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978-0-521-11233-8 - Socio-Historical Linguistics: Its Status and Methodology

Suzanne Romaine

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*Socio-historical linguistics
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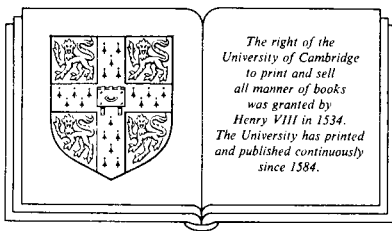
SOCIO-HISTORICAL LINGUISTICS

its status and methodology

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CAMBRIDGE UNIVERSITY PRESS

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Cambridge University Press
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CAMBRIDGE UNIVERSITY PRESS
Cambridge, New York, Melbourne, Madrid, Cape Town, Singapore, São Paulo, Delhi

Cambridge University Press
The Edinburgh Building, Cambridge CB2 8RU, UK

Published in the United States of America by Cambridge University Press, New York

www.cambridge.org
Information on this title: www.cambridge.org/9780521112338

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First published 1982
Reprinted 1985
This digitally printed version 2009

A catalogue record for this publication is available from the British Library

Library of Congress Catalogue Card Number: 81-12211

ISBN 978-0-521-23750-5 hardback
ISBN 978-0-521-11233-8 paperback

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FOR MY PARENTS
JOSEPH AND HELENE ROMAINE

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Preface

A few years ago I became interested in claims made about the epistemological status of sociolinguistic methodology and, in particular, the so-called empirical foundations of a sociolinguistic theory. My concern with the nature of sociolinguistic methods and data grew out of some of the difficulties I encountered in trying to present a sociolinguistic description of some variables in Scottish English (cf. Romaine 1975). Some of the problems (e.g. continuous vs. discrete variation, levels of abstraction in the construction of sociolinguistic grammars, probabilistic rules) still bother me. I deal with them again here, but this time with reference to another descriptive problem, namely, variation in the relative marker in Middle Scots. This also leads to consideration of some new issues, e.g. the scope of sociolinguistic theory and the relevance of sociolinguistic methods to problems in historical syntax.

It will become apparent that I am using the term 'sociolinguistic' primarily in a narrow sense, i.e. to refer to the work which has derived from Labov (1966). I have concentrated on Labov's research program because it has been so influential; supported by a substantial body of empirical research, it represents one of the most concrete proposals yet made for a sociolinguistic theory. However, I also discuss Bailey's work; and I attempt to show that much of the controversy between the so-called quantitative (Labovian) and dynamic (Bailey) paradigms results from a misunderstanding of the ontological status of some of the arguments and explanations which can be supported on the basis of sociolinguistic or variable data. I believe that both theories rest on shaky epistemological ground with respect to a number of claims, and most of what I have to say about both the major variationist theories is therefore critical.

I believe that a sociolinguistic theory of language provides a more comprehensive framework than an autonomous, e.g. 'asocial', one for dealing with dialect differentiation and language change, but that a sociolinguistic theory need not be completely (or even largely) empirical, or

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'be' linguistics, i.e. replace an autonomous or asocial linguistic theory, in order to be successful. I reject, for example, Labov's (1975a: 228) claim that we appear to be entering a 'dramatic and critical period in the development of linguistics as a natural science'. Even if a sociolinguistic theory were largely empirical (and I argue that in its present state it is not), it would not anyway give us the kinds of explanations for certain sociolinguistic phenomena that we want; furthermore, its scope would be very restricted. I have tried therefore to suggest some directions (or better, methodological guidelines) for a sociolinguistic theory which will be less empirical, but also, I believe, less narrow in scope than that proposed by Labov.

My own view is that a sociolinguistic theory which is truly integrative in its approach must transcend the traditional concept of grammar represented by both Bailey's and Labov's models; in this I agree with Hymes (1974b: 434). I also think that the increasing tendency towards emphasis on quantitative models and methods, and the development of computer-assisted analysis to the exclusion of all else is misguided; but any serious sociolinguistic work or critique of methodology must assess the contributions of such techniques to a sociolinguistic theory in terms of the claims made for its analytical tools. Therefore, I have analyzed my data by both variable rule analysis and implicational scaling as a means of dealing critically with theoretical issues in each model with a new set of data.

Synchronic sociolinguistics has been particularly convincing in its use of quantitative models to demonstrate how the 'present might be used to explain the past' (cf. Labov, Yaeger and Steiner 1972). There have, however, been few attempts to cross-fertilize historical linguistics with sociolinguistics in order to 'use the past to explain the present'. This book tries to develop a methodological and theoretical framework for a field of research I refer to as 'socio-historical linguistics'. The main goal of such a discipline would be to investigate and provide an account of the forms/uses in which variation may manifest itself in a given community over time, and of how particular functions, uses and kinds of variation develop within particular languages, speech communities, social groups, networks and individuals. There are a great many methodological and theoretical problems arising from the nexus of sociolinguistics and historical linguistics. Owing to the preliminary nature of this field of enquiry I have focussed my attention on one particular problem, namely, the development of the relative clause marker in a non-standard dialect of English, in order to show in some detail how one might treat it in terms of a socio-historical approach. The diachronic analysis of social dialects is, however, only one task for a socio-historical linguistic theory.

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This volume, therefore, tries to do a number of things simultaneously, namely, provide an account of variation in the Middle and modern Scots relative marker, lay the foundation for a socio-historical linguistic theory, and question the epistemological status of sociolinguistics. Not all the issues I have raised are resolved, but judging from the literature, some at least have not previously been recognized as issues and given the serious attention they merit.

I am very grateful to Roger Lass for his careful and critical reading of several versions of this book. His comments caused me to consider more deeply the implications of my research. I would also like to thank Bob Le Page, who very kindly read a first draft of the book; I have incorporated some of his suggestions and comments in this revised version. I am indebted to Nancy Dorian for commenting on an early version of the first chapter and, in general, for encouraging me to pursue this topic of enquiry at a time when no one else did. Since I completed a first draft of this book in 1978 I have had the chance to discuss a number of aspects of it with various people. I have particularly profited from my discussions with Elizabeth Traugott about socio-historical linguistics. There are no doubt some controversial points in this version; and I accept full responsibility for my own stubbornness, which compels me to retain them.

Birmingham 1980

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All decisive advances in the history of scientific thought can be described in terms of mental cross-fertilization between different disciplines. Some of these historic bisociations appear even in retrospect as surprising as the combination of cabbages and kings.

Arthur Koestler 1975: 230

Dass nun gar das Unterfangen mit der Natur zu laborieren, sie zu Phänomenen zu reizen, sie zu 'versuchen', indem man ihr Wirken durch Experimente blossstellte, – dass das alles ganz nahe mit Hexeri zu tun habe, ja schon in ihr Bereich falle und selbst ein Werk des 'Versuchers' sei, war die Überzeugung früherer Epochen: eine respektable Überzeugung, wenn man mich fragt.

Thomas Mann, *Doktor Faustus*

Charlatans have existed at all times and in the most tightly-knit professions.

Paul Feyerabend 1978: 219