of a kind rare among American linguists. Indeed, his historical work has an extra strength not usual among Americanists in its attention to expressive symbolism (e.g., in 1960j, 1962c; note support in the findings of Hymes 1957: 86-87 and Jacobsen 1969: 142-3, 144 for proposals by Swadesh [1956b and 1953a, respectively]). His studies of the patterning of phonetics among bilinguals (1941c) and of obsolescent languages (1948e) are pioneering contributions, and his work in the analysis and presentation of Nootka songs (1955d) and texts, including an ethnographic analysis on a topic of some general importance (1948c), is invaluable.

It would be incorrect to speak of a systematic "ethnolinguistics" or "sociolinguistics" for Swadesh, or others of the first Yale school, but it is important to note their readiness to notice and pursue a point of cultural pattern or social relevance. A concern for the relation of linguistics to public and practical affairs can be found throughout Swadesh's work. Such a concern was no stranger to the linguistics of the thirties. There was Sapir's semantic work for an international auxiliary language, and Bloomfield's development of a method of teaching reading on a linguistic basis. In a related vein, Sapir observed at the 1937 meeting on American Indian linguistics [Freeman 1977]: "Linguists do not realize the interest of scientists in various fields in matters of language. These scientists on the other hand do not realize that scientific linguistics concerns itself with matters of interest to them. They have a notion that linguistics is hopelessly exotic, whereas actually our interests overlap theirs to a large extent . . . Linguistics is the

'chemistry' of the humanities and the social sciences." Whorf interpolated, "Linguists should take it on themselves to make these things known by articles placed in various journals." One can glimpse the beginning of Whorf's famous series of articles in *The Technology Review*.

Swadesh wrestled with the relation of scientific work to social relevance in writing the main text of the Bulletin commissioned by that meeting (pp. 5-7, on "Practical Value"). A year or two later his concern reached print: "In one thing the Mexican Institute differs greatly from the glottologic interests in this country; its goal is social as well as scientific . . . Practical value and scientific interests are not necessarily contradictory" (1939m: 120). His main opportunity to act on his concern was to come shortly. At Wisconsin he had initiated a WPA Language Project in 1939, but it was left in the hands of Floyd Lounsbury when he accepted a call from the government of President Cárdenas of México to head a program of Indian education, undertaken by the new Department of Indian Affairs (Departamento de Asuntos Indígenas).

The next two years laid the foundation of Swadesh's long and intimate association with linguistics and anthropology in México. They were extraordinarily active years. He was Professor at the Instituto Politécnico Nacional de México and the Escuela de Antropología (1939-1941), Director of the Consejo de Lenguas Indígenas, Director of linguistics in the Departamento de Asuntos Indígenas, and President of the Linguistic Section of the Twenty-Ninth International Congress of Americanists (México City, 1939); he gave "Cursos de Técnica de enseñanza para profesores en zonas indígenas" [Course in instructional techniques for teachers in indigenous zones] and "Cursos de alfabetización para alumnos indígenas" [Course in alphabetization for indigenous students] for the Departamento de Asuntos Indígenas [Department of Indian Affairs] in Pátzcuaro, Michoacán (1939-40), and lectured at the Universidad de Primavera, Morelia, Michoacán in 1940. All the while his central concern was the Tarascan Project, a pioneer effort in literacy and fundamental education. To be able to teach Tarascans, prepare Tarascan materials, and administer the project, Swadesh learned Tarascan (the principal Indian language of Michoacán) and was instrumental in launching a Tarascan newspaper. He had not previously known Spanish, so within his first year he mastered that language, to the point of preparing *Orientaciones lingüísticas para maestros en zonas indígenas* [*Linguistic orientation for teachers in indigenous zones*] (1940a) and of lecturing in Spanish at the Universidad de Michoacán on linguistics and its practical applications ("Cursos IV Centenario," 1940). These lectures formed the basis of his first major treatment of linguistic theory, *La nueva filología* [*The New Philology*] (1941).

In these works there was much that was inseparable from the character of the man. There was his delight in seeing to the detailed work himself. His father had taught him the printing trade as a child, and in Mexico,

having persuaded a representative of a linotype company to create a new Amerind type font, Swadesh himself set publications in type in Tarascan on a hand press in the Indian Boarding School in Paracho. He set the text of *La nueva filología* himself on the linotype in the offices of the Mexico City newspaper "El Nacional" (McQuown 1968: 755, 756). There was his assumption of mutual responsibility with "informants." Through his Nootka work with Alex Thomas, Swadesh had been inducted into the Boasian practice of teaching Indians to write their languages. It was a tradition of work in a collaborative, even collegial spirit.²² The Oneida publication (1941b) was prepared by Floyd Lounsbury as a repayment of sorts to the Oneida community for their assistance to the WPA language project that Swadesh had launched (the title page statement, "F. Lounsbury né detolisdohlalak" means equally correctly "he printed it" or "he typed it"). Toward the end of the Tarascan literacy project, when money was not forthcoming from Mexico City, Swadesh sold his car and whatever else he could convert into cash in order to pay his staff of Indians and teachers before leaving.

The approach of war brought Swadesh back to the United States. Called into service, he received officer training and became a first lieutenant in the Signal Corps. Serving in the Language Section of the Army Service Forces in Washington and New York (at 165 Broadway, an office where many American structural linguists were concentrated) and in the Office of Special Services, he edited dictionaries, prepared linguistic analyses and teaching materials in Spanish, Russian, Burmese (he visited Burma briefly), and Chinese, and taught courses in these languages. His books on learning Russian (1945) and Chinese (1947) were among the results.

After World War II

A concern for the application of linguistics to human problems (McQuown 1938) and a desire to make linguistics useful and to interpret it to the man in the street (Newman 1967) continue throughout Swadesh's career. All his general books (1940a, 1941a, 1960a, 1966a), and many other publications and activities attest to this, and there is no reason to think that he regretted his contribution to what for him was honestly a war against fascism. Yet altogether, the Oneida project, the Tarascan project (1939–1941), and the war work (1942–46) had taken eight years of his life. It is not surprising that, a civilian once again, he returned to the field of his first commitment

22. See Sapir's discussion of such experiences with Southern Paiute, Sarcee, and Nootka in his famous essay, "The Psychological Reality of Phonemes": "The most successful American Indian pupil that I have had in practical phonetics is Alex Thomas . . . and it is largely from a study of his texts that I have learned to estimate at its true value the psychological difference between a sound and a phoneme" (1949 1933: 54). Cf. Swadesh 1954f: 640.

and first training with Sapir and resumed the program of research on Nootka language and culture left incomplete at Sapir's death in 1939.

The changes brought about in linguistics and anthropology by the Second World War and its aftermath are not well understood. Only now, as a new climate of opinion emerges, are the years from about 1942 to 1965 beginning to be seen as one period, significantly distinct from what preceded and what is beginning to follow. It was a period of academic expansion and of cold war. Anthropology reorganized its professional association and a new generation came to leadership. Linguistics was institutionalized in separate departments for the first time and grew greatly in numbers and influence. The relation between linguistics and anthropology, once so obvious and intimate, came to seem a problem, as methodological developments absorbed the attention of linguists and made their work and talk esoteric to many anthropologists. The unity of anthropological disciplines implicit in their common study of the American Indian broke down as American anthropologists followed, if not the flag, then trusteeship, trade, and new power through much of the rest of the world, and as the classifications of Amerindian tribes, races, languages, culture traits, and archaeological traditions that had once been common knowledge became one body of special lore among many others.

Within this novel scene, there was continuity and piety. Manuscripts by Sapir and Whorf were retrieved and posthumously published; a volume planned by Sapir and a mature example of his methods were published in an anthropological series (Newman 1944, Hoiijer et al. 1946); Boas' *International Journal of American Linguistics* was revived; and the 1937 meeting Swadesh served as secretary was looked back to as defining a subsequent decade (Voegelin 1947). The role of Boas' ACLS Committee on Native American Languages was taken up by the Library of the American Philological Society, where the Boas papers were housed, with first Zellig Harris and then C. F. Voegelin serving as Library Research Associates; Swadesh in 1945 added a gift of papers from Sapir (Lydenberg 1948: 124-5; Harris 1945:97, n. 2). But the men who had made Amerindian languages and anthropological work inseparable from the advance of linguistics (Boas and Sapir) were gone, and their tradition was to seem marginal to a "linguistics without meaning" and a study of "culture without words" (to quote from the somewhat exaggerated title of an article by Voegelin (1949b)).

Of the five tasks identified for the preceding period, the first, development of method, has preoccupied linguists from the war until today. The 1940's saw a flurry of methodological papers on morphology and syntax (see papers 12-26 from 1942-1948 in Joos 1957), and the students of Sapir were marginal to this activity. Swadesh contributed to the debate on English phonology (1947c) as described above, but in continuation of an old involvement, and did not take further part in discussions of method. Nor

did he engage in what became a focus of concern for linguistics in relation to other fields, the discussion of the relationship between linguistics and ethnographic method and the attempts to extend linguistic method to analysis of the rest of culture. The seed of such an effort can be seen in Boas' claim that the clarity of the unconscious patterning of language could be a key to the solution of ethnological problems, and Sapir (1929) explicitly stated that linguistic method had a lesson for the social sciences. This notion was taken up actively in the 1950's, especially among students of Sapir and those who shared in the Sapir tradition like Kluckhohn (who had contributed the first discussion of cultural patterning on the analogy of language to the Sapir memorial volume [Spier et al. 1941]), Voegelin, Trager, Pike, Lounsbury and Goodenough. Discussion of a substantive relation between language and culture, focussed upon the writings of Whorf, also was concentrated among such scholars (Trager, Hoijer, Kluckhohn, Newman, McQuown, Voegelin). The Nootka work did as a byproduct furnish a test case for George Kingsley Zipf's "principle of least effort" (Zipf 1949: 78-81; AA 51 [3]: 533 [1949]). But for Swadesh, linguistic method seems always to have been a practical way of dealing with language, and the connection between language and culture obviously lay in meanings, not in methods or form.

Swadesh indeed stood consciously apart from the dominant attitudes of the post-war years; the concentration on method, form and formal symbolism. The only published expression of his view is an article written in support of critiques of behaviorist psychology in linguistics by Margaret Schlauch (1946, 1947). The debate continues the discussion that began before the war (1935b). Pointing out inconsistency in the effort of Bloomfield, Bloch and Trager to avoid meaning, Swadesh analyzes the difficulty as evidence of a struggle "between the fact that meaning is an inseparable aspect of language and the fetish that anything related to the mind must be ruled out of science" (1948f: 254). Against Bloomfield's view that linguistic meaning must wait upon definition in scientific laboratory terms, he points out that "languages are spoken by people who have no such minute and verified knowledge of the whole universe" (256). With regard to method he goes on to observe: "It is characteristic of the linguistic mechanists that they have great confidence in the scientist and none in the native speakers of the language" (256). On the contrary, "the judgment of the speaker proves to be valuable to the scientist even in the case of the subtler problems" (258). The charge of "circularity" against relating linguistic facts to human minds is answered first by noting that the objectionable inferences from observable behavior to unobservable "mind" are quite similar to the inferences from known to reconstructed languages practiced by behaviorist scholars and second by noting the inadequacy of such a "philosophy of science" on its own grounds: It is not considered "mentalistic" to speak of atoms and alpha particles, even though they are known mainly by their effects.

A note of personal pique perhaps creeps into this paper when Swadesh notes that a writer on a French creole had written *lapoht* (corresponding to French *la porte*) "under the impression that this was the standard mode of analyzing and representing long vowels among all modern linguists," and when he describes the use of parallel slanting lines to indicate phonemes as a "functionless esoteric device . . . mostly a peculiarity of the journals *Language*, edited by Bloch, and *Studies in Linguistics*, edited by Trager . . . not . . . without qualification . . . the general practice of linguists" (259). Overall, the article upholds the tenets of the Sapir approach in terms that were to be accepted as a devastating, unanswerable critique of the fallacies and inadequacies of "anti-mentalism" a decade later.

Except for an ethnographic field trip to the Nootka on an SSRC grant-in-aid in the summer of 1949 (Freeman 2406-7)—like English vowels, a continuation of an earlier interest—Swadesh did not undertake sustained field work again except as it related to his historical concerns. Of the five substantive tasks identified earlier, the only one to which he had not made major contributions before the war, relationships among languages, was to become his central concern and to be the basis of the contribution he would make to all the others.

The building of a profession of linguistics continued to involve Swadesh as it had in the United States and Mexico before the war. In 1945 the Linguistic Circle of New York launched a journal, *Word*, and with its second volume (1946), Swadesh became editor for four years. The Circle and its journal were international in composition and orientation, reflecting the concentration in New York of scholarly refugees from repression and war in Europe. They stood apart from the "anti-mentalism" that came to be associated with "native American" linguistics and with some of the major contributors to the Linguistic Society's journal *Language* during this period.²³ It offered space to a wider range of contributions, sometimes uneven, but often more interesting. *Word* was for a time the place for unorthodox thoughts and concern with semantics, literature, creole languages, and the like. It is symptomatic that Swadesh was editor, for if simple rather than "sophisticated" in manner and taste, he was always cosmopolitan in outlook.²⁴

What Swadesh might have further contributed to the profession in the United States as editor, organizer of research, and teacher—he was an absorbing and inspiring teacher—was not to be known. In 1948 he was

23. I use "native American" advisedly. When Roman Jakobsen arrived in the United States, he was housed by Franz Boas, but some of the linguists at 165 Broadway circulated a petition (or signed a dollar bill—stories differ) to complain about carpet-bagging foreigners—presumably a joke.

24. Swadesh was joined at *Word* by Andre Martinet for Volume 3 in 1947. Vols. 3-5 list both Martinet and Swadesh as Managing Editors on the cover, but Vols. 3-4 list only Swadesh on the inside front cover as Managing Editor for receipt of mss. Vol. 5 (1949) lists Joseph Greenberg and Martinet on the inside front cover, not Swadesh.

appointed Associate Professor of Anthropology at the City College of New York. There was a student strike, and Swadesh openly supported the students. He was not reappointed.

The "Swadesh Case"

The onset of the cold war and of "McCarthyism" in the academic world had already begun to affect anthropology.²⁵ The Swadesh case was to play a prominent part in shaping the stance that the American Anthropological Association (AAA) would adopt, and the basis on which it would respond twenty years later to a new series of appeals. Fresh from a reorganization with new powers that made some members apprehensive, the Executive Board of the AAA reported proudly that it had taken "immediate action" with regard to the proposed dismissal of a Curator at the Ohio State Museum for alleged Communist associations (*News Bulletin II* [3]: 37-38 [June 1948]), referring to its own "celerity and vigilance in protecting the interests of Fellows of the Association and the name of anthropology." (The Board had also successfully protested a disparaging comment by Drew Pearson about government appointment of an anthropologist to a position concerned with the Pacific Trust Territories.) But it shortly took notice of objections that it should not take a stand on anything other than professional competence ("The Morgan Dismissal and the Powers of the Executive Board," *News Bulletin II* [4]: 51-51 [September 1948]) and agreed with its critics, pointing out that it had not tried to judge the merits of the case or apply sanctions, but merely to secure a fair hearing. It adopted the view that the case was "a civil matter, lying outside strictly professional interests of the Association, that . . . [it was] in such a matter not empowered by the Constitution to take action that would commit the membership without an opportunity for it to express its collective wishes" (*News Bulletin II* [5]: 72-73 [November 1948]; *American Anthropologist* 51: 347 [1949]; both reporting the meeting of the Executive Board of July 1, 1948).

Matters did not rest there; "new pressures and other cases that have been

25. In the same issue as an official notice regarding the Morgan case (see below) and a communication by Voegelin (1950) suggesting the separateness of language from culture, some problems of the larger context of mobilization for the war and of large-scale government support afterward were presciently noted by Embree (1950).

The war caused many social scientists not only to lose their objectivity in regard to the cultures of enemy nations, it revived in them serious acceptance of the white man's burden. . . . The whole philosophy presents a striking parallel to that of French and British colonialists who have devoted their lives unselfishly to administration of the affairs of their little brown brothers. . . . Just as America, now the richest and most powerful nation on earth, must learn some self restraint, if she is not to ruin the peoples and cultures of the rest of the world, so American anthropologists who have so many opportunities for intellectual leadership must be aware of falling in love with their own culture and their own professional folkways to such an extent as to lose sight of their primary object: to study the nature of man and his culture, of the relations between men and their cultures.

mentioned as cognate" (*AA* 51: 347 [1949]; Board meeting Dec. 27, 1948) led to putting the matter on the agenda of the annual meeting of the Council of Fellows, which, after lengthy discussion, passed a Resolution on Professional Freedom. Contrary to a narrow view, it resolved:

that the American Anthropological Association go on record as favoring investigation by the Executive Board in cases where the civil rights, academic freedom and professional status of anthropologists as such have been invaded and take action where it is apparent that injustice has resulted that affects their rights as citizens and scientists.

A Committee was to be appointed to recommend what action should be taken in such cases (*News Bulletin III*[1]: 1 [February 1949]; *AA* 51: 370 [1949]).

The report of the Committee on Scientific Freedom (G. P. Murdock, Chairman; E. G. Burrows, A. I. Hallowell) and Swadesh's case were both to come before the Council of Fellows at its next meeting (November 1949), and to be linked together. Swadesh's part in the 1948 and 1949 meetings and their character and conflict are barely suggested in the published report of actions taken. The Board reported:

Under date of June 29, 1949, Morris Swadesh, whose appointment as Associate Professor at CCNY was not renewed as expected, addressed a letter to the President [A. Irving Hallowell] asking the intervention of the Association on his behalf. Subsequent to lengthy discussion, correspondence, and examination of various documents and letters the following minute was unanimously adopted at the September meeting: 'The Executive Board of the AA has examined the facts in the case of the failure of City College of New York to reappoint Morris Swadesh as Associate Professor in the Department of Sociology and Anthropology for 1949-50. The Executive Board finds that the By-Laws of the Board of Higher Education of New York City do not require notification of intention not to reappoint individuals on one-year appointments and that therefore no violation of civil rights or legal forms is involved. The Executive Board sees no grounds for further action in this case, although it believes that the By-Laws of the Board of Higher Education and the proceedings of City College of New York may not be in accordance with the best practice in institutions of high academic standing.' (*News Bulletin III* [4]: 3, [October 1949]; *AA* 52: 136-137 [1950]).

At the next Council of Fellows (November, 1949), Murdock, after presenting the recommendations of the Committee on Scientific Freedom, "made a number of remarks concerning the circumstances under which the Recommendations were drawn up by the Committee, with particular reference to the action of the Executive Board during the past year in the case of Morris Swadesh, a Fellow of the Association"[n.b., not in the case of the Curator in Ohio] . . . [and] then moved that the actions of the Executive Board during the past year [e.g., in the case of Swadesh] be sustained by the Council. . . . After considerable discussion from the floor, President Hallowell called for the question and in a standing vote of the Council the motion was carried . . . 76 yeas, 6 noes, 26 abstentions." The

recommendations of the Committee itself were carried without dissent on a voice vote (*News Bulletin IV* [1]: 1–2 [January 1950]; *AA* 52[1]: 152–3 [1950]).

The climate of the time can be glimpsed in the wording of a resolution condemning dismissals at the University of California that became symbolic of the new situation:

“The Executive Board of the American Anthropological Association [one member dissenting] deplores the dismissal in August, by a bare majority of the Board of Regents, of members of the faculty of the University of California, all of them free of suspicion as to communist sympathies.” (*News Bulletin IV* [4]: 7 [1950]; not published in the *AA*).

Morris Swadesh was not a man free of suspicion as to communist sympathies. Nor was he inclined to be silent, on linguistic or on social matters, where he believed principles to be at stake. Newman recalls: “As a result of this episode (the CCNY strike) and of other less publicized ones, he became labelled unambiguously as a ‘leftist’ during the noisiest period of the McCarthy Era, and university administrators were unwilling to take the risk of hiring him” (1967: 949). True, his sympathies were not misunderstood by colleagues who knew him and his work. As editor of *Word* (1946–49), Swadesh preferred certain simplifications in spelling English words. Twitted in an editorial meeting that the Soviet Union, which had once endorsed them, no longer did so, Swadesh observed, “Now they are wrong” (or words to that effect). The leading eminent theoretical linguist of the period recalls with some amusement that he rather resented Swadesh’s reputation as a great radical. He himself had arrived at a particular position through agonizing consideration of the alternatives posed in the intellectual debates of the time, whereas for Morris it was simply a matter of family and what was right. There were, to be sure, enemies. It is reported that a motion of censure was introduced *against* Swadesh at the 1948 AAA meeting by the chairman of his department and that a leading anthropologist temporarily refused nomination to chairmanship of a committee pending its passage. In the end, he was not to have employment or to be able to teach for seven years, and not for more than a decade, and then only as a visitor, in the United States.

Swadesh managed to continue his research. After the Nootka field trip in the summer of 1949, he worked in the Boas Collection in the Library of the American Philosophical Society, and had some support from the Phillips and Penrose Funds of the Society over the next few years, with the encouragement of Carl Voegelin and Zellig Harris. The Library had decided to specialize its Phillips bequest in the American field of linguistics, ethnology, and archeology (Lydenberg 1948).

The Library was proud of its program in these years, as well it could be.

Since the inception of the new policy with respect to the use of the Phillips Fund, established in 1946, the projects centering around American Indian linguistics

and archaeology have been among the busiest and most fruitful of the Library's activities. Under the supervision of C. F. Voegelin and Z. S. Harris a program for comparative studies in the Iroquoian languages . . . together with plans to improve the quality and usefulness of that collection is well advanced. Under this program, the Boas Collection has been completely reclassified and catalogued by Morris Swadesh (*APSY* 1950: 88–89 [1951]; *PAPS* 95 [3]: ii [1951]). Results from the Phillips Fund projects prove again the justification for specialization in the American field of Indian linguistics and ethnology. (*APSY* 1951: 87 [1952]; *PAPS* 96 [4]: i [1952]).²⁶

Possibly there is a connection between the sense of accomplishment in these years and the contrary report for 1953—after Swadesh had left—that activities had slackened and that after ten years a reevaluation was needed to chart future policy (*APSY* 1953: 88 [1954]; cf. *APSY* 1954: 85 [1955]). In any case, during these years Swadesh not only completely classified the Boas Collection, but augmented it as well. Some 82 entries in the index to the Indian manuscripts of the Library (Freeman 1966: 486) represent his donations at this time. In addition to Nootka and Yana work of a descriptive sort sponsored by A. L. Kroeber, as Library Associate, he carried on the research into the relationships and rates of change among languages that was to lead him into the linguistic prehistory of mankind as a whole.

The Linguistic Approach to Prehistory

Swadesh's work in the history of language represents a second career, as it were, begun as he neared forty. All his published historical work comes after the Second World War²⁷ His 1941 study of Proto-Zapotec was revised then for publication (1947d), but this the first published work in this vein was based on his knowledge of Chitimacha (1946e, 1947a). The connections of Chitimacha and Atakapa were not pursued further at the time, perhaps because Mary Haas was at work on the deeper relationships in the Southeast. In any case, Swadesh turned to the other language with which he had worked most intensively, Nootka, investigating its relation to

26. For activities of the Library in this period, especially with reference to Swadesh, see the "Report of the Committee on the Library", *PAPS* 93 (2): iv–vii (1949), *PAPS* 94 (3): i–ii (1950), *PAPS* 95 (3): i–iii (1951), *PAPS* 96 (4): i–iii (1952), *PAPS* 97 (5): i–iii (1953), *PAPS* 98 (6) i–iii (1954); and the more extended "Report of the Committee on the Library" (although often with the same language with regard to Amerindian Linguistics and archeology) in *APSY* 1953: 87–99 (1954), *APSY* 1954: 83–99 [1955]. See also the "Note on the Papers in American Indian Linguistics and Archaeology" by W. E. Lingelbach opposite p. 367 in *PAPS* 96 (4) (1952), the issue containing Swadesh 1952a.

27. In reviewing Andrade's Quileute (1935c), Swadesh had included 10 likely Nootka-Quileute comparisons, after observing, "In phonetics and morphological structure it [Quileute] has a close resemblance to the Nootkan dialects, but lexical resemblances are not easy to find. Still one finds enough good comparisons to feel that the possibility of an ancient historical connection between Chimakuan and Wakashan may be worth investigation" (219). The same general methodological perspective was expressed in his review of Bunzel's Zuni two years later (1937f: 255)—see n. 30.

Kwakiutl within the long-recognized Wakashan family (comprising also Nitinat and Makah). Suddenly, within a few years, Swadesh had invented glottochronology, reinvented lexicostatistics, proposed new criteria of proof of relationships, and published path-breaking comparative studies of four different major language families (Mosan, Eskimo-Aleut, Na-Dene, Penutian), the last of which led him on to comparison on a continental scale. Just as he had pioneered in communicating the phonemic approach and in drawing others into its use (cf. 1937e), so he was now pioneering in communicating the new historical approach that he had largely discovered himself and seeking to draw others into its cooperative development and use. All this came to fruition in years when he was without academic position of any kind.

THE USES OF BASIC VOCABULARY

His work on linguistic relationships and the development of lexicostatistics and glottochronology were intertwined. Common to both is the pattern of setting out on a path opened up by Sapir and making discoveries of unforeseen scope.

The initial choice to study Amerindian relationships derived from Sapir's proposals of a generation before. Despite much new data and much work, the "official" classification of Amerindian languages north of Mexico remained that of Powell for which the evidence had never been published and which later evidence clearly superseded. The sense of fixity introduced by Powell's classification persisted until after the Second World War, abetted by an illusory sense of conflict between the extremes of Powell's 55 stocks, based on lexical inspection, and Sapir's 6 phyla, based on intuition, or at best on grammar alone. In fact, Sapir published a good deal of evidence, both grammatical and lexical, for some of the new relationships he proposed, and his six ultimate units were clearly presented as a set of hypotheses. Lost from sight was an intermediate classification in his famous 1929 article, assembling the results of collective Americanist effort in the 1910's and 1920's. That classification recognized some twenty-three groups, and had that scale of classification been advertised by a map (as were Powell's 55 and Sapir's later 6) it would have proved a sensible basis for further work after the war.²⁸ As it was, some of Swadesh's work appeared daring and controversial only because its continuity with a previous consensus was overlooked and the nature of the work on which it was built misunderstood.

Swadesh clarified the basis of Sapir's historical practice (1954f: 641-2, 1955f, 1961a) on such points as Sapir's requirement of both lexical and grammatical lines of proof.²⁹ Sapir's proposed Na-Dene family, joining

28. See discussion in Hymes 1963, and Darnell ms.

29. Cf. Swadesh 1937f: 255, noting that despite the striking structural resemblances of Zuni to Uto-Aztecan and Kiowa-Tanoan, lexical resemblances do not obtrude themselves, and "serious attempts at comparison will have to wait until a dictionary appears." He later (1955k: 377) found lexico-statistic criteria to show that Zuni has no special affinity to Kiowa-Tanoan; cf. 1967c: 295-301, and 1969b for other affiliations of Zuni.

Athapaskan, Tlingit, and Haida had been the focus of a famous controversy with Boas as to the possibility of detecting remote genetic relationship and distinguishing it from resemblance due to borrowing. Swadesh undertook a methodological experiment, assessing what could be shown of the connection, through Indo-European, of English and French, and whether the evidence of genetic connection ("archaic residue") could be separated from "diffusional cumulation" (1951b). The results were applied triumphantly to Na-Dene, and the old objection that grammatical structure showed convincing connection but that much of vocabulary did not, was met by the fact of elapsed time. (Cf. the situation in Wakashan, as noted by Sapir in footnote 32). Received opinion since Powell had seen only one Eskimo language from Greenland to Alaska. Carrying on his Eskimo work, Swadesh used lexicostatistical criteria to demonstrate a language boundary in eastern Alaska, thus validating an observation only now recalled from the explorer Rasmussen, and in the process christening the two "new" languages (Yupik, Inupik). With Marsh he gave substance to the Eskimo-Aleut relation traditionally accepted since Powell (1951d; cf. 1952a: 452-3, 5). Mosan had been privately suspected by Sapir, publicly suggested and named by Frachtenberg, and then given a wider possible link to Kutenai and Algonkian by Sapir, a generation before (cf. Swadesh 1953: 27-28). Mosan itself was proposed as comprising the families of Wakashan (Kwakiutl, Nootka, Nitinat, Makah), Salishan (many languages in British Columbia and Washington with an outlier in Oregon), and Chemakuan-Quileute, Hoh, Chemakum). Swadesh had glimpsed some connection early in his work with Nootka (see n. 27a). Now he approached it through study first of two of its branches, Wakashan (1948g, 1948k), and Salishan (1941b, 1950a, 1952), and then built on earlier comparisons of particular branches by others (Chemakuan-Quileute [1955h], Nootka-Quileute [1953c], Salish-Wakashan [1953g] and himself (Nootka-Quileute [1935c-see n. 27a], Salishan-Coeur d'Alene [1949b]), in a detailed study of lexical and grammatical connections (1953a, f). One purpose was to smooth the way for examining more distant relationships, "including particularly Sapir's Algonkian-Mosan hypothesis" (1953: 27). Sapir's barely sketched conception of Penutian became the framework of further methodological experiments (1956h, i, presented in 1952), and of a field survey (1954b), leading to broader, "Penutoid" perspectives (1954d, 1955k, 1956b, 1957b, 1957c).³⁰ Already in the mid-1930s Whorf and Trager (1937) had considered such a perspective.^{30a}

As evidence of connections in basic vocabulary accumulated, Penutoid deepened into a comparative study of the languages of the New World (1958a, 1959j), and Swadesh's knowledge and exploration of languages of the Old World led further into a glimpse of remote connections in the lexical

30. In a letter to me of May 12, 1957, he wrote: "Pretty soon, I hope to send you a chart of Penutoid, which is much more old than ever and I wish I had a better name for it."

30a. please see page 312 for the new footnote 30a

stock of all or most existing languages (1960c, 1960i, 1964f, 1965b).

Glottochronology, in the sense of a method assuming a standard rate of replacement in basic vocabulary, came about by accident. Swadesh began with a statement from Sapir's famous monograph on time perspective that diversity among related languages is related to time, and that the more diverse related languages are, the longer they have been diverging from their common ancestor. To this common-sense assumption Swadesh joined the standardized use of a basic vocabulary. The notion of basic vocabulary is at least as old as comparative linguistics. It has long been noted that words for common features of human experience—such as body parts and functions; phenomena such as water, fire, wood, sky, bjrd, fish, lake, smoke, mountain, rain; sense experiences and dimensions such as long, small, red, hot, cold, etc.—are valuable in tracing relationships among languages. Such words can be found in any language, are less often borrowed from another (there being no semantic gap to fill), and tend to persist longer in a language than other words. The fact of relationship and the relative degree of relationship among languages often has been assessed by the comparison of vocabulary of such kind. Obviously, to infer that languages which share more basic vocabulary are more closely related than others implies that the persistence (or replacement) of such vocabulary is a function of elapsed time. If the relation to time were random, no such inference could be drawn.

Swadesh became famous and controversial for attempting to make this traditional use of basic vocabulary precise. Stimulated by the discovery of radio-carbon dating, he began with the expectation that the rate of change in basic vocabulary had a definite maximum (1952: 454, referring to 1948g). The rate of change in language was known to be relatively slow, and the needs of communication between generations would impose a limit. His first experiment compared the percentage of basic vocabulary in common between English and German to the percentage in common between Nootka and Kwakiutl, obtaining an estimate of the time depth of the latter relationship. If 59 percent of basic vocabulary was common between English and German, known to be separated about 1,100 years, Nootka and Kwakiutl, having 30 percent in common, must have had a much longer period of divergence. "In this way historic knowledge serves to clarify prehistory" (1952a: 454). In the absence of any native written records or dated monuments, such an inference of time depth is quite useful, and a reasonable estimate is better than none at all.³¹

Working at the Library of the American Philosophical Society in the fall of 1949, Swadesh extended his experiments to Salishan, using the percentages

31. It is interesting to compare Sapir's earlier impressionistic judgment. "In regard to vocabulary Kwakiutl and Nootka differ greatly. Considering the very striking morphological agreement between them it is somewhat disappointing to find comparatively few stems held in common. It is highly important, however, to note that many of these are rather colorless in content and thus hardly to be suspected of having been borrowed in post-Wakashan times. . . . By careful comparison of the two Wakashan branches one can in part reconstruct a Wakashan 'Ursprache' but the actual differences between the Kwakiutl and Nootka are in fact very great; they differ perhaps as much as Slavic and Latin." (Sapir 1911: 19, 15).

of words retained in English from Old English (about 1000 years older) as a convenient yardstick. When the rate of change in the test vocabulary among Salish languages was repeatedly found to be consistent, he decided to try the technique on languages of known history. By the time his second Salishan study appeared in print (1949b, 1951a; cf. 1952: 455, Voegelin and Harris 1951: 324–326), several colleagues had joined him in making such studies. Robert Lees undertook to provide a general statistical analysis and mathematical basis (1953; read as a paper at the Linguistic Institute, summer 1951), and the technique was incorporated provisionally into Swadesh's comparative study of Eskimo–Aleut (1951d) and his study of Na–Dene (1951b). In the next year a full-fledged presentation of the approach was published (1952a) and quickly became a focus of discussion and cooperation among anthropologists (cf. 1953h, 1954e, the companion papers in the special issue of *IJAL* [1955i, 1956f], and the special session held at the 1955 meeting of the AAA in Boston.)

In all this, Swadesh was conscious of his predecessors. He edited and published their work to give them credit and to show the continuity of his work with theirs (Mosan, 1953c, 1953g cf. 1953a; 278); Penutian 1953b, 1964c). He referred to lexicostatistics as having had to be “reinvented,” after its earlier discovery by Dixon and Kroeber (1950b, 1956b). And as the scope of the horizon opening before him grew, he became more than ever conscious of the need of collaborative work. With support from the Columbia University Social Science Research Fund, he collaborated with Joseph Greenberg in studies of several linguistic stocks in Africa, Australia, and America (cf. Greenberg and Swadesh 1953, Greenberg 1953, and Swadesh 1952: 455). Through Greenberg the same fund also supported his survey of Penutian vocabulary. With a small grant from the American Philosophical Society, he undertook to enlist the aid of others in obtaining the vocabularies that had become the tool of exploration:

At the outset the grantee had a collection of about 30 diagnostic vocabularies, including 17 in the Penutian phylum. It was necessary to find a way of rapidly assembling similar material from other Amerindian languages, ideally from all of the 1,500 or so languages of the New World. He undertook to accomplish this by communicating, by mail or personally, with linguists, ethnologists, teachers and missionaries, wherever he knew of anyone in a position to provide the needed material. Each was requested to furnish a list of 200 selected words, and also phonetic and grammatical notes of each language. The project has been met with widespread interest, and responses have been received from individuals and institutions working on Indian languages of Canada, United States, Mexico, Honduras, Guatemala, Costa Rica, Panama, Colombia, Venezuela, Brazil, Bolivia, Peru and Chile. About 100 vocabularies have been submitted and many more have been promised. They are being published by the grantee in a series called Amerindian Non-Cultural Vocabularies (in mimeographic form), which is being distributed to individuals, educational institutions and libraries (1955h).

In 1953 Swadesh moved to Denver, Colorado, for the sake of the health of his second wife, Frances. The Amerindian Non-Cultural Vocabularies (1954a) were launched there, from the basement of his home. Many scholars and field workers received Swadesh's encouragement and help in return for accepting copies of word lists with a promise to fill them out.³² The method was evidently capable of gathering a complete collection for the hemisphere. As the APS report goes on to say, however, "the grantee, with a minimum of clerical assistance, is unable to keep pace with the large volume of correspondence and editorial work." Almost without research funds, Swadesh could not keep a typist consistently employed or meet requests for copies of vocabularies without outside assistance. He worked with help as it could be found. A little money from the Department of Anthropology of the University of Chicago enabled him to analyze the individual persistence rates of words in the diagnostic list and to revise the list itself (1955i).

Fresh field work in Amerindian languages was at a low ebb outside California, while for many languages the number of speakers continued to dwindle. For the sake of anthropology, Swadesh suggested that scholars reverse their usual priorities and publish first not a grammar, but a dictionary with a brief description of phonetics and structure as an aid to its use "the exhaustive grammar can be left to future generations, if necessary" (1954f: 640-1). And he urged that the shortage of manpower be met by reviving the training of gifted native speakers, and by welcoming and encouraging (not discouraging) the contributions to lexical work of ethnographers, missionaries, and scholars in other fields (1954f: 640).

In the spirit of the Boas Committee on Native American Languages, Swadesh sought funds in 1955-56 for an ethnolinguistic survey of native America. He envisioned it as a program that would provide basic data for systematic historical work and would salvage a usable minimum of information, including the diagnostic list, from the many languages on the verge of extinction. At the same time, aware of hostility to the new methods, he stressed general goals and benefits, and judged that the Survey, however valuable it would be to Americanist studies, would fail if notably associated with glottochronology or himself.³³

Some impetus to the Survey came from the Consejo de Lenguas Indígenas [Council on Indigenous Languages] in Mexico, and the plan was given the

32. From a postscript to a letter to me of March 26, 1954: "In a letter I just got, Carl [Voegelin] mentions you among his students who have had field experience and who might be willing to fill out vocabularies for my collection. So I enclose blank sheets with my request that you give me whatever vocabulary or vocabularies you may have . . . I can repay your kindness in vocabularies."

33. Plans for a small volume on glottochronology, to include the discussions at the 1955 Boston meeting of the AAA, were suspended by a university press with the apology that they had not realized linguists and anthropologists were so divided on the subject; other evidence reaching Swadesh suggested that a colleague had introduced political considerations as well. (Letter to me of February 20, 1956).

blessing of Alfred Kroeber and Clyde Kluckhohn, as well as other colleagues. It failed to find funding, probably mainly because the study of disappearing languages, although it cannot be deferred, often strikes sources of funds as the last thing that needs doing at the time.

MEXICO AGAIN

In 1954 Kluckhohn had tried to bring Swadesh to Harvard. The proposal was supported by anthropologists, but characteristically opposed by the head of the linguistics group there. During these years, Swadesh visited Mexico a number of times, and in 1956 he returned permanently to Mexico as Research Professor of Prehistoric Linguistics in the Instituto de Historia of the Universidad Nacional Autónoma de México, and as Professor also at the Escuela Nacional de Antropología e Historia. He resumed an active part in the linguistic and anthropological life of Mexico. The number of publications in Spanish signed "Mauricio Swadesh" naturally increased, as did studies of Mexican languages.

Swadesh's concern for practical, efficient means in the face of the great scope of the work to be done, found expression in prehistory as in phonemic field work (1937e, 1965d, 1965e). At the time of the 1959 meetings of the AAA in Mexico City, he had discovered the computer and had begun to cooperate with an interested engineer. One midnight, when no one else was there to mind, he commandeered me to spend several hours punching cards for a Chinookan-Tsimshian comparison in the University electronic center. The former printer did some punching of his own, and investigated the way in which the card sorter and print-out might work. His concern to overcome the bias introduced into prehistory by the accident of what language a scholar knew well or thought to compare. He wanted to develop a general method of comparing language for special affinity at any level of relationship (cf. 1963b, 1966d). His involvement with computer work also led to his taking responsibility for coordinating a project for the decipherment of Mayan hieroglyphics (1968b).

The opportunity to work continuously in Mexico gave Swadesh fuller acquaintance with its major language families and a broader foundation for his exploration of the connections of Amerindian languages as a whole (1964e, 1967c, 1967d, 1969b). The center of gravity of New World linguistic relationships is indeed in Latin America, and only from the standpoint of the major families found in Latin America can the deeper problems of New World linguistic prehistory be properly posed and solved. Penutian was leading Swadesh into the Mexican connections partially detected earlier by Sapir; and in Mexico, his habit of relating his work to the interests of those around them meshed with the natural development of his own explorations. And Mexico brought him personal happiness and collaboration in many studies with Evangelina Arana.

Swadesh was never a parochial scholar, but Mexico gave him a special

opportunity to be fully cosmopolitan and to contribute more effectively to an international perspective on linguistic prehistory. It was there that his conception of the "world linguistic net" developed (1960c, 1960i, 1964f). It was through this work, and his analyses of the basic lexical stock of some major Mexican languages (1965a, 1966b, 1967a, 1969a), that much of his conception of the evolutionary history of human languages emerged (1965b).

In his teaching and research in Mexico Swadesh was to be judged "the only linguist who from this country has made theoretical and practical contributions to his discipline, and the only one who in Mexico has formed a school of linguistic anthropology which he himself took charge of activating and stimulating" (Wigberto Jiménez Moreno, quoted 1968a: 7). He attracted students from the United States as well (cf. Troike 1969: 183, n. 1). He lectured for brief periods at several Mexican and Latin American institutions (e.g., Universidad de Nuevo León (1956), Universidad San Luis Potosí (1958), Universidad Iberoamericana (1959-60), Universidad Central de Venezuela (1959), Colegio de México (1964)), and in the 1960's he began to be invited to teach in the United States, first at the Linguistic Institute at the University of Washington (summer, 1962), then at Columbia University (summer, 1964) and Syracuse University (fall, 1965). He was also invited to teach in Canada at the University of Alberta (summer, 1966 and spring, 1967). In early 1965 Swadesh spent several months working on lexicostatistic relations among West African languages while at the Institute of African Studies in Ghana. With his usual energy and enthusiasm he soon had a large group of colleagues assisting him. There followed travel in Europe, with lectures at Hamburg, Berlin and Prague, and consultation with colleagues and books in Paris, Tervuren, and elsewhere in further pursuit of his studies of linguistic relations.

On July 20, 1967, while awaiting the arrival from the airport of Sapir's youngest son, he died suddenly of a heart attack at his home in Mexico City.

His colleagues and students had been planning a volume on the occasion of his sixtieth birthday, then two years away. A series in his honor has now begun with the new edition of *La nueva filología* (1968c). Tributes at a memorial meeting of the Sociedad de Alumnos de la Escuela Nacional de Antropología e Historia (August 24, 1967) are to be included in a book in the series, and the April 1969 issue (vol. 35, no. 2) of the *International Journal of American Linguistics* was dedicated to his memory.

The Man and the Work

One can only speculate on what would have followed for Swadesh, for linguistics, and for anthropology, if he had remained in or returned to the United States and worked at an appropriate institution. To dwell on the loss to linguistics and anthropology in the United States betrays a certain

ethnocentrism. Certainly there were economic and personal difficulties for Swadesh. Yet he can be seen as a student of Sapir who effectively broke the barrier of parochialism that characterizes the organization of American Indian linguistics in the United States. He was to become so fully a part of Mexican work that the writer of a preface to one of his books referred to the years from 1942 to 1956 as a temporary interruption.³⁴ In an elegy, the West Indian poet John Figueroa was to find himself quoting Mauricio in Spanish and using Spanish for the refrain.

It is coincidental but appropriate that Swadesh returned to Mexico and renewed involvement with its languages and their speakers just before the enlarged Joint Committee on American Native Languages, of which he was a member (1949–1956), was reduced to a few official representatives of organizations, and the journal that Boas had launched to contribute to the study of native poetry (among other things), was nudged onto another course. Increasingly, the concern of the journal became the relation of the study of American Indian languages, not to the study of the American Indian, but to the study of other languages, such as those of Oceania and Africa. Its unifying principle thus became a particular method of work, the field recording and analysis of little known languages, and the relation of a language to its culture was replaced by the relation of a linguist to his data.³⁵

It is safe to say that such was not Swadesh's way of working. In the academy and in the field, he saw the work as collaborative, and the speakers of languages as his partners and ultimate beneficiaries. (One incident relates that a year after the Penutian survey, an old man in Florence, Oregon, spoke warmly of the good times he had with Swadesh during the few days they worked together). He could be commandeering and certainly was bold, yet always with a directness and simplicity based on confidence in sharing a worthwhile goal. Figueroa speaks justly in his poem of "Mauricio el suave." Though he was *engagé*, it is a striking fact that in the controversy over his linguistic work, disparaging remarks are to be found only in the writings of others. His work in glottochronology and lexicostatistics has to defend itself against claims it did not make, pretensions to which it did not pretend, arguments repeated after they had been answered, the turning of technical corrections into invented controversy (cf. Hymes 1969: 33, n. 12), and outraged denials that mathematics and statistics could have anything to do with language (cf. some of the comments on van der Merwe 1966).

34. "La labor del Dr. Swadesh se halla íntimamente ligada a México, donde ha permanecido, con breves períodos de ausencia, desde 1939" [The work of Dr. Swadesh has been intimately tied to Mexico, where he has permanently resided with brief periods of absence since 1939.].

35. The organized advisory board of IJAL has since been replaced by an ad hoc Conference on American Indian Languages, convened by an editorial board that continues to have no representative of the ethnology or folklore of the countries and languages where most American Indian languages are spoken today (Canada, Latin America), or indeed, of those who speak them.

Sometimes a note of bitterness would appear momentarily in private, but Swadesh invariably tried to see in what way another's work or approach could contribute to the general goal.

This indeed must sometimes have been unintentionally infuriating. Swadesh always acted as if there *were* a common goal; that if everyone would try his best, it could be reached; that details would be corrected later, and that all would come right in the end. The essential thing was to be working together as practically and efficiently as possible, getting the main lines clear. He saw himself as the patient secretary of a commune devoted to prehistory.

It was not the time for such a commune. It is accidental but significant that Swadesh returned to Mexico just before the work of Chomsky initiated a revolution within structural linguistics in the United States. In the early 1950's the achievements in structural phonology and morphology, and to some extent syntax, were sufficiently taken for granted that a number of extensions of linguistics and linguistic method were beginning to be revived and explored. In these years came the beginnings of paralinguistics and kinesics, of Greenberg's work on typology and universals, of kinship semantics (Goodenough, Lounsbury) and folk taxonomy (Conklin, Frake), and of linguistically inspired models for the analysis of social structure and myth (Lévi-Strauss). The initial interest and excitement attendant on lexico-statistics and glottochronology was part of a scene in which new discoveries and departures into spheres of general anthropological concern were quite expected.

The Chomskyan revolution displaced these trends, so far as the central participation of linguists was concerned. They remained alive and well in anthropology to varying degrees. Without judging the relative merit or importance of these trends, one can suggest an explanation for their differential success in the succeeding period. Lévi-Strauss's proposals for the analysis of social structure and myth have become central to the study of these two subjects. Kinship semantics and folk taxonomy have prospered. Transformational generative grammar itself can be placed in this category, for the data it required were in the heads of the linguists it inspired. In each case the initial impetus was a new approach to data already in hand or accessible. Where a new kind of data was required, success was slow or interrupted. Such has been the case in linguistics itself, where the socio-linguistic approach of Labov has had to win its way slowly because it depends on social and linguistic data from the community as well as on a linguist's introspection. Such was the case with paralinguistics and kinesics, because a continuing body of workers was not developed, and the initial specification of features did not progress to an independently usable method (or to an organized description of the relation of features to social meanings that others could use). A similar difficulty befell the work of Whorf, developed by Hoijer, and the work of Swadesh. The parallel is instructive.

WHORF AND SWADESH

Both Whorf's proposals as to the relation of language to habitual thought and Swadesh's proposals as to linguistic prehistory can elicit prejudgment and some of the passion reserved for heresy. Pointing out the fallacy of extreme positions is the stock in trade of many an introductory course, and "Whorf" and 'Swadesh' are sometimes used as tags in this way. Just as Whorf has been taken as having said that language wholly determines thought and world view and that language worlds are incommensurable, so Swadesh has been taken as having said that all of language changes at the same rate, that it is always at constant rate, and that lexicostatistics and glottochronology displace other methods. If the straw men attacked under these two names were collected, one would have fodder for all the cows in Kansas.

Both men pushed their insights and inferences as far as they could go. When these end-points are taken in isolation, it is easy enough to regard them as idiosyncratic, or impinging upon anthropology and linguistics from some other sphere. Original and inventive as both men were, however, their work grew naturally out of the tradition of Sapir. Their work is bold, but fledgling linguistics and Sapir were bold. In the post-war era the two men seemed unique perhaps because the climate of opinion in which they and their kind of boldness had developed had largely disappeared from view. Some of their statements or inferences taken in isolation appear extreme because the general experience from which they stem is not grasped; because their ideas require a supporting structure of work that has not been forthcoming; and perhaps because others are unwilling to draw the logical conclusions of what are in fact shared experiences and observations.

In Whorf's case, the observations are first of all that languages differ as they are used. In other aspects of life the instruments that are used are accepted as conditioning what is done. We see the relation of hand tools to what is made, and what can be made, at a given place and time, and we accept this dependence without concluding that a technology wholly determines a culture. For reasons apparently having to do with the history of philosophical and ideological debate in our culture, we resist the parallel that words and sentences are "mouth tools" conditioning what is done and thought, and what can be done and thought, at a given place and time. Yet if children growing up in a community are not in a position to substitute a different technology, neither are they in a position to substitute a different set of means for storing, reporting, and manipulating information. The differences among languages in vocabularies, grammatical categories, and grammatical relations are evidence that different communities have adapted their linguistic means to facilitate doing and thinking some things rather than others. New participants in a community (children) adopt and adapt to those means largely as given. Languages do change and adapt, shaped by the creative responses of their users. But the community's principal tool for

handling information is necessarily functionally connected with the assumptions and activities of its way of life. This fact is perhaps hidden when one thinks in terms of a relation between "language" and "culture," if one understands by the terms a grammar abstracted from meanings and use and an ethnography abstracted from speech. Whorf worked to develop modes of semantic description and saw the need for a kind of description cutting across the usual categories of grammars, a description of "fashions of speaking" that represented the adaptive edge of a language's engagement in social life. His ideas came to the fore, however, after the Second World War, at a time when almost no semantic description was being done or contemplated—little has been done in the delineation of ways of speaking until the recent emergence of sociolinguistics.³⁶

In the case of Swadesh, the development of his work depended upon meanings in a different respect. Recurrent patterns of semantic change and expressive symbolism became familiar to him as part of the "apperceptive mass" with which he assessed evidence (Cf. 1953a: 29, 1956: 30–32, 1960c: 139–140, 1962c, 1967c: 297–300 on "oblique cognates"). There was nothing in this to startle one familiar with the shapes and shifts of roots, stems, extensions and ablauts in the reconstructed etymons of the standard-bearer of comparative work, Indo-European. To workers coming from primary training in descriptive grammar, where segmentations, identifications, and patterns were evident and clearly present or absent, the flexibility required of prehistoric detective work could seem strange and rash. (Just so, Sapir's use of a sensibility trained in Indo-European detective work appeared "intuitive" and idiosyncratic to an earlier generation of Americanists conditioned to regard comparative grammar as a rigid machine from exposure only to its nearest examples.)

As evidence accumulated of lexical connections throughout both the Old and New World, Swadesh's conviction of the unity of the whole became firm. (Just such a conviction of the unity of the whole, stronger than conviction as to the specific affinities of particular parts, has been acquired by others who have explored connections of this scope and depth.) The main problem then came reasonably to be to work out the sub-classification; the alignments

36. Whorf has been further discussed with regard to the "First Yale School." Note that he contributed to all the tasks identified here. He developed a model of the structure of English phonemic distributions; his description of his own dialect of English was part of the discussion of English syllabics; he contributed to the development of concepts and methods for the description of grammatical categories and relationships; he sought to show others the value of support for linguistics; he worked with Hopi and Aztec with support from the Boas Committee on Native American Languages; he joined with Trager in reconstructing Uto-Aztecan-Tanoan and with Swadesh, Haas and Trager in (cf. n. 30a); broaching wider relationships; and of course he sought to discover the relation between linguistic and cultural patterns, a quest in the anthropological climate of the time, where the notion of patterning and the relation of cultural patterns to personality (and personal experience) were major concerns. For an explication and socio-linguistic critique of the tradition developed by Whorf, see Hymes 1966: 114–123, 1957–8. For the prior development of such notions by Sapir, see Hymes, 1970.

within the whole. From the standpoint of those who were not yet persuaded of a particular relationship and who did not bring to the evidence the same world-wide "apperceptive mass," Swadesh's proposals could seem arbitrary. To those who felt that the near-to-hand must be wholly secure before one could explore at a deeper level, Swadesh's assumptions could be infuriating.

What was missing was the participation of many scholars in developing etymologies of a middle range. But just as Whorf's ideas seemed reduced to grammar versus behavior, so Swadesh's ideas seemed reduced to the statistics of a 100-item word list versus systematic sound correspondences. Some of the work of each could lend itself to such an interpretation. Yet the context of the whole clearly shows Whorf to have been concerned with language and culture together in a common historical matrix, and Swadesh to have been concerned with explorations by basic vocabulary as one line of evidence to be checked, corrected, and integrated with others. He said so many times. Because the fact has been so often overlooked, some of these statements, spanning his comparative work, are given here:

"Relationships are manifested in many facets of language. Percentage of common basic vocabulary is but one of many lines of evidence bearing on the problem. Morphologic and phonological isoglosses can be examined with profit. So too, specific vocabulary isoglosses—the distribution of specific words both native and borrowed." (1950a: 167)

"Lexicostatistic data must be coupled with other evidence, including that of archaeology, comparative ethnography, and linguistic paleontology. The separate lines of study serve to verify or correct one another and to fill in details of the story." (1952a: 453)

"All the time depths given here have to be regarded as provisional in greater or lesser degree. This follows, first, from the fact that the lexicostatistic method has not yet been developed to its maximum reliability (it cannot be until more research has been done on control cases): second, the test lists are much too limited in some cases; and finally the phonological formulas and structural reconstruction necessary for maximally reliable identification of cognates in each pair of languages has not been sufficiently worked out." (1954e: 36)

"Preliminary time-depth estimates have been made for all the language groups for which vocabularies are available. This serves to indicate which languages are closer to each other, to set the general scale of each grouping, and thereby to provide a frame of reference for phonological and other detailed analysis. More carefully drawn chronologies, which are possible after phonological and structural studies." (1955k: 377)

"Clues as to additional and apparently closer affinities of Zuni came about ten years ago from exploration by means of diagnostic wordlists. However, by now such study has been supported by various other tests." (1967c: 296)

The essential thing is to see Swadesh like Whorf, as someone who pioneered within a tradition, whose work has stood out for a time as a dramatic manifestation of that tradition, but whose own desire would be, not to be judged as having heroically (or foolishly) sprung forth full armed, but as having contributed to furthering that tradition and its goals, even in unpropitious times.

SWADESH'S PLACE IN THE TRADITION

Swadesh's effort was one that should have been sustained by substantial funds over a long period of time. As it was, he accomplished much by his own effort, energy and commitment. Many scholars have discussed the classification and prehistory of American Indian languages. Of Swadesh it may be said that his work showed him to be the most serious man among them, the one who, having accepted the goal, made it his life work.

Swadesh's contributions to the methodology and results of language classification remain controversial. This is not the place to discuss technical problems in detail. Let me say only that I have sometimes expressed reservations, but that I believe his work to be the most significant since that of Sapir for an understanding of the linguistic prehistory of the world, matched only by the contributions of Greenberg (with whose procedures Swadesh's work has much in common). Without detracting from the merit of Greenberg's work, it is historically revealing to compare the reception of the classifications of South American languages by the two men. The two classifications agree on the essential unity of the languages of the New World, differing on various internal groupings. Greenberg's classification was obtained with a list of 30 to 40 glosses, Swadesh's with a list of 100 glosses (both sometimes obtaining less information for a language than they wanted). Greenberg published the result without supporting data, backed essentially only by personal authority. Swadesh presented an explicit account of his procedures, endeavored to make the data available, and regularly revised his findings in the light of new evidence and research. The classification based on authority without supporting evidence has been reprinted often in anthropological textbooks and journals; the work presented as an explicit, continuing scientific enterprise has not.

To some extent the controversy attaching to Swadesh was a product of the style of the man and of his work. The anthropological audience for work in linguistic prehistory wants general classifications, but generally does not want to understand the data and procedures either as part of a general theory of cultural change and historical inference or as the result of a particular process of inference. It wants simple answers and definitive guide-lines. For Swadesh, the work was a continuing process in which the published results were not authoritative gestures, but progress reports inviting revision and collaboration. The linguistic audience often prefers to dispute details rather than consider the general thrust of the case, and to argue for the primacy of one

line of evidence as against others (a habit that clouded understanding of Sapir's work until his approach was clarified by Swadesh [1961]). Proof of a single relationship is sometimes seen as the ultimate goal rather than as a means to other ends. More detailed reconstruction and inference, is gingerly postponed. Some want to work where the data is rich and provides many familiar types of problems—that is in relationships of little time depth—and avoid long-range comparison altogether. Some refuse to accept probabilistic inference, or set criteria of proof that exclude long-range relationships. Few besides Greenberg see the methodology of linguistic prehistory as itself a possible object of experimental inquiry, of basic research; many see the methodology as already fixed (since the nineteenth century perhaps). What was such an audience to make of a man who would sacrifice an occasional point of detail for the sake of a larger mass of evidence, a larger picture? Who considered all usable lines of evidence relevant? For whom a proof of genetic relationship was a means to an end? For whom the remotest past of language was in principle accessible? For whom the presumably long-established methodological foundations of linguistic prehistory were indeed a field requiring basic research?

To repeat an important point: some linguists have wanted to work as if each level of relationship had to be fully reconstructed before a deeper level of relationship could be broached, and as if the penetration of the linguistic past could be accomplished only in an additive, unidirectional, mechanical way. (The parallel to “phonology first” description is apparent.) I believe this approach to be demonstrably wrong. Certainly it was not the way of working of Sapir and Swadesh who moved back and forth between the immediate and the remote levels of prehistory, finding the two mutually illuminating. (Swadesh has pointed out that Sapir's correspondence with Berthold Laufer on the possibility of a Sino-Tibetan connection for Athapaskan is instructive on this score.) But it was just this strength (from the standpoint of discovering significant phenomena) that troubled and confused an audience wanting one neatly wrapped result at a time. An audience barely ready to consider evidence for a relationship between New World and Old World languages when the languages in question were geographically near each other (Eskimo and Chukchee) was not ready for glimpses of the interrelationship of geographically more distant languages of Asia and America. In fact, the editors of Swadesh's article on “Linguistic relations across Bering Strait” required him to omit these glimpses. While necessary in the circumstances, this omission deprived readers of a unique contribution.

When one understands Swadesh's work as that of a pioneer, an explorer of new terrain, one recognizes it as indispensable. Swadesh, like Sapir, was somewhat of an intellectual buccaneer. His work often troubled more cautious colleagues just because it could not be dismissed; it seemed to go too far, and yet it made substantive discoveries that could not be ignored or explained away, but had to be taken account of. There was just too much

evidence that the paths he blazed did go somewhere, and that one would eventually have to follow them out. The lexical sets and morphological processes he uncovered as pertinent to world linguistic prehistory are indeed pertinent, even if, by linking both the Old and New Worlds, they go beyond what we are able to incorporate in ordinary classifications at the present time (1960i). These data demand explanation, and it is to Swadesh that we are indebted for their discovery and for the first steps toward answering the problems they pose.

Swadesh's work has often been criticized in terms of detail, such as the mistaking of the analysis of a suffix in Tsimshian in the course of showing a relationship between Tsimshian and Chinookan. I am concerned about Tsimshian and Chinookan, their relationship and their suffixes; they are languages on which part of my life has been spent. It is too much to expect many others to be concerned, unless some more general question is thereby illuminated. It is Swadesh's great merit that he made many American Indian languages assume world importance through their role in his studies. His work can be corrected on details, like the Tsimshian suffix, but in point of fact, Tsimshian and Chinook are related, as he said they were, in the light of all the evidence; much of which he assembled. Moreover, in relating the two languages he brought them into the context of general questions of method and of relationships that extend to the Mayan languages of Mexico and Guatemala and beyond.

Swadesh's methodological explorations have been much discussed in the extensive literature on lexicostatistics and glottochronology (cf. references and comments in Hymes [1964]). Here it can only be said that future generations will honor him as one who saw the possibility and necessity of transcending ad hoc controversy and partial views in linguistic prehistory by converting its assumptions into explicit parts of a basic science subject to empirical test. His studies of rates of change, rates of borrowing, lexical versus grammatical retention and borrowing, basic vocabulary, persistence of similarity in phonological shape, and the like were not radical innovations in subject matter, but radical only in proposing to treat explicitly and systematically what had usually been treated implicitly and incidentally. Swadesh's methodological explorations tended to be part of the study of a concrete problem. Just as he put structural linguistics to work in describing languages rather than concentrating on methodology itself, so he constantly put to work the existing state of knowledge of lexicostatistics and glottochronology in problems of actual relationship. Once something serviceable was developed, he used it as the best tool available rather than postponing substantive work until methodological problems were resolved.

In recent years mathematics adequate to the problems of lexicostatistics and glottochronology have been developed and applied (van der Merwe 1966, Sankoff 1971). The empirical foundations of the subject need now to be restudied on this basis as part of the general problem of persistence and rate

of change in language, a problem which has many implications in addition to those of prehistoric dating. In the preponderance of cases, basic vocabulary is replaced at about the same rate and can be useful in historical studies (cf. Troike 1969). The 100-item test list has become a standard international instrument, known familiarly as the "Swadesh list." But Swadesh's most distinctive contribution inheres in his vision of linguistic prehistory as a whole. As he himself said of his conception of a world linguistic network, the whole is stronger than the parts. Working at frontiers of knowledge, he could not always be sure of details, and sometimes went too far and too fast for many of his colleagues to follow. He died before all could be woven together. Yet his explorations and the new dimensions he discovered have permanently extended our knowledge and conception of the contribution of linguistics to the understanding of the human past. Above all, Swadesh posed the true problem of linguistic prehistory in the context of an emerging world society. He saw that linguistic prehistory is ultimately justified by addressing itself to the problem of the unity of mankind.

NOTE (see page 298)

30a. According to Trager (1945: 188),

Whorf established, to the satisfaction of those who saw his material [presumably including Swadesh DH], that the grouping of Penutian, Sahaptian, Azteco-Tanoan, Zuni, Kiowa, probably Mayan and Totonac, and possibly Tunican, as stocks constituting a phylum which he called Macro-Penutian, was at least as good as the Algonkian-Mosan, or Na-Dene groupings of Sapir.

The "thousands of slips treating the comparative phonology of what he [Whorf] called Macro-Penutian" (Trager 1942: 305) have never been made public, but I have seen a typescript of some nine pages, bearing Whorf's name and dated January 1939, in which four pages on 'Relationship of Mayan' are mostly taken up with illustrative sound correspondences from Macro-Penutian. The grouping is conceived as Penutian (including Sahaptian), Azteco-Tanoan and Mayan. Further evidence, it is suggested, could lead to inclusion of Kiowa and Zuni. The manuscript concludes; "Nothing can be said about the place of Totonac until McQuown returns from Mexico with his information", indicating that the statement was addressed to a circle of intimate collaborators. That Swadesh was one of the collaborators is indicated by the fact that two other unpublished documents, one a photo copy of an autographed diagram of the differentiation of Azteco-Tanoan, the other a carbon copy of an outline arrangement of Macro-Penutian, each one page and prepared by Whorf in 1936, were later donated to the Library of the American Philosophical Society by Swadesh in 1950 (Freeman 3837, 2065; these documents have been reproduced in Gursky 1969: 12, 13).

Swadesh's participation is further attested by Trager in another context (1941: 357): "Some years ago, basing their opinion on Dr. Haas's material, Morris Swadesh and B. L. Whorf came to the conclusion that Tunica was distantly related to Azteco-Tanoan and thus to the larger Macro-Penutian group of languages". Trager went on to note that Whorf and Haas later thought that Tunica probably belonged with Muskogean (as was later demonstrated by Haas), and to observe similarities in structure and mention suggestive correspondences that would make careful lexical comparison between Tunica and Tanoan worthwhile. Whorf's work on Uto-Aztecan and Trager's on Tanoan seem to have been the center of the new conception, and Whorf and Trager's evidence for a connection between the two the one substantive result (Whorf & Trager 1937). Whorf's two schemes of Macro-Penutian differ principally in this regard. That of 1939 splits the 'Tanoan-Kiowa' of 1936 to join Tanoan with Uto-Aztecan (Azteco-Tanoan), making Kiowa a question along with Totonac and Zuni. (Sahaptian, a separate entry in 1936, is reintegrated with Penutian proper in 1939, as Sapir had had it a decade before.)

When Swadesh began work with Penutian in the early 1950s, he started with Sapir's more restricted Penutian (certain languages in California and Oregon, Sahaptian in the Plateau, Tsimshian in British Columbia, and Mixe-Zoque and Huave in Mexico). His work soon led him along lines similar to those of 'Macro-Penutian', with regard to Totonac, Mayan, and Zuni, not with regard to Azteco-Tanoan or Kiowa. The trail of mass comparison of basic vocabulary led him instead much further south in Middle and South America to include, among other groups, Tarascan and Quechua-Aymara (Swadesh 1957b).

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BIBLIOGRAPHY OF MORRIS SWADESH

This bibliography to a great extent reflects a collective effort. In 1967 I first prepared a bibliography in connection with my article on Morris Swadesh for *Anales de Antropología* (5:213-225 [1968]), using that of Juan Jose Rendon (*América Indígena* 27[4]:740-746 [1967]), and being aided by Regna Darnell and Joel Sherzer. My 1967 bibliography was not published, but contributed to the biobibliography in the second edition of *La nueva filología* (1968a: 311-332), prepared from the work of Evangelina Arana de Swadesh, Daniel Cazes, Jaime Espinosa, Juan Jose Rendon, Madalena Sancho, and myself. In June and July 1970 I have worked from the mimeographed 1967 bibliography, revising it in the light of Stanley Newman (*Language* 43 [4]:950-957 [1967]), *La nueva filología*, additional information from Evangelina Arana de Swadesh as to more recent publications, further research. I am indebted to Murphy Smith for assistance with unpublished materials in the Library of the American Philosophical Society, and to Freeman and Smith 1966, as a guide to them.

It has been possible to add a few minor items for earlier years, as well as more recent publications; to add a few unpublished manuscripts of significance to the development of Swadesh's work; and to amplify the information for a good many items, as well as to correct errors of date and pagination. With but one exception (1939i), it has been possible to verify all respects in which the information given here differs from previously published bibliographies. It is the hope of all of us who have contributed to this work that the results will be seen, not only as testimony to a varied and productive career, but also as an aid to the further study and understanding of that career, and of the many activities, significant to the present and future of linguistics and the human sciences, of which that career has been part.

Dell Hymes

Abbreviations

AA	American Anthropologist
A de A	Anales de Antropología (México, D. F., UNAM, IIH, Sección de Antropología)
ACLS	American Council of Learned Societies
AL	Anthropological Linguistics
Am Sp	American Speech
APS	American Philosophical Society
	APSY, PAPS, TAPS: Yearbook, Proceedings, Transactions
CA	Current Anthropology
ENAH	Escuela Nacional de Antropología e Historia
IIH	Instituto de Investigaciones Históricas
IJAL	International Journal of American Linguistics
INAH	Instituto Nacional de Antropología e Historia

INI	Instituto Nacional Indigenista
Lg	Language
PAPS	See APS
RMEA	Revista Mexicana de Estudios Antropológicos
SWJA	Southwestern Journal of Anthropology
TAPS	See APS
UNAM	Universidad Nacional Autónoma de México

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THE PRE-WAR PRAGUE SCHOOL AND POST-WAR AMERICAN ANTHROPOLOGICAL LINGUISTICS

I should like to discuss the relation between the pre-war Prague School and post-war American anthropological linguistics, first from the standpoint of my own experience of them, and then from the standpoint of the place of each in the emergence of 'functional' linguistics. My own work in linguistics owes much to the coming together of influence from these two intellectual currents.

I apologize for writing of my own experience, but offer in excuse that it may be of interest as a case that does not quite fit common notions of the recent history of linguistics and that it has led to the comparison to be drawn in the next section between the schools of Prague and Yale.

The years 1950-55 are almost pre-historic for many younger linguists today, at least in the United States. They are years "B.C.", "Before Chomsky", before the publication of *Syntactic Structures* in 1957. A common image of those years would be of the dominance of the 'neo-Bloomfieldian' or 'Yale' school, a school characterized by hostility to 'mentalism', a neglect of functional problems, and hostility or indifference to European traditions of linguistics in which 'mentalism' and 'functional' perspective were important. My own initiation into linguistics in those years was different in each respect. There was something of an earlier 'Yale school', that of Sapir; something of mentalism and of functional problems; and something of the Prague School.

I was a graduate student at Indiana University, starting in anthropology in 1950 and receiving the doctorate in linguistics in 1955, with anthropology and folklore as minor subjects. These three fields, which Boas had combined in one person and department at Columbia University in the period of the founding of American academic anthropology, were still closely allied (cf. perhaps the connection between the pre-war Prague School and the work of the late Petr Grigor'evič Bogatyrev :1893-1971)). At Indiana the early unity of

these three fields in the study of the American Indian was sustained as a dominant interest. The chairman and founder of the Department of Anthropology, C.F. Voegelin, was an Americanist and linguist; his colleague, George Herzog was primarily an Africanist and ethno-musicologist, but partly an Americanist and linguist. Two prominent figures in the Committee which guided graduate training in linguistics, Harry Velten and Thomas A. Sebeok, had both done some work with American Indian languages (Velten with Nez Perce in Idaho, Sebeok with Winnebago in Wisconsin, and also, through texts, with the South American Indian language, Aymara). In these years after the Second World War, much of American anthropology was turning away from the American Indian to study other parts of the world; folklore was academically marginal in most universities, if it existed at all; and many American anthropologists were finding their traditional obligation to understand linguistics an increasing strain, when confronted with the new rigor of linguistic method; but at Indiana in those years the American Indian, folklore, linguistics and anthropology were a natural unity and ambience.

The gods of this dispensation remained Boas, Sapir and Bloomfield, although all were dead. The tradition they represented was not transmitted wholly and purely — the view of it that is sketched in the next section of this paper is something I have come to understand only with recent research and is even yet incomplete. But the tradition was relevant, not rejected. In one seminar a professor asked, "Why had Boas been interested in grammatical categories?" The question reflected the climate of distrust of meaning, and turning of attention away from grammatical categories in 'advanced' circles; yet the asking of the question reflected also an honest puzzlement and an assumption that the answer was of interest. At the time I was shocked by the ignorance that the question seemed to betray. One had only to read Boas, especially his Introduction to the *Handbook of American Indian Languages* (1911), to discover why he had been interested in grammatical categories. A little exploration quickly disclosed a continuous tradition, on the one hand, leading back to Steinthal, von Humboldt and Herder, and known as such to Boas, Sapir and Bloomfield, and on the other, leading forward to Whorf. Whorf himself was clear that he followed in the footsteps of both Boas and Sapir. But Whorf did not apparently remember Steinthal or von Humboldt; and after the Second World War, no one, it seemed for a while, remembered Boas or Sapir. Whorf was celebrated posthumously as the discoverer of 'linguistic relativity'. (The use of 'relativity' in this connection had originated with Sapir -- cf. ch. 4, p.).

There was then a sense of continuity with a past tradition that offered alternatives to present doctrines, even though the tradition was conveyed mostly as great names and anecdotes about them — how much had been lost from view, how much the Second World War and the immediate post-war years changed the intellectual landscape in the United States, I have only recently come to realize. There was a sense of alternatives in space, as it were, as well as time. The late Harry Velten cast an amused and penetrating eye on problems of comparative and historical linguistics, and in some invisible, yet real way helped inspire me to take up historical linguistics in the first years after my degree. Thomas Sebeok kept reminding one of a rich, slightly risqué world in which linguistics dealt with semantics, poetry, myth, and such things, as honest linguists. Of course such work was welcomed in the partly anthropological, partly neo-Bloomfieldian, partly new-born American rationalist climate at Indiana. But in that climate such work seemed something that would have to start from scratch, and be done by linguists within the limitations of the descriptive linguistic method of the day. The other world seemed one in which linguistics had been already at work, with methods that left our far less of what one knows semantics, poetry, myth and the like to contain. Velten and Sebeok, of course, were adherents of the tradition of the pre-war Prague School.

In the summer of 1952 Voegelin, aided by Sebeok, organized a Conference of Anthropologists and Linguists. Its participants included Roman Jakobson and Claude Lévi-Strauss, and the resulting monograph was co-authored by all four. (I had prepared the digested transcript that constituted the remainder of the text, after the addresses by Lévi-Strauss and Jakobson, and was for a short time considered a co-author also. But Voegelin decided that co-authorship would be unfair, since it might expose me, a defenseless graduate student, to the ill will of those digested.) It would be dramatically pleasing to be able to say that the encounter, a decade before Lévi-Strauss had achieved his present preeminence, and a little before Jakobson's stature was fully appreciated in this country, had shaped my life. But it did not. I was not ready. The work in social structure whose inspiration Lévi-Strauss attributed in important part to Jakobson and Trubetzkoy was something I admired, but no more. Of Lévi-Strauss' initial proposals for the analysis of myth (1955), I was intensely critical, especially of the way in which the parallel with language was drawn. (And might have said so had a projected contribution to the same special issue of the *Journal of American Folklore* been realized). I was to come to terms with Lévi-Strauss' use of the Prague tradition later, both as to what to

emulate and what to abjure (cf. Hymes 1965, 1966, 1968, 1970b). A decisive impact was to come from Jakobson a few years later, and not at the University at which we were then colleagues (Harvard, 1955-1960), but again in Bloomington, at a conference on style organized by Sebeok.

At the conference on style (April 1958) Jakobson delivered a report, published under the title "Linguistics and Poetics", in which he justified linguistic attention to the poetic function, and in which he placed the poetic function in terms of a scheme of general functions of language. Whereas much of previous theory had begun with the linguistic sign but not gotten much beyond it, or had, rather arbitrarily, listed quite general sociological functions, whose relation to the linguistic sign was obscure, Jakobson proposed to begin with an analysis of the speech situation, placing the linguistic sign within it, and deriving an exhaustive typology of functions naturally and logically, by primary focus (*Darstellung*) on each of the constituent factors of the speech situation in turn (He maintained the Prague view that all the functions would be present in each case, only differing in hierarchy). The six components of the speech situation were the message, channel, context, code, sender, receiver, and the six corresponding functions the poetic, contact, reference, metalinguistic, expressive, and directive. (See Jakobson 1960).

It was this presentation that turned my thinking to a functionalist perspective, and that led, among other things, to the article published in *Slovo a Slovosnost* (Hymes 1970a). At first I remembered the number of Jakobsonian functions as five, not six as published, and when the approach had been thought through in an ethnographic framework, some fundamental differences would emerge despite a discussion of them with Jakobson. But the debt to Jakobson, and to the functionalist approach of the Prague School, was fundamental too. The paper (1962), which begins my turn to a sociolinguistic direction, and begins any distinctive contribution I may have made to sociolinguistics, is dedicated to Jakobson.

The crucial contribution was to introduce a 'functionalist' perspective, and to do so in a way that suggested an empirical, manageable way of dealing with speech functions. To some, Jakobson's discussion might have seemed simply a variant on an 'information theory' model of the speech situation, or even just an arbitrary classification, worked up for the occasion to give ad hoc organization to a host of examples. The presentation, however, was something more than a schema. There was the methodological spirit of linguistic inquiry, showing the principles of commutation and permutation, of paradigmatic and syntagmatic relations, at work. The host of examples showed lin-

guistic form to depend not only on the relations in grammar in the broadest sense (encompassing phonology and lexicon), but also on the relations among components of speech events such as channel, context, sender, receiver. To attend to this larger domain was not to attend to variation and style in some endlessly quantitative sense, but to an additional realm of structure. The host of examples showed linguistic form to covary as the relations among components of the speech event covaried. And the notion of alternative hierarchies of function opened up the prospect of treating function, not as given, but as problematic.

The impact of this perspective was gradual, or at least I can not now remember a particular hour of leaping out of the bath, shouting "Eureka!". The first public sign was two years later, when an invitation to give a talk at the University of Pennsylvania on anthropology and education led to a discussion of functions of speech from an evolutionary perspective (summer 1960; published 1961). The talk went past most of the audience of educators and students of education, but the published paper gained wide attention among anthropologists through inclusion in a reprint series. When invited to speak in a series sponsored by the Anthropological Society of Washington that fall (November 1960), the functional framework was developed into a call for an *ethnography of speaking*. As luck would have it, a presentation of the argument at the annual meeting of the American Anthropological Association that month was assigned to a general session on theory, scheduled at the same time as the major linguistic session of the meeting. The published essay came gradually to attention (even in disguised form, as when a student at Iowa wrote about "Durkheim's seven functions of speech"). With the rise of interest in 'sociolinguistics', the essay has become established as part of the programmatic literature paving the way for sociolinguistic research, and since my coming to the University of Pennsylvania, led to a body of empirical research that will just begin to appear this year. (Among the students involved: Regna Darnell, Michael Foster, H. Hogan, Dhanesh Jain, Elinor Ochs, S. Philips, P. and S. Seitel, Joel Sherzer, A. Strauss, K.M. Tiwary). A concern with the functions of speech has pervaded my theoretical writing (beside references already cited, note Hymes 1967a, 1967b, 1968b, 1971a, 1972). In this respect, then, a perspective of the pre-war Prague School is, to use a current colloquial expression, "alive and well" in post-war American anthropological linguistics (or linguistic anthropology as it is increasingly called).

A relevance of the essays by Jakobson and myself to pre-war discussions of *langue* and *parole* is pointed out by Uhlenbeck (1967:361). Functionalist

perspective has undergone elaboration and criticism both in Prague and elsewhere, and, as noted, my own views depart from those of Jakobson. It would not be possible or appropriate to argue those views here, and a good deal of their nature can be seen in a recent article of mine (Hymes 1970a). An understanding of the relation between pre-war and post-war views, however, requires some comment on another pre-war perspective, that of Sapir.

Let me approach this by discussing the later history of the Sapir tradition in post-war American linguistics (cf. chs. 4,6). It was this subsequent history that defined the relation between language and culture as a problem, to which a functionalist perspective could come to be recognized as an answer. And let me take as a text for this discussion a statement by Trnka and others in 1958:

"As to the study of the relationship between the relevant features of language and those of society, it must be admitted that all schools of structural linguistics – with the exception of that of Edward Sapir and his followers – have failed to develop it in a satisfactory way." (Quoted from Vachek 1964:477).

I can agree with the statement insofar as it is the tradition of Sapir and his followers to which I myself would point, as precedent in American linguistics for my own work. At Indiana, Voegelin and Herzog had both been associated with Sapir at Yale; Hoijer, who taught one summer at a Linguistic Institute, and with whom I studied for a year at UCLA, had been Sapir's student as had Swadesh, who visited Indiana, and whose historical interests, which shaped my own work for some years, were the following out of a program of historical research inspired by Sapir. In the immediate post-war years, when some used Bloomfield's name to define a very narrow conception of linguistics, and depreciated Sapir as a more intuitive genius without method, it was to Sapir that one looked as symbol of a wider conception of linguistics in which meaning and literature had a natural place. In 1959, on the 20th anniversary of Sapir's death, indeed, A.L. Kroeber organized at Berkeley a memorial meeting at which Yakov Malkiel spoke hopefully of a sign of a Sapir 'renaissance'. Another friend, the anthropologist R.H. Lowie, edited Sapir's letters to him (see Lowie 1965). These things seem symptoms of a sense that American anthropology and linguistics had not fully come to terms with the heritage of Sapir.

Such at least was my situation at the time when Jakobson's treatment of functions had its impact. In historical work on American Indian languages I identified with the aims of Sapir (and of his follower, Swadesh). A sometime poet, I rejoiced that Sapir had been a fairly successful one. A worker with

Chinookan grammar and texts, and inheritor of some of Sapir's unpublished notes, I pored over plain evidence that his has been the most brilliant and accurate mind to touch that language. For sustenance in a dry season, for evidence that linguistics and anthropology could have intellectual stature, I re-read his essays and book. And as a fledgling academic at Harvard, I was the protégé of Clyde Kluckhohn, to whom Sapir had been a major influence, the embodiment (later joined by Lévi-Strauss) of the idea that linguistics offered anthropology a way to be rigorous and yet true to the patterned nature of its materials, a way to be scientifically exact without aping inappropriate methods of the natural sciences.

Of all this I was aware at the time. There was a new element of which I also became increasingly aware. In its Boas and Sapir-like unity of linguistics, anthropology and folklore, around the American Indian, Indiana University had been something of a refuge area. As linguistics became an independent academic discipline after the Second World War, a discipline no longer dependent on anthropological and language department hospitality, tracks which had seemed to run parallel began increasingly to diverge. Yet it was my professional responsibility, as a linguist in anthropology, to travel both. A sense of strain had begun to appear right after the war, signalled by articles reassessing the relationship between linguistics and ethnology. Anthropologists not trained in structural linguistics often found its concepts alien. And linguistics had changed in scope in just a few years. In 1941 a memorial volume to Sapir (Spier et al. 1941) had contained articles expressing in a variety of ways the link between linguistics and anthropology. But the volume did not serve to define a post-war tradition. After the war the center of the stage was taken by concerns purely internal to descriptive linguistics. The contributors to the Sapir volume did not abandon their interests, but they and their interests seemed peripheral to a focus on meaning, to a 'young Turk' stance of washing away all previous work, 'contaminated' by meaning, 'metaphysics', and 'mentalism'.

In 1951 those at the heart of the methodological development seemed to feel that some sort of stable plateau had been reached. The *Outline of English Structure* by Trager and Smith, and *Methods in Structural Linguistics* by Zellig Harris, seemed to define the new approach rather satisfactorily. A new period of reaching out from linguistics to a larger sphere of investigation began. The study of gesture and body motion, and the study of the use of the voice, were rechristened *kinesics* and *paralinguistics*, and launched anew. Models for the analysis of all culture were derived from linguistics by Trager and Smith, and

by Kenneth Pike. (Pike, it should be stressed, was never part of the orthodoxy just discussed). At the same time a more generalized, less publicized use of linguistics was underway, in the work of Lévi-Strauss, inspired by the Prague School, and in the work of W. Goodenough and others, derived from the Yale School. No specific model of linguistic analysis was taken over. Rather, the goals of linguistic analysis were taken over, and adapted to the exigencies of ethnographic material. In brief, one did not look for 'phonemes' of culture, or for other superficial parallels of structure. One sought cultural units and structures that had the same methodological basis as phonemes, i.e., contrastive relevance within a locally valid frame, and conceived ethnography, like grammar, as discovering a theory for the particular case.

Much of the development sketched here was existing, all of it was interesting, and all of it had precedent in the work of Sapir, but it did not add up to an integration of the study of language with the study of culture. The two tracks might run parallel again for some, but they remained separate tracks. Linguistics was autonomous, and was what dominant linguists said it was. The only choices were to regard linguistics as unique, or to look for parallels to linguistics. The implicit assumption was that human life was manifested in series of separate domains. Behind the domains might lie the human mind, and thus some common structure. But no one suggested that something might have been left out of account. Pike began his generalization of method (dedicated to Sapir) by discussing activities in which linguistic and non-linguistic features were integrated, but he went on to concentrate on linguistic analysis proper. The original purpose of developing a method to deal with behavior, linguistic and non-linguistic, in an integrated way, was left behind. The relation of language to society was discussed again, only in terms of parallel domains and of analogues between them.

The continuing influence of Sapir that was most discussed was that mediated by Whorf. Here again the implicit assumption was that language was one domain, culture (or metaphysical assumptions) another, and that one should take the separation as given, seeking for correlations. Whorf's suggestion of 'fashions of speaking' was not developed into a novel method of description, but used only as a principle of selection within grammars written in usual ways.

These efforts to relate language to the rest of culture were put into the shade by a new development within linguistics itself, one which returned to Sapir somewhat in its 'mentalism' and morphophonemics, but that carried one trend of Sapir's thought to an extreme, and so severed its connection with

the rest. The kind of structural linguistics on which the efforts were based was eclipsed by transformational generative grammar. To continue the metaphor of 'tracks', linguistics and anthropology seemed hardly to run parallel anymore. They seemed almost to be oriented in opposing directions. As Chomsky's views developed, they seemed to seal language off almost altogether from the sociocultural sphere. Chomsky's notions of the acquisition of language, of language use, of language change, and of the origin of language, all conspired, as it were, to deny experience or social life any constitutive role, or even relation. The thrust of linguistic speculation was reversed. Whereas those influenced by the Sapir tradition looked out from language to other institutions, those influenced by Chomsky looked in toward the brain. Structure was immanent to the mind and innate in the organism.

The work of Chomsky now seems to me the ultimate development, the 'perfection', as it were, of the dominant trend of linguistics in this century. It is the trend that motivated much of Sapir's work, and that informed the recurrent efforts under his influence to relate language to culture. Briefly put, the trend is that toward the isolation of linguistic structure as an object of study. When de Saussure, Sapir and Bloomfield wrote, it was necessary to insist on the separation of language as an autonomous object of study. And it has been around that separation that modern linguistics has developed as a profession. The degree of separation, and the basis for it, however, have varied. In both the Sapir and the Prague traditions, recognition of the autonomy of linguistic structure was joined with recognition of the many connections of linguistic structure with social life. Chomsky severs the connections of linguistic structure, and in a way that motivates the severance. He does not, as Hjelmslev, simply call for the independence of linguistics. He argues that the ultimate goal of linguistic theory, explanation, lies in the brain, not at all in social life. To such an appeal it is not enough to insist on a tradition of looking for structural parallels, or of tracing the occurrence of language or linguistic features in social contexts. Even less does it suffice to insist on the practical value of sociolinguistic work. None of these positions counter the argument that scientific goals require solving the problems internal to the analysis of grammar, before secondary uses of those results can be seriously made. An alternative conception of scientific goal, of explanation is required.

In the sphere of linguistic change, such an alternative conception of explanation has been put forward and substantiated by the work of Labov. In a theoretical article, Weinreich, Labov and Herzog show themselves sympathetic indeed to the work of Mathesius and others of the Prague School

(1968:167-69), while finding it necessary to go beyond it. In the sphere of linguistic description, my article in *Slovo a Slovesnost* (Hymes 1970a) indicates some of the reasons why I believe an alternative conception of explanation in the synchronic sphere to be necessary.

More generally, I would call Chomsky's notion of explanatory adequacy a partial, 'essentialist' kind of explanation. Full explanation requires another kind, which may be called 'experiential' or 'existential' adequacy. The heart of the conception of 'experiential adequacy' is that linguistics has as its goal explanation not only of structure, but also of function. Explanation not only of the *means* of speech, but also of the relation of those means to the *ends* they serve. Such a conception, as the Prague tradition well knows, leads one to discover further aspects of structure in language — aspects of the organization of linguistic means in the service of expressive, stylistic, standard language and other functions. Chomsky's conception of explanatory adequacy effectively limits language to grammar organized in terms of the single function of semantic reference, excluding all the many other ways in which language functions. Chomsky's conception of the 'creative use' of language reduces to a conception of language use as monologue. Although he speaks of 'creative' use of language in terms of appropriateness to situation, his method can deal only with novelty of utterance.

Appropriateness is a relation between utterance and situation. A new utterance in an old situation, an old utterance in a new situation, may equally be creative use of language. One must be able to analyse the structure of situations, as well as the structure of sentences, and be able to establish the relations of appropriateness between the two, if the 'creative aspect of language use' is to be more than a slogan. In short, the Chomskyan conception appeals to our concern with the meaning of language in human life, but its definition of linguistic analysis will forever frustrate an answer to that concern. If linguistic research is to serve human freedom, it cannot limit itself, as does Chomsky's work, to freedom *from* situation. It must also address the use of language in context in order to account for freedom to master situations. Such an approach will require a method of description that is both social and linguistic.

This critique of Chomsky's conception of explanation, and of the relation of language to social life, is not a defense of either the Sapir or Prague tradition in their pre-war form. Both traditions, like other lines of linguistic research, participated in the long-run trend of the century, the separating out of linguistic structure for study in its own right. Neither resolved the problem of a descriptive method encompassing both linguistic and social features. Both

traditions, however, may be said to have based the separation of linguistic structure on a conception of the *autonomy of linguistic structure*, not of the *complete independence* of linguistic structure. Chomsky has attempted to find a ground in the brain and biology for *independence*. When the inadequacy of that attempt is realized, when the goals of linguistics are understood to require a broader base, it is to the traditions of Sapir and Prague that one can turn.

Ultimately both the Prague and Sapir traditions have become intertwined in my work. Looking back, I see in Sapir's 1925 paper, "Sound Patterns and Language", a fundamental methodological principle that applies successively to all levels of structure, and that embodies the point that function is prior to structure. The development of the tradition of linguistic work in ethnography, to which Sapir and his students contributed so much, has led to the standpoint from which I criticized and revised Jakobson's formulation of components and functions of speech events, treating their number and kind as problematic, and needing to be determined for the particular speech community. And I can see now in Sapir's work in the last years of his life (1931-1939) a development that quite anticipates the 'ethnography of speaking', and the perspective from which I have criticized Chomsky. In those years Sapir stated his dissatisfaction with the impersonal, generalized patterns of anthropology and sociology, and asserted the need to understand such patterns in terms of their meaning in personal lives. Indeed, he proposed that new kinds of organization of patterns would come into view from that standpoint. He spoke of the need to

bring every cultural pattern back to the living context from which it has been abstracted in the first place and, in parallel fashion, to bring every fact of personality formation back to its social matrix... The social psychology into which the conventional cultural and psychological disciplines must eventually be resolved is related to these paradigmatic studies as investigation into living speech is related to grammar. (Quoted from Mandelbaum 1949:592-93)

The quotation could be applied to our present situation, by speaking of "The sociolinguistics into which the conventional linguistic and social disciplines must eventually be resolved..." (cf. Hymes 1967a). Sapir did not develop this new perspective in empirical studies, and this aspect of his thought was not carried further after the Second World War. The dominant characteristics of the first Yale School, the Sapir tradition, before and after the war, continued to be shaped by the general situation of linguistics as a still emergent discipline in the United States, and by the anthropological context in which much of linguistics then grew. Briefly put, the main tasks were seen as:

(1) to develop the methods of structural linguistics, and to test their application in both exotic and well known languages; (2) to sustain a profession of linguistics, where almost none existed; (3) to rescue knowledge of disappearing languages; (4) to pursue proof and establishment of genetic relationships among aboriginal languages; (5) to relate linguistic inquiry to other disciplines and subjects. (See discussion in Hymes 1971b).

One can see here important points of contrast with the Prague tradition. The Sapir tradition recognized expressive and other functions in language, and occasionally described them, when they became salient in ordinary descriptive work, but mainly it was concerned to write basic grammars of languages little known. The Prague School, on the other hand, worked to a considerable extent with languages well known to it, and could, given the appropriate perspective, penetrate further into their functional complexity. The Sapir tradition worked mostly with languages of American Indians, whose societies showed relatively little differentiation, and no class structure. Questions of a standard language did not arise. Questions of the language of verbal art might arise, and to some extent did, but not in the marked form in which they were posed in a European country. The Sapir tradition was at work in a native linguistic situation in which many questions of genetic relationship remained unresolved, and in which solution would be of considerable interest to colleagues in anthropology. The Prague tradition worked mostly with language families whose relationships were established, and perhaps could more easily give attention to phenomena of convergence. Such phenomena were never excluded in the Sapir tradition, but they remained at the periphery, the main task appearing to be the working out of genetic classification. Finally, despite Sapir's remarks on 'living speech' quoted above, there never developed an explicit attention to variability or 'potentiality' such as was present in the Prague School. Again, the primary focus on basic grammars of American Indian languages may well have been responsible.

In sum, the work of the pre-war Prague School is more in keeping with the world linguistic situation today in a number of respects. The Prague tradition early shook off the limitations of what I have called the 'Herderian' perspective of equating a language, a culture and a society. It addressed itself to the synchronic diversity of linguistic varieties within a speech community. It moved beyond constructing the results of the diversification of language, to the affinities and specializations of function which languages acquire within a common society or social field. These attributes are of prime importance for an adequate linguistics today.

If I may end on a personal note, it was a linguist from Prague, visiting Philadelphia, who pointed out to me the absence from my own thinking of attention to linguistic situations such as those that are familiar in Europe, and who made me aware of the unconscious limitation of the anthropological tradition in which I had worked. I can not say, then, that the Sapir tradition, any more than any other, has developed the study of the relationship between the relevant features of language and those of society in a satisfactory way. Much of the basis for such a development is present in the Sapir tradition; much of that basis is shared with the Prague tradition, and the Prague tradition has essential elements lacking in the Sapir tradition. To speak only for myself, it is in the fruitful combination of the two that I see the main hope for the progress of linguistics in the remainder of this century. We need not look back a century or two, but only a generation or two, for inspiration.

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TRADITIONS AND PARADIGMS

Linguistics is a discipline and a science, and its history is part of the general history of disciplines and sciences. Nevertheless, this book (Hymes 1974b) is written and will be read mostly by linguists. Analogous situations seem to prevail in the historiography of the various human sciences. Efforts toward adequate history may borrow a notion and a term, such as 'paradigm', from general writings on the history of science, but writers on the general history of science do not much notice the efforts. This situation must change, but the basis for change must be laid principally by work to establish the relevant facts and to discern the intrinsic relationships in the particular field. We still have far to go to achieve the historiographic equivalents of what Chomsky once labeled 'observational' and 'descriptive' adequacy.

In this introduction I shall say something about the contributions its contents make to the achievement of such adequacy. The book is, I think, a timely one, that speaks very much to our present condition. In the course of the introduction I shall say something about our present condition and lines of work that are needed. In particular, I shall discuss the use and abuse of the notion of 'paradigm', when the topic of the history of general linguistics has been reached.¹

Let me say something about each section of the book in turn, then, considering general questions as they arise. And let me stress that the sections *are* conceived as groups of papers, whose parts should be read in mutual relationship. So much may seem obvious. The papers do come in groups. Reviewers and readers, nevertheless, tend to take papers in isolation, evaluating them in relation to almost anything except their partners and the plan embracing them. Of course, that is their right, and I cannot pretend that the present papers are as integrated, say, as a sonata movement. That lack reflects, not on the abilities of the contributors, but the state of the art. Each section does have a point, and so does the order of the whole, as I shall try to bring out.

Contrastive Beginnings

We tend to take linguistics for granted, since its florescence since the Second World War. It is sobering and healthy to consider the possibility that it might not exist, even though it is one of the oldest intellectual activities to become codified and distinct. Were present linguistics to disappear, one would conjecture, surely practical needs and curiosity would bring it into being, as they did before. Such needs and curiosity, however, are not everywhere or always the same. Cultural values (explicit and latent), matrices of institutions, individual personalities and abilities, and the nature of materials at hand, all shape the needs that are felt and the directions that curiosity takes. (Cf. the delineation of factors in Malkiel 1964.) There is also the stage to which prior configurations of such factors will have brought matters; given a fresh start, without prior configuration as a determinant, the outcome might be quite different. It is a useful thought-experiment to imagine what the outcome might be.

In effect, the question of beginnings is always with us. As generations change, so do the 'beginnings' of linguistics in the contexts and motives (partly unconscious) of those who enter linguistics and renew it. And the complex history of linguistics, closely considered, resolves into an overlapping series of local scenes, specific 'structures of feeling' (Williams 1965: 64-66), approachable through biographies (as in Sebeok 1966—cf. Austerlitz 1972) and lesser writings, more than through isolated classics. There is a certain logic in the course of ideas, yes, but the pace and places of its working out, and whether a particular logic is worked out at all, is invisible in the 'great works'. Isolated works may suffice, if our interest in the past is only to praise or condemn. If our interest is to know what happened, we must enter into the contexts of personal and institutional origins. Our history must become a history, not only of great men, but also of circles, and not only of circles, but also of institutions, governments, rulers, wars and the ways in which these have shaped the renewed origins of linguistics in successive generations.

Folk Linguistics

Such a perspective gives reason to consider the nature and sources of reflection on language in all conditions of community. Scholarly studies have begun with the point at which reflection on language, and analysis of it, has left written trace. There is something to be gained, however, from inquiry into the conceptions of language held by societies without institutionalized linguistics. Comparative study of such information can shed light on the range of interests and motives for reflection on language. Given due caution, such

study can suggest something of the earliest matrices of interest in language, out of which what we know as linguistics has grown. It can suggest something of the matrices of interest in language in which linguistics takes root (or fails to take root) in the world today. (For while linguistics aspires to be the science of language as a whole, in subject, it is the science of but part of the world, in participants. On this problem, see Hale 1973.)

The best definition of the temporal scope of the history of linguistics thus is probably the widest. Following Hallowell (1960) in his depiction of anthropology as an emergent out of folk-anthropology, as having continuity with man's earliest efforts to obtain reliable knowledge about himself and his place in the universe, we can begin with 'folk linguistics'. (Cf. Hoenigswald 1966 for a valuable discussion of the scope of the topic.)

The motives most frequently cited for emergence of attention to linguistic features can be generalized as recognition of difference or discrepancy. What sort of difference is recognized may vary. Most often cited are differences between social varieties or levels of speech within a single community; between distinct communities; and between the current state of a language and retained knowledge of an earlier stage. (The types are not mutually exclusive, and may merge.) It is this last kind of recognition that figures in the historically known rise of linguistic study in the classical societies of India, China, Greece, and Rome, and that figures again in the positive interest of many American Indian communities today in the services of linguists. It seems likely, however, that in any community at any time, differential individual competence, including that between adults and children, as well as between neighbors, may have sufficed to give rise to some overt linguistic consciousness.

Sapir (1915) remains the fullest study of conscious exploitation of phonological features, followed by few others (cf. Frachtenberg 1917). Gudschinsky (1959) has reported a Soyaltepec Mazatec conception of phonemic tones as 'thick' vs. 'thin' and J. D. Sapir (1973) another use of the 'thick': 'thin' contrast among the Diola of Senegal. Elmendorf and Suttles (1960) have put into social context what amounts to virtual recognition of the distinctive feature of nasality among the Halkomelem Sälsh. Lowie (1935) has documented normative consciousness of linguistic features among the Crow, and Bloomfield devoted an article to his unexpected discovery of its pervasiveness among the Menominee (1927). Ferguson (1971) has brought to the fore the kind of implicit analysis involved in simplification for talk to those outside the normal adult community, foreigners and babies. Generally, elaboration of folk analysis of speech, as codified in vocabulary, appears to be in regard to speech styles and speech acts. Communities with little more than a general word for 'language, speech, word(s)', may have quite precise and interesting terminology for talking like X, Y, or Z or talk to accomplish A, B,

or C. Gossen (1972, 1974) and Stross (1974) provide rich accounts of this from Mayan communities.

It remains true that we lack systematization and comparative study of such systems of terminology and conception as to speech. The concerns of the 'prehistory' of linguistics and of the ethnography of speaking here coincide, so that it can be hoped that the lack will be made good, given the surge of activity in the latter field.

Sometimes scholars have perpetuated notions of the discontinuity between 'primitive' and 'civilized' societies, on scientific grounds, by ignoring, and even denying, the presence of reflection on speech, among the non-Western communities in which they have worked. Such notions have been held to be the product of rationalization, probably of misguided school traditions, and a source of interference with one's task. Certainly notions about language cannot override or replace realistic analysis (as some linguists are today again discovering), but to ignore them is to miss one aspect of reality. Indeed, the very interaction of linguist with native speaker (another or himself) is subject to the influence of terminology, even if ad hoc terminology, and hence the validity of the resulting data, is very much subject to conceptions acquired from the linguist (whether fieldworker or self-consulting). One might think that the self-consciousness about sources of social knowledge so widespread today would rear its head in linguistics; but the only mention of the emergence of terminology in linguistic investigation that I know is reported from his Nootka work by Swadesh (1960).

When we investigate a language, another's or our own, we cannot avoid in some degree terms and notions already locally extant; if we ignore them, they may influence the outcome of our work nonetheless. Thus the interests of validity in linguistics, and the curiosities of ethnographers, folklorists, and historians, come into contact here. If demonstration is needed that notions about language show continuity from the earliest known societies to modern times, White's chapter on philology (1896) and Borst (1957-63) are excellent.

The origin and spread of writing systems plays a part here. Invention or adaptation of such systems is *prima facie* evidence of analysis of language on the part of the inventors or adaptors. Kroeber (1940) has called attention to cases of 'stimulus diffusion' in this regard, and indeed, our interest here is not satisfied by restriction to one or a few cases of wholly unmediated invention of writing from scratch (no pun intended). Evidence of analysis is given, as has just been said, in diffusion as well as in pristine invention. The role of knowledge of the alphabet as stimulus to Sequoyah's invention of a Cherokee syllabary is of much less interest than what Sequoyah did with Cherokee. Similarly with the question of possible stimuli to the Korean Hangul system. The present century offers rich opportunity for understanding writing systems as manifestations of pre- or para-disciplinary linguistic analysis, where

literacy has not been imposed by fiat, but introduced to a language in collaboration with its users.

National Philologies

For the most part, it is true, writing systems involve a second stage of conscious attention to linguistic features, one that may be called the stage of *national philologies*. It is not that there is a necessary relationship between the two; at least it would be rash to take one for granted. The Greek tradition, insofar as it was a matter of rhetorical education, might have carried on with oral means; and the Indic tradition, centering around maintenance of sacred texts, was so carried on in the first instance, continuing to focus on oral memorization and recitation after writing was introduced into the society. Only in the cases of the Babylonian and Chinese traditions, and those much later in which a national philology is begun by foreigners, can one say that writing is intrinsic. The stage of national philology represents a focussing and cultivation of consciousness of language which might arise repeatedly, requiring, not special means (graphic), but only special interest.

The ancient Mesopotamian attempts at analysis of language, described by Thorkild Jacobsen (1974), are earlier than any other known attempt, dating from about 1600 B.C. They are also very little known. (I am indebted to Roman Jakobson for bringing the possibility of including this work in the volume to my attention.) Professor Jacobsen brings out the nature of analysis in terms of paradigms in the first period known to us (Old Babylonian) and the analysis of elements in the later period (Neo-Babylonian). He concludes that efforts to consider and order in isolation elements which had no independent existence seem to have taken place.

The circumstances of the Babylonian work seem to have been similar to those of other classical civilizations: concern to preserve a literature embodied in a language becoming obsolete. In the Old Babylonian period, Sumerian (in which the major part of Ancient Mesopotamian literature was written) was rapidly dying out as a living language, being replaced by Akkadian. It would seem that consciousness of imminent loss is what counts, and that it does not matter whether the linguistic varieties involved are separate languages, or related dialects, or styles.

In India, as Staal (1974) brings out, the primary aim was to preserve a heritage required for recitation in ritual. The earliest evidence of great activity, word for word recitation, may be from between the tenth and seventh centuries B.C., some centuries later than the origins of the Babylonian tradition (although the Babylonian manuscripts are from roughly the sixth and

fifth centuries B.C.). Explicit analysis went much further, as is generally known, and as Staal succinctly shows. It is in India, indeed, that one can earliest identify distinct authors and schools, in developments over time, and in controversies. In particular, although the tradition stemming from Pāṇini became central to linguistics in India, it is important to remember that it was not the beginning nor did it become the only tradition. The richness of the Indian history of linguistics goes further, in that its work was applied to other languages, in India (e.g., Tamil) and in other countries (e.g., Tibet). We have here an invaluable contrastive case for the comparative study of the relation between linguistic models and languages of structure different from those in connection with which they were developed. We have, too, an instructive contrast to the situation in the West, in which linguistics has often aspired to the prestige of mathematics or a natural science (with results of mixed value). In India it was linguistics that was considered the norm of a science, a status to which other traditional subjects aspired.

Most accounts of the history of linguistics begin, whether insightfully or patronizingly, with the Greeks and Romans (sometimes missing the significance of the Romans [Romeo and Tiberio 1971]). Conceptions of the history of linguistics so limited to the standard conception of the 'march of civilization', are prone to error. The essays by Jacobsen and Staal, together with the discussion of 'folk linguistics' in this introduction, hopefully may help to correct the situation. The history of linguistics, like linguistics itself, must aim for universality of scope and explanation.

For the general study of the development of classical traditions of national philology, Kroeber (1944) has a chapter on philology which may serve as an introduction. The material for the several lines—Indian, Chinese, Japanese, Korean, Arabic, Greek, Roman, and others—is unfortunately difficult of access for any but the specialist in each. Dispassionate case studies, conceived so as to facilitate comparative study, are greatly to be desired. In some cases a particular scholar might be able to provide comparison (Japanese: Korean, Indo Aryan: Dravidian, etc.). Ultimately the roles played by type of linguistic study could be assessed.

In this last respect, our present recognition of linguistics as an independent discipline can do us a disservice. (Nor does calling it a branch of some other recent discipline, such as cognitive psychology, help.) The treatment of linguistic structure has been closely linked to other interests, such as logic, rhetoric, poetics, philosophy, theology—in short, with the uses of language recognized and valued by the societies in which lines of national philology have emerged, or to which they have spread. Some modern histories do treat together two such foci: Steinthal (1863) joins classical linguistics with logic, Sandys (1921) joins classical linguistics and literature). Mostly today we get treatment of one contemporary focus of interest—linguistics,

logic, literary criticism, philosophy, rhetoric—by itself. We do not get treatment of the study of language, or the field of linguistics, as conceived and conducted in history itself. The separateness is understandable, and difficult to surmount, but it is unfortunate for our understanding of the development of the subject, cutting off essential aspects of generalization and explanation. (It is also unfortunate for the contribution that comprehensive historical studies could make to a general theory of the use of language. From one standpoint, the history of linguistics, if it seeks explanation, is an aspect of sociolinguistics.)

The traditions of national philology of course persist past the emergence of general linguistics, and the interactions between the two are of special interest. One such interaction is the emergence of new traditions of 'national philology', initially motivated by general linguistic concerns, as when the work recorded and published by an anthropologist or missionary comes to be the baseline for later activity in a traditionally nonliterate community. It is a distinctive trait of the period of general linguistics indeed, that many 'national' philologies are begun by strangers to the people and language studied (e.g., Eskimo philology by the Danes). This fact of provenience may reflect normative attitudes, as when vernacular Semitic languages and various creoles are written down and analyzed by foreigners who do not share the attitudes toward them of their own users. At this step one has elementary or anthropological philology (which can be roughly defined as the philology of peoples without indigenous philology of their own). At a later step an interplay between indigenous and exogenous traditions, concerned with the indigenous subject-matter, may arise. Noteworthy in this connection are Whitney's arguments as to the 'autonomy, equality, and in some respects superiority of Western studies of Indic philology, as against those conducted within the Indian setting (1867). Of course, those who work in the name of general linguistics may confuse themselves with the office, and ignore or distort indigenous testimony that is correct.²

General Linguistics

We think of the history of linguistics principally in terms of the third of the three stages, that of *general linguistics*. Particular philologies and lines of work are considered of interest insofar as they contribute to the development of this stage. Yet it is difficult to say when and where this stage begins. Generalizations about the nature of language and of grammar were made in ancient India and in classical Mediterranean antiquity. Given the continuity in the history of ideas in both Indian and European civilization, a general

linguistics broad enough to include aspects of language beyond technical grammar can easily be traced, then, for some 2500 years.

A generation ago it would have been common sense to say that a general linguistics grounded in knowledge of but one or a few languages could not be truly 'general' in a scientific sense. The history of linguistics proper would have been felt to begin with the development of a methodology giving a new depth of insight into particular languages by means of comparison of many. In particular the kind of depth and of comparison associated with the separating out of language study as a discipline with academic chairs, the comparative-historical approach of the early nineteenth century, would have been seen as the start of linguistics proper. As will be seen with regard to section III of this book, that starting point seems now more an institutional than an intellectual 'breakthrough'.

We are familiar with the increase in interest in the history of linguistic ideas stimulated by Chomsky's notion of 'Cartesian linguistics' (Chomsky 1966). Chomsky brackets the hitherto celebrated history of true linguistics in the nineteenth and twentieth centuries, to find predecessors among grammarians and philosophers of the seventeenth and eighteenth centuries. Yet there is an element of contradiction in appeal for a starting point to the period of Descartes. To be sure, it fits well with the general history of the rise of natural and social science, which must be located in the period. That has long been recognized by historians of natural science, and is becoming widely recognized among students of the history of the social sciences. The nineteenth century is the century of the separating out of special disciplines, of great advances in institutionalization and in empirical scope and precision of method, but the seventeenth and eighteenth centuries saw the development of comprehensive frames of reference, initial institutionalization, and accomplishments of still some validity. But the appeal to 'Cartesian' precedent is strange in a science. Perhaps some physicists appeal to seventeenth century precedent as warrant and legitimation for their work, or out of a sense of honor to neglected merit, but one is inclined to doubt it. We have then the somewhat contradictory spectacle of an approach hailed and felt as a triumphant new 'paradigm' (cf. Koerner 1972), an approach which eclipses its immediate predecessor, as a paradigm should, but which is so far from having a sense of building upon cumulative efforts, as to seem to want to regret a century and a half, because of what is regarded as philosophical misdirection.

It is clear from Chomsky's own account, and even more so from the accounts of his critics, that the original 'Cartesian' linguistics does not represent a successful scientific paradigm; it represents rather a tradition of inquiry, a set of questions and problems, which were addressed for a time, and which are now addressed again. In effect, the clearest case we have of a

linguistic 'paradigm' in the United States declares the history of general linguistics to be a history, not of paradigms and continuity, but of traditions and discontinuity.

I think that this view of the history of general linguistics is correct, and not only for the past, but also for the history that is accomplished in the present and the foreseeable future. Let me try to show some of the reasons why.

On Paradigms and Cynosures

It may appear anachronistic to assess a notion about the history of a science in terms of the recent and contemporary scene, especially when space cannot be taken to develop and support statements that are not neutral to current controversies. I can refer to other places where some basis for the view taken here is given (Hymes and Fought 1975; see also the historical treatments in Hymes 1970a, 1970b, 1971a, 1971b, 1972, 1973a). And I believe it is necessary to discuss the notion of 'paradigm' in this way, because its interest for linguists is partly a function of their sense of the contemporary scene, and because the notion has indeed entered into the contemporary scene as a factor in its development. Kuhn may have intended the notion of 'paradigm' to illuminate the past of science, but of course the notion also involves a conception of the nature of science, specifically, of the nature of scientific progress. This aspect of the notion has been taken up as something of a scenario of what future progress will be like. A good deal of controversial literature takes the form, not just of the adequacy or inadequacy of this or that finding or idea, but of contention for being the next stage in a process conceived as unilinear. All this has roots in the experience of linguistics itself, of course; the notion of 'paradigm' may be as much a convenient label as anything else. In any case, the joint result is unhealthy, I think, for genuine progress in linguistics, and I am sure that it is disastrous for progress with the history of linguistics. The lessons drawn from recent experience, and the interpretation given to the notion of 'paradigm' are both, I think, incomplete. To assess the two together may contribute something to current linguistics; it is indispensable, I think, for development of the history of linguistics.

The term 'paradigm' is deceptive. It answers to something real in the recent history of linguistics, but not to enough of reality. Many linguists have rightly sensed in the success of the Chomskyan approach something corresponding to Kuhn's concept of a paradigm, as a revolutionary, eclipsing, general view (cf. Thorne 1965, Grace 1969, Wells 1963, Voegelin and Voegelin 1963, and Lounsbury 1961). One can also see the dominance of a 'neo-Bloomfieldian' approach from late in the Second World War until the

emergence of Chomsky's approach (as in important part a thoroughgoing reaction against it) as constituting a preceding 'paradigm' in the United States. One cannot readily identify a central 'paradigm' before then (and it is one of the grave defects of the discipline's current account of its own history that the 'neo-Bloomfieldian' approach is left to stand for all of American structuralism preceding Chomsky (on the fallacies involved in this, see Hymes and Fought 1975; on the Sapir tradition in this period, see Hymes 1971a).

These temporal facts indicate what is missing. The notion of 'paradigm' has been taken to refer to philosophies of science, to psychological assumptions, to analytic practices, but not to social realities. Yet it is no mere coincidence that the dominance of the 'neo-Bloomfieldian' approach came with the emergence of linguistics as a distinct academic profession, separate from philology, language departments, and anthropology departments, in the United States. The approach provided the ideology for a separatist movement. Numbers grew, departments and programs grew, focussed around the new concepts of structural units and relationships, and concomitant methodology; and the views that were furthest from those of the preceding period (and other contemporary disciplines) had the most appeal. The phenomenon is frequent enough in political life for us to recognize it in our own profession. The Chomskyan approach seized hold of linguistics coincident with a second surge in the profession. The preceding group had sought independence. The Chomskyan group inherited independence, and attracted numbers of new students, who entered linguistics as if it had begun with Chomsky, having in turn a novel conception of structural units and relationships, and methodology.

In their political aspect each approach in turn has dominated journals, professional meetings, textbooks, and the like. But neither has been exclusive holder of the stage. Each has had the center, but not the whole. Perhaps the same sort of thing is found in the history of the natural sciences—one cannot tell from Kuhn's account, which is largely silent on the articulation of paradigms to each other, within disciplines, and between disciplines. In the case of linguistics, at least, other approaches than the dominant ones continued, and indeed sometimes emerged, contemporaneously. One can say that each dominant approach was successively the *cynosure* of its discipline. One can say that its participants, and others around them, had a *consciousness* of a revolutionary change, and that there was indeed a *paradigmatic community*. The paradigmatic community, however, has never come to be equivalent to the whole of the discipline. Nor has this been due to holdovers from the past alone. Each new 'paradigm' simply has not succeeded in establishing complete authority. For example, the authority of the 'neo-Bloomfieldian' approach was never accepted by such major figures as Swadesh (cf. his 'mentalistic' critique of it [1948]) and Pike (1954 is a major

source for his views). In important part they considered themselves to be continuing the valid structuralist tradition of Sapir; in part, their influence was limited because of political and religious commitments, respectively. In the time-honored manner of economically and politically successful approaches, members of the inner paradigmatic community could afford to mock them, rather than have to take them seriously. Similar conduct of course has been familiar within the succeeding Chomskyan approach, in regard, say, to the work on universals of Greenberg. But the lines of work represented by these and many others have not simply lingered on to die. They have had varying fortunes, but many have thrived or been revitalized. How can this be so, if paradigmatic succession is unilinear, in the image of an Einstein succeeding a Newton as law-giver?

Part of the answer is that each approach has not been a matter of scientific methods and findings alone, but also a complex of attitudes and outlooks. The congeries of attitudes and interests of each has been roughly in keeping with ingredients of the outlook of the youngest generation in the period in which each came to the fore. On this more specific scale, as on the broader scale of international changes over many generations, *climates of opinion* play a part. (Malkiel and Langdon 1969 discuss, for instance, the role of reaction against German culture in the acceptance of the Swiss-French linguist, de Saussure, after the First World War, as well as in the local popularity, for a time within Germany, of Vossler's idealism. Bloomfield's 'mechanism' and its appeal to young American linguists can hardly be understood apart from the skeptical climate between world wars). Particular *social origins* are a factor as well, although seldom considered by linguists engaged in their own history. It seems significant that the leaders of the 'neo-Bloomfieldian' approach after the Second World War had mainly entered the new profession of linguistics from backgrounds in European languages and literatures, and work in American dialectology, as against the anthropological and field work backgrounds of a number of established figures who did not accept their hegemony. There is in fact an irony here, in that the militant behaviorism of the 'neo-Bloomfieldians', along with their concentration on phonology, is often explained in terms of anthropological field work. The truth of course is that structuralists everywhere concentrated on phonology at the time—that is where the structural breakthrough had been dramatically accomplished, the new territory that traditional grammar had not conquered, and that leading fieldworkers such as Morris Swadesh and Kenneth Pike, men most notably concerned with practical field procedures, were vigorous opponents of behaviorism and algorithms.

In short, membership in the self-conscious paradigmatic community has entailed allegiance to a variety of ideas, only some of which have been essential to new insights and genuine problems. (In this volume, Kiparsky

provides a fine example of this phenomenon with regard to the generation of the Junggrammatiker in the seventies of the nineteenth century.) One could rightly understand one's work as contributing to the advance of the subject without sharing the conscious ideology of, and membership in, the group commanding the center of the discipline. In this respect, linguistics in the United States clearly has not corresponded to the picture of a paradigm initially provided by Kuhn.

The complexity of the relationship between the community holding the center of the stage, and the discipline as a whole, goes further. Just in terms of scientific content, each has comprised a bundle of features, understood from the beginning by some to be not necessarily interdependent (cf. Wells 1963), of equal merit, or of equal relevance. In some cases, indeed, relevance could not readily be found. To illustrate: Chomsky has maintained that linguistics should be considered a branch of cognitive psychology. This view is accepted by many linguists. But how is, say, Romance Philology, to be understood as a branch of cognitive psychology? The world of the serious study of languages is in fact composed in large part of persons trained in, and responsive to, the traditions of work of particular languages and language families. To this extent work in linguistics inevitably reflects some of the diversity of national and cultural contexts, attitudes and interests, associated with the cultivation of these diverse subjects. A student of the Romance languages, a student of American Indian languages, a student of West African languages (in Francophone or Anglophone territory), a student of the creolization of languages, has each to come to terms with different contexts. Not least in these contexts may be the interests and aspirations of the speakers of the languages, which may place cognitive psychology, in the sense of generalizations about the human mind, below literature, say, or national development.

Such illustrations bring us to the nub of the matter. One response to such cases would be to say that they are external to linguistics; Romance philology, perhaps, is not part of linguistics at all. On this answer, the nature and history of linguistics clearly is not continuous and cumulative. (Hence, so we have noted, Chomsky's rediscovery of pre-nineteenth century philosophers and grammarians.) Might one conclude that linguistics has only properly existed in such times and places as current definition of its nature has flourished? That might suit a self-image of having appeared essentially *de novo*, and avoid questions of indebtedness to immediate predecessors and contemporaries outside one's paradigmatic pantheon. Some people indeed write histories, or paragraphs on history, that come to little more than hailing moments of 'true' linguistics, and speculating as to causes of the mysterious prevalence of 'false' linguistics at other times. One would think, however, that Kuhn's monograph would have warned adherents of the notion of 'paradigm'

against such travesty. Carried out in a thoroughgoing way, indeed, it would lead to the necessity of devising a different name for the study of language in excluded times and places (a hand stamp, to imprint 'Not Linguistics' on offending materials, might be useful). One would have to pose as perhaps the crucial question of the history of the subject, why did (true) linguistics *disappear* during, say, the nineteenth century? Was the Humboldtian tradition from Wilhelm himself, through Boas, Sapir and Bloomfield (cf. Bloomfield 1914, ch. 10) responsible? How was it possible to reinvent linguistics so quickly in the 1950's, when it had not existed for so long?

More seriously, there is an obvious generalization, when the human sciences are considered as a whole, namely, that the intellectual periods we loosely designate as the 'Eighteenth', 'Nineteenth', and 'Twentieth' centuries have had characteristically different dominant orientations, and that these have been shared by linguistics among other disciplines. The generalization only states an obvious fact, it does not explain it. The generalization does avoid an absurdity, and narrow our choices essentially to two. The first is to think of different interests and orientations as competing to be 'linguistics', with one succeeding in displacing another at intervals. Such a choice is implicit in most histories of linguistics, which, like most histories of other disciplines, adopt a quasi-Hegelian perspective, telling a chronological story that moves from place to place, with the 'Spirit' of progress. (Such a choice may also agree with the outlook of those who reserve phrases such as 'linguistic theory' for their own work.) The result, however, is to make of the history of linguistics something very much like a game of 'King of the Mountain'. And, like the absurdity suggested above, it leaves much out of account, and much unintelligible; continuities and causes are hard to come by, and most of the development of linguistics is in shadow or off-stage most of the time.

The second choice is to think of different interests and orientations, different lines of work, as all part of linguistics, although competing for attention and such limelight as is available. From the standpoint of past history and present humility, this is the better conception. It makes possible a general study of the history of the study of language.

Our subject, then, is a general *field* of the study of language, of *field of linguistics* (cf. Hymes 1968). The very importance of language to human life, which linguists often note, implicates language in facets of human life that engage the attention of many disciplines and perspectives. At a given time, in a given place, the range of interests in language is variously institutionalized, variously distributed among disciplines, and flourishes differentially. Within the general field, the cynosure, or paradigmatic community, at any one time is just one of the communities and kinds of interest in language that serious scholars pursue. (For an instructive case study, see Malkiel 1964, and cf.

Malkiel and Langdon 1969; cf. also Hymes 1963 and 1970a for another case.) Many, but not all, of these interests are pursued within the professional, academic discipline. Their diversity has not yielded to any synoptic perspective, and, short of a millenium for the human sciences as a whole, is not likely to.

All this is not to dismiss or ignore the relevance of the notion of 'paradigm', as sketched by Kuhn. Despite the controversy over the notion, it is clear that it has caught essential features of the history of scientific and scholarly disciplines. Yet with linguistics, there is this irony. Kuhn intended his notion to counteract the bias of practicing scientists, in favor of a picture of their discipline's history as one of continuous, cumulative progress; the bias failed to capture the genuine discontinuities of meaning and purpose, by assuming that earlier work was to be assessed as answers to one's own questions (one's own questions being taken to be the only and permanent questions—cf. Collingwood 1939). The success of Kuhn's notion among a younger generation has encouraged a bias of practicing linguists toward their own history of a complementary kind, in favor of a picture of their discipline's history as one of great gaps and discontinuities, and, so far as the history informs the present and future, toward an image of 'permanent revolution'. But whereas Kuhn would have thought that recognition of discontinuity would carry with it recognition of the need to interpret other periods of linguistics in their own terms, the assumption has persisted that earlier work is to be judged in terms of one's own questions; periods perceived as discontinuous and different from one's own are ignored or left to the mercy of stereotypes. Instead of the whole of the history of the discipline seen as a progress toward the present (the so-called 'Whig' interpretation of history), we have selected moments of the history of the discipline, seen as types of a scholarly Old Testament anticipating the salvation in the New. This gives us the same kind of bias, and less history.

There is a great virtue in Chomsky's appeal to 'Cartesian' linguistics; it shows that the understanding of past efforts, not at first thought pertinent, can have interest for our own work. If this promise of a dual benefit, to both history and present science, is to be realized, however, what has been made of the notion of 'paradigm' must be analyzed critically. The conception of a field of linguistics, advanced above, suggests two themes of special importance. One has to do with recognition of the continuities that occur across changes of paradigm and paradigmatic community, and that need to occur; the other has to do with sources of change in paradigm that come from outside linguistics itself, stemming from its social contexts. In both respects we must criticize ways in which the notion of 'paradigm', as an implicit theory of the nature of science and scientific progress, may be used as a scenario.

'Paradigm' as Scenario

Certainly there is something to a conception of the history of linguistics as exhibiting a succession of paradigmatic triumphs. Since the academic institutionalization of linguistic study early in the nineteenth century in Prussia, successive groups of scholars have been able, with some justice, to identify advance in the field as a whole with their own activity. To a fair extent, there has been consistency across countries and periods. The comparative linguistics that first took root, as a cumulative discipline, in Germany, became the standard-bearer for study of all the world's languages in whatever country during the course of the nineteenth century. The structuralism that took root in Europe outside Germany, after the First World War, became the standard bearer for our own century.

The difference from a view of paradigms as successively replacing each other lies in this: comparative linguistics has continued to develop. If the qualitative study of grammatical structures should be succeeded as *cynosure* by a quantitative paradigm, concerned with variation and use, still structural linguistics will continue to develop. What changes from one to the other is not so much explanation of the same phenomena, but the phenomena one wants most to explain. One does not supersede the other, but shunts it from the center of the stage.

It is true that comparative-historical linguistics has benefitted from developments in structural linguistics, and has changed in consequence, since the early integration of the two in the work of Bloomfield in the United States, and Jakobson and Trubetzkoy in Europe. Yet the benefits and integration are not the whole story, and the influence is neither in only one direction or unequivocal. The autonomy of comparative-historical linguistics is in fact essential as a check against rash and unfounded claims, inspired by a new paradigmatic community (cf. Maher 1973); and if approaches in terms of variability and 'dynamic synchrony' (Jakobson's term) come to the fore, comparative-historical linguistics will have more than a dependent relation to synchronic interpretation of dialects, creoles, and languages generally.

The same sort of thing holds true with regard to the successive foci of structural linguistics itself. The early structuralist approach in the United States tended to focus on phonology as an autonomous subject, neglecting non-phonemic phonetic phenomena on one side, and grammar on the other, in its enthusiasm for the discovery of the phonological sphere itself. In the first flush of enthusiasm for syntax as a deep and central sphere, the initial Chomskyan paradigm denied the relative autonomy of phonological study, on the one side, and of semantic study, on the other. Now the second generation generative approach, 'generative semantics', has denied the relative autonomy of syntactic studies, on the one side, while seeking to preserve intuition and

logic as sufficient bases for dealing with social context, on the other. The enthusiasms have been right in affirming a new sphere of relationships and (in some respects) its autonomy from others, or the dependence of others on it. The enthusiasms have been wrong in dissolving or denying the integrity of the other levels. A variety of structural, historical, and social factors combine to require recognition of the relative autonomy of each, and consequently, the possibility of some continuing independent study of each. (There is evidence of the truth of this analysis in the way in which previous approaches, and lines of inquiry within phonology, morphology, lexicography, and the uses of language, are coming to be reconsidered and revalued by some participants in the Chomskyan paradigm.)

Kuhn takes for granted that a new paradigm, a new outlook, is not just different from a preceding one, but successful because superior; in particular, the new paradigm explains new things that the old could not, but it continues to be able to explain what the old one could as well. Within linguistics, the successive 'paradigms', or *cynosures*, have not fully had both properties, which account, of course, for much of their failure to command complete authority within the field.³ This gap between true 'paradigmatic' status and the lesser authority of a '*cynosure*' may, indeed, help explain the polemical overkill that has characterized a new group's treatment of the predecessors and competitors. (The neo-Bloomfieldian reaction to traditional grammar, and the Chomskyan reaction to preceding structuralism, are cases in point.) It may be that, one's scientific base not being sufficient for complete hegemony, rhetoric and ideology have had to be called upon to fill the gap.

In any case, both on the broad scale of the 'neo-Bloomfieldian' and 'Chomskyan' approaches, and the scale of successive foci within the analysis of language, from phonology to conversation, we seem to be dealing with something more complex than scientific advance. The tendency for each successive focus of interest to present itself as a fundamental change, eclipsing and antiquating others, suggests that Kuhn's notion of paradigm, when diffused into linguistics, has suffered the fate of many an idea that crosses boundaries. It has lent itself to reinterpretation in terms of the successive, scientific-*cum*-ideological successes of the 'neo-Bloomfieldian' and Chomskyan groups, and, more recently, to individual role-modelling in terms of the image provided by the dramatic conjuncture of Noam Chomsky's brilliance, personality, training, and (later) moral-political stature; the arrival of structural linguistics in the United States at the frontier of formal models of syntax that would go beyond descriptive classification; and the influx of a new generation into a disciplinary scene, temporarily dominated by established scholars of middle years. The attractiveness of Kuhn's notion undoubtedly is due to the fact, as noted, that it summarizes, and dignifies, a genuine sense of the recent past. The difficulty is that it seems to come to summarize and dignify also an aspiration, that of a scientific triumph that will

constitute the new 'paradigm', with oneself perhaps as Chomsky II (or III or IV). And where Kuhn intends his notion to designate unequivocal scientific revolution, there is reason to think that in linguistics now 'revolution' becomes confused with another phenomenon, that of being 'center of attention' (what is here dubbed *cynosure*).

The implicit scenario for succeeding as *cynosure* seems often enough to consist as much of discrediting, and forgetting or ignoring, other work, as of making new discoveries and of integrating what has already been discovered on a new foundation. The temptation to join, or create, a contender for 'King of the Mountain' and disciplinary *cynosure* seems almost irresistible, once the possibility of success in this regard is scented. In both the clear-cut cases, the only cases experienced by most linguists, the neo-Bloomfieldian and the Chomskyan, there has been a heady atmosphere at the center of building everything new. Linguistics in the United States, in short, seems to have developed its own 'tradition of the new', and to have taken on, perhaps not surprisingly, cultural characteristics shared with urban developers and the Army Corps of Engineers, (contrast the last sentence of Sapir 1924 [in Mandelbaum 1949: 159]).

There is satisfaction and, indeed, gaiety, in this. As Yeats put it, "All things fall and are built again/And those that build them again are gay" (in his poem, 'Lapis Lazuli'). The trouble is that not all things had in fact fallen, requiring to be rebuilt. One needs to bear in mind a point that Kuhn himself has now accepted, namely, that the phenomena of paradigms and paradigmatic succession can be found outside the sciences. We tend to think of paradigms in terms of intellectual innovation winning its way by merit, but what is fundamental to the whole thing, I think, is the existence of the paradigmatic community. Kuhn himself has nothing to say about the criteria by which the scientific status of the successive paradigmatic innovations is guaranteed. It would seem entirely sufficient, to produce the phenomena he describes, to have a community, guided in problem-solving by a dominant model, and sharing over any particular period of time some common commitment to the purposes of both. Given these ingredients, one might find an unending series of successive 'paradigms', as one takes up the anomalies of another, without any continuity of progress to be observed across the whole. To a skeptic or atheist, the history of theology may appear to be of this sort; to many a natural scientist, the humanistic disciplines may seem to afford many examples. I am afraid that current linguistics will provide a few, although I think that there is the possibility of cumulative advance in the fields indicated, and am far from denying the genuine advances in parts of current linguistics.

The problem is this: some of the sense of advance in current linguistics is due to true discovery. Some is due to rediscovery. The sense of progress and building anew is made possible partly by the opening up of new

perspectives, but also partly by ignorance and incomplete training, on the one hand, and by the pursuit of artifactual phenomena, *ignes fatui*, on the other. It has become possible for a leading scholar, fully sympathetic to the mode of formal linguistics now dominant, to distinguish between work which contributes to knowledge of a particular model, and work which contributes to knowledge of language. (He finds relatively little of the latter.) Such a situation is due in part to the diffusion of the culture of mathematics and logic, in which precociousness with abstract relationships is naturally highly valued, as well as to the fact that Chomsky's personal goals for the use of linguistic findings place a premium upon pin-pointing universal underlying formal properties. There is a permanent role for formal ability of this kind in linguistics, a role established by the work of Harris, Greenberg, Hockett, Hiž, Chomsky and others, but a limited one. Many of the problems of linguistics do not yield to such an attack, and cannot wait upon or much use the results of such an approach. (See Hymes 1973c for fuller discussion.) Fortunately or unfortunately, depending on one's view, the problems of linguistics require a diversity of kinds of talent, and many of them always will require skill in the acquisition and interpretation of elementary data, on the one hand, and upon cumulative knowledge and experience, on the other. In this respect, much of linguistics is inescapably akin to the humanities, in which major work often must come later, rather than earlier, in a career. More generally, much of the difficulty is due to a tendency of longer standing, which may be called 'simpling'. Like sampling, 'simpling' is a technique for reducing the complexity of reality to manageable size. Unlike sampling, 'simpling' does not keep in view the relation between its own scope and the scope of the reality with which it deals, but denies the difference (as 'not linguistics', perhaps). It then secures a sense of progress by progressively readmitting what it has first denied. 'Simpling' is as much a professional fact as a scientific one, and is unfortunately easily confused with genuine simplification by valid generalization.

The prestige of the notion of 'paradigm', then, may well have a disabling effect on the discipline. We would do well to ponder instead the 'darker' side of the notion. For Kuhn (1962) can be read as describing scientists as sacrificed victims. For the sake of the ultimate progress of science, they commit themselves to a kind of 'tunnel vision'. The implications of a certain paradigm can thus be worked out completely, to the inevitable point at which it breaks down, and a more adequate one is propounded successfully, usually by someone outside their own group.

Possibly the authoritarian, even totalitarian, character of the notion of paradigm, as presented by Kuhn, is a good description of what the progress of particular lines of science has been like. If so, it remains unclear as to how the notion relates to the mutual articulation of the various branches of any

science as a whole. At what level is a drastic change a paradigmatic change? And may it not be possible that the kind of rigid control described by Kuhn reflects a particular phase of the history of science, and is not inevitable? To be sure, any closely knit community is likely to develop its own view of itself and its relation to the rest of the world; it is likely to have its own internal discipline and values. But the rigid control described by Kuhn for science suggests something of the characteristics of the upwardly mobile, especially the *petit bourgeois*, in many societies. One's claim to respectable status being suspect, or not universally granted, one's insistence on rectitude is all the more intense. Such motives have had much to do with the doctrines of correctness in language over the generations. Perhaps they have had something to do with the self-consciousness of scientists. Perhaps, then, Kuhn's account of 'paradigm' can lead us, not to emulate it as a badge of status, but to transcend it. Perhaps it can lead us to recognize the ingredients of paradigm in any cohering community of scholars, and also to recognize the multiplicity of such communities within our discipline, conceived as the field of linguistics as a whole. We could then accept that different of these communities answer to the interests and needs of different extra-disciplinary communities, different aspects of the role of language in human life. In our media-mediated world it would no doubt be impossible to escape entirely the pressure to have some one community touted as 'the' community. Linguistics no doubt will continue to appear in the pages of *Time Magazine* and the *New York Times*. But we can lend ourselves to the need of the media for stereotyped struggles and victors as little as possible. In sum, critical reflection on the concept of 'paradigm' may lead us to transcend the 'paradigmatic' phase, and enter into a phase that is pluralistic in ideal and in practice—even, to borrow an analogy from Chomsky's political interests, anarchistic.

Such a perspective is at least clearly required for the understanding of the past development of linguistics. It is to repeat, not adequately understood as a succession of *paradigms*. Rather, it has had a history of the rise, and variegated development, of a plurality of *traditions*. These traditions of inquiry, sets of problems, have each their own record of continuity in and of themselves. In relation to the center of the intellectual or disciplinary stage, to the succession of *cynosures*, as it were, the record is one of discontinuity. So much, as we have seen, is implicit in Chomsky's recourse to 'Cartesian' linguistics. And so much is evident in the differential sense of relevance of different current vantage points. The twentieth century development of glottochronology, for example, motivates analysis of hitherto neglected nineteenth century developments (Hymes 1974a). A particular professional affiliation and subject matter created a particular vantage point on the history of the study of language as a whole (cf. Hymes 1963 for one such; part of this

introduction draws on that essay). New linguistic interest in discourse, rhetoric, and poetics may bring their past histories within the felt scope of the relevant past.

All this leads to the conclusion that the self-consciousness of the members of a paradigmatic community—the aspect of historical change to which Kuhn's notion essentially appeals—is a treacherous guide. It is an element, but only one element, in a situation. Its sources may not be solely scientific, intra-disciplinary, but may be extra-disciplinary as well (a prospective instance is noted below), and its understanding of what it is doing, and why, may be very partial, even quite mistaken (cf. Hoenigswald's paper of 1974). The actual history of advance, and more generally, of change, in linguistics in the present period, as in past periods, appears a far more tangled matter than can be sorted out at first glance.

Cynosures and Contexts

To delimit the subjective element is not to eliminate it. For one thing, it is an essential part of the history itself, and, for another, an essential part of motivation to write the history. The effective scope, and the internal organization, of what is taken to be the history of linguistics, will change, as contemporary interests and cynosures change. One can simply accept the rewriting of history with each change in cynosure, since it no doubt will always occur. More responsibly, one could insist that histories, although inescapably subjective in part, be also consistent with the data and each other. In this, at least, lies the irreducible difference between the art of the novelist and the scholarship of the historian. The former is free to express insight through imagining the detail, motive, or relationship that should have been true, and to accept different logics of situation in different books. In histories of linguistics it should be possible to recognize a common subject matter by more than the recurrence of certain key characters. It would be rash to say that this is possible today.

The two requirements would seem to be, first, cultivation of basic scholarship, so that eliminable conjectures and misrepresentations are indeed eliminated; and second, a comprehensive conception of the scope of what is to be written about. Research such as that represented in this book, the first of its kind, so far as I know, is prerequisite, as is the development of basic sources of information (such as a *Biographical Dictionary of Linguists*, once planned by Sebeok, and Koerner's series of classics (Amsterdam, 1973-), stock-takings of current knowledge (as in the volume on the history of linguistics, vol. 13, of *Current Trends in Linguistics*), and the provision of forums for sustained discussion of research (such as the new journal,

Historiographia Linguistica, being launched under the editorship of E. F. K. Koerner). The second requirement would seem to be to find vantage points for the conception, and writing, of history that are more adequate than the Hegelian highlighting of disconnected great books and moments that the usual history resolves itself into, and that the 'paradigm' notion has been made to lend itself to. The history of linguistics in relation to any one *continuing* community of scholars, or social community, such as a nation or region, clearly would not show an unrelieved chain of successes and advances. By comprising ups and downs, and shifts within a sociologically continuous group, such a history would make possible an actual account of causes of change. In short, to use current terms, a 'sociolinguistic' approach to the history of linguistics is necessary, if it is to approach 'explanatory adequacy'. Such an approach might be dubbed the study of 'cynosures and contexts', insofar as it takes its starting point from the former.

In the writing of such history one would seek to discover the range of interests in language of a given place and time; to place both central and peripheral traditions, or competing central traditions, in relation to each other; and to interpret the pattern, or organization, of diversity thus found. Clearly merit alone will not explain the centrality of a tradition, and concentration of personal merit in the tradition may be a response to its centrality as much as a cause of it. In treating a cynosure, or central 'paradigm', over time, one will attend not only to the fascination of its introduction and the salient facts of its development, but also to the recognition and embracing of it by one or more groups, which see in its application an opportunity for the protection or advancement of its own interests. One will see not only the benefits, but also the costs, of the innovation, and attend to the specific problems which exploitation of the development of the paradigm may create, together with the responses thereto. And one will attend to the justifications which participants in the paradigmatic community offer for their roles, and which others offer for their relationships to the paradigm; what, from another or later point of view, may appear as rationalizations. In sum, one will deal with the occurrence of a paradigm, or cynosure, as more than an intellectual accomplishment; one will deal with it as a process of sociocultural change.⁴

Attention to community contexts is likely to be necessary if one is to understand the factors shaping the present and future history of linguistics. Within linguistics in the United States, the almost exclusive study of their own language, English, by so large a proportion of the world's linguists, has seemed to the participants a source of deepened insight into the underlying structure of all languages. Leaving aside the methodological difficulties that have become increasingly apparent, we must consider that to many other

communities, including those of American Indians, such a concentration may seem an expression of ethnocentrism at best, a hostile turning of the back at worst. Proclamation of universals without need to engage the languages of the peoples of the world may strike some as reactionary. In any case, many participants in formal linguistics are liberal or radical in social views, and yet their methodological commitments prevent them from dealing with the verbal part of the problems of the communities of concern to them. This extra-disciplinary source of tension is increasingly felt in linguistics (cf. Hymes 1973b). If it has significant effect, the resultant changes will not be due to appearance of a more successful explanation of anomalies within the currently dominant approach. The changes will be due to changes in self-consciousness and motivation, having to do with reconciliation of scholarly and personal concerns. One will have seen the working of a factor that has played a part in past periods of the history of linguistics as well; I have cited this current instance to highlight its perennial presence.

Within the study of cynosures in contexts, as part of a recognizable common subject matter, the subjective element will remain. The issue will not be between what was said and thought, and what was done and accomplished; or 'between different definitions of perimeter; but between different weightings given to phenomena recognized by all. This kind of difference is inescapable, since history obviously does not come in 'natural', 'objectively given' periods and patterns. There are many lines of work and interest connecting generations, and mixtures of novelty and continuity in each. Depending on differences in experience and empathy, capable scholars may differ in the weightings of importance, of continuities and innovations, to which they come. And often they will find themselves in competition for the use of standard terms, whose general acceptance makes them assets, and hence counters. (This phenomenon has been nicely analyzed by Levich 1962, with regard to controversy over the definition of the notion of 'Renaissance'.) With the advent of transformational generative grammar, 'structural' linguistics came to signify an eclipsed predecessor for many younger linguists. Commonly it was characterized in terms of the 'neo-Bloomfieldian' outlook, or, more adequately, in terms of an approach limited to 'taxonomy' and 'surface structure'. There was of course already a complication in defining as 'structural' an approach considered not to deal with the most important part of structure, and denying the term to the approach which did. Recently the rise of challenges to Chomsky's conception of structure has led some to give greater weight to his continuities with preceding 'structuralists', and less to his differences from them. Viewed from the standpoint of work which gives up the notion of a general system to a language, or which regards variation, time, and diversity of function as the bases for an account of the linguistic

means of a community, Chomsky may appear as the great defender of twentieth century structuralist principle (see further discussion in Hymes and Fought 1974 and ch. 7 in this book).

Such issues repeatedly arise, notably recently in discussion of Chomsky's attempt to characterize a 'Cartesian' type of linguistic thought. One man's relevant resemblances will be trivial to another, and the second's crucial difference trivial to the first. (Cf. discussion as to whether or not Du Marsais was a 'Cartesian'.) For Chomsky the important thing about Wilhelm von Humboldt is the respect in which he fits with the 'Cartesian' developments of the Enlightenment. For myself, the important thing about him is the respect in which he leads on to the general linguistics of the nineteenth century, and the tradition of Boas and Sapir. The latter view seems to me to have the better historical foundation, since it encompasses all of von Humboldt's importance, not just a part; but if that argument is in itself an historian's criterion, it is advanced partly in a polemical spirit.

There can be as many histories of linguistics, then, as reasons for interest in the history, and should be. Still, chaos is not the inevitable outcome, and history is not illimitably plastic. We may delight at the discovery of forgotten merit, and relish fresh vantage points on erstwhile familiar ground, but major patterns of cynosure and context, and major traditions will remain fairly fixed points of reference. The fundamental difficulty is that what we can today take to be fixed, common knowledge is so vulnerable. For there has been very little in the way of adequate historical research. Most of what we understand as the fixed reference points of the history of linguistics is a palimpsest of past selective vantage points (cf. again Hoenigswald's study of 1974, and Romeo & Tiberio 1971). The primary task of research is to overcome this limitation. We may wish to choose our own histories, but there is a great difference between choice made in a clear light and groping amidst chiaroscuro.

The papers in Hymes (1974a: 77-232) represent important contributions in this regard. Each has to do with major traditions of the relevant past which our extended horizon now encompasses.

Traditions: The Nature of Language and of Grammar

The general grammar and philosophical grammar of the early modern period cannot be adequately understood as direct reflections of their own period alone. On the one hand, they show little impact of the accumulating knowledge of the world's languages beyond Europe until well into the eighteenth century. On the other hand, they show much of a tradition of grammatical study continuing from the Middle Ages. The medieval sources of

much grammatical theory, indeed, have been emphasized by Roman Jakobson (cf. Salmon 1969: 169-176; Uitti 1969: 81-2). Bursill-Hall (1974), a leader in the investigation of the primary data for knowledge of medieval linguistics, calls attention to how much must be done, before a full, continuous history of linguistics in the period can be written. Grammar enjoyed a privileged position throughout the Middle Ages, and its status in the curriculum is well known, but not the facts as to the nature and development of theory. Most general historians of linguistics, indeed, have been unsympathetic to the period, dismissing it (with R. H. Robins and F. Dinneen as notable exceptions). Bursill-Hall provides us with an account of our present state of knowledge, and of work underway to enlarge it. Implicit in his paper is a general point needing to be taken to heart by linguists. The materials of our history do not remain, waiting to be taken up when one gets around to it; continuing activity is necessary, to preserve them. (Nor is this a problem for remoter periods alone; the records of quite recent developments may be lost, through carelessness or disposal, if no interest is taken in their historical significance.)

Aarsleff (1974) presents in full, almost monographic scope an analysis of part of the 'Cartesian' period that has become already of more than historical concern, and a factor in controversy over current directions of linguistics (cf. Aarsleff 1970, 1971; and Bracken 1972). Controversy aside (I shall return to it below), Aarsleff makes a major contribution to the substance of the history of linguistics. Bursill-Hall deals with a tradition which has been known but neglected; Aarsleff brings into focus a tradition which has been effectively forgotten. Condillac's role has been sometimes noted (significantly by Uitti 1969) but has hardly entered at all into general historical treatment. The essays on the origin of language by Herder and Rousseau are celebrated, but their intellectual background, in debate in the Berlin Academy, and ultimately in the Abbé Condillac's *Essai sur l'origine des connaissances humaines*,—the fact of an extended tradition—has been missed. Aarsleff restores to us a knowledge of this major tradition, set in the problem of the origin of language, as a problem of the nature of language; in his own words, this tradition is "the linguistic theory that underlies and finds expression in efforts to deal with the question of the origin of language".

A consequence of recognition of this tradition is revaluation of Rousseau and Herder in this regard. A further consequence is to unravel the necessary connections that one is often led to assume between views. Those of us old enough to remember, know that structuralism in the hands of most linguists of the period before Chomsky was compatible with, indeed interwoven with, a mentalistic view (cf. Swadesh 1948, for one 'taxonomic linguist's' attack on mechanism), and directed toward the discovery of semantics and language universal. Aarsleff's scholarship enables us to

'remember' that a view of language as the distinguishing feature of man, and as a subject whose study gave insight into mind and nature of man, was part of the origin of language tradition as much as of the tradition of universal grammar. Both saw language as striking proof of man's creativity, and a condition for further creativity; both had rationalist qualities.

The two traditions became interwoven, and Aarsleff suggests that the interest in universal grammar in the later eighteenth century, and the insistence of the Romantics, chiefly in Germany, on the creative aspect of language and speech, both derive from the influence of Condillac.

Aarsleff's work has become part of controversy over the political consequences of ideas, associated with various theoretical dichotomies, and symbolized by the figures of Descartes and Locke. As for the political consequences of ideas, one can only observe that some critics employ an amazing double standard. Aspects of Locke unsavory today are singled out, while his anti-royalism and forced exile and his stimulus to the American and French revolutions are ignored, as is the fact that in his own day and long after he was attacked as subversive of religion and authority. Nothing of the political position or implications of Descartes are noticed (Bracken 1972). No matter, Locke, liberalism, and empiricism are attacked as 'counter-revolutionary' (Bracken 1972). But the association between theoretical views and political implications is seldom so tidy. The meaning of views is interdependent with context. One would indeed have thought it a commonplace that the revolutionary method or concept of one period may become the conservative force of another, and conversely. (Presumably it is the present, not eighteenth century context that motivates attack on Locke as counter-revolutionary.) Despite the attempt, in Marxism or Christianity, to unite ontology, epistemology, social position, and convictions into a single package, both experience and analysis have shown such 'packaging' to be a matter of empirical concatenation, not logical necessity. Attempts to enforce such packaging are totalitarian in scholars as in governments. One might indeed play this particular game by treating 'Cartesian' linguistics as an ideology of elitist intellectuals, who wish to claim credit for views of the greatest radical consequence, while keeping professional hands utterly clean from the actual problems of language in society today (cf. Hymes 1973b). If 'Lockean empiricism' = liberalism = counter-revolutionary subordination of scholarship (Bracken 1972), then perhaps 'Cartesian rationalism' = 'radical chic'. (I say all this as a member of the editorial board of a Marxist historical journal.)

But let us return from polemic to history. The fundamental point is that the package of ideas presented as Cartesian linguistics does fall apart under historical scrutiny, through positive analysis of a major and distinct tradition of the period following upon Descartes and the work of Port-Royal.

For the period preceding Descartes and the Port-Royal work, Vivian Salmon (1969), cited as 'an exemplary model of the kind of good scholarship Chomsky's work has provoked' (Bracken 1972: 15), politely but utterly demolishes the putative link between Cartesian philosophy and grammatical analysis, together with the claim to originality of the latter. She concludes an extended analysis (1969: 185):

The major 'distortion' (here picking up a word from Chomsky 1966: 73) . . . is the attribution of the form taken by the Port-Royal grammar to Cartesian inspiration with little or no attempt to take into account the total intellectual context in which it appeared. No one would deny the general debt of Port-Royal to Descartes, in whose philosophy they were interested because they saw it as 'a revival of Augustinian thought and therefore an ally of their own kind of theology' (Kneale, 1962: 316). But when we examine the actual details of the grammar we find that they were mainly a reworking of certain features of (then) current grammar, logic and rhetoric which was already characteristic of the grammars of Campanella and Lobkowitz . . . (who) were consciously reverting to the Middle Ages

Much more remains to be learned of the concrete character and development of linguistics in early modern times. Editions of essential texts are much needed (cf. again Uitti 1969: 80-81). When we attempt to interpret the connections and contexts of the work of the period, to provide history (rather than projection), the short history of 'Cartesian' linguistics, and the kind of scholarship manifested in the work of Aarsleff and Salmon, will be essential. Actual connections and underlying causes will have to be sought. We cannot settle for apparent surfaces.

Like the origin of language, and universal grammar, the question of the ranking of the parts of speech was given new direction and impetus with the eighteenth century. In a study by Stankiewicz (1974) we have a bringing to recognition of a topic lost sight of with the rise of structural linguistics in the United States. Stankiewicz shows how one of the oldest topics known to the history of linguistics took on a different character in a different climate of opinion, being formulated as a genetic, rather than a logical, problem. In this respect his study is a substantial documentation of Aarsleff's point as to the invigoration of general grammar within the tradition of the origin of language, and the linking of the two. Stankiewicz shows how assignment of priority to one principal part of speech, the noun, came to be replaced by assignment of priority to the verb. He regards the problem-tradition as one which, unlike that of general grammar and the origin of language, has a chronological end with recognition of the mutual necessity of both noun and verb in the twentieth century. Still, echoes of this problem have continued into recent

years with discussions of the priority of one or the other part of speech in the reconstruction of a language or language family, and with discussions of the starting point within a formal grammar for the assignment and mapping of features of selection between parts of sentences. Mutual interdependence does not preclude differential weighting. The long history of this controversy (extending also into the early history of Indian linguistics, e.g., the *Nirukta*) may seem to us to demonstrate mainly the projection onto linguistic structure of cultural attitudes and values (with consequences for classification of language, analysis of Indo-European, and syntactic analysis); but it may be that such attitudes and values have indeed differentially weighted the form classes of individual languages, and that a revival of the interest of Boas and Sapir in grammatical categories as evidence of cognitive styles (cf. Hymes 1961) may further extend the history of this question.

The recurrence of problems, and their transformation in new contexts, is the theme of the essay by Verburg (1974). He draws on a major work (Verburg 1952), unfortunately still untranslated from the Dutch (but cf. the account by Faithfull 1955). The dependence of the scope of history on current interest is especially well displayed here. A few years ago many linguists might have said that one of the two recurrent orientations traced by Verburg is central to the history of linguistics—the orientation toward the analysis of internal structure—and that the other is philosophy or something else. The growth of interest in sociolinguistics, as analysis of language functions, and the growth of interest among linguists in practical problems, wherein functional questions loom large, makes this pioneering, unique study freshly relevant. Most of all, the study indicates that the question of the nature of language is not exhausted by the question of the nature of grammar.

To sum up the significance of this section: recent years have made linguists aware that their relevant history begins before the institutionalization in the nineteenth century of their profession, but adequate knowledge of that greater history is hardly available. A combination of linguistic knowledge and scholarly competence, rarely to be found, is required. The studies presented here represent substantial additions, and in some respects changes, to what the history has been taken to be.

First Paradigm (?): Comparison and Explanation of Change

Although our retrospective horizons have been enlarged, standard accounts of the history of linguistics have not been wrong to locate a crucial, qualitative change somewhere within the last part of the eighteenth century and the first part of the nineteenth century. The papers in Hymes (1974b)

under the heading of this section (III, 233-358), give reason to think that the nature of the change has not been well understood or explained. Addressing as they do the long-standing conventional origin myth of 'scientific linguistics', indeed, these papers constitute the centerpiece of the book.

Metcalf, Gulya, and Diderichsen (all 1974) show from complementary vantage points that a novel conceptual or methodological breakthrough cannot be an adequate explanation. Metcalf, the principal scholar in his subject, demonstrates that the Indo-European hypothesis, far from being new or sensational, was derivative and even typical of at least one important strand of linguistic tradition in Northern Europe. Religious tradition itself, in the Biblical myth of the Tower of Babel, may be thought to have facilitated the linguistic interest. Metcalf does find a methodological break between the centuries he treats and the later, nineteenth century profession. Addressing the etymological tradition from before modern times, and within the eighteenth century, Diderichsen finds the essentials of the nineteenth century method, however, to develop within the eighteenth century. He can see no sharp break at the time of supposed paradigmatic breakthrough. Gulya casts profound light on the problem from the vantage point of an adjacent language family, the Finno-Ugrian, where the question to be asked is why the paradigmatic breakthrough did NOT take place, given the presence of necessary concepts and methods. Both Metcalf and Gulya discuss the general character of the work they treat, and place it with regard to the notion of paradigm, Metcalf finding a good fit in the sixteenth and seventeenth centuries for Kuhn's depiction of a pre-paradigmatic state, while Gulya adds dimensions to the conception of paradigmatic formation, stressing among other things the need for continuity of accumulation of relevant materials. Diderichsen treats the paradigm usually thought first, that of historical-comparative linguistics of the nineteenth century, as at best second. One original paradigmatic approach in the study of language, he argues, was that of traditional grammar, connected with the establishing of a standard literary language and the teaching of its most important rules. A second such approach, deserving the name of tradition of inquiry and explanation certainly, and perhaps that of paradigm, was the etymological. Not the least interesting aspect of his paper is the informative account given of the roles of Turgot, de Brosses, and Adam Smith, men not usually placed in any paradigmatic relation to linguistics in conventional accounts.

Just as Diderichsen shows us the central relevance of men not usually considered, so Percival (1974) shows that the merits of men usually praised may be misconceived. It takes nothing away from Rask's great achievements to recognize that he was not a literal founder of the tradition of work that followed him, but that principles in his work which seem such foundations to

us were, for him, parts of a quite different general conception, one which was not to be taken up. And just as Diderichsen shows us the continuity in what was later to be perceived as revolution, for all linguistics, so Malkiel (1974) has shown in a particular branch of linguistics. His essay is not less, but more significant for its particularity. The great fallacy and disservice of cursory accounts (to which the notion of 'paradigm' too easily lends itself), is that only the 'great' changes are noticed. The greater part of the actual history of linguistics is lost from view and from accounting. But, as has been argued above, it is inherent in the nature of the subject that it is a congeries of histories, a collection of the traditions of lines of work on particular languages, language families, and language areas. The general changes are partly influenced by their location in their data and traditions of some one or a few such lines of work. Their successes, through diffusion, and the character they come to have in their many particular instauration, depend in important part on continuities independent of them. The interaction of these two forces, a novel method and genre, on the one hand, and long cultivated sources of data on the other, is nicely brought out here. We need many more studies of this kind. Notice that Malkiel finds Diez' truly original contribution, the first union of several ingredients, to be something unadmitted and inexplicit at the time.

The problem of appearance and reality, regarding continuity, has been given a new twist by Kiparsky (1974). Whereas Diderichsen demonstrates continuities across the onset of the conventionally recognized 'paradigm', Kiparsky discovers a hidden discontinuity within it. More precisely, he shows that the discontinuity has been mistakenly located. It does not lie in the formulation of the famous thesis of the exceptionless nature of sound laws, associated with the *Junggrammatiker*, which has often been taken to have defined a paradigmatic 'breakthrough'. Kiparsky points out that men on both sides of that issue contributed to a new kind of historical linguistics. This development, not articulated at the time, was a drastic change in the nature and direction of the explanations sought.

An essay by Hoenigswald (1974) sums up the topic, reflecting on the earlier standard views of the subject—the most cultivated area of the history of linguistics perhaps—and with cautions born of experience. Hoenigswald stresses particularly the recurrence of discrepancy between the conscious controversies of a period, and the changes taking place that prove truly significant. Scholars like Schleicher, for example, he finds, make crucial contributions, yet receive a bad scholarly press for what they said and thought they were doing. Before him, Sir William Jones cannot, in Hoenigswald's view, be credited with having meant the splendid things his famous statement (often quoted as the founding charter of linguistics) seems to say to us. Hoenigswald indeed comes down on the opposite side of the

fence from Diderichsen. Whereas Diderichsen locates the crucial change earlier than is commonly thought to be the case, Hoenigswald locates it later (in Schleicher). This difference of judgment may be taken to demonstrate three vital points: the continuity of the whole development was indeed far greater than textbooks suppose; the placing of formation of a paradigm in time is inescapably a matter of weighing of criteria: and the perspective of time changes the weighting one gives. One has to choose which new things are most important, as against the background of the many things to continue. It is fair to suggest that the twenty-first century is unlikely to agree in locating the major points of change in the twentieth century where we do today.

The whole section points to one great limitation of what we can do today in the history of linguistics. The subject is studied mostly by linguists, and we are equipped to recognize and trace ideas perhaps, but not usually equipped to probe social contexts and institutions. Yet, as Paul Thieme pointed out in discussions of Hoenigswald's paper at Burg Wartenstein, the indispensable foundation of what took place in the German area in the early nineteenth century was institutional. It had to do with the short tenure of an important ministry in the Prussian government by Wilhelm von Humboldt, famous to linguists for his ideas, but not his administration. By establishing chairs in Sanskrit and general linguistics in universities, a model emulated outside Prussia, von Humboldt may be said to have created the first successful paradigmatic *community* in linguistics; and he ought to be celebrated for this as much as for his ideas as to the nature of language and languages.

The Slow Growth of Grammatical Adequacy

Next to institutional perspective, the greatest lack at present in the history of linguistics probably is knowledge of the growth of the methods and practices on which linguistic work, and theory, have depended. Throughout its history, success of linguistics has come down to its ability to say accurate and worthwhile things about languages. Counted by centuries, far the greater part of this history has been concerned only with one or a few great languages of literature, religion, and cultural hegemony. It is with the modern period that we begin to get the 'democratization' of linguistics, as it were, in which gradually each and all of the languages of the peoples of the world come to have the right to contribute to knowledge of language. This principle reached something of a peak in the structuralism of American anthropologically oriented linguistics in the second quarter of this century, and in the interests of Trubetzkoy and Jakobson in Europe in typology and universals. Despite some apparent backsliding in recent years, it appears to be a permanently won gain; but it has been slowly and painfully won. We are far from being

able to give an adequate account of the subject, but the papers in this section demonstrate that what may now be taken for granted was only recently achieved at all.

Rowe (1974) has provided a basic step in assessing the growth of grammar in the early modern period. We need to know not only what was variously said about grammar, but also what kinds of grammars were written. Rowe comments on the implications of his findings, as to the slow and in part accidental way in which opportunities for broadening and deepening knowledge of language were taken up and publicly realized. Much depended in practice on what may have been the first institutionalized body of fieldworkers in ethnography and linguistics, the missionaries; but the public availability of their work had as much to do with expulsion of the Jesuits from the New World, Rowe suggests, as with anything else. The missionary grammatical work of the period is a fascinating scene of the interaction between received genre and novel information. Rowe's study of the publicly available, printed grammars is complemented nicely by Hanzeli's monograph (1969) on unprinted grammars of northeastern America; Hanzeli's discussion of the relation between concepts of 'grammar' and of 'economy' (the character of the language actually being described) is especially noteworthy.

In regard to this topic, Percival (1974b) has brought out yet another side of Wilhelm von Humboldt. In addition to the theoretician and the administrator, there was also the describer of languages, and Percival analyzes Humboldt's treatment of a problem in the language to which his most famous theoretical work (1836) was an introduction. One of our most widely experienced fieldworkers, Hamp (1974) has provided evidence bearing on the recurrent problem of preconception vs. data, with regard to phonology. He finds that an amateur, Prince Bonaparte, in some respects was more successful than professional linguists of his day, whose work conditioned their expectations of what they would find.

Alongside the development of the comparative-historical approach, the development of the structural approach is the problem of greatest general relevance to modern linguistics, and the structural approach began, effectively, with the development of phonology. Just as the comparative-historical approach has its mythical founder in Sir William Jones, so the structural approach has had its mythical founder in Ferdinand de Saussure. The great respect one must have for both men does not bar inquiry into the actual part they played; in both cases, it was, most dramatically, in the symbolic use made posthumously of each. The explanation of the symbolic significance of each is not fully available, but institutional and social factors clearly are important. And if with comparative-historical linguistics the causes had something to do with the institutional and cultural aspects of German scholarship, so with structuralism they seem to have had something to do

with a reaction against things German, after the First World War (Malkiel and Langdon 1969). And if the institutional success of both developments came after the symbolic intellectual breakthrough, so the true intellectual origins appear in both cases to precede it. The European ground has already been investigated with valuable results by Jakobson, Stankiewicz, Coseriu, Koerner and others. Here we are able to contribute two studies of the much more neglected development in the United States. The neglect perhaps is due to the tendency to retrospectively assign origination of all things structural to de Saussure. Whatever the case, it is clear that synchronic, penetrating accounts of novel languages were being written by Boas and Sapir well before de Saussure's lectures were given, let alone published. Wells (1974) development of recognition of structure in phonology, culminating, so far as he carries the story, in the work of Boas. Wells finds Boas' own insight into the principle of phonology incomplete, though path-breaking for its time. In retrospect we can see the step taken by Boas not only as a matter of structural insight, but also as the equally crucial matter of the *universalization* of the principle of structure. Well into the twentieth century, *a priori*, ethnocentric notions of the inadequacies of languages other than the relatively familiar and well-studied Indo-European languages were current. Well into the twentieth century, and in some respects still today, the equality of all varieties of language in terms of structure, has had to be argued. It was through their study of the individual characters of lowly-regarded languages that Boas, Sapir and others were able to demonstrate, as against received general linguistic notions, the truth of this principle. It is a kind of linguistic relativity, a reciprocal relativity of perception, that informs the remarkable article by Boas with which Wells ends, and in a sense, Stocking (1974a) begins. For Stocking finds in embryo in the article the principles that were to inform the long remainder of Boas' career as a shaper of the study of languages in American linguistics and anthropology. His study has the great merit of being based on research into unpublished materials, including correspondence—a hallmark of the maturity of a historical discipline, which the history of linguistics has only partially approached. And it has the great interest of dealing with a problem of authorship—collective or individual, and so of reminding us that even the seemingly most obvious facts are not to be taken at face value. In the course of his study Stocking sheds light on the institutional bases of descriptive linguistic work in the United States, reminding us not to take this for granted either.

All together, the individual studies in this volume are valuable contributions to the growth of the history of linguistics; and at the same time, they point up how far we have yet to go. There is a seeming paradox in what they collectively show. It is our own activity and period that we likely least adequately understand. This, of course, is the point of Hegel's remark that it is at dusk that the owl of Minerva takes her flight, that it is when a

period draws to a close (or is over) that it can be wisely understood. The paradox, of course, is only seeming, because the reasons that scholars cannot both pursue original research and stand back from it are obvious enough. The maxim of the history of linguistics, then, might be an adaptation of Marx' last thesis on Feuerbach. Linguists undoubtedly will continue to change the world (of linguistics); the problem is to understand it. And for understanding, history of linguistics is indispensable, even perhaps the main means.

Complementary Perspectives

It seems likely that we shall see a continuing growth in studies of the history of ideas in linguistics, especially of the major ideas, that have been central to whole periods. It is worthwhile, then, to urge studies of three complementary kinds: of the institutional bases of linguistics; of the diverse lines of work and traditions of work that enter into the field of linguistics at any time; and of the available means, methods, and in general, the practice, of a line of work, a scholar or a time. Focus on general ideas alone will never suffice to explain why and when events occur, and is likely to mislead us badly. It is by situating scholars and ideas that we can come to assess them adequately and to hope to give valid accounts of what has happened to create the history that linguistics has had. The papers of the last section of Hymes (1974b) are contributions in this regard. An eminent historian of science, J. Greene (1974), draws on his experience to suggest ways in which the history of linguistics can benefit from longer established areas of the general field to which it belongs. As a sociologist of knowledge, Wolff, and his associate, Barrie Thorne (1974), suggest some of the considerations that that discipline provides for gaining insight and explanation. Finally, Stocking (1974b) responds by discussing his own experience and views as trained historian, approaching a discipline as a novel subject matter. The history of linguistics always will depend on the contributions of linguists, for the assessment of technical matters and the recognition of significance in many ways, but the subject is unlikely to prosper without the complementary contributions of other standpoints. We may even hope that the day is not too far distant when the study of the history of linguistics, having become already more than an avocation, will be recognized as of interest to more than linguists, and as a vital part of the general field of the history and sociology of the human sciences.

NOTES

1. I want to thank Al Romano for help in preparing the manuscript of this introduction, and John Fought for discussions of the history of linguistics during the writing of it.

2. There is a story that can serve as a parable. A learned French Catholic is supposed to have said of the claims of Protestantism to be valid, because based on the original gospels, 'Do you expect me to take the word of four ignorant fishermen against all the glories and traditions of the Church?'

3. Not only is there persistence of work which the new 'paradigm' does not adequately incorporate, but there is also what has been called in the study of culture change, the 'loss of useful arts'. Where the study of structure has shaped linguistic training, and particularly since the new 'paradigm' of formal grammar; and where anthropologists have not maintained interest in practical linguistics and ethnography; the phonetic training once considered mandatory for both linguists and ethnographers has largely lapsed. Relatively few linguists, relatively few anthropologists, could today record oral data. Yet within linguistics new interest in rhetoric, poetics, and conversation call attention to oral aspects of speech that are not available in ordinary writing; and there are of course many languages not yet adequately described, for whose study transcription is necessary.

4. In doing so, one will find the elaboration of Kuhn's notion by Wallace, on which I have drawn in the preceding paragraph, to be a cogent, penetrating guide (see Wallace 1972). A longer and more general treatment of sociocultural order and change, but also cogent, is that by Cohen (1968). The major assumptions as to social groups that a historian of linguistics is likely to use find a place there. On assumptions and inferences generally, see Fischer (1970).

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