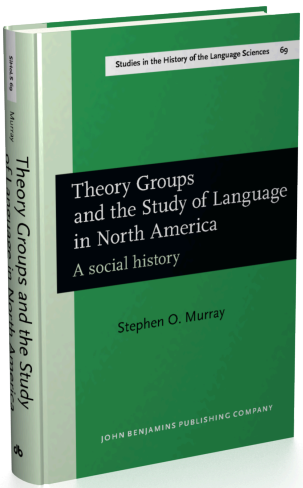
Studies in the History of the Language Sciences 69

Introduction

https://doi.org/10.1075/sihols.69.03int



Pages xvii–xx of
Theory Groups and the Study of Language in North America: A social history
Stephen O. Murray
[Studies in the History of the Language Sciences, 69]
1994. xx, 598 pp.

Introduction

This study of anthropological — and not-so anthropological — linguistics in North America tests a formalization of the theory proposed by Griffith and Mullins (1972) to explain the formation of scientific groups and to account for differences between what Kuhn termed 'scientific revolutions' and changes within 'normal science.'[1] The cases deal with leading theorists, the formation of groups around some of them, and the failure of groups to form around others. The cases provide a continuum from isolated scholars to successful groups dominating entire disciplines.

The first chapter explores the background to the claim that small, coherent, consciously-organized social groups are the agents of scientific change. After discussing some of the problems and proposed solutions in the comparative sociology of science, I formalize the Griffith-Mullins theory. Those uninterested in cross-disciplinary comparison may be tempted to skip the first chapter. Doing so may make the categories that are used recurrently in the historical case studies mysterious, so I would not recommend doing this.

Readers interested in the sources of data and in the choice of cases to study will want to read the methodological appendix after reading the first chapter. I know that I would do so if I encountered such a book — not just because I'm a skeptic, but because I'm very interested in acknowledgments, and that's where mine are, along with, according to the reviewer for *Language* of an earlier incarnation (Leeds-Hurwitz 1987c:671), other vital groundings of this book.

Chapters 2-17 deal with leading theorists about the place of language in society. The focus is on the formation of groups around some theorists and the failure of groups to form around others, not a year-by-year survey of what was

1. My sociological conception of 'revolution' was significantly shaped by hearing what became Mullins (1975) and Nisbet (1976, 1977) before I started doctoral studies.

going on in American theorizing about language in (or, eventually, entirely abstracted from) social interaction.

Sociologists, beginning with the committee before which I defended an earlier version as my Ph.D. dissertation (Murray 1979a), have wondered if I went native and became more interested in the history of linguistics than in sociological generalization. Fortunately for me, linguists and anthropologists have not found my work on the history of social science inordinately driven by alien theoretical concerns. To some extent I think that I did go native, but I've never understood how good sociology can come out of bad history. I have, therefore, striven to 'get it right.' Of course, this is a utopian aspiration, if so egotistical a goal can be utopian. I think that I have gotten various things righter than in other treatments of the historical subjects, including my own earlier publications.[2] Nevertheless, more research and better interpretations are always needed. Scholarship and science produce provisional results and past American theorizing about and research on language will and should continue to be re-examined. Different facets will interest different quests for a usable past.

In the case studies. I include considerably more detail than would be necessary to assess the explanatory power of the model. Although they are organized in the terms of the model, the case studies also seek to provide information crucial to differing interpretations and to provide historicist correctives of some of the more egregious distortions in writing about past linguistics.

The history of no science consists of a neat chronology of advances. Although the book advances chronologically, there is not a single narrative. Rather early in the book (the last half of chapter 2) chronological order is overwhelmed by the need to discuss simultaneous lines of work. The flow of

---

2. At the same time, I hope that I recognize those things that I knew as a graduate student in the mid-1970s that I don't know better (or at all) now, and that I have had the good sense to leave these alone! Published revisions of parts of my dissertation (Murray 1980a, 1981a, 1982, 1991a) have been incorporated along with material from 1983b, 1986, 1987a, and 1989 papers into this, the 'master narrative.' Work on some counter-hegemonic groups in American sociology (1980d, 1988b, 1991d) lurk in the background, but have not been included in the analysis in this book. Comparison of the histories of anthropology, economics, linguistics, and sociology that are more explicit in Murray (1983a) are a higher-level master narrative I have not attempted here (for that level, see Furner 1975 or D. Ross 1991).

chapters 2-8 is fairly obvious, even conventional, although Boas outlived Sapir and both of them significantly influenced 'Neo-Bloomfieldians.' The bulk of the book (chapters 9-14) is about simultaneous perspectives during the 1950s and 60s. In chapters 15-16 I return to follow three of the major streams (TGG, ethnography of speaking, and ethnoscience, discussed in chapters 9-11) through the 1970s and 80s. Chapter 17 is a sketch of the declining status of language study within anthropology during the 1980s.

Topical organization makes it inevitable that the reader must sometimes puzzle out what events were contemporary with other events detailed in other (sometimes distant!) sections. I hope that a detailed table of contents provides a useful map of the streams in which I paddle about for those not interested in exploring the whole delta of American research and theorizing about language. The already-mentioned appendix attempts to explain how I gathered and processed data, where and from whom.

In the final chapter, cases are compared and the initial theoretical model is simplified.