STUDIES IN FORMAL HISTORICAL LINGUISTICS

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For GSH

PREFACE

These separate but related essays owe their existence to a combined concern for the workings of text criticism and historical linguistics and for the history of scholarship in these fields. On earlier occasions I have suggested certain views on the development of the so-called comparative method. Few things are more rewarding than to bring implicit preconceptions of the past and present out into the open, as I aimed to do then and as I aim to do now. This time existing tradition is treated as a body – without, I hope, being seriously distorting – and one small portion of its working assumptions is examined.

My thanks go to the colleagues and students with whom I have had fruitful discussion, but especially to Zellig S. Harris, and to Henry Hiž who expended much more than just his excellent editorial care on these efforts. I only hope that I have learned as much from him as he has patiently tried to teach me.

Lloyd W. Daly has kindly read parts of an earlier version and has contributed valuable suggestions.

H. M. H.

PRELIMINARY NOTE

The relationship between historical linguistics and its humanistic sisters has been complex and subtle. What set historical linguistics apart almost from its modern beginnings was a formalistic bent which was by no means always gladly professed but which in fact became more and more pronounced as time went on. At least since Schleicher, primary work along historical and 'comparative' lines (primary as distinct from the more derivative efforts to codify it and to define its standing with regard to other disciplines) has been concentrated in areas that are particularly amenable to formal procedures. There were rewards: in the event, methodological clarity and consistency was seen to pay off in the form of coherent and sometimes testable results. There are other humanistic fields, to be sure, to which considerations of this sort are not entirely alien. Notably this is true of textual criticism, and it is interesting that the historian of philology and linguistics can detect some of the academic and biographical ties that went back and forth among the practitioners of those parallel disciplines. There are procedural analogies between the approach which made Karl Lachmann's name famous, on the one hand, and the later development of linguistic reconstruction, on the other. There are indications now that these analogies may go a good deal beyond the world of languages and manuscripts and may embrace other processes which occur in prehistoric, even geological and cosmic, time.

If the scholarly record contains those formal linguistic factors in some indirect fashion only, this is a challenge to make them explicit. Sometimes indirect mention consists in an impatient and disdainful hint that such and such an interpretation or inference is obvious and inescapable, not on some factual grounds, but on grounds of logic. It is implied that other choices would lead to internal contradiction, or be meaningless. Sometimes the hint is well-founded, especially when it is given in a concrete context in which the investigator has explicitly or intuitively tested the alternatives. Whether or not this is sufficient is a matter of preference or, perhaps, strategy. Procedures and principles in linguistics are best developed in close touch with nature. Some of the greatest linguists have, in fact, thought it more important to develop than to formulate them, and many great linguists have had better success developing than formulating. Yet, there are situations in dealing with natural data – polemical situations, in particular – in which a statement of principles saves waste and leads to the realization that certain factual prob-

lems were ill-formulated; or that some factual problems are related to other factual problems as special, or as more general, cases. If formalization needs a defense, this would be part of it.

Much of the uncertainty in historical linguistics centers on the concept of the change event. It is easier to understand the relation between whole linguistic systems – successive stages; related languages – than to isolate particular historical events the sum total of which produces the systemic difference. (It will be suggested in the following pages that this parallels a familiar difficulty in synchronic work: the segmentation of the flow of speech is still a major theoretical problem.) It might be best if the notion of ‘one change event’ could somehow be abandoned. But it so happens that formal statements, or rather hints at an existing formal necessity of a sort that would be decisive in favor of one over another interpretation of language history, are most often made about such allegedly discrete events and their relative position in time. The student of past forms of language frequently meets this problem in a special way. He sees two languages, decides that they may be stages of ‘one’ language, and begins to try to bring order into the differences between them. What gives him the right to isolate events and order them? One thing is certain: the identification of the several steps and their ordering are not independent of each other.

In particular, it would be important to understand by what reasoning scholars will reconstruct ‘intermediate’ stages when such stages are not given. Some of this is here attempted. It is done in a spirit of recognizing preconceptions and, perhaps, prejudices, and not of devising some sure road to historical truth. One soon learns that certain formal principles, appealed to indirectly perhaps, but with an implication of evident validity, are less cogent than they are claimed to be. And yet the practices and concrete interpretations which they are designed to buttress do stand up as reasonable and, on occasion, as verifiable. The first essay is devoted to this discussion.

The scope of the second and third essay is narrower. In both the subject matter is trees of descent; but a number of much-labored problems pertaining to such trees are not touched. One is the appropriateness of the very tree model to language history. It is well known that this appropriateness is limited and that there may not be any situations in history where ‘clear cleavage’ of the kind which makes the tree an adequate representation of what happened occurred in all purity. Then the question is only whether the model is still useful in those situations in which clear cleavage is approximated; pragmatic tests of this are rather encouraging. The other problem is that of evidence in glottochronology. The statistical standing of the glottochronological, and in particular lexicostatistical, parameters has recently been scrutinized again by Sankoff (1973). But on a less technical level there is,

after all, the not unreasonable belief in the existence of some factor (amenable to quantification) which is correlated to the remoteness, either by ancestry and descent or collaterally, among related languages. On these hypothetical but not exactly irrational grounds we ask, in the third essay, certain questions about the glottochronological properties to be expected in languages which form given trees of descent of the more familiar, classical type. Some important points of this type are, of course, treated in the work of Dyen *et al.* (e.g., 1967). In any event, the term 'glottochronology' is here used in a wide sense; it is not meant to cover the specific efforts that have been made to find a reliable parameter, such as basic vocabulary loss, to measure time depths.

To return briefly to the subject of the second essay: the comparison between linguistic and textual stemmata is a special challenge, since there are in existence excellent formalizations, as well as excellent discursive statements, of textual reconstruction. Particular tribute must be paid to the works, referred to below, by Greg (1927) and Maas (1960) – each reflecting in a most interesting way the different traditions of the two fields of scholarship in which those two men moved, English studies and the Greek and Latin classics, respectively. One mentions their names with trepidation, both because of their general stature and for one special reason. Both Greg and Maas, but Greg in particular, were outspoken on the formally degenerate nature of the very object to which we address ourselves here: the three-witness tree. They were right: three manuscripts, or three related languages, are not sufficient to allow a good many of the more important generalizations. For this reason as for others, the scope of the second chapter, like that of the third, remains limited.

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