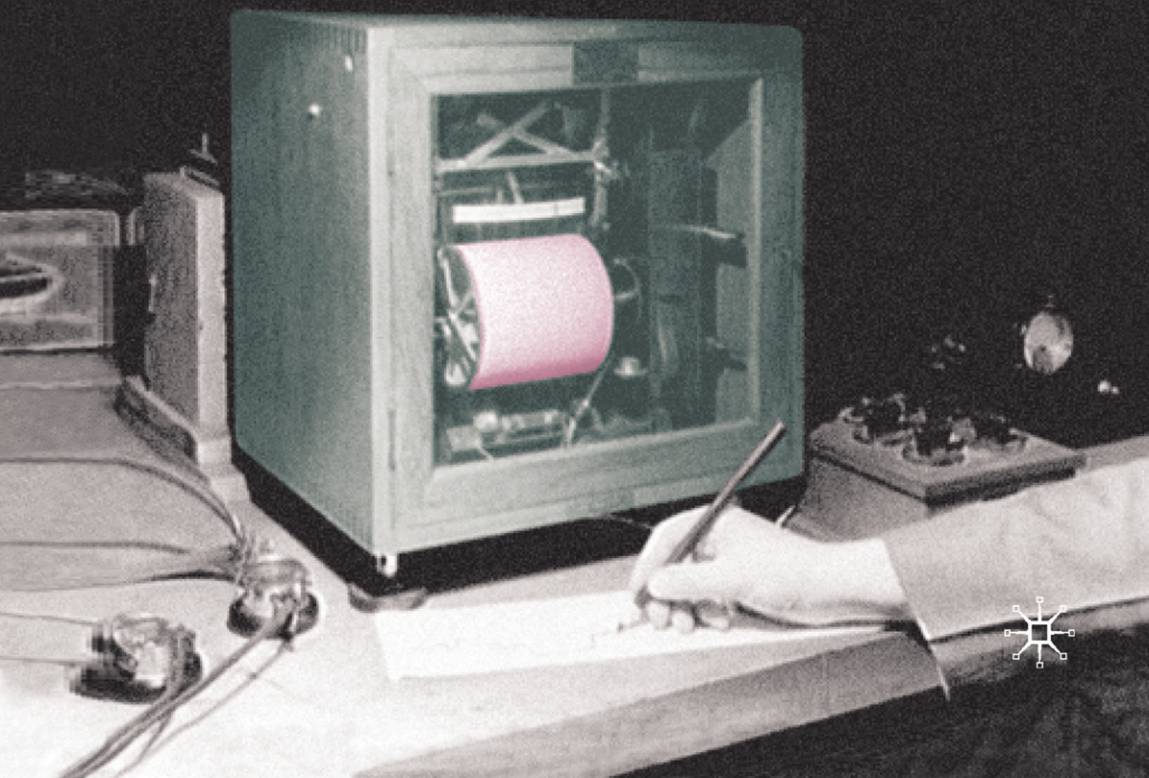
EDITED BY NAOMI QUINN

Culture, Mind, and Society

Finding Culture in Talk

A Collection of Methods



Culture, Mind, and Society

The Book Series of the Society for Psychological Anthropology

With its book series Culture, Mind, and Society and journal *Ethos*, the Society for Psychological Anthropology publishes innovative research in culture and psychology now emerging from the discipline of anthropology and related fields. As anthropologists seek to bridge gaps between ideation and emotion or agency and structure—and as psychologists, psychiatrists, and medical anthropologists search for ways to engage with cultural meaning and difference—this interdisciplinary terrain is more active than ever.

This book series from the Society for Psychological Anthropology establishes a forum for the publication of books of the highest quality that illuminate the workings of the human mind, in all of its psychological and biological complexity, within the social, cultural, and political contexts that shape thought, emotion, and experience.

Titles in the Series

Adrie Kusserow, *American Individualisms: Child Rearing and Social Class in Three Neighborhoods*
Naomi Quinn, editor, *Finding Culture in Talk: A Collection of Methods*

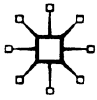
Finding Culture in Talk

A Collection of Methods

Edited by

Naomi Quinn

palgrave
macmillan



Softcover reprint of the hard cover 1st edition 2005 978-1-4039-6914-9

First published in 2005 by
PALGRAVE MACMILLAN™
175 Fifth Avenue, New York, N.Y. 10010 and
Houndmills, Basingstoke, Hampshire, England RG21 6XS
Companies and representatives throughout the world.

PALGRAVE MACMILLAN is the global academic imprint of the Palgrave Macmillan division of St. Martin's Press, LLC and of Palgrave Macmillan Ltd. Macmillan® is a registered trademark in the United States, United Kingdom and other countries. Palgrave is a registered trademark in the European Union and other countries.

ISBN 978-1-4039-6915-6 ISBN 978-1-137-05871-3 (eBook)
DOI 10.1007/978-1-137-05871-3

Library of Congress Cataloging-in-Publication Data

Finding culture in talk : a collection of methods / edited by Naomi Quinn.
p. cm.—(Culture, mind, and society)
Includes bibliographical references and index.
ISBN 978-1-4039-6915-6
1. Discourse analysis, Narrative—Research—Methodology. 2. Language and culture. 3. Methodology. I. Quinn, Naomi. II. Series.

P302.7.F533 2005
401'.41—dc22 2005049183

A catalogue record for this book is available from the British Library.

Design by Newgen Imaging Systems (P) Ltd., Chennai, India.

First edition: October 2005

10 9 8 7 6 5 4 3 2 1

Transferred to Digital Printing 2011

To all our students, those who took our classes and those who found their way to our doors, for contributing to our research, adapting our methods to their own projects, and helping us figure out how to find culture in talk.

Contents

List of Tables and Figures		viii
Notes on Contributors		ix
Chapter One	Introduction *Naomi Quinn*	1
Chapter Two	How to Reconstruct Schemas People Share, From What They Say *Naomi Quinn*	35
Chapter Three	Some Methods for Studying Cultural Cognitive Structures *Roy D'Andrade*	83
Chapter Four	Uncovering Cultural Models of Gender from Accounts of Folktales *Holly F. Mathews*	105
Chapter Five	Finding Culture in Narrative *Jane H. Hill*	157
Chapter Six	Analyzing Discourse for Cultural Complexity *Claudia Strauss*	203
Chapter Seven	"Good Enough" Methods for Life-Story Analysis *Wendy Luttrell*	243
Index		269

List of Tables and Figures

Tables

4.1 Rewrite Rules for a Story Grammar of *La Llorona* Morality Tales 116
4.2 Texts and Functional Analyses for Two Versions of the *La Llorona* Tales 119
4.3 Symbols and Descriptions for Functions in the *La Llorona* Tales 123
4.4 Specific Actions that Comprise Each Function by Gender of the Characters 124
4.5 Variant Types of Male Versions of the Tale as Analyzed by Sequences of Functions 127
4.6 Variant Types of Female Versions of the Tale as Analyzed by Sequences of Functions 130
4.7 Models of the Mediation of Opposition in Folktales (Adapted from E. K. Maranda and P. Maranda 1971:36, 89–90) 139

Figures

4.1 A Diagram for the Rewrite Rules for a Morality Story Grammar 116
4.2 Diagram of Rewrite Rules for Folktale M-30 118
4.3 Diagram of Rewrite Rules for Folktale F-28 118
4.4 Diagram of Models of Mediation from E. K. Maranda and P. Maranda (1971:89–90) 138
4.5 Diagrams of Models of Mediation for Initial Marital Conflicts in the Six Type Variants of the *La Llorona* Tales 140
4.6 Diagram of the Concatenation of Models of Mediation in the Type Variants of the *La Llorona* Tales 144

Notes on Contributors

Roy D'Andrade obtained his PhD at Harvard in 1962. He is Professor of Anthropology at University of Connecticut. His interests are in cognitive anthropology, general theory, quantitative methods, and American culture. Among his many publications are *The Development of Cognitive Anthropology* (Cambridge, 1995) and the volume coedited with Claudia Strauss, *Human Motives and Cultural Models* (Cambridge, 1992). He is a Fellow of the American Academy of Arts and Sciences (elected 1990) and a Member of the National Academy of Sciences (elected 1998).

Jane H. Hill earned her degree at UCLA in 1966. She is Regents Professor of Anthropology at University of Arizona. Her interests are in general linguistics, language and culture, sociolinguistics of Native American languages—most notable Mexicano—and language, race, and political economy. She is coeditor (with Judith Irvine) of *Responsibility and Evidence in Oral Discourse* (Cambridge, 1993), coauthor (with Kenneth C. Hill) of the book, *Speaking Mexicano: Dynamics of Syncretic Language in Central Mexico* (Arizona, 1986), and author of many other publications. She is past president of the American Anthropological Association, and currently editor-in-chief of the journal *Language in Society*, and a Fellow of the American Academy of Arts and Sciences (elected 1998).

Wendy Luttrell obtained her PhD from UC-Santa Cruz in 1984. She is Associate Professor at the Harvard Graduate School of Education. She conducts research in the United States, investigating class/gender/race relations and consciousness, especially in the context of American schooling. Her other interests are in the sociology of knowledge, sexuality, and feminist theory. Her most recent book is *Pregnant Bodies, Fertile Minds; Gender, Race, and the Schooling of Pregnant Teens* (Routledge, 2003). Her first book, *Schoolsmart and Motherwise: Working-class Women's Identity and Schooling* (Routledge, 1997)—the conduct of research for which she describes in this volume—won the 1998 Oliver Cromwell Cox Award from the American Sociological Association's Section on Racial and Ethnic Minorities.

Holly F. Mathews earned her PhD degree from Duke University in 1982. She is Professor of Anthropology at East Carolina University. Her research and theoretical interests include psychological anthropology, gender roles,

and medical anthropology; she conducts research in Latin America—particularly, in Oaxaca, Mexico, the site of the study she discusses in this volume—and in the southeastern United States. She is coeditor (with Carmella Moore) of *The Psychology of Cultural Experience* (Cambridge, 2001), and author of numerous articles in both medical and anthropological journals.

Naomi Quinn earned her PhD at Stanford University in 1971, and is currently Professor Emeritus of Cultural Anthropology at Duke University. Her areas of interest are psychological anthropology, culture theory more generally, the cultural analysis of discourse, and American and Ghanaian culture. Her study of American marriage is the subject of her chapter in this volume. She has coauthored, with Claudia Strauss, *A Cognitive Theory of Cultural Meaning* (Cambridge, 1997), and coedited, with Dorothy Holland, *Cultural Models in Language and Thought* (Cambridge, 1987). She is a past president of the *Society for Psychological Anthropology*.

Claudia Strauss obtained her PhD from Harvard University in 1988. She is Associate Professor of Anthropology at Pitzer College, and pursues interests in psychological anthropology, discourse analysis, culture theory, the anthropology of policy, the welfare state, and the United States. Some of her most recent work, taken up in her volume chapter, explores Americans' attitudes toward work and welfare. Among other publications, she has coauthored, with Naomi Quinn, *A Cognitive Theory of Cultural Meaning* (Cambridge, 1997), and coedited, with Roy D'Andrade, *Human Motives and Cultural Models* (Cambridge, 1992).

Chapter One

Introduction

Naomi Quinn

The idea for this book came with my dawning realization that, over a period of years, colleagues and I had invented a new family of methods adapted to the cultural analysis of interviews and other discourse, and that these methods remained under-explicated, underappreciated, and underpublicized as a body. My conviction that such a book was needed grew, over time, with the accumulation of requests, many of them from graduate students, to tell them how to perform my kind of analysis. The book is dedicated to these novice anthropological researchers, but it is also addressed to the many more seasoned anthropologists who have occasion to investigate cultural understandings through interviews and other kinds of discourse, and who are in search of more satisfying analytic methods for doing so.

Two incidents juxtaposed in a single week galvanized me, finally, into action. First, during a committee discussion of a student's proposal for a project I would be supervising in an interdisciplinary program, a biological anthropologist objected that the planned collection and analysis of interviews provided only, in his words, "anecdotal evidence." Later that week, in the discussion following a department talk by a cultural studies scholar, which I attended, an audience member asked the speaker how she thought we could ever study "subjectivity." The speaker threw up her hands. These two small and ordinary moments in the course of academic life, happening as they did side by side, made me realize that the methodological tradition of which I was a part was endangered. I saw that the present disciplinary and interdisciplinary climate, with its false dichotomy and unfortunate antagonism between scientific and humanistic approaches, was likely to obliterate the particular methodological contribution that my colleagues and I had made. On the one side stand the methodologically scientistic, who are ready to assume that it isn't systematic evidence if it hasn't been reduced to numbers. On the other side are the methodologically agnostic, who are ready to altogether dismiss systematic data collection as a handmaiden of bad Western imperialist science, and who scorn the very word "data." (For those who would object that I am invoking overdrawn caricatures, both these sentiments have actually been expressed to me in these very terms on one occasion or another.) All of the contributors to this volume are committed to systematic—not anecdotal or

impressionistic—data collection and analysis.[1] Thus, there is concern in these chapters for adequate representative sampling and adequate fully disclosed evidence. At the same time, systematicity does not always mean large samples or quantitative findings. Indeed, the cultural analysis of discourse often mitigates against both, since it is so time-consuming to collect, transcribe, and analyze very large samples of the rich discourse required, and since techniques for quantification may, for all their advantages, also have the disadvantage that they wring meaning, including cultural meaning, out of these data.[2] Thus, in these chapters, sample size tends to the modest side and analysis does not involve much counting.[3] The proviso about sample size is true for both number of speakers sampled and number of discourse units and features sampled. (It should be remembered, though, that a small number of speakers can produce a large corpus of discourse and, within that body, a large number of instances of the discourse feature under analysis—instances which, for many analytic purposes, can be treated as independent.)

I use this introduction, first, as an opportunity to delineate the common ground upon which all of the volume contributors stand, a stance that joins us in our commitment to systematic data collection of a certain kind. Then, in the second half of the introduction, I explore several of the more significant dimensions of difference across the contributors' analytic approaches. A final section will provide some guidance to the different strategies authors have used to present their methodological stories and the lessons to be derived therefrom.

A Shared Tradition and Its Methodological Implications

The work of all the volume contributors begins with the assumption that people in a given group share, to greater or lesser extent, understandings

[1] An even more fundamental point is made by Wendy Luttrell, in the opening paragraphs of her contribution to this volume. With Luttrell, all of the volume contributors believe that, in spite of the potential for distortion from researchers' biases, it is worth trying to do systematic field research. Rather than give up on this possibility, we are committed to documenting our data gathering and analysis and opening these processes for inspection and potential critique and correction. Indeed, such critique and correction is a normal part of science—of what D'Andrade (this volume) identifies as "contexts of verification." To slightly paraphrase Luttrell (fn. 1, this volume) and Faye Harrison, whom she quotes, I do not believe that most anthropologists are ready to abandon the systematic study of culture.

[2] See an article by Linda Garro (2000) for a useful comparison of how two kinds of analysis of the same material, one based on cultural models theory and adopting a methodological approach akin to those presented in this volume, and the other based on consensus theory and employing methods more amenable to quantification, can be used in collaboration.

[3] D'Andrade does use word frequencies to winnow out "gist propositions"; Quinn demonstrates that over 400 metaphors fall into just 8 categories; and Mathews tallies male and female versions of the *La Llorona* folktale.

of the world that have been learned and internalized in the course of their shared experience, and that individuals rely heavily on these shared understandings to comprehend and organize experience, including their own thoughts, feelings, motivations and actions, and the actions of other people. Theoretically, this view of culture as shared understandings based on shared experience spans two contemporary subfields of psychological anthropology—cognitive anthropology, in particular that school of cognitive anthropology known as *cultural models* or *cultural schema theory*, and psychoanalytic anthropology. Both subfields are represented in this volume. In accord with the mission of the Society for Psychological Anthropology book series in which the volume appears, these approaches from cognitive and psychoanalytic anthropology seek, and more often collaborate, to illuminate the workings of the human mind, in all its cognitive, emotional, and motivational complexity, and to trace the role of cultural meanings in these complex workings. Also in line with the mission of the book series, contributors in this volume are dedicated to empirical research of a certain kind.

Importantly for this brand of research, we recognize that these shared, or cultural, understandings are largely tacit, and referentially transparent to those who hold them. How can such cultural knowledge best be recovered or reconstructed? All the contributors represented in this volume have staked their efforts on the "cultural analysis" of discourse as the most effective single method for doing so. Our collective experience has been that doing such cultural analysis requires a reasonably extended sample of rich discourse. While a majority of us have resorted to our own version of the "interview" as the method of choice for gathering such rich talk, some have found it useful to exploit other discursive lodes, such as writings elicited for the purpose or spontaneously occurring genres of discourse such as folktales or other narratives. However it is collected, the cultural analysis of talk of all kinds always requires that it be tape-recorded and transcribed. I take up each of these aspects of our methodological approach, about cultural analysis, about interviewing, and about tape recording, in turn.

Cultural Analysis

The chapters in this book are all about the *cultural analysis* of discourse. Discourse, rather than other kinds of human activities or behavior, is the object of investigation for all of us because we deem it to be the best available window into cultural understandings and the way that these are negotiated by individuals. Culture in this sense of understandings encompasses the largely tacit, taken-for-granted, and hence invisible assumptions that people share with others of their group and carry around inside them, and draw upon in forming expectations, reasoning, telling stories, and performing a plethora of other ordinary everyday cognitive tasks. This internalized side of culture has been the object of study by cognitive anthropologists for

half a century now, and has become an integral strand of the contemporary psychological anthropology represented by the contributors to this volume. In recent years, cognitive anthropologists have developed a schema theoretic account of how these shared understandings are learned and organized in the mind. (See the opening pages of Quinn, in this volume, for a brief historical recounting of this theoretical development, and a definition of schemas and cultural schemas in particular. See Mathews (pp. 112–113, this volume) for another explicit application of schema theory in this collection. Finally, see Strauss and Quinn 1997 for one theoretical approach to cultural schemas.)

The other side of culture is the visible, but always partial and often cryptic, manifestations of these shared understandings that people produce. Of all such cultural products, the things people say offer, certainly not an unproblematic record of the cultural understandings that people have in mind when they say them, and certainly not the only record of these shared understandings, but simply the fullest and most decipherable record available. As Hill (p. 159) puts it in her chapter, rightly drawing our attention to culture's emergent as well as its constraining dimension in any human interaction, discourse "is the most important place where culture is both enacted and produced in the moment of interaction." Hence our attention to discourse. This is certainly not meant to exclude others' efforts to reconstruct culture through the analysis of other patterns of human action and its products.

Cultural analysis, then, refers here to the effort to tease out, from discourse, the cultural meanings that underlie it. These cultural meanings are implicit in what people say, but rarely explicitly stated. In schema theoretic terms, this lack of isomorphism between what people know and what they can state arises because cultural understandings reflect the experience from which they have been learned, and this experience often occurs in nonlinguistic contexts, unattached to language. Only under special circumstances (such as some kinds of formal teaching) does experience come to us codified in language or is it translated into language. So the tacit understandings that underlie discourse must be reconstructed from the clues that this discourse provides. As I say in my volume contribution, "I came to see my analytic approach as the reconstruction, from what people said explicitly, of the implicit assumptions they must have in mind to say it" (Quinn, p. 45). In a closely parallel way Hill (p. 157), in her chapter, says with respect to her topic, narrative, that anthropologists apply "methods that will permit us to identify these covert—yet publicly available—presuppositions, so that we can make the same kinds of inferences that speakers must make when they find meaning in narratives." This goal of reconstructing meaning, and especially cultural meaning, from the clues provided by discourse is articulated again and again in the chapters to follow.

The term *discourse* is used by the authors in this volume in the way linguists commonly use it, to mean language in use, either spoken or written,

and typically consisting of segments of speech or written text longer than single words or sentences (see Fairclough 1992:3 and also Hill, p. 159, this volume). We use *discourse* interchangeably with the less technical term *talk*, which we have chosen for our volume title. Given the popularity of another, narrower, definition of the term "discourse" in contemporary usage, it needs to be added that our usage here is not being limited here to that other sense: its Foucauldian sense of a way of talking and a set of associated practices, forms of subjectivity, and power relations that together constitute a body of knowledge, identified with members of some subgroup of society—for example, the medical establishment or defense intellectuals. Strauss (fn. 14, this volume) neatly distinguishes the ways of talking that are a component of Foucauldian "discourses" from other usages of "discourse" in this volume, by calling these "social discourses." Nothing, of course, prevents methods of discourse analysis, including methods presented in this book, from being used to analyze features of social discourses. Indeed, Fairclough (1992:37–61) recommends just such a move. In this volume, Strauss (pp. 224–227) suggests that inconsistency in an individual's ideas can arise from the contradictory social discourses of two different subgroups to which the speaker belongs. She gives the example of an African American minister who delivered an antiwelfare view in his preacher's voice, using born-again Christian discourse and, in a subsequent interview, expressed a pro-welfare view in a very different, Black Power, discourse. Arising from given subcultural groups as they do, social discourses are certainly one legitimate focus of cultural analysis, and Strauss's example suggests how our appreciation of social discourses and their deployment might benefit from close linguistic analysis such as hers.[4]

At the same time, the focus of the book on cultural analysis does distinguish it from the sizeable number and variety of other writings on discourse analysis. It should not be surprising that there are so very many approaches to discourse analysis, since discourse is an exceedingly rich source of data, from which many different kinds of information can be retrieved. Other existing approaches may be linguistic, psychological, political, sociological, sociolinguistic, or literary in orientation—that is, examining patterns of linguistic usage in and for themselves; searching for the cognitive and/or motivational bases of this usage; tracing the deployment of power and ideology coded in it; identifying differences in speech due to gender, class, or other social categories; documenting the patterns of conversational exchange that characterize ordinary speech or speech in specialized settings such as the therapeutic or the legal, or addressing discourse genres that have become established in given literary traditions (see for example, van Dijk 1985). All of these matters are deserving of study, but they do not constitute the primary subject of this book. The primary subject of this book is the cultural meanings that infuse people's talk.

[4] Fairclough (1992:37–61) offers his own approach to analyzing social discourses.

Some of the book's contributors, as will be elucidated in the second half of this introduction, are more concerned with reconstructing the way cultural meaning is itself organized—studying it for its own sake. Others are more intent on delineating just those cultural meanings that form a critical context for the interpretation of individuals' understandings of their lives, motives, and identities.

Of course, language, power, ideology, gender, class, interpersonal interaction, and other aspects of social life cannot be disentangled from culture, and thus none of these is entirely absent from the book. For some contributors, indeed, the ultimate point of analysis is to explore the relations among culture and one or more of these other dimensions of social life that language encodes. Nor are our methods for analyzing culture of a type apart from other methods that have been devised by specialists from other fields for studying other aspects of discourse. Quite the opposite: We are not here staking claim to a unique methodology; we are trying to bend existing methods to the study of culture, amending and revising these or, when they do not reach all the way to our goal of cultural analysis, inventing our own.

Indeed, the heavy influence of methods of discourse analysis drawn from other fields is everywhere evident in this collection: in Roy D'Andrade's use of the linguistic philosophers as a starting point in his reconstruction of a folk model of mind (detailed more fully in D'Andrade 1987); in the vexed influence on my own work of that by cognitive linguist George Lakoff; in the inspiration both Wendy Luttrell and I have drawn from sociolinguist Charlotte Linde's delineation of the "life story"; in Luttrell's adaptation of further methods from the symbolic interactionists and the feminist relational psychoanalysts; in Claudia Strauss's application of methods developed by critical discourse analysts, and other linguists, along with the insights of Bakhtin, Bourdieu, and other theorists; in Jane Hill's presentation of methods garnered from linguistic anthropologists and sociolinguists, most especially Linde's teacher William Labov; and in Holly Mathews's adaptation of methods devised by folklorists, notably Vladimir Propp, by cognitive psychologists, and by Lévi-Straussians for the analysis of folktales. If there is considerably more dispersion than overlap in the sources from which we have drawn for methodological applications and adaptations, this very variety is a hallmark of our approach. Beyond our diverse theoretical and topical interests (which I turn to in the second half of this introduction), our methodological opportunism unites us. Any method from any quarter of discourse analysis will earn our consideration, if it can be recruited to our special purpose of reconstructing the cultural meanings that inform and organize talk and other discourse. Of course, the application we make of a given method is likely to be entirely new. This opportunistic orientation toward method, and the resulting eclecticism of the methods we are willing to employ, is one of the central messages we hope that readers will take away from this volume. That is, we want readers to treat the contributions to this volume, not as a methodological canon, but as the basis for a methodological repertoire to

be expanded and selectively drawn upon according to the demands of their own cultural analyses.

The Interview

As noted, a majority of contributors to this volume rely for their analyses on discourse collected in interviews that they themselves, or others working with them, have conducted. It needs to be said immediately that, in ways that will emerge, the interviews that contributors favor are far from the traditional kind of social science interviewing in which the researcher asks a set of predetermined questions, establishing a mechanical, "lets-get-this-over-with-as-quickly-as-possible atmosphere" (Strauss, p. 239, this volume). At its most typical, that kind of interviewing is better thought of as face-to-face survey research. (Unable to find a good distinguishing label for our very different kind of "interview," I find myself resorting to surrounding it with scare quotes.)

Charles Briggs (1986) has provided a trenchant and important critique of interviewing as it is typically conducted across the social sciences. We would argue that our method of interviewing circumvents two of Briggs's chief concerns. First, he interrogates the common expectation that the interviewer controls the interview, while the interviewee's role is confined to answering questions. Says Briggs (1986:26), "Indeed, as many writers have argued, interviews are not supposed to be conversations." With Linde, and contrary to these many writers, the contributors to this volume view the interview, as we conduct it, as a special form of conversation. As will emerge, what is perhaps most special about this form of conversation is its one-sidedness—that is, the degree to which its control is granted to the interviewee. Secondly, Briggs makes the case that traditional interviewers, intent on amassing as much information on a given topic as possible, foreground the referential content of surface forms to the neglect of the web of meaning on which the interviewee constantly draws (1986:22), and privilege conscious models and explicit presuppositions over that which is outside the limits of the interviewee's awareness (1986:117). As will become evident, our methods of discourse analysis, quite opposite to this characterization, are designed to mine implicit meaning. Indeed, I hope that our methods of conducting and analyzing interviews can serve as a model for addressing and surmounting these two serious drawbacks of traditional interviewing, to which Briggs has rightly called attention.

Nevertheless, we do interview (and Briggs does not advocate the abandonment of interviewing). There is a good reason why we depend so heavily on the interview for the discourse we subject to cultural analysis, and it is directly tied to our common focus, delineated in the last section, on the cultural understandings underlying discourse. As I discuss at the beginning of my volume chapter, interviews can provide a density of clues to cultural understandings that is virtually unobtainable in any other way. This is largely because interviews frame the interviewee's task as one of communicating what he or she knows to the interviewer.

Performance of this task produces a relatively dense frequency of what Schiffrin (1987:17) calls *expository discourse* that would seem to correspond to what Hill in this volume (p. 160) terms *argumentative discourse*, and includes what Linde (1993:90–94) calls *explanation*. As Linde (1993:90) defines it, the explanation is a unit of discourse that "begins with a statement of some proposition to be proved, and then follows it with a sequence of stated reasons (often multiply embedded reasons) why the proposition should be believed." In its fullest form, such a sequence may end with a coda repeating and reinforcing the initial proposition. Often, though, an expository discourse is much more attenuated than a full-blown explanation, dropping not only the final, finishing coda, but also most or even all of the reasons—when the speaker assumes that the listener already knows and agrees upon these reasons, for example, or when the speaker is intent on making a larger point to which the immediate assertion is subsidiary. In this volume, expository discourse ranges from interviewees' explanations about marriage, in my analysis of them, to the statements of opinion—common, debatable, or highly controversial—such as are frequently offered by Strauss's (pp. 232–238, this volume) interviewees on political matters. Explanation of this kind is ideal material for the reconstruction of cultural understandings.

This is not to say that such discourse automatically makes implicit cultural understandings explicit; rather, the expository discourse in interviews provides relatively rich and frequent clues to these tacit understandings. From another perspective, that of the interviewer, D'Andrade (p. 90, this volume) observes that, in general, "it is better not to ask informants directly about their models, but rather to ask something that will bring the model into play; that is, something that will make the person *use* the model." Interviewing as we conduct it is well designed to "bring the model into play." By contrast, in other more ordinary kinds of conversation, participants begin with the assumption of shared cultural understandings. Rather than being "brought into play," these understandings tend to be indexed in passing in the course of some other immediate task, such as mutual problem solving, or gossiping, or reminiscing. Such indexing can result in highly condensed references, often cryptic to outsiders. While such unelaborated referencing is likely to be heightened in talk among familiars on topics that they routinely discuss, it figures more or less prominently in all ordinary conversation between or among those who share a common culture. The interview situation minimizes—though by no means eliminates—the indexing of shared cultural assumptions.

At their best, too, interviews of the sort we conduct encourage those who are interviewed to be forthcoming. As Hill (p. 183, this volume) observes, the interview is unique among discourse genres in that "the interviewer may grant to the subject narrative privileges that he or she would not normally enjoy"—and, I'm sure Hill would agree, discursive privileges more generally. Several other contributors (Strauss, Quinn, and Luttrell) build on Hill's point, noting how crucial it is to cultivate this discursive

privilege, by not just ceding but actively granting control to the interviewee—encouraging the interviewee to organize the interview (or series of interviews) his or her own way and to pursue his or her own thoughts, conveying the interviewer's openness to the interviewee's own perspective, unique insight and special knowledge, and being an extraordinarily good listener and a nonjudgmental one. It should be clear that our approach to interviewing differs radically from the traditional method of question-asking that Briggs critiques.

Another of Briggs's (1986) major concerns about interviewing, its insensitivity to social context, is, however, not overcome by our method. Because the interview is, by definition, a conversation between two people (one of whom, the interviewer, does not normally belong to the immediate social world of the other), and because the logistics of interviewing and taping make it highly impractical to conduct this conversation when and where it might overlap or interweave with ongoing talk that is part of that social world, interviews cannot capture the complexities of social relationships that are often revealed in spontaneously occurring group conversations. As Hill notes, the granting of narrative privilege makes the interview an insensitive instrument for purposes such as observing adjustments of norms of reportability that occur in spontaneous discourse, and that may signal shifting social relationships or differing cultural settings. This may simply have to be accepted as a limitation of interviewing. It is not a good method for the investigation of social relationships in process (other than the somewhat strange one between interviewee and interviewer themselves, of course).

There are important counter-advantages to the granting of narrative privilege to an interviewee, though. "[B]y the same token," Hill (p. 183) goes on to say, "interviews may elicit very striking and interesting narratives from people whose voices would not be heard if collection methods were restricted to 'socially occurring' discourse."[5] And, a psychological anthropologist would want to add that interviews may elicit discourse and reveal the cultural understandings underlying that discourse that would not otherwise be voiced by any people under any other circumstances, in any type of discourse. That is to say, some cultural knowledge that we as researchers want to retrieve would remain forever untold if not for the interview as a stimulus to its revelation. Some of my American interviewees mentioned to me that they had never thought about or talked with their spouses about their marriages in the same depth or come to the same realizations about their marriages before being interviewed about them.

Interviews as we conduct them also produce longer stretches of uninterrupted discourse, including both exposition and narrative, by one person

[5] I prefer "spontaneous discourse" or "spontaneously occurring discourse" to Hill's "socially occurring discourse," in order to avoid any confusion with Strauss's term, "social discourses," which she uses for an entirely different purpose (see the end of the last section). Further, "socially occurring" might suggest that interviews themselves were not, as Linde argues they are, social events in their own right.

than are likely to be sustained naturally. In ordinary conversation, participants typically take shorter conversational turns; indeed, in much social interaction, such turn-taking is only polite. Also, the discourse produced in interviews is directed to the interview topic and stays on topic more predictably than in spontaneously occurring talk. Indeed, interviewers can encourage continuous discourse on the topic of their interest by redirecting an interviewee's attention to the topic at hand, and by subtler conversational maneuvers designed to keep the interviewee on track. Topic-switching in ordinary conversation between or among other parties, by contrast, is out of the researcher's control.

Interviewing also permits the researcher to arrange with a given interviewee to return to the same topic on later occasions. Such long, multiple interviews with each of her interviewees permit Strauss (pp. 208–221, this volume) to trace out personal semantic networks and identify inconsistencies of belief that show up only across a great deal of a given interviewee's discourse. More generally speaking, the clues that allow us to reconstruct culture are often widely dispersed and would be difficult or impossible for the researcher to trace across spontaneous conversation.

"Ideally," comments D'Andrade (p. 89, this volume), "one would like to encounter multiple natural situations in which people discuss with each other the relevant topic." Unfortunately, the relevant topic may come up only rarely in natural contexts (see my discussion of the problem of collecting talk about marriage, p. 40 of my volume chapter). A final reason for relying on interviews, then, is the pragmatic difficulty of assembling a sufficient corpus of spontaneously occurring discourse on a given topic. D'Andrade considers an alternative to the interview—arranging discussions between people, somewhat like focus groups—that would circumvent this last difficulty while arguably coming closer to the ideal of collecting spontaneously occurring talk on a topic. But it should now be clear that group interviews can have other drawbacks (most of which are addressed by Agar and McDonald 1995) for the cultural analysis of discourse. Most importantly, like the conversation in natural settings that they mimic, they encourage non-expository, highly condensed talk and short conversational turns. As already observed about spontaneous conversation, cultural assumptions in such talk are likely to be referenced only in passing, glossed over or highly abbreviated, subordinate as they are to the main conversational task—in the case of arranged group discussions, the task of debating and reaching clarity, if not agreement, on the topic under discussion. There are other limitations to such arranged discussions as sources of discourse for analysis. Like spontaneously occurring conversations, they may permit some participants to dominate the discussion, and some things to be discussed, while discouraging other participants from saying what they have to say and other things from being spoken. In addition, focus-group-style discussions may curtail the researcher's ability to keep discussion on the topic. And they may be difficult to transcribe. In the end, D'Andrade concludes, "easiest to arrange is the standard

one-on-one interview." Of course, nothing prevents one-on-one interviews from being supplemented, or perhaps preceded, by group interviews, which, like group conversation of all kinds, do turn up information and insights that might not otherwise emerge. And, in spite of all the logistical difficulties they pose, if spontaneous conversations on the research topic can be depended upon to occur in groups—say, at regular group meetings organized around a particular group interest—they should hardly be rejected as a potential source of rich discourse for cultural analysis (see for example, Cain 1991, Mathews 2000).

Having mounted this forceful case for interviews, I must immediately qualify it. Nothing rules out the cultural analysis of spontaneously occurring discourse, if only that discourse is available in sufficient quantity and is sufficiently extended and rich enough.[6] Some kinds of exposition-rich discourse, such as public debate, dispute settlement, or therapeutic talk in our society, may occur spontaneously in predictable, accessible settings. There is no reason, too, why interview discourse cannot be supplemented by spontaneously occurring discourse overheard or picked up on tape by the researcher, and that addresses an analytic point—as illustrated by Strauss's inclusion in her analysis (p. 204, this volume), of short examples from a Thanksgiving dinner conversation she taped and a published text. Nor is the distinction hard and fast: Somewhere between interviewing and collecting spontaneously produced discourse, lies the strategy of eliciting, for the researcher's purposes, some otherwise "entextualized" discourse—that is, discourse that "has become relatively fixed, shareable, and transmittable" (Hill, this volume, fn. 5) in a given speech community. Certain narratives provide a good example. When narratives are told in designated settings or in the course of ordinary conversation, it is feasible to ask them to be retold to the researcher—even if the setting for this retelling and the occasion for it are out of the culturally usual. Such narratives, like interviews but unlike ordinary conversation among multiple participants, can occasion relatively sustained and uninterrupted talk by the narrator, providing lengthy stretches of discourse for analysis. Mathews adopts this strategy, describing in her volume chapter how she collected and analyzed multiple versions of a folktale that is commonly retold in everyday contexts, most often as a cautionary tale to engaged couples, newlyweds, or those experiencing marital problems. She was interested in alternative male and female versions of a certain Oaxacan folktale that she had come across. She collected some tellings of it that she was lucky enough to be present for; but the tale was simply told too infrequently to make it practical for her to collect a sample of its tellings large enough for her analysis. Mathews was able to capitalize on the entextualization of this folktale to elicit recreations of it told especially for her.

[6] Alexei Yurchak (1997a,b), for one good example, has collected and analyzed Soviet political discourse, including written (textual) materials and spontaneously occurring talk, augmented by his observations of other, nonlinguistic practices.

Interestingly, the interview genre itself can come to have an "entextualized" quality to it, posing difficulty for any hard-and-fast distinction between spontaneous and elicited discourse. Interviewees of mine would occasionally tell me, when I showed up for our weekly interview, that they had "saved" some incident or insight to tell me about. In all these cases, the regular interview had taken on a life of its own, as a highly specialized kind of entextualized discourse that occurred in a special setting, within the intimacy of the dyadic relationship that the interviewer had developed with the interviewee.

Charlotte Linde has made a more fundamental proposal, that interviews themselves are a naturally occurring genre of talk. As Linde (1993:59–60) comments, "[I]t is a mistake to try to make a sharp distinction between the interview situation and so-called real life, or between the interview situation and non-contrived social interaction." She (1993:60) emphasizes that the interview itself "is part of real life too," that (1993:59) it is "an existing social form used as a technique to achieve all kinds of social purposes"—for example, doctor–patient, lawyer–client, hairdresser–client, and decorator–homeowner interviews. More generally, interview data are valid, she believes (1993:61), because what people do in interviews—represent themselves and tell their life stories—is something that they do spontaneously in a wide variety of other contexts. That this task of self-presentation makes sense to interviewees—in the context of an interview just as in other, more ordinary contexts—is responsible for their willingness to take charge of the interviewing process and to talk. Further, it accounts for what Luttrell (1997:8 and p. 247, this volume) describes as the "narrative urgency" with which people like the women she interviewed tell their stories and thereby "define and defend their selves and identities."

Of course, Linde (and Luttrell) interviewed English-speaking Americans. The experiences of anthropologists working in other places suggest that, if the interview cannot be treated as "an existing social form" everywhere in the world, perhaps people everywhere at least practice some form of interlocution that is close enough to the interview so that either this alternative discourse genre can be adapted to the anthropologist's purpose, or the speaker can make the conversion into an interviewee. The presence of interview-like genres in other places should not surprise us, any more than Hill's conclusion that there is a universal human narrative competence surprises us. Language evolved, after all, for the purpose of communication, and something akin to an "interview" in format appears to be one of the modes of linguistic communication, along with narrative and conversation, granted to us within the constraints of human cognition and social life.

Yet, it would not do to be too sanguine. It is telling that most of the interviews subjected to analysis in this volume were collected from middle-class English-speaking Americans. Caution, at the very least, is merited before assuming the transferability of this method of data collection, intact, across cultural setting—and across class and educational disparity.

How to locate and adapt some other, local, discourse genre that can serve the same purpose may be far from obvious. A striking case is that of Debra Skinner's experience attempting to interview Nepalese women about their lives, summarized in Holland and Lave (2001:11): "Expecting a narrative, a life story, when she asked them to tell her about their lives, she was surprised when they sang songs for her instead, especially ones that had been collaboratively produced by groups of local women for the Tij festival." One kind of song, *dukha* (hardship, suffering) songs, told by women for an audience of women, vividly depict moving scenes of inequality in the domestic lives of women. Prose stories of actual life events are also told by these rural people from central Nepal; *dukha*, while they may be based on individual women's life stories, are clearly more generic than the life stories of particular individuals (Holland and Skinner 2001).[7] But the songs are just as clearly a significant cultural medium for enunciating women's perspective on, and evaluation of, their lives—a rich resource for the reconstruction of cultural understandings of gendered domestic relationships, and an invaluable one for the interpretation of women's lives themselves. In a comparable way, Mathews (personal communication) reports that she could not get Oaxacans to talk about marital relationships until she happened on the device of presenting them with renditions of the *La Llorona* tale, which unexpectedly opened the floodgates to their discourse on marriage. Researchers working in places where interviewing is an unfamiliar genre must, as Briggs (1986:59) advises, do early and extensive ethnography of speaking to identify local genres such as *dukha* songs and *La Llorona* tales that can provide entrée into topics or deepen and expand knowledge of them.

Briggs's critique goes further, though. He worries not only that researchers may be inattentive to local discourse genres, but also that we may be insensitive to the local, culturally valued, meta-communicative norms embedded in these genres. And that interviewing may impose our own meta-communicative norms, ones that violate local ones (1986, especially pp. 90–92; Briggs calls this "communicative hegemony"). We claim to have adapted interviewing to one set of local meta-communicative norms, those for conversation among Americans. Can interviewing as we do it be combined with attention to other local genres, and re-adapted to other local norms of communication, in such a way as to lay Briggs's concern to rest? Admittedly, we have not yet demonstrated this. This question will have to be addressed as those who pursue the cultural analysis of discourse attempt to extend it beyond the American middle class to other,

[7] These hardships and suffering include preferential treatment of brothers, displacement by co-wives, abuse by husbands, and the like. The actual stories that find their way into such songs are especially likely to be those told by recently married young women who have returned to their natal villages, sometimes for the first time since marrying out, to attend the annual Tij festival and join in the composition of songs being prepared for the festival (Holland and Skinner 2001).

comparative, research settings so important to the cultural anthropological enterprise.

From a different angle altogether, defense of the interview and other "contrived" opportunities to collect discourse is necessary because, in our reliance on it, those of us in the cultural discourse analysis tradition differ sharply from many linguistic anthropologists. Hill is the one volume author trained as a linguist and, as she (p. 161) points out, many of her colleagues strongly prefer to work on narratives, and by extension, other types of discourse, that occur spontaneously "in everyday contexts of family, work, play, religious observation, courtroom procedure, medical treatment, scientific conferences, and the like, that are not elicited or organized by the anthropologist."[8] But this preference ought not to harden into a prejudice against the interviews favored by cultural discourse analysts.

To see that analyses of both kinds of discourse have their place, what must first be appreciated is that linguistic and psychological anthropologists who analyze discourse have different research foci and goals. Linguistic anthropologists, understandably, have their eye on language—a cultural domain in its own right, of course, if a highly specialized one—and have as their goal a better understanding of how language works. This is an enterprise well served by analysis of spontaneous discourse. A good recent example of this focus exclusively on language, and based on spontaneous discourse, is the fine-grained, masterful study by Eleanor Ochs and Lisa Capps (2001) of everyday storytelling or conversational narrative, as this goes on around the dinner table and in other family settings. While the authors (Ochs and Capps 2001:7) subtitle their book *creating lives in everyday storytelling*, and observe early on that "conversation is the most likely medium for airing unresolved life events," it is neither the creation of lives nor the life events aired, as these may be culturally patterned and hence revealing of cultural presuppositions, that are the focus of this study. Instead, and, again, understandably, the focus is on patterns in the structure of such narratives themselves, and in the process of their telling: what about them makes them tellable, for instance, or how they get launched, or how their temporal sequencing is managed, or how children, over the

[8] Hill's contribution to this volume illustrates a more relaxed approach to using discourse from various sources. One of the three narratives that she analyzes in her chapter ("An immense fall") did occur spontaneously in a natural setting—a meeting that was being taped by the researcher. The second ("A dollar and a quarter an hour"), while it arose "spontaneously," did so in the course of a longer interview arranged by the researcher. The third narrative ("The Rattlesnake Story") was told under circumstances that were even more contrived: Hill and her colleagues were interviewing its narrator as part of a study of Tohono O'odham dialect variation. The study involved taking the subject through a notebook with pictures of items known to exhibit dialect variation. Asked in her native language, of each new item, "What do you call this?" the speaker chose not just to identify the item but to follow this with a story related to it. This is one of those stories. As Hill (personal communication) says, these narratives were "a quite unexpected bonus of the dialectology project." That the subject of this experiment found a way to reshape it to her own linguistic style attests further to the fuzziness of a distinction between spontaneous and elicited discourse.

course of development, are incorporated into their telling, or how these narratives embody moral assessments, or how their plots are structured. Analysis of these moral messages themselves is not the point. Emplotment is viewed "as a collaborative, sense-making practice that attempts to reconcile sociocultural and personal realities" (2001:207), but these realities are not explored. This is not by any means to dismiss the value of Ochs and Capps's work. Indeed, psychological anthropologists may envy how far linguistic anthropologists have gotten in understanding the organization of their chosen domain of study, discourse itself. It is merely to point to a sharp division of disciplinary labor, observed by both linguistic and psychological anthropologists, that has obtained between their work and our own. Nor is this observation as to division of labor a merely self-indulgent exercise in disciplinary boundary maintenance for it's own sake, as one reviewer of this book seemed to think. The point is, rather, to specify and underscore the methodological gap that this volume begins to fill—a methodological need that the many books using discourse analysis and having superficially similar-sounding agendas, like Ochs and Capps's book, may seem to answer but do not. They are doing something entirely different.

This same focus on language is evident even in Hill's chapter for this volume, which is organized around a single discourse type—again conversational narrative, defined as "oral narratives that emerged as interactional moves within larger conversations" (Hill p. 158)—and asks how this kind of narrative can be made to reveal cultural presuppositions, including presuppositions about the narrative itself. Her presentation, too, ranges opportunistically across the assortment of substantive topics that happen to arise in the narratives she analyzes, from being bitten by a snake to a failure of rock-climbing equipment to attitudes about immigrant workers. (In the same way, any and all narratives that family members tell, on whatever topic, are grist for Ochs and Capps's mill.) Hill's is the only presentation in this volume that is so organized.

This, of course, is no criticism of Hill's contribution, which is so valuable precisely because it does the much-needed work of extracting, from the very large body of literature in sociolinguistics that addresses narrative, those theoretical and methodological approaches to this important discourse type that are most likely to be useful for cultural analysis of discourse. (Mathews's chapter does somewhat the same job, it should be noted, for linguistic approaches to folktales.) Instead, this is an observation about a telling subdisciplinary difference. The other contributors to the volume, as is typical of psychological and other cultural anthropologists, are primarily concerned, not with some particular linguistic feature, discourse type, or speech genre, but with some underlying cultural topic, theme, or schema. These contributors investigate cultural models of marriage (Quinn) or of society (D'Andrade), or beliefs about welfare (Strauss), or about gender difference and relationships (Mathews, this volume), or the divided identity conferred by schooling (Luttrell—a sociologist who

might best be characterized as a "cultural sociologist"). Because their eye is on that underlying topic of interest, psychological anthropologists will organize their presentation around that topic and are likely to be eclectic—as are most of the contributors to this volume—as to the discourse types and genres they submit to analysis in the pursuit of insight into the cultural question before them. (Conversely, Hill's eye is on a discourse type, and she is eclectic as to discourse topic.)

It is true that Mathews's analysis sticks to a single genre, folktales, and is highly attentive to the linguistic features that typify this genre. But, as she explains (p. 110, this volume), she chooses to analyze variations on a particular folktale only because that folktale happens to be a rich source of insight into the gender relationships with which she is concerned. Moreover, it needs to be said, her analysis of folktales is extensively informed by a separate set of interviews that she conducted with men and women of the community about gender and marriage. This interview material helped Mathews decipher the meanings underlying the folktales; the tales themselves, not unlike group conversations, index cultural assumptions that need not be remarked or expanded upon, because of the expectation that the audience shares them with the tale's narrator. So, while Mathews's approach might seem to resemble Hill's in its attention to a particular discourse genre, her interest in a given genre is really incidental to her pursuit of a particular topic of discourse. Comparison of these two cases thus further clarifies the distinction that I am drawing between the way linguistic anthropologists approach the analysis of discourse and the way psychological anthropologists do.

Even within interviews and other relatively rich sources of discourse for cultural analysis, the topics of interest to psychological anthropologists are likely to arise much less frequently than the features of language that are the focus of sociolinguists' and linguistic anthropologists' study. A good illustration of this difference comes, again, from the volume chapters. From the point of view of linguistic anthropologist Hill (p. 160), for whom narratives on any subject are grist for the analytical mill, narrative is "extremely common," especially brief narrative embedded in other types of discourse. But from my vantage point (Quinn, pp. 43–44), narratives on the particular topic of my research—marriage—did not occur commonly enough to be useful for analysis; so I rejected them as an object of analysis in favor of the much more frequently occurring metaphors interviewees used for marriage and the reasoning embedded in their explanations of it.

For those of us who do cultural analysis of discourse, what matters is dishing up a big scoop of language—one we can be relatively certain will contain plentiful, rich clues to the topic under study. And it is because we are intent on reconstructing the presuppositions underlying language, and not with language per se, that we are less concerned than are linguistic anthropologists with whether the discourse on which we draw for our analysis has been produced spontaneously in natural settings. The discourse we analyze must conform to a less stringent standard of "naturalness"

than that analyzed by linguists; but that doesn't mean that there is no standard. This discourse must be collected in such a way as to minimize distortion to the underlying cultural presuppositions we wish to recover. But the paranoia of some anthropologists—who believe that nothing people tell us is to be trusted because nothing people say is free of distortion—is unjustified. As I point out in my volume paper and illustrate with regard to such features of speech as metaphor use, the invocation of cultural presuppositions in interviews, as in any spoken discourse, is largely beyond the conscious control of the speaker. The corollary to D'Andrade's observation that speakers typically cannot state cultural assumptions directly, is that they are unaware of using them. These taken-for-granted presuppositions therefore have a good chance of surviving both any foreignness of the interview genre, and any intrusions on the interview by the interviewee–interviewer relationship. Of course, this caveat does not relieve us of acknowledging, and taking measures to minimize, these two sources of bias. Once again, however, our attention to non-referential assumptions underlying prepositional knowledge, rather than to the latter "information" itself, marks ours as a very different breed of interviewing than the traditional "information-amassing" task that Briggs critiques.

It should be stressed that these various suggestions for letting the interviewee talk freely and expansively, even to the point of pursuing tangents, do not preclude leading the interviewee back to the topic when he or she has strayed altogether too far for too long, or asking questions about matters that the interviewee seems to have forgotten or avoided. A technique used by several contributors (see, especially, Quinn pp. 41–42) is to develop a comprehensive checklist of items that have ever been raised by any interviewee in connection with the topic at hand, or that the researcher thinks may be relevant to the topic, but not asking about these matters until the very end of the series of interviews with each interviewee. In this way, exhaustiveness of inquiry does not have to be sacrificed for freeness of form.

Luttrell (pp. 260–263, this volume) offers suggestions for what she calls *psychoanalytic listening*—actively paying attention to how individuals make sense of their own stories, to their fantasies, images, feelings, and the other associations they make. Luttrell found especially useful those "intruding associations" that initially seem to the interviewer to disrupt the interview and the research focus, but which led, on rethinking, to major breakthroughs in the research process. Equally rewarding for this process was attention to instances of her own reluctance to deal with topics that evoked strong or mixed emotions—"what could be called countertransference in my fieldwork relationships," she says (p. 261). Relatedly, Strauss (p. 239, this volume) advises that we should "let interviewees speak in a stream-of-consciousness fashion," which parallels the clinical technique of encouraging free association. There is also my own suggestion (p. 41, this volume) about making a mental note of, and following up on, cues that an interviewee might have more to say on a given

topic; and Luttrell's tip (p. 246, this volume) about treating interviewees' conversational "tangents" as clues. While interviewees might be culled from an initial telephone survey, as Strauss's originally were, neither she nor any other contributor would suggest anything but face-to-face interviewing, and it should be apparent, from these suggestions for psychoanalytic listening, why not.

Tape Recording

Discourse analysis, including the cultural analysis of discourse, requires some kind of written record of that discourse to work with (see Briggs 1986:99). Discourse analysts, including those appearing in this book (compare D'Andrade, this volume, p. 91, with Hill, this volume, p. 161) differ as to the types of notational systems they use for transcribing and the level of notational detail these systems require, depending on their analytic purpose (see Strauss's shift to a finer level of detail in her passages 34 and 38, this volume). As Catherine Riessman (1993:56–58), in her useful discussion of transcribing, recommends, I myself work initially from a very bare-bones transcription, returning to the tapes to listen to selected passages that bear closer analysis, and augmenting their original transcription with further detail. The initial stages of analysis typically require, as Reissman (1993:57) reports, considerable time spent just scrutinizing one's transcriptions, and going back and forth among them. (At some point in working with mine, I found I had become so familiar with them that I could quote them extensively in my head.) Of course, what and how one transcribes itself reflects the analyst's interests (Ochs 1979).[9] In general, contributors to this book, in line with our commitment to ceding control of the interview to the interviewee, try not to interpose too many transcriptional conventions and devices between the tape recording and the analysis (a point I owe to Holly Mathews, personal communication). And, in line with our interests in underlying cultural meaning, broadly speaking we begin with *what* people are saying, both explicitly and implicitly, and only subsequently and selectively turn to relevant aspects of how they say it—including, for example, metalinguistic features of discourse which may not have been captured in the original "bare-bones" transcript. Thus, for example, in her elucidation of interviewees' personal semantic networks, Strauss (pp. 205–221, this volume) initially isolates their usages of a key word ("work," in her chapter example) in her transcripts, and the most frequent associations of that word to other words and phrases, and the recurrent themes surrounding it, and then takes up associated "emotional and motivational hot spots" which may be reflected in "emotional tone," such as the pride or outrage with which memories are reported or opinions given. To take this analysis of "hot spots" one step further, she might find it worthwhile to include tone of voice in the assessment of emotional tone,

[9] Ochs suggests ways to reduce bias in transcription of child language.

returning to her tapes to listen for and label this paralinguistic feature of her interviewees' talk.

Whatever their procedures, all the volume contributors, like other discourse analysts, rely on close analysis of tape-recorded, transcribed discourse. A cultural anthropologist I know once told me that she didn't need to tape record, because she had such a good memory, which she relied upon to write everything down immediately after an interview. But cultural analysis, we have already seen, does not rely on the manifest or referential content of what is said in an interview. It requires a search for evidence of tacit understandings, and comparison across multiple samples of discourse to find patterns in this evidence. These clues are to be found, for example, in metaphor usage, in the recurrence across varied language of underlying propositions, in departures from temporal narrative sequencing, in subtle linguistic markers of the cultural standing of ideas, in nonobvious asymmetries between folktales told by men and by women, or in occasional metacomments on what had previously been said—to name just a few of the features of discourse that receive attention in the pages of this book. These features of discourse are so subliminal as not to be noticed by either interviewer or interviewee while the interview is ongoing, never mind remembered after it.[10] Even scrupulous notes taken during the interview will miss these clues. Notes are likely to capture no more than the overall sense of the conversation, along with, perhaps, brief fragments of actual speech. Moreover, such notes are likely to be rendered in the tacit, taken-for-granted, cultural understandings of the interviewer—which are not necessarily those of the interviewee. (This is decidedly not to say that tape recording relieves the researcher of keeping separate field notes, which have their own role to play in field research; detailed field notes augment several of the analyses in this volume.)

In linguistics and in most quarters of sociology and the other social sciences in which researchers use the interview or collect other kinds of discourse, the use of tape recorders would require no defense. Today, among cultural anthropologists like my colleague with the good memory, this practice does need defending. Anthropologists who object to tape recording do so on three grounds. The first reason likely to be given is that tape recording may arouse suspicion or occasion outright objection. It is true, certainly, that an historical association of recording equipment of any kind with government surveillance has made people in some nations very jumpy about its use in any context. Certainly, also, the cosmological beliefs of some other groups of people, or their attitudes toward anthropologists, or

[10] And, it needs to be inserted, the features of discourse that enter into our analyses are so often tacit, subtle, subliminal, as to require of the analyst native or near-native control over the language and culture of the interviewee. This is undoubtedly another reason why so many of the volume contributors have worked in our own language and culture. Working in another language and culture, and lacking full control of it, the outside analyst will surely want to partner with a local one.

other concerns, make them resistant to the use of recording devices, cameras as well as tape recorders, for research purposes. It is also the case that some individuals do not want some of what they have to say taped because of the potential loss of privacy. Research dependent upon analysis of taped discourse must be limited or redesigned in such circumstances. But it seems to me misguided to make these particular circumstances under which tape recorders cannot be used, into a blanket justification for not using them. All the analyses presented in this volume depend upon tape-recorded discourse. The field researchers undoubtedly had to explain, initially, the presence of their recorders and obtain permission to use them under stipulated conditions. Such explanations and negotiations are well worth making.

A second, related, objection anthropologists commonly bring forward is that the tape recorder is too invasive—too Western, too technological—a research tool. We should remember, though, that all over the world people are becoming more familiar with and more technologically sophisticated about the use of tape recorders (and even their more advanced cousins, video recorders) all the time. But, there is a more fundamental problem with this bias against the tape recorder. To single it out as overly invasive is to displace onto it a larger issue—the invasiveness of even paper and pencil as a technology, of participant observation as a method, of the more general Western project of making records of people's talk and other practices and publishing these, along with our interpretations of them, in books.

Thirdly, even if they accept the use of the tape recorder in principle, other anthropologists are primed to assume that these devices will create practical difficulties—like the graduate student I asked whether she intended to take a tape recorder with her to the field, who answered without a blink, "Oh, but tape recorders are sooo intrusive." In my own view, nothing is more intrusive—even, among some groups and in some contexts, rude—than a listener scribbling madly rather than looking at you while you talk. (I feel this way about students taking notes in classrooms, where I myself am subjected to it.) Tape recorders, in this sense, may well be *less* intrusive than paper and pen or pencil (or their replacement, the laptop), since they free the interviewer to be a good listener and pay attention. They are also small and quiet, so that their presence is almost always quickly forgotten. In my view, anthropologists ought to rethink what may amount to a romantic prejudice against the tape recorder.

This issue is important because audiotapes are an unparalleled source of data. And, as Hill (p. 161, this volume) points out, the rapid development of audio and video recording and analysis technology will only bring more and better data (as it also increases the welter of this data, raising new problems about how and what to select for transcription). Perhaps the prospect of so much data to process and analyze is daunting to some. However, if anthropologists today truly want, as we say we do, to make heard the voices of the people we study, and to avoid distorting these

voices through the filter of our own meta-narratives,[11] then we ought to welcome use of the tape recorder as an essential aid in that effort. None of the researchers who describe their methods in this book could have succeeded in their objectives of reconstructing culture from what people say, other than by close linguistic analysis of a sort that can only be accomplished from taped, transcribed discourse.

Methodological Variations

Beyond the areas of common agreement I have outlined, the collection before you is diverse in several ways that I believe will enhance its usefulness. By locating their own research interests and projects along these dimensions of difference, readers can gain a better idea of those chapters that may prove to be most helpful to them. Novice researchers among the readers will also gain a fuller appreciation of the trade-offs that they will inevitably face in their own research as to, for example, the type or types of discourse to be mined, the particular features of this discourse to be attended to, the level of detail at which these features are analyzed, the comparisons to be made, the time investment demanded and—as Luttrell (p. 254) puts it in her contribution to this volume—exactly what is to be gained and what is to be lost in each trade-off.

The contributions differ most generally in (1) whether the theoretical focus is on the cultural meaning on which individuals draw, or on individuals as negotiators of that cultural meaning—on what Hill (see below) labels the cultural "enactment," versus what she distinguishes as the cultural "production" side of discourse. And those contributors who background culture to focus on the individual differ as to (2) whether or not they work with psychoanalytic assumptions about deep motivation. Finally, the contributions differ not only, as we have already seen, in the existing methods for discourse analysis on which they draw, but also in (3) the type of discourse that is the focus of analysis, along with the theoretical concerns that underlie and motivate that choice of analytic method and discourse type. I will consider each of these dimensions of difference in turn.

[11] D'Andrade (1995:405) has pointed out that the recent anthropological experiment with narrative ethnography does not keep anthropologists from speaking for others as practitioners might wish. "By telling a story about someone," he observes, "the ethnographer does not have to make any generalizations and thereby appears to avoid the danger of hegemonic discourse. However, the appearance is deceptive, quite the reverse happens in fact. It is a natural assumption of the reader that any narrative is, in some important sense, typical of what happens in that place, unless told otherwise . . . the world is 'summarized by' and 'reduces to' the story one tells about it. *Presenting an anecdote is just as essentializing and totalizing as stating a generalization.*"

Stability and Flux, Sharedness and Variability

Theoretically, at least, as Hill (p. 159, this volume) asserts, the analysis of discourse holds the potential of ending the debate over "structure" and "agency," because "[t]o study discourse is to examine both dimensions simultaneously, since discourse is 'duplex' not only in the sense that it both enacts and produces culture, but in the sense that as social actors produce discourse, they simultaneously negotiate emergent meanings and draw on shared understandings . . ." With Hill, all of the contributors to this volume would readily agree that structure versus agency is a false dichotomy—that the enactment and production of culture are inextricable. In point of fact, though, most of the contributors fall on one side or the other with regard to which of the two claims their analytic attention.[12] Hill truly does show us how we might do both at once, but she is able to do so because her lack of commitment to any single substantive domain of culture allows her to move back and forth between pointing out evidence of "structure" or culture enactment and pointing out evidence of "agency" or culture production in the narratives she analyzes. Analyses of given domains of culture, however, are immensely time-consuming and effortful. It is difficult if not impossible to carry out an in-depth, sustained cultural analysis that will hold structure and agency both in focus simultaneously. In the end, what the analyst most cares about wins the analyst's time and effort.

D'Andrade and I are both intent on reconstructing and comprehending an underlying cultural model or schema, in its entirety. These models are widely shared and can be presumed to have considerable historical durability and, for both reasons, the models define the givens within which individuals puzzle out matters of social inequality and married life, respectively. On the continuum of *cultural standing* that Strauss (pp. 232–238, this volume) lays out, the understandings that these models embed lie at one end, being commonly held opinions or entirely taken-for-granted, leaving room for differences of opinion about occasional details only, never susceptible to controversy. Although we exploit, in reconstructing these models, the speakers' discursive use of them, neither D'Andrade nor I explore individual variation or strategic variability in that use.

Other authors, by contrast, focus on the ways individuals deploy such cultural understandings—for example, how they incorporate these shared understandings into *personal semantic networks* and compartmentalize or integrate contradictory ideas in these networks (Strauss); or struggle to explain and affirm their identities (Luttrell). These two researchers' compromise is to settle for providing just enough background to the cultural models that frame these negotiations so that the deployment of these models is comprehensible to the reader—without fully delineating the models themselves.

[12] This trade-off is not the same as that with which Luttrell (pp. 253–255, this volume) struggles, as to whether to preserve individuality or pursue patterns across individuals.

Mathews's case that manages to encompass both preexisting cultural meanings and the negotiation of these is instructive. The folktale she analyzes embodies general cultural presuppositions, about how one person's action will lead to an emotion and hence a predictable reaction on the part of the other, and, in the realm of spousal relationships, about the much more restricted range of possible reactions open to wives than to husbands. At the same time, the folktale permits alternative male and female versions, allowing men to assert their domination and women to resist it. But the tale is dense with further cultural presuppositions, about gender and marriage, motherhood and masculine status, sexuality and household honor, that intersect with the story, framing the larger significance of its tellings and defining the cultural limits, not only of the room men and women have to maneuver, but of their ability to imagine possibilities for maneuver. These Mathews sketches based only on the folktale itself and the interview material she collected to augment it. (Undoubtedly, she could have more fully reconstructed some of these cultural models from the interview material, had this been the focus of her efforts.)

The question, then, is, what does the researcher hope to gain from the analysis? In my own case, for instance, I started out by questioning whether there was a set of understandings Americans shared about marriage. More generally, I wished to use this case study to explore the nature of culture, defined as shared understandings. Although the body of discourse with which I worked would have allowed me to explore variation across individuals, marriages, or genders, or variability in, for example, the way in which individuals mold the cultural model of marriage to their own personal experience, or manage their personal psychic conflicts with respect to marriage, I have not pursued these lines of inquiry (but see Quinn 1992). Of course, one choice leads to another, ending in unexpected places. The discovery and delineation of a highly shared cultural model led me to further theorizing about the function of such culturally shared event sequences in reasoning, their role in the production of metaphor in discourse, and the basis of this cultural model in deep motivation. In one way or another, though, these are all questions about how and why cultural sharing arises.

It would be a careless mistake to assume that culture is the only source of constraint on what individuals are free to think, say, or do. Another source, of course, is the individual's own distinctive lifetime of experience, including those early, traumatic, or otherwise signal experiences that give indelible structure to the individual's ongoing concerns—the structure of the personality, if you will. In this volume this point about distinctive individual orientations to culturally shared matters is made by Strauss (pp. 209–221), in a comparison of the very different personal semantic networks surrounding two of her interviewees' views of work and welfare, and her speculation about the experiences in each of their lives leading them to these personal webs of meaning. Interestingly, the personal semantic networks of each interviewee may share themes—such as the assumption

that work is fulfilling, or the belief that welfare mothers have too many children—with other interviewees, though these themes may bear distinctive resonances, and be more or less central, for each individual. These shared themes point to subcultural clustering.

Strauss works with discourse across interviews taken from the same person at different times, allowing her to assess better the enduringness of given themes across these different tellings by the same individual. That some themes prove to have constancy is not to say that the interviewer is never implicated in the form and content of the interviewee's story. But those who emphasize only how individuals negotiate culture to suit the immediate circumstances of the interview and the interviewee–interviewer relationship, all too often work with isolated pieces of discourse collected from different interviewees. This practice (easy to fall into when the researcher does not use a tape recorder that would allow collection of longer, fuller bodies of discourse) may promote a false sense of the mutability of meanings for individuals.

Moreover, the practice of analyzing a few scattered excerpts of discourse can give no sense at all of the shared understandings that inevitably, in any society or group, frame the ideas and actions of individuals, even when these individuals, as do Strauss's interviewees, draw only selectively on what is widely understood and shape these wider understandings in distinctively personal ways, or reject them altogether. At the other end of Strauss's continuum of cultural standing from what interviewees take for granted or regard as common opinion, are ideas they proffer that they mark as matters of opinion, or even highly controversial. These are matters of opinion or controversy, Strauss stresses, with respect to what the interviewees imagine these widely shared understandings to be. An analysis founded on an inadequate corpus of relevant discourse leaves the impression, and sometimes fuels the claim, that there are no such shared understandings.

Motives and Deeper Motives

Another divergence among the volume contributors is whether or not the analyst is on the lookout for deep psychic conflict and the repression of motives to which this gives rise. Once again, hypothetically, it ought to be possible to attend at once to all kinds of motives, repressed and otherwise; but in practice the contributors to this book are attentive to different kinds of motivation. This has to do with training and theoretical interest. Luttrell, unlike other contributors, is schooled in psychoanalytic theory. Moreover, she is centrally concerned with the formative experiences that give rise to one's identity and to the inevitable lifelong conflicts that surround identity and necessitate the repression of motives. She asks how her interviewees' early school experiences motivate their struggles with a divided sense of self. It is this kind of deep motivation, implicated in their very identity, that gives Luttrell's interviewees' telling of their school

experiences the "narrative urgency" of which she speaks (see section on The Interview, above). And it is this deep motivation that the "psychoanalytic listening" she advocates is designed to recover (again, see section on The Interview).

By contrast, evidence of deep unconscious motives is likely to be peripheral to the interests of those of us whose backgrounds are in cognitive anthropology. We may be unprepared for such motives when they do emerge. In my chapter, for example, I tell how successive stages of analysis of Americans' discourse about their marriages led me, ultimately, to a psychoanalytic interpretation of a cluster of three expectations that are central to the American cultural model of marriage. I was surprised by, and wholly unprepared for, this discovery and in my published research on American marriage, it ends up as a kind of theoretical addendum, if not quite an afterthought.[13] Strauss, for another example, tells how one of her interviewees, Carol Russo, used the word "indifferent," to describe her mother and also to characterize the many people in the world who "climb all over everybody else to get to the top" and "don't care who they hurt." When Strauss points this parallel usage out to her, Russo becomes upset and denies thinking that her mother was a bad person. Strauss suspects that this might be a case of unconscious feelings, but she does not pursue it. Such ambivalence about their mothers, when Luttrell's interviewees express it, is a clue that becomes key to her analysis. But Strauss is frying other fish.

Strauss is bent on retrieving meanings, motives, and the conflicts among them that are not unconscious in the sense of repressed, but may be no less unaware. She charts the way speakers manage inconsistent beliefs by compartmentalizing them so they need not become aware of their inconsistency; express assumptions that they are not aware of holding; and mark the cultural standing of their assertions, often, again, without being aware that they are doing so. As Hill (pp. 194–195, this volume) points out, not all "disfluencies" stem from psychodynamic processes: Alternatively, such false starts, hesitations, and other pauses may be markers of novel topics that the speaker has not yet routinized; or a display of the exactitude with which the speaker is choosing her words. In fact, in "A dollar and a quarter an hour," one of the narratives that Hill examines in her volume chapter, the interviewee's disfluencies appear to be products of the "hard interactional work" that goes into not offending her interviewer, whose viewpoint she guesses differs from her own. Speakers are sometimes no more aware of this interactional work than they are of the work that goes into strategies, like compartmentalization, for cognitive conflict reduction. Therefore, it should come as no surprise that some of the psychoanalytic listening techniques advocated by Luttrell for the recovery of unconscious motives, surface in both Strauss's and Hill's methods of transcript analysis.

[13] Although, subsequently, this unexpected psychoanalytic finding has taken me in a whole new research direction and occasioned a crash course in theories of attachment and separation.

The contrast with Luttrell's approach makes clear, however, that the methods presented by the other contributors to this volume have not been designed with the recovery of deep motivation in mind. It would be extremely helpful to all of us who do cultural analysis of discourse, I think, if psychoanalytic anthropologists would collect a volume of papers, parallel to this one, on methods they have developed for psychoanalytic listening in anthropological research—with all that such methods entail in terms of attention to subtle clues in the content of what interviewees say, in their manner of saying it, and in the interaction and the larger relationship between interviewer and interviewee. One already published example of such a psychoanalytically informed method that comes to mind is Jeannette Mageo's (2001) jewel of an article on her collection and analysis of Samoan students' reported dreams.

Of course, people do not live their whole lives at the level of deep unconscious conflicts surrounding their senses of identity. Many other motives, and, sometimes, conflicts among these, arise in the course of people's negotiations of their own beliefs and feelings and their social interactions with other people who may believe and feel differently. Although not all of people's beliefs and feelings need be deeply implicated in their identities, their management of these less identity-bound beliefs and feelings has no less practical consequences for the way they act, as Strauss shows in the context of beliefs about welfare. And other motives can have powerful directive force over people's beliefs, feelings, and consequent actions. This can happen when, for example—as with the feeling/motivation/action templates that Mathews reconstructs in her analysis of the *La Llorona* tale—the motivational link between feeling something and taking some course of action has become so naturalized as to be accepted without question as a guide to—or in the case of this Oaxacan folktale, a cautionary lesson about—one's own behavior. For Oaxacan narrators of *La Llorona* and their audiences, a woman's public shaming leads to her suicide.

This point about the ubiquity and consequences of other than unconscious motives must be stressed, if only because some who are committed to psychoanalytic theory are inclined to privilege the conflict arising from unconscious motives over other kinds of conflict and other kinds of motives. (Exemplifying this stance is Charles Nuckolls [1996:9–17], who has taken Strauss to task for not exploring, in her work, the unconscious motives held by her working class interviewees. Strauss's analysis reveals lasting, consequential motives of other kinds—ones not addressed, as it happens, by Nuckolls.) The point bears emphasis: No analysis of discourse can reasonably be expected to do a multiplicity of analytic tasks.

Types of Discourse Analyzed

The contributions, finally, represent three broad strategies for selecting discourse types for analysis. Some contributors—D'Andrade in his classroom exercise on social inequality, Strauss in her analysis of people's attitudes toward welfare, and I in my analysis of people's understandings of

marriage—focus on passages containing the *expository discourse*. This consists in propositions stated in the form of explanation, belief, opinion and the like, and, as I argued in a previous section (The Interview), is especially rich in linguistic clues to underlying cultural understandings. D'Andrade's (p. 90, this volume) student interviewers asked their interviewee colleagues a series of questions such as, "What does social equality mean to you?" "What are examples of social inequality?" and "Why does this happen?" and probed for clarification and amplification in a way that was well designed to elicit explanations of social equality and inequality. Strauss and I also asked our interviewees questions on our respective topics, but over longer, less tightly focused, interviews that encouraged the interviewees to stray to matters that they perceived to be related.

As already noted, Hill and Mathews analyze *narrative*. For Hill the most general point is to demonstrate how culture-full such narratives are, and how this rich cultural meaning is made through narratives, as well as how these cultural meanings can be extracted from them. She chose them to illustrate, more specifically, the structure of narrative and a series of theoretical problems and possibilities in the cultural analysis of narrative sequence, attempts at coherence construction, apparent deviations from narrative structure, the dialogic dimension of narrative, and the embedding of narrative within longer discourse. Mathews analyzes variants of the folktale, *La Llorona* (The Weeping Woman), for the obvious reason that she was concerned with the domain of gender and this particular folktale—variants of which are widespread in Mexico, and which is a conspicuous part of folk tradition in the Oaxacan Mexican community she studied—is a matchless window onto gendered relationships, particularly marital ones. The tale is used by Oaxacan elders as a dramatic device to inculcate in young men and women certain moral values concerning marriage. Hence it is made up of injunctions about the ideal marital relationship and illustrations of how this relationship can fail.

Finally, Luttrell and Strauss both analyze *life stories* (Linde 1993). Strauss (p. 204, this volume) terms these *life histories* to mark the fact that she gathered this life story material by asking interviewees to tell the complete stories of their lives (rather than about some particular part of their lives as, in Luttrell's case, their school experience). These life stories or life histories are larger units of discourse that contain both conversational narrative and explanation (Linde ibid: 67–94).[14] Notably, they are *stories* rather than *a story*—that is, segments of a longer story of one's life that is

[14] Linde (1993:85–90) describes a third discourse type, the *chronicle*, that occurs in life stories. Perhaps because it is less useful for their various purposes, none of the contributors make analytic use of the chronicle. It is also possible for a fourth discourse type, the *conversational exchange* between story teller and listener, to occur during the telling of life stories, in interviews and in other venues. Deliberately ceding control of the conversation to the interviewee, as we advocate, tends to minimize the number of conversational exchanges. However, those who are concerned with deep motivation and the evidence for such motives that is provided by transference and countertransference, such as Luttrell in this volume, may find such exchanges especially telling.

told discontinuously at different times, and that is always provisional, being supplemented, updated, and revised over the course of one's life (see Quinn, p. 42, this volume). Even the "complete" life histories collected by Strauss have this selective and unfinished quality of all such life stories.

What motivated the contributors in their choice of discourse type? This choice depended on a purely pragmatic assessment of the likelihood that exposition, narrative, or life story would be dense with cultural presuppositions on the topic of study. For example, Mathews (personal communication) decided to elicit tellings of the *La Llorona* folktale after she had heard the tale told in spontaneous conversation and realized that men and women were telling strikingly different versions of this same tale. She also noticed that the tale was being, not actually told, but referenced, in the context of talk about others' inappropriate spousal behavior or the state of their marriages. She surmised from all this that the folktale would be a rich source of cultural presuppositions about marriage and gender.

For another example, asking people to tell me about their marriages suited my purpose of reconstructing their understandings of marriage, because their marriages are typically an important, richly discussed segment of married Americans' life stories. D'Andrade might have used the same device, asking students about their personal experiences with social inequality as they were growing up and going to school, for instance. But he could not be certain, as I was with regard to adults' marital experiences, that all students would have had or recognized experiences of social inequality, or found them important enough to incorporate into their stories about their lives. He and his student researchers might have failed to pick up, from these stories, a sampling of interviewees' views on social inequality and inequality that would be extensive and comprehensive enough for analysis. On the other hand, some of D'Andrade's students' interviewees undoubtedly did proffer, in the course of their interviews, brief anecdotes about their own experiences of social inequality. These stories would be just as useful as the other discourse gathered, for reconstructing the "gist propositions" D'Andrade extracted from interviewees' talk on the subject.

Researchers who are directly interested in the topics of self and identity are most likely to find life stories analytically helpful, since these stories contain such a concentration of presuppositions about one's self and one's identity. It is no accident, then, that Luttrell, setting out to develop a theory of "the links among school structure, culture, identity, and self-understanding" (p. 254, this volume), chose to analyze life stories. Strauss, too, in that part of her presentation in which she analyzes her interviewees' life stories (especially pp. 213, 218–219, this volume), employs them to interpret individual interviewees' *personal semantic networks*—key elements of which turn out to be central to the interviewee's self-image. Strauss, however, is more concerned with personal semantic network analysis as a first step in the discovery of central themes (clusters of shared associations in such a network) across people, and the assessment of how central or peripheral a given theme might be for most people.

As Luttrell (p. 262, this volume) makes plain about her efforts to reconstruct her interviewees' models of schooling, whether we are intent on exploring presuppositions about gender, attitudes about welfare, beliefs about marriage, or identificatory struggles, we all provide good examples of D'Andrade's principle of indirection. That is, we all ask about something that will make the speaker use the model under examination. What that model is, and what are the discursive contexts and formats for talk about it, dictate the discourse type most suitable for its reconstruction.

A Digression on Life Stories Cross-Culturally

Life stories are actually somewhat more common in cultural analyses of discourse than suggested in the last section. Several contributors to this volume, other than the two named in the last section, also elicited some version or segment of life stories. For example, I began each initial interview with the question, "How did you and your husband (wife) meet?" Luttrell and Strauss, however, are the only contributors who ground their analysis in the overall structure of the life stories they collected. Others of us (including Strauss in other parts of her analysis), instead, mine the life stories we collect for the narrative or expository discourse that occurs within them. For instance, I focus largely on explanations people give about their marriages. The reliance on life stories, not only as analytic units in and of themselves, but also as handy vehicles for collection of the discourse types that occur within them, raises a question about the feasibility of collecting them cross-culturally.

Linde (1993) based her theory of life stories on middle-class Americans' stories about how they had come to be in the occupations they were in. As a practical matter, can researchers working in other societies and other subcultures of our own expect to encounter life stories for use in their analyses? This question is not unconnected to the issue, discussed earlier, of the cross-cultural transferability of interviewing; that is, one impediment to collecting life stories cross-culturally may be that interviews do not work very well to elicit such stories. But there may be another, more fundamental, complication. Is the life story itself universal?

At the very least, in conducting cross-cultural research we should be prepared for the probability that the narratives people produce about themselves and their own lives will vary in unusual and unexpected ways. As Linde (1993:11) concludes, "Other cultures may include different items and use different forms." She (1993:47–48) cites the ethnographic case in which an Ilongot man considered by anthropologist Renato Rosaldo to be especially intelligent and introspective produced, instead of the "deep and intricate life story" the anthropologist anticipated, an account that "focused on his public self and public actions, but hardly touched on what Rosaldo considered a necessary description of his private self." Rosaldo might be surprised to learn that eighteenth century Euro-American life stories had more in common with that of his Ilongot narrator than with our

own. Describing the modern self as "an autobiographical self," Arlene Skolnick (1991:165) goes on to observe,

> Marti Kohli, a scholar of the life course, notes a shift in the idea of selfhood starting at the end of the eighteenth century. Earlier, autobiographies had been organized around frameworks external to the person, such as historical or seasonal events. They later came to be organized around a developmental, individualized conception of the self.

There are undoubtedly other dimensions of autobiographical variation cross-culturally (see, for example, Luttrell, fn. 11 in this volume, on Ruth Behar's Mexican interviewee's definition of what was worth telling in her life story). Sudhir Kakar (1990:2) makes the intriguing observation that the folktales traditional Indians hear and tell "are worked and reworked into the stories of their own lives. For stretches of time a person may be living on the intersection of several stories, his own as well as those of heroes and gods." Kakar notes the Indian inclination to prove a point or convey one with a story, speaking of "the marked Indian proclivity to use narrative forms in the construction of a coherent and integrated world." He (1990:3) speculates that "the Indian celebration of narrative (and the dramatic) has its roots in one of the more enduring and cherished beliefs of the culture." This is the belief in another, higher, level of reality above empirical reality. This ultimate reality can only be apprehended experientially, it is believed—and hence is best conveyed through story. That is, the fact that Indians are fond of explaining themselves with stories, and interweaving their own life stories with tales of mythical figures, can be attributed to their belief in the rhetorical power of storytelling. If Kakar is right about this, the Indian penchant for telling traditional stories would seem to be a cultural embellishment (like, for example, the West African penchant for salting speech with appropriate sayings). Where the life story is recognized, as in India, it may be "a cultural genre that varies from place to place" (Peacock and Holland 1993:376), marked by differing cultural and social conventions in different places.

More radically, there are likely to be many other groups, as Linde (1993:11) surmises, whose "members do not conceive of themselves as having a life story." This does not mean that they do not tell about their lives. We might reasonably expect to find, among the members of any human community, the same impulse to manage the psychic conflict they feel about their pasts that Luttrell's interviewees display in their school stories, the same desire to justify themselves that Strauss's interviewees exhibit in their renditions of their life histories, the same attempt to do so coherently that Linde ascribes to Americans telling stories about how they chose their occupations. Perhaps these inclinations to manage felt conflicts in one's life, to justify that life, or to make it coherent will find voice and even require articulation. Perhaps such narratives about one's life will be associated with, and prompted by, talk about happenings or in contexts

that the anthropologist could not have guessed and must discover—disputes over land, for example, or the death of relatives, or religious rites. Such is fieldwork.

Still, such possibilities, and the observations about the differing assumptions that govern Ilongot and Indian life stories, should alert those who intend to transport life story collection and analysis cross-culturally, to be sensitive to the cultural variation they may encounter in such stories in addition to the unexpected difficulties they may encounter in trying to elicit them by means of interviews or otherwise.

Pedagogic Strategies

The emphasis of this book is on the *how to*. At the same time, I hasten to add, these chapters do not contain recipes. Far from it. Instead, in different ways, their authors attempt to engage the reader in the actual process of doing their kind of research. They adopt somewhat different pedagogic strategies for presenting their research methods and the lessons to be derived from their research. Contributors have chosen, in differing degree and combination, different ways to convey how to actually do analysis. All of the authors have elected to situate their account of how to use given methods within certain philosophical stances or guiding convictions or words of wisdom that drive their research and make sense of it for them. Different authors have chosen to highlight different lessons as these relate to the cultural analysis of discourse. Collectively they convey a number of such messages that I otherwise might have felt obliged to cover in this introduction, where, stated in the abstract, they would have lost the immediacy and passion with which the authors' accounts infuse them. The authors tie these philosophies of doing research to the actual doing of their own research as reported in their chapters. The reader is led to see how otherwise abstract-seeming research philosophies are inextricably related to concrete research methods, and how the two mutually inform and invigorate each other.

From this point, the contributors follow three somewhat different strategic directions. Some of the authors (Luttrell, Mathews, and myself in this volume) have elected to tell the story (a segment of our own life stories, Charlotte Linde would observe) of how we conducted one major piece of research. In a variation on this strategy, D'Andrade gives an account of the innovative and useful method he developed to teach undergraduates to do research on American culture, using one particularly successful case study. This strategy, of telling the story of a research project from start to finish, has the advantage of addressing method at a macrolevel: Where to start? What leads us from one stage of analysis to the next? How do we have the confidence to take that next step? And how do our research and our theory evolve as we go? Hopefully, too, we convey the visceral side of this process: the inevitable drudgery, the hesitations and

miscues, uncertainties and anxieties, as well as the happy serendipities, and the satisfying moments of discovery, that attend analysis. The overall lesson of Luttrell's story (which has also been a theme of this introduction) is that at each point in the analysis trade-offs have to be made. The largest lesson of my story (and a subtheme in Luttrell's) is that the course of theory-building—the phase that D'Andrade, in this volume, calls the context of discovery—can never be known in advance, but is always an adventure. Mathews counsels us that the ultimate way to theoretical insight is the deliberately eclectic, but always systematic use of discourse analytic methods. D'Andrade teaches us that this discovery process, no less than that of verification, can be a systematic one and can yield valid, robust, results.

Hill, by contrast, has elected to home in or, in terms of the narrative analysis to which she introduces us in her chapter, "zoom in" on one or more smaller segment of discourse for analysis. The advantage of this strategy is that no step in that portion of analysis under scrutiny is left to the reader's imagination. Indeed, a larger lesson Hill's chapter teaches, a lesson cultural anthropologists can learn from linguists more generally, is that no detail of syntax or lexical choice is too fine a clue to consider. A second, related, lesson of her chapter is that especially useful clues are deviations from the expected structure of discourse—in this case, narrative—and narrative options not chosen.

Hill then "zooms out" again. She and Strauss organize their chapters in still a third way, to present an array of methods united by their suitability for addressing a particular set of theoretical problems—in something that approaches a culling of the relevant methodological literature. In Strauss's case, these are questions about variability in the way individuals interpret cultural beliefs: in the meanings different individuals associate with these beliefs; in the extent to which a given individual holds competing beliefs, and in the cultural standing that a person grants his or her beliefs. Hill raises a set of related questions about conversational narrative, and reviews methods for answering these questions.

An editor from one academic press advised me against the format of this edited collection, assuring me that what readers want is a single-authored book that lays out and evaluates all these methods in a single, uncomplicated voice. To the contrary, the great advantage of a collection like this is that it illustrates the research process as it really is. It lets the individual researchers speak and their individual research projects and approaches live in such a way as to bring home, and validate, the great diversity of personal approaches that researchers bring to their work, and the necessity of finding your own. As Luttrell (p. 266, this volume) quotes Karen McCarthy Brown as saying, anthropological research is "a social art form." Researchers should not approach this book thinking that they will find just that method or set of methods they can mechanically and unquestioningly apply to their own research. Instead, each researcher will have to find and create his or her own personal approach. Guided, in each

case, by local opportunities for and limitations on collecting discourse, by the nature of the resulting corpus and, in the end most decisively, by the theoretical questions being asked and perspective being taken. I hope readers will treat the chapters in this book as inspirational reading, providing them with some ideas and some of the self-assurance to help them adapt and invent methods to suit their own diverse research purposes, just as the contributors have done. For the most general lesson of this book is that the cultural analysis of discourse can be done by anyone with the patience for close, attention-demanding, time-consuming work and an eye for pattern, detail, and nuance.

As a corollary to this appeal for methodological open-endedness, this book is not by any means the last word. I hope that it will spur others, you yourself perhaps, to invent further methods or adapt the ones presented here, for your own research ends. If this happens, and enough of you do so, eventually there will be a *Finding Culture II* (to be edited by someone other than myself, however).

Acknowledgments

This is a much better introduction for the efforts of two astute anonymous manuscript reviewers who, as good reviewers do, held me accountable at every turn. It is much better, too, for the two rounds of extensive, thoughtful revisions suggested by Holly Mathews and by Claudia Strauss. I owe much to both of them, not just with respect to their careful and intelligent readings of this manuscript but in the long intellectual run. I am also indebted to Judith Irvine for her insightful and helpful comments on this introduction, and to Jane Hill and Stephen Leavitt for more particular but nonetheless invaluable suggestions for its revision. On a few matters, I admit to going my own way in the face of the contrary advice or expressed concern of one or another of these reviewers.

References

Agar, Michael and James MacDonald. 1995. Focus groups and ethnography. *Human Organization* 54:1:78–86.

Briggs, Charles L. 1986. *Learning how to ask: A sociolinguistic appraisal of the role of the interiew in social science research*. Cambridge, England: Cambridge University Press.

Cain, Carol. 1991. Personal stories: identity acquisition and self-understanding in Alcoholics Anonymous. *Ethos* 19:2:210–253.

D'Andrade, Roy. 1987. A folk model of the mind. In *Cultural models in language and thought*, ed. D. Holland and N. Quinn, pp. 112–148. Cambridge, England: Cambridge University Press.

———. 1995. Moral models in anthropology. *Current Anthropology* 33:3:399–408.

Fairclough, Norman. 1992. *Discourse and social change*. Cambridge, England: Polity Press, Blackwell Publishers.

Garro, Linda. 2000. Remembering what one knows and the construction of the past: a comparison of cultural consensus theory and cultural schema theory. *Ethos* 28:3:275–319.

Holland, Dorothy and Jean Lave. 2001. History in person: an introduction. In *History in person: Enduring struggles, contentious practice, intimate identities*, ed. D. Holland and J. Lave, pp. 3–33. Santa Fe, NM: School of American Research Press.

Holland, Dorothy and Debra Skinner. 2001. From women's suffering to women's politics: reimagining women after Nepal's 1990 pro-democracy movement. In *History in person: Enduring struggles, contentious practice, intimate identities*, ed. D. Holland and J. Lave, pp. 93–133. Santa Fe, NM: School of American Research Press.

Kakar, Sudhir. 1990. *Intimate relations: Exploring Indian sexuality*. Chicago, IL: University of Chicago Press.

Linde, Charlotte. 1993. *Life stories: The creation of coherence*. Oxford, England: Oxford University Press.

Luttrell, Wendy. 1997. *Schoolsmart and motherwise: Working-class women's identity and schooling*. New York: Routledge.

Mageo, Jeannette. 2001. Dream play and discovering cultural psychology. *Ethos* 29:2:187–217.

Mathews, Holly. 2000. Negotiating cultural consensus in a breast cancer self-help group. *Medical Anthropology Quarterly* 14:3:394–413.

Nuckolls, Charles. 1996. *The cultural dialectics of knowledge and desire*. Madison, WI: University of Wisconsin Press.

Ochs, Elinor. 1979. Transcription as theory. In *Developmental pragmatics*, ed. E. Ochs and B. B. Schieffelin, pp. 43–72. New York: Academic Press.

Ochs, Elinor and Lisa Capps. 2001. *Living narrative: creating lives in everyday storytelling*. Cambridge, MA: Harvard University Press.

Peacock, James and Dorothy Holland. 1993. The narrated self: Life stories in process. *Ethos* 21:4:367–383.

Quinn, Naomi. 1992. The motivational force of self-understanding: Evidence from wives' inner conflicts. In *Human motives and cultural models*, ed. R. D'Andrade and C. Strauss, pp. 90–126. Cambridge, England: Cambridge University Press.

Reissman, Catherine Kohler. 1993. *Narrative analysis*. Qualitative Research methods Series 30. Newbury Park, CA: Sage Publications.

Schiffrin, Deborah. 1987. *Discourse markers*. Cambridge, England: Cambridge University Press.

Skolnick, Arlene. 1991. *Embattled paradise: The American family in an age of uncertainty*. New York: Basic Books.

Strauss, Claudia and Naomi Quinn. 1997. *A cognitive theory of cultural meaning*. Cambridge, England: Cambridge University Press.

van Dijk, Teun ed. 1985. *Handbook of discourse analysis, Vol. 1: Disciplines of discourse*. London: Academic Press.

Yurchak, Alexei. 1997a. *The cynical reason of late socialism: Language, ideology and culture of the last Soviet generation*. Ph.D. dissertation, Duke University.

———. 1997b. The cynical reason of late socialism: Power, pretense, and the *Anekdot*. *Public Culture* 9:161–188.

Chapter Two

How to Reconstruct Schemas People Share, From What They Say

Naomi Quinn

Over twenty years ago, when I began the research that illustrates the methods advocated in this chapter, the theoretical issues about the nature of culture that I intended this research to address were very much unsettled. An older theory of linguistic meaning, on which the theory of cultural meaning that I had learned in graduate school had been predicated, was failing. It was up to my generation of cognitive anthropologists, I felt, to build a new and better theory of culture. An exciting new framework, that held promise for cognitive anthropology, was emerging from the multidisciplinary enterprise of cognitive science. But the ideas we were borrowing from cognitive science were themselves still young, undeveloped, and disputed.

This theoretical shift in cognitive anthropology demanded wholly new methods; and, when appropriate methods were not to be found in the existing research literature, they had to be invented. They were not, as methods never are, invented out of whole cloth; rather, I drew upon several sources for inspiration, particularly work in linguistics then coming to my attention. I did not feel any more bound to these approaches than I did to earlier methods from cognitive anthropology, however; I adapted them to my own uses. The first important methodological lesson I learned is not to assume that existing methods define the range of possible ones, and not to shrink from inventing our own ones. I hope the story that this chapter tells will inspire readers to invent their own methods, when the time comes, to suit their own theoretical and research needs.

The new methods I made up were quite different from those I had been trained to use. They were designed for a more naturalistic cognitive anthropology and for the wholly different kind of linguistic material—extended discourse—that this naturalistic approach demanded.[1] And these methods involved me in what were, for me, unfamiliar kinds of analyses of new features of language, and a different overall style of analyzing it. This new style was less mechanical than what I had been taught, and—what? More "organic," dare I say? By *mechanical*, I mean a method involving

[1] Parenthetically, it has been difficult to persuade funding agencies to cover the very labor-intensive collection and transcription of this discourse.

procedures specified from the outset and applied in an unvarying way and order to produce a unique solution. By *organic*, I am trying to convey how one analytic move grew out of the last, how each new analysis drew opportunistically on features of the material at hand, and how consecutive layers of analysis eventually added up to a whole. These characteristics of my method mark the second major methodological lesson of this chapter.

Time and again, I have seen graduate student dissertation research proposals flounder at the point of describing the data analysis phase of the proposed research. In part, this is because cultural anthropology graduate students do not learn to think in terms of data analysis, much less learn specific methods for doing it—a deficiency in their training that this volume hopefully begins to address. But in part also, the difficulty is generic to naturalistic research of the sort I do, and the sort that many cultural anthropologists want to do. In this kind of research, it is impossible to fully specify one's proposed methods in advance. This does not mean that one cannot and should not suggest, in one's proposal, general methodological approaches that one intends to pursue. But one is unlikely to be able to spell out, on the basis of these existing approaches, how one is going to analyze the discourse one has not yet collected and the characteristics of which one does not yet know, for research objectives of one's own.[2] Below I show how I analyzed metaphors, reasoning, and key words in the discourse I collected. Yet, it would be unhelpful to reify these analytic strategies as methods—to call them "metaphor analysis" or "reasoning analysis" or "key word analysis," for example, as if one were going to utilize these and only these methods, and as if they were "canned" so that one could apply them in a preordained way. Instead, as I have suggested, and as the collective chapters in this volume illustrate so well, each researcher is in the business of developing his or her own methods as these best suit the kind of discourse that has been collected and the research objectives for analyzing it.

I would not want to leave the impression that I foresook everything I had learned about method in graduate school. Quite the contrary. I had learned to believe, in the most general way, in the importance of method to good science—of being able to know, and demonstrate, how one had arrived at one's claims. To many social scientists this may seem too obvious to bear saying, but it is certainly a contested position in contemporary cultural anthropology. Two other general methodological lessons had also rubbed off on me in the course of my graduate training. One was a pragmatic, how-to approach to devising methods. The other was the value

[2] In experimental science, methods are specified in advance. Indeed, doing so, and following these methods exactly as specified, grants experimental findings much of their convincingness. Naturalistic research has different standards of convincingness, such as comprehensiveness, parsimony, and generalizability of the explanatory account. To the degree that agencies that fund cultural anthropology implicitly adopt the methodological standards of experimental science and assess naturalistic research proposals by these standards, a great injustice is done to prospective research in our field.

of employing systematic and close analysis. These are points that are illustrated in what is to follow.

The Methodological Challenge

By the semantic theory I had learned, words derived their meaning from the larger set of related words—the so-called lexical contrast set—of which they were a part. Thus, in a favorite example of the day, the word *bachelor* was said to be defined as an unmarried man, contrasting, along one dimension of meaning, with a married man (for whom no discrete lexical term like "bachelor" exists in American English), and, along another dimension with an unmarried woman (for which the corresponding term is *spinster*). Similarly, *orphan* could be defined as a parentless child. Cognitive linguist Charles Fillmore (1975) argued against this "checklist theory of meaning," as he called it. He pointed out that such a theory did not account for why one could not properly refer to the Pope, for instance, or to a wolf-boy grown to manhood beyond the pale of civilized society, as a "bachelor." Fillmore suggested an alternative theory that made sense of such anomalies. We understand what it means to be a bachelor, he said, in terms of the "simple" or "prototype world"[3] that we imagine bachelors to inhabit. In this world, men become eligible to marry and are expected to do so around a certain age, and bachelors are men who, for one reason or another, have delayed marriage beyond this expected time.[4] The Pope, however, has foregone marriage at any age, while the wolf-boy is excluded from consideration as a marriage partner on grounds of unsuitability. In other words, neither inhabit the prototype world of marriage practices that *bachelor* invokes. In this world, boys are deemed eligible for marriage when they have grown into men, not only physiologically but socially, and, as social adults, are ready to leave their natal families and establish their families of procreation. Neither the Pope nor the wolf-boy follow this standard course of social maturation. The very decline in usage of this word in the United States today, and its seeming quaintness to us, are another kind of evidence pointing to the embeddedness of bachelorhood in a larger set of social conventions and understandings about marriage and the life-course. This decline signals the near-disappearance of a world in which it made sense to mark the marriageability of young men and women and worry about their marriage prospects—and hence to distinguish a man as a "bachelor" or a woman as a "spinster."

[3] Elsewhere in the same paper he also called these "scenes," and linguist Ronald Langacker (1979), to whom the *orphan* example is owed, named them "functional assemblies."

[4] We could note, although Fillmore does not, that recognized variants on such prototype worlds—worlds within worlds—may be identified by the terms assigned to them, too: So, for example, an *eligible bachelor* is one who is eminently marriageable, while a *confirmed bachelor* (sometimes used as a euphemism for homosexual) is one who has decided never to get married.

As things developed, Fillmore's notion of "prototype worlds" turned out to belong to a larger set of proposals that emerged from the cognitive sciences of the day, and that were to change the way everybody thought about cognition. I myself was initially and most deeply influenced by Fillmore's formulation, because it posed a direct challenge to the semantic theory that had governed the comparative study of kin terms, address terms, and ethnobiological terms and the like (hence its early names of *ethnoscience* and *ethnosemantics*), with which my subdiscipline of cognitive anthropology had been preoccupied. While Fillmore and other linguists were concerned to build a theory of word definition, and interested in the worlds or scenes behind words for their bearing on word definition, I had a different interest. I was drawn to the idea of these prototype worlds because they seemed to me to be exactly the kind of construct needed to capture the complexity of cultural meaning.[5] As time went by, a number of variant proposals for conceptual entities like "prototype worlds" and "functional assemblies" were subsumed under the label of *schemas*, and what schemas were began to be worked out. Cognitive anthropologists like myself found ourselves borrowing schema theory to reconceptualize cultural understandings in its terms.

A *schema* is a generic version of (some part of) the world built up from experience and stored in memory. The schema is generic—as Fillmore said, simplified and prototypical—because it is the cumulative outcome of just those features of successive experiences that are alike. Although schemas can change, those built on repeated experiences of a similar sort become relatively stable, influencing our interpretations of subsequent experiences more than they are altered by them. To the degree that people share experiences, they will end up sharing the same schemas—having, we would say, the same culture (or subculture). The social world is constructed in just such a way that many of our experiences—the language we speak, for example, or the way we are brought up as children, or the built environment we inhabit—are indeed shared. Hence, many, many of our schemas are cultural ones.[6]

Schemas can include words, but are hardly limited to these. They can include experience of all kinds—unlabeled as well as labeled, inarticulate as well as well-theorized, felt as well as cognized. Schemas, in short, can be as various and complex as the experience from which they are derived. The same is true, of course, for cultural schemas, which do not differ from other schemas except that they are built up from experience that has been shared.

[5] Subsequently, seeking a more developed theory of the prototype worlds behind words, along with many other people I became intrigued with the notion of *scripts* proposed by Roger Schank and Robert Abelson (1977). Ultimately, I found Schank and Abelson's formulation wanting, both in sensitivity to the cultural constructedness of understandings, and in generalizability to all the various ways (other than conventionalized sequences of temporally ordered events) in which cultural understandings can be organized.

[6] See Strauss and Quinn (1997).

The shift from a "checklist," word-bound, theory of cultural meaning to the theory that these shared meanings are embedded in complex schemas, carried with it an equally radical methodological departure. The method I knew, in which I had been so relentlessly schooled as a graduate student, was the formal analysis of lexical sets. The word *bachelor* provides a quick illustration of this method, by which a formal analysis of the word's meaning could be represented:

	Male	*Female*
Married	0	0
Unmarried	*bachelor*	*spinster*

The zeros in the upper row indicate that there is no single word for *married man* or *married woman* analogous to *bachelor* and *spinster*. (*Husband* and *wife* refer to the reciprocal relationship of a man and women who marry each other, but not their status as married people in the same way that *bachelor* and *spinster* refer to these individuals' status as unmarried; one says, not, "He is a husband" or "She is a wife"—unless one were stressing that being a wife was a woman's sole occupation—but "He is her husband" or "She is his wife.") Analyses like this one are accomplished by equating the meaning of cultural objects and events with the meaning of the words that label them, and then reducing word meaning to just that component of it that is contained in the contrast between each word and other words in its lexical contrast set. The dimensions of contrast that define *bachelor* are "sex" and "marital status." Such analyses have the great advantage of resting on specifiable, largely formalizable operations; this is a big part of their scientific appeal. The convincingness of the method rested (and sometimes also, as in the great dispute about the componential analysis of American English kinship terms, fell) on its ability to produce analyses in which these operations led to unique solutions.

Cognitive anthropologists who subscribed to this theory of word meaning often spoke (and some still do) as if the meaning derived from contrasts among words subsumed all of cultural knowledge. However, once we take cultural meaning to be much more than the words we attach to the objects and events in our world, the formal method I have illustrated no longer serves us as a useful tool for recovering that meaning. What kinds of method might capture the cultural schemas that cultural modelers theorize to underlie shared understanding? In my search for such a method, I had to start from scratch. As will emerge, I turned to a kind of linguistic data, and developed a mode of analyzing this, that were to share none of the old method's formalism.

I was determined to open an entirely new research project, one that would provide me with the rich material I needed in order to explore

cultural schemas. I made the decision to conduct this fresh research in the United States.[7] Because it seemed a topic on which Americans would have much to say and be willing to talk about, and also because it intrigued me personally, I decided I would investigate Americans' shared understandings of marriage. The most obvious and direct way to get access to these understandings appeared to me to be through what people had to say about marriage.

Because I was interested in ordinary people's understandings, this meant investigating what ordinary people had to say. It seemed extremely inefficient, though, to stand around in likely public settings—bars, perhaps—waiting for occasions on which people happened to talk about my research topic. Private occasions on which talk about marriage was likely to be thicker, such as married couple's tête-à-têtes or their marital therapy sessions, hardly seemed accessible to me. Thus it was that I embarked on an interview project.[8] I think interviews must always be the methodological strategy of choice for collection of discourse on a topic like marriage, a topic that cannot conveniently be recorded as it occurs naturally in discourse, because it neither arises frequently and regularly in all everyday talk (as do address terms, for example), nor appears predictably in a well-defined setting (like legal discourse, for example).

On the other hand, I chose marriage as the topic of my interviews precisely for the reason that people seemed ready to be interviewed about it at the drop of a hat, freely, and at length. Other researchers may have theoretical or policy-related reasons for investigating specific other cultural understandings. The topics they set out to study may turn out to be topics that people do not talk about all that often in any setting. Interviewees may not treat such topics as part of their life stories, for example, and may not be prepared to produce extensive discourse on this topic, nor find it easy to do so. Steven Bialostok (personal communication) has shared with me his difficulties in getting people to talk about literacy, for example. In such a case, the researcher will need to structure interviews much more tightly, around a series of queries about other topics that do arise in natural discourse, and within which the topic of research is likely to arise. In the case of literacy, Bialostok found, some queries that worked were asking about literacy-related activities such as reading to one's children, or specific literacy-related memories such as those of books and other reading materials that were part of one's world when one was growing up. Ingenuity and trial-and-error will certainly be required to locate such topics and design the right questions to ask about them. Even then, as Bialostok discovered,

[7] I should explain that this point in my research career coincided with the childrearing years of my life. I was a single mother, and my choice of field site had a lot to do with my misgivings about taking two small children to the field. (I had managed previous fieldwork among the Mfantse people of coastal Ghana with one child, and seen how much it slowed down my research.)

[8] See Linde (1993:57–58) for this same point, which bears repeating.

interviewees are likely to have much less to say about each topic, and it may be necessary to conduct shorter interviews with many more individuals in order to amass a corpus of discourse of desirable size.[9]

The So-called Interview

I aspired to collect interviews that resembled as closely as possible the spontaneous discourse about marriage that might occur in all the likely places—for example, between strangers at bars, friends in coffee klatches, married couples themselves in moments of confidence, or married people and their marital therapists. I wanted ordinary talk, but found it impractical to collect. Can interviews come close enough to ordinary talk to provide ordinary cultural understandings? I think so.

I developed a style of interviewing in which I and my research assistants[10] deliberately ceded control of the "interviews" to the "interviewees," allowing them to decide how their interviews should be organized over all, what topics should come next and what might have been overlooked or unfinished, and when we were done. Our role was that of a good listener in a decidedly one-sided conversation. Our only intervention was to guide speakers back to the topic when they occasionally wandered off too far. We made every effort not to interrupt. As interested listeners, however, we asked our interviewees, whenever it seemed appropriate, to expand on their points, explain what they meant, spell out the implications of examples they gave, and give examples of generalizations they made. We also made note, either mentally during the actual interview or listening to it before the next, of comments dropped, key terms or phrases used, and paralinguistic, kinetic, and other clues that there might be more to tell; then, at appropriate junctures in the same or later interviews, we brought the conversation back to these topics. The quotation marks around "interviews" and "interviewees" at the beginning of this paragraph are meant to indicate how far from a traditional interview these ended up being; perhaps we need a different and more descriptive name for them, but I haven't been able to think of one.

At the end of each set of these interviews, we did do something more unnatural, taking each interviewee through a checklist we had developed of every aspect of their marriages and marriage in general that any interviewee had ever raised, including items such as pet names couples had for each other, the kinds of birthday gifts they gave each other, and dreams about their spouses interviewees had had. We did this to make sure we

[9] A possible advantage of such a research strategy is that individuals could be asked to write briefer accounts—say, of literacy-related activities or memories—circumventing time-consuming interview transcription.

[10] Then sociology graduate student Rebecca Taylor, who interviewed six couples, and Laurie Moore, a then Duke undergraduate, who interviewed one couple. I interviewed the remaining four couples.

were finding out everything each interviewee had to say, and eliciting roughly comparable material from all. That few interviewees had much to add in response to these checklist questions suggests that the approach of letting them organize and run the interviews, and talk as long as they wanted to, succeeded in eliciting from them all that they did have to say, at that time, about marriage.

I say "at that time" because I was very much influenced, in designing the interview process this way, by the manuscript of a book I had read by sociolinguist Charlotte Linde, since published (in 1993) as *Life Stories: The Creation of Coherence*. The *life story*, Linde tells us, is one common kind—though, of course, not the only kind—of narrative people tell in the course of their everyday lives. People do not ordinarily unburden themselves of their life stories all in one sitting; instead, they tell them snippets at a time. Linde's insight is that these stories express people's senses of themselves and are central to their ongoing efforts to create coherence out of their lives. As such, they are always being updated and revised, so that the story one hears at any given time is always provisional. I came to understand that my interviews with people about their marriages tapped into a segment of their life stories at a given time.

Life "stories" are not cast exclusively in narrative form. Indeed, as it emerges particularly sharply in my discussion of the key word "love" later in this chapter, narratives about marriage comprised only a small fraction of our interviewees' discourse, and these were typically the bare frame for the much more extensive commentary on, and explanation about, what had gone on or was going on in their own marriages, other marriages, and American marriage as an institution. (It was, we see, the reasoning in these explanations that I was to capitalize on most heavily in my analysis.) It seems that when given the opportunity to talk about something meaningful to them, Americans not only report their own experience—their own life story—but they also contextualize, compare, reflect upon, and analyze it.

Interviews were each about an hour, a period of time that seemed both ample enough to encourage people to talk freely, and not so long as to tire them out. These interviews were usually held a week apart, and went on, as I indicated earlier, until a given interviewee had nothing left to say—which ranged from a taciturn 11 interviews to a garrulous 28. The thought behind this exhaustive interviewing was that it would yield a body of discourse rich enough for recuperation of the cultural schemas embedded in it. I also wanted to be able to sort cultural understandings from those that came from more individual or subcultural experience, in order to know when I was dealing with which and to be able to explore how the idiosyncratic and the cultural interacted.

A worthwhile side effect of lengthy interviewing was how comfortable interviewees became. If, when they began to be interviewed in my project, "the interview" was a strange experience to them compared to "the coffee klatch" or "the therapy session," it did not remain so. More than one interviewee, near the beginning of the first interview, asked, "Is this what

you want?" Encouraged to define the task for themselves, by the third interview or so all had done so. Not only did they treat it as an occasion for telling part of their life story, but also they made that telling meaningful in different ways. Their ease in turning the anthropologist–informant interview into something more familiar supports Linde's observation that there is no sharp distinction between the interview situation and "so-called real life." Some came to see it as an opportunity to record the history of their marriages, or to make public statements about what these marriages stood for. One of these couples contributed documents from their wedding, including the marriage vows they had written themselves; another interviewee contributed the thesis she had written on the topic of contemporary changes in the institution of marriage. Others viewed this as a chance to reflect on their marriages, as a way of gaining appreciation for them or a therapeutic time for rethinking them. One such couple reported talking over the interviews with each other in between times, and another requested copies of the interview transcripts to study. In this way I collected miles and miles of talk about marriage. All of it was tape-recorded and the tapes transcribed. Of course, the use of tape recorders needed little explanation to these American interviewees. One interviewee, in particular, would very occasionally ask the interviewer to turn off the recorder, when she wanted to tell the her something very private; otherwise, taping never posed an issue nor appeared to be felt as an intrusion.

Once I had a corpus of such discourse, what was I to do with it? This is the point in my research at which I really had to become inventive in my methods, and where I began to develop the organic methodological style I alluded to at the beginning of this chapter. What follows shows how my analyses were invented as I went along. It shows how each of these forays into analysis led me to the next. It shows how each consecutive analysis was opportunistic in the sense of taking advantage of features of the discourse that were at hand and that lent themselves to my research objectives. And, finally, it shows how consecutive analyses added necessary pieces to the final interpretation of my findings.

The Analytic Approach

From the beginning, and unlike various other approaches to discourse analysis, mine was a search for patterns *across* interviewees and passages, that would be evidence of shared, stable understandings. My beginning search for features of the discourse that would reveal such patterns could only be described as groping. I remember, for example, that in early proposals for this research, I wrote that I intended to look at interviewees' use of "aphorisms" about marriage, and their invocation of "imagined scenes" from their marriages and those of other people. These seemed like promising possibilities, in the abstract. I soon abandoned examination of these particular features of talk about marriage in my analysis, as I did

examination of the narratives interviewees told, because none of these cropped up nearly often enough in this talk to make them helpful clues to underlying understandings of it.[11] I gravitated instead to the analysis of features of discourse that did occur frequently.

These turned out to be the *key words* and the *metaphors* in people's talk about marriage, and the *reasoning* that they did about it. In addition to their frequency, these features of talk about marriage that ultimately played major roles in my analysis are, in different ways that will be illustrated in what follows, *culture-laden.* To anticipate briefly and generally, because selection of metaphors, reasoning, and use of key words are all in different ways governed by cultural schemas, each provided an excellent window into the shared schema on which its usage was predicated.

A third important property of metaphors, reasoning, and key words, for my purposes, was that their usage was largely out of speakers' conscious control. Anthropologists have an uncommon nervousness about using what people say as evidence for what they think, as if their words were always bound to mislead us. Of course, the colonized and postcolonized people we have studied have often had good reason to try and mislead us, which may explain one source of our anxiety. The interviewees in the present study may have had various other reasons to mislead their interviewers. They might have wished to, and some certainly were overtly concerned to, put a good public face on their marriages. A few saw themselves and their marriages as exemplary, and the interview task as an opportunity to record and publicize their marital philosophies and accomplishments. Even these few, I should say, became increasingly less guarded and more confiding over the long course of the interviewing. In an important sense, however, interviewees could not have misled us even had they been bent on doing so to the end. Probably all but the most self-critical did represent their marriages as more successful than they really were, themselves as having fewer marital difficulties or resolving these problems more willingly or readily than they actually did. They undoubtedly distorted events to make themselves look good, and omitted others that might have discredited them. But it was not an evaluation of their marriages, or of themselves as spouses, that I was after. It was the framework within which

[11] Ochs and Capps (2001:7) make the point that "informal conversation with those one knows or trusts," such as the spontaneous conversations among family members that they record and analyze, "rather than more formal genres is the medium of choice" for narratives. This may explain why narratives were scanty in my interviews. These authors' larger argument is that, in these informal conversations, people tend to relate events, "not as a tidy narrative package but as incomplete and unresolved." Ochs and Capps (ibid.) argue persuasively that "conversation is the most likely medium for airing unresolved life events," and that "mundane conversational narratives of personal experience constitute the prototype of narrative activity rather than the flawed by-product of more artful and planned narrative discourse" (ibid.:3). Just because they are so unfinished—for example, their plot lines often lack a beginning, middle, and end (ibid.:57)—and so highly indexical, conversational narratives may be of limited use to cultural analysis such as I was attempting, even had I had access to such narratives on the topic of marriage.

they talked about these things. Within this common framework, as I describe more fully in the next section, marriage was a matter of compatibilities and incompatibilities, difficulties and effort, success or failure. There was no other way to talk about it. This framework emerged, willy-nilly rather than as a product of deliberate presentation, from the metaphors they drew upon, the reasoning they did, and their usage of key words. Speakers do, of course, choose particular metaphors deliberately to highlight, for example, the nature and extent of their compatibility (or incompatibility) with their spouses; what they do not and cannot choose is whether to talk about marriage in terms of compatibility, incompatibility, and metaphors for these.

As I have indicated, the premise behind interviewing was that people's talk on a subject is the best available window into its cultural meaning for them.[12] I came to see my analytic approach as the reconstruction, from what people said explicitly, of the implicit assumptions they must have had in mind to say it. My assumption is that the shared understandings I seek lie behind what people said—not, as our folk "Whorfian" theory of language makes us prone to assume, that these are meanings embedded in the words themselves. As is seen in what follows, the systematic analysis of multiple features of discourse that I favor converges on a substratum of cultural understanding underlying this discourse. I did indeed discover a level at which Americans shared a schema for marriage.

Schemas, I have noted, are built up from experience. In part, the shared schema I uncovered is built up from earliest experience and hence taps the deepest meanings marriage has for Americans, meanings that are shared because that early experience is shared. I return to this point when I later discuss my analysis of talk about marital love. At the same time, this schema serves other purposes. It supports internalized "mediating structures" (Hutchins 1995:290–312) or "scaffolding," (Clark 1997:46), that reframe and assist our performance of everyday cognitive tasks; it has evolved and spread in part, presumably, due to the repeated experience of many people who have confronted these tasks (Quinn 1997a). Coordinated with the task world, such structures facilitate actors' performance of these tasks. In my analysis of discourse about marriage, as we see, I identified and described the usage of two such mediating structures. One was the speakers' deliberate selection of metaphors from culturally exemplary domains to clarify their intended points about marriage. The second mediating structure

[12] I would not wish to be interpreted as saying, or believing, that talk is the *only* window onto cultural meaning, or that its analysis captures *all* such meaning. This is just the approach I have adopted because I have found it the most fruitful at my disposal. Some anthropologists are unconditionally hostile to the analysis of discourse in general and interview discourse in particular because it connotes, for them, a radical decontextualization from the complexities of actual behavior in real life. This is an old anthropological anxiety. In fact, any and all selections of what to analyze necessarily decontextualize; the only other choice open to us is to present without analysis, and even then we make choices in what we present. I believe that the discourse I have analyzed is a particularly rich segment of actual behavior in real life. I hope that any reader who brings to this chapter a categorical distrust of interviews will suspend judgment until they have seen for themselves how much can be learned from such materials.

assisted speakers in reasoning about marriage. I describe each of these more fully in later sections.[13]

These mediating structures are unreadable from language in any direct way, and otherwise quite transparent, so that their roles in selecting metaphors for marriage and in reasoning about it was previously un-guessed-at and totally surprising, and this is so even though we all rely on them all the time. It took some time for me to realize what they were and how they were being used by speakers. Philosopher Andy Clark (1997:80–81, 92) tells us how tricky, methodologically, it is to discover the kinds of content-bearing, jerry-rigged, unexpected task solutions that organisms evolve, let alone to figure out how these work. Of course, it is a good deal easier to identify them and figure them out when they are tangible and hence observable. Anthropologist Edwin Hutchins, for example, has studied ship navigation, demonstrating the way in which, and extent to which, the structures that mediate navigation tasks are embodied in physical artifacts and practices distributed across people, outside of individual minds. This demonstration is founded on meticulous, hard-won dissections of task performance and descriptions of the task world that surrounds and enables that performance. Even so, Hutchins (1996:67) has written about his choice of ship navigation as an object of study, how fortunate he was that "many of the resources available to the participants are directly observable by the researcher" as well, making "the analysis of the use of those resources much easier than it would otherwise be." He suggests that, more generally, "settings where problems and their solutions have been crystallized in physical artifacts are simply easier to study than settings that lack that kind of structure," and he recommends that we "tackle the methodologically easy cases first" when doing theoretical exploration.

Perhaps. Without diminishing Hutchins' achievement and its importance, I would defend my choice of a research domain at the nexus of ideationally dense, culturally salient, psychologically laden understandings. I believe it is critical to psychological anthropological theory that we do not shy away from investigating such domains of experience, but, instead, develop methods for tackling them. Like the one I studied, many culturally, psychologically, and theoretically important domains are likely to be ones about which reasoning and other task performance is conducted by individuals or couples rather than large groups, out of public view much of the time, and unassisted by observable physical artifacts.[14] I am grateful that language afforded me a way, however indirect and imperfect, into people's understandings of marriage. I hope my analysis of this discourse demonstrates the feasibility of analyzing wholly internalized, largely tacit, but culturally shared understandings.

[13] Readers interested in the details of how they work should refer to Quinn (1991, 1996, and 1997a).

[14] Perhaps I might have studied the process of group problem solving in marital therapy sessions. However, I was intent on investigating people's ordinary, everyday understandings

I have said that the method of my parent school relied for its convincingness on its ability to produce analyses that led from specifiable operations to unique solutions. In the absence of such a formal method supplying such interpretive determinacy, I sought, in my analysis, another kind of convincingness. I relied on demonstrations that common patterns lay beneath considerable linguistic complexity, and that separate analyses of distinct linguistic features converged on these patterns. When one is able to reconstruct the same structure from the talk of different people, then this is evidence that they share the understandings embodied in that structure—that these understandings are cultural. When these speakers repeatedly, in different linguistic forms, express these shared understandings, this argues for their relative centrality and stability. One can have all the more confidence in the centrality and stability of these shared understandings when they are, as I have said these understandings are, implicit and hence not deliberately manipulable or readily suppressed.

I now attempt to demonstrate the method by which I reconstructed cultural understandings of marriage from discourse about it. I cannot, of course, recapitulate my entire analysis. Instead, I present some bits of it. Importantly, though, my presentation of these will preserve two things about the analytic process itself. First, I try to convey how opportunistic the analysis was, in exploiting what discourse revealed as these revelations were encountered. Second, I keep the order in which I devised the analysis itself, to show how one thing truly did lead to another, and how integral to the analysis was this process of working forward, from past patterns discovered to the next analytic move which, if it was not directly entailed by the last discovery, at least would not have suggested itself at an earlier point in the analysis.

I show, first, how regularities in metaphors for marriage provided the first evidence of a cultural model that interviewees shared. I go on to show how that provisional model led me next to an examination of interviewees' reasoning about marriage, and how that reasoning filled in the shared model, setting it in motion and suggesting how speakers used it to reason with. Finally, I describe how a separate analysis of the key word, "love," added a new, motivational, level to the analysis.

Doing the Analysis: Metaphor

A striking early discovery was that the metaphors different speakers used to talk about marriage in varied contexts fell into just eight classes. These

---

of marriage. Perusal of marital therapy manuals convinced me that this was an expert domain having, not only its own specialized language, but also its own goals and concerns. (Of course, not inconsiderable therapeutic language and thinking about marriage have crept into ordinary, everyday ideas and talk about it, but this is another matter.) As I have indicated, another consideration was inaccessibility, which also made it seem impractical to try and study occasions on which married couples talked about their marriages alone together.

were metaphors of *lastingness*, *sharedness*, *(mutual) benefit*, *compatibility*, *difficulty*, *effort*, *success (or failure)*, and *risk*. Within each class, marriage can be and is portrayed metaphorically as lasting or not lasting (although lastingness is the expectation and marriages that do not last are regarded as unfortunate), as being more or less difficult (although some degree of difficulty is expected), as succeeding or failing, and so forth. Here are some examples, chosen for their brevity, of each metaphor class;[15] in these cases, speakers refer to their own marriages and spouses, compare their marriages with others they know, and speak about marriage hypothetically:

lastingness: "To have that **bond between us.** I think he felt that once we had a child **we wouldn't split as easily**" [3W–4].
sharedness: "[O]ur existence is so **intertwined**" [9H–7].
(mutual) benefit: "But I feel pretty mutual about, we both have **as much at stake** in the relationship as the other person does" [4W–7].
compatibility: "We've scarred each other, and we've helped each other, and we've kind of **meshed in a lot of ways**" [4H–11].
difficulty: "[O]ver the years we've bit by bit **negotiated our way through the rough spots**" [7W–5].
effort: "[T]hey were different issues that were being **worked on** those marriages than in ours, I think" [5W–7].
success (or failure): "[*referring to circumstances that might lead to divorce*] [I]f you're **in a no-win situation**, you've got to take the best door out" [10W–8].
risk: "[*When you get married*] you're **playing the odds**; you're **playing percentages**. You're **betting** that the great majority of the time with that certain person that you will enjoy being there" [7H–2].

Metaphors such as these are mappings from some source domain (the domain of things that are durably joined together by virtue of being bonded; the domain of things that are inseparable by virtue of being intertwined; the domain of economic investment in which one might find oneself having much at stake; the domain of machinery with its meshing parts, and so forth) onto some target domain (in this case, marriage). Of course, interviewees can and do talk about marriage, as any subject, nonmetaphorically. They said things like "You have decided that this is a person that you are going to exert yourself to spend your life with" (10W–10) to indicate their shared expectation that marriage is a lasting arrangement; or conversely, "I'm a firm believer in divorce if things are not going well" (7W–6) to convey the expectation, also shared by interviewees, that a marriage in difficulty can be expected to end. Hypothetically, then, I might have reconstructed the shared schema for marriage from such statements. Certainly explicit statements of this kind were useful to my analysis. What were the methodological advantages of relying primarily on metaphors? There were three such advantages.

[15] Other examples and, in particular, multiple examples of the metaphors for marital lastingness, are provided in other publications, especially Quinn (1987, 1991, and 1997a).

Advantages of Metaphor Analysis

First, metaphors are frequent in speech. Because speakers take for granted its lastingness, benefit, and the other expectations they have about marriage, they do not often articulate these expectations as explicitly as they are stated in the two examples given in the last paragraph. Instead, these assumptions arise implicitly, as called upon in the course of interviewees' reasoning and other talk. Much more commonly than they made such announcements as "I'm a firm believer in divorce if things are not going well," interviewees made the same argument about the relation between marital lastingness (and success) and marital difficulty metaphorically.[16] They produce such metaphor-ridden discourse as "It's a rough time, I think, for marriages to make it" [5W–1]; or, "We are always surprised when we find out that finances and stuff is a point of contention really driving a wedge between people" [10H–3]; or, "It seemed we had weathered it and that at least gave us some precedent for staying together" [5H–5]; or, "It's a matter if you can deal with being hurt and move on, you know, and sort of be able to hang on to each other" [2H–8]; or, "I would make it clear that something's got to be done and I can't cope with it as it is and I'm getting out until you figure out whether you can cope with it or not" [10W–8]. Indeed, it appears that we are unable to talk for long on any topic without speaking of it metaphorically. My analysis exploited the relative frequency of such metaphors in my corpus of discourse.

Secondly, metaphors in speech are like flags waving, or Xs that mark the spot. Indeed, as my analysis progressed, the metaphors soon began to pop out at me. If the frequency with which we use metaphors suggests that they have some crucial role to play in our speech, this perceptual saliency effect supplies a clue to what that role is. Metaphors, I have elsewhere argued (Quinn 1997a), are used by speakers to clarify the points they are trying to get across to listeners. For this purpose speakers choose metaphors that are cultural exemplars of the point being made. A speaker can reasonably assume that such a cultural exemplar will be well known to listeners, who will not only readily apprehend the metaphor, but also readily understand the point the speaker intends to make with it. In other words, metaphors are particularly salient intersubjectively shared examples of what they stand for; it is for this reason that I call them *culture-laden*. That is how they do their work—and that is also how they help an analyst do hers.

The final methodological advantage of examining metaphors was that they gave me a convenient way of knowing that my analysis was comprehensive. All (with only a handful of possible exceptions)[17] of the

[16] Whether a phrase such as "if things are not going well" should be treated as metaphorical or nonmetaphorical is considered at the end of this section.

[17] I identified, in all, fifteen possible exceptions, six of which came from the same speaker—a man with a penchant, more generally, for novel metaphor creation. I say "possible": It is hard to say how many, if any, of these fifteen metaphorical usages stand as true exceptions because virtually all are open to interpretations that explain them away or stretch them to fit into one of the eight classes.

over four hundred metaphors for marriage that I analyzed fell into one or more of the eight classes I had identified. From this finding, I deduced that the metaphors captured a shared schema for marriage, each class of metaphors representing a key concept in this schema. It does not stand to reason that some shared concepts speakers had about marriage would be

---

Two of these seemingly aberrant metaphors, for example, seem to be referring to marriage as something that is expected to "evolve" and not become "static." One wife complains about some "other people's marriages" that "they haven't evolved or they haven't—I mean they're still operating like they did day one" [8W–5]. And a wife says about her own marriage that "If it gets static in our relationship then that's when we'll split, I guess . . ." Certainly interviewees also talk nonmetaphorically about how their own marriages, in particular, change over the course of time. Perhaps the evolution of marriage is a minor theme that informs Americans' understandings of both how married couples learn, over time, to cope with inevitable marital difficulties, and how individual spouses change over the course of a marriage, developing new needs the meeting of which engenders new marital challenges and, sometimes, difficulties. Interviewees influenced by the growth psychology of the sixties and seventies, like the wife just quoted, may view change in marriage as not only inevitable but salutary—perhaps even one of the benefits of a marriage.

Other odd-seeming metaphors, upon closer examination, prove to fall into line with the larger analysis rather than introducing new, if minor, themes into it. For example, the following pair of metaphors for marriage as something "shiny" or in need of "spicing up" may appear, on first encounter, to reflect a previously unidentified expectation that marriage be novel and exciting. Examination of the contexts in which they occur, however, shows that these metaphors are being used to emphasize the *breach* of familiar expectations about marriage.

In the first of these cases, a wife remembers standing and ironing her husband's shirts in the first few months of her marriage and wondering,

> 5W–13: "Oh, is this what it's about?" But still it was shiny and fun enough and we were going out and meeting new people and all that kind of stuff and I was having fun setting up homemaking kinds of habits, so that I don't think that I allowed myself to think very much about whether I was happy or not.

On first consideration, it seems decidedly odd to describe one's marriage as "shiny" (and "fun"). However, context reveals that a "shiny and fun enough" marriage is being retrospectively critiqued by contrast to one that would make her happy, as a marriage should. Happiness in marriage comes about as the result of marital benefit, and to say that one is happy (or unhappy) in one's marriage or has a happy (or unhappy) marriage are common ways of talking about the expectation that the marriage be beneficial. This woman's "shiny, fun enough" marriage was not, if she had actually allowed herself to think about it, a happy, beneficial, one. Another passage from the same interview strengthens this interpretation. In another odd-seeming way of talking about marriage, in the second passage, this wife describes thinking that hers was "nice." She then observes that she may have been repressing her worries about her marriage, and goes on to report that, after seeing other couples with children, she began to think, " 'Oh well that's part of what makes marriages good too. It's about time that we do that.' And we did." [5W–13]. The word "nice," then, like "shiny and fun enough," reflected both the papering over of early worries, and the superficiality of her understanding of the marriage during this early stage. The marriage was "nice," but not yet "good"—the latter a common shorthand for describing a beneficial marriage. Just as a "shiny, fun" marriage is counterposed to a "happy" one in the first passage, in the second a "nice" marriage is counterposed to a "good" one.

Not dissimilarly, a husband notes that his wife has never had to "come to the door in cellophane," an infamous recommendation from Marabel Morgan's book, *The Total Woman*;

routinely expressed in metaphor, while others of these concepts would not. Therefore, when I had exhaustively enumerated and classified all the metaphors for marriage, I felt confident that I had discovered the major pieces of the puzzle I was putting together. Other scholars who have looked at my material and at other metaphors for marriage have never found cause to challenge this finding. These facts convince me, and I hope, also, will convince readers that I have identified all the important components of a cultural schema of marriage that Americans share. (This claim to the exhaustiveness of an analysis based on metaphor has its limits, however, as will emerge in a final section of this chapter.)

Finding Metaphors in Discourse

I turn to some actual analysis. Here I try to show how one would actually go about identifying and classifying metaphors in discourse. I do so using examples that, so far as possible, I have not published elsewhere, or, in the case of those few that have been published, I have not analyzed for the same purpose before. These cases will also provide readers with an opportunity to try their own hands at identifying and classifying actual metaphors as these occur in actual discourse. A handicap under which my demonstration labors, however, is that the metaphors may not jump out at the reader, at first, in the way I have told that they came to be so salient to me. At the same time that metaphors have a certain perceptual saliency, this saliency must be primed; in the ordinary course of using and hearing them, they recede into the background, along with much of the rest of our linguistic apparatus, to allow us to proceed smoothly and expeditiously with the business of speech production and comprehension. See if you can identify the metaphors in the following passages and then, perhaps, begin to notice metaphors for marriage in other talk that you encounter.

In this next passage, a husband is explaining what has been good about his marriage:

> 6H–4: I think that we were so different, and we had such complementary differences that our weaknesses—that both our weaknesses were such that the other person could fill in. And that quickly became apparent to us, that if we wanted to not deride the other person for their weaknesses, we would instead get their strengths in return. And that's what I think has been the asset—these are the assets that have been very good for us. And I suppose what that means is that we have both looked into the other person and found their best parts and used those parts to make the relationship gel, and make the relationship complete.

---

nor has his wife resorted to any such books that were popular in the seventies, and that tell you, according to this man, "Here's what to do to spice up your marriage." He goes on to say that "[W]e don't need that either. You know, that's for "a marriage that's troubled" [6H–9]. So, once again, the anomalous-seeming idea of "spicing up" is revealed to be this man's metaphor for what a marriage should *not* need—and an indication of marital difficulty.

Three classes of metaphor are represented in this brief excerpt. The first, and the one that can be said to be the passage's major theme, is a metaphor of *compatibility*. As interviewees talk about it, compatibility has a fairly complex folk social psychology with several aspects: primarily, the ability of each spouse to meet the needs of the other so that both will be fulfilled and hence benefited; but also, the capacity of each to change in order to do so; and sometimes, too, the willingness of each to overlook the incapacity of the other to meet certain needs and to stress, instead, those needs that do get met and the degree to which the relationship is fulfilling in balance. Interviewees may also stress the ways in which they and their spouses are compatible in the sense of being alike in crucial respects, so that they need and want the same things and hence work toward the same goals. Or, as in this case, they may stress the ways in which the two of them are complementary in crucial respects, so that they can compensate for each other's shortcomings and together fashion a viable relationship. This last sense of compatibility is captured in the initial metaphor of the passage, the idea that the other person "could fill in." It is iterated in a different metaphor of finding and using the "parts" of each of them to make a relationship that both "gels" and is "complete." The first of these metaphors conjures up for me the two chemical components in something like epoxy glue, that together make the glue harden. The second metaphor puts me in mind of the cannibalization of two old, broken machines to put together a working one. The complementarity of both the chemical components and the machine parts stand for the couple's compatibility.

The second metaphor class represented in the excerpt is *mutual benefit*. Mutual benefit is introduced, first, in the hint of an exchange metaphor: each person filling in for the other person's weaknesses would "get their strengths in return," where the two spouses' compensating strengths are the benefit that is being exchanged. These returns are then characterized in a further metaphor of mutual benefit, as "the assets that have been very good for us." The final metaphor class in the passage, *lastingness*, is represented by the comment about "their best parts" making "the relationship gel." Metaphors are capable of multiple entailments, and are not infrequently chosen precisely because these entailments allow the speaker to capture several aspects of a cultural schema at once (Quinn 1997a). Such metaphors may bear assignment to two or more metaphor classes. The metaphor of best parts that gel is an example: It can be said to capture benefit and lastingness as well as, we have already seen, compatibility. Benefit is expressed in the idea that these are the "best parts" of each spouse, and hence useful ones. That the resulting chemical compound is a "gel" allows the metaphor to be stretched to make the point that a compatible, beneficial marriage will last.

Of course, I had to find and sort many more metaphors than the few that appear in this passage. At the same time, I was able to cross-check my analysis against all these cases of metaphor. A further necessary limitation

of my demonstration here (besides the possibility that readers will not be as keyed as I am to notice the metaphors embedded in discourse) is that I cannot replicate, and readers cannot undertake, the full process by which I derived my analysis and was able to verify it. I go as far as I can in describing and illustrating this process.

How does one go about identifying the classes into which a set of such metaphors falls, and assigning these metaphors to their appropriate classes? I first identified all the metaphors I could locate in the transcripts from the first eight hours of interviews. I typed (using an old-fashioned typewriter; this was the early eighties) the excerpts onto three-by-five index cards (which I still have occasion to consult). The typing chore itself unexpectedly became a part of the analytic process because it overfamiliarized me with the material—to a point at which I could recall like metaphors and even recite whole interview lines. (Because I often returned to the original tape to verify particular words or phrases, for a long time afterward interviewees' voices ran around in my head.) Then I did a great deal of examining, shuffling, thinking, and reshuffling of these cards. The analysis ultimately "fell out." Of course, once I had noticed one or two metaphor classes, I was alerted to others. The truth is that, at first, I missed one of these metaphor classes altogether—compatibility—because there were relatively fewer instances of metaphors for compatibility than for some other metaphor classes[18] and because, like the case of the spouses who used their "best parts" to make a marriage that "gelled" and was "complete"—a metaphor that stands simultaneously for compatibility, benefit, and lastingness—a substantial proportion of these metaphors for compatibility had other meanings, had already been assigned to other classes, and did not call out, in any obvious way, for further analysis. Readers with long memories will recall that compatibility was altogether missing from the earliest publication (Quinn 1987) in which I analyzed the metaphors for marriage.[19]

[18] The roughly 400 metaphors I analyzed fell unequally into the 8 classes—ranging from 70 to 80 for *lastingness*, and a nearly equal number for *mutual benefit*, to 15 or 20 for *compatibility* and a slightly smaller number for *risk*. The undoubted reason for this variation in frequency is that, as we see in the next section, marital lastingness is the central conclusion to which speakers reason when they consider marital problems or dilemmas, and benefit an immediate cause of lastingness, while compatibility and risk are more distant causes in this chain of reasoning. When people want to explain why a marriage didn't last, for example, they have only to assert that it wasn't beneficial, for us to infer that the couple was not compatible; or that it faced difficulties and so it failed, for us to imagine that these difficulties posed a risk of failure while the marriage was still ongoing. These more remote links in the causal chain only receive mention when a speaker is concerned to convey in particular detail why some marriage may have succeeded and lasted, or not.

[19] In the 1987 analysis, as well, the class of metaphors for marital "sharedness" was labeled, instead, "marriage is joint." And the missing class of metaphors for compatibility was replaced by a class labeled "marriage is unknown at the outset." I have more to say, at the end of this section, about the decision to exclude metaphors for marriage as unknown at the outset.

Metaphors of Marital Benefit

I think I can give readers at least a better sense of this process of identification and classification, finally, by describing the full range of metaphors I found for one analytic class. I demonstrate with metaphors of marital benefit. Analysis of this class shows how cultural exemplars of a given concept become favorite sources of metaphors for it. It is interesting to see what source domains Americans draw upon for metaphors of benefit, lastingness, difficulty, and so forth—the domains that, in our minds, are exemplary of each of these aspects of experience. In the case of mutual benefit, the favored cultural exemplar appears to be valued resources. Note, however, that identification of culturally exemplary *source* domains is not the primary point of the present analysis. Its point is to use these metaphors to identify all the classes of metaphors that speakers use to describe the *target* domain of marriage.

Some of interviewees' metaphors for valued resources—like the ones about the "assets" of marriage and what spouses "get in return" in the passage quoted earlier, and the one about both spouses having "as much at stake in the relationship" introduced at the beginning of this section—have an economistic flavor to them. These metaphors, and the ones below, suggest that economic exchange is a prime exemplar, for Americans, of mutual benefit. Thus one man said of his wife, "She's a great asset to me in my life, in dealing with my problems" (3H–2). Said a woman, "I'm scared it's going to cost me too much and leave me without being able to stay in the relationship" [4W–12]. Similarly, other interviewees said such things as, "You don't feel that you're being short-changed in this relationship" (7H–5); or spoke of "how much you have to give of yourself and feel like you're giving up and trading off" (5W–1). Another interviewee thought that it might be time to divorce "when the effort is more than the reward" [7W–6]. Similarly, a man reported that his wife "talks about marriage as some sort of reward" for prior time she spent in a religious order, serving humanity; her husband went on to say that this woman thought that "God was repaying her by giving her a good marriage" [9H–1]. Some of these metaphors, of being "short-changed," and "trading off," and "repaying," make especially plain the economic calculus being invoked.

But valued resources more generally, not just those with a market value, serve American speakers as cultural exemplars of benefit, and hence a source of metaphors for the benefits of marriage. This is illustrated by such comments as, "We have a very good thing together" [3W–14]; or "[*Marriage*] is something that I really hold as a treasure" [11W–16]. Others alluded in abstract terms to marriage and "what we hope to get from it and give to it" [7W–1]; or to "what we did or didn't want in our marriages" [5W–13]; or made observations such as, "There was no alternative, we were just married and you had made your choice and that was it. So you had to make the best of what you had" [6W–4]—resources that, while perhaps not optimal, can be converted into a successful marriage. Like the man who referred to his wife as an "asset," interviewees sometimes also spoke

of spouses themselves as valued resources: "He's become everything that I've always wanted" [7W–2]; or "I would tend to get very morose and gloomy and, you know, it's just a really great find to find somebody that could pull me out of that" [4H–6]. By extension of this metaphor, these valued resources were useful, productive, and irreplaceable: in the words of the husband quoted earlier, "[W]e have both looked into the other person and found their best parts and used those parts to make the relationship gel"; or, as other interviewees said, "Why in the world would you want to stop and not get the use out of all the years you've already spent together" [4W–3]; "What rate of positive experience do you have to have before a marriage stops being a productive one?" [7H–2]; "I couldn't find a replacement. I couldn't find another woman to replace Beth" (3H–2).

Interviewees also talked of marriages as resources of special value to particular people: as "a nice place to hide, if you wanted to hide" (9W–3) from a stressful work world as the speaker felt some women did. Spouses, in a parallel way, could be considered strategic resources. One woman reported that she thought of her husband as "an oasis" (2W–3) where, if one had been hurt by relationships as had this speaker, one could feel unthreatened, comfortable, and safe. A man observed that, after she finished college, his wife was "just sort of floating," and "looking for something stable"; he described how "I sort of provided a touchstone for her in terms of having something that she can rely on" who was stable and predictable (7H–7). Another told of how "I was predisposed to be out there like a kite floating over the earth, you know, the string has been cut or something like that" and how he "would have floated away . . . had not Nan—Nan was the string that held on to me" (4H–11). It can be noted that these metaphors of husbands who are their wives' oasis and touchstone, and the wife who is her husband's metaphorical kite string, are susceptible to an additional interpretation. These spouses are all highly compatible with their spouses' particular needs.

Once one has identified a domain that predominates as a source of given metaphors, in the way the domain of valued resources predominates as a source of metaphors for marital benefit, it becomes easier to notice and classify further metaphors that draw on this same source. However, valued resources and their use, production, and exchange are not the only choices open to Americans who wish to speak metaphorically about the benefits of marriage. That this is so is revealed by two cases of another metaphor that I found, of marital benefits as desired destinations. Thus, speaking of marriage, one husband observed, "And it can, you know, be upwards or downwards, I guess, or you know, you can go to some place that you'd like to be at or you can not" (4H–2). And a wife remarked, "It could have gone in so many different directions and that it didn't is incredible. But I think both of us take a whole lot of credit for the direction it went in, that we worked at this really hard" (5W–1).[20] However, these

[20] Note that the "oasis" categorized above as a valued resource might arguably be reconceptualized as a desired destination. But what distinguishes the last two cases from that of the "oasis" is that both of the latter emphasize that benefit is an outcome of travel or some kind

latter metaphors assume minor status beside those of valued resources that constitute the vast bulk of metaphors for marital benefits. This pattern of usage makes sense given the great emphasis put on commodities and other things, and on the value of things, in our society. One can imagine other cultural worlds in which desired destinations or some other kind of beneficial outcome—perhaps even the benefits of marriage itself and other social relationships—would play a much larger role in metaphors for benefit.

Indeterminacies of Metaphor Analysis

We have already encountered one complication of metaphor analysis: the fact that some metaphors, like that of the marriage made up of the best parts of each spouse, may belong in more than one metaphor class. In such cases an analyst must be alert not to overlook additional metaphorical meanings, as I initially overlooked metaphors of compatibility. Other common pitfalls of metaphor analysis bear noting.[21] In particular, (1) some metaphors are used by speakers as metaphors of something other than the target domain under analysis—a fine distinction that may not always be easy to make; (2) some usages of metaphor are so sketchy and abbreviated that the metaphorical meaning the speaker intended is left uncertain; and (3) some metaphors are either so entirely conventional, or such in-built elements of the syntax of the language,[22] that they are likely not being deliberately selected and intended metaphorically at all. I take these up in turn.

1. Does the metaphor belong in the analysis? Consider the case of metaphors such as that contained in the comment, "People really do go into marriage with their eyes closed," [4W–1] from a passage I analyzed in 1987. In that analysis, I classified this as an example of "marriage is unknown at the outset," a class which I dropped from subsequent descriptions of the cultural schema for marriage (see fn. 19). I did so because I decided that such metaphors were not actually metaphors for marriage itself; rather, they described the way in which people characteristically *entered* marriage. People go into marriage unknowingly—"with their eyes closed," or, as another interviewee said about his own marriage, "We didn't really have any idea what we were getting into" [6H–4]; and this is tied to the understanding that they go into marriage precipitously—"And Sue really did jump right

---

of directional movement. As a result, both are amenable to the implication, which both speakers seem to want to highlight, that benefit requires effort ("hard work") to overcome difficulty ("upwards or downwards"). We have already seen that a given metaphor may have multiple entailments. Speakers may choose to highlight or ignore a given entailment, depending upon their purpose of the moment.

[21] I owe this subsection to the insistence of Steven Bialostok (personal communication) that I address these "nuts-and-bolts" problems of analysis.

[22] See Quinn 1999 for a discussion of the latter cases.

out of one and into the other. She really didn't have much of—any time. Maybe six months. Or maybe a year in between the breakup of her first marriage and marrying Greg" [5W–9]; or, "I don't think either one of us ever consciously thought about marriage. I mean we never talked about it . . . It was just something that we both kind of fell into together" [7W–1]. It is true that spouses' characteristic lack of preparation for marriage has a role to play in our expectations about how marriage will go—it is a large factor in the inevitability of marital incompatibility and hence difficulty.[23] Thus, when we ask Americans why married couples turn out to be incompatible, we are likely to hear, chief among reasons, that they didn't know each other well enough when they got married. In effect, I chose to regard this understanding about how people marry as a discrete schema that was *linked to* the schema for marriage. But schemas are only ever relatively discrete complexes of understandings; they are always linked to other such complexes. Therefore the question of whether to have included "people really do go into marriage with their eyes closed" and other metaphors of its class as part of my analysis of the schema for marriage was ultimately a matter, not of principle, but of pragmatism—of diminishing analytic returns. Its inclusion would have added detail and complexity to the analysis but not altered it; its exclusion did not invalidate it. Moreover, the decision to exclude it is not irreversible. All analysts of cultural schemas will face such decisions; but they should not anguish unduly over them.

2. Is the meaning the analyst assigns to a metaphor the actual meaning intended by the speaker? Consider a subset of the metaphors I listed as examples of marital benefit: "what we hope to get from it and give to it"; "what we did or didn't want in our marriages"; and "we were just married and you had made your choice and that was it. So you had to make the best of what you had." Do speakers really intend these as metaphors of resources deployed in making a marriage beneficial, as I have labeled them? It is hard to know for sure, because the speakers are vague about what these benefits are. The only clue we have to go on is the verbal constructions in these sentences: These verbs are used, typically, to talk about resources of value to those who get, give, want, have, and make the best of them. Whether to count these relatively underdeveloped metaphors (and many other examples could be given) as standing for marital benefit is, once again, an analyst's judgment call. Some may decide that the case of "what we want in" marriage is less explicit, and hence less clearly metaphorical, than that of "what we get from and give to it"; what we "want," after all, is so general, applicable to far more than what we value. Others may accept the "want" example but draw

[23] Thus, interviewees can say about their experience as newlyweds, "I think also it raised for me kind of the whole idea that I really didn't know who she was very much" [5H–4]; or, "[W]e were relying on the kind of looking at each other and saying, 'Well,' you know, 'Who are you?' " [6H–2]. The precipitousness with which Americans fall in love and marry, and hence their lack of preparation for marriage, is only one source of marital incompatibility and difficulty. People can marry for the wrong reasons, for example, or they can change in such a way as to grow apart after they marry.

the line at "make the best of what you had" that seems to stress, not the value, but the limitation, of marital resources. I am comfortable in including all three instances because they fit the larger pattern established by many other, more obvious metaphors for marital benefit. Obviously, I would not determine this pattern from these cases alone.

3. Do speakers intend given statements metaphorically at all? There are many cases of metaphors that speakers use out of linguistic habit or convenience rather than select intentionally, to make a deliberate point. Some of these are usages of syntactic forms that may have originated in metaphor, but have become incorporated into syntax so that we no longer even recognize their metaphorical meanings. An example comes from the assertion examined earlier, "I'm a firm believer in divorce if things are not going well." We understand "things not going well" as a reference to unresolved marital difficulties. Are we to treat this as a metaphor casting these interpersonal difficulties as a hardship-beset passage over some physical course? The speaker may not intend any such thing; she may just be drawing upon a construction with the verb *go*—we can also speak, for example, of something going fast or slowly; smoothly or not; as expected or not; and so forth—available for talking about the progress of an undertaking. (Or, if asked, she might report that she was indeed thinking of the hardships of a physical journey, in which case this instance would be better understood, not as unintended as a metaphor, but as an underdeveloped metaphor like those considered under 2, above. I am guessing that this one was unintended; but we cannot know for sure.) This case, of things not going well, contrasts with others in which speakers clearly do intend a metaphorical meaning, as does the interviewee quoted earlier as saying "[O]ver the years we've bit by bit negotiated our way through the rough spots"; or another, reflecting on his difficult marriage, who remarked, "[H]owever long and stony a road it was we had agreed to set out on it" [4H–7]; or another, considering what he would do if his wife was persistently unfaithful: "I'd just say, 'Let me off. Stop the boat I get out here. Carry on with your love-life elsewhere' " [1H–13].

Also unintended are usages of what are called conventional metaphors, metaphors that have been so overused as to have lost their original meaningfulness. A good example of a conventional metaphor from the domain of marriage is the term "couple." We might imagine speakers using this term to make the point that a marriage is shared: The two spouses are "coupled" together (like two railroad cars, if you will). Indeed, one interviewee, talking about how her parents handled the fact that she and her husband-to-be were living together before getting married, makes plain that this is just the meaning she intends to convey:

> 7W-1: And they were wonderful . . . My maiden name is Dalton and with Dusseldorf, my mother just decided she really had to get something better than this Dusseldorf so she named us the Daltondorfs. And John and I were the Daltondorfs and it stuck, a lot of our friends still refer to us as the Daltondorfs. So they really did consider us a couple.

Equally, another interviewee uses the term to emphasize that some friends of theirs were socially recognized as being together even before they were married:

> 6H–2: And I knew them as a couple, even then. You know I always saw them as a couple, even though they really—they weren't in the same relationship that the other couples that we knew, but they ended up that way. What's amazing is how that couple formed the same sort of marriage that the other married couples that we knew already had.

These, of course, are metaphors of sharedness—shared identity in the eyes of others. But most usages of "couple" are not like this. As in "the other married couples that we knew," above, or "I can think of one couple in particular . . ." [8H–7], "couple" has come simply to stand for two people married to each other, without any metaphorical meaning attached to it. Or, as in the next two comments, there is the slightest whiff of intended meaning, a possibility that the speakers, perhaps even unconsciously, chose the term "couple" rather than, say, "marriages" (in the first instance) or "being married" (in the second) because the contexts were ones of "de-coupling":

> 6H–4: Should these couples that break up, as a result of having these things [*marital therapy and encounter groups that focus on the individual*], should they have been married at all?

> 3W–4: The only thing I can think where that [*divorce*] would happen would be where our values and priorities got so far apart that we wouldn't be able to continue as a couple.

I would treat the Dusseldorf example and that of the "couple" who later formed a marriage, as metaphors—though they are not metaphors of marital sharedness; in both cases the point being made is about a shared social identity attained before marriage. However, I do not regard the rest of these examples as intentional metaphors. I excluded all these instances from my analysis. It is important to realize that metaphors that cannot be interpreted, because it is impossible to know for certain what the speaker meant by them within the contextual information given (indeterminacy 2), or even if the speaker intended them metaphorically at all (3), do not constitute analytic exceptions, anomalies, or disproofs. They are simply not very analytically useful. Fortunately, there are plenty of other metaphors to be found that have obvious and unambiguous interpretations.

Doing the Analysis: Reasoning

As I have indicated, in addition to metaphors for benefit, I uncovered metaphors for lastingness, sharedness, compatibility, difficulty, effort, success, and risk. I mulled over this set of metaphor classes for quite a while before finally making any larger sense of it. As happens many times when one is struggling with one's findings, there was a critical moment.

In August 1982 at the annual conference of the Cognitive Science Society, I sat in my hotel room with George Lakoff and Mark Johnson, showing them the analysis of metaphors for marriage that I had completed so far. It was the manuscript of Lakoff and Johnson's (1980) book, *Metaphors We Live By*, that had first led me to the metaphors in my own material. Lakoff and I, especially, after meeting at an earlier conference where he had introduced me to his and Johnson's manuscript (and I learned from him that we were long-lost distant cousins), had embarked upon a long, intermittent discussion of metaphor. Their theory, first developed in that book, was that metaphor was fundamental to thought—that metaphors constituted our understanding and entailed the conclusions to which we reasoned.[24] Lakoff and Johnson were convinced that they could show how lastingness, benefit, and the other categories I had discovered all derived from a single, central metaphor which would prove basic to Americans' understanding of marriage. They tried and they tried, but they couldn't come up with a satisfactory metaphorical analysis. I remember napping (I was jet-lagged) and waking up and finding them still at it. As they wrestled with my material, I became more and more convinced that something other than a central metaphor, something that I remember describing to Lakoff and Johnson as a "cultural story," and that I would now call a cultural schema, underlay and organized the metaphor classes I had found. I also came to believe that my metaphors for marriage posed the challenge they did for Lakoff and Johnson's theory because my data was much more systematic than that which they had typically employed in their analyses. In culling all the metaphors used to describe one domain of experience, from an extensive corpus of discourse on that domain, I had unwittingly invented a new, more rigorous method for collecting metaphors.[25]

What Reasoning Reveals

The story behind these metaphors that I had in mind was a product of intuitions I had, as an American myself, about American marriage. Roughly, I knew, people regarded their marriages as successful if they lasted. In order to last, though, a marriage had to be beneficial, and in order for it to

[24] See Quinn (1991, 1997a) for a critique of this position.

[25] As illustrated in the previous section with the case of metaphors of marital benefits as valued resources, metaphor classes may vary widely in the frequency with which they are drawn upon in ordinary speech. The frequency of their use might depend on, for example, their cultural currency and hence popularity, or the degree to which they may have become conventionalized in language. Without systematic culling of metaphors from actual discourse, it is easy for analysts to miss metaphors that are used with less regularity, and hence to mistake the most frequently used metaphors or frequently drawn-upon metaphor classes for "basic" ones. It is a short next step from this mistake to the position taken by Lakoff, Johnson, and their colleagues, erroneous in my view, that these so-called basic metaphors underlie and constitute the concepts for which they stand. I suspect that such a lack of systematic analysis of metaphors, as these occur in discourse, to have been at the root of this theoretical confusion. See Quinn (1997a:152–153).

be beneficial, its difficulties had to be overcome, requiring effort. How could I verify this story? What kind of evidence would convince others that it existed in Americans' minds, and convince scholars like George Lakoff that it did so independently of the metaphors they used to talk about marriage? It occurred to me at some point to cull out and examine instances of reasoning that interviewees did about marriage, to see if this reasoning conformed to, and supported, my intuitions about what led to what in the cultural story of marriage. Lo and behold, interviewees spelled out exactly the sequence of events I had surmised, not only confirming my "story" but filling in additional pieces of it.

I thought to look for evidence of this story-like cultural schema after I first formulated an idea of it, and I formulated the idea of such a cultural schema for marriage through my prior analysis of metaphors. Conceivably, I could have, or another analyst might have, begun with an examination of reasoning. I believe we would have ultimately arrived at the same analysis. Indeed, analysts working in other languages and cultures than their own who cannot rely so dependably as I did on their own intuitions to kick-start their analyses, may wish to go directly to the reasoning in discourse.[26] In this instance, I had my intuitions to fall back upon and I used them. The methodological lesson is not that a culture member's intuitions are indispensable for analysis of cultural schemas; they are not. The lesson is that every researcher follows her or his own nose, drawing upon any and all sources of inspiration encountered along the way.

On reflection, it is not surprising that their reasoning exposed these reasoners' cultural schema for marriage, because it is this schema that structures this reasoning. Only much later (Quinn 1996), did I come to see that what I had been thinking of as a somewhat disembodied cultural model *of* marriage was better understood as a model *for*—a schema designed for reasoning about marriage, and that had evolved and spread just because it served this purpose well. What this insight suggests is that reasoning about all kinds of widespread, recurrent dilemmas is likely to be similarly *culture-laden*, because organized around such shared structures for performing this everyday cognitive task.[27] Therefore, in the same way that metaphors are windows into shared knowledge of cultural exemplars, reasoning is an especially good analytic window into the shared structure or cultural schema being used to do it. This is a methodological observation

[26] As Edwin Hutchins (1980) did in his elegant analysis of reasoning in Trobriand land disputes. Hutchins focused on reasoning about a circumscribed set of causal relations that arose in the formal arena of land litigation, and he cast his analysis of this reasoning in a language of propositional logic that I think he might disavow today on theoretical grounds; while this logic is a good device for describing the cultural event sequence people reason from, it is a less adequate rendition of the way in which people actually reason (see Quinn 1996, 1997a). This said, Hutchins' book, which I reviewed (Quinn 1982), was a great influence on my thinking at the time of its publication, and I am sure it was a factor, along with the work to be described by Charlotte Linde (1993) on explanation, in directing my attention to the reasoning in the discourse I had collected.

[27] See Quinn (1996, 1997a).

that should hold for reasoning about, as for metaphors used to talk about, a wide variety of research topics.[28]

In the event sequence reconstructed from interviewees' reasoning on the topic, it emerged that *lastingness*, *sharedness*, and *benefit* played the role of prior expectations about marriage. These expectations were not unrelated. A marriage had to be shared in order to be beneficial, given the psychological fulfillment that spouses were expected to afford each other as the chief benefit of marriage—of which we have seen some examples in the hiding place, oasis, shrink, touchstone, and kite string metaphors. And, as I have said, it had to be beneficial in order to last—because twentieth century Americans regard marriage as a contractual relationship, and individuals will not remain in a marriage, as they would not remain in any contractual relationship, that does not benefit them. Lastingness and benefit then, were in potential conflict: A marriage should last, but if it is not beneficial, interviewees reasoned, it should not last. It is this conflict that sets the rest of the cultural story about marriage in motion. In their reasoning, interviewees resolved the potential contradiction between lastingness and benefit in a thoroughly American way: They tried to achieve a beneficial marriage and hence one that would last by overcoming the difficulties that stood in the way of benefit, and they did so by exerting effort. The risk, of course, was risk that despite their best efforts, difficulties would not be overcome, benefits not attained, and the marriage would fail to last. Compatibility came into the story because it was compatibility that insured benefit, incompatibility that posed the difficulties standing in the way of benefit, and the attainment of compatibility—through learning about the spouse's needs, learning how to fulfill them, sacrificing to do so, adapting to changes in their needs, and the like—that required such effort in marriage. In particular, because of the way Americans marry—for love—they almost always (as the wife I quote in Quinn 1987 so eloquently explains) "go into marriage with their eyes closed," without considering how well-equipped each spouse might be to meet the other's needs. So compatibility is not a given; a certain amount of incompatibility and hence difficulty is inevitable.[29] While the account assembled here may seem commonsensical to American readers, its elements and especially the way they are configured are not fully shared by other peoples in other places,

[28] At this point a theoretical caution may be in order. As popular, in recent years, as the idea that metaphor is somehow fundamental to human thought, has been the proposal that narrative (or, at least, "a readiness or predisposition to organize experience into a narrative form," Bruner 1990:45) is. To that claim, whatever its ultimate merit proves to be, my findings here cannot speak. The importance of reasoning in human affairs, and the consequent frequency of cultural schemas in the form of event sequences for reasoning with, like the one I next describe, should not be read as support for an argument that these culturally shared event sequences are somehow fundamental to human thought or privileged by neural wiring. There are many other cognitive tasks that humans perform besides reasoning, and other kinds of cultural schemas designed for the performance of these tasks.

[29] See Quinn (1996) for a discussion of how the cultural schema for marriage allows reasoners to use a simplified causality.

although of course this account is bound to overlap substantially with a parallel account of western European marriage. The product of a particular American history, this schema for marriage is distinctively American.

Analyzing Reasoning in Discourse

The sequence of events I have outlined was embedded in speakers' reasoning about marriage, but reconstructing it from this discourse was far from straightforward. I had first to locate and identify instances of reasoning about marriage. (This task was accomplished by simply scanning and marking the original transcripts, rather than copying the rather lengthy passages of reasoning onto cards.) Reasoning about marriage occurred fairly frequently in my interviews—certainly not as pervasively as metaphors for marriage, but still five or ten times in an average interview. Just as some interviewees were more imaginative metaphor-makers, some were more interested in explaining. Nevertheless, all used metaphors prolifically, and all provided multiple instances of reasoning. But this reasoning was not always easy to find. It stuck out most plainly when it was lengthy and well-formed.

Well-formed reasoning is what Linde (1993:90–94) calls "the discourse unit of explanation"; it begins with a statement of the proposition to be proven, follows with a sequence of statements as to why the proposition should be believed, and ends with a coda reasserting the original proposition. Such was the form of two longish piece of explanation that I have published elsewhere (Quinn 1987 and Quinn 1996, 1997a). Here let me supply a fresh example, of a wife explaining why she doesn't foresee her marriage ending:

> 7W–6: I don't know whether just at some point little things would mount up over the years to the point where one of us couldn't take it anymore but I can't imagine what. And I lose my patience with Tim a lot because he is constantly blaming me for starting arguments and maybe I am. Maybe it's because I am a little bit bored with being around the house sometimes until I find that job. And there are times that he's tired and I'm tired and he comes home from work and expects not to have to be hassled with things at home. And I have something on my mind and he just doesn't want to hear it and I get fed up with that. And I do get tired of always being blamed, having the burden of all our arguments past, present and future on my shoulders but at the same time I don't think something like that would ever be enough to make me pack my bag and go. Although this weekend—I thought in the past about packing my bag, not to leave forever, just to go away for a weekend and he was really—at first he thought I meant that I had thought about leaving him. Just for a weekend. Just to do something for a weekend but I can't think of something that would make me want to leave now or in the future.

The structure of this explanation is relatively simple. The proposition, put forward in the first sentence, is that there is not enough lack of benefit

("little things" wrong) in the marriage for this wife to imagine either she or her husband ever leaving it (i.e., things that would "mount up" to "the point where one of us couldn't take it anymore"). Having characterized these lacks as "little things" that would have to "mount up over the years" to some "point," the interviewee then sets out to demonstrate, by listing these things and considering them individually and collectively, that they do not, in fact, amount to a reason for leaving. She next bolsters her argument with the observation that the problems she has listed have never motivated her to leave. She has wanted to leave—but only for a weekend away and not, as her husband mistakenly thought, forever. Finally, she iterates the original proposition: "But I can't think of something that would make me want to leave [*permanently*] now or in the future." Benefit (posed, in this case, as insufficient lack of benefit) makes a marriage last (or, in this case, keeps it from not lasting). Such well-formed explanations are especially likely in ordinary speech when the speaker stops to provide evidence for some assertion he or she has made. What then seems to motivate a speaker to repeat the original proposition at the end, is a sense that the listener may not have held onto the original point in the course of the longish presentation of evidence.

These well-formed explanations are relatively uncommon, however. Much more typically, interviewees provided shorter fragments of reasoning devoid of supporting evidence; they may have felt their point was too obvious to require such support, or this point might have been a passing one, which they did not wish to stop and defend, on the way to some larger conclusion. Some of this less fully developed reasoning is as short as a single sentence or two, some instances of which were seen in the previous section. Consider: (1) "To have that bond between us. I think he felt that once we had a child we wouldn't split as easily"; (2) "[I]f you're in a no-win situation, you've got to take the best door out"; or, (3) "I'm a firm believer in divorce if things are not going well" (made by the same wife who asserts, in the passage just cited, that the little things in her own marriage will never amount to cause for divorce). As in each of these cases—which can be summarized as (1) sharedness helps to prevent a marriage from not lasting; (2) lack of marital success justifies a marriage not lasting; and (3) difficulty leads to a marriage not lasting—such reasoning typically asserts a causal relation between just two terms of an argument. Sometimes, as we see in the passage to be considered next, relations among three or more terms are introduced in a single piece of reasoning.

Not only are the majority of cases of reasoning fragmentary and relatively undeveloped; when they grow longer than a sentence they are not always particularly orderly. Yet, they are commonly occurring and potentially valuable evidence for the cultural schema that governs them. How does one identify them? As with the case of metaphors, knowing what I was looking for helped me find it; once I had familiarized myself with the reasoning in well-formed examples, and the causal relations governing these, it was easier to recognize the same causal relations between the same

terms of argument in more fragmentary cases. The explicit language of causality in which the terms of arguments were often linked together also helped to signal a piece of reasoning. This language takes many forms, ranging from brief and fairly regular markers of causality such as *once X, not Y*; *if X, Y*; and *X if not Y* illustrated in the three sentences above, to fuller and more variable expressions of causality of the sort found in the longer passage above: *X to the point where* Y, or *I can't think of an X that would make me want to* Y. However, sometimes causality is not so well marked in English, and must be inferred from the order of sentences or clauses.[30] In identifying all these varied expressions of causality, I relied on an eye for them that I developed with practice.

Once a number of instances of reasoning about marriage had been found and collected, their structure had then to be deciphered beneath its metaphorical, causal, and other language. Since I had already done an analysis of metaphors for marriage, decoding these was fairly routine, but the language of causality was a new challenge. Cultural patterning had to be discerned underneath much other linguistic variation and the particularities of given marriages and marital situations. Because speakers leave many basic assumptions implicit, knowing that listeners share these assumptions and fill them in automatically, it was necessary to make this implicit assumptions explicit in the analysis.

I have illustrated the process of reconstruction from reasoning elsewhere (Quinn 1987); here I introduce some new examples in order to give readers some feel for how it is done. Let us begin with the now-familiar passage I earlier used to illustrate the identification of metaphors and their classes. The reasoning in this particular passage is not especially regular or explicit, making it a good illustration of the problems I encountered in my analysis. Can this sequence of reasoning be reconstructed?

> 6H–4: I think that we were so different, and we had such complementary differences that our weaknesses—that both our weaknesses were such that the other person could fill in. And that quickly became apparent to us, that if we wanted to not deride the other person for their weaknesses, we would instead get their strengths in return. And that's what I think has been the asset—these are the assets that have been very good for us. And I suppose what that means is that we have both looked into the other person and found their best parts and used those parts to make the relationship gel, and make the relationship complete.

As we saw when we first examined this passage, the first two sentences simply describe the nature of this couple's compatibility. Reasoning about the consequences of this compatibility for the marriage begins with the speaker's assertion about the assets in this marriage. In "**that's** what I think has been the asset" and "**these** are the assets that have been very good for us," *that* and *these* refer to the couple's compatibility—specifically in

[30] See Quinn (1987, 1991) for examples.

this case their ability to fill in with their strengths for each other's weaknesses—and this compatibility is equated with mutual marital benefit, in "that's what I think **has been** the asset—these **are** the assets." We grasp, although the speaker's equation of compatibility with benefits leaves this understanding implicit, that the equation between the two stands for a causal relation: Compatibility causes benefit. Other instances of reasoning by this and other interviewees make this causal relation plain.

The passage contains two other causal relations. The remainder of the argument is that the couple's compatibility—iterated here, we have already seen, as using the "parts" of each of them to "make the relationship complete"—has caused (here expressed as "used to make") the marriage to last (or "gel"). Moreover, spousal compatibility has been beneficial: These parts are not only complementary to each other, they are the spouses' "best parts"—a reference to their previously mentioned "strengths," the "assets" or benefits of the marriage. That these "best" parts of each stand for mutually beneficial attributes of the two spouses is also clear from the fact that they can be used to make the marriage last. The speaker leaves implicit the final causal link in his argument, that compatibility makes a marriage last *because* compatibility enables marital benefits and these benefits then lead to a lasting marriage. Instead, compatibility and benefit, equated earlier in the passage, are now compressed into the same metaphor of complementary, useful parts. A causal connection between compatibility and benefit is at least implied, however, in the phrase, "And I suppose what that means is . . ." that links the two halves of the passage. In the first half, the speaker has established that spousal compatibility causes mutual marital benefit. In the second half, that compatibility and mutual benefit cause a marriage to last. If, then, compatibility causing benefit *means* that compatibility and benefit cause a marriage to last, it must be because compatibility causes benefit that then causes a marriage to last.

Overlapping Pieces of Reasoning

It is interesting that speakers are able to produce such relatively elliptical, dense, and convoluted reasoning, and listeners can follow it, so readily and rapidly. This must be because both are so well-acquainted with the schema that underlies this reasoning. While the analysis I have provided of this passage may be intuitively convincing to those who share this cultural schema and can fill in its missing parts, this analysis may also seem to rest on fairly fragile and partial evidence. That would be so if it were the only piece of reasoning available. Analysis of multiple pieces of reasoning achieved two things. First, this analysis established that the structure encountered was indeed shared. Second, analysis of multiple instances of reasoning was necessary in order to piece together the cultural schema in its entirety. This was so because, in a given piece of reasoning, reasoners like this last one typically focused on one link at a time, between two terms

in the larger story of how to achieve a successful or lasting marriage. The speaker in the passage just analyzed is focused on what made his wife and him compatible, and how this made their marriage lasting. In so focusing, speakers set aside or leave implicit other terms in the story—as the causal relationship between compatibility and benefit is left somewhat vague in the previous passage. Juxtaposing separate pieces of reasoning provided the overlap between terms that was necessary to reconstruct and verify the whole event sequence that comprised this schema.

1. Let me show first how different pieces of reasoning can be used to corroborate each other. Just as with metaphors for marriage, reasoning about it is highly regular; interviewees repeatedly followed the same causal chain to reach their conclusions. If they did not do so, we would have to conclude that there was no cultural schema for reasoning about the topic under investigation. In the case of marriage, there proved to be such a structure. For illustration, I provide, here, just one additional example each of the two causal links in the relation between spousal compatibility and marital lastingness that we examined in the case of the gelled marriage. The first causal relation is that between compatibility and benefit. Remember that, in the gelled marriage excerpt, it was necessary to infer that compatibility enabled benefit from the assertion that compatibility *was* benefit—"these," the strengths gained from filling in for each other, "were the assets that have been very good for us." In the next excerpt and others, however, this causal link between compatibility and benefit is spelled out. In this excerpt, a husband is reflecting on how things worked out in his marriage:

> 7H–1: I didn't have any long-range understanding of what was going to come, or—I just felt, as I guess we both did, that we'd live things as they came along and make adjustments and be prepared to adjust and change course if necessary and just somehow things would work out. And so far they have, and very satisfactorily.

In my initial outline of the schema that governs Americans' reasoning about marriage, I noted that marital compatibility—the fit of one spouse to the other and, in particular, of each spouse's needs to the other's capacities for fulfilling these—was not automatic. Here the aspect of compatibility that is stressed is the capacity of the two spouses to change in order to become compatible. This is expressed in the two metaphors of making adjustments and changing course, and benefit is expressed as things working out satisfactorily—with a hint, in the metaphor of "working out," of a middle term, difficulty caused by initial incompatibility being overcome and compatibility achieved, for the attainment of a beneficial outcome. Beneficial because satisfactory; here, note, that we fill in some basic folk psychological knowledge, namely, that people's satisfaction is a reliable sign that they feel they have benefited. Causality is expressed, as it often is

in English, by the order of linked clauses: adjust *and* things will work out satisfactorily (with the "just somehow" adding a pinch of blind luck to this formula for marital satisfaction). Spousal compatibility leads to marital benefit.

Next, consider the causal relation between spousal compatibility and marital lastingness. Elsewhere (Quinn 1991) I have published three passages containing reasoning that exemplified this particular link; once again, let me provide a fresh example here. A husband describes the basis for compatibility in his marriage:

> 10H–7: I explained to you how, at least I felt, that in effect we made a good team in regard to that. That we complement each other in handling things with the kids. And we don't have basic disagreement on any kind of principles that have to do with it. And I guess that, you know, that may be the key issue. I married someone that came from a similar background. And I think other things equal, we're more likely to have similar attitudes, criteria as to what's important, what's not so important, standards for this or that. And that's probably been a very important factor. That's probably what—one of the contributing things to make us feel that we had a strong bond between us.

Most of this passage is about the couple's compatibility, and, as the metaphor of teamwork reflects, here the aspect of compatibility that is being stressed is the shared attitudes, priorities, and standards that allow this couple to agree on common goals. There is also the hint of another aspect of compatibility, in the comment about "complement each other in handling things with the kids"; in this domain at least, the couple may have found themselves able to complement the weaknesses of each with the strengths of the other, in the way that the husband with the gelled marriage stressed that his wife and he were able to do. The argument linking compatibility with marital lastingness does not occur until the last sentence in the passage, where "That" is a reference back to the entire previous discussion of their compatibility, and causality is complexly rendered as *X is one of the things to make us feel that Y* (in which *X* stands for similar background and attitudes, and *Y* stands for having a strong bond).

"That," this man says, contributed to the "strong bond between us" that signifies a lasting marriage. One of the common metaphors for marital lastingness is that of such a bond, as in comments like, "We're much more tied to each other now than we were then" [6H–1]; or "That just kind of cements the bond" (3H–2); or "And even though you have a good friend, if something really happens, you're not bound to them like you are when you're married" [6W–8]; or "There's a certain Biblical rightness to bonding together and, you know, still through sickness and health for you and me too by the by, this sort of thing" [5H–9]. As all these examples illustrate, metaphors that picture marriage as a tie or bond between two people carry the further implication that it is shared as well as lasting. Two people bound or tied or bonded together share a common fate, and they share it

for the long run. This latter meaning, of lastingness, is reinforced by adjectives like "strong," adverbs like "much more," and verbs like "cements," as it is in the content of remarks like, "through sickness and health." In saying, "That's probably . . . one of the contributing things to make us feel that we had a strong bond between us," the husband quoted above is concluding that his and his wife's compatibility contributed to their marriage lasting.

2. Second, Let me show how reasoning by different speakers (or by the same speaker on different occasions) makes explicit different parts of the whole causal sequence, filling in pieces that are left implicit or ambiguous elsewhere. We do not have to guess that the husband who says, "And I suppose what this means is . . ." alludes to the under-specified causal link between mutual benefit and lastingness. We find this causal relation made perfectly explicit in a great deal else that this man and others say. For example, a wife makes it in the following interview excerpt (the first part of which was quoted in the previous section):

> 4W–7: But I feel pretty mutual about, we both have as much at stake in the relationship as the other person does. We both express to each other the same desire to keep things going.

Here causality is implicit in the order of the two sentences: *Because* they both benefit from the relationship, listeners understand, they both have (and express) the same desire that it last.

For just one more example of the same argument, the next wife has been trying to convince her husband that an affair she is having does not threaten their marriage:

> 3W–4: Like what I tried to explain to Dan was that one person can't be expected to fulfill everything because they're not exactly the same. You know, fulfill everything that one person needs. And that Ron fulfilled something for me that Dan couldn't, you know. And, it wasn't as much—like Dan fulfills so much for me that I would never want to leave him for Ron, you know. Because Ron just fulfills this one added little block that Dan doesn't. I'm not going to leave thirty for one, you know, that's just—I mean, you know—I mean, I can't put a number on what he fulfills for me, but you know, that kind of ratio.

Fulfillment is the benefit of marriage, and this speaker, believing that one's spouse can not necessarily fulfill every need one has, still argues that the vast proportion of her needs are fulfilled by her spouse, and that therefore she would never leave him. Causality is made clear by the construction, *so much X that not Y*. A beneficial marriage will last.

For icing, let me lastly provide an example which, although somewhat complex linguistically, makes explicit all the links between compatibility, benefit, and lastingness. Readers can test themselves by trying to trace this chain of reasoning before reading my analysis of it. In this passage, a husband is talking about the other significant relationship he had before that with

his wife, and explaining why he ended up marrying his wife instead. This passage was prefaced by the speaker's telling how he had been looking for someone who shared his life philosophy and had the same value system, with whom he could really be friends and have a good time. Of his wife, he says,

3H–2: She fit the general mold I had conceived in my head.
I: Was that a surprise, I mean did you keep being surprised that . . .
H: No. No I more or less—I kind of knew where she was coming from, from early on. I was, you know—I wouldn't have made a commitment to a woman who didn't fit that kind of general image. The other heavy relationship I had with a woman before Beth Ann, that woman didn't fit the mold. She fit some of it, but it was—it was as much a physical attraction as anything else. Though we did—said we loved each other and we felt love. The love with Beth, I feel, is a lot more—it's a lot deeper and a lot—'cause we think alike. This other woman Karla and I didn't always agree, you know, about a lot of things.
I: Mm hmm. And that's why it ended?
H: But we both claimed we loved each other and it ended for all kinds of reasons but it ended, and that was probably for the best. 'Cause I don't think—I don't—I'm pretty sure I wouldn't have been as happy with her as I am with Beth. The type of thing—that whole relationship probably would have ended in divorce, chances are, in a few years.

This excerpt is woven together by a single metaphor, which is introduced in the sentence, "She [*his wife*] fit the general mold I had conceived in my head," then rephrased as "fit that general kind of image," and, finally, repeated in "That woman [*his former girlfriend*] didn't fit the mold." In one sense these comments allude to the image the speaker had in his head, of the kind of woman he wanted to marry, an image that one woman fit—in the sense of matched—while the other woman didn't. As the passage goes on, however, the speaker develops the metaphor in another direction that is well-served by the shift he makes from "fit the image" back to "fit the mold." In this second sense, we understand, the speaker and his first girlfriend would have been incompatible—she would not have met his need[31] for someone who shared the same values, the two of them thinking alike and agreeing about things.

Reasoning about this incompatibility is contained in the passage's last two sentences. Even had not this prior relationship ended when it did, and had this man married his first girlfriend, they would not have been happy.

[31] This man surely understands, as do other interviewees, that *both* spouses must meet *each others'* needs in a marriage. However, because he is focused here on his own side of this reciprocal relationship, and on the moment when he was preoccupied with whether he had found someone who had met *his* needs, the "mold" metaphor serves him well. Speakers intent on emphasizing the reciprocal nature of spousal compatibility are inclined to use different metaphors, such as that of two spouses "meshing" or being "fitting parts" of some larger whole. They also indicate that they appreciate the mutuality of need fulfillment and marital benefit by their use of the plural "we," by metaphors of resource exchange, and by phrases such as, "and vice versa" or "and I for her."

Happiness—like satisfaction, in an earlier passage in which things worked out "very satisfactorily" for another couple—is an emotion people feel when they are fulfilled and their marriages are beneficial. Hence, we understand, the lesser happiness this man speculates he would have felt in a marriage with his former girlfriend, in comparison with his happiness with the woman he did marry, stands for the lesser benefits of the hypothetical marriage compared to the actual one. Because the girlfriend and he were less compatible, their marriage would have been less beneficial. And, he goes on to say, because less beneficial, it would not have lasted but "probably would have ended in divorce." We understand the causal links this speaker is making between a potential spouse not fitting the mold and marital unhappiness, and between this unhappiness and divorce, because we are able to infer this causality, once again, from the order in which he presents these outcomes.

Doing the Analysis: A Key Word

What followed after I had succeeded in reconstructing the shared schema for reasoning about marriage, was another lengthy hesitation in my analytic progress. Indeed, for a long time, I thought my analysis was finished—that I had delineated the American model of marriage in its entirety. I took to heart what one of my own interviewees (the same one who provided us with the piece of reasoning about making marriage "gel," in the passage we analyzed earlier) reported having told a Navy shipmate who was thinking of getting married. From his perspective as a married man, my interviewee warned the other,

> 6H–4: I hope you think about it real hard because I think you might find marriage to be a little bit surprising than what it is. Because it was for me. Shocking sometimes, you know, that it wasn't all love and sex and that's it. Yeah, that there was some work to be done.

That was the lesson that had emerged, so far, from my analysis too. But if it wasn't all love (and sex), any American, knowing firsthand how much we all make of the connection between love and marriage, might well have asked, Where *was* love, and why didn't it appear at all in my analysis of marriage? That is exactly what fellow anthropologist Michael Moffatt asked me, sometime during the year we got to know each other, the 1982–1983 academic year that I was at the Institute for Advanced Study in Princeton, New Jersey, not far from Rutgers where he is on the faculty. I had no immediate response to Moffatt's question, but it provoked me to wonder.

In another way, it made sense to examine usages of the word "love" in my corpus. We might expect that key words such as this one—words that name culturally distinctive concepts or schemas and that arise frequently

in talk about a given domain—will prove to be culture-laden, just as are metaphors and reasoning. Why key words should carry a heavy load of cultural information is obvious. As anthropologists working in other societies and languages than their own have long appreciated, key words arise to permit speakers easy reference to the salient cultural concepts that they mark. Based on anthropologists' experience—even apart from the intuitions of a culture member like Moffatt—the word "love" could be predicted to be an excellent guide to what was salient to Americans about marriage and, hence, what should matter in my analysis of it. Eventually, I did take a systematic look at interviewees' usages of the word "love." I discovered a cultural schema for love, and it was one that did have important and interesting implications for marriage.

Finding Correspondences between Love and Marriage

The analysis of usages of "love" proved to be straightforward. In the first place, key words are easier to find in discourse than are instances of metaphor or reasoning. And usage of this word turned out to be quite regular. Again, as with the analysis of metaphors, my method was standard pattern-seeking: making three-by-five cards again, sorting these cards, puzzling over them, and regrouping them until my cases fell into a set of categories. I found two kinds of detailed correspondences between love and marriage, in the way that interviewees talked about these, that I take to be aspects of a shared schema. The first involves an alignment between marriage as a social status, and love as an emotional state. In the second set of correspondences, the emotion of love instigates certain motivations in people, and these prove to fill in the motivational structure of marriage.[32] Having reported these findings and given illustrations of them elsewhere (Quinn 1997b), I only summarize them here. I then single out one feature of the alignment of love and marriage and one feature of the motivational structure that love provides for marriage, offering a few examples of each of these two pieces of my analysis for readers to follow.

The alignment between love and marriage is readily summarized. Americans know that, as the old song says, "love and marriage go together," and by this they mean that if you love someone, and only if you love them, you should marry them, if you are married to someone you should love them and nobody else, and if you no longer love someone you should end your marriage with them.[33] Here I focus on the first of these injunctions. Three different kinds of discourse proved particularly useful in revealing this expectation that if you fall in love you get married: speakers' tacit

[32] This was not all that interviewees had to say about love and marriage (see Quinn 1997a). Here I set aside these details of the story about love and marriage, confining myself to a demonstration of evidence for the fundamental correspondence between the two.

[33] With violations of this dictum, such as "loveless marriages"—one common variant of this is "staying together for the sake of the children"—being not only anomalous, but unfortunate and even morally questionable in the minds of most Americans.

assumption of it; their attempts to patch up its occasional violation; and narratives of this violation. While I do not know how general the utility of these three features of discourse for uncovering cultural expectations in other domains of experience than this one will prove to be, I believe all three are likely to have some application beyond my own analysis.

The expectation that if you love someone you should marry them was revealed most frequently in interviewees' tacit assumption of it.[34] This expectation has as its corollary that if you marry someone you do so because you love them, and it is this tacit assumption that is revealed in the next tale, by a woman whose husband found a way to tease her about it:

> 1W–5: I'd say, "Bobby, that's not true. Tell them the truth. You married me because you loved me." He'd just laugh and he'd say, "Ah I fooled you," or he'd say, "You didn't see that big wad of money that your dad gave me before we got married?"

Somewhat less frequently, but more strikingly, the expectation that people fall in love and get married was exposed in the way interviewees tried to repair violations of it. The man quoted earlier, who didn't marry his former girlfriend, provides a case in point. This man has a slight dilemma: He and the girlfriend "said we loved each other and we felt love." If so, according to American expectation, they ought to have gotten married. The interviewee resolves the apparent violation of this expectation by recasting the love he and his girlfriend felt and declared: first of all, it was not as deep as the subsequent love between his wife and him proved to be; furthermore, the girlfriend and he, it turned out, only "claimed" they loved each other. Both these disclaimers attest to the fact that what this interviewee and his girlfriend felt was not "true" love. Hence, they were right not to get married.

Finally, this same expectation was revealed in narratives interviewees told, that derived their reportability from the unusual circumstance that people who fell in love ended up not getting married, or, conversely, people ended up marrying who were not in love. Such narratives were infrequent compared to other evidence for the alignment of love with marriage, making them undependable sources of primary evidence for patterns like this relationship between love and marriage. When they do occur, however, they are telling confirmation. Probably, the American cultural emphasis on falling in love makes narratives about people who do so but then do not marry especially reportable. Elsewhere (Quinn 1997b) I gave an example of one such story I found in the discourse I collected; here is the other, an old-fashioned story about the interviewee's grandmother:

> 4W–5: She was very much in love with a young artist who had tuberculosis and went away for a cure and was gone for about three years. And had

[34] For example, the Navy man's comment that "it wasn't all love and sex and that's it" reveals his tacit assumption that married people love each other.

> stopped writing at some point or other. Then her father and mother really put a lot of pressure on her to marry my grandfather because he was obviously successful and well connected and going to go places and very smitten with her and she did it. 'Cause she thought her boyfriend was basically gone. And I think she was always very fond of my grandfather and appreciated him in a lot of ways but wasn't in love with him, and was never really in love with him. Many, many years later when my father was grown, she met again the young man she'd been in love with. And she said—she must have been eighty years old, Naomi, when she was telling me this. There were tears in her eyes. She said, "It was so sad. Here I was a grown woman with a child and I—my heart turned over." She said, "It was, you know, just so sad." And I don't—she didn't go into details so I don't really know any more about that. But obviously she had always regretted not having been able to follow through on having been in love and, you know—just really sad. I can see how it would happen that things that aren't finished stay with you.[35]

Next let us turn to the motivational structure of this schema for love. In short, interviewees said, if you love someone (1) you don't want to lose them; (2) you want to be with the person you love; and (3) you care about that person and want to do things for them, as they do for you. I illustrate with the third and most complex of these expectations. I found neither explanations nor narratives concerning violations of this expectation that people who love each other will care about and want to do things for each other. Perhaps this is because this motivation is regarded as flowing so naturally from the feeling of love as to be unproblematic. This is in contrast to the various ways circumstances can misfire—as illustrated by the story about the interviewee's grandmother—so that people who love each other don't end up getting married. Of course, even when they love their spouses and want to meet their needs, people can and do fail to do so; but that is another story.

Interviewees voiced the understanding that loving one's spouse made one care about them and want to do things for them. Once again, I have elsewhere (Quinn 1997b) published a string of illustrations in which husbands and wives said such things as, "[H]ow do you explain love? Except that you just—you care for somebody and that you want to do things for them" (1W–3); or "[L]ove is—to me, is the desire to give more to the other person than you're giving to yourself, at times" (6H–2). Here I just add a few more illustrations. The methodological point to be drawn from these cases is that, just as with metaphors and reasoning, they vary a great deal in both the specific content of the expectation, and the explicitness with which it is stated. Once again, it is necessary to look beneath this variation to discover the shared expectation itself.

[35] Interestingly, the teller of this story, some time after being interviewed by me, left her husband to return to an earlier relationship, one she would probably have characterized as having been "unfinished." I think she would have also agreed that she had always loved the former boyfriend to whom she returned. She is the same interviewee who said (quoted in Quinn 1997b:194) that she had always felt like she was cheating her husband because she was not really in love with him.

A first challenge to the analysis of usages of love comes from the variety of ways in which people may demonstrate their care for each other. The brief examples given above are nonspecific about it: you want to "do things" and "give more." The next woman, asked what she means by "being taken care of," gives a somewhat more specific answer:

> 4W–5: I definitely think that it's this feeling of having real faith and confidence that somebody else loves you and this—wants to support you through the trials and tribulations of life. Be there emotionally and that you want to do the same with them.

Other interviewees provide even more specific examples of what spouses do for each other. "If you love a person, you stick by them, for better or worse" [3H–16], one husband reports having told his mother when she worried that the woman he was going to marry had health problems. Another husband explains that a part of love is that you can be yourself and "it's never used against you" in the way people at work use it against you if you are frank and open about your feelings. A wife underscores the importance of providing one's spouse with unconditional acceptance when she criticizes her husband's family for pressuring him to achieve, saying, "I certainly didn't want him to feel that my love and acceptance of him depended on his achieving. And it really is so with his family" [9W–10]. The potentially open-ended variety of what people are motivated by love to do for each other challenges analysis, to be sure. However, once decoded, these often explicit statements about the particular things one does out of love for one's spouse provide rich evidence for the general understanding that love makes one want to do such things.

A second challenge to analysis is that, in the same way that, we saw, causal links in other reasoning are not always well marked, that between love and what it makes you want to do for the loved one is not always spelled out. Sometimes, speakers make the relation of love to wanting to do things for their spouse entirely explicit, as does the next man, talking about wanting to help his wife. He concludes, "[Y]ou do it out of love" [3H–2]. At other times, this assumption is left implicit. Remember the woman who talks about the "real faith and confidence that somebody else loves you and this—wants to support you through the trials and tribulations of life." As we have seen with other reasoning, that the other person wants to support you *because* they love you must be inferred, here, from the order of the two connected clauses. Even more has to be inferred from comments like these: "You do good and you get back something and, you know, being good to each other you're thereby getting back this love" [6H–2]; and, "I think that we both know that we have all the love that we need between us" [6H–6]. It would be difficult to interpret such shorthand statements about love—to understand why this husband thought he and his wife were getting back love, or why they felt they had all the love they needed—without first having analyzed passages in which the motivation

and behavior that love engenders—caring about and doing things for the person you love—are articulated and clearly distinguished from the emotion itself. Only then does it become clear that love is being made to stand, in these comments, for the things people do, and the benefit their spouses receive, as a result of it. As with metaphor and reasoning, I found, it is better to work with fuller, more explicit usages of a key word—certainly to begin the analysis with these—than trying to make sense initially out of fragments like the last two, in which the speakers have left crucial parts of the schema they have for love unsaid.

It may seem contradictory to have claimed, at the beginning of the earlier section on metaphor analysis, that one advantage of such an analysis was its comprehensiveness—and now to admit, at the beginning of this section, that this selfsame analysis of metaphor proved to be incomplete in such a crucial way. Why did marital love—so important in Americans' understandings and made so glaringly obvious in popular culture—not emerge from an analysis of metaphors for marriage? This is not to say that people do not use metaphors to talk about love, just as about anything else: They do. Like the interviewees quoted above, they say things like, "[Y]ou're getting back this love," and "[W]e have all the love we need between us." They say other things like, "I feel so filled up—all filled up with love" [6H–4]; or, "I think my love grows more and more every day" [1W–3]; or, "In my earlier days, I sort of threw love out the window" [2H–2]; or, "[L]ove doesn't conquer all" [7W–6]; and they use a number of more conventional metaphors such as "showing love," "making love," and "falling in love," all the time. But metaphors for *marriage* do not capture the expectation that married people will love each other, and other expectations about marital love, in the way they refer explicitly to the expectations that marriage be beneficial, say, or lasting. Nor, I should add, do people reason about the relation between love and marriage in the way they reason about the causal relation between benefit, lastingness, and their other expectations about marriage. The reason they did not do so is interesting. Love is not an explicit expectation about marriage; rather, it provides the implicit structure of marriage. This structuring of marriage by love is neither reasoned about nor highlighted in metaphor because, as we have seen, it is taken-for-granted and normally remains entirely tacit. Indeed, as I suggest next, it is partly unconscious. A methodological lesson to be drawn from my initial failure to recognize this important term in the analysis is that different methods of analysis are likely to be needed to reconstruct tacit understandings, than those about which individuals make deliberate points and explicitly reason. The more general lesson is that we should never rely wholly on analysis of metaphors or any one single mode of analysis, however seemingly rich its yield.

Interpretative Leaps

The analysis of usages of the word "love," in this discourse, may have been relatively direct and obvious; but the next step—interpreting the pattern

I had found—was a leap. Occasions for such leaps inevitably confront us in the interpretation of our findings and they often lead us, as this one did, in new and interesting theoretical directions. In this case, I noticed an intriguing correspondence between the motivational structure of love and the three primary expectations interviewees had about marriage, as these had emerged from my analysis of metaphor. Put succinctly, just as we don't want to lose the person we love, but want that love to last forever, marriage is supposed to last; just as we want to be with the person we love, marriage is supposed to be shared; and, just as we want to do things for the person we love, marriage is supposed to be mutually beneficial in the sense of need fulfilling. My interpretation was that marriage is, in our society, the institutional realization of love.[36] As the song tells us, love and marriage go together in a particular way—"like a horse and carriage." Love is the "horse" that pulls marriage.

What comforts me about my failure to analyze usages of "love" in this talk about marriage earlier than I did, is that I could not have arrived at a meaningful interpretation of this analysis, even had I performed it at the beginning. This is because the interpretation, when I did arrive at it, depended on noticing the correspondence I have described between the motivational structure of love, and the three expectations that marriage be lasting, shared, and mutually beneficial. These three expectations emerged from my analysis of metaphors for marriage. As I have said, they set in motion the story about marital compatibility, difficulty, effort and so forth. But where did the three a priori expectations themselves come from? Only now was I in a position to speculate that they came from Americans' schema for love.

Still, this analysis was unfinished: Where did these understandings about *love*, in turn, come from? One final interpretive leap was left for me to make. I began with a sense that the three motivational components of love—not wanting to lose the person we love, wanting to be with that person, and caring about and wanting to do things for that person—had a regressive look to them. In making sense of this observation, I was drawn to a dim memory of something I had read in college by Sigmund Freud.[37] I realized that the three motivational components matched Freud's description of infantile preoccupations and anxieties. The infant fears that its caretaker will leave it, wishes to be with the caretaker, and is concerned that the caretaker fill all its needs (the adult version, of course, requires

[36] This mapping of love onto marriage has a history, and the certainty with which Americans today believe that "love and marriage go together" surely owes something to the struggle, beginning in the late eighteenth century and continuing on into the nineteenth, of individual couples to make their own marriage choices on the basis of romantic love, instead of entering marriages arranged to serve the purposes and reflect the judgment of parents and other kin. See Griswold (1982:1–17), among other historians, for a good summary of these developments.

[37] Which, when I tracked it down, proved to be from the third of his *Three Essays on the Theory of Sexuality*, called "The Transformations of Puberty" (Freud 1962:88).

reciprocity, so that each fills the others' needs). The double correspondence I had discovered—first, between infant love and the way my interviewees talked about adult love, and, second, between their schema for adult love and the expectations they held for marriage—struck me forcefully. I concluded that infant experience was the motivational wellspring for marriage, as Americans knew and practiced it. Like the infantile roots of adult understanding and motivation more generally, the roots of this one are all the more implicit in discourse, because defended against and hence unconscious. (As I have pointed out elsewhere (Quinn 1997b:201), interviewees could tell of talking baby talk to each other, while indicating not a hint of awareness of the implications of this behavior.)[38]

There is a methodological footnote to this story. Love was not the only key word I examined. Indeed, much earlier in the course of this research, even before I analyzed metaphors for marriage, I had analyzed interviewees' usages of the word "commitment." As reported in Quinn (1982), I found that these usages fell into three classes: use in the sense of *promise* (as in, "We were making a commitment together" or "The marriage commitment is a commitment to grow old together"); in the sense of *dedication* (as in, "I feel totally committed to the relationship" or "It's a commitment to our marriage, a commitment to wanting our marriage to work"; and in the sense of *attachment* (as in, "We feel married already; we have the commitment to each other" or "Was I willing to commit myself to her?"). In retrospect (Quinn 1997b:fn.2), I conclude that commitment reinforces the institution of marriage by supplementing the powerful but sometimes erratic motivation of love with a more dependable source of motivation.[39] As patterned as was its usage, and as integral its role in marriage, however, commitment did not drive the schema for marriage I had derived from my analysis of metaphor and reasoning, in the way that love did. Love, as I have described, led back to the schema's motivational source in early childhood.[40] It turns out, then, that not all key words are equally key.

[38] See Quinn (1997b) for a more fully developed argument. My interpretation of the relation between marriage, adult love, and early experience remains speculative, certainly. But it is speculation that anticipates a promising synthesis of cognitive and psychoanalytic anthropology, suggesting how psychodynamics can provide an explanation for some of the most deeply motivating cultural schemas such as Americans' schema for marriage. (See, among other arguments for such a synthesis, Paul 1990 and Nuckolls 1996:3–23.) And, it is speculation that I plan to pursue as my next major research project.

[39] Indeed, commitment may predate love, in the history of Euro-American marriage, as a motivation for staying married.

[40] Chris McCollum (personal communication) has suggested to me that the relation between marital love and marital commitment can usefully be viewed in terms of Obeyesekyere's (1990) distinction (drawing on a point made by Freud in *Interpretation of Dreams*) between *regression* and *progression*. Regression involves a return to psychic origins in childhood, while "a progressive movement of unconscious thought involves the transformation of the archaic motivations of childhood into symbols that look forward to the resolution of conflict . . ." (Obeyesekere 1990:17). Commitment, in these terms, can be seen as the progressive element in marriage. The married adult does not simply regress to a state of being

And no matter how intuitively we approach our analysis, and how attuned we are to the logic of what follows upon what in this analysis, and how opportunistically we utilize available clues in our data, we do not always hit upon the most important evidence first.

Conclusion

While the theory of cultural meaning is still unsettled, it is less so than it was when I began. We can now explain how cultural understandings, or schemas, are built up from shared experience.[41] My own work on cultural understandings of marriage has led me to an appreciation of the way different sorts of shared experience eventuate in different kinds of shared schemas. I hope to have provided, as well, a description of the American cultural model of marriage that is fuller and more dynamic because of its grounding in schema theory. I hope, further, that my work so far has not only contributed to a theory of the cognitive basis of culture, and led to a better description of American marriage, but has also illuminated the inextricable involvement of culture with cognitive task performance, and the equal complicity of culture with deep human motivation. Such theoretical work, wherever it leads, cannot be separated from empirical work, and the methods that the latter demands. In this chapter, I have tried to demonstrate that, indeed, theoretical gains depend on the most minutely detailed empirical investigation and on systematic methods designed to do it. Most of all, I have tried to convey what the process of such an investigation is like, in research of the kind I do. In this process, the work does not stop with conceptualization of a research problem and application of some preselected methods to address it. Instead, there is an ongoing need to invent appropriate methods, to match these to opportunities provided by existing data, and to pursue the logic of each new finding to the next analysis. Like an American marriage, research takes continual effort to succeed.

Acknowledgments

Thanks to Claudia Strauss for her usual careful reading and savvy suggestions for revision, and to Steven Bialostok for raising, and making me confront, questions that other novice users of these methods are also likely to have.

---

loved and fearing love's loss, but takes responsibility for loving reciprocally, and for putting in the effort necessary to make this love and the marriage last. Infantile dependency is recruited to an adult cause.

[41] See Strauss and Quinn (1997).

References

Bruner, Jerome. 1990. *Acts of meaning*. Cambridge, MA: Harvard University Press.

Clark, Andy. 1997. *Being there: putting brain, body, and world together again*. Cambridge, MA: MIT Press.

Fillmore, Charles. 1975. An alternative to checklist theories of meaning. In *Proceedings of the first annual meeting of the Berkeley Linguistics Society*, ed. C. Cogen, H. Thompson, G. Thurgood, K. Whistler, and J. Wright, pp. 123–131. Berkeley, CA: University of California Press.

Freud, Sigmund. 1962. *Three essays on the theory of sexuality*. New York: Basic Books. (Tr. J. Strachey; published in German in 1905 as *Drei Abhandlungen Zur Sexualtheorie*; 1st published in English in 1910.)

Griswold, Robert L. 1982. *Family and divorce in California, 1850–1890: Victorian illusions and everyday realities*. Albany, NY: State University of New York Press.

Hutchins, Edwin. 1980. *Culture and inference: A Trobriand case study*. Cambridge, MA: Harvard University Press.

———. 1995. *Cognition in the wild*. Cambridge, MA: MIT Press.

———. 1996. Response to reviewers. *Mind, Culture, and Activity* 3:64–68.

Lakoff, George and Mark Johnson. 1980. *Metaphors we live by*. Chicago, IL: University of Chicago Press.

Langacker, Ronald. 1979. Grammar as image. Paper presented at conference, *Neurolinguistics and cognition*, Program in Cognitive Science, March, University of California, San Diego.

Linde, Charlotte. 1993. *Life stories: the creation of coherence*. Oxford, England: Oxford University Press.

Nuckolls, Charles. 1996. *The cultural dialectics of knowledge and desire*. Madison, WI: University of Wisconsin Press.

Obeyesekere, Gananath. 1990. *The work of culture: symbolic transformation in psychoanalysis and anthropology*. Chicago, IL: University of Chicago Press.

Ochs, Elinor and Lisa Capps. 2001. *Living narrative: creating lives in everyday storytelling*. Cambridge, MA: Harvard University Press.

Paul, Robert. 1990. What does anybody want? Desire, purpose, and the acting subject in the study of culture. *Cultural Anthropology* 5:431–451.

Quinn, Naomi. 1982. "Commitment" in American marriage: a cultural analysis. *American Ethnologist* 9:755–798.

———. 1987. Convergent evidence for a cultural model of American marriage. In *Cultural models in language and thought*, ed. D. Holland and N. Quinn, pp. 173–192. Cambridge, England: Cambridge University Press.

———. 1991. The cultural basis of metaphor. In *Beyond metaphor: trope theory in anthropology*, ed. J. Fernandez, pp. 56–93. Stanford, CA: Stanford University Press.

———. 1996. Culture and contradiction: the case of Americans reasoning about marriage. *Ethos* 24:391–425.

———. 1997a. Research on shared task solutions. In *A cognitive theory of cultural meaning*, ed. C. Strauss and N. Quinn, pp. 137–188. Cambridge, England: Cambridge University Press.

———. 1997b. Research on the psychodynamics of shared understandings. In *A cognitive theory of cultural meaning*, ed. C. Strauss and N. Quinn, pp. 189–209. Cambridge, England: Cambridge University Press.

———. 1999. Metaphor and culture. In *The MIT Encyclopedia of the Cognitive Sciences*, ed. R. Wilson and F. Keil, pp. 537–538. Cambridge, MA: MIT Press.

Schank, Roger and Robert Abelson. 1977. *Scripts, plans, goals and understanding: an inquiry into human knowledge structures*. Hillsdale, NJ: Lawrence Erlbaum.

Strauss, Claudia and Naomi Quinn. 1997. *A cognitive theory of cultural meaning*. Cambridge, England: Cambridge University Press.

Chapter Three

Some Methods for Studying Cultural Cognitive Structures

Roy D'Andrade

Human cognition appears to involve two different kinds of cognitive structures. One kind of structure consists of the organization of a small number of elements into a *gestalt* that functions as a "chunk" in short term or working memory. Examples are objects like the human face, events like something falling down, relations like X happening before Y, and so on. Typically these cognitive chunks are symbolized in language by single words or short phrases and can be unpacked into the kinds of simple universal concepts or "primes" outlined by Wierzbicka (1996).

Such chunks contrast with more complex types of structure. There appear to be a variety of these complex types of structure: taxonomies, paradigms, partonomies,[1] scripts, models,[2] semantic networks, story grammars, narratives, discourses, associative nets, and so on. These complex structures consist of a number of chunks in various relationships with each other. While a chunk can typically be defined in a sentence, description of a complex structure requires a number of sentences—something like a paragraph. The full representation for something like a story or taxonomy cannot be held in short-term memory. (However, one can easily hold in short-term memory the *name* for a story (e.g. the story of *Adam and Eve*) or a short descriptive phrase for a taxonomy (e.g. the *Tzeltal plant taxonomy*.)

These two types of cognitive structure exist because the severe limitations of short-term memory force the human operating system to use different strategies. One strategy is to reduce the total number of things that need to be represented by chunking or grouping things into a *unitary* representation that can function as single item in short-term memory. However, the small size of short-term memory limits the number of things that can be chunked together to five or so (Wallace 1964, Mandler 1985). How then can relations among a sizeable number of elements be represented? The answer is that a *serial* strategy must be used, which is what happens

[1] A partonomy is a taxonomic like structure built on the "X is a part of Y" relation. A protypical example is the partonomy for the human body.

[2] Models as defined here are cognitive structures used to reason about something. Some models are small enough and well organized enough to function as chunks in short-term memory, but most models are complex cognitive structures.

when we represent some state of affairs in a paragraph.[3] A paragraph presents in verbal form a string of conceptually linked chunks.

Current work on neural networks is generally not concerned with the distinction between structures that can serve as single chunks in short-term memory and structures that cannot. Both can be modeled as neural networks, as described by Strauss and Quinn (1997). However, for an analyst of culture or language, the distinction is an important one. Not everything can be adequately represented in a word or single phrase. When the number of things to be represented gets larger than five or so, serially organized structures need to be built, and these can take a number of different forms (D'Andrade 1995:150).

Activation of short-term memory chunks is experienced consciously as the perception or thought of objects and events. Identification and analysis of these chunks is relatively straightforward—rather like writing a dictionary. Of course, some kinds of cultural chunks are hard for informants to verbalize, such as the meanings of grammatical categories: For example, what does "the"[4] mean? But, as a general rule, most cultural chunks can be identified and analyzed without special problems. Doing so is the bread and butter of ethnography.

Adequate description or understanding of a culture requires more than a listing or a dictionary of cultural items; it requires an account of various ways these items are organized. Debates in cultural anthropology and linguistics often revolve around issues of complex structure because people usually cannot describe these structures adequately[5] and because it is often impossible to observe the organization of cultural items directly. A description of a complex cultural structure is typically a theory about the internal mental world of one's informants. This chapter discusses some heuristic procedures for constructing such theories—heuristic in the sense that these procedures can be helpful but are not guaranteed to always produce optimal results.

[3] Although humans can construct ever larger chunks by unitizing smaller chunks, this repackaging strategy has its limits. Try to imagine a language that has just single words. The single word "mazix" might mean "John went down town," while the single word "kiftof" might mean "John stayed down town for an hour." The obvious problem is that one would need too many words to talk intelligently about the world. There are just too many combinations of objects and events for one to construct a language in which every combination has a single word.

[4] An example I like of how badly people understand their own grammatical categories is the belief that many social scientists have that the word "data" is a plural, and that therefore one should say "these data" rather than "this data." The idea that "data" is a plural is wrong; we don't use it that way. No one says "two data," or "one hundred and twenty seven data." Data is always *used* as a mass noun, like "sweat" or "sand." However, because it is a plural in Latin, purists think it must be a plural in English even though *they themselves have never said "two data."*

[5] An exception is when cultural items have been organized by intellectual experts into a system, such as the axioms and theorems of mathematics or the topical outline of a legal system.

Contexts of Discovery and Contexts of Verification

To find something out about the world one must first have some idea about the world, and second, have some way to find out whether or not this idea about the world is true. Reichenbach (1938), a philosopher of science, has discussed these two aspects of science, calling the first *contexts of discovery* and the second *contexts of verification* or *justification*. Both aspects of investigation are necessary, but the difficulty of each varies greatly by circumstance. In some cases the discovery of ideas is extremely difficult. For example, currently there are no clear and plausible ideas about how consciousness is produced by the activation of neurons in the brain. This is the great mystery of our time. In other cases the difficulties center on finding ways to test whether or not some idea is true. For example, it took hundreds of years before anyone knew how to construct a demonstration to show that light doesn't always travel in straight lines.[6]

Philosophers of science tend to have more to say about methods of verification than methods of discovery. There are established procedures for trying to determine the truth of some idea; for example, first deduce from the basic idea some specific consequence ("hypothesis"[7]) that could be observed if the basic idea is true, then develop ways to obtain reliable observations about whether or not this consequence occurs, and then analyze these observations to see if the consequence really occurs. Experimental methods and a variety of correlational methods (surveys, natural observations) are well formulated procedures for testing ideas, and are a standard part of graduate training in many of the social sciences, although currently not taught in most anthropology departments.

Issues with respect to the discovery of ideas are less well formulated. Obviously, one can get ideas from dreams, or from intuition, or wherever. It doesn't matter where an idea came from; all that matters is whether or not the idea is true (Hempel 1965:5–6). But it is very helpful to have some heuristics for finding good ideas—ideas that are *likely* to be true. Looking long and hard at data is one such heuristic. Using analogy—that is, finding something that is well understood and using what is known about it to try to understand something else—is another well-known heuristic (Gentner and Holyoak 1997). However, these two procedures are not sufficient. Part of the problem is that discovery heuristics are often domain specific; a heuristic that works well for geological problems may not work for linguistics problems.

[6] This is taking the word of Richard Feynmann (1985), who says the idea that a photon of light travels in straight lines is only an approximation to the way light acts in the real world, and that photons of light actually go every crooked way, but most of the crooked paths are cancelled out by the same photon which is also taking other nearby crooked paths at the same time.

[7] The word "hypothesis" has come to mean just a guess. But in standard philosophy of science language, a hypothesis is a logically derived and potentially testable consequence of some general proposition or law.

Quinn's Model of Marriage and the Context of Discovery

Quinn, in her chapter in this volume, describes the process by which she discovered her hypotheses about the American model of marriage. After a trial and error period, Quinn hit upon the idea of searching her interview material for metaphors. She found she was able to classify the things that her informants made metaphors about into eight classes.

> How does one go about identifying the classes into which a set of such metaphors falls, and assigning these metaphors to their appropriate classes? I identified all the metaphors I could locate, then I did a great deal of examining, shuffling, thinking, and reshuffling until the analysis ultimately "fell out." The truth is that, at first, I missed one of the metaphor classes . . . (p. 53, this volume).

There are two parts to Quinn's heuristic discovery technique. The first is the process of *winnowing*. That is, by selecting from the thousands of sentences of her informants just those that contained metaphors, Quinn drastically reduced the sentences she had to process. Also, sentences that contain metaphors are meaningful in a special way. Selecting just these sentences for analysis not only cut down the total number of things to be analyzed, it also picked a rich kind of material for analysis. The second process is the *classification* of the reduced data set on the basis of similarity. This resulted in the discovery of the categories of mutual benefit, sharedness, compatibility, lastingness, and so on. The analysis "fell out" and Quinn found that almost all of some four hundred metaphors could be placed in eight categories.

Given the eight categories, Quinn discovered another idea; that there was something story-like about the relations between these categories. She says:

> The story I had in mind was purely a product of intuitions I had, as an American myself, about American marriage. Roughly, I knew, people regarded their marriages as successful if they lasted. In order to last though, a marriage had to be beneficial, and in order for it to be beneficial, its difficulties had to be overcome, which required effort (p. 60, this volume).

Here is a nice example of idea discovery. An important issue, assuming that the eight categories were real, is the nature of the organization of these eight categories. Are they just a checklist, like a feature analysis? Or are they part of a taxonomy, with some categories standing above others? Or some kind of partonomy? Quinn found an analogy between the way stories are structured and the way she thought about marriage. There is an event-like sequencing in the model (e.g. a marriage is successful if it lasts), although the

structure that Quinn finally arrives at is a set of causal contingencies rather than a narrative of the type analyzed by Holly Mathews in this volume.

Verification

In her chapter, Quinn then asks:

> How could I verify this story? What kind of evidence would convince others that it existed in American's minds . . . ? It occurred to me at some point to cull out and examine instances of reasoning that interviewees did about marriage, to see if this reasoning conformed to, and supported, my intuitions about what led to what in the cultural story of marriage (p. 61, this volume).

Here is a clear statement with respect to contexts of verification. Relating reasoning to cultural models was developed more or less independently by a number of anthropologists; for example, Gladwin in his study of Trukese navigation (1970), I in my study of American beliefs about illness (1976), Quinn in her study of Fante market decision making (1978), and Hutchins in his study of models of land tenure in the Trobriands (1980). The idea that people's reasoning is related to the content of their cultural models follows naturally from the very idea of a model, since that is what a model is for (Craik 1943). What Quinn did was to derive the hypothesis that informants would use the same concepts and causal relations found in the cultural model of marriage when reasoning about marriage and use this to verify her model.

Verification, whether in physics or anthropology, always entails a *demonstration*—one has to show *something* (Campbell 1986). In physics, for example, one computes the results of an experiment from a theory. In principle, anyone can reproduce the experiment and see the results for themselves and reproduce the computations. What is the *demonstration* for cultural models?

One of the verifying demonstrations used for cultural cognitive models is also used in linguistics and in psychology. The demonstration consists of presenting a transcript of what someone has said and going through the process of showing how something in the transcript is a consequence of the proposed model (or grammar or pathological idea or whatever). In a number of publications Quinn has presented a line by line analysis of reasoning about marriage by her informants (Quinn 1987, 1992, 1997: 160–175) and pointed out how the ideas and relationships they use in reasoning correspond to the ideas described in the cultural model of marriage. The match of the propositions of the model to the propositions used in the informant's reasoning is impressive. Similar demonstrations have been presented by Strauss (see this volume), Hutchins 1980, D'Andrade 1987, Parish 1991, Linger 1992.

When Can the Facts That Lead to the Discovery of an Idea Also be Used to Verify that Idea?

Sometimes the same fact can be *both* a stimulus to discovering an idea and verification of the idea. Consider the case of a watch in a box. One opens the box and sees the watch. What made one think that there is a watch in the box? What one saw. What is the evidence there is a watch in the box? One saw it. Here the same event is both the stimulus for discovering an idea and the evidence that the idea is true.

Now consider a different case. There is something in a small box. One shakes the box. Shaking small boxes to get an idea about what might be in them is a well-known discovery heuristic. Suppose the noise sounds as if a hard object is sliding around in the box and the idea that it might be a watch comes to mind. Shaking the box produced a sound that stimulated an idea about what is in the box. Does the sound also count as *evidence* that there is a watch in the box? Well, some. But not much. It wouldn't really count as *verifying* that there was a watch in the box. Why not?

At this point in an earlier version of this chapter I attempted to use Bayes's theorem with an example of urns that have different proportions of red and white beads in them to explain why not. The example was unenlightening; readers in fact found it worse than no example. The problem was not with the small amount of algebra involved (though that didn't help), but with understanding how urns and beads could be related to the things anthropologists do. So, let us stay with the example of the watch. Why doesn't the sound of a hard object sliding around verify that the object in the box is a wristwatch? The obvious and good answer is that there are too many other things that could make the same sound. There are too many plausible alternative hypotheses. *Verification requires eliminating alternative hypotheses*. (A brief appendix is included at the end of this chapter for readers who would like to see the numbers.)

Suppose one holds the box to one's ear and hears the ticking? Have we now verified that the object in the box is a watch? Well, the evidence is stronger, but plausible alternative hypotheses still remain. It might be a bomb. Or any other kind of mechanism that ticks. If one can't open the box and see what is in it (and one can't open human heads and see how the ideas are organized there), then direct verification is impossible, and one must try to figure out what is there by eliminating the plausible alternatives. Bayes's theorem simply tells us how to reassess the probability of one hypothesis *relative to an alternative hypothesis* when given new evidence. The theorem is based on the simple idea that if one encounters facts that support one hypothesis *more than alternative hypotheses*, one should believe more strongly in that hypothesis.

In general, facts that stimulate one to have an idea about the world may, because they are facts, increase one's confidence that this idea is true. But

estimating how *much* the stimulating facts increase the probability this idea is true requires knowing how much the facts increase the probability of this idea *against* competing ideas. And very often, after discovering a good hypothesis, there are still plausible alternatives. So the best thing to do is to find other facts that would support one hypothesis against its plausible alternatives. In short, don't think of the facts that caused one to think that something might be the case (the context of discovery) as *confirming* evidence that something is the case (the context of verification), unless one can think of no alternative explanation that could also fit the facts. This is a tough but good rule.

In my study of the model of the mind (1987), carried out at about the same time Quinn was working on marriage, my problems were primarily those of verification. There have been several thousand years of philosophic and scientific discussion about how the mind works. The basic categories (perceiving, thinking, feeling, wanting, intending) are articulated everywhere—from the organization of Roget's Thesaurus to linguistic classifications of verbs of mental processing and speech acts. Discovering hypotheses about how these categories affect each other was not a problem. The problem was to show that there is a simple set of ideas ordinary people have about what happens in the mind. I also wanted to argue on the basis of a small amount of evidence that the model is universal. (This is an idea that runs against current trends, although Wierzbicka's [1996] finding that "see," "hear," "think," "know," "feel," and "want" are candidates for universal semantic primes may give the universality argument additional support.) The kinds of verification I presented included informants' answers to direct questions about how the mind works and an analysis of stated and unstated propositions about how the mind works in a selection from a Jane Austen novel. These were not very powerful verification techniques, but they did represent a good faith effort. Subsequent quantitative work on the model of the mind has generally supported this model (Wellman 1990, Schwanenflugel et al. 1994, Lillard 1998).

More Heuristics for Discovery of Complex Cultural Cognitive Structures

Over the past decade I have developed a set of *discovery* techniques to help graduate students and undergraduates investigate cultural models and other cultural cognitive complexes. For classroom exercises the first problem is the selection of a target model; until some exploration has been done, one cannot be sure that anything exists that corresponds to the model one thinks one wants to investigate. Once, based on whatever evidence, a reasonable target model has been selected, the second issue concerns the interview situation. Ideally one would like to encounter multiple natural situations in which people discuss with each other the relevant topic. An

arranged discussion between informants can sometimes come close to this ideal; long before the current interest in focus groups, Ruth Benedict and Margaret Mead made use of this small group technique in their studies of culture at a distance (Mead and Metraux 1953). Less ideal but easier to arrange is the standard one-on-one interview with an informant.

In general I have found it is better not to ask informants directly about their models, but rather to ask something that will bring the model into play; that is, something that will make the person *use* the model. One relatively successful classroom assignment concerned the American undergraduate's model of *society*. Interviewers were asked to investigate ideas about *social equality*. In discussing the problems of achieving social equality the informants had to use their underlying model of American society to explain how inequality comes about and is maintained.

For this assignment, student interviewers were instructed to develop a series of questions to explore the topic of social equality—for example, "What does *social equality* mean to you?" "What are examples of social equality?" "What are examples of a lack of social equality?" "Why does this happen?" "What makes for greater social equality?" and so on. Interviews were done individually and tape-recorded. Appropriate human subject protections were provided, including guarantees of anonymity and erasure of tapes. One might think that such protections are unnecessary for a topic as innocuous as social equality, but informants often provide personal details and tell stories about people they know, and it is surprising how fast information can spread about exactly those bits of information that can cause harm.

The interviewers were instructed in the use of nondirective methods, such as probe questions—for example "What do you mean when you say . . .," "I'm not sure I understand what you said about . . .," "Could you say more about . . .," and the ubiquitous use of "uhuh" and a long wait to let the informant continue. Interviewers are also instructed in the technique of rephrasing what the informant says in the informant's own words that lets the informant know that they have been understood and encourages them to expand on some statement or clarify it.

It is difficult for inexperienced interviewers to completely restructure their normal conversational habits. However, this particular kind of semi-nondirective interviewing appears to be one of the normal interpersonal skills of an American undergraduate; that is, how one should act in a situation in which one is really interested in what somebody is saying about something and one wants to find out what this person *really* thinks about it. Perhaps because this kind of interviewing is related to an already learned skill, some undergraduates become adept in a short period of time.

In transcribing their tapes interviewers find they have made many mistakes, but they also discover that a cooperative informant will often repair errors of technique and that informants say the same thing in many ways, so that basic ideas usually get stated. Use of leading questions is an especially common failing. Students quickly come to recognize that this

can impede discovering what the informant really thinks unless the informant contradicts the suggestion.

The next set of issues concerns transcription of the interview. The degree of detail needed for the transcription depends on the goals of the research. If disguised attitudes and feelings are an important issue, a detailed recording of pauses, repeats, unusual emphasis, and so on, may be needed. If the main purpose of the research is to describe the cognitive content of a cultural model that is intersubjectively shared, less detail seems to be needed. For such work I have found something like the verbatim record from a law court sufficient.

Once the transcription has been prepared, the next step is to excerpt what has been said into simple propositions. This is the first winnowing procedure. The point of this process is to recover the *gist* of the informant's speech. In so far as possible, gist propositions should use only words the informant has said. For example, one informant, interviewed about social equality, said:

> The problem is, you can moan and carp and whine and complain all you want about it, but there are some people that don't want to be helped and want to be left alone. If a guy wants to sit on top of forty acres on top of a mountain in Tennessee and make whiskey and drink himself blind drunk, leave him the hell alone.

The gist propositions here are:

1. Some people want to be left alone and do not want to be helped.
2. People who want to be left alone and do not want to be helped should be left alone.

Another example:

> Human equality? That's the supposition by some group of idiots that all men are created equal in reality when they're not. Each person is an individual. Each person has God given talents, just like fingerprints. There are no two of us alike. No matter how hard people try to make us alike, we are not alike individually.

The gist is two propositions:

1. All people are not created equal because people are created with different talents.
2. Some people who suppose that humans are not created with different talents try to make us all alike but they cannot.

Transforming real talk into propositional gist seems to be a task that normal undergraduates can do with reasonable reliability. I have found that when different students excerpt the same transcript 80 to 90 percent of the excerpted propositions show a clearly recognizable match.

The next step is to try to work out the basic objects that are the participants in the complex structure. To help in this process one can take all the propositions and do a simple frequency count of words. If we look at the word frequencies for the 600 propositions, eliminating words like "the," "and," "is," and so on, we find the following words that occur five or more times:

Nouns	*Verbs*	*Adjectives*
people 63	help/helps/helping 16	equal 24
America/can 32	need 9	different 19
prejudice 18	achieve 8	good 12
education 18	fight 7	same 10
money 14	own 7	hard 9
inequality 14	feel 5	rich 7
job/jobs 14	guarantee 5	unfair 6
others 13	judge 5	financial 5
class 12		ideal 5
rights 12		racial 5
women 11		poor 5
groups 9		
freedom 8		*Other*
law 8		not 39
minorities 8		should 25
affirmative action 7		because 20
chance 7		more 17
culture/cultural 7		against 6
levels 8		
men 7		
disadvantage 6		

From the propositions in the outline and the word frequencies one can begin to build a simple cognitive cultural model of society. The following represent some of what appear to be the basic building blocks of the model:

people categorized as men and women, rich and poor, black and white, and so on, who have, because of birth or background, different characteristics such as talents and drives.

nations such as America and Russia, which have governments that have constitutions, laws and regulations of various kinds.

businesses and other institutions that employ people.

jobs positions people hold in business and other institutions that pay them money to perform services.

money received as salary or obtained from property or inheritance.

educational institutions schools and colleges, which among other things, train people and give them credentials or background training for various jobs.

opportunity something that gives one a chance to achieve some goal, such as get a better job, start a business, make more money, and so on.

capitalism a political and economic system based on individual ownership of property and businesses.

social levels social ranking and group affiliation based primarily on wealth, which give people differential advantages in obtaining wealth.

prestige, esteem, respect, status a sentiment or feeling about the worth of people.

drive or *ambition* a motivational characteristic of people that explains why some people work harder to achieve status or other goals.

beliefs ideas people have about the world and other people. When one group is incorrectly believed to be worse or better in some way than they really are, these beliefs are called *prejudices*.

discrimination when some person or institution treats someone or some group unfairly on the basis of some prejudice.

The next step is grouping the propositions. One technique is to type each proposition on a three-by-five card and group the cards into piles by topic. Topics are arrived at inductively, and it is often helpful to have a large table to spread the cards out on, beginning by putting the obviously similar propositions together, and from these groupings moving to construct larger groupings. These groupings are an aid to the analysis, but are not in themselves an analysis. The main use of the groupings is to identify central ideas of the model.

In the exercise on society, it was found that each informant produced around thirty propositions in an hour length interview. Over six hundred propositions were collected by the class. Four large categorical grouping of these propositions were found; these are presented below with examples.

Hierarchy and Level

The idea of different levels of people in society is ingrained in our culture.
The existence of classes influence the way people perceive others.
In capitalism, there's always going to be poor and there's always going to be rich.
The different classes in our society are determined by income.
Inequalities in status, with some people rising to the top, is something we learn from the time we're kids.
Social interactions—what you do, where you live, who your friends are, are largely determined by how much money you make.
Different groups don't have the same opportunities.
The very customs and ways of life that bind us together as groups separate us from people in other groups.

Our classes in America are a flexible ladder that one can move up on, as opposed to rigid castes into which one is born.
Being equal to someone else means feeling easy with that person.
People feel resentful when treated as an inferior.

Mobility and Striving

Americans want to be wealthy.
In our society our minds have been ingrained with the idea of getting to the top, trying to climb the ladder of success.
The American way of life means that if you work hard you'll be able to get the things you want.
We all have different talents and abilities and differ in how we use and develop them.
Trying hard is the most important thing to doing well.
Education is the key to equal opportunity in the job market.
Inequalities emerge because of potentials that haven't been developed.
Although everyone in this country has the right to achieve their goals, it is easier for the rich to do so.

Equality and Rights

Equality is something that we as a nation believe in.
Legally people are all equal in this country.
You are born equal, but after that the world is not an equal place.
Equality is difficult to achieve in America because of our diverse backgrounds.
Society is constantly progressing toward greater equality.
Different groups of people need to respect each other in order to get along.
Financial and language barriers keep immigrants from being equal.
It is unfair to let immigrants take our people's jobs away.
Men have an unfair advantage over women in jobs.

Prejudice

Judging people by an arbitrary factor such as race, color, or religion and limiting their opportunities is wrong.
Not everyone has an equal chance of getting to the top because of prejudices.
Laws help to reduce bias.
Affirmative action programs have been effective in removing some of the inequalities in the job market.
Affirmative action programs have caused resentment by those who have been displaced.

The next and most important step is to examine these propositions and explicate the basic relations between objects being talked about. These

ideas are often assumed or presupposed. For example, one of the informants said:

> Our classes in America are a flexible ladder that one can move up on, as opposed to rigid castes into which one is born.

What is presupposed in such a statement? Among other things, that there is a place, called "America," in which there are things called "classes," that these classes are hierarchically ordered, that the American people can "move up" (and presumably "down") from the class into which they are "born," and that there are other places in the world where this is not true because people are fixed in their classes or "castes." This seems to be a highly shared knowledge structure, and many of the propositions presented above presuppose more or less directly this structure. Some other examples taken from the 600 are:

> The chance to rise above others gives people hope.
> Different financial levels motivate people to move up.
> One can feel pity for those on a lower level.
> Opportunity for advancement gives people the spirit to live.

The up/down metaphor is obviously one of the important ways this hierarchical layering of classes is expressed. However, as Quinn has argued (1997:156–160), overall structure in a model can be metaphorized in a variety of ways (the class hierarchy can be expressed as "advancement," as "having more," as "doing better," as "inequalities," as well as by up/down). When real talk rather than invented examples are used, the evidence indicates that the model selects the metaphor rather than the metaphor structuring the model.

Once some of the major relationships within the model are formulated, one can begin to concentrate on sub-relationships. For example, one often presented sub-schema concerns social mobility. One informant says:

> People who have worked and strived to reach higher levels deserve to be above others.

What seems to be presupposed in this proposition is the idea that people can move from one level to another because of work and striving. While informants differ in whether they believe that *anyone*, if they work and strive, can move up, there seems to be no disagreement that there is a relationship between moving up and work. Work and effort are seen as probably necessary but not always sufficient conditions for achievement.

One striking thing about these propositions about society is that informants do not seem to distinguish between "achievement," "making money," "being successful," "doing better," "getting ahead," and so on. It is as if, for this task, money = success = superior achievement. It appears

that when these informants think of society they think of strata or classes where position in these classes is predominantly a matter of some kind of generic "money/success/achievement." Of course, a different task might elicit cultural understandings about the ways in which money and success and achievement are different from one another.

Another part of the model is a substructure in which equality leads to opportunity and opportunity leads to people advancing or rising in the class hierarchy, which in turn makes for differences which lead to inequality. This is a paradox for some informants—creation of equality of opportunity results in inequalities in wealth and power because differences in motivation and talents produce differences in wealth that produce inequality of opportunity. Behind this causal chain seems to be the deep assumption that people naturally take advantage of opportunities—not always, but it can be expected—and that taking advantage of these opportunities will result in disadvantaging others. The rich family can give a better education to their children, the elite can use their connections for their advantage, and so on. As one informant put it:

> People born into rich families start out sixteen steps up the ladder compared to the poor family, but that's the way it goes.

The informants occasionally suggest that it is the responsibility of the government to add extra money to the education and training of those at the bottom to try to preserve equality of opportunity. However, in the great majority of transcripts no sub-model appears about what the government does or should do. Government, businesses, families, classes, schools, the law, and other institutions seem to be taken for granted as parts of society, but little is stated by informants about how these institutions are constituted or how they function. Also religion and church are rarely mentioned. The center of the model seems to be the individual's relation to wealth and success. That people will take advantage of opportunity and thereby take advantage of each other seems to be unproblematically assumed. Businesses are the source of jobs, jobs the source of wealth and success, and businesses just do what individuals do—take advantage of opportunities to gain wealth.

One political issue that is mentioned in a number of the propositions is prejudice. While opinions were divided about the morality and effectiveness of affirmative action, prejudice per se was always condemned and legal sanctions against prejudice presented as correct and necessary. The importance of prejudice as a topic seems to be due to the general structure of the model. The central motor of human social action in this model is the wish to succeed. Success is based on getting jobs and getting jobs is based on education. Within such a framework, prejudice strikes at the heart of the legitimacy of the system, because it blocks opportunities and keeps people from getting proper rewards for their achievements. If one thinks making use of one's talent and drive to take advantage of opportunities and get good jobs and make money is what society is all about, then prejudice is naturally seen as a great threat to way the system is supposed to work.

This model is remarkably similar to the Utilitarian model of Adam Smith. Individual self-interest is the primary relevant human motive as expressed through attempts to gain wealth and position. This is what makes society work. It is surprising how government and political parties are treated so peripherally by these informants, even though the topic for discussion was "social equality." The model is certainly not Marxist; there are no organized ideas about economic determinism, or exploitation, or class warfare, or false consciousness. The model is certainly not Durkheimian; the sacred is unmentioned, there is no clear conception of culture or of a collective conscience, and social solidarity is ignored.

An outline for this model is presented below.

The American Model of Society

1. There are different levels of American society based on wealth and social status.
2. In America people move up (and down) these levels.[8]
3. Success means either moving up (getting ahead) or staying at the top levels.
4. Money and social status motivate people to try to succeed.
5. People can succeed if they have opportunities, work hard, and have talent.
6. In America, people have more opportunity than in other countries.
7. Although people in America have equal rights, they do not have equal opportunities.
8. People have more opportunity to succeed if they come from families with money, or have special connections, or if they have good luck.
9. People who have worked to reach high levels deserve their wealth and superior position.
10. Everyone wants to be treated as an equal because it is painful to be treated as inferior.
11. People feel more comfortable with others who are similar to them with respect to wealth and social status because they feel equal to each other.
12. Differences in drive, talent, and opportunity produce differences in wealth and position.
13. Differences in wealth result in inequality of opportunity because the rich and well placed can give special advantages to their children with respect to education, social skills, and connections.
14. No group should be given special opportunities or privileges.
15. Prejudice is morally wrong.
16. Prejudice prevents people from receiving equal opportunities they should have and withholds proper rewards for achievement.

[8] The term "levels" is used here as a very general term for a position within a hierarchy. The term "class" has too many meanings to be useful here. For example, Americans are likely to say that (almost) everybody is middle class, but they certainly do not think everyone is on the same level.

A number of other propositions about society could be added to these 16. The boundaries of this kind of cognitive structure are not fixed. Networks of relationships may expand the structure in any direction. The propositions presented above are not an attempt to present all the ideas about society agreed upon by Americans, but to present some of the core ideas that an American must know in order to reason about the social world.

One likely response to this model is: "I already know this. I could have written this without all the abstracting of propositions, sorting, and frequency counts." Hopefully, the first sentence is true, since this model aims to represent cultural knowledge that by definition is *shared*. The second sentence is more problematic. Perhaps some people can, if the task is explained to them, write out something similar to the outline above. But in my experience, undergraduates produce different lists of propositions and any one respondent's list is likely to reflect special concerns and experience. Collecting a large sample of propositions makes it possible to get a better idea of what should be included in the model.

Obviously, verification of this model requires other kinds of data to help eliminate possible alternative models, such as a Marxist or a Durkheimian model. One might look at reasoning about social issues, or stories about the lives of individuals, or one might use the central relations in the model to generate a series of new propositions and ask a sample of Americans if they think these propositions are true or false.

Although lacking good verification, it seems to *me* likely that the outline presented above is at least part of a common cognitive structure used by Americans to understand and reason about society. (This is one of the dangers of using discovery procedures. After one has worked out a model, it is hard not to believe that it is true.) My hypothesis is that this Utilitarian-like model is experienced in the United States as *common sense*, although in fact it is a highly selective lens—a lens which is invisible to its viewers.

Some Final Remarks on Sample Size

One frequent reaction to work on cultural cognitive complexes is the outraged response "how can you even think of generalizing the results of a small number of informants to a whole country of two hundred and sixty million people?" There are answers to this.

If one looks at the verification demonstrations used in different disciplines, one discovers that different disciplines and even different fields within the same discipline have different norms for what constitutes an appropriate sample size. In many areas of biochemistry, for example, there are strong assumptions about the underlying sameness of biochemical reactions, so that something found in the cells of five or six mice can provide a strong demonstration of a universal process. What gives the demonstration such great reach is not any explicit rule, but, as I understand

it, simply the past experience of biologists that certain kinds of processes are found universally in living cells. The alternative, of course, is that some other creature has cells that respond differently. However, in many areas of biology the onus to find such a creature is on the person who does not believe the process is universal, since the high prior expectation is that it is.

The exploration of cultural models is not yet well enough established for there to be sufficient experience to justify good prior expectations for appropriate sample sizes. However, data collected so far indicates that cultural models tend to be strongly shared. Romney and associates (Romney, Weller, and Batchelder 1986) have found widely shared agreement among informants in a variety of cultural domains. For example Weller and Baer (2001) found, in comparing the beliefs about AIDS, diabetes, the common cold, empacho, and mal de ojo, that beliefs about each of these illnesses were very widely shared across groups by Puerto Ricans in Connecticut, by Mexicans in Guadalarjara and South Texas, and by rural Guatemalans. My personal estimate is that a sample of 20–30 is sufficient to obtain a reasonable estimate of the degree of agreement for the items of a cultural model.

Perhaps it would be helpful to look at some of the statistical issues. Let us suppose that a cultural model is part of the high concordance code of a society (Roberts 1964). Let us arbitrarily define a model as part of a society's high concordance code when each of its constituent propositions is shared by 95 percent or more of the population. If one asks a sample of 20 respondents whether or not they agree with these propositions, based on binomial probabilities one can expect that 92 percent of the model's propositions will be agreed to by 18 or more of the respondents. The point here is that it is not surprising that a small sample shows high agreement about certain things *if those things are highly agreed about in the whole population*. Perhaps what is surprising is that *anything* is agreed upon by 95 percent of the population of a large modern society! But that is exactly what the concept of culture leads one to expect.

Much of the attitude/belief research in sociology is oriented toward those attitudes and beliefs that vary across social divisions such as gender, class, ethnicity, and region. Typically, such research does not focus on beliefs and attitudes that are part of the high concordance code because shared beliefs yield no interesting information about social differences. Thus, it is surprising to many researchers who are used to standard survey work to encounter the high degree of consensus displayed by cultural structures. For example, Leon Zamosc, a sociologist at the University of California, San Diego who is interested in urban/rural differences in modern Spain, developed a questionnaire about the *meaning* of the city and countryside (the best and worst attributes of city life, the best and worst attributes of country life, etc.). He was surprised to discover that unlike many of the attitudes and beliefs he had previously studied (e.g., liberal versus conservative attitudes), the ideas about the city and the

country are very widely shared by both urban and rural respondents (Zamosc, personal communication). Or, to take a different example, Fabricius, discussing his and his associates work on the folk model of the mind, says: ". . . in our previous studies on folk psychology, we have used children and adults from both Arizona and Georgia and have yet to notice a single difference attributable to region (or socioeconomic status for that matter). Consequently, we feel that the folk theory of knowledge we have described is a rather monolithic one for Anglos." (Fabricius 1999:4).

After questioning a decade of undergraduates from a variety of ethnic groups and different social levels about Quinn's model of marriage without finding competing models, I have begun to wonder if there are any. Objections about sample size need to be supported by something more than outrage. At this juncture, to be fair, those who wish to raise the question of sample size should produce some evidence that something somewhere that contradicts what has been found so far in research on cultural models, and stop simply registering complaints.

Appendix: The Box that Made Noise

To apply Bayes's Theorem to the box and watch problem, let us imagine a world of boxes of a certain type (a watch size type) that either do or do not have watches in them. The first thing we must do is estimate the likelihood that the box we have in our hand has a watch in it *before* we hear whatever noise it makes. How can we make such an estimate? Well, we use past experience and treat the problem as if we were setting odds for a bet. Let us bet that there is one chance in five ($p = 0.20$) that this box has a watch in it, symbolized $p(\text{watch}) = 0.20$. Let the alternative hypothesis be that this box has no watch in it. The probability for the alternative hypothesis is 1.00–0.20, or 0.80.

So far we have:

$$p(\text{watch}) = 0.20$$
$$p(\text{no watch}) = 0.80$$

Next we need to estimate two conditional probabilities. An example of a conditional probability is the probability that some person is a blond *given* that person is a Swede, symbolized as:

$$p(\text{blond} \mid \text{Swede})$$

The first conditional probability to estimate is the probability that a box will make a noise if it has a watch in it. The second conditional probability to estimate is the probability that a box will make a noise if does *not* have a watch in it. Let us symbolize these as:

$$p(\text{noise} \mid \text{watch})$$
$$p(\text{noise} \mid \text{no watch})$$

How are we going to estimate these two probabilities? A box with a watch in it should have a higher probability of making a noise if shaken than a box without a

watch in it, because the box without a watch in it might have something soft in it that couldn't make a noise even when shaken. But how much higher? Again, we can only try to make a judicious bet. Something like the following seems plausible:

$$p(\text{noise} \mid \text{watch}) = 0.50$$
$$p(\text{noise} \mid \text{no watch}) = 0.10$$

These numbers indicate that we think that boxes with watches in them are *five* times more likely to make a noise when shaken than boxes without watches in them.

What we want to know is, given these estimates, how much does the fact that box makes a noise when shaken increase the probability that there is watch in the box? In Bayesian terms this is called the posterior probability and is symbolized:

$$p(\text{watch} \mid \text{noise})$$

The answer to this question can be calculated using Bayes's theorem. The formula for our example is:

$$p(\text{watch} \mid \text{noise}) = [p(\text{watch}) \times p(\text{noise} \mid \text{watch})] / [[p(\text{watch}) \times p(\text{noise} \mid \text{watch})] + [p(\text{no watch}) \times p(\text{noise} \mid \text{no watch})]]$$

This formula looks complex but it's proof is simple, requiring nothing beyond high school algebra, and can be found in most logic textbooks. Putting in the numbers:

$$p(\text{watch} \mid \text{noise}) = [p(\text{watch}) \times p(\text{noise} \mid \text{watch})] / [[p(\text{watch}) \times p(\text{noise} \mid \text{watch})] + [p(\text{no watch}) \times p(\text{noise} \mid \text{no watch})]]$$

$$p(\text{watch} \mid \text{noise}) = [0.20 \times 0.50] / [[0.20 \times 0.50] + [0.80 \times 0.10]]$$

$$p(\text{watch} \mid \text{noise}) = 0.56$$

Thus, given our estimates, the posterior probability is somewhat above 50 percent. We can be more confident there is a wristwatch than we were *before* we shook the box, but we are still some distance from reasonable certainty.

Well, a skeptic might say, I don't trust these results because I don't trust the initial estimates. So let's see what happens when we change the estimates. In this example there are really only two estimates we can change. The first is the initial estimate of how likely it is that there is a watch in the box. Below are some figures showing what happens to the posterior probabilities as the initial probability estimate increases and the other estimates remain the same.

Initial p(watch)	p(noise | watch)	p(noise | no watch)	Posterior p(watch | noise)
0.10	0.50	0.10	0.36
0.20	0.50	0.10	0.56
0.50	0.50	0.10	0.83
0.80	0.50	0.10	0.95
0.90	0.50	0.10	0.98

What we see is that the probability that there is a watch in the box, given that the box makes noise when shaken, increases as the initial estimate that there is a watch in the box increases. Also one can see from these figures, that one would have to start out pretty strongly convinced (85 percent) that there was a watch in the box to bring the level of certainty up to a reasonably high level (95 percent).

The ratio between the two conditional probabilities p(noise | watch) and p(no noise | watch) is the second parameter we can change. In Bayes's theorem, only the *ratio* between the conditional probabilities affects the posterior probabilities, not their individual levels. As the *ratio* of p(noise | watch) to p(no noise | watch) increases, the posterior probability that there is a watch in the box increases. This makes intuitive sense. The better the occurrence of a noise is at discriminating between boxes that have watches in them from boxes that do not, the more certain we can be about whether there is watch in the box if we hear a noise. The figures below show how this ratio affects the posterior probability that there is a watch in the box given the initial 20 percent probability estimate.

Ratio of p(noise | watch) to p(noise | no watch)	Initial p(watch)	Posterior p(watch | noise)
2 to 1	0.20	0.33
5 to 1	0.20	0.56
10 to 1	0.20	0.71
20 to 1	0.20	0.83
100 to 1	0.20	0.96

As the ratio of p(noise | watch) to p(no noise | watch) gets larger, the probability that there is a watch in the box increases. But given the initial 20 percent probability of there being a watch in the box, one would have to believe that a box with a watch in it was a hundred times more likely to make a noise when shaken than a box without a watch in it to reach a strong level of conviction. Here in numerical form is the argument made in the body of the paper: *Given that one is not already pretty sure that one's hypothesis is right, only if one's evidence strongly rules out the alternative hypothesis can one say that some hypothesis is verified.*

Bayesian ideas like these make some scientists uneasy because so much depends on the estimates one begins with. What do we do about the fact that different folks have different initial estimates? That depends on whether or not one cares what other folk think. If we care, then we have to use *their* initial estimates and try to obtain further evidence until the posterior probabilities increase to the point that even they, if they are rational (i.e., they follow Bayes theorem), have to agree that our hypothesis is very likely.

Unfortunately, if they set the initial probability of our hypothesis at zero, no amount of evidence will ever change their minds (because the numerator of the Bayesian equation will always be zero, and therefore the final result will always be zero). That is why Bayesians do not argue with true believers. However, if we are merely dealing with skeptics who are reasonable, then we can use their estimates and our evidence (if we have enough of it, and it is strong enough) to bring them eventually to agreement about our hypothesis.

References

Campbell, Donald. 1986. Science's social system of validity enhancing collective belief change and the problems of the social sciences. In *Metatheory in social science*, ed. D. Fiske and R. Shweder, pp. 108–135. Chicago, IL: University of Chicago Press.

Craik, Kenneth. 1943. *The nature of explanation.* Cambridge, England: Cambridge University Press.

D'Andrade, Roy G. 1976. A propositional analysis of U.S. American beliefs about illness. In *Meanings in anthropology*, ed. K. Basso and H. Selby, pp. 155–180. Albuquerque, NM: University of New Mexico Press.

———. 1987. The folk model of the mind. In *Cultural models in language and thought*, ed. D. Holland and N. Quinn, pp. 112–147. Cambridge: Cambridge University Press.

———. 1995. *The development of cognitive anthropology.* Cambridge, England: Cambridge University Press.

Fabricius, William V. 1999. Cultural variation in the development of folk theories of knowing. Proposal submitted to the National Science Foundation.

Feynmann, Richard. 1985. *QED: The strange theory of light and matter.* Princeton, NJ: Princeton University Press.

Gentner, Dedre and Keith J. Holyoak. 1997. Reasoning and learning by analogy. *American Psychologist* 52:32–34.

Gladwin, Thomas. 1970. *East is A big bird.* Cambridge, MA: Harvard University Press.

Hempel, Carl G. 1965. *Aspects of scientific explanation.* New York: Free Press.

Hutchins, Edwin. 1980. *Culture and inference.* Cambridge, MA: Harvard University Press.

Lillard, Angeline. 1998. Ethnopsychologies: cultural variations in theories of mind. *Psychological Bulletin* 123:1:3–32.

Linger, Daniel. 1992. *Dangerous encounters.* Stanford, CA: Stanford University Press.

Mandler, George. 1985. *Cognitive psychology: An essay in cognitive science.* Hillsdale, NJ: Erlbaum.

Mead, Margaret and Rhoda Metraux. 1953. *Culture at a distance.* Chicago, IL: University of Chicago Press.

Parish, Steven. 1991. The sacred mind: Newar cultural representations of mental life and the production of moral consciousness. *Ethos* 19:3:313–351.

Quinn, Naomi. 1978. Do Mfantse fish sellers estimate probabilities in their heads? *American Ethnologist* 5:2:206–226.

———. 1987. Convergent evidence for a cultural model of American marriage. In *Cultural models in language and thought*, ed. D. Holland and N. Quinn, pp. 173–192. Cambridge, England: Cambridge University Press.

———. 1992. The motivational force of self-understanding. In *Human motives and cultural models*, ed. R. D'Andrade and C. Strauss, pp. 90–126. Cambridge, England: Cambridge University Press.

———. 1997. Research on shared task solutions. In *A Cognitive theory of cultural meaning*, ed. C. Strauss and N. Quinn, pp. 137–188. Cambridge, England: Cambridge University Press.

Reichenbach, Hans. 1938. *Experience and prediction.* Chicago, IL: University of Chicago Press.

Roberts, John M. 1964. The self-management of cultures. In *Explorations in cultural anthropology: essays in honor of George Peter Murdock*, ed. W. H. Goodenough, pp. 433–454. New York: McGraw-Hill.

Romney, A. Kimball, Susan C. Weller, and William H. Batchelder. 1986. Culture as consensus: A theory of culture and informant accuracy. *American Anthropologist* 88:313–338.

Schwanenflugel, Paula J., Fabricius, William V., Noyes, Caroline R., Bigler, Kelleigh D., and Alexander, Joyce M. 1994. The organization of mental verbs and folk theories of knowing. *Journal of Memory and Language* 33:376–395.

Strauss, Claudia and Naomi Quinn. 1997. *A cognitive theory of cultural meaning.* Cambridge, England: Cambridge University Press.

Wallace, Anthony F. C. 1964. On being just complicated enough. *Proceedings of the National Academy of Science* 17:458–461.

Weller, Susan C. and Roberta D. Baer. 2001. Intra- and intercultural variation in the definition of five illnesses: AIDS, diabetes, the common cold, empacho, and mal de ojo. *Journal of Cross-Cultural Research* 35:201–226.

Wellman, Henry M. 1990. *The child's theory of mind.* Boston, MA: MIT Press.

Wierzbicka, Anna. 1996. *Semantics, primes and universals.* Oxford: Oxford University Press.

Chapter Four

Uncovering Cultural Models of Gender from Accounts of Folktales

Holly F. Mathews

In a 1986 essay, the folklorist Claire R. Farrer raised an important yet still largely unanswered question: "How are a culture's perceptions of women and expectations of them expressed folkloristically"? (1986:xviii). Although many investigators have collected folklore by and about women (Fischer 1956, Hymes 1971, Sapir 1977, Dwyer 1978, Baldwin 1985, Mitchell 1985, Rowe 1986, Mark 1987, Chernala 1988, Gottleib 1989, Crain 1991) few have attempted to analyze systematically the ways in which cultural models of gender are expressed in and give meaning to such discourse (for an exception see Taggart 1990[1]), or conversely, the role that linguistic forms play in the representation and communication of these models. Gender ideology is constructed in discourse both through what women and men do with language as well as through what speakers say about women and men. Thus, it is a central contention of this article that the study of linguistic form cannot be separated from a study of the cultural content conveyed within a particular genre.[2] It is important, moreover, to identify a set of methodological techniques that can enable us to recover

[1] Taggart's 1990 work examines gender relations in Spanish folktales of courtship and marriage. He analyzes 69 accounts of several common European folktales collected from male and female storytellers in Spain and presents some contrasting material from his Nahuat research in Mexico. Taggart's approach is somewhat different from the one I follow in this chapter. He does not do a structural analysis of the features common to the tales. Instead, he focuses on broader psychological themes, especially as these are adapted to family life in the region. He also compares the personal experiences of the narrators to the content of their stories and examines their roles as responses to the ideas that one gender has about another (1990:12).

[2] One of my initial sources of inspiration in studying the *La Llorona* tales was Hymes' (1971) structuralist analysis of the Clackamas Chinook myth, "Seal and Her Younger Brother Lived There," previously interpreted by Melville Jacobs (1960). Because Hymes treats folktales not as "traditional" or "authentic" texts, but rather as sets of underlying rules, he shifts the interpretive focus to productivity. This shift allows for the possibility of variation in both the construction and interpretation of tales. In other words, Hymes argues that the members of a culture have the ability to use the implicit knowledge of a genre structure to interpret tales, retell them, and assimilate new materials into older genre forms (1971:52).

from folklore the cultural models that organize people's understandings of the world and that can enable us to demonstrate how such cultural understandings guide the production of linguistic forms like folktales.

In the following sections, I outline the steps I took to find such a set of workable methods as I analyzed 60 accounts of a single folktale, the story of *La Llorona* or the "Weeping Woman," collected from a sample of 30 male and 30 female informants in a Mexican community in which I had worked previously[3] (see also, Mathews 1982). Although these accounts are all versions of the same base folktale, they vary considerably in the events recounted, in the motivations attributed to the main characters, and in the moral meanings derived from the characters' actions. The challenge that I faced was how to understand the meaning of this variability, especially with regard to gender. While it was obvious to me as I collected the data that male and female accounts differed significantly, it was equally clear that variations existed among the tales told by women as well as among those told by men. I knew that unless I could more precisely specify both the nature and the range of variation found in these accounts, I would be unable to theorize about the relevance of the tales for understanding cultural models of gender or to explain how the different versions were used by community members in everyday contexts to evaluate the actions of themselves and others.

In this chapter, I describe in some detail three of the methodological approaches that I employed in this endeavor and the modifications that I made to each in the process of completing my analysis. In particular,

---

Thus he disagrees with Jacobs and concludes that the Clackamas myth under consideration tells something about the aboriginal society's feelings about women but from a female perspective, a fact confirmed by the myth's transmission through a line of women. Hymes goes on to warn readers that it would be foolish to assume that aboriginal audiences had a uniform interpretation of myths. Instead, he notes, "the persistence of interpretive differences between men and women can be documented even today" (1971:76).

[3] Sixty versions of the *La Llorona* tale, 30 from men and 30 from women, were collected during field research between 1983 and 1985 in a Oaxaca, Mexico community of 2000 inhabitants in which I had worked previously (see Mathews 1985 for a more complete description of the community). In addition, I also collected in-depth interviews with a different, stratified sample of 50 male and female informants and a set of life histories from a third, distinct sample of 30 male and female informants. All these materials as well as extensive participant-observation informed the analysis of the folktales presented in this article. Community residents have a Zapotec heritage although they increasingly define themselves as *mestizos*. The majority of inhabitants are subsistence farmers, and family farms traditionally passed to the eldest son. Recently, however, rates of male outmigration to the United States are on the rise. While kinship is traced bilaterally, the ideal residential unit is the patrilocal extended family, and brides are expected to move after marriage to reside with the husband's family. Twenty years ago most marriages were still arranged by parents, and future spouses had limited interaction before they married. Today, the arranged system is breaking down and an increasing number of youth select their own marriage partners, although parents are still quite influential in either approving or vetoing such choices (see Quinn and Mathews 1998 for a more complete description of notions of love and patterns of marriage in the research community).

I discuss the development of a story grammar to describe the major plot units or moves structuring the tales; the delineation of the major tale types as defined by the sequences of functions and subactions attributed to the main characters; and the construction of models of mediation, derived from Lévi-Strauss' (1958/1976) structuralism, to depict the various ways in which these tales attempt to resolve the fundamental opposition posed between the main characters.

My approach to the analysis of these tales has been eclectic from the beginning, both because my interest in folktales bridged disciplinary boundaries and because my own theoretical orientation toward the study of culture and cognition, as well as my sense of the key issues being addressed in the folktales, shifted over time. Thus while my discussion of the analyses undertaken and the methods utilized is presented sequentially, my actual work on the folktales was much more intuitive and discursive. Sometimes I alternated between analytic techniques as I focused on different problems raised in the folktales. When a particular method no longer seemed fruitful, I was often spurred to modify it or look for a new way to analyze the data. Alternatively, on at least two occasions, my theoretical understanding of the issue under investigation shifted, and this led me to rethink previous conclusions. Fundamental to my approach throughout, however, was a commitment to systematic methods that could be used to account exhaustively and reliably for the patterns found in the folktales, while also yielding new insights into the complex relationship that obtains between cultural models and discourse.

The structure of this chapter, therefore, is somewhat complex. I begin by providing background on the folktale itself and describing the steps leading up to my research plan. Next, I review briefly the different structural approaches to the analysis of folktales that I employed and contrast these with theoretical developments in cognitive anthropology, emphasizing the importance of hierarchically related schemas in organizing knowledge. I then review the three methodological techniques used in this study and highlight the strengths and limitations of each with reference to the actual data analyses produced. Finally, I conclude with summary comments on what these data suggest about the relationship of cultural models of gender to the discourse about men and women encapsulated in the folktales.

Background to the Study

La Llorona is a figure of considerable importance in Mexican history. She is variously depicted as a grieving widow weeping for her orphaned children, a virgin bride who dies on the eve of her wedding, a seductive siren followed by men, and a disgraced woman murdered by a jealous husband (Mirande and Enriquez 1979:32). In all cases, she has returned to earth and is said to wander the streets wailing and weeping in the dead of

night. Men often follow her, but she always disappears or floats out over the water and becomes submerged. While interpretations of the events leading up to her brief and subsequent death have differed over time and place[4], all versions of the tale attempt to inhibit immoral behavior by illustrating the terrible consequences that may result from a failure to act in accordance with culturally prescribed notions about gendered human nature and the life goals valued by the members of each sex as well as about spousal duties and obligations in marriage.

My initial dissertation fieldwork in Mexico focused on describing women's roles and levels of participation in the traditional civil and religious hierarchy known as the *cargo* system (see Mathews 1985). I also attempted to correlate those behavioral data with the perceptions of male and female characteristics held by community members as ascertained through a series of formal elicitation tasks. I asked samples of male and female informants to list the different types of men and women found in the community and to sort them on the basis of similarity. A cluster analysis of these data indicated that female types were more likely to be grouped together on the basis of ascribed or personalistic criteria while male types were more often sorted by achieved criteria. A further examination of the types contained within clusters revealed a striking difference in the perception of men and women in the community. Both male and female informants grouped sexually adventurous, adulterous, and aggressive men together and evaluated these as positive types, while they grouped sexually adventurous, adulterous, and aggressive women together as negative types (Mathews 1982:153). Thus, although women and men tended to participate equally and in parallel roles in the community's civil and religious government, women were more often judged negatively than men on the basis of certain personal and behavioral characteristics. These task groupings, moreover, seemed to correlate with a prevailing ideology of male dominance in the community.

Yet, I could sense that the description of the gender domain derived from the use of these formal, elicitation techniques was incomplete. I had spent enough time in the community to recognize that people were drawing upon more complex understandings of gender in the process of interpreting each other's behaviors, making decisions about how one acts in particular situations, and in evaluating their own life situations, particularly within the context of marriage.[5]

I began to suspect that clues to these broader issues might be found within folklore. I had heard, previously, many references in casual conversation to

[4] For a more detailed discussion of the history of the *La Llorona* legend and of variations in the tales told in different locales, see Janvier 1910, Kirtley 1960, Horcasitas and Butterworth 1963, Kearney 1969, 1972.

[5] The research of Holland and Skinner (1987) describes a similar methodological progression in the understanding of an American gender types. Initially they used multidimensional scaling, another type of cognitive structure analysis, to sort types of males and females as

the figure of *La Llorona*. But a key episode during my second research stay illustrated the importance of these tales in encapsulating important information about gender. One afternoon, a young wife in the family with whom I lived came back from running an errand for her mother-in-law. She had been gone a long time and her father-in-law, who had been sitting in the courtyard all day, was unhappy with her behavior. He scolded her for lingering in the streets and gossiping with others and said, "If you aren't careful, you are going to end up like *La Llorona*." Clearly this reference was meant as a corrective to inappropriate behavior, and both individuals understood its meaning even though the tale itself was not recounted in the course of the conversation. Indeed, the chastened daughter-in-law responded by retreating to the kitchen and resuming her chores while weeping quietly. When I later asked the father-in-law who *La Llorona* was, he recounted the following tale:

> *La Llorona* was a young woman married to a good man. They worked together and had children and all was well. But *La Llorona* liked to gossip and she began to walk the streets and was often gone. She did not feed her husband or tend to her children. When her husband found out, he beat her. And she was filled with shame (*pena*) and so she drowned herself in the river. And that is why my daughter-in-law should be careful not to end up the same way.

The next day I had a chance to ask the young woman what she thought her father-in-law had meant by the reference to *La Llorona*. She responded:

> *La Llorona* is a wandering spirit. She was once a woman who was happily married. But then she started to walk the streets instead of doing her work at home. When her husband found out, he was angry and beat her. She was very sad (*triste*) because everyone knew, so she drowned herself. So my father-in-law is warning me to be careful so I don't end up the same way.

As we continued talking, however, she said that she had always heard the story differently from her own mother. I asked her to tell me that version and she replied:

> *La Llorona* was an innocent, young girl married to an older man. At first all was well. But he began to drink and spend money on other women. When

---

reported by college students along different dimensions. Further research analyzing in-depth interviews and "talking-diary" materials led the investigators to conclude that the cognitive structure methods were not adequate for understanding how American students actually talked about such types and used those types to judge the behaviors of their peers. Instead, the investigators found that talk about gender types was constructed in relation to a prototypic scenario or taken-for-granted model of how relationships between males and females develop. Scenarios of disruption are associated cognitively with this prototypic model or schema, and most gender-marked types are the types who cause such disruptions.

> she asked him to give her money for food, he cursed her and beat her in front of the family. So she killed herself. And now she often comes back as a beautiful spirit dressed in white. And when men are out drinking and running around on their wives, they often see her and follow her into the river and die. And all because they do not treat their wives well.

Needless to say, I was struck by the differences in the male and female versions of this common folktale and intrigued that the daughter-in-law understood, but did not necessarily embrace, the male perspective on the tale that was used to reprimand her. As I talked with more individuals, it became clear that the folktale was a popular reference point for understanding and commenting on the behaviors of men and women, and that substantial variation existed in the way that it was told. I subsequently developed a research proposal around this topic and returned to the community for a third stay in order to examine the folktale and the ideas about gender it drew upon as well as conveyed more systematically.

Theoretical and Methodological Models from Folklore and Anthropology

Because folklorists have long been interested in analyzing the constraints of genre on the content of particular tales, I looked to their methods initially for help in specifying the range of variation that existed in the form of the folktales I collected and in the sequences of actions undertaken by the characters in those tales. Scholars interested in folklore historically emphasized, as Hymes (1971) points out, the collection of "authentic," or "traditional" tales, which they assumed had passed down unaltered from generation to generation. This approach contrasts with a more recent view, derived from transformational grammars in linguistics, of folktales as actively generated from a shared system of underlying rules (Hymes 1971:50–53). These two views have led to very different methodological approaches to the analysis of folktales. The former emphasizes the tale as a text or product requiring analysis as an integrated system of psychological and symbolic meanings without regard to the structural elements comprising it. These interpretations, of necessity, rely heavily on intuition and, as Leach points out, "rest ultimately on a number of sweeping presuppositions about the psychological unity of mankind" (1971:23). Such interpretations, moreover, are seldom compared for verification to those of native respondents since alternative versions of the same folktale or text are often seen as novel or inauthentic and therefore compromised productions. While many of these psychosocial interpretations have been insightful, the danger in such an approach, as Hymes warns, is:

> the greater temptation to take a shortcut, to assume that a purportedly universal theory, be it psychoanalytical . . . dialectical, or whatever, can go

> straight to the heart of a myth before having considered its place in a genre structurally defined and functionally integrated in ways perhaps particular to the culture in question. (1971:51)

Yet, as Hymes demonstrates in his reanalysis of a Clackamas Chinook myth, variants of a tale may exist within a single culture and these may reflect differing interpretations about the point of the story. If the participants in a culture possess knowledge of a genre (i.e., of the rules used to structure a locally appropriate type of tale), then there can be no one authentic or true version. Instead, the act of telling is a generative act where individuals use the same underlying structural principles to reinterpret older tales or to produce new and innovative ones. What is identified as the same genre by analysts (i.e., folktales, myths), moreover, may differ significantly, Hymes contends, in the structural characteristics and functional role served ethnographically within individual cultures. Therefore, he argues, "one must define a genre in terms of features and relationships valid for the individual culture; and that the meanings and uses of individual texts are to be interpreted in the light of the formal features and relations found for the native genre" (1971:51). I would certainly add to Hymes' statement that finding a native genre does not preclude its universality, only that specifying the genre and its structural aspects within a culture is a necessary first step before further comparative work can be undertaken.

Structural analyses premised on these transformational linguistic principles were stimulated as well by attempts to account for multiple types of folktales within broad genre groups or for variant accounts of the same folktale found within individual cultures. There have been two major structural approaches to the study of folktales. The first, pioneered by Propp (1958/1986) in his study of 100 different Russian folktales, attempts to discover the sequential, compositional structure of the plot, defined as a series of functions and subactions undertaken by the dramatis personae or the main characters. Such a technique is valuable in enabling the investigator to specify more precisely the ways that different accounts of a folktale vary, but in and of itself, does not provide a theory for why such variation occurs.

The second structuralist approach, derived from the work of Lévi-Strauss (1958/1976), is premised on the assumption that myths and folktales exist to resolve fundamental oppositions through the process of mediation, which they accomplish with varying degrees of success (Kongas and Maranda 1962, Dundes 1971:171). The analyst, therefore, must look beyond the sequential ordering of surface content in order to ascertain this "deep structure" or pattern of opposition and mediation, which is thought to be the actual organizing principle of the myth or tale. While this approach is valuable in focusing the investigator on the "bigger" conflicts that a folktale is attempting to resolve, structuralists traditionally have been more interested in delineating and manipulating the structural relations

among units that they assume characterize all tales and myths than in understanding and explaining the culturally specific nature of such conflicts.

My ongoing struggle to understand the implications of the ways in which men and women communicated assumptions about gender in the *La Llorona* tales for interpersonal relations in the community coincided with a shift in the way that cognitive scientists, including cognitive anthropologists, were thinking about the organization of knowledge in the human mind (see Quinn's chapter in this volume). In the 1970s, as D'Andrade points out, cognitive scientists from a variety of disciplines began to embrace a new theory about knowledge that emphasized the importance of schemas as mental devices for organizing experience (1995:122–123). Strauss and Quinn further specify the nature of schemas, noting that "schemas, as we think of them, are not distinct things but rather collections of elements that work together to process information at a given time" (1997:49). In other words, schemas are the generic versions of experience that remain in memory. Such schemas, they contend, sometimes reconstruct our memories of past events, determine the meanings we impart to ongoing experience, and give us expectations for the future (1997:49). We might, therefore, define schemas as learned expectations about the way things usually go. To the extent that such expectations are shared, they are cultural, and these cultural schemas not only supply individuals with interpretations of their experiences and inferences about them, they also generate goals, and, when wedded to affect, supply the motivational force individuals need to take action in the world (Strauss 1992).

Schemas, D'Andrade points out, are also hierarchically organized such that a simpler or more restricted schema may be embedded in or recruited by a more complex or general one as people reason or make decisions about how to act in the world (1995:124). The term cultural models has been used by many analysts to refer to more conceptually complex, cultural schemas. These schemas or models, as Strauss and Quinn define them, "connect and organize an interrelated set of elements and hence not only delineate but serve as working models for entire domains of activity in the world" (1997:140; see also Quinn and Holland 1987, D'Andrade 1995:151–152).

For example, a friend recently told me about meeting a man for a blind date in a local restaurant. They were so taken with one another that at the end of the evening they left without paying the check. This snippet of a story is all about the "falling in love at first sight" model, which is conceptually complex and shared by many Americans. In this episode, however, parts of the model are being communicated with reference to the more conceptually restricted "restaurant" schema (cf. Schank and Abelson 1977:42–57). Because Americans all know the steps of the restaurant schema (entering, ordering, eating, and paying for the food), we understand that this violation of the schema (forgetting to pay) is meant to imply that the couple was so overwhelmed by their deep and instant attraction that logic and reason flew out the window, a key component of the "love at first sight" cultural model (see McCollum 2000).

Much of human discourse, D'Andrade writes, makes reference to unstated schemas and cannot be understood without knowledge of them (1995:125). This is particularly true of folktales. Because they are so strongly conventionalized in form, folktales must encapsulate a great deal of cultural wisdom economically. In the course of my research, I came to realize that they often do so by referencing elements of widely shared cultural schemas or models without stating them explicitly. I also discovered that the form in which folktales are conveyed (i.e., the genre) can itself be viewed as a type of cultural schema, which then shapes the way that content derived from broader cultural models is expressed in communicating specific messages. In the following sections of this chapter, I review the three methods that I used to analyze the tales and demonstrate how I arrived at these conclusions about the importance of interacting schemas.

Development of a Story Grammar

To begin untangling this relationship between the form of the tales and the content conveyed, I first attempted to analyze form by determining the significant units or components that comprised the *La Llorona* tales recounted by my informants. I wanted to know whether all variants contained specific elements and if so, what those were. I also wanted to know if there was a fixed order in which certain elements appeared in the tales and if variability in the ordering of elements ever occurred. As the folklorists E. K. Maranda and P. Maranda (1971) point out, the first task in such a structural study of narratives is to find the "phonemes" in the corpus of discourse. They write, "This can be done, as in language, only by studying the organization of the corpus itself and by defining the differences which make a difference, those features that are distinctive" (1971:22). With folktales, researchers usually begin with the largest units first—those key elements of the plot that make a difference in the action of the story. Propp (1958/1986:92) labels these units *moves*, which as B. N. Colby and L. M. Colby (1981:168) clarify, consist of a motivating problem and the events that lead to its solution. In the Russian folktales studied by Propp, the moves consist of a hero attempting to solve a problem caused by villainy or by something lacking. Propp found that the plots of a hundred different Russian folktales all consisted of the same moves sequenced in the same, predictable fashion. I subsequently reviewed the work of Colby (1973) and the modifications he made to Propp's method in developing a partial grammar for published versions of Eskimo folktales and for the tales he elicited from an Ixil Maya diviner (with Lore M. Colby 1981).

None of these works, however, seemed quite applicable to the analysis of plot moves in the *La Llorona* tales, in part, I now believe, because of elements that may be unique to the genre of morality tales. Morality tales

are concerned with evaluating and shaping courses of action; they are all about people doing bad or immoral things that cause problems for others who are trying to live in the culturally prescribed way. Hence they contain a great deal of information about cultural expectations for behavior as well as about the bases of individual motivations for that behavior. The *La Llorona* tales, in particular, focus on a mythical woman's marriage and the problems she encounters relating to her husband. The plot involves the frustration by one spouse of the marital expectations of the other, and it unfolds as the two main characters attempt and ultimately fail to solve their problems. Failure is the key since it sets up the moral at the end that enjoins the audience from making the same mistakes as the characters in the stories. Consequently, these morality tales differ significantly from the Russian folktales studied by Propp. In the *La Llorona* story, there is no hero. Rather, there is a couple living happily until one begins to cause problems. So the two main characters are opposed from the beginning as protagonist and antagonist. Thus, as might be expected, the key plot units or moves, which oppose one character to the other and involve actions and reactions by each, are somewhat different in these tales than in the Russian ones.

In my initial attempts to understand the structure and sequencing of the main plot moves in the tales, I began by looking at the individual accounts and abstracting the key units or moves as I defined them. I then attempted to model the sequences of actions in the plots of individual accounts, looking for corroborating examples as well as exceptions in other versions. While I felt that I was having success in establishing that a limited number of plot moves characterized the folktale, I did not have a good way to think about how these units or moves were structured internally in relation to one another. I could see that episodes were often embedded in prior episodes, and it appeared that embedding occurred under some conditions but not others in the tales. I came to realize that part of my difficulty stemmed from the fact that while the structural methods of Propp and others involved inductive attempts to find regularities in the data, these methods did not derive from any theoretical framework that would help the investigator begin to anticipate and explain variability in an overall pattern of sequencing. Because of my interest in schema theory, I began to look at some of the work that cognitive psychologists like Abelson (1975), Rumelhart (1975), Johnson and Mandler (1978), and J. Mandler (1984) had done in developing grammars for American and European stories of various types, ranging from simple to complex, in order to find some way out of this methodological impasse.

Story grammars derive from transformational generative grammars. In the latter, rules, known as "rewrite rules," are formed to generate certain elements of sentences. For example, the entity SENTENCE can be rewritten as a: NOUN PHRASE plus a VERB PHRASE. This would be diagrammed in a transformational grammar as:

$$\text{SENTENCE} \rightarrow \text{NOUN PHRASE} + \text{VERB PHRASE}.$$

Similarly, we might find that the entity STORY can be rewritten as a SETTING plus an EPISODE. This would be diagrammed in a story grammar as:

$$\text{STORY} \rightarrow \text{SETTING} + \text{EPISODE}$$

Setting and episode are technical terms just like noun and verb phrase. They describe elements of the story's structure regardless of the particular instance of setting or episode being represented and follow rules that tell how they are to be treated in the story. Thus, a story grammar can be conceptualized, J. Mandler notes, as:

> . . . a rule system devised for the purpose of describing the regularities found in one kind of text. The rules describe the units of which stories are composed, that is, their constituent structure, and the ordering of the units, that is, the sequence in which the constituents appear. (1984:18)

I drew inspiration from this approach in devising my own grammar for the *La Llorona* tales. The process of constructing it involved working with individual accounts in an effort to abstract the key units or moves, to construct the ordering sequences of moves, and to formulate rules that accounted for the sequences found. These rules, framed as tentative hypotheses, were then applied to additional versions to see how well they succeeded in accounting for the moves present in them. The grammar was modified until it could account exhaustively, with the rules specified, for the moves and the order structuring them as found in all the variants of the tales collected. This final grammar is represented as a set of rewrite rules in table 4.1.[6] My initial contention was that any knowledgeable person listening to a *La Llorona* tale would use this story grammar or *schema* to parse the tale into these constituent elements and their interrelations. However, as I developed the story grammar, I eventually came to realize that for listeners to understand the tales, they also had to draw upon a number of culturally shared schemas or models of different types which, although not directly stated, gave meaning to the events, or plot moves, enacted in the story. These upper level schemas are discussed in more detail in the next section and include cultural models of marriage and life goals embraced by the members of each sex.

[6] My grammar for the *La Llorona* tales incorporates elements developed in the grammars of J. Mandler (1984) and Rumelhart (1975). The main difference is the depiction of the SETTING as a nonterminal node incorporating both an introduction of characters and a scenario depicting the ideal conditions existing prior to the beginning of the immoral acts. Similarly, I have rewritten the ENDING node as one that incorporates both an outcome to the episode(s) and a moral that involves commentary on the preceding events in the story. The remaining difference is some refinement of the elements in the beginning and development constituents to capture more accurately the actions occurring in the tales.

Table 4.1 Rewrite Rules for a Story Grammar of *La Llorona* Morality Tales

STORY	→	SETTING and EPISODE
SETTING	→	introduction and scenario
EPISODE	→	{ BEGINNING cause DEVELOPMENT cause ENDING EPISODE ({ and / then } EPISODE)^n }
BEGINNING	→	(precipitating { event / episode }^{n} then) initiating event
DEVELOPMENT	→	{ COMPLEX REACTION cause PLAN emotion cause desire DEVELOPMENT (cause DEVELOPMENT)^n }
COMPLEX REACTION	→	emotion cause desire
PLAN	→	(preaction)^n then action cause CONSEQUENCE
CONSEQUENCE	→	{ event^n / EPISODE }
ENDING	→	OUTCOME (and moral)
OUTCOME	→	{ Ending event / EPISODE }

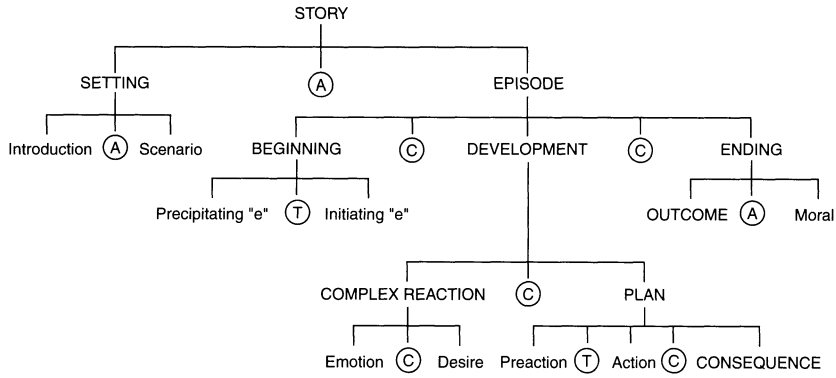


Figure 4.1 A Diagram for the Rewrite Rules for a Morality Story Grammar

Notes
A—and; spatial connection between elements
T—then; temporal connection between elements
C—causal; causal connection between elements

In table 4.1, I have followed the conventions for rewrite rules employed by J. Mandler (1984:24). Nonterminal units that can be rewritten into other units are shown in upper case letters. Terminal nodes are not rewritten into other units but are expressed by the states and events that make up the actual events of the tales. These are written in lower case. Brackets indicate mutually exclusive choices in the rewriting of a nonterminal node;

that is, any option may be used, but only one can be used at a time. Simple parentheses indicate optional elements. Superscripted parentheses indicate sets of elements that are used at least once when the rule is applied but may be used recursively (cf. J. Mandler 1984:23–25). These rules are capable of generating any of the folktales I collected because episodes may be embedded in other episodes at certain key points. The rewrite rules are also diagrammed schematically in figure 4.1.

The *La Llorona* tales consist of a *setting* and an *episode*. The setting's *introduction* presents the main characters, the locale, and sometimes the time at which the story takes place. The setting also contains a *scenario*, which depicts how an ideal marriage is proceeding and, occasionally, makes a comment on the emotional status of the partners to that ideal marriage.

The body of the folktale consists of one or more episodes that comprise the plot of the story. These episodes, regardless of content, follow a particular form. Each consists of three constituents: a *beginning constituent*, a *development* and an *ending constituent*. The beginning consists of both a *precipitating event* and series of events (i.e., episode) and an actual *initiating event* undertaken by the antagonist character. In the content of the tales, the precipitating event(s) set in motion the condition for the violations of marital expectations while the initiating event is the actual transgression. It is this transgression that elicits a responding reaction from the protagonist character.

The second constituent, the *development*, has several parts. The first thing that happens is that the offended spouse or the protagonist reacts to the antagonist's transgression. This *complex reaction* component in the tales includes an experience of *emotion* that is shown to cause a *desire* that, in turn, leads the protagonist to formulate a *plan* to achieve it. The plan itself may consist of certain *preactions* necessary to bring about the desired response action or it may only involve the response action itself, which meets the aim stimulated by the desire. The *consequence* of the action may be either success, if the protagonist spouse is able to correct the antagonist spouse's misbehavior, or failure if the antagonist begins another episode of misbehavior. Thus the *consequence* may cause the ending of the story or it may lead, instead, to the enactment of another, embedded, episode of misbehavior.

The *embedded episode* usually involves a *complex reaction* by the offending or antagonist spouse to the action just perpetrated by the protagonist. This second, *embedded episode* will, like the first, have a beginning, development, and an ending constituent consisting of these same elements. Episodes conclude with an *ending constituent* that involves both an *outcome* (either an event or an embedded episode) and some kind of commentary on the preceding events—the *moral*.

To show how this grammar depicts story structure, I have diagrammed two examples of the *La Llorona* tales in figures 4.2 and 4.3. These are accompanied by the text of the tales broken down into numbered propositions in table 4.2. The numbers in the grammar correspond to the

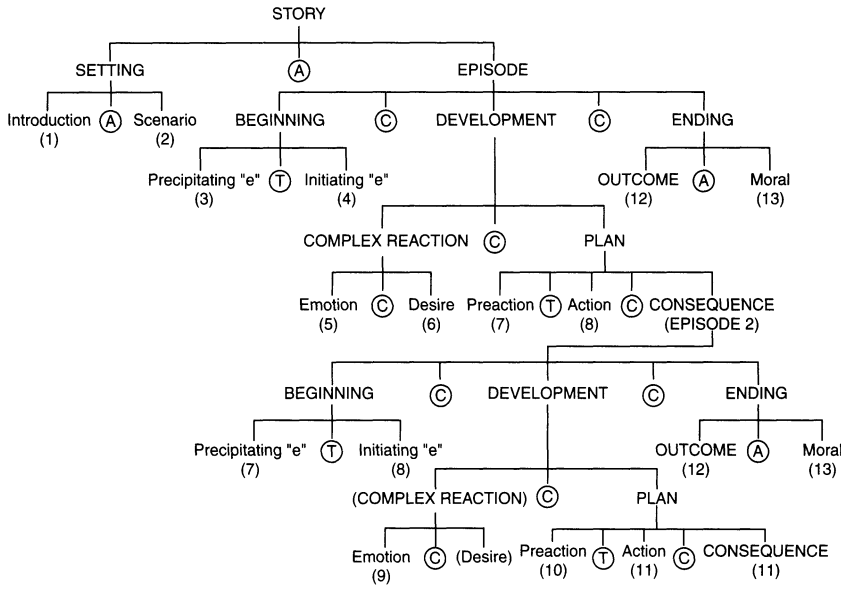


Figure 4.2 Diagram of Rewrite Rules for Folktale M-30

Note: Parentheses enclosing a constituent indicate that it was not explicitly mentioned in the folktale.

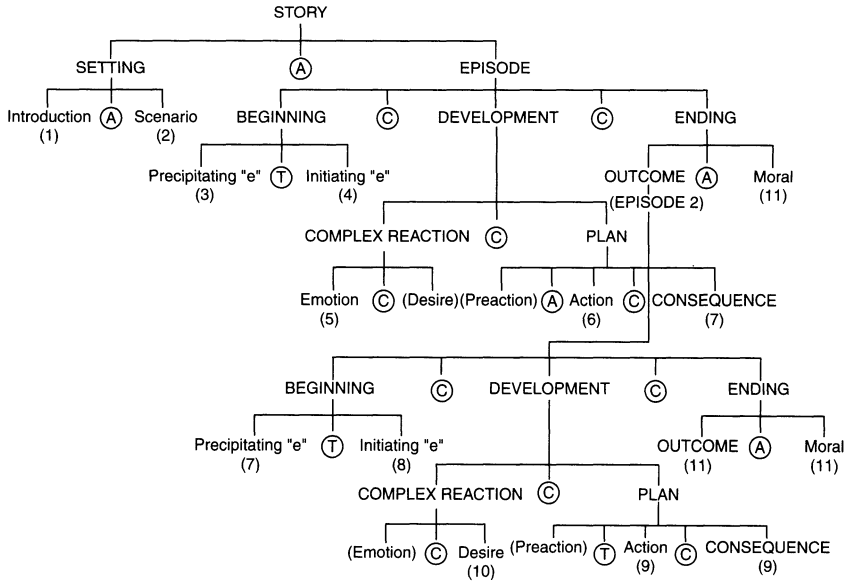


Figure 4.3 Diagram of Rewrite Rules for Folktale F-28

Table 4.2 Texts and Functional Analyses for Two Versions of the *La Llorona* Tales

Male Version (M-30):
This tale is presented with each separate proposition numbered. The numbers are used in diagram of the grammatical structure of this version presented in figure 4.2.

(1) *La Llorona* was married to a hardworking man.
(2) They had many children and worked hard to get ahead. All was well . . .
(3) One day she began to walk the streets, but her husband did not know.
(4) When her husband found out,
(5) he had much *pena* (shame)
(6) and he wanted to chastise her.
(7) So he beat her and cursed her
(8) and told her that her actions had caused him much pena.
(9) But the beating made her ashamed (*pena*)
(10) and late that night she walked into the river
(11) and killed herself.
(12) And now she knows no rest and must forever wander as a wailing spirit,
(13) and all because she was a bad wife.

This version conforms to Variant Type 1A and is schematically diagrammed for functions as follows (see table 4.3 for key to abbreviations):

$$@\sim A_1C_1D_{3c,d}E_1F_{5c}G_1H_{10}{}^*$$

Female Version (F-28): (Diagrammed grammatically in figure 4.3)

(1) *La Llorona* was married to a man
(2) and they had many children.
(3) But one day he started drinking,
(4) and he lost money.
(5) She had much *pena* (shame)
(6) and so she became cold to him.
(7) But he kept drinking,
(8) and losing money.
(9) And so she killed herself
(10) so that he would have no one to care for him and no one to help him.
(11) And so he ended his life with no money and no family.

This version conforms to Variant Type 4 and is analyzed functionally as:

$$@\sim A_3C_1D_{3b}A_3D_{5c}G_2H_{3c}$$

separate, numbered, propositions in the text. These two accounts were chosen for illustration because they are moderately complex, yet they spell out almost all the links present in the hypothesized grammar.

The grammar, then, can be viewed as an ideal model of a shared story *schema*, which includes all the moves and links that exist in any version of the tale. Hence it is capable of accounting for all of the versions collected even though any individual version may omit certain elements of the plot moves. A number of accounts, for example, jump from a precipitating event by one character to an action and consequence on the part of the

other without explicit reference to the emotion felt and the desire it generated. In such accounts, the listener, who shares the same cultural story schema as the teller, must fill in the blanks by making inferences from this schema about the motivations of the characters (see Mathews 1992: 135–137 for a fuller account of this process). In developing the grammar, the analyst must tease out the links present in the tales by working between fully and partially instantiated versions in order to gain some understanding of the links that are implied in more cryptic accounts.

A key component of cognitive schema theory is that these schemas are hierarchically organized with simpler or more restricted schemas often being embedded in more general or complex ones. From this premise, it is possible to use the fully developed story grammar or schema to predict the elements likely to be omitted in any individual account of the folktale. The setting in general, or the scenario in particular, may be omitted from a story without destroying its sense. However, the episodes that form the plot are never omitted. A story would not be a story without them, although certain of the internal elements of episodes (the units that comprise the beginning, the development, and the ending) may be dropped. The most common omissions in the *La Llorona* tales that I have analyzed are elements of the complex reaction and the plan. This, I have argued elsewhere (see Mathews 1992), is because the emotional reactions likely to result from a disruptions of marital expectations are easily inferred from culturally shared schemas for the self, marriage, and emotion.[7]

The construction of a story grammar is valuable because it demonstrates that all the variants of the tale conform to the same underlying *schema* that structures events in sequences of episodes that are causally and temporally linked. Thus one event is shown to trigger another until the final outcome is reached. Because these connections are fixed, and because the story grammar itself is a shared form of expression, the chains of events they create in the stories seem natural, inevitable, and convincing, even though they may depart from real-world events.

[7] In a preliminary study of accounts of the *La Llorona* tales collected in natural contexts (see Mathews n.d.), I hypothesize that extremely cryptic versions of the tale, that is versions that omit elements of the complex reaction and the plan, are those most often recounted in the context of group negotiations about the attribution of blame in ongoing marital disputes. In these more cryptic versions, there is room for some variability in the interpretation different listeners can advance about the motives of the two spouses involved in a dispute, although the number of plausible interpretations is limited by the schemas of marriage operative in the community. The more fully instantiated accounts, on the other hand, are used to convey strong moral opinions in the contexts of socializing the young to appropriate gender role behavior and/or in justifying the actions taken by one spouse against the other in a marital dispute. In other words, the more fully instantiated accounts are didactic. They make connections explicit in order to teach others just those emotional responses that are appropriate in certain situations and how such emotions can be expected to lead to predictable goals and actions. The cryptic versions, therefore, are deliberately vague because the teller is either unwilling to commit publicly to a particular interpretation or seeks to generate group discussion about the characters' motivations.

Researchers involved in developing story grammars have argued that this attempt at formalism is important because it allows the analyst to identify the patterns of a particular genre or domain. Yet, most of these analysts are careful to point out that while grammars lay bare the patterns, they do not predict the content of those patterns. Grammars are said often to be descriptors of form only. This disclaimer stems from the assumption, prevalent in objectivist semantics, that form and content are separately distinguishable entities or that syntax is independent of semantics and that semantics is independent of pragmatics (Lakoff 1987:256). A number of cognitive scientists, however, have challenged this position. Lakoff, for example, argues persuasively that language is indeed based on cognition, and that the structure of language is based on the same devices used to structure other cognitive models. He writes, "Language is made meaningful because it is directly tied to meaningful thought and depends upon the nature of thought" (1987:291). He therefore proposes that a key goal for linguists should be the development of a theory of grammar in which syntax is not independent of semantics. Instead, he proposes that the ordering of syntactic units may reflect and be rooted in certain basic assumptions made in the cognitive models of the culture about how people can be expected to act and about how actions unfold. Consequently, for Lakoff, grammatical constructions have a real cognitive status. They are not mere epiphenomena arising from the operation of generative rules. Moreover, a great many syntactic properties of grammatical constructions are consequences of their meanings (1987:582).

This is the conclusion that I myself had arrived at after developing the *La Llorona* grammar but before reading Lakoff. It seemed clear to me that the grammar gave force to the meanings conveyed in the content of the tales, because the ordering of events reflected a more general cultural model of human behavior found in the research community that linked thought to action. In this model, an offensive or disruptive action by one person is expected to lead to an emotional reaction and consequent responding action by the other. The grammar reflects this link between thought and action and indeed "lays bare" the connective function of emotion schemas in the cultural model. It is the experience of emotion, according to the cultural model, that links desire to the formation of intention and to action (see Mathews 1992:151–157 for a fuller explication of the three emotions schemas of *pena, coraje*, and *tristeza* operative in the tales). Consequently, the form the stories take is not arbitrary but is linked to the message that they are designed to convey: that is, that people who are offended will react emotionally and be motivated, therefore, to take some counteraction in response. An immoral act by one person is likely to lead to a responding action by another that may result eventually in dire consequences for both. The story grammar, therefore, reveals shared patterns rooted in a cultural model of human behavior. To the extent that the schematic structures of grammars for morality tales are shared across cultures, moreover, they may all be tapping into a similar, universal model of

human motivation linking the experience of emotion to the formations of desires and intentions to act (see D'Andrade 1987).

The discovery of these shared patterns in the grammar, however, was clearly not the whole story of the messages conveyed in the tales. While the plot units were the same in all the variants collected, the content of those units was not. Rather, the specific actions undertaken by the husbands in the tales were often quite different from those resorted to by the wives. I knew from my ongoing research in the community that the actions of the main characters were referencing broader cultural notions of appropriate life goals and gender roles as realized in marriage. In order to determine just what these notions were, I knew that I would have to probe further into the specific content expressed in the tales, and how it varied. I was particularly interested to know if such variation was patterned and if it could be accounted for systematically. To aid in this endeavor, I returned to one of Propp's (1958/1986) methods of structural analysis.

The Analysis and Description of Functions in the *La Llorona* tales

The main moves of the plot in the *La Llorona* tales are depicted in the grammar. The moves themselves, however, can also be described as consisting of a limited number of basic functions. *Functions*, according to Propp (1958/1986), are the actions of the dramatis personae defined from the point of view of their significance in the course of action of the tale as a whole. In the Russian tales he studied, the hero is the main dramatis personae whose major actions are motivated by and opposed to those of the villain.

In the *La Llorona* tales, two main dramatis personae are always the husband and wife. Although the character of *La Llorona* is mentioned most often, she may be portrayed as either the protagonist or the antagonist in the relationship. The definition of her role implies the opposite function for her husband. The two are always counterpoised, and as the grammar illustrates, one causes the main problem while the other takes steps to solve it. Major plot moves are composed of the actions and reactions of these characters, which are of a limited number and appear in the tales in a relatively fixed order.

In determining the functions of the moves, the goal is to specify in some detail the limited range of actions undertaken by the main characters and the order in which these appear. The study of functions and of the subactions comprising them involves lower-level units than the moves captured in the grammar, although the technique used to uncover them is similar. Because I knew from collecting the data that men's accounts differed significantly from those of women, I decided to analyze the versions separately by gender. I began with male accounts, working out the major categories of action and the subactions taken by the two main characters

in each. I attempted to formalize the sequences in which the different categories of action occurred and then use them to try and account for the functions present in the remaining male narratives. Eventually, when I had completed the specification of functions in the male narratives, I turned to the female versions and worked to specify the functions found there. In all of these accounts the responses of the husband and the subsequent actions undertaken differ in important respects from those of the wife, although the sets of emotions experienced and actions taken may be viewed as parallel in range and in the locations in which they occur in the stories.

The key difference to emerge between male and female accounts was the positioning of the two characters as protagonist and antagonist. In male accounts the husband is usually portrayed as the protagonist while in female accounts it is the wife. This does not mean that men cannot tell the tale from the female perspective, or vice versa, just that a major source of variation stems from perspective, which corresponds overwhelmingly to the gender of the teller.

The descriptions of the kinds of functions undertaken by the main characters (the protagonist and antagonist) and the symbols used for these functions in the *La Llorona* tales, are listed in table 4.3. They are presented in the order in which they normally occur in a tale, although some sequences of functions may repeat within a single tale. Within a single function, such as "Transgression," it is possible to list all of the actions that either the protagonist or the antagonist carries out in the tales. An exhaustive list of the actions portrayed in the tales is presented in table 4.4.

Table 4.3 Symbols and Descriptions for Functions in the *La Llorona* Tales

The actions of the dramatis personae in all 60 versions of the tale can be accounted for by the following limited set of functions. Although the tales may be nested by embedding repeated moves and sets of functions at specified points, the functions listed below always occur in the plot sequences in the order listed:

@ **Setting** (location and time)
~ **Scenario** (depiction of how the marriage proceeded in the ideal early stage before the transgression occurred)

A. **TRANSGRESSION** by Antagonist
B. **IMMEDIATE CONSEQUENCE OF TRANSGRESSION**
C. **EMOTIONAL REACTION** of Protagonist
D. **RESPONSE ACTION** by Protagonist
E. **EMOTIONAL REACTION** of Antagonist
F. **RESPONSE ACTION** by Antagonist
G. **OUTCOME**
H. **CONSEQUENCES** for Dramatis Personae

* **Moral**

Since the setting, scenario, and moral are not actions of the dramatis personae, they are listed with symbols here to indicate their presence as plot elements but *not* as functions of the characters.

Table 4.4 Specific Actions that Comprise Each Function by Gender of the Characters

A. TRANSGRESSION by Antagonist (i.e., Violation of marital expectations)
1. Absenting of antagonist from home
2. Antagonist commits adultery
3. Antagonist neglects duty to care for spouse/family
4. Antagonist refuses to provide sex for spouse
5. Antagonist publicly acknowledges relationship with person other than spouse and legitimate children

B. IMMEDIATE CONSEQUENCE (i.e., Stemming from transgression)
1. Child dies from neglect by antagonist
2. Antagonist's transgression becomes publicly known
3. Antagonist is repudiated by natal family

C. EMOTIONAL REACTION of Protagonist
1. Protagonist experiences *pena* (shame)
2. Protagonist experiences *coraje* (anger/rage)
3. Protagonist experiences *tristeza* (sadness)

D. RESPONSE ACTION by Protagonist (to Transgression and/or immediate consequence of transgression)
1. Protagonist denies consequences of transgression
2. Protagonist escapes consequences of transgression
3. Protagonist attempts to correct/eliminate behavior that caused transgression
   a. by denying antagonist the material/labor support due a spouse
   b. by denying antagonist the sexual privileges due a spouse
   c. by publicly humiliating antagonist
   d. by physically punishing antagonist
   e. by sending antagonist back to natal family for parental discipline
4. Protagonist redefines terms of marriage
   a. by finding a source other than antagonist for material/labor support in marriage
   b. by finding a source other than antagonist for sexual privileges
5. Protagonist ends marriage
   a. by killing antagonist
   b. by killing antagonist and antagonist's lover
   c. by killing self
   d. by killing self and children
   e. by turning antagonist out of house forever

E. EMOTIONAL REACTION of Antagonist (repeat same actions as in function B)

F. RESPONSE ACTION by Antagonist (repeat same actions as in function D)

G. OUTCOME
1. Protagonist survives and Antagonist dies
2. Antagonist survives and Protagonist dies

Continued

Table 4.4 Continued

H. CONSEQUENCES for Dramatis Personae
1. Protagonist finds new marriage and lives happily ever after
2. Protagonist ends suffering at the hands of the antagonist
3. Protagonist takes revenge against antagonist
   a. by causing death of antagonist
   b. by committing suicide and bringing shame to antagonist
   c. by ending the marriage and hence denying antagonist the privileges of marriage
   d. by ending the marriage and hence denying antagonist adult status in the community
4. Protagonist takes revenge against all members of opposite sex by haunting and killing them
5. Protagonist suffers eternally wandering in Purgatory as a restless spirit
6. Antagonist finds new marriage and lives happily ever after
7. Antagonist ends suffering at hands of protagonist
8. Antagonist takes revenge against protagonist
9. Antagonist takes revenge against all members of opposite sex by haunting and killing them
10. Antagonist suffers eternally wandering in Purgatory as a restless spirit

To determine if the actions carried out by either the protagonist or by the antagonist varied when the gender of the character occupying these roles shifted, I had to figure out how the sequencing of functions and actions varied in male and female versions of the tales. I simplified this task by using the abbreviations given for these functions and actions as a code that allowed me to represent a version of the tale schematically. The actions of the characters are coded in the order in which they occur from left to right. The capital letters represent the main functions, with the subaction categories and variant choices presented as subscripts in numbers and lower case letters. A comma separating subactions indicates that any of these may be substituted in the slot accorded the main function, or that more than one of these may occur in that slot. The "&" symbol indicates that the specific subaction following the "and" connection must occur after the subactions listed as preceding the connection.

There are some story elements that do not involve specific functions or actions by the characters and so are not included in the functional analysis. These include the "setting," which describes where the tale takes place, the "scenario," which is a comment on how relations have been going between the spouses, and the "moral," which is a commentary on what is to be learned from the tale itself. Each is listed in table 4.3 with symbols at the point in which they occur in the narrative, but they are not considered further in the functional analysis (i.e., @ for setting; ~ for scenario of marriage; and * for moral of the story).

Tale F-28, diagrammed grammatically in figure 4.3, is analyzed schematically according to functions below the text presented in table 4.2.

In this version, the wife is the protagonist. The husband transgresses by wasting all the money and not providing for his family. His wife reacts emotionally with shame, which motivates her to take a corrective action by becoming cold or refusing him sex. Her action has no effect, as he continues to transgress until he loses all their money. At that point his wife responds by killing herself to end the marriage and to take revenge on him by making sure that he no longer enjoys the privileges of marriage or adult status in the community. Thus the outcome is that the antagonist survives but the protagonist dies. The moral is not stated explicitly in this version.

The value of my approach is that it enables the determination of just how many variants of the tales occur and the form these variants take. To find the variants, I first made schematic diagrams for all the tales told by men. I then grouped those that recurred into types and subtypes, based upon the points at which the actions diverged, and in terms also of the conditions surrounding and giving rise to such divergence. I then moved on to do the same for the female versions. When, as happened in three cases, a woman told a male version of the tale, or a man told a female version, I grouped them in with the subtype associated primarily with the other sex. In using this approach, I was able to delineate six different type variants of the tales—three found in the male versions and three in the female versions. These different type variants reflect intra- and inter-gender variation in story construction, and they provide important windows into the broader cultural models of appropriate gender roles in marriage being referenced in the tales. Although they are distinct, the male and female types do parallel each other in some important ways that I discuss further in the next section.

The types and subtypes for each gender group are diagrammed schematically and discussed in tables 4.5 (male versions) and 4.6 (female versions). The nonfunctional elements of the tales (i.e., setting, scenario, and plot) may be dropped out of some accounts, but they are listed for each type in the position in which they would normally occur. The tales fell into a remarkably few categories by type with the least common (Subtype M2b) occurring in 8 percent of the male versions, and the most common (Type F3) occurring in 41 percent of the female versions. I found only four accounts that I judged to be almost totally idiosyncratic. These either had no coherent narrative structure conforming to the grammar outlined, or the actions of the characters matched none of those abstracted from the rest of the tales. I judged these four versions to reflect idiosyncratic variation stemming from, in one case, the person's unfamiliarity with the story and, in others, with their attribution to the characters of certain elements of their personal reactions and responses to marital problems. I deleted these four from the schematic analysis of functions presented here, although I do consider them to be an important source of data in other analyses.

The most common variant of the male versions (see table 4.5) is **Type M1** (*"wife transgresses against husband"*). The husband responds by attempting

Table 4.5 Variant Types of Male Versions of the Tale as Analyzed by Sequences of Functions

Abbreviations for the functions and actions comprising the moves in the tales are used to make a schematic analysis of the basic types of functional sequences found to predominate (see table 4.3). The tale elements of setting, scenario, and moral are indicated with symbol abbreviations at the point in the sequence where they usually occur. Not all tales include complete setting, scenarios or morals.

A *comma* separates actions that may be substituted in the plot implying that one and possibly others in the series will be present in any sequence. The "&" symbol indicates that the specific action *following* the "and" connection must occur after the presence of any of the actions preceding the connection. Brackets, *{ }*, around a partial sequence of functions indicate that these may be repeated as an embedded episode in the tale.

Functional Sequences in Tales Collected from Male Informants

Type M1: "*Wife transgresses against husband*"

This is a general sequence of functions that has two subtypes predicting the occurrence of different specific actions. The husband is the protagonist. The wife transgresses. The husband attempts to correct or eliminate the behavior causing the transgression. She either reacts emotionally (or transgresses again to which he responds with correction—this can be an embedded move) and finally ends the marriage by killing herself.

Subtype M1A: "*Wife behaves inappropriately*"

The husband is the protagonist. The wife transgresses by being absent, committing adultery, or neglecting duties to the family. The husband responds with shame or anger and acts either to publicly humiliate, physically punish, or send the wife home to her natal family for discipline. The wife reacts to these efforts with shame and acts either to kill herself or to kill herself and her children. The outcome is that the protagonist lives and the antagonist dies. The consequence is that the husband may either find a new marriage or live happily ever after and/or the wife may suffer eternally for the sin of suicide by wandering in Purgatory as a restless spirit. This sequence is diagrammed as follows and structures 27% of all the male accounts of the tale:

$$@\sim(A_{1,2,3}C_{1,2}D_{3c,d,e})E_1F_{5c,d}G_1H_{1,10}*$$

Subtype M1B: "*Wife refuses sex with husband*"

The husband is the protagonist. The wife transgresses by not providing her husband with sex (turning cold to him). The husband reacts emotionally with sadness. He then responds by redefining the terms of the marriage and finding a source other than the spouse for sex. The wife reacts with anger and responds by either killing herself or killing herself and the children. The outcome and consequence are the same as in type 1A. This sequence is diagrammed as follows and structures 23% of the male accounts:

$$@\sim A_4C_3D_{4b}E_2F_{5c,d}G_1H_{1\&10}*$$

Type M2: "*Wife commits adultery*"

This is another general sequence of functions with two subtypes that predict the choice of specific actions within functions.

Continued

Table 4.5 Continued

Subtype M2A: "*Wife walks the streets and husband kills her*"
The husband is the protagonist. The wife transgresses by walking the streets looking for men and committing adultery. The immediate consequence is that her actions become publicly known. The husband reacts with shame or anger and proceeds to kill her and/or her lover. The outcome is that the protagonist lives and the antagonist dies with the consequence that the husband takes revenge against the wife by causing her death, and the wife is doomed to suffer in Purgatory for her sins. This sequence is diagrammed as follows and structures 15% of the male accounts:

$$@\sim A_{1\&2}B_2C_2D_{5a,b}G_1H_{3a\&10}{}^*$$

Subtype M2B: "*Wife walks the streets and kills herself*"
The husband is the protagonist. The wife transgresses by walking the streets looking for men and committing adultery. The immediate consequence is that her actions become publicly known. The husband reacts emotionally with shame and acts to turn her out of the house forever. The wife responds by killing herself. The outcome is the same as 2A, but the consequence is only that the wife is once again doomed to wander in Purgatory for her sins. This sequence is diagrammed as follows and accounts for 8% of the male accounts:

$$@\sim A_{1\&2}B_2C_1D_{5e}F_{5c}G_1H_{10}{}^*$$

Type M3: "*Wife's neglect causes death of a child*"
This a general type with no variant subtypes.

The husband is an absent protagonist. The wife transgresses by walking the streets looking for men, committing adultery and therefore neglecting her duty to care for her family. The immediate consequence is that one of her children dies. She reacts emotionally with same and/or sadness which leads her to kill herself. This outcome and consequence is the same as in Subtype 2B. This sequence is diagrammed as follows and structures 27% of the male accounts:

$$@\sim A_{1,2\&3}B_1E_{1,3}F_{5c}G_1H_{10}{}^*$$

to correct her behavior through a public scolding or a beating. She may respond emotionally to his actions and end the marriage by committing suicide or she may ignore his warnings initially, transgress again, and receive a more extensive punishment. If the husband, for example, chooses to turn her out of the house, she is left with no option but to commit suicide since she cannot return to her natal family or divorce her husband. Eventually, therefore, the wife is always shown to react emotionally to her husband's chastisements, which prompts her to end the marriage by taking her own life. The outcome is that the antagonist wife is shown to have died a sinful death and to be doomed to wander as a restless spirit in Purgatory, while the protagonist husband, wrongfully treated by his wife, finds new happiness without her.

The two subtypes of this variant differ on the basis of the type of transgression committed initially by the wife. In **subtype M1A** ("*wife behaves*

inappropriately"), she either leaves home to walk the streets, commit adultery, or she neglects her duties to the family. In **subtype M1B** ("*wife refuses sex with husband*"), she turns cold and denies her husband sex. This second transgression is fundamentally different from the others since, according to men's views about women, it may or may not be the wife's fault. A key assumption men make about women in the community is that by nature they dislike sex and only engage in sexual relations in order to conceive children or to fulfill their duties in marriage (cf. Cardozo-Freeman 1986:13). Thus, if the condition of turning cold is a product of female nature, not an effort by a wife to control or punish the husband, then it is judged not to be her fault (examples of this latter point appear in other tales to be described later). The husband would then respond by seeking a sexual outlet outside of marriage (see Mathews 1992). Subtype M1B ("*wife refuses sex with husband*") reflects this sequence of actions. The wife refuses her husband, and he looks for another partner. But in this variant of the tale, the wife is upset that he does so and responds emotionally to his adultery by killing herself. The outcome and consequence are the same as in subtype M1A.

Type M2 of the male versions ("*wife commits adultery*") stems from the wife's inappropriate sexual behavior. The difference between this and M1 is that her actions become publicly known. The husband reacts with anger in **subtype M2A** ("*wife walks the streets and husband kills her*"), and his anger motivates him to seek revenge by murdering her and her lover. In **subtype M2B** ("*wife walks the streets and kills herself*"), the husband reacts with shame, and his shame motivates him to turn her out of the house forever and end the marriage. Because women, once publicly shunned in this way, have nowhere else to go, suicide is seen to be their only option. Thus the husband's actions in both M2A and M2B lead to the wife's death. As antagonist, moreover, the wife is in the wrong and it is her wrongdoing that ultimately causes her death and dooms her to wander in Purgatory as a restless spirit. Type M2 of the male versions does not occur as frequently in the tales as Type M1.

Type M3 of the male versions ("*wife's neglect causes death of a child*") has no subtypes. In this type, the wife transgresses by either walking the streets or committing adultery, which then leads her to neglect her family. The new twist is that the immediate result of her transgression is the death of her child to which she, but not the husband, reacts emotionally. Her shame and grief at her child's death motivate her suicide and ultimate suffering in Purgatory. The husband is the protagonist, but he is absent from the main action, having nothing directly to do with her tragic end that is brought about by her own wrongdoing as it affects her child.

The female types of the tale are discussed and diagrammed in table 4.6. These are also divided into three types that parallel in important respects the male versions. The main difference, as I stated previously, is one of perspective. In the female versions the wife is the protagonist. In Type F1 ("*husband neglects duties to family*"), for example, the husband transgresses

Table 4.6 Variant Types of Female Versions of the Tale as Analyzed by Sequences of Functions

Type F1 is a general type with no variant subtypes.

"*Husband neglects duties to family*"
The wife is the protagonist. The husband transgresses by being absent and/or either committing adultery and/or neglecting his duties to provide for the family. The wife reacts with shame and responds by attempting to correct/eliminate the behavior causing the transgression by denying material/labor support, sex, or by publicly humiliating her husband. (At this point an embedded move may have him transgress in the same way again.) Eventually the husband responds emotionally to her corrective efforts with the same and acts to publicly humiliate or physically punish his wife. She then responds by killing herself or herself and the children. The outcome is that the protagonist dies and the antagonist lives, but the consequence is that the protagonist either ends her suffering at the hands of the antagonist husband or takes revenge against him by either bringing shame to him with her sinful type of death, or by ending his access to the privileges of marriage, or by denying him adult status through the ending of the marriage. Finally, she may also take revenge as a spirit against all men by haunting and killing them.

This sequence is diagrammed as follows and structures 37% of the female accounts:

$$@\sim(A_{1,2,3}C_1D_{3a,b,c})E_1F_{3c,d}G_2H_{2;3b,c,d;4}{}^*$$

Type F2 is a general type with no variant subtypes.

"*Husband publicly acknowledges adulterous relationship*"
The wife is the protagonist. The husband transgresses by publicly acknowledging that he has a relationship with another woman or by publicly acknowledging the illegitimate children that resulted from that relationship. The wife reacts with shame or anger and acts to kill herself or herself and her children. The outcome is that the protagonist dies and the antagonist lives, but the consequence is that she ends her and/or her children's suffering at the hands of the husband.

This sequence is diagrammed as follows and structures 22% of the female accounts:

$$@\sim A_5C_1D_{5c,d}G_2H_2{}^*$$

Type F3 is a general type with no variant subtypes.

"*Husband's neglect causes death of a child*"
The wife is the protagonist. The husband transgresses by being absent and/or committing adultery *and* neglecting his duties to provide for the family. The immediate consequence is that one of his children dies. The wife reacts with anger or sadness. She responds by killing herself or herself and her children. The outcome is that the protagonist dies and the antagonist lives. The consequence is that she either ends her suffering and that of her children at the hands of her husband; or she takes revenge against her husband by bringing shame to him with the form of her death; by ending the marriage and thereby denying him its privileges and/or denying him adult status; or by taking revenge against all men by haunting and killing them.

This sequence is diagrammed as follows and structures 41% of the female accounts:

$$@\sim A_{1,2\&3}B_1C_{2,3}D_{5c,d}G_2H_{2,3,4}{}^*$$

against his wife by wasting family funds on liquor, by seeing other women, by giving money to other women, or by neglecting his duties at home. His wife responds with shame and attempts to correct his behavior by publicly scolding him or by turning cold to him and refusing him sex. At this point the husband either reacts to her correction emotionally or ignores her and transgresses again (an embedded move) prompting the wife to take further corrective action against him. Ultimately he does respond emotionally to her attempts at chastisement by becoming angry. This emotional reaction then leads him either to publicly shame, beat, or turn his wife out of the house. Again, the wife's resultant humiliation or her lack of alternatives for survival is shown to motivate her suicide and/or the murder of her children. The outcome is the same as in the male versions—the wife dies; but in this case she is the protagonist. The consequence of the outcome is that she is seen as either having taken revenge on her husband through her suicide, by depriving him of a family, or as having acted to relieve her own suffering in life. She may also, as a spirit, act to take revenge against all men who transgress by attempting to lure them into the river.

Type F1 can thus be seen to parallel developments in Type M1 of the male versions ("*wife transgresses against husband*") although the perspective, the particular types of actions taken, and the resultant consequences of the story are different. To summarize these differences briefly, in M1, the husband is the protagonist who is wronged by the wife when she behaves inappropriately by walking the streets, neglecting her family, committing adultery, or refusing sex with him. The husband responds by scolding publicly, beating, or turning his wife out of the house, which leaves her no option but to commit suicide. Her death, however, is her own fault and because it is shameful, she is doomed to wander in Purgatory as a restless spirit while the protagonist husband is seen to benefit by being free to find a better wife. In the female version, F1, the wife is wronged by the husband who either wastes family resources on drinking and other women or neglects his wife and children. She attempts to correct his behaviors with a public scolding or by withholding sex, but the husband reacts to this angrily and, in turn, publicly humiliates, beats, or turns his wife out of the house. Being shamed and out of options, the wife reacts by committing suicide and possibly killing her children as well. But in the female version, her death is caused ultimately by the husband and sometimes, as a wandering spirit, she takes revenge on other men who are neglecting their families by luring them to their deaths.

In **Type F2** of the female versions ("*husband publicly acknowledges adulterous relationship*"), the antagonist husband undertakes a special kind of transgression that parallels developments in Type M2 ("*wife commits adultery*") of the male stories. The husband commits adultery, which is not an unusual act in this community. Wives will generally accept men's infidelities, as long as they do not become the subject of public gossip. In this type, however, the husband acts to acknowledge publicly his lover and/or his illegitimate children by her. This causes the wife to react with

shame or anger and respond by killing herself and/or her children. The outcome again is that the wife as protagonist dies, but the consequence is that she has ended her suffering and that of her children at the hands of her husband, while also avenging herself upon him by depriving him of his family and his good name.

Type F3 of the female versions (*"husband's neglect causes death of a child"*) parallels the structure of Type M3 (*"wife's neglect causes death of a child"*) in the male stories. In F3, the wife is the protagonist. Her husband transgresses by neglecting his duties to his family, specifically by not providing the resources they need to eat or to heal an illness, or he refuses to come and assist with a sick child. The immediate consequence is the death of the child, which in this version, is the fault of the husband. The wife reacts to this event with anger or sadness, but in either case, the emotion prompts her to kill herself and/or her remaining children. The outcome is the same as before but the consequence, if the wife's motivation was anger, is the exacting of revenge on the husband by depriving him of his family and his good name. Alternatively, if her motivation was sadness, her suicide enables her to end her suffering and/or that of her other children.

This schematic analysis of functions allows us to move a step beyond the grammatical analysis. We now know that although all the tales reflect the same underlying schema for human behavior (that actions prompt emotional responses which in turn stimulate desires and the formations of intentions to act) and that they follow the same structure with regard to plot moves, all accounts do not fill in the content slots in the same way. Rather, the specific actions undertaken by the characters, and the order in which these arise in response to previous events, are both predictable from and revelatory of the cultural models of gender and marriage that underlie and give meaning to this discourse (see Mathews 1992:141–151). Moreover, the actions specified in the tales can serve as independent verification of descriptions of such cultural models derived from participant-observation and field interviews (see D'Andrade's chapter in this volume).

In describing my sense of these cultural models of gender and marriage derived from fieldwork (Mathews 1992:141–151), I argued previously that the most important life goal for women in the community studied was to have children. Children mark the achievement for women of adult status and are valued for the companionship they provide and the assistance they render with farm and household labor. Women, moreover, believe that children will provide financially for them in later years, thereby helping them attain some measure of independence from their husbands and their husbands' families. In keeping with this emphasis on self-definition through children, it is crucial to women to be recognized both within their families and in the larger community as the mothers of their husbands' legitimate offspring. This status implies a legal and legitimate marriage.

Men, on the other hand, value the achievement of success and public status above all, and this success can be measured in personal, family, and

community terms. On the personal level men want to attain a reputation as sexually accomplished, as demonstrated by having many affairs and fathering many children. At the same time, however, a man wants to be respected for maintaining control at home, as evidenced by the sexual fidelity of his wife and the respect of his legitimate children. Success on the family level is measured by a man's ability to provide for these individuals, and such household success ultimately enables participation in the community-wide status system known as the civil-religious hierarchy or *cargo*, which I studied in my initial fieldwork (see Mathews 1982). Men from households that show success in *cargo* participation often attain considerable status and political influence on the community level.

Thus women want children and the status and security they provide; men want children, financial success, and community-recognized status. Marriage makes the attainment of these life goals possible, and marital expectations are framed accordingly. Couples want marriages to function smoothly so that life goals can be met. Women, who are concerned with raising their children successfully, expect a husband to father children and support them financially. They do not expect a husband necessarily to be sexually faithful or to take much of an interest in raising his children. But he should accord his wife and legitimate children a primary place of respect, by being discreet in his extramarital affairs, and by always placing a priority on the provision of financial and moral support for them.

Men who want public status expect that their wives will not only bear and nurture children, but also provide the labor that helps support their quests for public status. A wife must willingly meet her husband's sexual needs as well, although she is not expected to enjoy the experience. However, she should be faithful sexually to her husband so that the legitimacy of his children is never questioned. To that end, a wife should uphold the household's reputation as well as her husband's by behaving in public with dignity and decency.

These cultural models provide a framework that community members use to evaluate the behaviors of individual men and women. These models are referenced in abbreviated form within the content slots of the *La Llorona* tales. We have seen that the actions of women that disrupt a marriage, from the point of view of men, include walking the streets and gossiping, committing adultery, refusing sex to their husbands, and neglecting their duty to care for their husbands and children. The actions of men that can disrupt a marriage, from the female point of view, include neglecting their duties to provide financial and moral support to their wives and children and publicly acknowledging relationships with other women and illegitimate children.

Similarly, the responding actions a character can take in the folktales to try and correct a spouse's misbehavior also vary by gender in accordance with the expectations framed by these models. Wives, who move into the husbands' household, have less power and limited alternatives when a husband strays. They can try to chastise their husbands or refuse them sex, but

these seldom succeed because, in terms of the cultural models of gender and marriage, they elicit anger from the husbands so challenged. Men, on the other hand, have more options available and can chastise, publicly humiliate, beat, kill, or turn a woman out of the house. Women, because of their powerless position in the patrilineal, extended family, are shown in the folktales to have recourse only to passive submission or resistance through suicide.[8] Thus, the systematic specification of functions and subactions in the tales is one means of verifying, as well as elaborating upon, the components of cultural models originally derived from in-depth interviews and participant observation.

This schematic analysis of functions also makes possible the delineation of three key variants of the tales within each gender group, which then enables us to specify how often these variants occur and how the emphasis shifts in each. We see that although the key types parallel each other in important respects, they are not the same tales with just a simple shift in perspective. Even when the wife is in the right, her only option for ending a marriage, according to the folktales, is to die, although at times her death may also have negative consequences for her husband. Men, on the other hand, have more behavioral options available when they want to end a marriage. They may kill the wife, cause her to take her own life, or as nonactors, allow events to take their course when the wife, as in type M3,

[8] In a seminal article on women in patrilocal, extended households, Collier (1974) argued that political power in those households rests on the size and cohesiveness of the coresident kin group. Women who move into their husbands' households have limited power. They may try to control the activities of sons and husbands, but these strategies are difficult to enact in a system premised on advancing the fortunes of a group of male agnates. Often, therefore, Collier maintains, women find that they can affect decisions by causing an uproar that forces others to pay attention to their wishes (1974:94). Although Collier focuses in her paper on disputes initiated by quarreling women and the challenges these pose to male control, threats of suicide could be viewed as the ultimate expression of this tendency. Margery Wolf's (1968) study of life in patrilocal extended households in rural Taiwan is suggestive in this regard. In describing the effects of one daughter's threats of suicide on her family, Wolf writes, "A young woman's threat of suicide is not taken lightly on Taiwan—their alternatives are too few" (1968:108). The uproar thus created through a threat not only calls attention to women, but may also help them advance their claims and attain some measure of power, however temporary, in an otherwise powerless situation. The importance of this theme is seen in folktales from rural China as well (Thompson 1955). Writer, Amy Tan, in her novel depicting conflicts between Chinese-born mothers and their American-born daughters, picks up on this theme, when one of the characters in the novel, Yan Chang, tells a story about a second wife. She says, "and everybody knows that suicide is the only way a woman can escape a marriage and gain revenge, to come back as a ghost and scatter tea leaves and good fortune" (1989:234). A comparison of folktales from cultures characterized by patrilocal extended households and male dominance might reveal the degree to which the threat of female suicide emerges as a theme. Kirtley's 1960 article, for example, points out similarities between themes in the *La Llorona* tale and the German legend of "*Die Weisse Frau*" or "The White Lady," first recorded in 1552. While he suggests that the German tale may have been the source for the *La Llorona* legend, it could also be argued that both arose independently as commentaries on similar types of gender conflicts generated within systems of arranged marriage and patrilocal extended household residence.

recognizes her own culpability for her child's death and commits suicide as a self-inflicted punishment. Interestingly enough, husbands never recognize their own transgressions, nor do they ever take steps to correct their own misbehaviors. Moreover, men never die as a consequence of the actions set in motion by their transgressions or those of their wives.

While Propp's method enables us to pinpoint some of the asymmetrical aspects that characterize relationships between husbands and wives in the folktales and to demonstrate how these asymmetries are differentially portrayed in male and female versions, this method has two key limitations. First, it does not does help us understand or explain why these variant patterns exist or why they take the forms that they do in the research community. Dundes (1971) points to a similar deficiency in Propp's work. He notes that while Propp's analysis of functions convincingly demonstrates the uniformity of Russian folktales, the technique itself does not enable Propp to theorize further about why the particular pattern he found existed in terms of Russian culture or personality (1971:173). Clearly, the limitations on women's autonomy portrayed in the *La Llorona* tales are related to features of the patrilocal extended family as well as to a public ideology of male dominance (cf. Taggart 1990:201–203). A full explanation of the origins of these models of gender and marriage, however, would require a more extensive historical and cultural analysis of gender roles in the community.

A second limitation of Propp's method is that it does not help us understand the meaning of these interpretive differences in a broader framework. In other words, what are implications of such differences by gender for the perpetuation or contravention of the public ideology of male dominance found in the research community? Do female variants of the *La Llorona* tale provide the basis for opposition to male dominance or do they, in certain respects, reaffirm that dominance? In searching for answers to these latter questions, I turned to Lévi-Strauss' work on myth, because his approach emphasizes that all myths function to depict and then resolve certain basic contradictions in life.

Structuralism: Modeling the Mediation of Oppositions

Lévi-Strauss (1963) argues that myths are not solely a form of pseudohistory, pseudoscience, or social charter. Rather, the elements of myth acquire meaning when these combine to form a structure. Thus, myths contain a kind of coded message that the analyst attempts to decipher and reveal. Structuralism is the method proposed by Lévi-Strauss for decoding myths. All myths, he argues, can be understood in terms of a progression from an initial opposition between key terms or symbols through a succession of mediating terms. Lévi-Strauss' (1958/1976) method is based on his view of

human cognition as structured by binary oppositions. A fundamental characteristic of human thought, he maintained, was the desire to find a midpoint between such oppositions. This characteristic is found as well in mythical thought, which he writes, ". . . always progresses from the awareness of oppositions toward their resolution" (1963:224). For example, Lévi-Strauss developed an explanation for why the coyote is a trickster figure in much of Native American mythology. He noted that many Native American tales posit an opposition between herbivores and carnivores, who represent two separate and distinct components of the animal world. Coyotes as carrion-eaters are capable of mediating between these two because, like beasts of prey, they eat animal food, but, like herbivores, they do not kill what they eat (1963:224–225). Thus coyotes, and ravens, for similar reasons, are usually the animals selected to serve the ambiguous, mediating role of trickster in these myths.

Lévi-Strauss (1963:224) also notes that there are various orders of mediation, because in myth, two opposing terms with no intermediary, such as life and death, always tend to be replicated in the tale by two equivalent terms that admit of a third one as mediator. So, in the tale of "Tricky Coyote," for example, life and death are posed as fundamental oppositions at the outset. This distinction is replicated in the tale by an opposition between herbivores and carnivores that is mediated by coyote (the one who eats meat but does not kill) and by agriculture and warfare (life-giving and life-taking) that is mediated by hunting because hunting, Lévi-Strauss contends, preserves human life while leading to animal death. Thus, Lévi-Stauss concludes, a myth can have mediators of the first order, of the second order, and so on, each term generating the next by a double process of oppositions and correlations (1963:225).

A structural analysis, therefore, involves rearranging the elements of myth into sets of opposing terms or symbols and their mediators. Form is emphasized over content because, while surface content may vary, the coded message or deep structure of myth is always the same; myths depict fundamental contradictions in life and then attempt to resolve them. A structural analysis, therefore, concentrates more on the logical relations that exist between the terms of opposition and mediation in a myth than on the specific characters or events the myth depicts. Lévi-Strauss's method can be applied to the analysis of folktales because, as Leach (1971) notes, in folktales, the dramatis personae involved stand in a particular relationship to one another at the beginning of the tale and in a different configuration at the end. The primary interest of the investigator conducting a structural analysis of folktales, he writes, "is principally in the transformation which is brought about in the overall patterns as a result of the action of the drama" (1971:23).

In applying Lévi-Strauss's scheme to the study of other types of folklore, E. K. Maranda and P. Maranda (1971) found it to be deficient. They noted that in some tales the mediation of opposition may not succeed and hence the tale ends with a return to the initial state of unresolved opposition.

In others, no mediation of the initial opposition may occur. On the basis of their own research, they proposed a set of models, each of which depicts a different process and outcome stemming from a central opposition (1971:35–81). Their four models are presented in order of increasing complexity in table 4.7 and are illustrated diagrammatically in figure 4.4. The "+" and "−" symbols reflect the different valences of the key pair of opposites. These symbols are not meant to imply any evaluation of positive or negative, although given tales may value them so. They merely reflect opposition, while the arrows demonstrate the direction of the outcome. In model I (see table 4.7), for example, there is no mediator and no possibility of a mediating process. In model II, there is an effort at mediation, but it fails. In model III, there is successful mediation but it produces no gain over the initial state. In model IV, there is successful mediation that results in a gain over the initial state.

I found the application of these models from Maranda and Maranda to the *La Llorona* tales to be instructive. The main opposition in the tales between husband and wife is manifested in the plot by a conflict emerging when one spouse transgresses against the other. The offended spouse attempts to resolve the conflict and preserve the marriage, but this main event, which can be interpreted as an attempt at mediation of the fundamental opposition, always fails with the wife killing herself and thereby ending the marriage. Interestingly, only models I and II capture adequately the structure of this first order of attempted mediation. Either no mediation of the opposition is seen to be possible (model 1), or the mediation reflected in the spouse's efforts to resolve the conflict fails (model II).[9]

The presence of only these two models in the first order of mediation in the genre under study is logical. Morality tales are based on failure. In the *La Llorona* tales, the immoral act of one person causes problems for a second. Their attempts to resolve the problem are doomed to failure because that failure then sets up the ending or moral of the story, which is a warning to the listener about the likely results of an immoral act. The moral would not be operative if mediation succeeded.

Model I (figure 4.5), for example, represents the developments in type M2 ("*wife walks the streets and is killed or kills herself*") of the male versions and type F2 ("*husband publicly acknowledges adulterous relationships*") of the female versions. In these variants, the antagonist spouse commits a severe transgression that subsequently becomes publicly known. At that point the marriage is effectively over. The protagonist spouse sees no possibility of mediation or of a return to the pre-conflict state. Hence the protagonist is motivated to end the marriage without attempting any

[9] Taggart's (1990) book on gender relations in Spanish folktales explores this type of communication between male and female storytellers. These storytellers carry out an exchange of images through the telling and retelling of the same stories. Unlike my Mexican informants, however, the dialog of these storytellers emphasizes some of the ways they believe couples can work through their contradictions according to a traditional model of marriage.

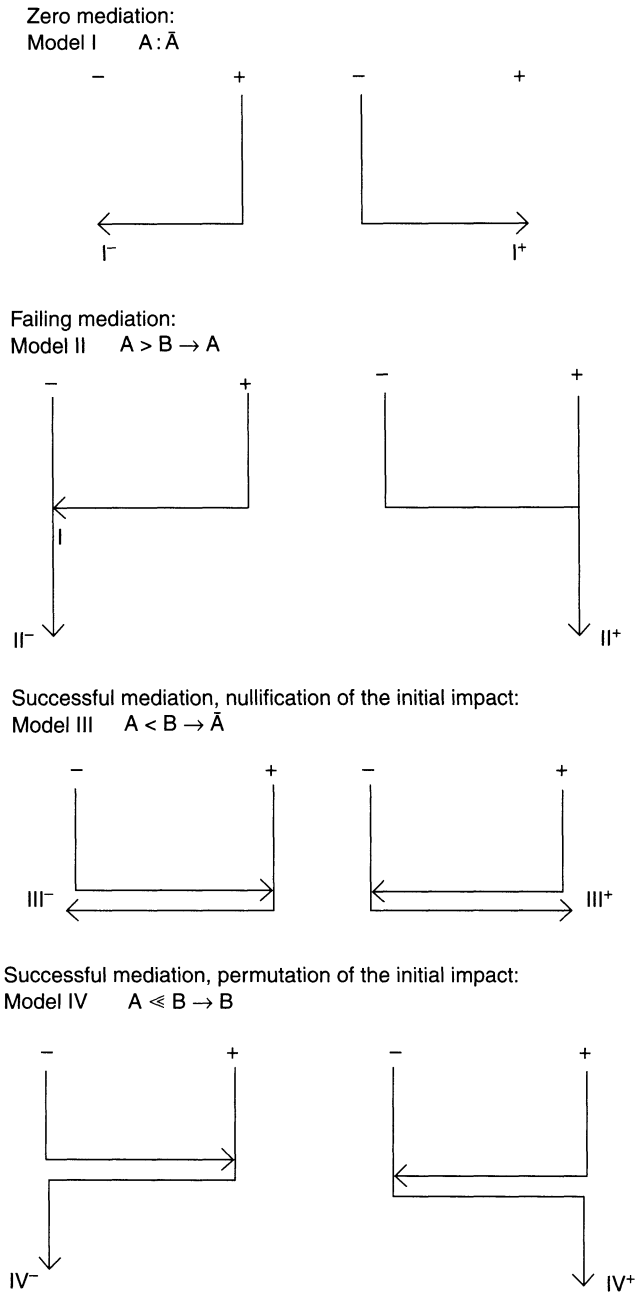
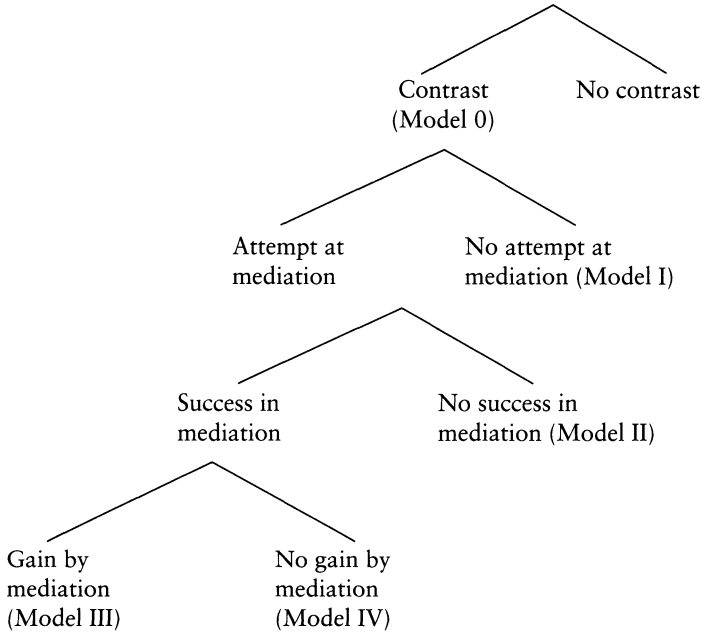


Figure 4.4 Diagram of Models of Mediation from E. K. Maranda and P. Maranda (1971:89–90)

Table 4.7 Models of the Mediation of Opposition in Folktales (Adapted from E. K. Maranda and P. Maranda 1971:36, 89–90)

Types of Models	
Zero mediator	(Model 1)
Failing mediator	(Model 2)
Successful mediator:	
Nullification of initial impact	(Model 3)
Successful mediator:	
Permutation of initial impact	(Model 4)

Tree Diagram of Models (also a decision model for storytellers):

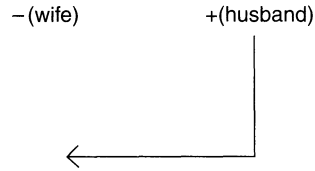


corrective action. The only difference between the two variants would appear to be in the direction of the transgression. In type M2, the wife transgresses and the husband ends the marriage; in type F2 it is the reverse.

Yet, the crimes committed by each spouse are not the same, and the differences reflect key beliefs about the behaviors viewed as appropriate for each spouse in the cultural model of marriage. In type M2, the wife commits adultery and it becomes publicly known. This threatens the honor of the husband who must, at that point, take steps to end the marriage. No reconciliation is possible once the community knows his wife has cuckolded him. He attempts to end the marriage either by killing his wife or by turning her out of the house. Since the wife cannot go anywhere else, this latter action by the husband in effect drives her to suicide.

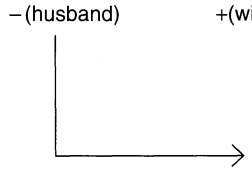
The use of "*" and "–" symbols to indicate opposition by E. K. Maranda and P. Maranda (1971) signal in these diagrams, "protagonist" and "antagonist," respectively. The direction of the outcome is *always* diagrammed from the point of view of the main character or the one mentioned most often, the wife or *La Llorona*. The story is always about her although she may be depicted variously as the protagonist or the antagonist in the marriage.

Type M1 *(male version)*
(wife behaves in appropriately or refuses sex to husband)



Outcome: Model I–

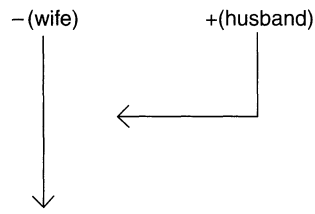
Type F1 *(female version)*
(husband neglects duties to family)



Model I+

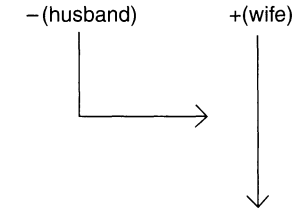
No mediator and to mediation possible; marriage ends.
(+ versus – signals the direction of the transgression from the point of view of the main character, *La Llorona*).

Type M2 *(male version)*
(wife walks the streets and is killed or kills herself)



Outcome: Model II–

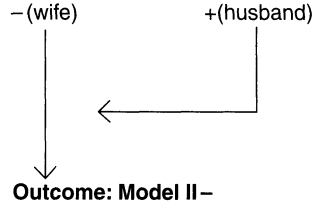
Type F2 *(female version)*
(husband publicly acknowledges adulterous relationship)



Model II+

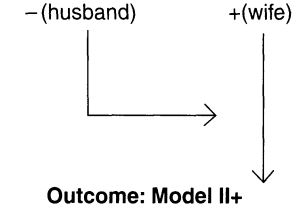
Mediation is attempted by the protagonist spouse in each account, but it fails. The marriage ends with the death of the wife.

Type M3 *(male version)*
(wife's neglect causes child's death)



Outcome: Model II–

Type F3 *(female version)*
(husband's neglect causes child's death)



Outcome: Model II+

In these variants, the actual mediator is the child whose death is a means for mediating the original dispute between the spouse. But, in terms of the cultural model, children are the reason for marriage and so a child's death nulifies the conditions of marriage and fails as a mediation. The marriage then ends with the wife's death as caused most immediately by child's death, but ultimately by either her own or her husband's

Figure 4.5 Diagrams of Models of Mediation for Initial Marital Conflicts in the Six Type Variants of the *La Llorona* Tales

In type F2, on the other hand, the husband transgresses by acknowledging publicly the status of his lover and/or his illegitimate children. When this happens, the marriage is over from the wife's point of view and she kills herself. This outcome reflects the cultural view discussed in the previous section, that men are allowed to have, and indeed gain status from, having extramarital affairs, so long as they maintain the conventions of marriage by putting their wives and legitimate children first. Once these conventions are violated by a public acknowledgment of the lover or of the illegitimate children, the wife's standing as a man's legitimate and publicly recognized spouse is compromised, and the family standing that a woman gains from marriage is lost. Thus for the wife, the basis for the marriage is dissolved, and she can have no other option but suicide.

In model II, a mediation is undertaken but it fails. This model represents the events in types M1 ("*wife behaves inappropriately or refuses sex to husband*") and M3 ("*wife's neglect causes child's death*") of the male versions and in types F1 ("*husband neglects duties to family*") and F3 ("*husband's neglect causes child's death*") of the female versions. In these types, the antagonist spouse transgresses and the protagonist spouse attempts to resolve the conflict by taking some corrective action, which can be read as an attempt at mediation. Ultimately, however, the mediation attempt fails and the marriage ends. The difference between these two groups (M1/F1 and M3/F3) is the person who initiates the attempt at mediation, and the direction of the failure.

For a structuralist, the important observation gained in comparing these male and female versions is that the underlying deep structure, the relations among terms, and the outcome (i.e., an opposition between husband and wife, mediated by one spouse's corrective actions, which fails) is the same in all the accounts. What a structural analysis cannot explain is why the specific mediations attempted and the ways these fail vary. This limitation of structuralism is due in part to its antiquated theory of human cognition as based solely upon processes of binary opposition. A plethora of research in cognitive science in the last 30 years indicates that binary oppositions are one cognitive structure among many utilized by humans to organize their knowledge about the features of different domains. Clearly, in the *La Llorona* tales, a key opposition is posited between two characters who then undertake efforts to mediate or resolve it. But the form those efforts take and the outcomes that result depend upon ideas linked together in high level, more conceptually complex schemas or models of gender and marriage. It is evident, for example, that the female versions of the tale undergo more complex twists in plot because women have no culturally legitimated method available to them to end marriage. When wronged, wives cannot simply punish their husbands, throw them out, kill them, or leave them. Instead, in the tales, they attempt to correct misbehavior by the only means available—chastisement or the refusal of sex. These actions are shown always to anger husbands who then take revenge on wives, leaving them with no option but suicide in the

end. Suicide, however, produces a double bind, since it is by definition in the Catholic religion a sinful act that denies the person an afterlife in Heaven. Women who commit suicide not only end their present lives and marriages, but end all future reward as well. Men face no such double bind. Without knowledge of the cultural models being referenced in the tales, it would be difficult to determine why specific acts of mediation fail to resolve the initial opposition between spouses. Yet, such a determination is crucial if listeners are to understand and believe in the possibility of the final, horrific outcome these tales depict.

Type M3 of the male versions and type F3 of the female versions also conform to model II (table 4.6) in which mediation fails. The difference in these types is that the mediator of the conflict is a child whose death is the potential corrective measure to save the marriage. In M3, for example, the wife transgresses and her neglect causes the death of her child. She could, hypothetically, be remorseful about the child's death, learn from her mistakes, and vow to reform her behavior, thus preserving the marriage. But, instead, her remorse leads her to kill herself. In F3, the husband transgresses and his neglect causes the death of the child. Again, the child's death could cause the husband to come to his senses and reform, but instead, it is the wife who responds to the child's death by killing herself. In both cases, the marriage ends and the mediation fails.

These two variants draw upon and provide verification for a basic premise underlying the cultural model of marriage outlined in the previous section. Marriages are made for the purposes of having children, and the birth of children confers adult status on the spouses and legitimates their union (see Quinn and Mathews 1998). Consequently, when the behavior of one or the other causes the death of a child, that death nullifies the conditions of the marriage, which cannot then be salvaged through mediation. The actual way this problem is handled in M3 and F3, moreover, derives from the tenets of a cultural model of gender that posits different orientations for each sex toward children. Women are said to marry to have children, and children mean more to them than they do to their husbands (see also Mathews 1982, 1992:141–142). Moreover, women are presumed to have the sole responsibility for the care of children. Even though a husband is expected to provide for his family, the wife is the one who must insure that he does so for the sake of her children. In M3, therefore, the wife is depicted as being solely responsible for the child. Her transgression and neglect cause the child's death. Upon learning of the death, she accepts full responsibility and immediately kills herself because she recognizes that the child's death signifies the end of the marriage. The husband is the absent actor in this variant. He does not find the dead child and confronts his wife. She alone is both perpetrator and judge of her actions.

Men, according to cultural models of gender and marriage, are more interested in sex than in the children sex produces (see also Mathews 1992:141). Because men are like children themselves, moreover, they

cannot be counted on to assume responsibility. Thus the wife must be mother to both the husband and the child (Quinn and Mathews 1998). If she fails to rear the child, then she fails in her role of wife as well. On the other hand, while the man may fail as a father, this failure is not equated necessarily to failure in his role as husband. Instead, it is attributed indirectly to his wife, because she was not able to force him to do his fatherly duties. These premises underlie and give meaning to the actions unfolding in tale type F3 in which the husband's neglect causes the child's death. It is the wife who finds the child, experiences the resultant grief, realizes that the marriage is over, and acts to kill herself. The husband is never confronted about the death of the child, nor does he censure himself or take action against himself for it. While he may suffer in the end by no longer having a marriage, he is not depicted as directly accountable for his actions toward his children. Once again, even though both variants demonstrate structurally that no mediation is possible, the circumstances creating the situation, and the eventual outcome, are not parallel for the two sexes.

Many versions of the *La Llorona* tale do not end with the wife's death. Rather, her suicide creates a new permutation of the initial opposition between husband and wife, which is reconceptualized as one between the living and the dead. As Lévi-Strauss notes (1963:224), myths frequently encode a sequence of oppositional terms and a corresponding sequence of mediators. The first order of mediation is the attempt to resolve the initial contradiction or opposition between two elements or symbols in the myth. If this mediation fails, the terms may be transformed into a related set with a second order mediation attempted and so forth. Since many real world conflicts are never resolved, Lévi-Strauss contends that myths often continue these permutations of oppositions, each one slightly different from the last, until the intellectual impulse that produced the myth is exhausted (1966:229). E. K. Maranda and P. Maranda (1971:88–92) refer to these sequences as episodes of mediation within a single tale. They illustrate this sequencing graphically as a concatenation of models (see figure 4.4, models 3 and 4).

If we reexamine the *La Llorona* tales, we can see that the "consequence" function (labeled H in functional analyses, table 4.4) describes the outcome of a second order of mediation in the tales. The "consequence" tells what the characters experience and how they act after the death of the wife ends the marriage and the first order mediation, depicted as one spouse's efforts to correct another's misbehaviors, has failed. Often the "consequence" asserts that the spirit or ghost of the dead wife (the figure of *La Llorona* or the "Weeping Woman") is a type of mediator that acts to resolve the initial opposition between spouses, resulting in a state of gain for one or the other (model IV in the Maranda and Maranda scheme, see figure 4.4).

We can begin by examining tale types M1 ("*wife behaves inappropriately or refuses sex to husband*") and F1 ("*husband neglects duties to family*") to see how this second order mediation is depicted (see figure 4.6).

The concatenation stems from the posing, in the Consequence of the tales, a second episode of mediation enacted by the death of the wife. Male versions assert that the wife's death produces a loss for her (and by implication a gain for the husband) over the initial state (Model IV–, from the point of view of the main character, *La Llorona*). Female versions assert that the wife's death eventually leads to her gain (and by implication a loss for her husband) over the initial state (Model IV+). These second order ediations in each type variant are diagrammed as follows:

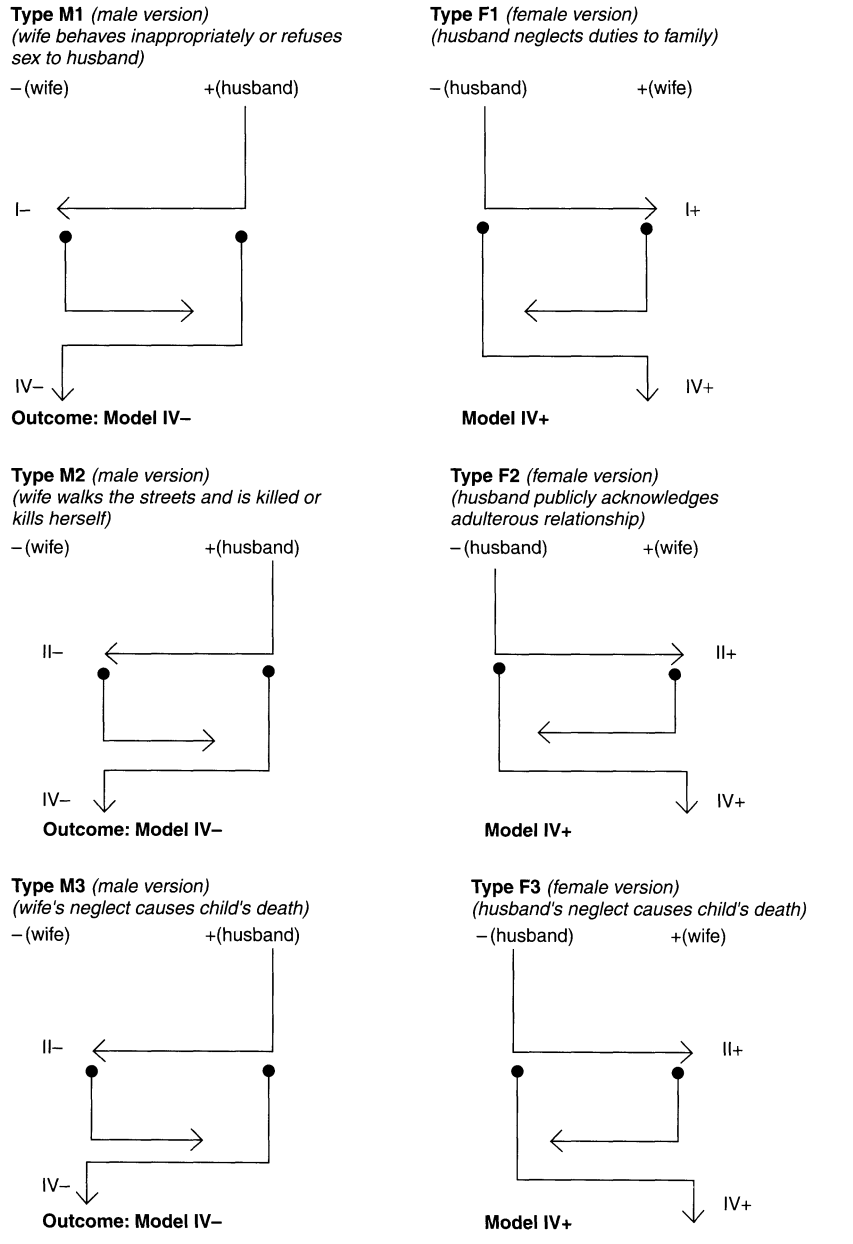


Figure 4.6 Diagram of the Concatenation of Models of Mediation in the Type Variants of the *La Llorona* Tales

The initial opposition and the conflict it engenders in these types conform to model 1 (figure 4.5) in which no mediation is deemed possible and the wife kills herself to end the marriage. Before the moral is drawn, however, the "consequence" function of the tale often asserts that the dead woman's spirit or ghost has mediated the original conflict in a specific way (see figure 4.6). Consider the following example of a tale conforming to Type M1:

> *La Llorona* was a young woman married to a good man. They lived together well and she worked hard. But one day, she started walking the streets and gossiping all the time. She never had food ready for her family. Her husband was very angry and beat her. She was filled with shame (*pena*), and one day she walked into the river and killed herself. And now her spirit is doomed to wander restlessly and she knows no peace.

A male informant used this version to suggest that the consequence for the antagonist wife is eternal suffering as a restless spirit wandering in Purgatory and denied entrance to Heaven because of her sinful death. The protagonist husband, moreover, benefits implicitly from this result because the end of his marriage to a lazy wife frees him to find a better one. When diagrammed from the point of view of the main character, *La Llorona*, this second episode conforms to model IV^-, in which the mediation has led to a loss over the original state for her but to a gain over the original state for her husband.

In type F1, the reverse often occurs. Consider the following example:

> *La Llorona* was a good woman married to a bad man. At first they were happy, and her husband worked hard. But then he began to drink and he was gone all the time. And she did not have enough money to buy food for her family. One day she followed him to the *cantina* and publicly scolded him for drinking all the time. This gave him much *coraje* (anger/rage), and when he returned home he beat her and turned her out of the house. She had nowhere to go and so she killed herself and her children so that they would no longer suffer at his hands, and he would be all alone. And even now, when men are out drinking and neglecting their families, the spirit of *La Llorona* often appears to them. And when they follow her, she leads them to their deaths so that their wives and children will no longer suffer either.

In this version, female informants suggest that the consequence for the protagonist wife is to end her suffering and that of her children (a gain—model IV^+) and to gain revenge on her husband by leaving him all alone (a loss for the husband—model IV^-). Yet, such an assertion would seem to contradict ideas found in earlier type variants depicting men as able to end marriages and find new spouses. What this ending implies is that a man may be able to turn his wife out, but if he is at fault and his wrongdoing becomes publicly known, it may be difficult for him to find another woman to marry. Thus he could end up alone, denied both the privileges

of marriage and the community standing that all men value highly and that can only be obtained through marriage.[10]

Type F1, moreover, holds out another possibility in the "consequence" not paralleled in type M1. The wife as a spirit may take revenge against all husbands who transgress by killing them. This coda is quite interesting because female informants are asserting that even though they do not have the option in life to end a marriage other than by suicide, they may, after death, be able to assist other suffering wives by returning from the grave to kill their errant husbands. This assertion warns that women may not be as powerless as they seem and is, in fact, the statement of another moral to men—they should be careful about committing a transgression because they too may have more to lose than a marriage; they may lose their lives.

In types M2 ("*wife walks the streets and is killed or kills herself*") and F2 ("*husband publicly acknowledges adulterous relationship*") mediation of the initial opposition also fails. In these types, the consequences of the failures are somewhat different, reflecting the double bind experienced by women. Consider the following example of type M2:

> Once a young man was married to a hard-working young woman. All was well, but then she began to walk the streets and she was found with another man. Everyone knew, but her husband did not know. When he found out, he was filled with rage (*coraje*), and he killed her and her lover. Now her spirit is forever restless and doomed to wander without rest. Often people hear her weeping and wailing in the night. And all because she was a bad woman who shamed her husband.

In this version, the protagonist husband is said to have taken revenge against his antagonist wife by killing her, and she is then, ironically, doomed to spend eternity in Purgatory because of her sin against him. So the wife as the main character experiences a loss from the original state while the husband implicitly gains (model IV^-, figure 4.6).

In type F2, the consequence of death for the wife is that she ends her suffering at his hands (model IV^+, a gain). Consider the following version:

> *La Llorona* married a young man and went to live in his house. They had children and worked hard and all was well. But he began seeing another

[10] Taggart (1990) points out that Nahuat men of Huitzilan and Yaonahuac, Mexico (see also, Taggart 1979) as well as Mayan men of Zinacantan (Laughlin 1962) tell "Lost Wife" tales that describe men who lose and then struggle arduously to find their mates. He suggests that these tales reflect their anxieties about "losing the nurturance of women on whom they heavily depend, and they capture the actual experience of many young husbands whose wives have returned to their parents after a family quarrel" (1990:211). This deep fear, which Taggart attributes partly to the dynamics of marital relations in patrilocal extended households, is also characteristic of the men in my study. This is probably one reason why women's versions of the *La Llorona* tale, which imply that a wife's suicide leads to male abandonment, resonate with and have the effect of cautioning men to be careful about how they treat their wives.

> woman and had a son with that woman. He was gone all the time and did not provide for his family. *La Llorona* begged him to once again care for his family. But then one day she heard that he had brought his other woman and son to town and had asked the Priest to officiate at the boy's First Communion in the church. She was filled with shame (*pena*) and that evening she drowned herself and her children so that they would not suffer from her husband's public betrayal of them.

In this version, *La Llorona* is aware of her husband's infidelity and begs him to redirect his attention back to his own family. But this attempt at preserving the marriage fails when he publicly acknowledges the other woman and the illegitimate son. For her, there is no recourse but suicide, as the marriage is irrevocably broken. Her death and that of her children is depicted as ending their suffering (a gain) at his hands and implies a loss for him of his legitimate family and possibly of public standing for causing this chain of tragic events through his inappropriate behavior. So even though mediation fails to nullify the initial opposition and the wife dies in both types M2 and F2, in one case she is depicted as the loser and in the other as the victor over her husband.

In types M3 ("*wife's neglect causes child's death*") and F3 ("*husband's neglect causes child's death*") the child dies and mediation also fails. Yet, the consequence again asserts that the outcome may undergo further change after the death of the wife. In type M3, for example, the wife walks the streets and neglects her child who dies of a sudden illness while she is gone. Her anguish and shame lead her to commit suicide, and she is said to then wander in Purgatory forever for her sin. Hence she experiences no gain through the ending of marriage, but her husband does (model IV^-, figure 4.6). In type F3, the antagonist husband neglects his family and his neglect causes a child's death. The wife experiences overwhelming sadness and kills herself and sometimes her remaining children. The husband is variously depicted as publicly shamed by his wife and child's death, implying that he has lost the privileges of marriage and adult status forever since no woman would ever marry him again; or he, like all men, is depicted as subject to the revenge taken by the spirit of the dead woman who lures them, in the dead of night, into the river. The wife in type F3, therefore, gains (model IV^+) and the husband loses.

These tales are designed to show that immoral acts will likely lead to the end of marriage. While the first order mediation fails to resolve the opposition between spouses and the marriage ends, the second order mediation by a spirit sets up the possibility in the consequence unit that one or the other may still gain in the end. Although Lévi-Strauss's method helps us determine that sequences of mediation occur in all variants of the tales, its emphasis on structure over content limits its usefulness in determining why the first and second order mediations take the form that they do. Yet, the positioning of the dead wife's spirit as mediator is crucial because it enables the narrator to use the consequence function of the tale either to

reaffirm or to protest against the dominant beliefs about the order of things. Indeed, as Cardozo-Freeman (1986) suggests, folklore may often act as a safe vehicle for protest against the harsh restrictions and attitudes that women experience in certain societies. While women may not be allowed to rebel or change a situation, there is, I would argue, more latitude for them to express their frustrations in accepted genre tales which are, on the surface, fictional, and therefore less challenging than direct confrontation.

Thus, it can be argued that male versions, which attribute the wife's eternal suffering to her own misbehavior, implicitly reaffirm the public ideology of male dominance in the community. The initial opposition is mediated by the wife's death with the result that she loses and her husband gains. Hence, the secondary moral to be drawn from the consequence of types M1, M2, and M3 is that a wife's transgression against the husband is a transgression against authority and is doomed to failure—a failure that costs her greatly in terms of her life and her eternal rest.

Types F1, F2, and F3, however, assert a very different consequence to the wife's death. These variants express women's protests against male authority as upheld by public ideology. The husband's transgression may lead to his wife's death, but her death is shown to result in her own gain and his loss—of marital privileges, of public status and reputation, and possibly of his life. Here, too, a second order moral or, in this case, a threat, is made. Male versions stress that suicide itself is a sin and dooms the wife to eternal punishment for the immoral act that dissolved the marriage. Female versions stress that suicide is a method for women to do more than end marriage. Through that action they may also exact a more lasting punishment against their husbands, who are still enjoying the privilege of life. Furthermore, women as spirits have a special power and option that living women do not—the ability to kill other husbands who transgress.

It is important in this regard to emphasize the method by which men die in the tales. Those who see the spirit of *La Llorona* late at night follow her. She lures them into the river where they drown. Drowning was considered by the Zapotec, and many other Native American groups, as the worst death a person could suffer, since a person who drowned could never pass into the afterworld but forever remained in a state of limbo between the living and the dead. In this consequence to the female variants, women are asserting that a fate equivalent to their own may come to men who act immorally toward their wives.

This analysis of the mediation of the central opposition helps us to understand something about the ultimate meaning of each tale as a structural whole. Moreover, the determination with more specificity of the different models of mediation structuring each type variant enables us to pinpoint some key differences between the likely outcomes of a marital transgression for husbands and for wives. Mediation is either impossible (model I) or it fails (model II) to resolve the initial marital opposition

between husband and wife. Thus the marriage ends and the moral, be a proper spouse or else, is upheld.[11] The wife's death, however, sets up a second order mediation through the medium of her ghost or spirit, which resolves the initial opposition in the marriage and leads to a gain for the husband and a loss for the wife, thereby reaffirming male authority in marriage, or it leads to a gain for the wife and a loss for the husband, thereby questioning the basis for such absolute male authority. This analysis demonstrates, as E. K. Maranda and P. Maranda write (1971:52), that the type of mediation as formalized in the models cannot be read or understood independently from the message of the tale. And the type of mediation specified in different versions of the tale, moreover, is crucial for determining how those versions might be used by individuals in natural contexts to either reinforce or undermine a public ideology of male dominance (see Mathews n.d. for further work on this latter topic).

Conclusion

After I completed preliminary work on the analyses presented in this chapter, I sent them to a prominent folklorist and requested feedback. He replied that while he found my material fascinating, he wished that I had not "cluttered up" my presentation with all those formal cognitive and structural models. He suggested that I should "forget" the modeling and concentrate, instead, on writing my own interpretation of meaning of the tales. Needless to say, I was dismayed and somewhat taken aback by this response. I thought long and hard about his comments. Were the methods I employed merely formal exercises that "refound," in needlessly complex ways, conclusions that I could have arrived at more directly through cultural immersion and introspection, or were these methods instrumental in advancing my theoretical understanding of the meanings of the *La Llorona* tales? My conclusion is that far from being irrelevant, the methods I employed were essential for helping me specify and then interpret the meaning of gender variation in the folktales, for helping me advance my theoretical ideas about the role of schemas in organizing knowledge and in shaping forms of linguistic expression, and for helping me delineate the complex relationship that exists between linguistic form and the content conveyed in genres like morality tales.

The basic goal of any scientific enterprise, including anthropology, is to figure out what we know and how we know it. Only then can those of us studying cultural meanings begin to interpret these meanings in a way that is verifiable. As Hymes (1971) points out, an older approach to the study of myth and folktales emphasized the search for the one authentic or traditional text that the analyst then interpreted for its deeper psychosocial meanings. The danger of such an approach, he notes, is that the analyst's

[11] See note 9.

own theoretical biases may shape the interpretation in ways unsupported by the evidence at hand. The evidence in myths and folktales, he argues, must be examined for the features and relations revealed, especially since these features may be specific to certain genres or cultures. Only then can the analyst begin to undertake a fuller explication of the "meaning" of the tales.

I found Hymes' observations relevant to my own fieldwork experience. Had I sought to advance a psychosocial interpretation of the *La Llorona* tales prior to completing the analyses presented here, I might have been inclined to argue that despite the existence of alternate male and female versions, the tales all acted to reinforce male dominance because women always committed suicide in the end. It was not until I specified more precisely the nature and range of this variation between male and female accounts that I was able to determine that they were really about more than just presenting alternative perspectives on marital problems.

The use of an eclectic approach to the analysis of these tales, moreover, proved crucial in enabling me to recover from them the schemas organizing people's understandings of human motivation, gender roles, and marital expectations. Additionally, these methods helped me delineate three different sources of meaning found in the tales and, therefore, arrive at a fuller theoretical understanding of the interlocking nature of cognitive schemas. To the extent that these analyses also revealed patterns that accounted exhaustively for the variation present in the tales, they proved important for validating information on cultural models gained through other ethnographic fieldwork. Finally, these analyses demonstrate that the study of linguistic structure cannot be divorced from the study of content. Rather, the form of expression is dependent largely upon the types of ideas being conveyed, while linguistic form or genre, in turn, acts as a type of schema that shapes the way in which individuals construct and communicate their ideas to one another.

This analysis began at the unit of basic category or phoneme in the corpus. The moves of the plot are those key events that order the tales. The structural organization of moves or grammar is itself a cultural schema that remains invariant across all versions. I have argued that this grammar or schema is not arbitrary in form but that the sequencing of the key units and the content they contain are derived from a cultural model of human behavior such that one person's action produces an emotional response and counteraction on the part of another in a stepwise fashion that is convincing to listeners.

The analysis continued with a delineation of the functions and actions of the characters that led to a more specific level of content analysis. The content conveyed in these tales is also convincing precisely because, in specifying the range of behaviors that are appropriate and inappropriate for each spouse, these tales reference more broadly shared, conceptual schemas or models of gender roles as realized in marriage. Thus the characters' actions in the tales are seen to be "naturally" motivated by

emotional reactions to behaviors that are either in keeping with or are clear violations of the expected roles of the spouses in a marriage.

A subsequent schematic analysis of functions revealed the existence of three key variants of the tales in each gender group. A systematic comparison of these variants showed that, even though male and female versions parallel each other in some key respects, they are not the same tales with a simple shift in perspective. Rather, women are portrayed as having more limited behavioral options than men and as unable to end a marriage except by suicide. Moreover, they always die in the end.

This empirical demonstration of patterned variation then led to a broader question: how are we to understand the meaning of these interpretive differences? Lévi-Strauss's structural method suggested that the *La Llorona* tales, like other myths, were all about examining a fundamental postulated opposition, in this case, between husbands and wives, and about attempts to resolve that opposition. For structuralists, however, the form of all myths and tales is the same, two terms in opposition progressing through a series of mediations. However, the content of the *La Llorona* tales shapes the nature of the mediations undertaken in significant ways. The first episode of the tales suggests that either no mediation of the opposition between spouses is possible, or that if one is undertaken, it will fail. Such a structure makes sense because these morality tales are all about convincing people that inappropriate behavior will inevitably lead to a bad outcome, the end of marriage. Because men and women are also using the tales to communicate with one another about contested views of male dominance, they often include a second order mediator, the dead wife's ghost or spirit. Depending upon the version told, this spirit is able to resolve the initial opposition between spouses, leading to a gain for one and a loss for the other. The analysis suggests that the form and type of mediation posed at each level derive directly from the premises of the cultural models of marriage and gender referenced in the tales, and that they vary in accordance with the goals of the teller.

The father-in-law quoted at the beginning of this chapter, for example, used a male version of the tale to chastise his daughter-in-law. This version ended after the first-order mediation failed. Its content and structure, therefore, highlighted the wife's inappropriate behavior and linked it to the end of the marriage and her death. When the daughter-in-law told the version acquired from her mother, however, it not only emphasized a different perspective (i.e., that the husband's wrongdoing in the marriage as cause of his wife's death), but it also contained a second order mediation that suggested that the spirit of *La Llorona* would come back to take revenge on all errant husbands. This tale was the daughter-in-law's response to her father-in-law's assertion of male dominance and his attempt to ensure female submission. It suggests not only that women can "think beyond" the dominant ideology, but that they may encode messages into folktales that communicate these thoughts to men in ways that can be very convincing, yet not as threatening as direct confrontation would be. These

findings about the relationship between the content a teller wishes to convey, and the particular form of mediation chosen in recounting the tale, moreover, provide the basis for a future exploration of how these different variants are deployed by men and women in natural settings to advance claims, evaluate one another's behaviors, jockey for public support, and perhaps contest dominant ideological beliefs (see Mathews n.d. for a preliminary exploration of these issues).

The main conclusion of this chapter, then, is two-fold: theoretical insights depend upon the use of systematic methods, and an eclectic approach, drawing upon a range of techniques, is necessary when one is attempting to recover cultural schemas and models from natural discourse. Eclecticism is especially crucial when the genre requires economy of expression, so that references to shared schemas are often cryptic, requiring listeners to fill the gaps of missing information by drawing upon their own tacit cultural understandings of the situation being described. Recovery of such information requires careful, empirical investigation and systematic comparison across versions. Only then, as Hymes (1971) points out, can the investigator attain descriptive adequacy (Chomsky 1964:923–925) and provide a valid account of the knowledge needed to both generate and interpret folktales within a particular culture.

References

Abelson, Robert P. 1975. Concepts for presenting mundane reality in plans. In *Representation and understanding*, ed. D. G. Bobrow and A. Collins, pp. 273–310. New York: Academic Press.

Baldwin, Karen. 1985. "Whoof!" A word on women's roles in family storytelling. In *Women's folklore, women's culture*, ed. R. A. Jordan and S. J. Kalcik, pp. 149–162. Philadelphia, PA: University of Pennsylvania Press.

Cardozo-Freeman, Inez. 1986. Games Mexican girls play. In *Women and folklore: Images and genres*, ed. C. R. Farrer, pp. 12–24. Prospect Heights, IL: Waveland Press. (Reissued Version of 1975 special issue of *Journal of American Folklore*.)

Chernala, Janet M. 1988. Some considerations of myth and gender in a northwest Amazon society. In *Dialectics and gender: Anthropological approaches*, ed. R. R. Randolph, D. M. Schneider, and M. N. Diaz, pp. 67–79. Boulder, CO and London: Westview Press.

Chomsky, Noam. 1964. The logical basis of linguistic theory. In *Proceedings of the Ninth International Congress of Linguistics*, ed. H. Lunt, The Hague: Mouton & Co.

Colby, Benjamin N. 1973. A partial grammar of Eskimo folktales. *American Anthropologist* 75(3):645–662.

Colby, Benjamin N. and Lore M. Colby. 1981. *The daykeeper: The life and discourse of an Ixil diviner*. Cambridge, MA: Harvard University Press.

Collier, Jane Fishburne. 1974. Women in Politics. In *Woman, culture, and society*, ed. M. A. Rosaldo and L. Lamphere, pp. 89–96. Stanford, CA: Stanford University Press.

Crain, Mary M. 1991. Poetics and politics in the Ecuadorian Andes: Women's narratives of death and devil possession. *American Ethnologist* 18(1):67–89.

D'Andrade Roy G. 1987. The folk model of the mind. In *Cultural models in language and thought*, ed. D. Holland and N. Quinn, pp. 112–148. Cambridge: Cambridge University Press.

———. 1995. *The development of cognitive anthropology*. Cambridge: Cambridge University Press.

Dundes, Alan. 1971. Making and breaking of friendship in a structural frame in African folk tales. In *Structural analysis of oral traditions*, ed. P. Maranda and E. K. Maranda, pp. 171–188. Philadelphia, PA: University of Pennsylvania Press.

Dwyer, Daisy Hilse. 1978. *Images and self-Images: Male and female in Morocco*. New York: Columbia University Press.

Farrer, Claire R. 1986. Introduction. Women and folklore: Images and genres. In *Women and folklore: Images and genres*, ed. C. R. Farrer, pp. xi–xii. Prospect Heights, IL:Waveland Press. (Reissued Version of 1975 special issue of *Journal of American Folklore*.)

Fischer, J. L. 1956. The position of men and women in Truk and Ponape: A comparative analysis of kinship terminology and folktales. *Journal of American Folklore* 69:55–62.

Gottleib, Alma. 1989. Hyenas and heteroglossia: myth and ritual among the Beng of Cote d'Ivoire. *American Ethnologist* 16(3):487–501.

Holland, Dorothy and Debra Skinner. 1987. Prestige and intimacy: The cultural models behind Americans' talk about gender types. In *Cultural models in language and thought*, ed. D. Holland and N. Quinn, pp. 78–111. Cambridge: Cambridge University Press.

Horcasitas, Fernando and Douglas Butterworth. 1963. Llorona. *Tlalocan* 4:204–224.

Hymes, Dell. 1971. The "wife" who "goes out" like a man: Reinterpretation of a Clackamas Chinook myth. In *Structural analysis of oral tradition*. ed. P. Maranda and E. K. Maranda, pp. 49–80. Philadelphia, PA: University of Pennsylvania Press.

Jacobs, Melville. 1960. *The people are coming soon. Analyses of Clackamas Chinook myths and tales*. Seattle, WA: University of Washington Press.

Janvier, Thomas A. 1910. *Legends of the City of Mexico*. New York: Harper and Brothers.

Johnson, N. S. and Jean M. Mandler. 1978. A tale of two structures: underlying and surface forms in stories. *Poetics* 9:51–86.

Kearney, Michael. 1969. La Llorona as social symbol. *Western Folklore* 28:199–206.

———. 1972. *Winds of Ixtepeji*. New York: Holt, Rinehart, and Winston.

Kirtley, Basil F. 1960. La Llorona and related themes. *Western Folklore* 19:155–168.

Kongas, Elli and Pierre Maranda. 1962. Structural models in folklore. *Midwest Folklore* 12:133–192.

Lakoff, George. 1987. *Women, fire, and dangerous things*. Chicago and London: University of Chicago Press.

Laughlin, Robert. 1962. *Through the looking glass: Reflections on Zinacantán courtship and marriage*. Harvard University, Cambridge, Ph.D. Dissertation.

Leach, Edmund R. 1971. Kimil: A category of Andamanese thought. In *Structural analysis of oral traditions*, ed. P. Maranda and E. K. Maranda, pp. 22–48. Philadelphia, PA: University of Philadelphia Press.

Lévi-Strauss, Claude. 1958/1976. La Geste d'Asdiwal. *Annuaire* 1958–1959. Paris, France: Ecole Practique des Hautes Etudes, pp. 3–43. Reissued in 1976 and translated by Monique Layton, in *Structural Anthropology*, Vol. II by Claude Lévi-Strauss. New York: Basic Books.

———. 1963. The structural study of myth. In *Structural anthropology* (translated from the French by Claire Jacobson and Brooke Grundfest Schoepf), pp. 206–231. New York: Basic Books.

Mandler, Jean M. 1984. *Stories, scripts, and scenes: Aspects of schema theory.* Hillsdale, NJ: Lawrence Erlbaum Associates.

Maranda, Elli Kongas and Pierre Maranda. 1971. *Structural models in folklore and transformational essays.* The Hague and Paris: Mouton.

Mark, Vera. 1987. Women and text in Gascon tall tales. *Journal of American Folklore* 100(398):504–527.

Mathews, Holly F. 1982. Sexual status in Oaxaca, Mexico: an analysis of the relationships between extradomestic participation and ideological constructs of gender. Ph.D. Dissertation. Department of Anthropology, Duke University.

———. 1985. "We are Mayordomo": a reinterpretation of women's roles in the Mexican cargo system," *American Ethnologist* 12(2):285–301.

———. 1992. The directive force of morality tales in a Mexican community. In *Human motives and cultural models*, ed. Roy D'Andrade and Claudia Strauss, pp. 127–162. Cambridge: Cambridge University Press.

———. n.d. The use of the *La Llorona* tales in natural contexts. Unpublished manuscript. Greenville, NC: Department of Anthropology, East Carolina University.

McCollum, Chris. 2000. The cultural patterning of self-understanding: A cognitive-psychoanalytic approach to middle-class Americans' life stories. Ph.D. Dissertation. Durham, NC: Duke University.

Mirande, A. and E. Enriquez. 1979. *La Chicana.* Chicago: University of Chicago Press.

Mitchell, Carol. 1985. Some differences in male and female joke-telling. In *Women's folklore, women's culture*, ed. R. A. Jordan and S. J. Kalcik, pp. 163–186. Philadelphia, PA: University of Pennsylvania Press.

Propp, Vladimir. 1958. *Morphology of the folktale.* Trans. L. Scott. Publication 10 of the Indiana University Research Center in Anthropology, Folklore, and Linguistics.

———. 1986. *Morphology of the folktale.* Austin, TX: University of Texas Press. Paperback Reissue of Revised Edition published as Volume 9 of the American Folklore Society Bibliographical and Special Series; and as Publication 10 of the Indiana University Research Center in Anthropology, Folklore, and Linguistics.

Quinn, Naomi and Dorothy Holland. 1987. Culture and Cognition. In *Cultural models in language and thought*, ed. D. Holland and N. Quinn, pp. 3–40. Cambridge: Cambridge University Press.

Quinn, Naomi and Holly F. Mathews. 1998. Rethinking cross-cultural variation in adult love in light of a Mexican case. Paper presented to the 97th Annual Meeting of the American Anthropological Association, Philadelphia, PA.

Rowe, Karen E. 1986. To spin a yarn: the female voice in folklore and fairy tale. In *Fairy tales and society: Illusion, allusion, and paradigm*, ed. R. B. Bottigheimer, pp. 53–74. Philadelphia, PA: University of Pennsylvania Press.

Rumelhart, David E. 1975. Notes on a schema for stories. In *Representation and understanding*, ed. D. G. Bobrow and A. Collins, pp. 211–236. New York: Academic Press.

Sapir, J. David. 1977. The fabricated child. In *The social use of metaphor: Essays on the anthropology of rhetoric*, ed. J. D. Sapir and J. C. Crocker, pp. 193–237. Philadelphia, PA: University of Pennsylvania Press.

Schank, Roger and Robert Abelson. 1977. *Scripts, plans, goals and understanding.* Hillsdale, NJ: Lawrence Erlbaum Associates.

Strauss, Claudia. 1992. Models and Motives. In *Human motives and cultural models*, ed. R. D'Andrade and C. Strauss, pp. 1–20. Cambridge: Cambridge University Press.

Strauss, Claudia and Naomi Quinn. 1997. *A cognitive theory of cultural meaning.* Cambridge: Cambridge University Press.

Taggart, James M. 1979. Men's changing image of women in Nahuat oral tradition. *American Ethnologist* 6(4):723–741.

———. 1990. *Enchanted maidens: Gender relations in Spanish folktales of courtship and marriage.* Princeton, NJ: Princeton University Press.

Tan, Amy. 1988. *The joy luck club.* New York: G.P. Putnam and Sons.

Thompson, Stith. 1955. Motif-index of folk-literature. Vol. 1. Bloomington, IN: Indiana University Press.

Wolf, Margery. 1968. *The house of Lim.* Engelwood Cliffs, NJ: Prentice Hall.

Chapter Five

Finding Culture in Narrative

Jane H. Hill

Introduction

The study of narrative in its many forms is emerging as an important method in cultural and linguistic anthropology. Students of narrative can build on a solid set of questions and hypotheses about this genre that have emerged from centuries of study in literature, folklore, sociolinguistics, and discourse analysis, as well as from the anthropological tradition itself. Cultural and linguistic anthropologists now agree on one fundamental point: that narratives are not merely overtly "about" some "content," such as what happened when, where, and to whom, but that they somehow make public the covert underlying presuppositions that organize the worlds in which speakers live. The cultural knowledge that resides in these presuppositions is often so "transparent" to speakers that it is unutterable. Anthropologists build on this insight through the application of methods that will permit us to identify these covert—yet publicly available—presuppositions, so that we can make the same kinds of inferences that speakers must make when they find meaning in narratives. In the analyses below, I review some of these methods. They build on three general assumptions: first, that the narrative genre is universally defined by the iconic match between a sequence of narrative clauses and a sequence of events, second, that narratives are universally built from certain structural components, and, third, that interlocutors exploit covert knowledge of the narrative icon and of these components in the work of building coherence, a collaboratively achieved sense that an interpretable stretch of talk has been produced. I argue that the best opportunities for analysis arise when it is difficult to see how this coherence has been achieved: where there seem to be logical gaps, logical clashes, and unexpected silences, or disturbances and violations of the presumed default universal structures of narrative. The assumption behind the analyses illustrated here is that these incoherencies are merely apparent—that in fact interlocutors in a discourse community are able to "fill in" the gaps and silences, resolve the clashes, and draw inferences from the violations, by drawing on covert cultural knowledge and on universal human narrative competence.[1] Thus the gaps, clashes,

[1] I use "competence" in the sense proposed by the linguist Noam Chomsky. That is, I assume that part of the human cognitive endowment, developed in evolution, is the capacity to produce and interpret narratives.

silences, and violations become, for the analyst, clues to what that cultural knowledge and narrative competence might be. Even where narratives seem normal and reasonable to the analyst, it is her job to "make them strange," to seek for other ways that the narrative might have been told and understand why, in local cultural terms, such ways were not chosen. Thus these methods can be used to explore, not only exotic narrative traditions, but the most mundane and familiar talk, in order to expose the—often surprising—presuppositions that underlie it.

The complex meanings that are created in narrative can be explored with any of the several methods that are used in the analysis of text and discourse more broadly; examples of such methods are found in other chapters in this volume (see especially the chapters by Mathews, Strauss, and Quinn).[2] However, this chapter looks only at discourse constraints that are specific and peculiar to narrative, and shows how attention to these can motivate hypotheses about culture and context. I do not illustrate here methods that depend on instrumental analysis, as with the analysis of intonation. Nor do I look at visual dimensions of narrative performance such as gaze. However, such methods are becoming increasingly important, as we recognize that any gesture that can be made by the human voice or body can constitute what Gumperz (1982) calls a "contextualization cue," that signals the kind of meaning that an interlocutor should seek in the text.

Many kinds of narratives are of interest to students of culture, but I focus here on three examples of a subgenre of so-called conversational narrative: oral narratives that emerged as interactional moves within larger conversations. While some conversational narratives last for several minutes, the three narratives discussed here are very brief, permitting us to look at them in some detail within the scope of a single chapter.[3] Following the example set by Strauss (this volume), I present these materials here for the first time. My previous publications on narrative (Hill 1990a,b, 1995, 1997, 2000; Hill and Zepeda 1992; Hill and MacLaury 1995) have been about texts that were not in English. In order to avoid having to spend time sketching in the background grammatical apparatus of languages that would be known to very few readers of this article, I discuss only briefly a translation of a Tohono O'odham text, "The Rattlesnake

[2] The state of the art of the study of narrative in anthropology and related disciplines is reviewed by Ochs (1996), Ochs and Capps (1996). The journal *Narrative Inquiry* (formerly The *Journal of Narrative and Life History*) is an interdisciplinary journal with many papers on narrative. Bernard and Ryan (1998) review a wide range of methods in text analysis. Hanks (1989) provides a review of the theory of text and textuality; recent theoretical work that emphasizes that "texts" are the product of "contextually contingent semiotic processes" (p. 2) is reviewed by Silverstein and Urban (1996).

[3] Longer narratives obviously permit other methods. An analysis of a relatively long narrative (17 minutes) is found in Hill (1995). The longest narrative I have ever collected in a single field session is 45 minutes; K. Hill (1985) and Hill (1990a) examine some of the features of the 45-minute long text. Note that long narratives are often divided into episodes, where each episode replicates the narrative structures discussed in this chapter, and is embedded in a larger narrative organization that also exhibits those structures.

Story," in order to illustrate a particular type of treatment of temporal sequence, and then turn to two narratives, "An immense fall" and "A dollar and a quarter an hour" that were told in English—although the teller of "A dollar and a quarter an hour" has Spanish as her first language. I give these narratives titles for convenience here; there is no evidence that the narrators thought that they had titles.[4]

Before turning to the detailed analysis of the individual narratives, I review some background theoretical foundations. Linguistic anthropologists today argue that discourse—the production of the talk and texts that are the vehicles of so much human interaction—is the most important place where culture is both enacted and produced in the moment of interaction (see Farnell and Graham 1998 for a review of this perspective). Not only is discourse obviously a key site for human interaction and thereby deserving of anthropological attention, but placing research on discourse at the center of the study of culture can help us resolve an important dilemma for cultural analysis today: Is culture to be found in constraining "structure" or in emergent "agency"? (Farnell and Graham 1998). To study discourse is to examine both dimensions simultaneously, since discourse is "duplex" not only in the sense that it both enacts and produces culture, but in the sense that as social actors produce discourse, they simultaneously negotiate emergent meanings and draw on shared understandings—including understandings about the organization of genres of talk and text—that create the very possibility for such negotiation.

The study of narratives provides a particularly favorable site for what has been called a "discourse-centered" study of culture (Sherzer 1987; Urban 1991; Farnell and Graham 1998). First, while of course many questions and problems remain for future generations, most students of narratives assume that they share, cross-culturally, certain universal structural properties that are reviewed below in the discussion of the individual narratives. These appear very early in narratives produced by children (cf. R. Scollon and B.K. Scollon 1981; Berman and Slobin 1994 for two important crosslinguistic studies of child narrative). While such universal discourse constraints are

[4] Narratives that become "entextualized" (are available to be retold in new contexts—see Silverstein and Urban 1996, and note (5) below) may acquire informal titles, as when a speaker is urged to "tell about the time when . . . ," or "tell about what happened to you in . . ." Jacobs (1957) reported a formal system of titles for narratives in the winter myth cycle among the Chinookan peoples of the lower Columbia River. Narratives were known by the names of their principal characters, in order of social rank, *not* in order of their prominence in the narrative itself. A famous example is the story, "Seal and Her Younger Brother," which, in the telling by Victoria Howard, includes the line "In vain, in vain I tried to tell you" made famous by Dell Hymes (Hymes 1981), who at first used his own title, "The wife who goes out like a man." In fact the most important character in "Seal and Her Younger Brother" is Seal's daughter (she utters the famous line); Seal's younger brother dies fairly early in the story and speaks no lines. However, since Seal's daughter is of relatively low rank, her name does not appear in the formal Chinookan title. Jacobs' example shows that inquiring into the possible titling of narratives is worth doing; both formal and informal titling may prove to be of cultural-analytic interest.

always violable (and the literature provides many examples of such violations), we can draw on an understanding that speakers possess knowledge of these universal constraints, so that violations of them can be made "visible"—through forms of marking that are often highly local and culturally specific—and trigger inferences about meaning. Interlocutors use this background knowledge about the structure of narrative discourse as a set of relatively firm anchors for the inferential reasoning that produces emergent meanings.

A second reason that narrative analysis is useful in the study of culture is that narratives are extremely common. While long and highly "entextualized"[5] narratives such as the winter myth cycles of indigenous communities in North American or the Christmas-time reading of the story of the birth of Jesus in Christian families may be restricted in their contexts of occurrence to particular seasons, locations, and tellers, short narratives of the type I analyze here occur with very high frequency in ordinary conversation. Narratives are often embedded in other types of discourse, such as explanatory or argumentative discourse, moral reflection, and the like. Narratives can be very ephemeral, occurring only once and never circulating again, but narrative does seem to be particularly susceptible to what Urban (1996) calls "replication." Narratives are often "retellable," whether by the original narrators or by audience members who may pass them on. Narratives can be bounded elements that begin and end in the same interactional context, but they can also be open-ended, as with the "life stories" (Linde 1993) constructed by middle-class Americans out of many episodes that are considered to be "connected" by a cultural logic of the life course, or the "narrative of the law" constructed in a series of decisions by the U.S. Supreme Court (Mertz 1996).

Third, narratives are interesting because they have diverse functions, and often seem to be producing many simultaneous cultural results. Of special interest recently for anthropologists have been narratives through which people create "selves" by imposing a discursive order on events and observations that, in themselves, have no particular coherence and may even seem unintelligibly cruel, but can be made meaningful as part of a "life" or "career" through the work of narrating (cf. Harding 1992; Stromberg 1993; Capps and Ochs 1995; Ochs and Capps 1996). Ochs and Taylor (1992, 1994; Ochs, Smith, and Taylor 1996) have shown how dinner-table narratives told by parents and children in middle-class American families enact implicit gendered hierarchies. Mathews (this volume) discusses the ways that men and women

[5] By "entextualization" I refer to the cultural processes by which a stretch of discourse becomes relatively fixed, shareable, and transmittable, as when something is said, or when something someone else said, is repeated, written down, translated, summarized, etc. The most extreme form of entextualization is rigid fixation, which is most obvious in writing but can also occur in dominantly oral forms such as advertising jingles or the Lord's Prayer. Another critical dimension is "contextualization," the cultural processes by which a stretch of discourse becomes utterable or usable at a particular moment (see Silverstein and Urban 1996 and papers in that volume for detailed consideration of these concepts).

shape a canonical text to reflect their ideas of the relative degrees of agency of males and females. Papers in Briggs (ed.) (1996) discuss diverse functions of narrative in a variety of speech communities. The work of Ochs and her colleagues, especially Ochs and Capps 1996, emphasizes the way that narratives may be fragmented and distributed across interactional sequences such that conversational narrative structures of the type observed by Labov (1972b) and Labov and Waletzky (1967) are no longer obvious. The present chapter discusses examples where this structure is visible.

Obviously there are methodological considerations, such as elicitation, recording, transcription, and the ethical treatment of narrators, that are prior to the kind of analysis that I illustrate below. Since these are not the main concern of this chapter I provide only the following brief overview. For elicitation, I find the work of Labov (cf. 1972a,b) especially useful, since most of the narratives that I have collected are part of sociolinguistic interviews. However, many scholars strongly prefer to work on so-called socially occurring narratives: Narratives that emerge in everyday contexts of family, work, play, religious observation, courtroom procedure, medical treatment, scientific conferences, and the like, that are not elicited or organized by the anthropologist. Here, the standard sources on the conduct of participant observation apply; see, for instance, K. M. DeWalt, B. R. DeWalt, and Wayland (1998) and Farnell and Graham (1998). An especially fine study of socially occurring speech that includes the analysis of several narratives and has many good hints on field procedure is M. H. Goodwin (1990). Ochs and Capps (2002) attend especially to the shared production of narrative in family settings. For recording, good sources include C. Goodwin (1993) and Farnell and Graham (1998). However, recording technology is changing rapidly, and many new techniques of instrumental analysis, such as the use of pitch-tracking programs for the analysis of intonation (cf. discussion in Farnell and Graham [1998:427–432]), work best if recording has been done with the most advanced equipment such as digital recorders. In addition, such interactional dimensions as gaze, posture, and gesture are turning out to be important for narrative analysis, requiring the use of video recording (cf. Farnell 1995). The choice of a transcription method is of considerable theoretical importance (Ochs 1979; Edwards and Lampert, eds., 1993). Bucholtz (2000) considers ethical and political considerations in transcription. An increasingly important consideration in transcription is the portability and robustness of a transcript across different computer platforms, including storage on web-accessible archives of narrative discourse that are currently under development. Here, I use the transcription system developed for web storage by DuBois, Schuetze-Coburn, Cummings, and Paolino (1993); a brief summary of the notational devices used in the transcripts presented here can be found in appendix A. The effort to compile large corpora of narrative for comparative study means that a transcript, and even the audiotape or videotape of a narrative, may be made available for study by generations of researchers of different backgrounds. This new approach raises complex ethical questions beyond those confronted when material is controlled exclusively by the individual researcher

or research team. Consent forms developed for narrative research should incorporate requests for permission to incorporate transcripts in such comparative databases.

The Rattlesnake Story: Problems in Narrative Sequence

"The Rattlesnake Story" will illustrate a problem with a fundamental point of theory: that the narrative genre is universally defined by the iconic match between a sequence of narrative clauses and a sequence of events. Such claims of universality are significant for cultural analysts, as I show below, but, like all theoretical positions, this one is contested. Labov (1972b) and Labov and Waletsky (1967) define narrative as follows: "one method of recapitulating past experience by matching a verbal sequence of clauses to the sequence of events which (it is inferred) actually occurred" (Labov 1972b:360). This match is often called the "narrative icon." A "minimal narrative" is a sequence of two temporally ordered clauses. Labov (1972b) states that these "narrative clauses" have verbs in the perfective or completive aspect; that is, the verb represents the event as "completed" in the moment represented in the clause. An example in "An immense fall" in the next section is at (2): *his rope went down.* In English (and, indeed, in other languages) narrative clauses are sometimes in the so-called historical present; an example, the locutionary verb *says*, introducing a stretch of direct quotation, is found below at (71) in the text "A dollar and a quarter an hour." The sequence of narrative or event clauses is often called the "main line" of the narrative (Longacre 1976; Polanyi 1985a). The clauses of the main line, each representing a completed moment of action, contrast with durative-descriptive or stative clauses that describe some on-going condition or state, and are used in narrative for background scene-setting. An example of such a durative-descriptive clause in "An immense fall" is (30): "*He was on El Capitan.*"

While most analysts of narrative concur that a defining universal property of the genre is the iconic match of the sequence of narrative clauses to a sequence of inferred events, there are of course departures from this match in many narratives. A phenomenon familiar from literary narrative, and found also (although, in my experience, not commonly) in vernacular oral narrative, is the "flash": either "flash-back," to a time prior to that of the immediately preceding narrative clause, or a "flash forward," to a time that that is after the time of narrative clauses that follow the flash.[6] The theory that the narrative icon is the universal default condition of the genre predicts that "flashed" clauses will always be specially marked in some way,

[6] Exhaustive treatments of temporal complexity in literary narrative may be found in Genette (1980, 1988) and Ricoeur (1988).

whether by a change in voice quality, a change in pitch, a discourse particle (such as "meanwhile" or "but"), a change in tense or aspect marking on the verb, or whatever cuing has developed locally in the discourse community under study. In Labov's theory of narrative, any flash-back or flash-forward clauses that depart from main-line iconic sequentiality must be interpreted as "evaluation," a concept to be discussed below, rather than as part of the main-line representation of the event sequence. Since "evaluation" highlights points of special interactional significance, flash-backs and flash-forwards may be important cues to the cultural underpinnings of narrative.

Contemporary literary narratives may quite purposefully depart from the narrative icon without using cues that signal flashes. A famous example is Martin Amis's novel, *Time's Arrow*, where the narrative is told in reverse order. Sherzer (1987:305) gives an example of disturbed temporal sequence from a novel by Claude Simon. Do such violations occur in vernacular oral narrative? Sherzer (1987) argues that they do, and that therefore the narrative icon is not universal. If this is the case, then the significance of flash-backs and flash-forwards can be evaluated only within the local context, and we must abandon the presumption that these are always rhetorical highlights likely to clue us in to culturally and interactionally significant moments.

Sherzer gives as evidence a text in the Kuna language of Panama, "The Hot Pepper Story," which he argues violates the narrative icon. However, while English-speaking readers of "The Hot Pepper Story" agree that the translation of the story is confusing, it is possible to interpret the disruption of the narrative line within a framework of flash-forward and flash-back—that is, the narrator uses what superficially appear to be narrative event clauses as evaluations or enlargements. Sherzer has not shown that the original Kuna version of the text contains no markers that would signal that the "out of order" clauses are evaluative flashes. For this reason I would argue that flash-backs and flash-forwards maintain their importance for cultural analysis. However, the contested significance of Sherzer's example highlights the necessity for detailed attention to the local linguistic and interactional means for cuing these devices. If Sherzer can indeed show that there is no flash-back or flash-forward in the Kuna text, and that the narrative icon is not important in Kuna narrative, then we must abandon this universal. However, he has not yet done so.

Grimes (1972) identified a second type of departure from strong main-line sequentiality, a structure that he calls "overlay." In overlay narrative, the first section is a brief precis, the next expands somewhat on the precis, and the next expands on that (presumably this could continue indefinitely, although the examples cited by Grimes have only two or three overlaid sequences). This is different from flashing back and forward. To illustrate this point I give here the translation of "The Rattlesnake Story," an oral narrative told to Ofelia Zepeda in 1989 by Marie Domingo. Ms. Domingo spoke in Tohono O'odham, a Uto-Aztecan language of southern Arizona

and northern Sonora. No further analysis of this text is given, since to do so we would have to turn to the Tohono O'odham original. The temporal relationships are not clarified by anything in the O'odham text, since O'odham does not have tenses. Verbs are either perfective/completive or imperfective (durative or stative). The senses of "future" or "conditional" are produced by adding particles to perfective constructions, while particles with meanings like "yesterday" or "just then" can be used as well to mark time. However, the absence of tense marking is not the reason that the O'odham text seems peculiar; as Sherzer (1987:305) has pointed out, "cultural logic is not a result of or an isomorphic reflection of a particular tense-aspect system. Rather, discourse . . . draws on tense aspect, as it draws on other features of the grammar and the lexicon, in the creation of temporal and spatial cultural logical systems." Instead, the O'odham text is confusing to many English speakers because it exhibits "overlay" rather than a linear sequence of event clauses.

The translation of "The Rattlesnake Story" is not marked up in full transcriptional style, because of course it is not what Ms. Domingo said! Here the lines match the syntactic structure of the O'odham text, with each line being a clause, except for truncated "and" in (6) and "just" in (21,22). Tedlock (1972, 1983) has made some very interesting proposals for how to represent something of the oral quality of the original in translation, but I neglect these possibilities here, since the sole purpose of introducing this text is to illustrate problems in the analysis of temporal sequence.

(1) The Rattlesnake Story

(Ms. Domingo has just given the word *ko'oi* "rattlesnake" as a response to a picture used in eliciting regional-dialect variation in the Tohono O'odham lexicon. She continues in O'odham as follows:)

1. One almost bit me,
2. these are real bad some of them.
3. Here's right where it fell right here,
4. and tore my dress.
5. It even got all the way to my slip,
6. and,
7. I thought perhaps if my dress was short,
8. it would fall against my leg,
9. and bite me,
10. and yet I always wear a long dress,
11. because it happened to fall right here.
12. It didn't bite me.
13. It was a big rattlesnake.
14. It crawled back there under a bench there,
15. which is there in that little building,
16. from where I was trying to take some paint . . .
17. It almost bit me.

18. I just took my dress straight off
19. and threw it right out.
20. I guess it's the poison,
21. just,
22. just,
23. just greenish yellow,
24. it just ran down here,
25. where it presumably squirted out from,
26. from where it fell and struck.
27. Where it fell,
28. presumably thinking,
29. that it fell against my leg,
30. and squirted out all that poison.
31. And when I threw it out,
32. I said to my aunt,
33. "Bring over something for me to put a dress on!"

We can infer that the sequence of events must have been something like this: A large rattlesnake had crawled back under a bench in a shed. Ms. Domingo goes to the shed to get paint. The snake strikes at her, tearing her dress. The venom stains the dress. She takes the dress off, throws it away, and calls to her aunt for a clean dress. But Ms. Domingo does not relate the events in this order. Instead, she begins with a relatively less detailed, but heavily evaluated summary (which might be an "abstract" in the sense to be discussed below), and then successive enlargements, which Keith Allan (personal communication)[7] called "episodes." Lines 1–13, which Allan calls "Episode 1," relate the crucial gist of the story—a rattlesnake strikes at Ms. Domingo, but she is not hurt since she is wearing a long dress. At lines 7–10, Ms. Domingo talks about her thoughts at the time; this is the kind of detail that often occurs at the climax of the story. Yet, it turns out that it is not the climax. Instead, in lines 14–17, which Allan calls "Episode 2," Ms. Domingo tells us that the snake crawled under a bench in a shed where she went to get paint. This sequencing is confusing for the English-language reader, who exploits the narrative icon to infer that the snake crawled back under the bench *after* it struck at her. Yet, Ofelia Zepeda states that we must understand the snake as having already been under the bench. I have not been able to identify any material at lines 12–14 in the O'odham text that clearly "mark" this material as a scene-setting "aside" or as a flashback; line (14) is definitely a narrative clause with a perfective verb. Note that an English speaker might have said something like "it *had* crawled back under a bench there," with the past-perfect verb marking (14) as a "flash back." One possible basis for an inference about flashback is that this section is bracketed by two clearly evaluative clauses, the negative "*it didn't bite me*" (12) and the near-negative

[7] Keith Allan transcribed and analyzed "The Rattlesnake Story," which was collected and translated by Ofelia Zepeda.

"*it almost bit me*" (17). Under Labov's definitions of "evaluation," which will be discussed further below, such negative clauses are always interpreted as not on the main line, because they are not events. This might key an interpretation of (14) as not on the event line, in spite of its perfective verb. However, a more likely interpretation of the relationship between Episodes 1 and 2 is that this constitutes "overlay": 13–17 go over precisely the same events covered in 1–13, but focus on slightly different details, on where the snake was and what Ms. Domingo was doing there, rather than about how its strike hit Ms. Domingo's dress rather than her leg. We can see a possible case of overlay again in the next section. In (18) and (19) Ms. Domingo relates two events: She took her dress off, and threw it out. Then, later, at (20–30), Ms. Domingo tells us *why* she threw her dress away, because gruesome greenish-yellow venom was staining the dress. Again (24) has a perfective verb and looks like an event-line clause. Ms. Domingo could not have seen the stain after she had thrown the dress away, so (24) is in a sense "out-of-order" in reference to (18–19). Line 24 is heavily bracketed in obviously evaluative clauses where Ms. Domingo talks about the color of the venom and about what the snake might have thought, which may force an interpretation of (24) as a flashback. However, this is probably also an instance of overlay, enlarging on the events of (18–19). Finally, the last event in the narrative is the reported speech in (32–33), where Ms. Domingo calls to her aunt for a clean dress.

The theoretical solution to the problems raised by Sherzer and by Grimes is important. If in fact the narrative icon is not universally a default presumption, seeming departures from the narrative icon would not necessarily have any evaluative significance but might be themselves the local default from which inference begins. In such a case, rigid adherence to the narrative icon, not violation of it, would have the stronger evaluative significance. But if the narrative icon is the universal default presumption, then departures from it are evaluative, and variability across discourse communities will lie in the choice of strategies that speakers use to mark deviations, and in the specific evaluative significance of such deviations. At the present state of our knowledge I am not ready to abandon the claim that the narrative icon is a universal default structure. All of the cases that I am familiar with in the literature that are said to violate it are either in literary narrative or are in exotic languages like Kuna or O'odham, where it is quite likely that we are not hearing or seeing the subtle, locally meaningful contextualization cues that signal that departures from the narrative icon are meant to be heard as flashes.

"An Immense Fall": The Major Structures of Narrative

In this section I analyze a single narrative, "An immense fall." The purpose of the discussion of this narrative is two-fold. First, the analysis develops

further the theoretical point raised in the introduction, that interlocutors aim at the construction of "coherence," a sense of "interpretability" and relative "completeness" in a stretch of text. Second, the analysis illustrates some major narrative structures and dimensions of organization, such as pattern numbers, transitivity, and the large-scale subunits within individual narratives that have been proposed by Labov (1972b) and Labov and Waletsky (1967). Third, it shows how hypotheses about culture can be generated as the analyst attends to such structural units and, especially, to apparent deviations from what would be predicted by the general theory of narrative. Such general theory, including claims about structural universals, is often considered to be inimical to particularist cultural analysis. I hope to show, however, that in the case of narrative analysis it is precisely our knowledge of claims about universals that motivate our questions about the meanings produced in specific moments of narration in specific discourse communities. Note that in the discussion of the major principles of organization seen in "An immense fall" I, from time to time, raise minor points as I reach particular sections of the narrative where they are illustrated. I return to "An immense fall" twice: first in this discussion of the major structures of narrative, and again in a discussion below of the interactional context, of the way meaning emerges from such interactional givens as a possible conflict between the autonomy of the speaker, who desires to tell, and the autonomy of the hearer, who may have better things to do than listen.

"An immense fall" was collected and transcribed by Kuniyoshi Kataoka for his dissertation research on spatial language used by rock climbers (Kataoka 1998). Kataoka tells us that the narrator is making a presentation to a rock-climbing club on the use of climbing "aids"—friction-hold pitons, bolts, ropes, and the like. Thus "An immense fall" is an example of a "socially occurring" narrative: a discourse that would have occurred whether or not the investigator, Kataoka, was present. The narrative was triggered, as is shown below in the section on interactional context, by an audience question about the purpose of a particular piece of equipment. I defer presentation of the entire interactional sequence within which "An immense fall" occurs until later in this chapter, giving below only the section in which the narrative is "monologic," without prompting from the audience. However, the English-speaking reader will surely suspect, looking at the first couple of lines, that these presuppose an immediately preceding interaction about "aid" in climbing—that is, the use of any elements other than hands and feet to secure the climber to the rock face.

I have never done any ethnography on mountain climbers and know relatively little about them, other than what I have learned in conversations with Kataoka and through watching the odd television program on ascents of Mt. Everest. Kataoka's own research was not an ethnography of the rock-climbing subculture, but was a study in cognitive linguistics that focused on spatial language. In his dissertation Kataoka, a rock climber himself, does provide some background information for "An immense

fall"—indeed, he insists that the complex coherence features of the text depend heavily on access to insider knowledge. My own proposals below must be thought of as hypotheses that would require further testing through ethnographic research. Nonetheless, it should be clear that close attention to narrative structure and apparent deviations from it can produce a rich range of hypotheses about cultural knowledge, each one suggesting potential lines of investigation.

(2) "An immense fall"[8]

24. . . . (.8) y'know .. just—
25. . . . (.8) it's ^^not very ^secure ^stuff.
26. . . . i mean ^it's—
27. . . . (1.4) ^people ^^fall on ^aid all the time.
28. . . . (1.2) there's a ^^guy who ^makes,
29. . . . (.8) ^^makes a lot of ^stuff I ^have.
30. . . . he was on El ^^Capitan.
31. . . . on an ^^entire pitch of these ^things/
32. . . . he was ^^two moves from the ^anchor.
33. . . . (.8) and ..^^this one ^blew,
34. . . . like one he was ^standing on ^^blew.
35. . . . his ^rope went ^down,
36. . . . the next one,
37. . . . ^^that ^blew,
38. . . . ^^that ^blew,
39. . . . ^^that ^blew,
40. . . . and the ^^entire ^pitch . . . just ^^zippered ^out.
41. . . . he ^^got ^down like,
42. . . . he ^^fell,
43. . . . (.8) something like ^two and—
44. . . . ^two and & fifty feet.
45. . . . ^just,
46. . . . just an ^^immense *fall.

My methodological presumption as I proceed with the analysis of "An immense fall" is that its audience found it to be coherent and even satisfying. Kataoka, himself a rock climber, selected it for an extended analysis from among many narratives he had collected, presumably because

[8] I begin numbering at (24); lines (1–23) are discussed in the next section. Note that the individual lines of Kataoka's transcription are "intonation units" in the sense of Chafe (1994). That is, they consist of strips of talk under a single intonational contour, defined according to its terminal pitch shape as rising (/), flat (,), terminal (.), or truncated (—). Intonation units sometimes coincide with clauses, as in lines (27, 30, 33, 35, 37–40) in "An immense fall," but they do not always do so. Kataoka has used the transcription system proposed by DuBois, Schuetze-Coburn, Cummings, and Paolino (1993), which uses very standard word-processing symbols to achieve a fairly detailed representation of the narrative. It is now possible, of course, to store original sound and video recordings of narrative on the internet.

he found it elegant and interesting.[9] Linde (1993) points out that coherence—the "interpretable wholeness" of a narrative—is not a property inherent in the discourse, but must be negotiated between narrator and interlocutor, in a process that inevitably leaves some gaps and failures of understanding. I assume that the drive for coherence is strong, since people seek a baseline from which they can begin to make conversational inferences. By assuming that coherence has been achieved (unless there is strong evidence to the contrary), I go against a contemporary theoretical grain that we live in a disordered world so that discourse should be disorderly as well, or that we live in a world so permeated with malignant power that many people find it impossible to construct an authentic "voice" that can appropriately represent their experiences. This theory about "post-modernity" predicts that we should find incoherence everywhere. However, I do not know what a method with an underlying presumption of incoherence would be like—even deconstruction itself has its own baseline assumptions. Furthermore, I have found repeatedly that precisely very marginalized people, whose lives are extremely disrupted by all the ills of the postmodern period, are often able, with great rhetorical competence, to structure their experiences into simple, elegant, even poetic narratives (Strauss (1997) illustrates this capacity for at least "partial integration"). I have also been surprised at how willing interlocutors are to find meaning even in very problematic texts.

Sometimes it is possible to demonstrate that a narrative is in fact incoherent or the product of a silenced voice (see Keesing 1985; Greenspan 1992 for possible examples); such narratives are of course of great theoretical interest, but they are relatively rare. Where a speaker consistently produces narratives that are heard by others as incoherent, and where this is not accounted for by cultural, linguistic, or class differences, we should perhaps suspect that we are in the presence of a mental illness (cf. Telles Ribeiro 1994).

The presumption that speakers and audiences seek coherence drives the analyst to determine how this is being achieved: why a speaker assumed that an audience would be able to follow the twists and turns of a narrative, to cross its gaps, and to understand it as a unit in discourse that permitted only particular kinds of uptake, and how and why an audience did or did not challenge this assumption. This presumption of coherence is maintained even when the analyst herself finds elements of the narrative, or perhaps the entire narrative in its context, puzzling. Such moments of bafflement are analytical opportunities—indeed, one of the most important—and difficult—techniques of the ethnographer is to try to continue to produce them, to avoid taking things for granted, to "make strange" the discourse and events that she observes (Agar 1980). So we will try to make "An immense fall" strange, and see how we can systematically move from

[9] There is a tendency for field workers to select "memorable" narratives for analysis. This is fine, but we should be sure also to look at unmemorable or awkward narratives, in order to test our theories about narrative coherence and structure.

"strangeness" to an understanding of its coherence that perhaps resembles the kind of understanding that teller and audience achieved. This is, of course, an endless task, the famous "hermeneutic circle" of interpretation, so the analysis here of "An immense fall" will be in no sense complete, but merely illustrative of what can be accomplished.

Elements of Narrative Structure

We now turn to the major structures that can be seen in "An immense fall." In contrast to "The Rattlesnake Story," "An immense fall" provides an example of a strong obvious main line. However, "An immense fall" does present us with a problem in temporal sequencing, because it represents two simultaneous events: the climber falls, and his equipment "blows." I will return to this point below. The main line of narrative clauses in "An immense fall" occurs in lines (33–40), where we experience the horror of 125 feet of pitons slipping out of the rock cracks as the climber cannot stop his fall. The events are: (33–34) *this one blew, like one he was standing on blew*; (35) *his rope went down*; (36–37) *the next one, that blew*; (38) *that blew*; (39) *that blew*; (40) *and the entire pitch just zippered out*. We can see here the strength of the narrative icon in the extremely economical language of (37–39): *that blew, that blew, that blew*. Shared access to the presumption of the narrative icon and a shared understanding of gravity and the force of the fall permits us to interpret this as three successive events: three pitons slip one after the other, the highest first, then the next highest, then the next, as the climber plummets down the face of El Capitan and the kinetic energy of his fall exceeds the resistance from the friction of each piton against the rock.

There are other iconic dimensions to this particular main line. Note that while there are measurable pauses (their length in seconds, when longer than 0.5 seconds, is indicated in the parentheses) both earlier and later in the narrative, the three intonation units (37–39) come in rapid succession, an icon of the speed of the fall. Indeed, the narrator "speeds up" even more by changing his strategy from mentioning each piton to using the image of a "zipper," an item with teeth so close together that we don't usually attend to them separately, at (40): *and the entire pitch . . . just zippered out*. Note that our clue to this "speeding up" comes from Kataoka's careful transcription, including the precise representation of pause lengths.

A point of interest in this main-line sequence is that its core consists of three clauses: "*that blew, that blew, that blew.*" It is well known that for speakers of English three is a "pattern number" (Hymes 1981); three repetitions gives a sense of coherence and "wholeness" to the text. The total number of events represented here is five: the sequence of three repetitions is bracketed by a first event—"*the one he was standing on blew*" and a final event "*the entire pitch just zippered out.*" The pattern numbers 3 and 5 are usually paired (and contrast with pattern numbers 2 and 4).

Thus, for the English speaker, the sequence of five narrative clauses with the three short clauses at the center is satisfying; the text feels "complete" and would perhaps be less memorable had the speaker repeated "that blew" a fourth time, or added a sixth clause. Note how this pattern numbering "structures" experience: we do not know how many pitons the climber had placed, and the pattern number preference constrains the narrator so that we do not find out. Pattern numbering is often apparent in vernacular narrative, not only in the number of main-line clauses, but, in longer narratives, in the number of "scenes" or "stanzas" (Hymes 1981). Pattern numbers may also show up in choices about numbers of characters that figure in a narrative, and in a variety of other ways. Hymes (1997) has recently discovered that the two classical pairings of pattern numbers, 3–5 and 2–4, can co-occur within the same narrative system and even within the same narrative. In such a case a shift in pattern number can signal a major structural element of a narrative, such as a change in point of view of narrator, or a change in the ritual status of scenes from one that is mundane to one that has sacred significance.

An especially interesting feature of the main line of "An immense fall" is that the verbs in the event clauses are of very low transitivity: *blew, fell, blew, blew, blew, zippered out*. What is meant by "low transitivity"? Grammarians distinguish between "transitive" sentences, where verbs take direct objects ("Judy kicked the ball") and "intransitive" sentences, where the subject of the verb is the only argument ("Judy walked slowly"). None of the verbs on the main line of "An immense fall" take direct objects. Furthermore, the (implied) subjects of these verbs are inanimate climbing aids (the pitons and rope), with a low potential to have any effects on other entities in the world. This is odd, because Hopper and Thompson (1980) found in a comparative survey of many narratives in different languages that in general main-line verbs tend to exhibit high transitivity: that is, they have animate subjects such as humans or animals, and objects that are acted upon by the verb. Thus the low transitivity in the main line of this narrative is a puzzling departure from what we expect based on Hopper and Thompson's proposal about universal features of the narrative main line. Such a deviation always should be taken as an invitation to analysis. At this point we do not have enough information to explore what this deviation might imply, so we return to it below after a bit more discussion of the narrative.

While the definitional property of narrative that distinguishes it from other genres is the main-line sequence of narrative clauses, narratives usually include other components, and the main line itself can be divided into sections. I illustrate these using the framework developed by Labov (1972b) and Labov and Waletzky (1967). This model is easy to use and makes a good introduction to the problem. Woodbury (1985, 1987) and Longacre (1976) have proposed alternative models, and these are discussed briefly below. In this section, except where otherwise indicated, I follow the decisions of Kataoka (1998:258) about the narrative structure of "An immense fall."

First in the ordered sequence of narrative structures is the "Abstract." The abstract is a brief summary of what the narrative is "about." Abstracts are optional, but they are in fact very common. Where we encounter abstracts, they often include important clues to the "point" of the story: that is, the role the teller expects the story to play in some negotiation over meaning. In "An immense fall" the abstract includes intonation units (24–27): "*Y'know, just-, it's not very secure stuff. I mean it's- people fall on aid all the time.*" This signals us that the narrative will be about "falling on aid." The analytic question that should occur to us is, why is a story about "falling on aid" being told? As ordinary human beings it is easy for us to imagine that a story about "falling" would be worth listening to, and would be of special interest for rock climbers. However, I propose an additional hypothesis: that the story is a move in an active controversy in the rock-climbing community about the relative virtues of "free climbing," using only the strength and skill of the climber to maintain enough friction on the rock face to prevent a fall, versus "aid climbing," using increasingly elaborate and expensive equipment which may permit climbers to reach places that would be inaccessible to free climbers. Perhaps the narrator tells the story to align himself with the "free-climbing" cause, or, at least, to suggest that he is not a mindless advocate of "aid climbing" who is uninformed of any alternative.

The second major structural element is the "Orientation." This section introduces the "who, what, when, and where" of the narrative. In "An immense fall" this component is found at (28–32): "*There's a guy who makes a lot of stuff I have. He was on El Capitan, on an entire pitch of these things? He was two moves from the anchor.*" In "An immense fall" the orientation is all found in a single section before the beginning of the narrative main line. However, Labov (1972b) notes that it is possible to interweave elements of orientation with the main-line clauses, deferring some scene-setting information to late in the narrative.

Note that we might want to think of line (32): "*He was two moves from the anchor*" as a narrative or event clause. However, in Labov's theory of narrative (32) is not on the narrative main line, since "was" is not a perfective verb representing an event that is "complete" in the clause, but is instead a durative-stative verb about the location of the subject. Labov comments that such durative-stative clauses, which he calls "progressive," are common in orientation. This clause, in fact, appears to be a sort of bridge between the orientation, "a guy was on El Capitan on an entire pitch of these things" and the Complicating Action, which really begins when the bolt or piton on which he was standing "blew." We can imagine, however, that it could have been handled as an "event," more or less as follows: "There was a guy on El Capitan on an entire pitch of these things. He got to two moves from the anchor." Had this narrative strategy, with "got to" or "reached," been chosen, then that moment would have been on the narrative main line. However, it is not: the boundaries of the main line, where the subject of every narrative clause is a piece of equipment,

shows that the narrative is not about the climber himself, but about the equipment, which fails him. Furthermore, the narrator makes the same kind of choice, to restrict mention of the climber to the structural margin of the narrative rather than to the main-line clauses, in (33–34), where he does not say "*And he stood on one and it blew*," but "*And this one blew, like one [he was standing on] blew.*" Again, we find the climber represented in a "background" durative-stative verb, "was standing," which is further backgrounded by being embedded in a relative clause (marked with square brackets) under the main clause "one blew." Kataoka (1998) refers to this as the "suppression of agency" of the climber. It is extremely interesting to see that the "aboutness" of the narrative, its "point"—that climbing aids don't prevent falls—is not only stated in the abstract, but is reinforced by the speaker's choice of fine details of narrative strategy, like choosing to restrict mention of the human actor to structures that do not contain main-line clauses. This consideration also begins to explain the curiously low transitivity of the event-clause verbs; this is a part of the overall "suppression of agency" in this text. There are two methodological points here: First, that the analyst should always try to imagine all the other ways that the story might have been told, and to ask why particular rhetorical choices occurred as against those which did not, and second, that such imaginings and questions require attention to fine details of syntax and lexical choice. Such attention is very important; narratives often do not have abstracts, and if this were the case with "An immense fall," these rhetorical choices by the narrator would be our main piece of evidence for the "aboutness" of the story.

In this section we encounter other matters about which we need to know more. For instance, "El Capitan" is introduced without further explanation; this tells us that the narrator simply assumes that everyone in his audience will know about this legendary granite face in Yosemite National Park, the site of many important rock-climbing feats. We can hypothesize, then, that the names of some inventory of well-known rock-climbing locations will be part of the shared cultural resources of this community, and we would want to know what these are, and their particular moral meaning. Second, the narrator uses the expression "pitch," apparently a technical climbing term that he also assumes will be known to the audience. Kataoka informs us that " 'One pitch' is usually the rope length in aid climbing. If he fell right before the (next) anchor point and all the gear blew, the result is self-explanatory—he would have fallen on the base anchor, which is usually secured by a couple of bolts or pitons, or sometimes, several cams and nuts" (Kataoka 1998:268). Clearly, the idea of "pitch" and the associated knowledge of rope lengths and hardware is of ethnographic interest; Hutchins (1995) points out how cultural knowledge is to some degree preserved in artifacts.

The next component of the narrative in Labov's model is the "Complicating Action," which he states can be summarized as answering the question, "then what happened?" (Labov 1972b:370). In Kataoka's

analysis, this is at clauses (33–40): *And this one blew, like one he was standing on blew. His rope went down. The next one, that blew, that blew, that blew, and the entire pitch just zippered out.*" The next component is the "Result," answering the question "what finally happened?" (Labov 1972b:370), which Kataoka assigns to clauses (41–44): "*he got down like—he fell, something like two and—two and fifty feet.*" Here, I dispute Kataoka's analysis, and argue for assigning lines (33–39) to the "Complicating Action," and considering line (40) to be the Result. This would require that we assign lines (41–46) to the last of Labov's sequential elements, the "Coda." There are two reasons to suggest this. One is the logic of the fall: presumably the possibility is that any piton in the sequence might hold. But as each one "blows" the climber is in more desperate shape, and when "the entire pitch just zippered out" we know that the climber has now hit his well-secured belay anchor—if that fails, the fall will probably be fatal. Thus "the entire pitch just zippered out" is the Result of the aids failing to protect the climber.

A second reason to suspect that lines (41–46) should be assigned to the Coda is that these are not events on the main line, because they are not in sequence. While the verbs are perfective: "*he got down . . . he fell,*" we cannot understand this as an "event" in a sequence with "*this one blew/his rope went down/the next one blew/that blew, that blew/the entire pitch zippered out.*" The reason is that the climber must have been falling from the moment that the first piton, "*like one he was standing on,*" "*blew,*" with the "fall" continuing throughout the sequence of events represented in narrative clauses (33–40). This is the sequencing complication mentioned at the beginning of this section. Here the narrator is forced into a choice by the one-dimensional linearity of the spoken language (note that the same problem would not occur in a gestural language like American Sign Language, where two kinds of events can be represented simultaneously). Slobin and his colleagues (Slobin 1991; Aksu-Koç and von Stutterheim 1994; Berman and Slobin 1994) point out that the solution to the problem of representing simultaneous events may differ across languages. In their study, children looked at a picture book, *Frog Where Are You?* and narrated the pictured story. A single picture shows, simultaneously, bees flying out of a hive in a tree, a dog running away from the bees, and a small boy falling out of the tree. Depending on their language (subjects spoke English, German, Spanish, Turkish, or Hebrew), children differed in how they represented the boy's fall. Some chose a perfective verb, "fell," as in "An immense fall." Others chose a backgrounding nonnarrative clause, "has/had fallen." Still other children chose a durative-descriptive clause, for example, "he was falling [while the dog ran off.]" Thus to some degree choices about the representation of simultaneous events are characteristic of discourse communities rather than being constrained solely by individual preferences.

One possibility among many would be the following: represent the beginning of the fall as "an event" somewhat as follows: *He was two*

moves from the anchor. He stepped on a protection and it blew and he fell. Then the next one blew and he couldn't get a grip so he kept falling . . . Note, however, that such a choice would do two things. First, it would violate what I, as an English speaker, feel is the prototypical sense of "fall": a complete incident, from top to bottom, rather than a series of inchworm-like increments. Second, and more importantly, such a choice would move the climber into the center of the narrative. Among the various choices available to the narrator, the deferment of "he fell" until after the event clauses about the sequential failure of all the climber's protection is consistent with the larger strategy of "suppressing the agency" of the climber, restricting his own actions to the margins of the text, both in terms of its total linear structure (where he appears only in the orientation and in the coda, on my analysis) and in terms of specific syntactic choices like representing his actions in a subordinate clause "like one he was standing on" in (34).

The last ordered structural element of narrative in Labov's model is the "Coda." Labov observes that codas do not answer any particular question (such as "what happened?" or "so what?"), and suggests that for this reason they are relatively uncommon. However, Labov provides no statistical evidence for this point; my impression is that codas are very common when narratives are being used as argumentative moves, and are common in episodes of life story narratives as well. Sometimes the coda simply summarizes the narrative, but it is often strongly evaluative, suggesting a moral lesson to be learned from the events. Codas sometimes repeat material in the abstract or in the negotiating material that precedes this, sometimes using very similar or even identical language to create a strong sense of "completeness" to the narrative. An example can be found in Hill (1995), where the wording of the abstract and the wording of the coda are almost identical—yet are separated by 17 minutes of narrative! In "An immense fall," I take the Coda to include lines (41–46): "*he got down like, he fell, something like two and—, two and fifty feet. Just, just an immense fall.*" This reasserts the abstract: "*People fall on aid all the time,*" (indeed, it uses the same word, *fall*—this kind of rhyming voicing is fairly common in vernacular narrative), and again suggests the hypothesis that what is going on is a negotiation with the idea that aid climbing, as opposed to free climbing, will protect climbers from really terrible experiences.

One reason to suspect that the lines (41–46) are Coda is that they are not, strictly speaking, required by the intrinsic logic of the narrative events. Once we know that the piton that the climber was stepping on has come loose, that three more pitons have "blown" as well, and "the entire pitch just zippered out," we know through our acquaintance with the laws of gravity that the climber must have fallen. A more "reportable" sequence would have been if the climber had somehow managed to catch himself and stop the fall. Instead, what is being reported here is not so much that the climber did fall, but the horrendous magnitude of the fall, a damning indictment against being overconfident when climbing on aid.

Finally, Labov identifies an additional component of narrative, Evaluation. This is probably the major contribution of Labov's work, and is especially pertinent for the place of narrative analysis in the study of culture. "Evaluation," says Labov (1972b:370), answers the question, "So what?"—that is, why should the interlocutor bother to listen to a narrative? Thus Evaluation includes all the strategies that a narrator uses to argue for the "reportability" (Linde 1981) of the narrated events. I return to the interactional dimensions of reportability in the next section, and focus here on the structural aspects of evaluation. Unlike the Abstract, Complicating Action, Resolution, and Coda (but somewhat like Orientation), Evaluation does not appear in a bounded section, but instead appears throughout the narrative, embedded within clauses in all sections as well as appearing as clauses in its own right.

Following Labov, the easiest way to understand evaluation is to identify it as material that deviates from the default structure. Thus, as noted above, flash-backs and flash-forwards should be evaluative. Interruption of the progress of the main line should be evaluative. Negatives are evaluative, because they are by definition not "things that happened," "events," but "things that did *not* happen." Also evaluative is any material not syntactically required in a narrative clause, such as adverbs and adjectives, relative clauses, conditional clauses, and the like. Notice that this property of evaluation requires the analyst to know something about the grammar of the language under discussion. Labov's model develops an elaborate taxonomy of types of evaluation, of which the most significant element is the contrast between "internal" and "external" evaluation. In "external" evaluation the narrator interrupts the flow of the narrative and speaks "in the interactional world" (rather than "in the story world" cf. Chafe 1979), directly addressing the interlocutors. In internal evaluation, the evaluation is "embedded" in the account of events in the story world. In real life there is a continuum of relatively deeply embedded evaluative elements between extreme surface external evaluation and fully embedded internal evaluation. The evaluation in "An immense fall" occurs mainly relatively close to the surface, and the coda can be considered as a clear instance of "external evaluation."

The first "evaluative" element here may be the exact specification of where the climber was. Simply saying, "*there's a guy, he was climbing*," would have been minimally adequate. Instead, we are told he was "*on El Capitan*," a notoriously challenging climbing site, so our interest is piqued by the potential danger. Also, by mentioning the specific site, the narrator reasserts his membership in the climbing community generally, as well as his solidarity with the club members in his audience, who know the inventory of important climbs. The second evaluative element is the elaborated description of "the guy" in (28–29). The narrator could have said, "*there's a guy, he was on El Capitan.*" However, we are told that "the guy" actually *makes* climbing aids. My hypothesis is that this fact about the "guy" is introduced in order to suggest that he is really quite expert, making his fate

all the more surprising, and constituting an even more damning case against putting undue faith in "aid." In the third, evaluation element, we are told that the narrator owns "a lot of stuff" made by the guy. This may be an additional assertion of how expert the guy is; the stuff he makes is so good that the narrator, himself an expert, has purchased it. However, it also suggests that "the guy" is known to the narrator. I hypothesize that this claim of a connection to "the guy" may "license" the narrator, endowing him with authority to report the particular events. In many communities, the authority to tell a particular story may be contestable; examples of this point can be found in Sansom (1982) and Shuman (1986). Thus, this mention cues us to the need for further exploration of this issue or claims of rights to narratve among rock climbers.

The next instance of evaluation is the adjective "*entire*" in (31). The adjective is not necessary: "*He was .. on a pitch of these things*" is perfectly grammatical in English. One hypothesis is that to say that the climber was on an "entire" pitch indicates that he was relatively far from his belayer, the stationary climber who plays out the rope from a secure position below the active climber. This is, then, a hint that we will hear a story of a very long fall. The next evaluative element is the entire clause at (32) "*He was two moves from his anchor.*" Why does the orientation include such a very specific placement of the climber in space, since the story of the fall could presumably have been told with the audience simply knowing that he was somewhere on the face of El Capitan? Probably, like "entire pitch," including this mention tells us that the climber was almost at the end of his rope, very close to his goal, again hinting at the long fall ahead. "Moves" here is clearly used in a technical sense, and the notion that a "move" in rock-climbing can be enumerated invites further exploration of this concept, which presumably involves the idea of the climber shifting in a single gesture to a new placement of hands and feet, or perhaps the placement of a new climbing aid and movement up to stand on it (see the next paragraph).

The next bit of evaluation is at (34) "*like one he was standing on blew.*" This is evaluation because it is a repetition of (33), "*and this one blew,*" and also because it contains an embedded relative clause. Here again, Kataoka helps us: He states that "What the climber is supposed to do is, in brief, to set protections (gear) in or onto the rock surface, clip a rope, and step on the aiders, repeating these procedures until s/he reaches the next anchor" (Kataoka 1998:273). Presumably, then, such aids are used when the rock face itself doesn't provide adequate purchase for the climber's feet. We might hypothesize that to mention that the aid that "blew" was "one he was standing on" again negotiates the unreliability of climbing aids. It may also suggest that this failure was especially devastating since losing his footing meant that the climber was less able to stop his fall.

The next evaluative element is the heavily evaluated clause at (40) "*and the entire pitch just zippered out.*" Note that this echoes the "*entire pitch*" at (31), and even displays the identical stress and intonation: "^^*entire* ^*pitch*", with presumably the same significance as discussed for (31).

In addition to "*entire*," the element "*just*" is also evaluative, suggesting the unexpectedness and entirety of the collapse of the carefully placed string of climbing aids. Finally, the choice of the colorful metaphor "*zippered out*" in contrast to some alternative expression like "blew one after the other" may be evaluative—or it may be the canonical way to speak of such an event in the rock-climbing community.[10]

The apparent repetition at (41–42): "*he got down like—he fell*" is problematic. We are tempted to dismiss the truncated intonation unit at (41): "*he got down like—*" as merely a false start or disfluency. Kataoka suggests (1998:271) that the choice of "*got*" may be an attempt to initiate a "got passive" clause (e.g., "he got dropped") that would express the weak agency of the climber; it may be that "got down" has a specific meaning for rock climbers that is less agentive than a sentence that I might use, like "I got down off the roof." It does seem like an odd way to talk about a fall. If it is indeed a usage that would be understood as "about a fall," then the repetition may be evaluative; we cannot solve this problem here.

The very specific mention in (43–44) of "*something like two and—two and fifty feet*" is evaluative; such exactitude is not really necessary. I have argued that attention to this kind of exact accounting—in contrast to some possible alternative expression like "a long way"—is very common in narrative performance, suggesting the "responsibility" of the narrator toward the representation of events (Hill 1995). However, while narrators often seem to be punctiliously exact about times, places, names, dates, and distances, it is important to recognize that we will never know exactly what happened on El Capitan—the narrative actually constructs the "event" that will be remembered in the climbing community, transforming "reality" into the terms of the genre. Indeed, some discussion later in the interaction to which we soon turn suggests that the narrator did not, in fact, see this fall. The elision of "hundred" is also interesting. When I first saw this passage I assumed that Kataoka had mistranscribed it, but in fact the word "hundred" is missing. This elision may be standard among rock climbers; such noncanonical number expressions are interesting in that they often define specific communities, such as gamblers and stockbrokers, for whom very large numbers are a default expectation.

Other Approaches to Narrative Structure

While I have centered the above exploration of "An immense fall" on Labov's model of the structure of narrative, the reader should be aware of other approaches, such as Longacre's (1976) work on "plot" that is discussed in more detail below. Woodbury (1985, 1987) has proposed that

[10] Naomi Quinn (personal communication) has a rock-climber acquaintance who confirms that "zipper out" is a standard way of describing the kind of event described in "An immense fall."

we think of narrative structure not so much in terms of a sequence of contained elements, but as a complex interweaving of "rhetorical structure components": "any well-defined recurrent, hierarchic organization that is present in a stretch of discourse and distinct from other such organizations" (Woodbury 1987:178). For instance, the distribution of pauses in a narrative constitutes the "pause component." The distribution of discourse particles constitutes the "particle component." The repertoire of intonational patterns constitutes the intonational component. The available selection of pitch and voice-quality options may be systematic and rhetorically significant. Pattern numbers constitute another system, and the syntax of clauses yet another. Narrative sequentiality can be thought of as the "sequence component." The sort of scene-setting that we see in abstracts can be thought of as the "setting component." Quoted material and lexical choice constitute dimensions of a "voice component" (Hill 1995). Each of these systems will have its own universal constraints (exemplified by the default presumption of the narrative icon), as well as its own locally expected patterns and expectations. For instance, in the pause component, we might find that listeners can generally distinguish between short pauses, medium-length pauses, and long, "pregnant," pauses. We might expect that long pauses would occur between the end of one bounded narrative element, such as an episode, and the beginning of another, with shorter pauses appearing within episodes. Woodbury would describe this as a default interaction between the pause component and the episode component. If we encounter a deviation from this default interaction—for instance, a long, pregnant pause occurs in the middle of an episode, or no pause occurs between two major episodes, we suspect that this deviation will have evaluative significance. Working with such subtle details of narrative, it is often extremely helpful to use methods borrowed from instrumental analysis in phonetics (cf. Farnell and Graham 1998). As noted in the introduction, the use of such analytic tools presumes that the original recording will be of a high technical quality.

A model of narrative analysis that is useful in elucidating narrative structure is Polanyi's "Linguistic Discourse Model," (Polanyi 1985b; Polanyi and Martin 1990) where narratives (and other discourses) are modeled as a left-to-right parse tree. Polanyi's goal in developing the Linguistic Discourse Model was to understand how it is that speakers can maintain so-called anaphoric accessibility or topic continuity (i.e., keep track of who is doing what) over stretches of discourse, yet rapidly shift topics (or, in narrative, shift "worlds") while preserving coherence. The discourse parse trees of the Linguistic Discourse Model, constructed according to strict formal principles (I have briefly reviewed these in Hill and Zepeda 1992), turn out to yield some interesting and surprising accounts of discourse units that sometimes clarify otherwise mysterious uses of discourse particles, peculiar tense sequences, and so on. Hill and Zepeda (1992) used Polanyi's model in the analysis of a Tohono O'odham narrative, and found that it illuminates some fine details of rhetorical choice by the speaker.

"An Immense Fall": The Interactional Context

For many centuries, the western tradition of narrative analysis assumed that narratives were monologues. However, the linguistic anthropological tradition, which sees narratives and other discourses as "enacting" and "producing" culture as well as representing it, requires that we attend to the dialogic dimension of narrative, to the presence of audiences and interlocutors. While Labov's model of narrative does not focus on interaction, this dimension of narrative is implicit when he defines narrative components as answers to questions that an interlocutor might ask, like "What happened?" or "So what?" More recent work on narrative has increasingly attended more explicitly to its dialogic and interactional dimensions.[11] Linde (1981), developing Labov's model of "evaluation," has emphasized the importance of the concept of "reportability." "Reportability" can be difficult to examine when narratives are elicited in interviews through questions like, "What's the worst fight you ever had?" or "Did you ever have an experience where you thought you might not survive?" These questions presuppose reportability, since they emerged from preliminary assessments of what kinds of event sequences were both easy and exciting for particular kinds of speakers in sociolinguistic samples to talk about.[12] However, outside the interview context speakers may have to struggle for permission to tell a narrative, and work to claim high "reportability."

"Reportability" has many dimensions. One is topic. Topic-based reportability is the source of one of the most interesting clues that cultural analysts can draw from narrative: that speakers do not tend to talk about mundane, taken-for-granted elements of their lives, but instead are more likely to narrate unexpected and deviant sequences of events. This is an extremely important point, since, as every ethnographer knows, figuring out what is mundane and taken for granted is one of the most important goals of cultural analysis. Thus one part of finding culture in narrative is noticing what speakers do *not* tell stories about, and what they seem to "leave out" as they narrate. For instance, Price (1987) analyzed stories of family illnesses told by Ecuadorian women. The women hardly attended at all to elements of their caregiving that astounded Price, such as carrying heavy

[11] In many communities in the Americas, the recitation of traditional narratives is not complete without audience response, which ranges from ritualized cries of awe and encouragement by the audience to the institutionalized role of the "What-sayer" (Basso 1985) to fully dialogic performance requiring two or more narrators. Treatments of audience interaction can be found in Duranti and Brenneis (eds.) (1986). So-called co-narration occurs widely in conversational narrative; see Briggs (1996) and Ochs, Smith, and Taylor (1996) for discussion and examples. O'Barr and Conley (1996) comment that speakers in civil proceedings often find it difficult to "tell their stories" since court protocol requires that the judge withhold the customary audience feedback.

[12] In such cases speakers often become absorbed in the art of the narration and drop their guard in regard to linguistic elements such as stigmatized phonological and syntactic forms that they might otherwise "monitor" and avoid. This relatively "unmonitored" speech was required for Labov's purpose, to study sociolinguistic variation in the speech community.

children long distances for medical attention. The idea that a mother would do such a thing was apparently taken for granted, and Price is able to exploit this realization to explore unspoken background understandings of gender roles. In contrast, the Ecuadorian narrators found "reportable" the fact that doctors or nurses had been kind to them; apparently this was unexpected and exceptional.

The concept of topic-based reportability helps us approach the problem raised in the discussion above of "An immense fall": the "suppression of agentivity" of the climber, manifested in the relegation of mention of the climber to non-main-line sections of the narrative, or to subordinate clauses. Instead, the main-line sequence of "An immense fall" is developed in a series of clauses of low transitivity, that report a sequence of failures of inanimate pieces of equipment. One hypothesis that we can advance here is that the taken-for-granted, "default" scenario for rock climbers is one in which the climber is absolutely in control, exhibiting very high agentivity. Such a scenario, of a successful climb, is not reportable unless the climb was exceptionally demanding, the first on a new route, and the like. So the suppression of agency of the climber in "An immense fall" not only permits the focus on the fallibility of climbing aids, but contributes as well to asserting the "reportability" of the events: this climber was *not* in control, but was at the mercy of gravity.

A very important property of narratives that can be treated as a dimension of the management of reportability is "plot," the trajectory of suspense in narrative. That is, a sequence of events should be, not merely a deviation from the norm, but should also be treated in such a way that the climax is not immediately apparent upon mention of the first event in the sequence. Labov (1972b) treats "plot" not as a separate component of narrative, but as two structural components, "Complicating Action" and "Resolution." Longacre (1976) treats "Plot" as a separate rhetorical system, in a way that resembles Woodbury's rhetorical components, or Labov's treatment of evaluation. The usefulness of Longacre's approach is made clear by looking at the management of suspense in "An immense fall." In the orientation section, before any "Complicating Action" has begun, we encounter certain "excessive" and hence evaluative elements that I discussed above: the mention of the "*entire pitch*" of climbing aids, and the mention that the climber was "*two moves from the anchor*"—that is, almost safely done with that section of the ascent. Thus the trajectory of suspense begins in the orientation. The main device used by the narrator of "An immense fall" to develop suspense is a rhetorical technique that Kataoka (1998) calls "zooming in": First, a very wide-angle shot: the climber is "*on El Capitan*"; second, a mid-range perspective: "*He was on an entire pitch of these things*"; finally, a close-up "*he was two moves from the anchor.*" Then, the first clause of the complicating action is an extreme close-up, "*The one he was standing on blew.*" Kataoka's analysis in terms of "zooming" shows us that all these clauses are part of a single structural trajectory, even though, in Labov's terms, they are in two different major structural units. The narrator then

"zooms out": in extreme close-up, he mentions each piton: "*that blew, that blew, that blew*," and then shifts out to the mid-range perspective: "*and the entire pitch just zippered out.*" During the "zoom out" the narrator uses a second strategy, of moving from a "speed-up" of the two-word narrative clauses ("*that blew, that blew, that blew*") to the longer sentence of the climax, "*and the entire pitch just zippered out.*" Longacre (1976) points out that such shifts in sentence length are common markers of "denouement" or "peak," the moment of plot trajectory when tension is highest. After the first four "blow-outs" of the climbing aids, presumably the interlocutor can still imagine that perhaps a piton will hold and stop the fall, but "*and the entire pitch just zippered out*" climaxes and "resolves" the plot by removing that possibility. The coda that follows—since all his aids failed, the climber fell 250 feet—in Longacre's terms is still involved in the trajectory of the plot, even though it is not on the narrative main line, in that it "untangles" the tension of the narrative. Note also that these coda clauses zoom out—not as far out as "on El Capitan," but to twice the length of "the entire pitch," since the fall takes the climber down the pitch and past the belay anchor to the full length of the rope.

The interactional importance of "plot" is that the suspense trajectory invites the interlocutor to continue to pay attention. In contrast to the constant shifts of floor that characterize much conversation, narrators occupy the conversational floor for relatively long periods of time (even though interlocutors/audiences may be providing feedback or even "co-narrating" (Briggs 1996; Ochs, Smith, and Taylor 1996; Ochs and Capps 2002)). Thus one can think of narration as an infringement upon the autonomy of the interlocutor, and as a claim of a privilege by the narrator. The structure of "plot" mitigates the threat of such a claim by implying a reward: the interlocutor will find out "how it all comes out" if she continues to listen. Many subsidiary evaluative devices are used to enhance the structure of plot and invite the mutual "involvement" of narrator and interlocutor, including vivid imagery, the use of reported speech, and the like; Tannen (1989) has reviewed a number of these devices in a monograph that supplements Labov's (1972b) work on the forms of evaluation. However, Tannen's emphasis on the "involvement" of narrator and interlocutor neglects the fact that the narrator in fact makes a very strong claim of autonomy by withholding the resolution of the plot from the audience during the recital of the complicating action or "build to peak" (Longacre 1976). Indeed, as Labov points out, what he calls the "suspension of action" is a very common evaluative device. I am fond of the claim for the narrator's power made by the Polish Nobelist Wislava Szymborska (1993:36)

> The twinkling of an eye will take as long as I say,
> and will, if I wish, divide into tiny eternities,
> full of bullets stopped in mid-flight.

A third dimension of reportability involves the social relationship of narrator and interlocutor. While reportability is in part predictable according to topic, and in part predictable by the possibilities for suspense that can

be made from a sequence of events, apparent deviations from topic-based and suspense-based norms of reportability occur—or, as Quinn (personal communication) has suggested, the norms of reportability shift depending on circumstances. For instance, power differentials may determine how long narrators are able to sustain plot through techniques such as slowing down and suspending action. A powerful person can command the attention of a subordinate even though her narrative may be an unsuspenseful account of a mundane occurrence. People without power, however, may find no audience even when they have something very important to say. Interlocutors who are accomplishing intimacy, such as husbands and wives, may also give attention to narratives that rank very low on a scale of plot- and suspense-based reportability, while strangers are unlikely to find opportunities to exchange narratives outside of institutionalized contexts. These facts mean that we cannot take heavy evaluation for granted as simply an indicator of the skill and fluency of the narrator. Instead, heavy evaluation can also be seen as part of a negotiation for permission to continue the telling, and may provide the analyst with important clues about social relationships among interlocutors.

A fourth dimension of reportability is institutional. That is, institutional circumstances can override the dimensions of topic, suspense, and social relationship. For instance, Ochs and Taylor (1992, 1994) and Ochs, Smith, and Taylor (1996) show that some middle-class American families give children special narrative privileges at the family dinner table, in the interest of socializing them into the proper telling of stories. So-called show and tell periods in American elementary-school classrooms are another example, where children who are neither more powerful than, nor intimate with, their interlocutors are invited to relate very mundane experiences (cf. Michaels 1981). In the context of courtrooms, witnesses are encouraged to narrate minute details that they would not ordinarily recount (and, ironically, are often not permitted to narrate material that they themselves believe is important and germane; cf. O'Barr 1982; Mertz 1996). The implication for cultural analysis is obvious: adjustments of norms of reportability along the various dimensions may be subtle signals of shifting social relationships among interlocutors and of distinctive culturally salient contexts such as "dinner table" or "deposition." Such adjustments cannot be observed in the context of interviews, where the interviewer may grant to the subject narrative privileges that he or she would not normally enjoy. However, by the same token interviews may elicit very striking and interesting narratives from people whose voices would not be heard if collection methods were restricted to "socially occurring" discourse.

Negotiation to Tell

Given the issues reviewed above about "reportability," it seems clear that even to begin a narrative, speakers are likely to undertake some preliminary negotiations with interlocutors. In recent work (e.g., Shuman 1986;

Goodwin 1990) considerable attention has focused on the social interaction that occurs around the edges of narratives, since it is during these nonnarrative interactional sequences that speakers may acquire the "authority" to accomplish tellings. To illustrate this point, I examine the larger interactional context of "An immense fall." Recall that the narrator is a speaker at a meeting of a rock-climbing club, who presumably has been invited ahead of time to talk about aid climbing because he is acknowledged as an expert. Thus some of his "authority" may be presupposed. However, we may also imagine that to tell a story constitutes a deviation from the expected "instructional discourse" that he has been invited to deliver. Thus, he must undertake some "social work" in order to acquire the right to narrate. The immediate context that elicits and partially licenses the telling of "An immense fall" is a question from the audience. I give at (3) the interactional material that preceded and followed the narrative (in the narrow Labovian sense) that is given above at (2). Again, the transcription and some of the analysis is from Kataoka (1998). BB and CC are two different audience members. The narrator apparently has a range of pieces of equipment laid out for the audience to see.

(3) Negotiating to tell "An immense fall"

1. <BB . . . ^hey ^what's the ^^purpose of ^this.
2. ..^cuz-
3. ..^if you could just ^^pull them ^out,
4. ..i'm ^not ^^understanding this BB>.
5. ..^Oh,
6. ..^well,
7. . . . (.7)^^OK.
8. . . . (1.5) the ^way they ^^hold is just by like ^friction.
9. . . . and ^just by —
10. . . . it's ^almost—
11. ..it's a ^^combination of ^like,
12. . . . ^like ah=,
13. ..like ^^friction,
14. . . . ^^suction,
15. . . . ^^faith in ^^go=d,
16. . . . <@HxHx @>
17. . . . ^^temperature/
18. . . . (1.3) i don't know.
19. . . . (2.5) if you like . . . ^^returned your—..your friend's ^water bottle or ^something,
20. ..you know,
21. . . . <Q ^it's gonna ^^ho=ld/
22. ..oh shit,
23. i ^^forgot that ^water bottle.Q>

((24–46 in (2) above, "An immense fall"))

47. <CC . . . ^^directly on his ^belay/
48. . . . i mean,

49. .. after he [XX]CC>—
50. [like—]
51. . . . yeah,
52. ..i ^think ^^so.
53. ..i ^think it was .. either ..^like,
54. . . . a few pieces ^off his ^belay or &just,
55. ..^^onto his ^belay.
56. . . . (1.2) and at .. the ^end of his ^^rope,
57. . . . (.7) was ^all his ^gear.
58. . . . (.8) just like ^^sitting ^there.
59. . . . (1.0) <Hx ^^Sick! Hx>
60. . . . (1.2) ^very ^sick.

In the transcript above, we can see that the narrator was invited to expand further on how the protection gear actually holds. Here we expect an attempt to reassure the doubting member of the audience. Instead, however, the narrator begins with the reassuring remark, "*Friction*," but continues with increasingly absurd explanations—*suction, faith in god*, (laughing) *temperature*—and eventually confesses, "*I don't know.*" He backpedals a bit by pointing out that the aid gear would permit you to retrieve a forgotten piece of equipment; he acts out the role of an imaginary climber who says, "*Oh shit, I forgot that water bottle.*" Then the narrator exploits the logic of his trajectory of explanation—that in fact climbing aids may depend on "*faith in God*"—to begin "An immense fall," which of course is about the failure of climbing aids. Thus an explanatory discourse on "how aid gear works" turns into a cautionary tale about "how aid gear does not work." I have argued above that this tale may be part of a larger debate among climbers about free climbing versus aid climbing.

In the light of the material at (3) we can also begin to understand the disfluencies in the abstract (24–27) in (2), which I repeat here for convenience as (4):

(4)

24. . . . (.8) y'know ..just—
25. . . . (.8) it's ^^not very ^secure stuff.
26. . . . i mean ^it's—
27. . . . (1.4) people ^^fall on ^aid all the time.

Here, we can take the disfluencies to index the difficulty of the social work required from the narrator, who must move from the nonnarrative discussion of "suction, faith in God, temperature, I don't know" and the silly bit about the water bottle to a more serious stance in which the story plays an important role. I discuss disfluency in discourse in more detail in the discussion below of "A dollar and a quarter an hour."

This new material from the interactional margins of "An immense fall" shows us examples of the dialogic dimension of narrative. Not only is the

narrative partially elicited by a question, but it exhibits a "second coda," also in response to an audience question. The narrative could perfectly well have ended at the remark, "just an immense fall." However, at (47) above CC asks about a technical detail: did the climber fall all the way such that his rope ended up attached only to the belay anchor? The narrator accomplishes some brief "responsible" attention to the detail: "*Yeah, I think so, I think it was either like, few pieces off his belay or just, onto his belay*," and then produces a new coda: "*and at the end of his rope was all his gear. Just like sitting there. Sick* (uttered with an audible exhalation, represented by Hx). *Very sick*." Kataoka (1998) explains the image that is thereby produced: the climbing rope would have been threaded through eyes in the protections that "blew" and "zippered out." As the rope extends below the belay point during the fall, every piece of equipment slides down and ends up at the end of the rope against the climber. This sequence is a good example of the "cooperative" production of an element of the narrative: the attention of an involved audience member provides the narrator with authority to continue to elaborate on the tale. Note that in some cases narrators actively seek out audience members to play this role; an excellent example is reported by C. Goodwin (1979), where a speaker shifts his gaze from one interlocutor to another seeking the attention that will permit him to complete a sentence.[13] In some narrative communities the role of "encouraging audience member" is rigidly institutionalized, as in the case of the "what-sayers" who accompany storytellers among the Kalapalo of Brazil (Basso 1985) and many other indigenous communities of the Americas. However, the example above, and the results of Goodwin, suggest that we must always regard narrative as constructed in interaction, even when the evidence for this is subtle.

"A Dollar and a Quarter an Hour": Logical Gaps and Disfluencies

"A dollar and a quarter an hour" permits me to illustrate some additional problems in narrative analysis. This example shows how speakers can embed brief narratives within argumentative discourse. It also illustrates an extremely common pattern in such brief narratives: the main-line sequence of events is a sequence of reported conversational utterances.[14] The narrative includes an interesting case of an apparent "gap" in logic of coherence, so that we can see how such gaps can be important clues to the analyst. It provides a useful illustration of a case where interlocutor "uptake," in this case by me, picked up on only one dimension of the narrative and thereby

[13] See papers in Ochs, Schegloff, and Thompson 1996 for additional examples of the construction of grammatical units in interaction.

[14] There is an extensive literature on so-called reported speech. Among especially useful references are Voloshinov 1973; Goffman 1976; Larson 1978; Levinson 1988; Lucy 1993.

perhaps shaped how it "came out." Finally, the interaction in which "A dollar and a quarter an hour" is embedded permits us to look more closely at the question of fluency and disfluency in discourse.

"A dollar and a quarter an hour" was collected in February 1997, during a study of responses to covert racism in media discourse. Dan Goldstein and I designed an interview project in which subjects were asked to look through a scrapbook of clippings of examples of "Mock Spanish" (Hill 1993, 1998; Hill and Goldstein 2001) and comment on their reactions. In this case, the interviewee is a middle-aged Mexican American business woman with a very responsible position in the Spanish-language communications industry; I will call her Ms. Rosales. She is going through the scrapbook and has reached clipping number 58, an article from the *New York Times* headlined "It's not easy being José," and illustrated with a picture of two tired-looking Latino women against a city backdrop. The reference was to the use of "José" as a label meaning "undocumented worker" in the stump speech of then presidential candidate Patrick Buchanan. The interviewee does not take time to read the article; her remarks are based on her interpretation of the headline and picture. I ask her to give the number of the clipping so that the information will appear on the tape.

(5) A Dollar and a Quarter an Hour

1. Oh <LO^^this is ^sad LO>—
2. <JHH Which one is ^this/JHH>
3. . . . ^^Fifty-eight\
4. <JHH ^^Fifty-eightJHH>.
5. Yeah, it's—
6. it's 'very ^difficult <CRKyou knowCRK[15]>—
7. . . . ^living sometimes—
8. . . . you ^know,
9. we, 'we ^think that umm
10. . . . <JHH The ^José is a reference to I think,
11. ^Pat Buchanan
12. ..^talked about the JHH>—
13. ^Yes, uhhuh
14. But..a=h,
15. . . . with like for ^example,
16. with uh—
17. Pat ^Buchanan,
18. and all of ^those that didn't,

[15] The transcription CRK brackets stretches of very low-pitched "creaky voice," defined as "an auditory event whereby low-frequency vibrations are separately resolvable to the ears of trained phoneticians" (Mendoza-Denton 1999). While this has been traditionally considered to be a voice quality most often found in male speech, Mendoza-Denton (1998, 1999) has found that it is very common in the speech of Latina gang girls in California. The case of Ms. Rosales, who uses creaky voice frequently, extends this to an older Latina (Ms. Rosales is probably about 40 years old), who is emphatically not a gang girl, but a highly respectable community leader.

19. ..a=h%,
20. . . . I was um,
21. seeing a,
22. um—
23. ^I don't know if you had a 'chance to—
24. ..last night just
25. ..when I was . taking a ^rest there
26. I was .. flipping ^channels and I sh-
27. saw on channel . ^Nine a program on um,
28. ..<CRKohCRK>,
29. ^hand<CRKoutsCRK>\
30. and 'people that ^go,
31. ^^freeloaders <CRKit was ^calledCRK>
32. <JHHumhm,
33. ohJHH>.
34. O^K,
35. and uh
36. . . . they have all these 'people standing out on the ^^stree=ts
37. and things like ^that,
38. and of course,
39. .. they uh—
40. .. %uh,
41. they say even the most m-
42. the 'biggest ^^millionaires are the biggest <CRK^^freeloadersCRK>\
43. and they were showing (Hx) how they get away with our li-
44. even ^^movie stars with keeping their ^dresses that they ^perform in
45. and <CRKthings [like ^that.] CRK>
46. [<JHHmhmJHH>
47. um—
48. and um,
49. . . . and um
50. .. and going back to ^^this is 'that,
51. .. ^you know,
52. a 'lot of the ^people that ^come,
53. .. in the ^boats an=d,
54. and that are (Hx) illegal ^aliens,
55. they come here to ^^work,
56. and they ^^do the most ^^menial <CRK^^jobsCRK>\
57. <JHH mhm>
58. for ^nothing—
59. and they ^^ta=ke-
60. I mean they take ^advantage
61. even ^no=w,
62. I just–
63. .. about a week or so ago,
64. I was 'speaking to this ^lady that was earning
65. (Hx) a <RH^^dollar and a ^^quarter an ^hourRH>.
66. <JHHOh my ^goshJHH>
67. A ^dollar and a ^quarter an ^hour\
68. working from ^six AM\

69. . . . to ^ten at night\
70. and I said <Q<BRso. ^when do you have time for your ^^family?BR>Q>
71. says <Q <LOmy ^familyLO> needs the ^^money and
72. .. nobody else'll give me a ^job
73. because I don't have the papers/
74. so 'where do I goQ>/
75. <JHH I'll bet that's just,
76. she works in somebody's house or something like thatJHH>/
77. ^No,
78. .. this is a ^business,
79. and I'm not gonna say the 'name of the ^business but,
80. .. ^[this is what they ^do.]
81. [<JHH Well if you ^did] I would file a ^report,
82. [[so.. you'd better ^not]]..JHH>
83. [[No @@ ah-]]
84. <JHHjust horribleJHH>
85. and th—
86. and th—
87. and this was just a couple of ^weeks ago,
88. and then last night I was looking at that ^program on ^free<CRK loadersCRK>,
89. . . . and they have all these people out on the ^street.
90. and then they—
91. they uh—
92. they put 'em in ^homes
93. .. and they put 'em in=^shelters and everything,
94. and the shelter has ^books,
95. . . . ^thicker than this,
96. with opportunities for ^employment,
97. but do you think they'll ^take it?/
98. No.\
99. <JHHmmJHH>
100. So then they get ^upset.
101. You know 'those jobs ^^have to be ^^done,
102. the people are <CRK^looking out there for those jobs to be ^doneCRK>,
103. .. and these people come and ^do those jobs
104. <JHH ^these are the people that are gonna go,
105. .. will ^do 'em JHH>
106. They will ^do 'em,
107. you ^know,
108. and so what .. ^harm are they <CRKdoingCRK>/
109. I think the ^harm that ^^we are ^doing is—
110. is .. offer ^freeloaders,
111. ^you know,
112. more ^handouts @@@,
113. <CRKso,
114. ^you knowCRK>
115. <JHH yeah, uhhuh.JHH>

The narrative "A dollar and a quarter an hour" actually begins only at (62) above. It is embedded in, and is part of, a larger argument, that undocumented Latino immigrants are good people who work hard for their families, and that the people who exploit them, and the "freeloaders" who will not take the jobs that the undocumented immigrants fill, are bad people.

To make this argument, Ms. Rosales cites the authority of a television program on "freeloaders," and then narrates a conversation she had with an undocumented worker to further bolster the argument. While we infer that the conversation that is reported really took place, the point of the narrative is not to represent the events of that conversation as exceptionally "reportable," but to lend force to the interviewee's defense of the position that people like the lady who makes a dollar and a quarter an hour don't really hurt anybody, and in fact are morally superior to native-born "freeloaders," both rich and poor, and to the citizens who give them "handouts."

This kind of embedding of brief narratives in argumentative discourse is extremely common; indeed, Ms. Rosales produced a half-dozen more such narratives, some slightly longer, during the course of the interview. The fact that the narrative is embedded in a larger argumentative discourse makes it difficult to determine its precise boundaries. It may be that we should consider the coda to be at (85–87), "*and th-, and th-, this was just a couple of weeks ago*"—perhaps implying "Don't think that this kind of exploitation is something from the past! It's going on right now!" In this case, we should consider the material in lines (88–114) to be a return to the argumentative discourse that preceded the narrative. However, it may also be that we should consider it to be a long coda. This would explain its relative fluency compared to the material that leads up to the narrative, and would permit us to divide the interaction into a disfluent pre-narrative strip and a fluent narrative. I return to the question of fluency below.

In order to accomplish the transition between the account of the television program and the narrative, Ms. Rosales says, "*and going back to this*" (at 50), where "*this*" means the exploited undocumented workers in the newspaper clipping, number 58. We are "going back," because Ms. Rosales has been talking about the wealthy "freeloaders" discussed on the television program she saw, and is now moving from the program to the clipping in front of her with its picture of the two tired women. We can take, "*And going back to this*" to be, in part, a negotiating move intended to seek permission to change the topic, in opposition to a "preference" (Sacks 1995) for topic continuity. However, Quinn (personal communication) has pointed out that it also indexes the speaker's larger strategy, of contrasting the "freeloaders" of the television program with the hard-working immigrants in the picture that she has "kept in mind," just as she has kept the interview scrapbook open to the page with the clipping. Ms. Rosales then begins an argumentative introduction (it is not an abstract), "*You know, a lot of the people that come in the boats and, and that are illegal aliens, they come here to work, and they do the most menial jobs for*

nothing, and they take, and they take advantage." Here, the mention of people "*that come in the boats*" probably refers to the then active concern about Haitians and Cubans in the Caribbean, as well as the offloading of Chinese immigrants onto U.S. beaches by smugglers. There are two "theys" here: the people that come here to work, and those who take advantage of them. This is a confusing repetition and forces us to explore why the speaker might have done it. The solution is probably that she does not believe (as she makes clear later in the interaction) that to "do the most menial jobs for nothing," could be interpreted as "taking advantage." This abstract signals that this will be a narrative about someone who was "taken advantage of" by being made to do a menial job for nothing.

The orientation is at (62–69): "*Just about a week or so ago I was speaking to this lady who was earning a dollar and a quarter an hour. A dollar and a quarter an hour, working from six* AM *to ten at night.*" Note that the repetition "*A dollar and a quarter an hour*" (perhaps triggered by my "*Oh my gosh*") elaborated by the very precise statement of the exhaustingly long work-day, suggests that these are pivotal issues and close to the "point" of the narrative. Yet, as we see, the narrative seems to suddenly veer sharply from this issue of wages and hours in the minimal narrative sequence of two clauses that follows. I return to this point soon.

In the actual narrative clauses (70–74), Ms. Rosales reports her conversation in so-called direct discourse reported speech. The narrative verbs are both "locutionary" verbs, "said" and "says." "*And I said, 'So, when do you have time for your family?' [She] says, 'My family needs the money, and nobody else'll give me a job, because I don't have the papers? So where do I go?'* "

These two clauses show a very common feature of such narratives of reported conversations: the first locutionary verb, at (70) is a perfective, "said," while the second, "says," at (71), is in the "historical present" (Wolfson 1982). The use of the historical present is often analyzed as a "high-involvement" strategy, one that the narrator might select to draw the interlocutor fully into her point of view. Here the shift from "*said*" to "*says*" constitutes a deictic "zoom in," moving us from the interactional present to the present in the narrative itself.

In the analysis of "A dollar and a quarter an hour," the advantage of using Longacre's (1976) concept of a "plot," distributed throughout the narrative, instead of the Labovian sequential components of "complicating action" and "result," is clear. The "complication" is embedded in the orientation: the lady is working for a dollar and a quarter an hour for 16 hours a day. At the time of the interview in February 1997 the legal minimum wage was $5.25 per hour, and most workers would expect to collect overtime pay for anything beyond eight hours of work each day. The "result" is really an explanation of why such a thing might happen: The lady has no immigration documents, so she is at the mercy of an unscrupulous employer. The narrator assumes that I share with her some background knowledge: If the lady complained to the authorities about the illegal

terms of her employment, she would simply be deported. The minimum wage in the Mexican state of Sonora at that time was approximately $3.00 a day. So, even though the lady is grossly exploited by American standards, she is still earning almost six times what she could make at home, and is unlikely, therefore, to challenge her employer's unscrupulous behavior.

In her brief narrative, Ms. Rosales plays what is for her, culturally, her strongest argumentative card: undocumented immigrants are people who place their "family" above their own interests. This move creates what is for me, as an Anglo, a logical clash or gap in the dynamics of the narrative. I hear a very sudden transition between the account of the lady's wages and hours and a new theme, about "time for the family," The narrator represents herself as asking, "*So when do you have time for your family?*"—instead of the question I expected, "Why do you stick with such an exploitative job?" (As I pointed out above, she knows the answer to that question and does not have to ask it.) And, for me, there is still another logical gap: the poor woman's reported reply doesn't really answer the question (by saying something like, "Well, I see the kids just before they go to bed," or, "I really don't have time for them."). Instead, while the reported speech of the poor woman does echo the word "family," she moves directly to the point that her undocumented status precludes her getting more desirable work. Such apparent gaps—what may seem to the listener to be lapses of logic or failures in rhetorical coherence—have been shown again and again to be key sites that reward careful analysis. Interestingly, such gaps often appear around "key words," what Briggs (1988) has called "multiplex signs" that can be "defined" with a particular reference, but in fact evoke a far larger world in which that referent is a central component. Armed with this knowledge, we go looking for a key word, and immediately find it in the repeated word "family." Ethnographers of Mexican and Mexican American communities have repeatedly documented the enormous importance of the idea of "family," and, especially, the role of women within this unit. By speaking of "family," the narrator is probably not referring to a nuclear family, but to an extended system of households, connected by consanguineal, affinal, and ritual kinship, focused around a core household where the most senior members of the extended family reside. Vélez Ibáñez (1996:144) reviews studies that show that even a first-generation undocumented immigrant like the woman who worked for a dollar and a quarter an hour would almost certainly have access to such an extended-family system, perhaps distributed on both sides of the U.S.-Mexico border. Vélez Ibáñez (1996:146) shows also that such familiar networks

> function not only as a reliable defensive arrangement against the indeterminacy and uncertainty of changing circumstances but also to "penetrate" the single strands of employee and employer relations and entangle them within the multiplicity of relationships of the network . . . Especially in the informal sector, which is marked by the lack of protection, security, and above-minimum wages, the network penetration also serves as the only means of minimum insurance against sudden firings.

Against the background of Vélez Ibáñez's analysis, we can see that the narrator's question, "So when do you have time for your family?" was not irrelevant to the issue of wages and hours, but instead bore directly on these—the poor woman's work was so exploitative that it could drag her into a hole that she could never climb out of, by prohibiting her from maintaining crucial network ties beyond her present workplace. Similarly, the poor woman's answer becomes clearly relevant: "My family needs the money." Vélez Ibáñez points out that part of contemporary patriarchy is the expectation that Mexican American women are expected to do "double duty," "to be excellent caretakers at home and to work in an exemplary manner on the job, . . . to fulfill both simultaneously, and be admired and rewarded . . ." (Vélez Ibáñez 1996:139). The poor woman presumably assumed that the narrator shared this understanding, and felt that her performance of her role was being challenged. Thus she is represented as making a very strong reply, that she suffers her work precisely for the sake of her family. She even takes the rather risky step of admitting her undocumented status, since this admission provides a justification for why she must compromise one dimension of her dual role to fulfill the other. Her question, "So where do I go?" then is really about how, in her situation, she might find a job that would permit her to better balance her responsibilities in order to fulfill the ideal, and definitely not about how she might seek legal redress. Thus the main argumentative "point" of this narrative is probably to illustrate the "virtue" of undocumented immigrants, as people who will go to great lengths to care for their families; this is consistent with the subsequent argumentation or coda.

It is interesting that my interjection at (75–76), "*she works in somebody's house or something like that?*" shows my ignorance of the diversity of the informal employment sector, and also utterly neglects the issue of "family." Had I noticed the repetition of the key word and picked up on it, the narrative coda might have gone off in a different direction. Instead, the narrator has to enlighten me: "*No, this is a business . . . this is what they do.*" My subsequent uptake at (81–82), a moment of rather empty middle-class indignation (I would not know how to report illegal practices in a business), is somewhat aside from the point of the narrator's overall argument. As I claimed above, her point is only partly about the wickedness of unscrupulous employers. Instead, the main line of her argument is against the position that undocumented immigrants are a threat to American society. The woman who worked for a dollar and a quarter an hour does not so much exemplify legal issues about fair wages and hours as she exemplifies a virtuous population that will work hard and do jobs that have to be done in order to support their families. The raising of the issue of "family" asserts her virtue in a specifically Mexican American way that I missed completely until I began looking closely at the narrative for this analysis. This suggests that it is important in field method to try to avoid simply filing taped narratives away for transcription and analysis at a later date (since transcription is extremely time-consuming, the temptation to do this

is strong). Instead, field workers should make every effort to do at least a preliminary rough transcription and analysis as soon as possible after collection. This exercise is likely to reveal themes that need further exploration, but also may help the fieldworker to be a more culturally sensitive interlocutor.

Let us turn now to the noticeable disfluency of the narrator's speech, including false starts, filled pauses and hesitation forms, and frequent short pauses, that are observable in almost every line of her argumentative discourse preceding the narrative. Such disfluency has been relatively little studied by linguistic anthropologists. Some interpretations of disfluency are framed within cognitive psychology. For instance, Linde and Labov (1975:926) argue that disfluency, especially the kinds of false starts that suggest that speakers have gotten tangled up in their own syntax, is typical of speech that is very "original" in its content, where speakers are trying to manage novel topics or genres for which they do not already have thoroughly routinized skills. This is not likely to be the correct interpretation of this case. The gist of the speaker's argument, that undocumented immigrants are harmless and in fact even a positive force, is a very common, although not universal, opinion among Mexican Americans,[16] and the relatively fluent expression of this opinion that the speaker presents at (101–108) is similar to many other examples of this argument that I have heard. Furthermore, she seems to hold this view so deeply that it is almost transparent for her, as illustrated by the apparent logical clash of the two "theys" in lines (20–25): "they do the most menial jobs for nothing, and they take, and they take advantage," where we must infer that the two "theys" stand for two different kinds of people, the good undocumented workers and the evil bosses who take advantage of them. Thus, this is not an "original" argument, and it is very unlikely that the narrator is making it for the first time.

A second common interpretation of disfluencies is derived from psychoanalysis: Speakers are disfluent because they are attempting to say the unsayable, to articulate representations or opinions that are normally repressed as dangerous to the speaker's mental stability. It is possible that the narrator would prefer not to talk about poverty and desperation among Mexicans and Mexican Americans, but is forced into it by her sense of obligation to engage with the *New York Times* clipping. However, her initial reaction is relatively fluent: "*Oh, this is sad . . . Yeah, it's, it's very difficult you know, living sometimes . . .*" It is interesting that she uses the expression "sad"; this is reminiscent of Vélez Ibáñez's (1996) choice of words in his analysis of social problems in the Mexican American community, which he refers to as "the distribution of sadness."

[16] In Mexico itself undocumented immigrants are hailed publicly (for instance, on television programs) as heroes who make great sacrifices and take great risks for the good of their families. However, some Mexican Americans resent the *indocumentados* because they are felt to drive down wages and also to threaten the hard-won respectability of better-established immigrant families.

In Hill (1995) I argued that disfluency might be, not a symptom of some repressed conflict, but a sort of performance of the narrator's "responsibility," the active consciousness of "choosing" a particular way of saying something. I based this on the fact that disfluencies often occur around very narrow specifications, like exact dates, prices, and the like.

It seems most likely, however, that the disfluencies in the lead-up to "A dollar and a quarter an hour" are not primarily due to difficulties in cognitive performance triggered by a failure of routinization or by some mechanism of repression, but are instead the result of difficulties in the interactional context. Like the disfluencies in the abstract of "An immense fall," we can understand this disfluency as the surface symptom of hard interactional work. Ms. Rosales has every reason to believe that I, as a middle-aged, middle-class Anglo woman, will not share her point of view about undocumented immigrants. In conducting the interviews, I was always very cagey about my own point of view about the materials in the scrapbook, because I did not want to bias anyone's responses. Thus Ms. Rosales is free to assume that I satisfy the stereotype of an Anglo who is very hostile to undocumented immigrants. Unfortunately I provide her with additional reasons to think this. I rather clumsily interrupt her at (10–12), immediately after her line (9), in which she begins "*we, we think that um=*." Her use of "we" here suggests that she might have been about to speak in "the voice of the Mexican American community." This "we" contrasts with "they" elsewhere in the interaction, except at (109), where her "we" is clearly constructing a category of "American citizens who unwisely permit handouts," My interruption may have suggested to her that I was not interested in hearing this voice, and derails her rhetorical plans. A second piece of evidence for this analysis is that the disfluency disappears at two points in the first part of the interaction: when Ms. Rosales cites the evidence from the television program under the locutionary verb "*they say*" (at 41–45), and during the narrative itself. The follow-up material through to (115), which may be the coda to the narrative, is also relatively fluent. In these stretches the main points of the clauses are not "in the interactional world," but are embedded in a previous world, in which she speaks, not in her own present voice, but in another voice, that of Channel 9, or of her own previous self, "*a couple of weeks ago*," or in the voice of the lady who worked for a dollar and a quarter an hour. Thus she is not engaging directly with me, and correctly assesses that it would be interactionally difficult for me to argue with these non-present sources, which she is merely "animating," for their opinions.

Conclusion

In the analyses above, I have built on a general theory that assumes that certain components and dimensions of narrative structure are human universals, and constitute "default" presumptions for interlocutors. These presumptions

permit them to derive inferences from apparent violations of narrative structures. I assumed also that interlocutors cooperate to produce "coherent"—meaningful and satisfying—narratives. These assumptions imply that the best opportunities for analysis arise where the analyst finds deviations from expected narrative structures, and where it is difficult to see how coherence has been achieved—where there seem to be logical gaps, logical clashes, unexpected silences, and disturbances of the proposed universal default structures of narrative. The assumption in the kind of analysis that I have undertaken above is that these apparent problems in fact are unproblematic within the discourse community, because interlocutors are able to "fill in" the gaps and silences and resolve the clashes by drawing on cultural knowledge that is not uttered overtly because it can be simply assumed. Thus the gaps, clashes, and silences are clues to what that knowledge might be. Where narratives seem normal and reasonable to the analyst, it is her job to "make them strange," to seek for other ways that the narrative might have been told and understand why, in local cultural terms, such ways were not chosen.

It is important to understand that this basic set of assumptions is a heuristic, a methodological stance that is adopted until the evidence drives us to some new position. Certainly some gaps and clashes are due to profound confusion and uncertainty, and some silences are due to terror. Furthermore, the study of narrative, exclusively, will never exhaust the cultural productivity of a discourse community. The work of constructing cultural meaning is not evenly distributed across genres, and may often be accomplished through nonverbal means. The understandings and knowledge of certain components of discourse communities may be encountered at unexpected and rarely encountered discursive sites, and never surface in the kinds of quotidian chat that I have reviewed above (see, for instance, Scott [1990] and Abu-Lughod [1986]). Yet, narrative deserves its reputation as a conveniently accessible site of cultural production, where the ethnographer's close attention will be rewarded with a rich harvest of new problems, puzzles, and hypotheses about how meaning is made.

Appendix A: Transcription Conventions

The following conventions, based on DuBois et al. 1993, are used in the narrative transcriptions:

{carriage return}	intonation unit
–	truncated Intonation Unit
[]	speech overlap
[[ ]]	speech overlap
.	final intonation contour (falling)
,	continuing intonation contour (flat)
/	rising to high intonation contour
\	falling to low intonation contour
^	primary accent
`	secondary accent

^^	extra-strong primary accent
=	lengthening
. . . (time)	long pause
. . .	medium pause (0.2–0.5 seconds)
..	short pause
(Hx)	exhalation
%	glottal stop
@	laughter
<BR . . . BR>	breathy voice quality
<CRK . . . CRK>	creaky voice quality
<LO . . . LO>	lowered pitch voice quality
<Q . . . Q>	quotation voice quality
<RH . . . RH>	rhythmic
<JHH . . . JHH>	speaker initials
XX	undecipherable syllables

Appendix B: Sample Consent Form

This consent form is based on one designed by Susan Ervin-Tripp and her students and circulated on the e-mail list LINGUIST. In the United States, such forms must be approved by Institutional Review Boards which review research involving human subjects.

LETTER OF CONSENT

I am conducting research on the uses of Spanish in contexts dominated by English. Examples might be an English speaker saying "Adios," or a non-Hispanic business with a Spanish name, like "Nada Cantina." I am interested in how people feel about these usages. If you agree, I will tape-record this interview. If you prefer, I will not tape-record, but will simply take written notes. If you agree to the tape-recording, I would like you to look over this consent form and initial next to uses of the tape that you agree to. You can hear the tape if you wish, and if any segments of the tape, or the tape as a whole, are objectionable, they will be erased at your request. This consent letter will be stored with tapes and interview notes and will restrict all future uses of these materials. Thank you for your help.

Jane H. Hill, Department of Anthropology, University of Arizona, Tucson AZ 85721 Phone 520-621-4735 (work), 520-327-0682 (home)

PLEASE INITIAL NEXT TO ANY USE OF THE MATERIAL WHICH YOU AGREE TO. BY "NO IDENTIFYING INFORMATION" WE MEAN YOUR NAME OR INFORMATION ABOUT YOUR WORK OR RESIDENCE THAT MIGHT PERMIT SOMEONE OTHER THAN THE RESEARCHER TO IDENTIFY YOU.

To prepare a written transcript which will be stored in the archives of the research project and will be used for analysis X_____

To make a copy of the tape for use in analysis of the transcript, with the restriction that only the edited transcript with no identifying information will be used with it X_____

To use segments of the edited transcript for research talks, research publications, and teaching, with no identifying information X_____

To put the edited transcript, with no identifying information, into a computer archive of transcripts for the use of other researchers X_____

To play parts of the tape in research talks at scientific meetings, with no identifying information X_____

To play parts of the tapes in teaching, with an edited transcript and no identifying information X_____

To give a copy of the tape to an archive for other researchers to use along with the edited transcripts with no identifying information X_____

I understand the project that has been explained to me, and give permission to make an audiotape and transcript. I understand that the tape will be used only in ways that I have approved by initialing above.

Name: _____ Date:

References

Abu-Lughod, Lila. 1986. *Veiled sentiments: Honor and poetry in a Bedouin society*. Berkele, CA: University of California Press.

Agar, Michael H. 1980. *The professional stranger*. New York: Academic Press.

Aksu-Koç, Ayhan A. and Christiane von Stutterheim. 1994. Temporal relations in narrative: simultaneity. In Berman and Slobin, *Relating events in narrative: A crosslinguistic developmental study*. Hillsdale, N.J.: Lawrence Earlbaum. pp. 393–456.

Basso, Ellen B. 1985. *A Musical view of the universe*. Philadelphia: University of Pennsylvania Press.

Berman, Ruth A. and Dan Isaac Slobin. 1994. *Relating events in narrative*. Hillsdale, NJ: Lawrence Earlbaum Associates.

Bernard, H. Russell and Gery W. Ryan. 1998. Text analysis: qualitative and quantitative methods. In *Handbook of method in cultural anthropology*, ed. H. Russell Bernard, pp. 595–646. Walnut Creek, CA: Altamira.

Briggs, Charles. 1988. *Competence in performance*. Philadelphia, PA: University of Pennsylvania Press.

Briggs, Charles. 1996. Introduction. In *Disorderly discourse: Narrative, conflict, and inequality*, ed. Charles Briggs, pp. 3–40. New York: Oxford University Press.

Briggs, Charles, ed. 1996. *Disorderly discourse: Narrative, conflict, and inequality*. New York: Oxford University Press.

Bucholtz, Mary. 2000. The politics of transcription. *Journal of Pragmatics* 32:1439–1465.

Capps, Lisa and Elinor Ochs. 1995. *Constructing panic: The discourse of agoraphobia*. Cambridge: Cambridge University Press.

Chafe, Wallace. 1979. The flow of thought and the flow of language. In *Discourse and Syntax*, ed. Talmy Givon. New York: Academic Press, pp. 159–181.

———. 1994. *Discourse, consciousness, and time*. Chicago: University of Chicago Press.

DeWalt, Kathleen M., Billie R. DeWalt, and Coral B. Wayland. 1998. Participant observation. In *Handbook of method in cultural anthropology*, ed. H. Russell Bernard, pp. 259–300. Walnut Creek, CA: Altamira.

DuBois, John W., Stephan Schuetze-Coburn, Susanna Cumming, and Dane Paolino. 1993. Outline of discourse transcription. In *Talking data*, ed. Jane A. Edwards and Martin D. Lampert, pp. 45–90. Hillsdale, NJ: Lawrence Earlbaum Associates.

Duranti, Alessandro and Donald Brenneis, eds. 1986. The audience as co-author. *Text* 6(3):239–347.

Edwards, Jane A. and Martin D. Lampert, eds. 1993. *Talking data*. Hillsdale, NJ: Lawrence Earlbaum Associates.

Farnell, Brenda. 1995. *Do you see what I mean*? Austin, TX: University of Texas Press.

Farnell, Brenda and Laura R. Graham. 1998. Discourse-centered methods. In *Handbook of method in cultural anthropology*, ed. H. Russell Bernard, pp. 411–458. Walnut Creek, CA: Altamira.

Genette, Gerard. 1980. *Narrative discourse*. Ithaca, NY: Cornell University Press.

———. 1988. *Narrative discourse revisited*. Ithaca, NY: Cornell University Press.

Goffman, Erving. 1976. *Frame analysis*. New York: Harper and Row.

Goodwin, Charles. 1979. The interactive construction of a sentence in natural conversation. In *Everyday language: Studies in ethnomethodology*, ed. George Psathas, pp. 97–121, New York: Irvington Publishers.

———. 1993. Recording human interaction in natural settings. *Pragmatics* 3(2): 181–209.

Goodwin, Marjorie H. 1990. *He-said-she-said*. Bloomington, IN: Indiana University Press.

Greenspan, Henry. 1992. Lives as texts: symptoms as modes of recounting in the life histories of Holocaust survivors. In *Storied lives*, ed. George C. Rosenwald and Richard L. Ochberg, pp. 145–164. New Haven, CN: Yale University Press.

Grimes, Joseph. 1972. Outlines and overlays. *Language* 48:513–524.

Gumperz, John. 1982. *Discourse strategies*. Cambridge: Cambridge University Press.

Hanks, William F. 1989. Text and textuality. *Annual Review* 18:95–127.

Harding, Susan. 1992. The afterlife of stories: Genesis of a man of god. In *Storied lives*, ed. George C. Rosenwald and Richard L. Ochberg, pp. 60–75. New Haven, CN: Yale University Press.

Hill, Jane H. 1990a. Weeping as a metasignal in a Mexicano woman's narrative. In *Native Latin American cultures through their discourse*, ed. Ellen Basso, pp. 29–50. Bloomington, IN: Indiana University Folklore Institute.

———. 1990b. The cultural (?) context of narrative involvement. In *Papers from the 25th Regional Meeting of the Chicago Linguistic Society*, Part 2: *Parasession on language in context*, ed. Brad Music, Randy Graczyk, and Caroline Wiltshire, pp. 138–156. Chicago: Chicago Linguistic Society.

———. 1993. Hasta la vista, baby: Anglo Spanish in the American Southwest. *Critique of Anthropology* 13:145–176.

———. 1995. The voices of Don Gabriel. In *The Dialogic Emergence of Culture*, ed. Bruce Mannheim and Dennis Tedlock, pp. 96–147. Urbana, IL: University of Illinois Press.

———. 1997. The revenge of Huitzilopochtli: A tale from Crónica X in Spanish and Nahuatl. In *The life of language*, ed. Jane H. Hill, P. J. Mistry, and Lyle Campbell, pp. 1–18. Berlin: Mouton de Gruyter.

———. 1998. Language, race, and white public space. *American Anthropologist* 100:680–689.

Hill, Jane H. 2000. Don Francisco Marquez's Story. In *Native Latin American Discourse*, ed. Joel Sherzer and Kay Sammons, pp. 1–12. Washington, DC: Smithsonian Institution Press.

Hill, Jane H. and Dan Goldstein. 2001. Mock Spanish, cultural competence, and complex inference. *Textus: English Studies in Italy* 14(2):243–262.

Hill, Jane H. and Robert E. MacLaury. 1995. The terror of Montezuma. In *Language and the cognitive construal of the world*, ed. John Taylor and Robert E. MacLaury, pp. 277–330. Berlin: Mouton de Gruyter.

Hill, Jane H. and Ofelia Zepeda. 1992. Ms. Patricio's trouble. In *Responsibility and evidence in oral discourse*, ed. Jane H. Hill and Judith T. Irvine, pp. 197–225. Cambridge: Cambridge University Press.

Hill, Kenneth C. 1985. *Las penurias de doña maría Tlalocan* 10:53–75.

Hopper, Paul and Sandra A. Thompson. 1980. Transitivity in grammar and discourse. *Language* 56:251–299.

Hymes, Dell H. 1981. *In vain I tried to tell you*. Philadelphia: University of Pennsylvania Press.

———. 1997. A Karok narrative dictated by Phoebe Maddux to J. P. Harrington. In *The Life of Language*, ed. Jane H. Hill, P. J. Mistry, and Lyle Campbell, pp. 281–298. Berlin: Mouton de Gruyter.

Hutchins, Edwin. 1995. *Cognition in the wild*. Cambridge, MA: MIT Press.

Jacobs, Melville. 1957. Titles in an oral literature. *Journal of American Folklore* 70:157–172.

Kataoka, Kuniyoshi. 1998. *The vertical experience in English and Japanese spatial discourse*. Ph.D. dissertation. University of Arizona.

Keesing, Roger. 1985. Kwaio women speak: the micropolitics of autobiography in a Solomon Island society. *American Anthropologist* 87:27–39.

Labov, William. 1972a. *Sociolinguistic patterns*. Philadelphia, PA: University of Pennsylvania Press.

———. 1972b. *Language in the inner city*. Philadelphia, PA: University of Pennsylvania Press.

Labov, William and Joshua Waletzky. 1967. Narrative analysis. In *Essays on the Verbal and Visual Arts*, ed. June Helm, pp. 12–44. Seattle, WA: University of Washington Press.

Larson, Mildred L. 1978. *The function of reported speech in discourse*. Dallas, TX: Summer Institute of Linguistics Publications in Linguistics 59.

Levinson, Stephen C. 1988. Putting linguistics on a proper footing: explorations in Goffman's concepts of participation. In *Erving Goffman*, ed. Paul Drew and Anthony Wootton, pp. 161–227. Oxford: Polity Press.

Linde, Charlotte. 1981. The organization of discourse. In *Style and variables in English*, ed. Timothy Shopen and Joseph Williams, pp. 84–114. Cambridge, MA: Winthrop Publishers, Inc.

———. 1993. *Life stories: The creation of coherence*. Oxford and New York: Oxford University Press.

Linde, Charlotte and William Labov. 1975. Spatial networks as a site for the study of language and thought. *Language* 52:924–939.

Longacre, Robert A. 1976. *An anatomy of speech notions*. Lisse: Peter de Ridder Press.

Lucy, John A., ed. 1993. *Reflexive language: reported speech and metapragmatics*. Cambridge: Cambridge University Press.

Mendoza-Denton, Norma. 1998. Low pitch in the linguistic performance of California Latina gang girls. Paper presented at the conference on *Perceiving and performing gender*, Kiel, Germany, November 13, 1998.

———. 1999. Creaky voice in narratives: Phonetics/phonology meets discourse. Manuscript, University of Arizona Department of Anthropology.

Mertz, Elizabeth. 1996. Consensus and dissent in U.S. legal opinions: Narrative structure and social voices. In *Disorderly discourse: Narrative, conflict, and inequality*, ed. Charles Briggs, pp. 135–157. New York: Oxford University Press.

Michaels, Sarah. 1981. "Sharing time": Children's narrative styles and differential access to literacy. *Language in Society* 10:423–442.

O'Barr, William M. 1982. *Linguistic evidence: Language, power, and strategy in the courtroom*. New York: Academic Press.

O'Barr, William M. and John M. Conley. 1996. Ideological dissonance in the American legal system. In *Disorderly discourse: Narrative, conflict, and inequality*, ed. Charles Briggs, pp. 114–134. New York: Oxford University Press.

Ochs, Elinor. 1979. Transcription as theory. In *Developmental pragmatics*, ed. Elinor Ochs and Bambi B. Schieffelin, pp. 43–72. New York: Academic Press.

———. 1996. Narrative. In *Discourse: A multidisciplinary introduction*, ed. Teun A. Van Dijk. London: Sage.

Ochs, Elinor and Carolyn Taylor. 1992. Family narrative as political activity. *Discourse and Society* 3:301–40.

———. 1994. Mothers' role in the everyday reconstruction of "Father knows best." In *Locating power: Proceedings of the 1992 Berkeley women and language conference*, ed. Kira Hall, pp. 447–462. Berkeley, CA: University of California Press.

Ochs, Elinor and Lisa Capps. 1996. Narrating the self. *Annual Review of Anthropology* 25:19–43.

———. 2002. *Living narrative*. Cambridge, MA: Harvard University Press.

Ochs, Elinor, Emanuel A. Schegloff, and Sandra A. Thompson, eds. 1996. *Interaction and grammar*. New York: Cambridge University Press.

Ochs, Elinor, Ruth C. Smith, and Carolyn E. Taylor. 1996. Detective stories at dinnertime: Problem solving through co-narration. In *Disorderly discourse: Narrative, conflict, and inequality*, ed. Charles Briggs, pp. 95–113. New York: Oxford University Press.

Polanyi, Lydia. 1985a. *Telling the American story*. Norwood, NJ: Ablex Press.

———. 1985b. A theory of discourse structure and discourse coherence. In *Papers from the general session at the 21s regional meeting of the Chicago linguistic society*, ed. W. H. Eilfort, Paul D. Kroeber, and K. L. Peterson, pp. 30–22. Chicago, IL: Chicago Linguistic Society.

Polanyi, Lydia and Laura Martin. 1990. On the formal treatment of discourse particles: The case of Mochó *la* and *-a*. Paper presented at the Annual Meeting of the Linguistic Society of America, New Orleans, LA, January, 1990.

Price, Laurie. 1987. Ecuadorian illness stories: Cultural knowledge in natural discourse. In *Cultural models in language and thought*, ed. Dorothy Holland and Naomi Quinn, pp. 313–342. Cambridge: Cambridge University Press.

Ricoeur, Paul. 1988. *Time and narrative*. Chicago, IL: University of Chicago Press.

Sacks, Harvey. 1995. *Lectures on conversation*. Oxford: Blackwell.

Sansom, Basil. 1982. The sick who do not speak. In *Semantic anthropology*, ed. David Parkin, pp. 183–196. London: Academic Press.

Scollon, Ronald and Suzanne B. K. Scollon. 1981. *Narrative, literacy, and face in interethnic communication*. Norwood, NJ: Ablex.

Scott, James C. 1990. *Domination and the arts of resistance: Hidden transcripts*. New Haven, CN: Yale University Press.

Sherzer, Joel. 1987. A discourse-centered approach to language and culture. *American Anthropologist* 89:295–309.

Shuman, Amy. 1986. *Storytelling rights: The use of oral and written texts by urban adolescents*. Cambridge: Cambridge University Press.

Silverstein, Michael and Greg Urban. 1996. The natural history of discourse. In *Natural histories of discourse*, ed. Michael Silverstein and Greg Urban, pp. 1–17. Chicago, IL: University of Chicago Press.

Slobin, Dan Isaac. 1991. Learning to think for speaking: native language, cognition, and rhetorical style. *Pragmatics* 1:7–26.

Strauss, Claudia. 1997. Partly fragmented, partly integrated: an anthropological examination of "postmodern fragmented subjects." *Cultural Anthropology* 12:362–404.

Stromberg, Peter. 1993. *Language and self-transformation*. Cambridge: Cambridge University Press.

Szymborska, Wislava. 1993. *View with a grain of sand*. New York: Harcourt, Brace.

Tannen, Deborah. 1989. *Talking voices*. Cambridge: Cambridge University Press.

Tedlock, Dennis. 1972. *Finding the center*. New York: Dial.

———. 1983. *The spoken word and the work of interpretation*. Philadelphia, PA: University of Pennsylvania Press.

Telles Ribeiro, Branca. 1994. *Coherence in psychotic discourse*. Oxford: Oxford University Press.

Urban, Greg. 1991. *A Discourse-centered approach to culture*. Austin, TX: University of Texas Press.

———. 1996. Entextualization, replication, and power. In *Natural histories of discourse*, ed. Michael Silverstein and Greg Urban, pp. 21–44. Chicago, IL: University of Chicago Press.

Vélez Ibáñez, Carlos. 1996. *Border Visions*. Tucson, AZ: University of Arizona Press.

Voloshinov, V. I. 1973. *Marxism and the philosophy of language*. New York: Seminar Press.

Wolfson, Nessa. 1982. *CHP: The conversational historical present in American english narrative*. Dordrecht: Foris Publications.

Woodbury, Anthony C. 1985. Functions of rhetorical structure: a study of Central Alskan Yupik Eskimo discourse. *Language in Society* 14:150–193.

———. 1987. Rhetorical structure in a Central Alaskan Yupik Eskimo tale. In *Native American discourse*, ed. Joel Sherzer and Anthony C. Woodbury, pp. 176–239. Cambridge: Cambridge University Press.

Chapter Six

Analyzing Discourse for Cultural Complexity

Claudia Strauss

There is no one right way to perform a cultural analysis of interviews and other discourse. What you choose to look at depends on your research questions. My research questions have been, "What political-economic ideologies and cultural understandings are powerful for people in the United States?" and "How do people internalize public culture, especially conflicting sets of cultural messages?" I have used a mixture of methods—some already existing, others that I made up—to answer these questions.

There are at least three different ways in which ideologies and cultural understandings have power over thought and expression. This chapter considers each of these in turn. The sections devoted to each topic can be read separately if only one or two of them are of interest.

First, ideas can become powerful by being so deeply internalized that people are hardly aware they hold these beliefs and do not consider any alternatives. This is the nature of the shared, taken-for-granted assumptions that are at the core of what is meant by "culture." More recently, conceptualized as holistic mental schemas, these taken-for-granted understandings (of everything from what kinds of supernatural beings there are to assumptions about what makes a good meal) have been called cultural models (Holland and Quinn 1987, D'Andrade and Strauss 1992). Currently some theorists refer to such assumptions as "cultural imaginaries." The first section of this chapter (pp. 204–221) discusses briefly how to find these implicit assumptions, then focuses on the personal meanings of shared schemas for individuals and social subgroups, for example, class differences in the connotations and emotional significance of cultural models.

Second, public ideas can have power as articulated ideologies, or *social discourses*. Unlike cultural models, social discourses have been made explicit—extensively by ideologists and at least in fragmentary form by people who have absorbed those ideas—amid general awareness of competing belief systems. The second section of this chapter (pp. 221–232) first explains how to find traces of social discourses in people's talk, then looks at different ways in which individuals might internalize and express multiple, conflicting social discourses.

Finally, public ideas can have power if they are seen as the common opinion. The power of public opinion may lead people who hold beliefs at

variance with it to utter their views defensively or censor them in public settings. The last section of this chapter (pp. 232–238) explains how people signal (deliberately or unconsciously) what they take to be the *cultural standing* of their views, that is, the degree to which their views are socially accepted. It is important in our cultural descriptions to be mindful of the difference between views that are widely accepted ones that are more controversial. Performing a cultural standing analysis alerts us to these differences.

These topics rely on a certain view of culture, in which in addition to shared, unifying understandings there is also interesting intracultural variation in people's perspectives (section 1), people are exposed to multiple discourses (section 2), and there is variation in the acceptability of competing views (section 3) (Bourdieu 1977, Williams 1977, Strauss and Quinn 1997).

Most of the examples that follow come from in-depth, semistructured interviews I conducted in 1995 about the welfare system (i.e., the system of government cash assistance to qualifying low-income individuals and families) with 16 Rhode Islanders chosen from a larger, randomly sampled group who had participated in a telephone survey I conducted on this topic.[1] These men and women were chosen for diversity in their class (from struggling single mothers to comfortable professionals and a near-millionaire), race, and ethnicity, as well as diversity of attitudes about the welfare system (from former recipients to fierce detractors). I met with each interviewee twice (the first time was usually in a public location they chose, the second time in their home) for two lengthy (each approximately an hour and a half) interviews. The first interview focused on their attitudes about the welfare system including any experiences they had had with it, as well as related topics such as gender roles, race relations, immigration, government programs, and the state of the economy. In the second interview I asked for a life history. Interviewees were free to propose additional topics, and I followed their lead. Some additional examples below will come from in-depth interviews I conducted in 1984, 1985 and 1990 in Rhode Island (broadly on the topic of the free enterprise system), a Thanksgiving dinner group conversation about the first Iraq war, and some printed texts. Most of the methods described here can be applied to different kinds of discourse; they are not limited in their application to talk from interviews, although that method of collecting discourse is preferable for answering certain research questions, such as what are the individual meanings of cultural models.

Section 1. Cultural Models

A. Finding Shared Cultural Assumptions

What is tricky about finding shared cultural assumptions in talk is that ordinarily these deep assumptions are left unsaid. Unless the interviewer

[1] There were also four North Carolinia interviewees in that project. Those interviews will not be analyzed here.

has asked the sorts of questions that require speakers to make their assumptions explicit, the analyst will need to figure out, on the basis of what people do say, what basic assumptions they are leaving implicit. I encourage you to read the chapters in this volume by Quinn and D'Andrade, which are devoted to methods for uncovering cultural models. Here, briefly, are some possible ways to go about this task.

Keyword analysis

Cultural keywords should show up repeatedly and express important meanings. A keyword isn't just any old repeated word. Articles, prepositions, conjunctions, and so on will rarely express important meanings; a cultural keyword is likely to be a noun, verb, adjective, or adverb.[2] Furthermore, it should be a word that has some expressive importance. In a printed text such as an advertisement, it might be in a different color or font; in speech it might be given a slight stress. Whether stressed or unstressed, however, you will notice that it conveys something important for the speaker. Assemble all the mentions of your keyword. What meanings are implicit in the way the term is used? For example, I found that *work* was a keyword in my interviews with Americans about the welfare system. Here are four examples, out of many more I could have given, of the way in which *work* arose in connection with the topic of welfare (**boldface** is used to highlight part of the text for the analysis; *italics* indicate stress in the original, and all names of interviewees are pseudonyms):[3]

1. I mean, everybody's always talking about how there's no jobs out there. There are no jobs out there. Okay, fine, there are not jobs out there. But I mean to go into a McDonalds and start **working**. I don't see a problem with that. I mean, it is, it is some type of **work**. [Peter Vieira]
2. Like you'll have some poverty-stricken woman, all she knows how to do is have kids. *One* after *another*. You know, like constantly having children. And, she's never been educated into the fact that she should get off her butt and go to **work**. [Carol Russo][4]
3. I think I let, uh, with things like *wel*fare, um, to me it's . . . it's a hard thing to justify. You know in terms of, you know, the whole capitalist, you know you **work** hard and I am a Midwesterner and I was raised with this *very* strong **work** ethic. [Linda Fuller]

[2] Or perhaps an interjection. According to a recent description of the culture of West Point military academy, the word *huah*, frequently used as both adjective and interjection, "is the romantic warrior code . . . put into verbal form" (Brooks 2003, review of David Lipsky, *Absolutely American*).

[3] *Additional transcription conventions*: Three dots indicate untimed pause, uncertain transcriptions are placed in parentheses with a question mark (uncertain transcription?), inaudible words are indicated with empty parentheses ( ), italicized material in brackets is added to help interpret the text, and nonitalicized material in brackets are the interviewer's backchannel signals. Where it is not relevant to the analysis I have deleted repeated words.

[4] This quote came from a 1984 interview, the main topic of which was the free enterprise system. Russo frequently brought up the topic of welfare on her own.

4. [*CS had asked what should be done to change the welfare system.*] I think people, getting people to **work** whether it's through volunteering or I mean, doing some sort of, I think, community service. Part of what the school [*where she volunteers*] does with the children is that these kids do about 40 hours of community service every year, every kid. That's just part of going to school there. I see that this instills some, a sense of responsibility and also a reward. I think that's the good thing about **work** . . . [Linda Fuller]

I would say that *work* is a cultural keyword in discussions of the welfare system because it was used repeatedly when my interviewees discussed welfare and it seemed to be invested with strong values for them. What does *work* mean for my interviewees? The word seems to be used in two senses. In passages 1 and 2 it means paid employment. Passage 3, however, is ambiguous (a work ethic can be applied to any task, not just paid employment) and in passage 4, Fuller talks about volunteer service as work. There she seems to be using the word to mean any kind of productive effort, paid or unpaid, as long as it involves regular responsibilities.[5] These different understandings about what *work* denotes are part of the cultural model of work and can be inferred from the way the word is used.

The cultural model of work, however, goes beyond denotations to include connotations of the term.[6] These can be determined from the other words and ideas that are regularly associated with the keyword. In the above passages there is "get off her butt and go to work" (2), a close association between capitalism and "hard work" (3), between work and responsibility (4), and the term *work ethic*. Out of the various elements of passages 1–4, I focus on these associations with work because they come up repeatedly when Americans talk about work and welfare (in other contexts, work may have other connotations).[7] These close associations with work (work is linked to capitalism, responsibility, it involves principles to live by ["work ethic"], *work* collocates closely with *hard*, putting an emphasis on effort, and is opposed to sitting around) are all part of a widely shared American cultural model of work. The cultural model of

[5] Later in the same passage, Linda said that her mother considered her housework to be her job, so work does not have to be outside the domestic sphere, but it usually does mean that. Notice the way Russo says, "go to work." This implies that work requires going out of the house, which is why asking married women "if they work outside the home," which acknowledges that housework is productive labor too, has never really caught on.

[6] This, of course, is using *denotation* and *connotation* not in the logician's sense as synonymous with *extension* and *intension* but in the common usage as "the meaning or signification of a term" versus "that which is implied in a word in addition to its essential or primary meaning" (*Oxford English Dictionary*).

[7] By contrast, among my Rhode Island interviewees, a connection between being a midwesterner and having a work ethic did not come up repeatedly, so I did not include that in my American cultural model of work. However, if I found that connection in a wider sample of Americans' discourse, then I would add it.

work, as well, includes understandings about its importance. Notice the way Vieira and Russo take it as not needing any further explanation that putting people to work is the best approach, while Fuller stresses hard work as a strong value. A fuller cultural models analysis would show, I think, that work is a sacred obligation in the United States (Mead 1986, Teles 1998; see also D'Andrade, this volume) and that in the United States the working person (prototypically, a working man), rather than a stay-at-home mother, wise elder, or salvation seeker, is the model of the ideal person. (See Yamada 1997 for a comparison with Japan.)

Cultural models without keywords

One way in which cultural models analysis differs from earlier methods in cognitive anthropology is that it is not as closely tied to lexical semantics, that is, word meanings (as Quinn explains in her chapter in this volume). There are a great many cultural assumptions that are not neatly tied up in word meanings. For example, in connection with welfare I have investigated ethnopsychologies, that is, folk psychological assumptions about human behavior. (Why do people do what they do? Do people have free will?) To uncover ethnopsychologies I started with the way people explained behavior, especially problematic behavior because that is more likely to require explanation, and then asked myself, "What assumptions about human psychology does this statement reflect?" Here are the ways two interviewees explained people's decisions to go on welfare. What psychological assumptions do they reveal?

5. And maybe if they were educated in a different *manner*, they would realize that it's a good idea to get the heck off your behind and go to work. But if they're brought up in an environment—but I don't know the answer. Because I know, like, um, if they're brought up in an environment where they see everybody sitting around smoking cigarettes or whatever [deletion]. You know, sitting around and the check comes and good, we eat this week and stuff like that. And so-and-so drinks. But, what would be a better environment? [Carol Russo]
6. Many people feel if they're unskilled and they can only earn $6 or $5 an hour, whatever the minimum wage is, they're better off going on welfare. And there again, the system *encourages* people to go on welfare because . . . the minimum wage is so low that . . . by having a minimum wage what it is, that encourages people to go on welfare. [Tommy Marino]

I would say passage 5 expresses the ethnopsychological assumption that people follow the role models provided in their environment, especially ones they saw when they were growing up. It is almost as if people are completely the product of their environment, without free will. Passage 6, by contrast, expresses a different psychological understanding: a rational choice model in which individuals make decisions, but decisions that are

highly influenced by incentives (particularly, economic incentives) provided by others. Ethnopsychologies are implicit in other sorts of discourse. For example, I have found a variety of assumptions about what motivated the Columbine shooters in public discourses on that topic (Strauss n.d.).

Here are some other ways to uncover taken-for-granted assumptions. When a speaker gives evidence to support a position, they assume that this kind of evidence can be trusted. When they discuss one topic in connection with another, without any explanation for the connection, they take for granted the cultural models that explain the connection. When speakers describe an object or sequence of events, they omit details they assume they do not need to explain. When they tell a story, it always has a point: the narrative evaluation, in sociolinguists' terms. The narrative evaluation reflects cultural assumptions about what is funny, shocking, embarrassing, and so on. (See chapter by Mathews, this volume.) In general, you could take what your interviewees say and consider what else they have to assume for those statements to make sense. It helps to keep in mind some alternative ways of thinking about the topic that one might find to put your interviewees' ideas into perspective. If you are analyzing cultural models from outside your culture or subculture, your sense of alternatives will be provided by the contrast between your cultural models and your interviewees'. If you are interviewing people whose cultural models you share, awareness of cross-cultural and intracultural variation will keep you alert to possible variation in schemas.

B. Personal Meanings of Shared Assumptions

In the last section I explained how to find shared meanings, such as the shared meaning of *work* for my interviewees. These are meanings they hold regardless of their own work experiences. But each person's experiences will shape their personal interpretive framework, or, as I have called it, their *personal semantic network* (PSN).[8] Studying PSNs can reveal subcultural differences. PSN analysis also sheds light on how important a schema is for someone, given their identity and motivating concerns. (Warning: It takes lengthy examples to demonstrate PSN analysis. If this topic does not interest you, skip to p. 221.)

To figure out PSNs I begin with a simple assumption, which I will call the assumption of *contiguity*: If topic B follows topic A when a speaker is allowed to talk without interruption, then A and B are linked in that person's PSN. Therefore, the first step in tracing someone's PSN is looking at what else they talk about in connection with the topic in question.

[8] As Teun van Dijk has pointed out to me (personal communication, August 2004), the term *personal semantic network* is confusing if one believes in distinct semantic and episodic memory systems, with semantic networks in the former and personal memories in the latter. I follow psychologists who question that distinction (e.g., as summarized in Baddeley 1999:516).

Temporal contiguity or lack of contiguity (mentioning B after A, or not mentioning B after A) does not necessarily indicate the presence or absence of a strong cognitive connection, however. Maybe the speaker never had a chance to bring up B even though it came to mind. Or maybe something about the context brought B to mind this time, even though there is not a strong link between A and B in the speaker's PSN. Thus, in addition to tracing associative links, it is also helpful to see whether A and B are discussed in the same *voice*. The concept of voice is tricky. It does not mean just tone of voice. I take "voice" from Bakhtin (1981), who uses it to mean the characteristic verbal expression of a personality and point of view. For example, we recognize when someone is going into the voice of a jokester, a moral crusader, an academic, or a therapist. It is not only their tone of voice that changes, but also their vocabulary and sentence structures. While a given person will usually have certain favorite expressions that are always typical of their voice, most people switch voices, depending on the context. A voice can be delineated by keywords, phrases, metaphorical imagery, sentence structures, and emotional tone. It may have characteristic prosodic and paralinguistic qualities as well. (See Hill 1995 for further discussion of *voice*.) If someone uses the same voice to talk about A and B, this suggests that A and B are closely connected in their PSN.

Contiguity and/or use of the same voice show what cognitive representations are closely connected in someone's PSN. Sometimes you want to know everything someone associates with a keyword or concept, but usually the interesting question is how the topic in question relates to what is *important* for a speaker. How do you find that? After collecting passages surrounding a word or concept to see what other ideas are connected to it, I then look at three things: (1) *strong versus weak associations* (strongly associated ideas are mentioned repeatedly and usually without prompting from the interviewer); (2) *self-relevant versus non-self-relevant associations* (self-relevant associations are tied to the person's self-image); and (3) *associations with emotional and motivational hot spots* (ideas connected to hot spots are expressed with strong emotions, trigger powerful memories, and are often associated with nonroutine goals speakers have pursued).

Let us take as examples the personal semantic networks that surround the keyword *work* in relation to welfare for two of my interviewees, Peter Vieira and Carol Russo. If you look only at passages 1 and 2, it would appear that Vieira and Russo use *work* in the same way, but analysis of their larger PSNs shows that this shared symbol is associated with very different outlooks and concerns. Vieira is a Portuguese American whose family emigrated from the Azores when he was young. At the time I conducted the interviews, he was in his early 30s, unmarried and with no children. Russo is a third-generation Italian American. I met Carol for six interviews when she was in her mid-40s, and I returned for three more interviews shortly after she turned 50. She was married and the mother of two teenagers. Vieira and Russo have similar working-class backgrounds, with parents who did exhausting factory work. While neither was particularly

well off at the time I conducted the interviews, their futures looked different. Carol and her husband (an unskilled laborer who was unemployed the first time I met her) owned their own home but money was tight and the future did not hold much promise of change. Peter, on the other hand, was a management trainee in a discount store chain, hopeful that he would be a store manager someday soon. These similarities and differences in their life experiences—as well as other important factors I reveal later—affected the meaning of work for each of them. The differences were significant, leading them to favor quite different welfare policies.

The first step in this analysis is to find every place where a speaker uses the keyword or discusses the concept in question. Here are all of the places where Vieira talked about work and welfare.[9] To keep this collection of quotes manageable, initially I just present passages in which Vieira or Russo talk about both welfare and work. There are other passages in which they talk about work outside the context of welfare, some of which will be introduced later. Passage 7 includes Passage 1 from above.

7. I mean, everybody's always talking about how there's no jobs out there. There are no jobs out there. Okay, fine, there are not jobs out there. But I mean to go into a McDonalds and start **working**. I don't see a problem with that. I mean, it is, it is some type of **work**. You're doing something, you're making some kind of money. Grant you it might not be enough money for you to live on, and at that point, yes, your assistance would still be provided. But at least you're there. And then from there after you have this job or whatever, there should be some way of finding out, well do you want to get into the restaurant business? You know, do you want to do this?
8. I really don't understand welfare. I don't know what it's for, first of all, I never used it. Never even been unemployed, you know. I've been fortunate enough where you know, the main thing that's been instilled to me is that if you don't have **work**, basically your worth actually as a person would, you would think less of yourself, I think. I've got a job. I've always had a job. I cannot see myself being unemployed, just lounging around. You go out of your head that way.
9. I mean you have people out there just making kids, just to get more money, and you hear about that all the time. And I, all right, I grant you, I can understand like a teenager, let's say a teenager gets pregnant. She needs assistance. Fine. If the family's not there, fine. Let's give them the assistance. If the family—if she's living at home and she wants to go back to school and she does have a child at home and someone is taking care of the child, a parent or something like that, I can see giving them a minimum amount of money, just to help her out, so she doesn't have to go out and **work** full time and not go to school and not finish the school. You know, go to school, have a part-time job, at least show that you are in the **work-**, out there in the **workforce**, doing something to earn some money. Because you got to understand that, you know, it does teach you lessons throughout your life. If you have a job, you know, as to what you

[9] One issue is how much of the surrounding passage to include. I have included the lines preceding and following the keyword that are on the same topic.

don't want to end up with. And I grant you, all right, let's say she's 16 years old. She's at home with her parents, she does have a child. She is collecting a little bit of money. But she should be out also, working at McDonald's, let's say, three nights a week. Just so she knows, if she doesn't finish off her education and everything else—this is all you'll end up with, you know.

10. But after that point, I mean she's got to find something to do, right? She wants to be with her child, fine. The childcare program that she puts the child into, to see if there's any **work** available there. She could, not only, take care of her own child, but maybe assist in taking care of some other children. And that would be a way for her to find out whether or not she wants to get into the childcare, you know, business. I mean, you never know.
11. Other than that, it goes back to where, let's say he's **working** at McDonald's for a year and I mean, he's not really making that much in money, I think somewhere along the line, someone should step in and say, "Well look here we want you to continue **working** here, but we also want you to take these night courses here to give you the skills you need to get a better job." I mean, at that point, at least the guy will be **working**. He's learning a trade, and somewhere along the line he's going to benefit himself. But not only himself, his child and his wife, or the mother of his child, however he decides to see, how he wants to do it. But I mean, I think that's a big problem where teenage pregnancies and I mean these teenagers think they can go out and make it on their own. And they're going to provide for their children and all this and then this is where they fall flat.
12. Everything should be geared to stepping them up, you know, into the right direction. Let's say there's a mother and a child in low-income housing. Well what is this mother doing, you know, to improve herself? I mean, just **working** and providing for her child is a job in itself, but no, where does she see herself, let's say five years down the road? Does she still want to be in the low-income housing, getting assistance? You know, I mean, that that just makes them want to be more dependent on what they're getting. I think something should be done . . . if you're going to move into low-income housing, I think it should be done, whereas, you know you've got a contract with the government. You know, five-year term. There should be some type of term there. Five-year term. This is what you've got to do within the first year. The first year, I think should be like a grace period. The second year, they should be made to do something. Get the skills that they need, you know. Go to school, do something. But to show some type of improvement before those five years are up. Once those five years are up, I would think she would be already set to move on and move forth. I mean, the American Dream is to own your own home. You know, and be self-sufficient, not self-sufficient, but to have a job and know that you're secure there in your house. What's the government doing about that, you know?
13. [*CS asked if immigrants should get welfare*] See, I'm an immigrant. I came, well I came over to the country when I was six years old. And I mean the most important thing that was instilled to me by my parents was to have a job. I mean, that's the first thing that they do. I mean, if

> they're from the old country, and they get here. The first thing they're going to do, see, it might be a mistake, then again it might not, but they do have a tendency of pulling their kids out of school and putting them into the **workforce** before they finish school. Is it right? Is it wrong? I don't know. I mean, I'm not going to answer that part of it, 'cause I really don't know if it's right or wrong. Personally I think it would be wrong, but they instilled values of the old country that your **work** is what makes who you are. And I believe that, I really do. I mean without a job . . . I can't imagine being without a job, first of all. But I do know people who are out of jobs and you can tell that their self-esteem is very low about themselves when they're, when they don't have something. And that's the one thing that's always instilled upon me, from my background, is that, you know, your job is what really, what makes you who you are.

All associations

Without making any initial judgments about what is important to Peter Vieira, let us look at what keywords, phrases, and topics he mentions when he is talking about welfare and work, taking the points he makes in order. We see that work is "doing something" (7, 9), earning money (7, 9), finding out what you want to do in the future, teaching lessons about what you might not want to do in the future, and imparting skills that will be useful in the future (7, 9, 10, 11). Work has to do with your "worth as a person" or what you think your worth is, your self-esteem, and makes you "who you are" (8, 13). He can't imagine not working (8, 13), and work keeps you sane (8). People sometimes have kids just to get welfare (9). Work can interfere with going to school (9, 13), and having a job was stressed by his immigrant parents, more than an education (13). Every job is valuable, even one at McDonalds, but some jobs have better earnings and future potential than others. To help low-wage workers the government should provide income supplements and any education or training they need to move into a better job eventually ("five years down the road", 12; see also 7, 9, 11).

Strong associations

The points that are repeated most often are that work is important for finding out what you want to do in the future, teaching lessons about what you might not want to do in the future, and imparting skills that will be useful in the future (7, 9, 10, 11), and that people should get the income assistance and any education or training they need to get a better job eventually (7, 9, 11, and 12). None of these points was stated or implied in my questions or comments. In other words, for Peter Vieira work is strongly associated with economic mobility, making a better life for oneself and one's family.

This is very suggestive. But mere frequency of mentions is not an infallible indicator of what is important to interviewees, because in semistructured interviews the particular topics discussed are not rigidly standardized. Furthermore, even if all the questions were exactly the same from one interviewee to the next, the number of times someone talked about something

could be influenced by the interviewer's subtle forms of encouragement (acting interested at some points, not at others). So we also need to examine interviewees' self-images and emotion-laden memories to see what is important to them.

Self-image

Peter Vieira says in the passages quoted above that having a job is central to his identity. The kind of job is not important: His identity does not rest in a particular profession, but rather in holding some job (see passages 8 and 13).

Vieira's identity as a worker was also the point of many of his life stories. A person's self-image—at least, the identity they chose to emphasize at a given moment—is revealed in the stories they tell about themselves. (See also Luttrell's chapter in this volume.) Vieira happily detailed his complete work history, going back to his first job of sweeping the parking lot of the fast food store near his house. The point of this was how important work has been to him, as it was to his parents. He said that as soon as he quit school and started working full time, his curfew was lifted and he was treated as an adult.[10] He also recounted a recent disastrous experience in which he had lost money on a tenement he had bought. This story made the point that he could accept setbacks and move on without getting angry. His self-image, in other words, is of someone who does not let his emotions interfere with his goals.

I was particularly struck by Vieira's discussion of why he chose to leave his previous job, where he was a successful department manager:

> 14. The only reason I'm leaving really is because I can see where I'd be in about five years, probably be in another [*similar department*] getting that one up to where it should be. And it's not really where I want to go. I mean, I want to go up, you know. I don't want to go lateral.

Here Vieira says, "I can see where I'd be in about five years." In passage 12 he says, "Let's say there's a mother and a child in low-income housing. Well what is this mother doing, you know, to improve herself? I mean, just working and providing for her child is a job in itself, but no, where does she see herself, let's say five years down the road?" This phrasing, along with the emphasis on economic mobility, is characteristic of Vieira's *voice* and suggests that he thinks about welfare using the same schemas he applies to his own life goals. Just as he is future oriented, not letting things bother him in order to get on with pursuing his goals and choosing his jobs "to go up," so he assumes that the best welfare policies are ones that help recipients "move on and move forth."

[10] According to another Portuguese immigrant I interviewed, and an anthropologist who has worked in that community, it is typical of Portuguese immigrants in Rhode Island and Southeastern Massachusetts to pursue a course of economic mobility by having every member of the household over 16 working full time and contributing their earnings to the family (Jim Ito-Adler, personal communication).

Emotional and motivational hot spots

Here I look for discussion of affect-laden memories (especially, although not exclusively, childhood memories), other points at which the interviewee expressed strong feelings, topics they brought up repeatedly throughout the interviews, and how they invest their time and energy now. A simple question to consider is, "Does the person have positive or negative memories in association with the word or topic?"

Peter Vieira's stories about his childhood were not entirely positive. He remembers shame that his parents could not or would not spend money so he could have the same clothes as "all-American Bobby." He tiptoes around the sensitive issue of being pressured to leave school to take a job (13 and elsewhere). But he also remembers with pride that his parents trusted him to keep the savings from his jobs, unlike his older brother, who was not trusted because he was not good at saving money. He showed no overt or covert resentment over his father "borrowing" those savings to remodel the kitchen. Instead, he figured he owed him something for making the hard decision to leave the Azores:

> 15. And every day I think about my dad, who just decided, you know, pack up and go to the new world and you know, I give him a lot of credit for that. 'Cause he just wanted to set up a better life for his kids, which he did, you know.

Vieira has replicated his parents' habits, disciplining himself to avoid unnecessary spending in order to save money for a house. He saves a remarkable $300/month from his modest paycheck. In just three years, he and his fiancée had built a nest egg of $17,000.

Summary and implications

Overall, it appears that work is a key element of Vieira's PSN. Work is good, for Vieira: it is central to your identity, keeps you sane, and leads to a better life. It is central to his self-image (he thinks of himself as a worker), and is necessary for the economic mobility that is so important a goal for him. Occasionally, Vieira expressed resentment toward welfare recipients, but the dominant emotion of his remarks was bewilderment that anyone would not want to work ("I really don't understand welfare," 8) and a pragmatic, problem-solving attitude. He assumes that most people share his values: "I mean, a lot of people do want to better educate themselves. So they can improve, move on, and so on and so forth." His approach to welfare reform is to institute programs that give others the help they need to realize their ambitions. Thus, he favors policies that would have the government assisting welfare recipients to get more education and training, or supplementing their income in entry-level jobs so that they could get the work experience they need to choose a career path. As he puts it, "Everything should be geared to stepping them up, you know, into the right direction" (12).

The PSN that surrounds work and welfare is very different for Carol Russo. Here are all the passages in which she talked about work and welfare.

16. [*CS had asked, "What are the biggest problems in America today?" CR was talking about how the people who run Rhode Island "step on you." She adds that America is still the best country around, but . . .*] It's just that these . . . people just want a—I think that's the biggest problem: People just want a free ride. And we've got a lot of people with free rides. But they're not educated. Like you'll have some poverty-stricken woman, all she know's how to do is have kids. *One* after *another*. You know, like constantly having children. And, she's never been educated into the fact that she should get off her butt and go to **work**. So she brings up like five or six kids the same attitude and they do the same thing again. Then you've got six, five more people, six people, right? ( ) Good example is someone mentioned to me about this young girl about 24 years old who had her fourth baby. All very young, like one, two . . . And the fourth baby was in the hospital on special care unit, because it was under, uh, underweight. And she was going to take this baby home to her home, which was unheated. Her husband wasn't **working**. She wasn't of course **working**. And they were going to take her to this—this young baby—to this, um, unheated house. They let her take the baby home! They didn't tell her, "Look, you're going to have to have your tubes tied." Which I feel is mandatory.
17. [*Recounts discussion she had with a professor who said people with low intelligence do not reproduce*] He said, "That's probably how it'll end." But I don't think so, because there's a lot of people that they don't even know where babies come from. And they just keep having them, and having them and having. And these babies are brought up in a system where they don't realize people are supposed to **work** for a living and become educated and stuff like that, and they just come, it becomes redundant [*i.e., gets repeated*].
18. [*CS had asked how she feels things are going in the country. CR explains her mixed feelings about immigrants, especially from Southeast Asia, who get too many special breaks, but do care about education more than American children. American children put having a good time first.*] But that's why we're Americans. You know, there are a lot of people who have died and suffered for these things that we have and it's unfortunate that we have a lot of bozos, a lot of imbeciles, a lot of people that they're allowed to have children one right after another. A lady went to that Cap—, no, wait a minute, State House, and she was out there with her kid, and her kid had on raggedy jeans, and she said to the man, "You don't care about us" 'cause they wanted more money than they were allotted, 'cause they're on welfare, for the kids, and he said, "Oh yes, we do." Well, first off I don't believe the politician cares one iota at all. But secondly, I don't care about her. Why does she have all these kids? Well, I mean, I have two jobs. You know, I'm looking forward to **working** this other job, but I'm **working** out of necessity and here are these people that sit around on their butts smoking cigarettes and cigarettes cost what? $1.50 now? It's outrageous. I don't smoke, and of course I don't drink and stuff like that. I'm a very low-keyed person but it's out of necessity a lot of times. It's because, that's it. You don't go on vacation. But a good example of the kind of person I am unfortunately or fortunately, I don't know, my cousin invited us up to New Hampshire. She

and her husband live way up in New Hampshire and she said, "Why don't you come for a weekend?" and stuff, and I said, "( ) I can't because of my kids." Now here's Sarah going to be 19 and Hannah's going to be 21 and—but I can't bear the thought of having these kids out on their own like if they're driving or if they're home alone. Not that it's any big deal, we don't live in that bad of a neighborhood, but these are my priorities and then of course my four animals, the two dogs and the two cats. But other people just pick up and screw, leave the kids, they leave kids in houses alone by themselves and then we have to pick up after them. I'm sick of it. But it's not going to change. It's not going to change because these people have nothing better to do than sit on State House steps and complain and say, "I want, I want, I want." That's my opinions about that, but I could go on forever.

19. [*CS follows up on the last comment, asking who else would feel the same way*] People in my group. You know, people who are just really ***working*** *people* that really have no connections? And you don't find people that, that ***work*** that have time to *go* to these meetings and *go* and complain.
20. [*CS asked how CR would feel about system where the government would provide jobs for everyone who needed one*] If it worked.[11] You know. I'd like to say, yes, that's a good idea. I think now people who are on welfare have to go and **work** in the park and cleanup, or
    CS: Oh, have they put in that **work**fare?
    CR: Someone told me. Someone's brother is on welfare; he lost his job. And he has to **work** in the park, cleaning up. There are jobs, there are all kinds of jobs. [Um-hm] We have welfare. So those people who are on welfare could just as well get the heck out and do something. There are a lot of parks that could be cleaned, a lot of streets, a lot of bottles that are on the roadside. That sounds good.
21. [*On how immigrants are hard workers. They buy houses and fix them up.*] Why it doesn't happen to the people who live in these areas in America, to me is, because they've always had it soft. [Mmh] You know, they're still going to get that check, that monthly check. They're going to get paid by the month. People that come from other countries, like even the Cambodians and stuff. They're hard-**working** people. They'll **work** for a small amount of money, whereas people on welfare say, "Well, I'm not **working** for minimum. Heck with that, I'm not **working** for that. I'd rather stay home and collect a check." And they do. You know, they never get anywhere and then you see these other people from other countries that start buying houses and property and educate their children.
22. [*On how the government should not waste money on worthless projects*] You know, like the Golden Fleece awards. [Yeah] You know, tha-, that bothers me too. [Yeah, yeah] That so much—I hate to see kids starving. And maybe if they were educated in a different *manner*, they would realize that it's a good idea to get the heck off your behind and go to **work**. But if they're brought up in an environment—but I don't know the answer. Because I know, like, um, if they're brought up in an

[11] *Work* is polysemous. Even though this use of *worked* to mean "operate effectively" is related to the meaning of *work* as paid labor, the schemas associated with each are quite different, so "If it worked" is not highlighted for analysis here.

environment where they see everybody sitting around smoking cigarettes or whatever—I just happen to hate cigarettes, that's why. You know, sitting around and the check comes and good, we eat this week and stuff like that. And so and so drinks. But, what would be a better environment? I don't know.

23. [*CS proposed providing jobs for everyone*] I'd like to see that. I want to see these people off the streets. **Working**. I'm sure there's jobs they could be doing. I'm sure a lot of those people have talents that they've never even discovered they have. They don't have any drive or initiative, then . . . forced into a particular situation, they just give up.
24. [*CS asked whether her parents believed in the American Dream, which Russo had just said she didn't think was true*] My parents were very hard-**working** people. Their parents came here from, from Italy. And just **worked** very, very hard. Very hard-**working** people. And that's what my mother and father always did. Just **worked**. My mother always, always **worked**. As a child I always remember my mother **working** [*outside the home*]. And, you know, like you just never expected to get handouts, or . . . it wasn't talked about.
25. You know, like, these people [*elderly couple that can't afford nursing homes*] **worked** all their lives and what's happening to them? And of course, if they were on welfare, it'd be a completely different story.
26. Someone I knew who **worked** for welfare had said that one of her clients had called her up and her son wanted to know where to register. And she said, "Register for what?" And she said, "Well he's getting married and he wants to register for, uh, welfare." Just like you go to register for ( ), they registered for welfare. And she said, "This is the type of mentality, 'Oh, my mother and father lived on welfare, now I'm going to get married and better go register to get welfare.' " Some of them don't even think that they should be going to **work** and they have all negative ideas about **work** and who would like to **work** at some of those jobs, you know? I'm not going to do that. But again I think it's the environment. You know? Why should they **work** if they accepted that type of lifestyle? You know? That's it. You know, like they say, Well it's Saturday night, they're going to go out. Saturday night they're going out . . . and raise hell. And Monday morning . . . Anyway, I don't think there's one right answer but I think one of the biggest answers would be to cut off all these illegitimate children.

All associations

What Russo discussed in connection with welfare and work is that people who do not want to work have too many children, especially, illegitimate children (16, 17, 18, 26); do not take good care of their children (16, 18); and do not model or teach proper values (including the importance of education), with the result that their children repeat the same behaviors when they are grown (16, 17, 22, 26). They waste money on cigarettes and alcohol (18, 22) and they waste time in political protests (18, 19). Everyone has a talent that would be useful in some job (23). Immigrants tend to have a stronger work ethic and appreciate the value of education more than some Americans (18, 21). Her grandparents were immigrants and her parents and grandparents worked hard (24). Welfare recipients should be

forced to work (20, 23) and women who have illegitimate children should be sterilized (16, 26).

Strong associations

Russo shares some associations with Vieira: Like him, she feels that one benefit of a job, even an entry-level job, is that it enables you to learn what talents you have that would be useful in earning more money later. That point, however, came up infrequently and only after I asked her how she would feel about government-provided jobs as an alternative to welfare. The topics she brought up repeatedly, without any prompting from me, were related to childbearing and childrearing: Welfare mothers have too many children, especially, illegitimate children (16, 17, 18, 26); do not take good care of their children (16, 18); and do not teach them proper values, with the result that their children repeat the same behaviors when they are grown (16, 17, 22, 26). The best policy would be to sterilize women who have illegitimate children (16, 26).

Self-image

In the above passages Russo describes herself as someone who does not smoke, drink, "and stuff like that" (18). Especially important is that she is a responsible mother. She offered as an example of the kind of person she is her declining to go to New Hampshire for a weekend because she did not feel she could leave her daughters home alone, even though they were 19 and 21 at the time (18). This is consistent with her explicit and implicit self-descriptions throughout the interviews. When I asked her to describe herself, she replied simply, "A mother." She frequently sought common ground with me through this identity we shared: "Really, as a mother, that's what you think."

Another consistent theme throughout the interviews was that she is someone who gets "stepped on" because she does not have influence. Thus, right after she complained about welfare mothers who, unlike her, leave children at home alone (18), she described her group as "people who are just really working people that really have no connections," thereby contrasting herself as both a responsible mother and a working person without connections with irresponsible welfare mothers who have time for political protests (18 and 19). The phrase, "without connections," carries a lot of meaning in Rhode Island, where being connected to people with political power means you can get things to go your way. Many of the stories Russo told were of times she tried to accomplish something (for her children's schools or for her home business) and was defeated by the local establishment.

Throughout the interviews, it was typical that when Russo talked about her work (she was a school secretary for a while, then had a home business, and only later started a full-time job after her daughters were grown), she tied her work to being a parent: "I have to work; I'm a mother. It's very important that I work. I need the money"; "I'm working to be a mother, a good mother." For the most part she discussed work as a necessary evil,

part of the expected life of a good person (and particularly important for a woman in case her husband leaves her), not something that conveys positive benefits in itself. Above (18) she says she is looking forward to her new job, but immediately adds, "but I'm working out of necessity." The emphasis is on a job as an economic necessity, not an activity that is inherently fulfilling or that gives someone their identity, as Peter Vieira stressed. She typically used verbs and nouns of obligation in connection with work ("should," "supposed to," "necessity," 16, 17, 18, 26). Vieira sometimes did too (9), but more often spoke of a job or work as something you "have" or "got" (8, 9, 12, 13).

Emotional and motivational hot spots

In passage 24, Carol Russo mentioned that as a child she always remembered her mother working. Elsewhere she had much more to say about her mother's working, because it was associated with very unhappy memories for Russo. Her mother worked the second shift, so she was never there when Carol came home from school. Afterschool care was the responsibility of her father, who kept Carol and her sisters housebound and beat them. This formative experience made Carol determined to take the sort of jobs that ensured she would be home when her daughters came home from school:

> 27. I thought it was important to have my mother home sometimes. Because sometimes you want to talk to somebody and she wasn't there. [. . .] I thought it was very important to stay home to be a good parent or to be accessible when they were home from school if something happened and I was able to get involved.

This leads me to speculate that when Russo talks about her hard-working parents, this is associated with her sense of loss that her mother was not available to her.

Russo may also have been angry with her mother for not taking better care of her. I wondered about this because she used the same word, "indifferent," both to describe her mother and, at another point in the interview, to describe bad people:

> 28. I would say ( ) that this is a very indifferent world, a lot of indifferent people. I think that. That's what it is. The world is made up of a lot of very indifferent people climbing all over everybody else to get to the top, they don't care who they hurt.

When I showed her a draft of an earlier paper that pointed out that she used the same word to describe her mother and bad people, she was very upset and denied thinking that her mother was a bad person. So either the coincidence in terms is not meaningful or these are unconscious feelings. (For more on methods for uncovering unconscious feelings, see especially the paper by Luttrell in this volume.)

Summary and implications

Given Russo's self-image and hot spots, it is not surprising that the dominant emotional tone in her discussion of welfare recipients is outrage. Instead of admiring welfare mothers for making the decision to stay home with their children, she accepts the stereotype, very prevalent in the U.S. media, that welfare mothers are neglectful (bring underweight newborns home to unheated houses, 16; leave their children at home alone, 18). Furthermore, work is not central to her identity, and she does not see it as very fulfilling in itself, so she resents working hard and having little provided for her when she sees others achieving what she thinks are the same results with less effort. (Welfare benefits are more meager than Russo thinks, but all that matters here are her perceptions.) She favored requiring welfare recipients to work after I suggested that (my suggestion had been phrased in terms of *providing* jobs) and most of all, sterilizing welfare mothers, which would solve both the bad parenting and freeloading problems from her perspective.

What makes PSN analysis a cultural analysis, instead of a psychological analysis? As I explain above, delineating a PSN is a step toward a cultural analysis when we repeat it for enough people to see clusters of shared associations. Peter Vieira's experiences as the child of ambitious immigrants, and Carol Russo's growing up with working parents who split shifts because they could not afford other options for childcare, are not unique. For example, two clusters I found could be grouped under the headings *Work is bad* and *Work is good*. Those who held unrewarding dead-end jobs assumed that people who are not working have to be forced to work, because work is not intrinsically rewarding. Those who found work fulfilling and important to their identity usually imagined everyone else does too. Their approach to welfare reform was to provide jobs for those who were unemployed and skills for those who could not find good jobs. There was a particularly interesting cluster of three men: Peter Vieira, Vincent Rocha, and Tommy Marino. Economic mobility was very important for all three: The latter two had already achieved it and Vieira was on his way. Rocha's and Marino's success at moving from working-class childhoods to the upper-middle class as adults made them very impatient with welfare recipients, who they felt were just not trying. At the same time, however, the welfare reforms they proposed were not punitive (as in Russo's case) but involved income supplements, job training, and subsidized college education, which they saw as the best route for maximizing one's future earnings. They assumed that what had been important for them was important for most people. In sum, PSN analysis can reveal how the standpoints of differently situated social groups affect the larger meanings they associate with a symbol or concept.

Personal semantic network analysis is also useful for determining the relevance of a cultural model for a particular group of people. One mistake cultural analysts can easily make is to find several people voicing a certain opinion and conclude that this is a central cultural belief. However, if you knew the people better you might discover that it is of little importance to them, because it is not closely associated with their self-image and personal

hot spots (see also Strauss 1992). For example, Marlene Randall, a working-class woman like Carol Russo, at one point said about women on welfare:

> 29. You know what I mean—having—you can make a mistake, yeah. But when you have child, and child, and child.

That sounds very much like Carol Russo's complaints about the supposed excessive fecundity of welfare recipients, and many of my other interviewees made similar comments.[12] "Child, and child, and child" was a verbal molecule, a formulaic phrase (see section 3 below), often voiced in discussions of welfare at that time. That does not mean that this issue had the salience or meaning for other interviewees that it did for Carol Russo, however. Neither Marlene Randall nor anyone else dwelt on this topic the way Russo did, and for everyone except Russo the issue was one of economic rationality (Why have more kids if you can't afford the ones you have?) or "making kids just to get more money," as Peter Vieira put it (9), rather than bad parenting. This topic was not linked to any other interviewees' emotional hot spots around parenting issues, and forced sterilization was not a popular proposal.

Clearly, conducting a PSN analysis is time consuming. You may decide it is not worth the effort, given your research questions. The benefit of it, however, is an analysis that gives more insight into the cognitive and emotional meanings of keywords and concepts, evidence for patterns of subcultural variation, and understanding of how people's ideas on one subject are related to other things they say, think, and do.

Section 2. Social Discourses

A. How to Find Traces of Social Discourses

Cultural models, such as the cultural models of work and the ethnopsychologies discussed in the last section, can come to be shared in a variety of ways. Sometimes they have their source in explicitly formulated social discourses. For example, the idea that people make choices based on the incentives (especially, economic incentives) available to them is a cultural model that most people do not bother to explain or defend, but this schema has both contributed to and derives from rational choice theories that are quite explicit in the writings of some social scientists (e.g., Becker 1976). Writers and speakers in a particular social discourse tradition usually develop a specialized jargon and phraseology. The easiest way to recognize traces of social discourses in people's talk, even if they are not aware of their ideological[13] sources, is to look for the *ideas, jargon* and

[12] In fact, historically welfare recipients have had no more children than the average U.S. American (Cammisa 1998:16–17).

[13] *Ideology* usually denotes tendentious rhetoric that aims to legitimize or change power distributions, which would be true of some but not all social discourses. However, I will use *ideology* here in a broader sense to mean all explicitly formulated social theories, including social scientific theories.

phraseology typical of that discourse. As Linde explains in her analysis of folk psychologies (Linde 1987), sometimes sentence structure can be a tip-off as well. For example, she found that folk Freudianism was expressed in sentences in which the agent of the action was not the speaker, but some force inside the speaker. This makes social discourse analysis quite similar to *voice* analysis, as I described it in the last section (p. 209). For example, just as was the case for voice, emotional tone (revealed not just in tone of voice but also word choice and metaphorical imagery) might be clues to use of a particular social discourse. Think, for example, of the pervading gentleness of New Age discourses or the cool rationality of strategic defense discourses. (For an explanation of social discourse analysis,[14] see Fairclough's [1992] discussion of Foucaultian discourse analysis, for example, Foucault 1972; for a very similar non-Foucaultian approach, see Linde 1987, 1993 on "coherence systems.")

Social discourse analysis is by necessity *intertextual* (Kristeva 1986), that is, it is concerned with the way other texts are incorporated into the text under analysis. This requires familiarity with prior verbal and written expressions of the ideologies in question, so you can recognize when a given text incorporates established ways of expressing certain sets of ideas. This may sound hard, but if the topic is one that you have been studying, this will not be difficult. For example, anyone with a passing knowledge of the abortion debate in the United States knows that *pro-choice* is the preferred self-designation of abortion rights supporters while *pro-life* is the preferred self-designation of abortion opponents, and that the former talk about the *fetus* and the latter about the *unborn child*.

Further examples of social discourse analysis are provided as we look at how to recognize the way individuals internalize multiple social discourses, to which I turn now.

B. The Organization of Multiple Social Discourses in Talk

At a minimum, any discourse analysis should show *all* of the viewpoints that are represented in the texts under consideration instead of arbitrarily picking out one or two that are especially striking (as is all too common in cultural analysis). This is a simple matter of intellectual honesty: We should not impose greater order and coherence than exists. There are practical consequences of being alert for competing ideas and discourses as well. For example, I found that while an individualistic approach to poverty (i.e., the cause of the poverty is the poor person's failure to work hard, so the best solution is to encourage or force poor people to work harder) is *dominant* for most of the U.S. Americans I interviewed, it is not

[14] I suggest the term *social discourse analysis*. Usually this approach is just called *discourse analysis*, which makes it hard to distinguish it from other ways of analyzing discourse.

the only way they look at the issue. When antipoverty activists fixate solely on this central cultural theme, missing competing views that are widely shared as well, they miss an opportunity to build political support for policies that are helpful rather than punitive (Strauss 2000, 2002).

Beyond demonstrating the diversity of views that speakers hold, or the disparate social discourses they have appropriated, we could also ask how speakers mentally organize their conflicting ideas or the diverse social discourses they have internalized. I have found three general cognitive patterns: *compartmentalization*, *ambivalence*, and *integration* (Strauss 1990, 1997).[15] In this discussion I focus on these ways of mentally organizing competing social discourses; the same methods could be used to find ways of mentally organizing competing cultural models.

When speakers *compartmentalize* (Singer 1972, Weiss 1990) conflicting ideas, they hold them in separate, largely unconnected cognitive schemas and are usually unaware of the conflict between them. Speakers who are *ambivalent* are aware that they seem to hold inconsistent ideas and show signs of psychic conflict as a result. When speakers *integrate* multiple social discourses, they draw on them selectively, blending them into a view that is consistent and makes sense for them, even if it does not fit any standard public theories. In that case you, as an analyst, might think there is a conflict, but that is a result of the imposition of your categories, which has led you to miss the logic that ties together their ideas. The usual disclaimer applies to these categories. They are ideal types; sometimes people are on a continuum between two of these types rather than neatly within one. I should also stress that I do not believe that compartmentalization is abnormal or a problem most of the time. Indeed, interviewees who had one primary social discourse that they applied to all situations struck me as overly rigid. They scared me more than those (the majority) who drew on different discourses in different contexts.

How do we tell when people compartmentalize, when they are ambivalent, and when they integrate a variety of social discourses? First, locate the traces of different social discourses by looking for ideas, jargon, and phraseology characteristic of each. Then consider their placement in relation to each other. Placement refers to whether the different ideas are expressed in close proximity as part of a connected discussion of a single topic or in different contexts. (Contiguity, which was discussed in the last section as a criterion of links in personal semantic networks, is a special case of placement in the same context, which would also include A and B both arising in the course of a longer connected discussion of a topic, but not one right

[15] In Strauss (1990) I discuss two kinds of compartmentalization: horizontal and vertical. In Strauss (1997) I mention another possibility: *unconscious compromise*, where competing ideas are internalized in dynamically linked schemas so that acting on one creates some anxiety or need to compensate later. Peng and Nisbett (1999) suggest another possibility: dialectical thought that embraces the contradiction. I will not go into those further possibilities here.

after the other.) I suspect that a speaker is compartmentalizing their different ideas if I find passages whose ideas are at odds, using the jargon and phraseology characteristic of different discourses, expressed in separate speech contexts (e.g., in connection with different topics, at different points in a single interview or different days in a series of interviews). Ambivalence is indicated by ideas that are at odds, articulated in the characteristic language of different social discourses, but in close proximity to each other along with indication that the speaker feels a conflict ("but on the other hand . . .," "I don't know," and nonverbal expressions of frustration, such as sighs). Cognitive integration is indicated by phrasing and contents that show that although the ideas in question were drawn from disparate social discourses, they are closely linked in the speaker's personal semantic network because as the speaker expresses them the ideas fit together, they are expressed in the same context, and the speaker shows no sign of discomfort or conflict when switching from one to the other.

These indicators should be employed as guidelines only. For example, as I mention in the first section of this chapter, placement is not an entirely reliable indicator of cognitive compartmentalization. Ideas speakers express at separate points in the discussion could be cognitively linked for them, but they lacked the opportunity to segue from one idea to the other given the flow of the conversation. Conversely, sometimes people will articulate one point of view, think about the topic more and come up with another point of view that they also hold, which they will express immediately afterwards. Normally, these separate schemas would not be expressed in the same context. In the course of the interview, however, they have more time to think and are often trying to be especially helpful, so they conduct a thorough mental inventory, unshelving schemas that are not usually displayed at the same time.[16] When only some of the indicators of compartmentalization, ambivalence, or integration are present, all you can do is point that out and make your best argument on the basis of the evidence you have.

Compartmentalization

Here is a probable example of compartmentalization. The speaker, Vincent Rocha, was one of the three men I mentioned at the end of the last section. An immigrant like Peter Vieira, Rocha had worked hard and become a successful engineer. For the most part he expressed only scorn for welfare recipients. Thus, early in the first interview when I asked him why most

[16] Some readers will object to the language of "unshelving schemas," because it implies that the ideas are stored rather than constructed through talk. It seemed clear listening to people that there is a range. Some schemas were well formulated before I got there; others were developed and modified through the course of our conversation. In this section I present examples of each.

people go on welfare, he said

30. Sometimes there's no choice but I'd say 60 percent of the time that person is going in [*the welfare system*] because they don't care. Maybe 30 percent is forced into it, and the other 10 percent is miscellaneous. But I'd say—yes, there is a percentage that's forced in there—I think the majority is because . . . it's a way of life.

Yet, much later in the interview when I asked if welfare is related to women's place in society he replied

31. Ninety-five percent of the time it's the women caught in this situation. The husband takes off and she's stuck with the kids. There's no way of getting out other than financial assistance, because she cannot work due to small children—sometimes sickness—and that's the reason probably 95 percent [*of the time*].

I would guess that the ideas Rocha expresses were learned in different contexts and are internalized in a compartmentalized way because the contents are at odds (in 30 he says 60 percent of welfare recipients are in the system because they don't care, a statement typical of antiwelfare personal responsibility discourses, while in 31 he says most welfare recipients are women, 95 percent of whom were forced into the system when their husbands abandoned them, a statement typical of prevalent discourses condemning "deadbeat dads") and because these passages came at different points in the interview and in the course of discussion of different topics (welfare in general in 30, women and welfare in 31).[17]

Here is another example of ideas that I suspect are largely compartmentalized. The speaker is Mason Carter, an African American minister. Try naming the social discourse or discourses represented below.

32. God gave us a will to choose between good and evil. I'm not a drug addict, I'm not a drunk, but if I wanted to choose to do that, I can be. If I choose that road, but I have not chosen that road. And so, therefore, a lot of peoples are pointing fingers at the government, and even with the television going on, hey, you can choose not to even watch television. The government says, "Hey, I'm going to put this out here." You can choose not to smoke cigarettes, it's your choice. It's not the government's fault. It's not the government's fault because we're in poverty; it's not the government's fault that the man down the street is an alcoholic, that somebody's on welfare. A lot of people are blaming the government for something because they are being irresponsible themselves. So therefore I have chosen, I used to be an alcoholic and I got saved, I received Jesus Christ as my personal savior. I had to make a choice, whether I wanted to

[17] Interestingly, the *voice* as I define it in the first section is similar in 30 and 31. This is the voice of the dispassionate, quantitatively minded engineer. However, the social discourses expressed are different. That is why voice is not a reliable guide to social discourses.

> be an alcoholic or I wanted to be a child of God. I made that choice. I could sit around and be an alcoholic and blame the government, but hey, I made the choice.

It would take more research in intellectual history than I can undertake now to pin down the complete genealogy of these ideas, but we could say that in terms of currently salient social discourses, Carter has drawn on evangelical Christianity ("I used to be an alcoholic and **I got saved, I received Jesus Christ as my personal savior.** I had to make a choice, whether I wanted to be an alcoholic or I wanted to be a child of God. I made that choice") and what could be called the personal responsibility discourse that was prominent during the 1990s in discussions of welfare and welfare reform. ("It's not the government's fault. It's not the government's fault because we're in poverty; it's not the government's fault that the man down the street is an alcoholic, that somebody's on welfare. A lot of people are blaming the government for something because they are being **irresponsible** themselves.")

In the first interview, however, Carter had made very different statements. For example, he talked about the time he was on welfare because he had gotten sick and lost his job. He praised the welfare system for helping him then. In passing, he mentioned that sometimes people manipulate the system, and I tried to get him to follow up on that. But he quickly returned to his main theme that sometimes people are poor for reasons that are beyond their control. What discourses do you think he draws on here?

> 33. [M]ainly all the drugs that's coming in here, the *user's* the one who's going to jail, not the ones with the big money—not the big guys, are not going to jail. They're not users. The *users* are going to jail. And they got the same thing in America where they say, "You're innocent until proven guilty." Lot of time they put you in jail and charge you with a big fine and you're guilty until proven innocent in America. That's why I don't even watch television no more. Old Columbo movies—you are guilty until proven innocent in the court of law, in the state of America. Somebody accuse you of something, they don't care you got . . . and really, if the people's on welfare, it's the low people that's being oppressed. Still being oppressed. Oppression—you can just go in a poor section of a neighborhood and you can feel the oppression there because of the government. It comes from the head first. That's where it comes from—it comes from the courts, high courts. And until the people come together to start protesting against certain things, there's not going to be no changes . . . without confrontation, there's not going to be any changes. We have to protest, we have to come together as a community, and protest against what the government is doing.

The content of 33 is very different from 32. In 32 Carter says it is not the government's fault if people are poor; in 33 he blames the government. In 32 the solution he favors is for individuals to exercise free will to make different choices; in 33 the solution he favors is for communities to come

together to protest inequities in the criminal justice system. The placement of 32 and 33 also suggest they are compartmentalized, for they were expressed in two different interviews held a week apart. The voice Carter uses is very different in 32 and 33, and even his dialect shifts, becoming much more marked by typical features of African American Vernacular English in 33 than in 32. Aside from the dialect shift, he also draws on black power social discourse ("it's the low people that's being oppressed. Still being oppressed. Oppression—you can just go in a poor section of a neighborhood and you can feel the oppression there because of the government," "we have to come together as a community, and protest against what the government is doing") and populist social discourse, which in America always sets up a contrast between the *big* (guy, government, business) and the *little* (guy, man, person) ("the ones with the big money . . . the big guys") in 33. Unlike 32, with its stern yet hopeful tone, the tone of 33 is cynical and angry.

Because passages 32 and 33 came at different points in the interviews and express conflicting ideas using the jargon and phraseology typical of different social discourses, my guess is that Carter learned these ideas in different circumstances and these schemas are cognitively compartmentalized. This does not mean that Carter is unaware of the conflict between them. When he voiced 32 Carter may have been thinking about some points he made in the first interview, including his comments in passage 33, and decided that that was not the message he wanted to leave with me. Nor are these two sets of ideas irreconcilable: Carter may feel that poverty has both systemic and individual causes. However, it seems clear that in some contexts he emphasizes the ideas of passage 32 and in others the ideas of passage 33, and that normally these are not joined because they do not mesh very easily.

Ambivalence

In the following passage Carol Russo expresses ambivalence because she favors policies that limit income support to poor families, but she hates to see children starving. The context was discussion of childcare for working mothers. Russo remembered a daycare center that her daughter attended for a short time that took low-income children. The program was available only in the mornings, but they always fed the children lunch. When Russo questioned why it was so, she was told that that might be the only meal the children had all day. Since I am making a line-by-line analysis of this passage, I number each line.

34. 1 She said, "This is probably all they'll eat, anyway."
    2 And that really, oh, that hurts bad.
    3 You know, uh—I don't want—
    4 but then, this is how they're brought up.
    5 So, if you're brought up this way, you just think this is right. [Mmh]
    6 You know no other way.
    7 You think that's in the movies, where you see everybody sitting around a beautiful table, all loaded with food.

8 It's movies.
9 You know, 'cause I see things in the movies and I say, "Well, that's just Hollywood."
10 I don't accept it.
11 And, maybe this is what these kids say, and feel, and think.
12 And so that's how they live.
13 Uh. I'd like to see that [*returning to a suggestion CS had made that welfare recipients be provided with jobs*].
14 I want to see these people off the streets.
15 Working. [*She continues with passage 23*]

The verbal fumbling in passage 34 ("You know, uh—I don't want—but then . . ." in lines 3 and 4 and the "Uh" between lines 12 and 13) mark where Russo switches voices. Pauses, verbal fillers, and disfluency often indicate schema boundaries (see Chafe 1977, also Hill, this volume, for other interpretations of disfluency). In lines 1 through 3 Russo expresses deep concern for poor children and the overall emotional tone is pity. The emotional tone of the primary voice she uses to discuss poor people, by contrast, is angry and resentful, a hard voice[18] that uses tough, direct language (e.g., "get the heck off your behind" in passage 22) and shows no sympathy ("I don't care about her," passage 18). This is the voice she returns to from line 13 on in passage 34. In between, from lines 4 to 12, she seems to be groping for a way to reconcile her conflicting schemas. Interestingly, she returns to a culture-of-poverty discourse based on a folk psychological model of people as constructed by what they are taught and observe, an appropriately in-between model (I found it voiced across the political spectrum, Strauss 2002) that now conveniently serves to rationalize why we do not need to act on concerns for hungry children (they've been brought up that way, so they're used to it). However, Russo seemed to be improvising in response to her awareness of inconsistency: "You know, uh—I don't want—but then . . ."She acts as if she's torn between competing ideas, not as if she has (until now) integrated them.

Integration

In *Habits of the Heart* (1985), Robert Bellah and his coauthors worry that managerial and therapeutic discourses, which posit no higher ends than the corporate bottom line and the needs of the self, are displacing religious and civic discourses of absolute values and community belonging in the United States. While these social discourses carry these implications, are they so conflicting in their meanings for U.S. Americans? Not necessarily.[19]

[18] See Kusserow (2004) on hard versus soft individualism. Hard individualism stresses a tough attitude toward life; soft individualism stresses a tender regard for others' feelings. While Kusserow seems to have found only hard individualism among her working-class interviewees, I heard switching between voices, with the hard voice as the one that they adopted when they were particularly working-class identified.

[19] For a similar critique of Bellah et al.'s tendency to see a sharp divide between an earlier America in which absolute values reigned and a newer one of relative values, see Quinn's (1996) discussion of how Americans think about marriage.

I found that for a number of my interviewees, therapeutic discourse (detectable by keywords like *self esteem* and *self worth*, an emphasis on people's psyches, and concern with the learned or biochemical causes of their problems),[20] was perfectly consistent with talk of work as an absolute value. We saw an example of this with Peter Vieira. Thus, in passage 8 he says, "[T]he main thing that's been instilled to me is that if you don't have work, basically your worth actually as a person would, you would think less of yourself, I think." From "your worth actually as a person," which seems to mean moral worth, an absolute value, he slides into the therapeutic language of "you would think less of yourself."

Joan Morse integrated all four of the discourses that Bellah et al. worried were at odds in contemporary America. She is a member of the baby-boom generation who, typical of those born in the second half of the twentieth century, was very comfortable with therapeutic discourses, employing them to make sense of her own feelings as well as those of others. She is an accountant, so capitalist entrepreneurial and managerial discourses, which focus on factors that improve the bottom line, also come easily to her. Finally, she is an evangelical Christian with a large sense of social purpose who frequently employed Biblical as well as civic/humanitarian discourses. When I knew her, she was writing a workbook on Biblical economics (joining Biblical, managerial, and civic discourses) and she had helped start a soup kitchen (the need for which she explained with a mixture of Biblical, civic/humanitarian, and therapeutic discourses). Yet, although she had learned a variety of social discourses, she melded them into a consistent whole. The underlying idea in the following passage is that if one knows God, one will receive love; receiving love makes people emotionally healthy; and if people are emotionally healthy, they will be successful economically. So the solution to poverty is to show love through one-to-one interaction and to take care of poor people's needs in a missionary outreach setting where they will also come to Jesus. Religious, humanitarian, therapeutic (see her emphasis on "self-image," the importance of feeling "okay about themselves", emotional "health"), and entrepreneurial capitalist discourses (the last is particularly evident in her discussion of success, failure, and risk below) are combined in a way that makes internal sense whether we agree with it or not. Furthermore, these ideas appear in the same context, with no abrupt switching of discourses, so we can conclude that Morse has integrated these social discourses:

35. God is a God of one-to-one relationships with *us* and unless we do it with other people . . . I led somebody to the ( ) last Sunday and he had gone to a place where it was very isolated and cold and he just didn't want to receive Jesus as his savior because it's like, if Jesus was like that, forget this. You know, I'm a person I have needs, I cry, I laugh, I—you know, whatever. I spent like 20 minutes talking to him and telling him that God *loved* him. Well, he *wanted* that kind of a God. So okay if you

[20] I am using "therapeutic discourse" to cover both of the opposing psychodynamic and biomedical clinical approaches.

> do that on a one-to-one basis, you know, somebody is hungry today and then they need some job skills tomorrow, and on a one-to-one basis you help them . . . pretty soon their self-image is better. When you think you're a jerk you act like a jerk. (laughs) When you think you're going to fail, you're going to fail. The only people who succeed are those who are willing to fail and take the risk, and the only people who can take risks are those who feel okay about themselves. My self-image is not going to *die* if I make a mistake with a client. I'm going to try like crazy not to ever make one. But the thing is I'm going to one day, I'm a human being. I spent time in my life where if I made a mistake, my gosh I was going to have to be blown away, I mean my image was totally wrapped up into whether or not I was totally successful. Seems like a contradiction in terms, but people who have a bad self-image set themselves up for failure, 'cause that's what they expect of themselves. People who are healthy will *risk* having a failure because their self-image is not tied up into their success. So, and the only way that can happen—you can't mass lecture people into having a good self-image, that doesn't make any sense. So you one-to-one take care of it, you do one family at a time, one person at a time . . . you know get them the job, get them the house, get them the training . . . . then they like, "Wow there's somebody that loves me, my heavenly father loves me. These people care enough about me to take time to touch me." I mean these people, nobody ever touches them. Remember—who was it that did the surrogate mother, with the monkeys? The surrogate mother? Uhm, it wasn't Pavlov, he did the dogs. The surrogate mother where they had the wire cage and then they had the cage with the lamb's wool all around it?[21]

I should note that Morse's integration of these discourses was not something she accomplished all by herself. (This discussion should not be taken as implicit praise for her integration in this realm in contrast to Rocha and Carter's compartmentalization or Russo's ambivalence.) She had probably read works and heard speakers who combined religious and therapeutic discourses as well as religious and capitalist discourses.[22] I know she belonged to a Gospel businessman's lunch group; I attended one of those

[21] Here is another example of Morse's integration of neoliberal, therapeutic, and religious discourse: "Well, the government is projecting itself to being my savior but it's not doing it because it can't. So if it's projecting itself to being my savior and it doesn't do it, then I'm angry because my expectations were thwarted. Well, looks like we've got to do two things, we've got to change the expectations and then have the civil government stay out of things that it has not business being in. Your protector is your heavenly father, and you know he uses your husband to do that on occasion, sometimes he may even use angels to do that. But for you to look to your husband and say, 'Well, you haven't taken care of me or protected me the way I wanted,' is this triangle thing of persecutor, persecuted. You know with this vicious circle then takes place because you have expectations about how you want to be protected and he's not going to know that. So he's not going to do exactly the way you wanted, and unless you're really healthy within whom you are, and in the relationship to God knowing that he's your protector, you're going to expect all of these things from people around you that they can't give you nor should they."

[22] See also Schram (1995) on the way "economistic-therapeutic-managerial discourse" is dominant in policy documents about welfare.

lunches with her. And it is very likely that she and the other members of her church had thought about how to integrate some of the sterner scriptural doctrines with the soup kitchen they started, because she discussed this problem:

> 36. So the scripture clearly says if you don't work you don't eat. So then you think, "Well, okay the soup kitchen is in violation of that." Well, nobody's taught them, they've got to be taught. Just like your two-year old, you're going to teach her and train her and train her well. You're not going to expect her to know what your twelve-year old knows. People have to crawl before they can walk and run, and that's what we're all about.

This is the same conflict that Russo dealt with: how to reconcile beliefs in self-reliance with the desire to help people who need help. In Russo's case it appeared that she had not thought about this conflict before. I suspect that the interview context itself raised the conflict, as a result of which she felt torn, and strove to reconcile the views. Morse, on the other hand, deals with the seeming conflict between the Biblical injunction and the soup kitchen with ease (no verbal fillers, false starts, or pauses), as if her answer to this conflict had been rehearsed. Probably she and her co-congregationists had discussed the issue previously because the conflict between scripture and their practice was too blatant to be ignored.[23]

Finding a way to integrate the competing ideas that one should feed the hungry and "if you don't work, you don't eat" had the practical consequence that Morse and the other members of her church could then throw themselves into their humanitarian work. This is why it is useful not only to catalog competing discourses and ideas but also to understand how people mentally organize them. The way people act on the diverse ideas they hold is probably influenced by the way in which they hold them. My guess would be that compartmentalized ideas would lead to people acting differently in different contexts, integrated ideas would lead to actions that are more consistent across contexts, while ambivalence would lead to paralysis or change, to resolve the inconsistency.[24]

Finally, like personal semantic network analysis, by investigating the way people organize the different discourses to which they are exposed we obtain a better sense of subcultural patterns and possibly historical shifts

[23] Interestingly, in this passage the solution Morse gives was the same Russo came up with: to mitigate individual or social responsibility by seeing people as constructed by their sociocultural environments. In Russo's case, that smoothes the way to the conclusion that poor people don't necessarily react to privation the same way as people who have known a better life; in Morse's case it leads to a much softer conclusion: They are not responsible for their failures to be self-sufficient.

[24] Naomi Quinn (personal communication, April 2001) reminded me that extreme ambivalence can lead to change as well as paralysis. Leon Festinger argued that cognitive dissonance is painful, hence leads people who hold conflicting beliefs to change one of the inconsistent beliefs to resolve the contradiction. What this overlooks is the possibility of compartmentalization, in which case the belief holder is not aware of the conflict.

in the meanings of key ideas. The Biblical admonition, "If you don't work, you don't eat," is much harsher without its newer codicil, "This injunction does not apply until you have received enough love to reverse the effects of your poor home environment."

Section 3. The Power of Public Opinion

In the last section we looked at the way people have internalized multiple discourses. Here we consider a related issue: Do speakers register awareness that there are ideas that compete with theirs, whether they hold these other views or not? If so, how seriously do they take the competition: Do they act as if their own views are embattled, a respectable alternative, or the common view? To put it another way, how does discourse reflect the *cultural standing* of different ideas, or the weight of public opinion?[25] The cultural standing of some idea is its acceptability in an opinion community. I believe that most people, most of the time, mark the perceived cultural standing of any opinions they voice, if these are opinions on topics that have been part of public discussion and they are aware of that. As Bakhtin put it, discourse "cannot fail to be oriented toward the 'already uttered,' the 'already known,' the 'common opinion' and so forth" (Bakhtin 1981:279). It is important in cultural analysis to pay attention to the cultural standing of the views we are studying because we can go seriously astray if we think a certain view is the common opinion when it is really just one perspective that is quite controversial.

Elsewhere (Strauss 2004) I give a theoretical background to cultural standing analysis. Here, I focus on how speakers mark cultural standing in American English. Cultural standing may have other markers in other speech communities.[26] Even among American English speakers, there may be differences in the way cultural standing is marked.

Cultural standing is a continuum. Four important points along this continuum could be labeled as follows:

Controversial Opinion—Debatable Opinion—Common Opinion—Taken-for-granted

[25] See Strauss (1997) for an earlier formulation and Strauss (2004) for a more complete account. Someone else whom I have heard speak on this topic is Victor Balaban, " 'You probably think this sounds nuts': Conflicting cultural models and how motivation is manifested in discourse," Southern Psychological Anthropology Reunion, Emory University, October 23–25, 1998. Deborah Schiffrin (especially 1985, 1987) provides a useful description of some of the same phenomena.

[26] The linguist Anna Wierzbicka observes that Anglo communicative norms rest on a "cultural emphasis on the value of compromise, of harmony in disagreement, of a balance between freedom of disagreement and a search for agreement" (as in the expression, "Let's agree to disagree") (Wierzbicka 1994:79). This may lead to culturally specific ways of marking cultural standing when the speaker expects the hearer to disagree.

We have already discussed the taken-for-granted end of this continuum, the cultural models that are so ingrained that they are not even considered to be matters about which one could have an opinion (Bourdieu 1977:167–170).[27] Social discourses, explicitly formulated in an environment of competing discourses, will instead be the common opinion, debatable or controversial. American English speakers have certain typical ways in which they mark each of these points on the cultural standing continuum.

Controversial Opinion

If a speaker feels that their views are highly controversial in the community represented by their audience (or in the larger society, if the speaker is unsure about where the hearer stands), sometimes they will simply *censor* them. For example, in an earlier set of interviews I conducted, one interviewee, Daniel Collins, called for a revolution for workers to regain their rights. At one point I asked him what he thought about socialism.

37. Collins: I'd rather not go into that . . . It might get into different things. CS: [*After long pause to see if Collins would say more*] I remember you saying last time that there's no freedom of speech; people call you a radical. [*Collins nods*] Because, actually, what you were talking about with the government owning the utilities and so on sounds like democratic socialism in Europe. [*Collins nods*]

I had the distinct impression that Collins did not want to be tape-recorded saying he is a socialist.

In other cases, speakers will state views they take to be controversial, but in a guarded way. To put it in the terms used by discourse analysts, they *modalize* their utterance, signaling low *commitment* to or low *affinity* with the proposition (Fairclough 1992:158, Hodge and Kress 1988:123). Fairclough lists some ways of doing this: use of modal auxiliary verbs such as "may" and "should"; modal adverbs such as "probably" and "possibly" or the corresponding adjectives ("probable," "possible"); other hedges ("sort of," "a bit"); and a hesitant way of speaking (Fairclough 1992:159, also Dijk 1987). Another way of modalizing I have observed is *lamination* (Goffman 1974, especially pp. 516–523).[28] Goffman used *lamination* to refer to layering of frames of activity or speech (e.g., pretending to be serious). In the case of narratives in which the speaker

[27] Many other anthropologists have commented on the fact that some cultural knowledge is held in this "transparent" (Hutchins 1980:12) way. The importance of Bourdieu's scheme is that it recognizes a range of cultural standing.

[28] *Modality* and *modalization* are used in different ways by different analysts. Here, I am using these terms to refer to all the devices speakers use as a way of commenting on the truth or acceptability of their own statements (drawing on Halliday's useful distinction between modality and modulation functions of modal auxiliaries, Halliday 1976). Unmarked, *modalization* refers to comments that express lower commitment or acceptability. Strengthening modalizers (Schiffrin 1985) express higher commitment or acceptability—but less so than no modalizers at all (Lyons 1977:763, cited in Simpson 1993:49).

tells a story about themselves, there is a necessary layering: The speaker is distinct from the self who is spoken about. (For one thing, the speaker has the wisdom of hindsight, while the self who is the protagonist of one's life story had only the knowledge available to him or her at that time.) Sometimes lamination has a humorous effect, for example, the self who is the protagonist of a personal narrative could be the young-and-foolish version, or the drunk-out-of-his-or-her-mind version, of the now mature, sober self telling the story. Sometimes, however, lamination serves the purpose of attributing to another version of the self a view the speaker is hesitant to embrace fully.

We can see all of these devices, and others as well, in the following passage. The speaker, Nancy Goodall, was the most radical of my 1995 interviewees (numbers in brackets indicate the length of pause in seconds):

38. NG: I view little facts like
most people when they hear welfare their portrayal is [1.9]
[*switches to a faster sing-song voice*] primarily a black woman
who's been on it for all her *life*,
her mother has been on it prior to her
and her grandmother,
she has 13 *children*,
and that's what we are supporting.
[*switches back to her normal voice*] Where in actuality, uh, most participants in welfare are *white*,
they have two or three *chil*dren
and they're on and *off* welfare.
Now a lot of people do need it
and use it as a short-term breach [1.2] for any type of uh economic [1.3] drop-off in their life.
Um, and, I think it is a very, very distorted view. [3.3]
And, I think it's *prop*agated by the media
CS: Why?
NG: And our government.
CS: Why?
NG: [***heavy sigh***] **Well [2.9] if you want to get into a real paranoid view**
you could almost say that [2.2] um [2.8]
Well, you've probably read Big Brother, *1984* [Um-hm]
and, um, if you can divert people's focus [2.4] from problems that perhaps would be a little more difficult for them to address [1.3], um,
you can divert them.
And, I think it works for our political system.

The sing-song voice that Goodall uses in lines 3–8 is a kind of lamination: Goodall makes it very clear that she does not subscribe to such racist views by using a different intonation, which says, in effect, "I don't hold this—I'm not even talking in my normal voice." (For more on the use of sing-song intonation, see the *Common Opinion* section below.) The main lines I want to discuss are 19–20, which begin with a heavy sigh and "Well."

"Well" often precedes what conversation analysts call a "dispreferred second," that is, the less typical "marked" response. Lines 19 and 20 are heavily modalized. Notice, for example, the way Goodall backs off from the forthright "I view" in line 1 and "I think" in lines 14 and 15 to an impersonal "you" who is now propounding her views in line 19 and 20, before finally owning her views again in line 24. She further signals low commitment to her views by hedging ("almost say") and by using the subjunctive tense and modal auxiliary ("could almost say"). She laminates, saying, "if you want to get into a real paranoid view." If she recognizes this is a paranoid view, then she is sane—it's her crazy alter ego that thinks that.[29] And, of course, she hesitates considerably. Hesitation is not always a sign of low cultural standing. For example, the 3.3 second pause between lines 14 and 15 probably marks the boundary between schemas. (See discussion of this on p. 228 above and in Chafe 1977.) The long pauses in lines 19 and 20, by contrast, along with her heavy sigh and other ways of modalizing all contribute to the sense that the ideas she expresses have low cultural standing. (Just as dragging out a word can also indicate reluctance to commit to the ideas it conveys.) Her hesitation could be a way of deliberately signaling low commitment to the proposition or may be an inadvertent delay caused by trying to put into words a view that she does not have much practice in expressing or hearing expressed. In the latter case, hesitation would be an indicator of objectively low cultural standing and not just perceived low cultural standing, for the views that come to our lips the most readily are the common opinion or debatable opinions that people are not afraid to discuss. If we have not heard it, it probably lies at one extreme (taken-for-granted) or the other (highly controversial) or perhaps off the chart altogether because it has no standing at all in the society (although it might in another culture or historical period).[30]

Debatable Opinion

Debatable opinions are in the realm of discussion and debate. It is recognized (in the communities represented by the parties to the discussion, or the larger society if it is not clear where one's interlocutors might stand) that there is more than one widely held opinion on the subject. Typically, American English speakers indicate that this is a debatable opinion by *explicitly marking it as their own opinion* with an expression like "I think," "I view," "In my opinion," or "To me" (as Nancy Goodall did in lines 1, 14, 15, and

[29] It would still be lamination if she used a first-person construction, for example, "In my more paranoid moments I think that. . . ." Here is a parallel example from Goffman: "Take this bit of melodrama: 'There is no excuse. You are right to hate me. I am coming to do so myself.' Warmly animated, this utterance is something of a paradox. After all, anyone who identifies himself with the standards against which the culprit is being judged (and is found wanting) can't himself be all bad—and isn't, and in the very degree that he himself feelingly believes he is" (1974:521).

[30] Daniel Segal pointed out the "off the chart" possibility when I presented this material in a talk at Pitzer College, February 2000.

24 of passage 38 above). Or they will *present the opposing point of view, or represent the discourse of the critic (e.g., by using words and phrases associated with that social discourse),*[31] *then respond to it.* Sometimes they acknowledge that there is another point of view implicitly by *giving reasons* for their views instead of stating them flatly.[32] A gradient of degrees of cultural standing can be inferred from the way in which an argument is framed, for it can be assumed that if a speaker offers proposition B in support of proposition A, then they judge B to be less controversial than A for the addressee.

Returning to passage 1 of Peter Vieira's we can see examples of all three of these ways of marking his views as being a debatable opinion:

1. I mean, everybody's always talking about how there's no jobs out there. There are no jobs out there. Okay, fine, there are not jobs out there. But I mean to go into a McDonalds and start working. I don't see a problem with that. I mean, it is, it is, some type of work. You're doing something, you're making some kind of money.

Vieira frames his view (working in a menial, low-paying job is better than not working) as a debatable opinion first by representing the other point of view "everybody's always talking about how there's no jobs out there." (From the context, it appears that "there's no jobs out there" means "there are no good jobs out there.") Then he marks this as *his* opinion: "**I don't see** a problem with that." "I see/don't see," like "I think" or "in my opinion," acknowledges that someone else might think differently. Finally, he gives reasons for his view: "You're doing something, you're making some kind of money." He assumes (rightly so, I would say on the basis of my research) that doing something and making some kind of money have high cultural standing in the United States. They are less controversial than the opinion that one should take any sort of menial job, so can be used to bolster his argument.

Common Opinion

Common opinions are the views that speakers assume are widely shared, either in the larger society or in the community of opinion they share with their interlocutor. They are typically *not modalized* at all, and show *no prosodic or paralinguistic signs of hesitation. Rhetorical questions might be used to express these culturally obvious truths.* Sometimes the common opinion is indicated by formulaic language, what I have called *verbal*

[31] Here is an example of representing the discourse of the critic. David Horowitz's controversial ad opposing reparations for slavery, which appeared in several college newspapers during the 2000–2001 academic year, had the headline, "Ten Reasons Why Reparations for Slavery Is a Bad Idea—and Racist Too." By calling reparations for slavery "racist," Horowitz uses (and preempts) the terminology of his critics. As this example indicates, this technique is used for controversial ideas as well as ones that are a debatable opinion.

[32] See also Schiffrin (1985) on features of "rhetorical argument."

molecules, that is, frozen bits of discourse repeated verbatim by different speakers or by the same speaker on different occasions (Strauss 1997:242). Verbal molecules are especially likely to be indices of the common opinion when they are adages (Perelman and Olbrechts-Tyteca 1969). Some examples invoked by interviewees who were unsympathetic to welfare were, "You make your bed, you sleep in it" and "If you play, you have to pay." On the other side, representing a more sympathetic approach, were "Walk a mile in my shoes" and "There but for the grace of God go I."

Sometimes a verbal molecule is not a seemingly timeless saying but a clichéd phrase that is in the air because it is the way a lot of people talk about that topic. These canned phrases serve two purposes. They give you something all packaged and ready to say when the topic comes up (Abelson 1968). Furthermore, because you have heard these phrases many times before from other speakers like you, they seem to be the safe way to express the common opinion. With verbal molecules of this sort wording may change a little, but the basic formula is preserved. Thus, Carol Russo says of welfare recipients, "she's never been educated into the fact that she should **get off her butt** and go to work" (2) and "here are these people that **sit around on their butts**" (18), and so does Marlene Randall, "these young girls . . . **sitting on their butt.**"

Deborah Schiffrin found that *you know* sometimes "marks the general consensual truths which speakers assume their hearers share through their co-membership in the same culture, society, or group" (Schiffrin 1987:274). She adds this is more likely if *you know* is spoken with falling rather than rising intonation (ibid:291).[33] Schiffrin also points to quotative expressions like "they say" as conveying consensus. Both of these features are combined in passage 26, where Carol Russo, talking about welfare recipients, says,

> 26. **You know, like they say**, Well, it's Saturday night, they're going to go out . . . and raise hell. And Monday morning . . . Anyway, **I don't think** there's one right answer but **I think** one of the biggest answers would be to cut off all these illegitimate children.

Notice that when Russo is repeating what people say, and what she takes to be the common opinion, she frames her comment with *you know*. As soon as she leaves what she takes to be safe, consensual common ground to voice the more debatable opinion that welfare recipients should be sterilized, she marks the latter with an *I think* rather than *you know* and acknowledges that there are other points of view: "Anyway, **I don't think** there's one right answer but **I think** one of the biggest answers would be to cut off all these illegitimate children."

[33] Macaulay (2002, see also Macaulay 1991) takes a different position on the function of *you know*. I am not claiming that every use of *you know* means "you know what I'm talking about," only that when it does have that meaning, it refers to a view that the speaker believes to be the common opinion.

Sometimes, a speaker frames the common opinion as *conventional wisdom*. We call a view "conventional wisdom" if it is a common opinion that we think is wrong. One way of showing we think this is the conventional wisdom is to use a mocking, sing-song voice, as Nancy Goodall does in repeating the standard stereotype of welfare recipients in passage 38.

Taken-for-Granted Ideas

I discussed earlier how to infer implicit, taken-for-granted ideas from what speakers do say. Sometimes views that are taken for granted, or lie somewhere between the common opinion and what is taken for granted, are buried in *embedded clauses* (Fairclough 1992:120–121). If a proposition is taken for granted, it does not have to be directly asserted, and indeed it would be odd to do so.[34] Thus, for example, at a Thanksgiving dinner I attended and where (with permission) I taped the conversation, one of the hostesses said, "The reason that Communism fell down was because of the prosperity in capitalist countries." This sentence has two embedded clauses: *Communism fell down* and *Capitalist countries are prosperous*. She presupposes these ideas instead of asserting them because she assumes they are beyond question. (If she had asserted them, she would have said, instead, something like "Communism fell down. Why? Capitalist countries are more prosperous.") Notice also her use of "the reason that" before "Communism fell down" and "the" before "prosperity in capitalist countries." *The reason that* and the definite article *the* are *presupposition triggers*,[35] that is, words and other utterance features that generate presuppositions. For example, if I say, "The reason that you are behaving so strangely is that you forgot to take your medication," the proposition asserted is that you are behaving strangely because you forgot to take your medication. That you are behaving strangely (which you might question!) is simply presupposed.

Final Thoughts: Collecting Discourse, Transcribing it, and Replication Issues

In the beginning of the chapter I said that the methods presented here are applicable to different kinds of discourse. Cultural standing, for example, can be analyzed using any kind of discourse, both oral (unelicited or elicited, one-on-one or group) and written (fiction, essays, letters, speeches, and so on). Notice in the following published essay the way Barbara Ehrenreich

[34] In line with Grice's Maxim of Quantity: "Make your contribution as informative as is required for the current purposes of the exchange; do not make your contribution more informative than is required" (Grice 1975).

[35] Levinson (1983) has a nice discussion of these, drawing on a manuscript by Lauri Karttunen.

uses lamination and the impersonal third person to indicate her awareness that her views are controversial:

> 39. **One could conclude, if one was in a very bad mood,** that it is not in the interests of affluent feminists to see the wages of working-class women improve. (Ehrenreich 2000)

It would be fascinating to take certain ideas and trace their rhetorical treatment historically to see if they shift from being controversial, to debatable opinions, to the common opinion, or vice versa. Certain signs of cultural standing (e.g., hesitation, repairs of a misstatement) will be lost in texts that have been edited for publication or that are rehearsed for delivery before a wide audience, but others will doubtless remain.

Analysis of personal semantic networks and the way people internalize social discourses can be performed on written as well as oral discourse, but for these analyses it is necessary to have a great deal of discourse produced by the same person. Published letters or several lengthy articles or essays might be good sources. On-line instant messages would probably be a poor source.

Interviews are not suitable, either, if they are too short or too mechanical because the interviewer has created a let's-get-this-over-with-as-quickly-as-possible atmosphere. Inconsistent ideas are most likely to be revealed in a lengthy interview, or over the course of two or more interviews, as a result of the interviewer asking different kinds of questions and eliciting different kinds of discourse (e.g., personal narratives as well as general statements of opinion). For personal semantic network analysis, it is imperative to let interviewees speak in a stream-of-consciousness fashion, moving from one topic to another as the interviewee sees fit instead of adhering to a fixed interview schedule. For interviewees to reveal their emotional hot spots, interviewers have to be nonjudgmental and friendly: A stiff, formal approach will not work. That does not mean the interviewer pretends to be a friend. Conversations between friends are reciprocal. An interview is different: It should be the interviewee's opportunity to speak at length, without the interviewer competing for conversational turns. The interview transcript should contain long turns by the interviewee, broken up only infrequently by the interviewer.

Oral data has to be transformed into written text so you can mull over it and present your analysis to readers. How much detail is necessary in transcription? You probably noticed that different passages in this article include different levels of detail. In passage 38 pauses were timed, with their length indicated in square brackets; in all other passages pauses were indicated only by ellipses. In passages 34 and 38 I numbered every sentence; in the rest I did not. Others would argue for greater consistency, but I think this should be a matter of convenience. It is extremely time consuming to take a stopwatch to every pause or to number every line, and it is distracting for the reader to deal with that extraneous information. If the length of

pauses is relevant for your analysis, you should measure it, and if it improves the readability of your exposition to number lines in a long quoted passage, do it. If not, why bother?

Qualitative discourse analysis methods inevitably raise the issue of replicability. How likely is it that two people will find the same personal semantic networks or social discourses or markers of cultural standing in the same texts? I tried this once. I gave a graduate assistant the complete interview transcripts for one of my interviewees and taught her how to do a personal semantic network analysis. She came up with the same key elements I had. Clearly some parts of the analysis are more cut-and-dried than others. It is straightforward to determine what topic follows what in a PSN analysis. Determining emotional hot spots is not. I would argue, however, that it is better to try to find emotional hot spots and the other features described here, presenting all the evidence that you used so readers can judge for themselves whether your analysis is plausible, than to leave out these important aspects of discourse on the grounds that a machine could not do it.

References

Abelson, Robert P. 1968. Computers, polls, and public opinion—some puzzles and paradoxes. *Transaction* 5(9):20–27.

Baddeley, Alan. 1999. Memory. In *The MIT encyclopedia of the cognitive sciences,* ed. Robert A. Wilson and Frank C. Keil, pp. 514–517. Cambridge, MA: MIT Press.

Bakhtin, Mikhail M. 1981. Discourse in the novel. In *The dialogic imagination,* ed. Michael Holquist, pp. 259–422. Trans. Caryl Emerson and Michael Holquist. Austin, TX: University of Texas Press.

Becker, Gary. 1976. *The economic approach to human behavior.* Chicago, IL: University of Chicago Press.

Bellah, Robert N., Richard Madsen, William M. Sullivan, Ann Swidler, and Steven M. Tipton. 1985. *Habits of the heart: Individualism and commitment in American life.* New York: Harper and Row.

Bourdieu, Pierre. 1977. *Outline of a theory of practice.* Trans. Richard Nice. Cambridge, England: Cambridge University Press [Orig. publ. 1972].

Brooks, David. 2003. Huah. (Review of David Lipsky, *Absolutely American: four years at west point.*) *The New York Times Book Review,* July 13, 2003:9.

Cammisa, Anne Marie. 1998. *From rhetoric to reform? Welfare policy in American politics.* Boulder, CO: Westview Press.

Chafe, Wallace. 1977. Creativity in verbalization and its implications for the nature of stored knowledge. In *Discourse production and comprehension,* ed. Roy O. Freedle, pp. 41–55. Norwood, NJ: Ablex.

D'Andrade, Roy G. and Claudia Strauss, eds. 1992. *Human motives and cultural models.* Cambridge, England: Cambridge University Press.

Dijk, Teun A. van. 1987. *Communicating racism: Ethnic prejudice in thought and talk.* Newbury Park: Sage.

Ehrenreich, Barbara. 2000. Class ceiling. *Independent,* January 5–11, 2000.

Fairclough, Norman. 1992. *Discourse and social change*. Cambridge, England: Polity Press.

Foucault, Michel. 1972. *The archaeology of knowledge and the discourse on language*. Trans. A. M. Sheridan Smith. New York: Pantheon Books.

Goffman, Erving. 1974. *Frame analysis: An essay on the organization of experience*. Cambridge, MA: Harvard University Press.

Grice, H. Paul. 1975. Logic and conversation. In *Syntax and semantics*, ed. Peter Cole and Jerry L. Morgan, pp. 41–58. Vol. 3. New York: Academic Press.

Halliday, Michael A. K. 1976. Modality and modulation in English. In *Halliday: System and function in language*, ed. G. R. Kress, pp. 189–213. London: Oxford University Press.

Hill, Jane. 1995. The voices of Don Gabriel. In *The dialogic emergence of culture*, ed. Bruce Mannheim and Dennis Tedlock, pp. 96–147. Urbana, IL: University of Illinois Press.

Hodge, Robert and Gunther Kress. 1988. *Social semiotics*. Cambridge, England: Polity Press.

Holland, Dorothy and Naomi Quinn, eds. 1987. *Cultural models in language and thought*. Cambridge, England: Cambridge University Press.

Hutchins, Edwin. 1980. *Culture and inference: A Trobriand case study*. Cambridge, MA: Harvard University Press.

Kristeva, Julia. 1986. Word, dialogue and novel. In *The Kristeva reader*, ed. Toril Moi, pp. 34–61. Oxford: Basil Blackwell.

Kusserow, Adrie. *American individualisms: Child rearing and social class in three neighborhoods*. New York: Palgrave Macmillan.

Levinson, Stephen C. 1983. *Pragmatics*. Cambridge, England: Cambridge University Press.

Linde, Charlotte. 1987. Explanatory systems in oral life stories. In *Cultural models in language and thought*. ed. Dorothy Holland and Naomi Quinn. Cambridge, England: Cambridge University Press.

———. 1993. *Life stories: The creation of coherence*. New York: Oxford University Press.

Lyons, John. 1977. *Semantics*. Vol. 2. Cambridge, England: Cambridge University Press.

Macaulay, Ronald. 1991. *Locating dialect in discourse: The language of honest men and bonnie lasses in Ayr*. New York: Oxford University Press.

———. 2002. You know, it depends. *Journal of Pragmatics* 34(6):749–767.

Mead, Lawrence. 1986. *Beyond entitlement: The social obligations of citizenship*. New York: Free Press.

Peng, Kaiping and Nisbett, Richard. 1999. Culture, dialectics, and reasoning about contradiction. *American Psychologist* 54:741–754.

Perelman, Chaim and L. Olbrechts-Tyteca. 1969. *The new rhetoric: A treatise on argumentation*. Trans. John Wilkinson and Purcell Weaver. Notre Dame, IN: University of Notre Dame Press.

Quinn, Naomi. 1996. Culture and contradiction: the case of Americans reasoning about marriage. *Ethos* 24:391–425.

Schiffrin, Deborah. 1985. Everyday argument: The organization of diversity in talk. In *Handbook of Discourse Analysis*, ed. T. Van Dijk, pp. 35–46. Vol. 3. London: Academic Press.

———. 1987. *Discourse markers*. Cambridge, England: Cambridge University Press.

Schram, Sanford. 1995. *Words of welfare: The poverty of social science and the social science of poverty*. Minneapolis, MN: University of Minnesota Press.

Simpson, Paul. 1993. *Language, ideology, and point of view*. London: Routledge.

Singer, Milton. 1972. *When a great tradition modernizes: An anthropological approach to Indian civilization*. London: Pall Mall Press.

Strauss, Claudia. 1990. Who gets ahead? Cognitive responses to heteroglossia in American political culture. *American Ethnologist* 17:312–328.

———. 1992. What makes Tony run? Schemas as motives reconsidered. In *Human motives and cultural models*, ed. Roy D'Andrade and Claudia Strauss, pp. 197–224. Cambridge, England: Cambridge University Press.

———. 1997. Research on cultural discontinuities. In *A cognitive theory of cultural meaning*, ed. C. Strauss and N. Quinn, pp. 210–251. Cambridge, England: Cambridge University Press.

———. 2000. The culture concept and the individualism-collectivism debate: dominant and alternative attributions for class in the United States. In *Culture, thought, and development*, ed. Larry Nucci, Geoffrey B. Saxe, Elliot Turiel, pp. 85–114. Mahwah, NJ: Lawrence Erlbaum Associates.

———. 2002. Not-so rugged individualists: U.S. Americans' conflicting ideas about poverty. In *Work, welfare, and politics: Confronting poverty in the wake of welfare reform*, ed. Frances Fox Piven, Joan Acker, Margaret Hallock, and Sandra Morgan. Eugene, OR: University of Oregon Press.

———. 2004. Cultural standing in expression of opinion. *Language in Society* 33:161–194.

———. n.d. Blaming for Columbine: cultural models of agency in the United States (available from author).

Strauss, Claudia and Naomi Quinn. 1997. *A cognitive theory of cultural meaning*. Cambridge, England: Cambridge University Press.

Teles, Steven M. 1998. *Whose welfare? AFDC and elite politics*. Lawrence, KS: University Press of Kansas.

Weiss, Robert S. 1990. *Staying the course: The emotional and social lives of men who do well at work*. New York: Free Press.

Wierzbicka, Anna. 1994. "Cultural scripts": A new approach to the study of cross-cultural communication. In *Language contact and language conflict*, ed. Martin Pütz, pp. 69–87. Amsterdam: John Benjamins Publishing.

Williams, Raymond. 1977. *Marxism and literature*. London: Oxford University Press.

Yamada, Haru. 1997. *Different games, different rules: Why Americans and Japanese misunderstand each other*. New York: Oxford University Press.

Chapter Seven

"Good Enough" Methods for Life-Story Analysis

Wendy Luttrell

Introduction

Researchers of culture and consciousness who use narrative are caught between the proverbial rock and a hard place. On the one hand, we strive to listen and represent those we study "on and in their own terms." On the other hand, we recognize that our role in shaping the ethnographic encounter is huge; that whether consciously or not, we listen and make sense of what we hear according to particular theoretical, ontological, personal and cultural frameworks and in the context of unequal power relations. There is always the worry that the voices and perspectives of those we study will be lost or subsumed to our own views and interests. Given all this, it is understandable that some researchers see no way out of this dilemma.[1] But I advocate a different way of looking at the problem. I don't believe that researchers can eliminate tensions, contradictions, or power imbalances, but I believe we can (and should) name them. I like the way that feminist researchers Mathner and Doucet (1997) put it:

> The best we can do then is to trace and document our data analysis processes, and the choices and decisions we make, so that other researchers and interested parties can see for themselves some of what has been lost and some of what has been gained. We need to document these reflexive processes, not just in general terms such as our class, gender and ethnic background; but in a more concrete and nitty-gritty way in terms of where, how and why particular decisions are made at particular stages. (138)

[1] Many people have written about different forms of this dilemma. I like the way Behar and Gordon (1995) describe the dilemma in terms of anthropology's "double crisis," which they argue, has its roots in the postmodern turn and critique of the "realist" tradition in ethnographic writing and in the critique of white, middle-class feminist versions of women's experience that lesbians, women of color, and working-class women have been so effective in making. These debates and crises notwithstanding, and despite the fact that there has been much hand wringing and spilt ink over the question of what it means to be an anthropologist "writing culture," I agree with Faye Harrison who has observed that most anthropologists do not want to give up on written representations of culture, nor do we want to refashion ethnography as a "literary enterprise" (Harrison 1993:403).

I have written elsewhere about how my own background has affected my relationships, identifications, and exchanges with the working-class women and pregnant schoolgirls I have studied (Luttrell 1997, 2003). In this chapter, I focus on key decisions I made during my fieldwork and their consequences for life-story analysis reported in *Schoolsmart and Motherwise: Working-Class Women's Identity and Schooling* (1997).[2] I want to make a case for what I call "good enough" methods, whereby researchers think about their research decisions in terms of what is lost and what is gained, rather than what might be ideal. Accounting for the decisions one makes is the nitty-gritty of researcher reflexivity and the hallmark of "good enough" research. In this chapter, I discuss my emergent research design and decision-making about choosing methods of discourse analysis that would prove to be most useful in building a theory about school structure, culture and identity.

Tracing the Steps

I entered the field profoundly influenced by the ethnographic study of working-class high school boys by Paul Willis (1977). I was especially intrigued by Willis's analysis of the lads' knowledge—what he called their "cultural penetrations."[3] According to Willis, the lads had insights into their futures as working-class, manual laborers that led them to reject certain school values. These insights were not fully conscious—the lads were not aware that their resistance to school knowledge and values, their rebellious attitudes toward teachers' authority, and their hyper-masculinity were all part of the shop-floor culture for which they were destined. Willis's contribution to the study of culture and consciousness was to suggest that the links between structural determinants, cultural beliefs, and individual agency were far more complicated than first imagined. He represented the lads as being neither dupes of nor rebels against an educational system designed to keep working-class students in their place. Willis's work provoked a flurry of school ethnographies, including my own, in which researchers set out to discover pieces that he had left out, particularly regarding gender and race.[4]

[2] Another version of this chapter appeared in Harvard Education Review 70(4): 499–523 (2000). The initial inspiration to write an article came from Naomi Quinn's invitation to contribute to a methods volume she was editing. As time passed, I decided to submit the article for publication. For this volume, I have revised the chapter to focus specifically on decisions related to methods of discourse analysis rather than the larger issues of research design about which I had already written.

[3] The term "penetration" struck me as quite masculinist and I was interested in finding more gender-neutral ways to describe the knowledge people hold about the way the world works that they are not always aware they hold. This is where feminist versions of psychoanalysis would come in handy.

[4] Willis was successful in showing that structure, culture and agency are linked, but he did not explain the "how" of these links.

From 1980 to 1983 I observed and interviewed women who were enrolled in a community-based adult education program in Philadelphia. Like Willis, I wanted to learn how the women saw themselves in relation to school, now and in the past. I began the project by conducting short, semi-structured interviews with 200 women asking them why they were returning to school and what getting a high school diploma meant to them. The responses I collected provided a baseline set of issues I wanted to probe with a smaller sample of women. The following issues are some examples: What did it mean when almost every respondent said she wanted a high school diploma so that she could "become somebody"? What did it mean when so many women said that they were returning to school to become "better mothers?" (80 percent of the women offered this as a reason they had enrolled in the program.) And what did it mean when so many women described feeling "uncomfortable" in school? I wanted to understand the connection between education, betterment, and mothering, which was not a topic I had initially planned to study.

As an ethnographer, I believed it was important to investigate the women's responses as part of a larger fabric of social life and cultural beliefs. I had already gathered some clues from previous experience as a teacher in the adult education program. But when I returned to the program as a researcher, I began taking daily field notes about my everyday conversations and interactions with students and staff members in the program, in classroom settings, and in the neighborhood. In all three contexts the women provided me with examples of what might be called a cultural model of education and success—that there are those who are expected to "become somebody" and those who aren't. There are people who do the right thing (finish school, get married, etc.) and those who don't (or can't), which explains who gets ahead and why. There are those with brains, ambition, and drive who can make it in school, while others are lacking in these and therefore can not succeed. In one sense, I understood these beliefs in the context of "American individualism"—the common and unreflected-upon view of the individual as the only or main form of reality; a view of individuals that stresses self-sufficiency and independence as the most salient characteristics of the "free," American, adult subject.[5] But I also noticed that embedded in this shared talk about education and success was a critique of the American model of success and "betterment":

> Just because a person has a college degree doesn't mean he is any better than me, it doesn't give him the right to talk down to me with any less respect than a college professor. But I want a high school diploma so I can feel like I'm somebody.

I wanted to probe this equivocation, expressed over and over again by the women I spoke with and observed.

[5] See Linde (1993:200) for her discussion of ontological individualism in American culture.

I then selected 15 women who would represent (generally speaking) those enrolled in the program.[6] Everyone I asked to participate in the study agreed. These women were all white and had been born and raised in the neighborhood.[7] They had attended local schools (two-thirds went to public school and one-third went to parochial school) and had moved in and out of the labor force as waitresses, factory hands, and clerical workers. Because a relationship between schooling and mothering had emerged so saliently in the short interviews, I decided to interview women who were mothers with children still living at home. I want to emphasize that I did not realize, even when making this sampling decision, that I was taking a theoretical step in my conception of the research as a study of the relationship between mothering and schooling. This realization would come later.

I interviewed each woman at least three times in her home over the span of a year's time. The "official" portion of the interview lasted from one and a half to three hours, but I was often invited to stay for tea and more discussion after the tape recorder had been packed away. I transcribed the taped interviews and wrote up field notes after each interview, including my own reactions and interpretations.[8]

My interviewing strategy was unstructured and open-ended, interrupted only by clarifying questions. For the first interview, I opened by saying, "tell me what you remember about being in school?" In the second interview, I followed up on issues left over from the first interview (there were always questions I overlooked asking or events about which I needed clarification). In the third interview, we talked about why the women had returned to school, what, if any events had led to their decision and how they would describe their experiences. I tried my best to follow the women's lead, to consider seeming "tangents" as important clues. For example, many of the women talked about their early work experiences when I asked what they remembered about being in school. Rather than redirecting the conversation to discuss school, I pursued these work experiences, which turned out to shed important light on the women's class-, race-, and gender-concepts of knowledge and authority.

In rare cases when a woman did not offer an example to illustrate her point, I would ask for it. For example, one woman said that what she remembered most about school was that teachers treated students like her differently. She said she didn't have much more to say than that school was

[6] See Luttrell (1997:132) for a description of how I selected the sample.

[7] The neighborhood was known for its stability and yet, was experiencing tumultuous community relations, especially in terms of increasing drug use, racial violence, and domestic abuse. The year I started working in the local Lutheran Settlement House all three issues were on the top of the social service agenda which was the context within which the Women's Program opened its doors.

[8] See Kleinman and Copp (1993) for a good discussion of "notes on notes" in which field-workers record their doubts, feelings, and worries, as well as their emerging theories about what they are seeing.

"boring" and she didn't like being treated differently. I asked, "Can you remember a time when you were treated differently? What happened?" She proceeded to tell a string of stories about the cruel punishments she had suffered, including being locked in a closet. But usually I did not have to prompt the women; they offered up story after story about their life experiences. I came to view the women's passionate storytelling, what I called "their narrative urgency to tell it like it was," as an expression of the emotional salience of school and its formative role in the women's identities and self understandings.

Decision #1: Collecting Life Stories

I explained to the women selected that I was doing a research project about women's education and was interested in learning about their past experiences as girls, and why they had decided to return to school as women. My request for their school experiences was most often greeted with the refrain, "You want to know about my life? I could write a book about that." I was surprised by this response, but sociolinguist and life-story theorist Charlotte Linde (1993) would not have been. She argues that the life story is a taken-for-granted interpretive device, a discursive category furnished by American culture—the idea that we have a life story to exchange with others. She says, "In order to exist in the social world with a comfortable sense of being a good, socially proper, and stable person, an individual needs to have a coherent, acceptable and constantly revised life story" (1993:20).[9] Realizing, and then accepting, the fact that the interview material I was collecting was in the form of life stories, changed the course of this project and my career as a researcher. While I hadn't planned my project to be about life stories, the subjects of my research held their own notions of what my project was about and what they wanted from it. I learned two things from this. First, that the extent to which respondents actively shape the research process, while not well documented within scholarly reports, cannot be underestimated. Second, that reconstructing culture from discourse depends upon attending to the richness and variety of discourse types one may encounter. Whereas I began my research expecting to identify "accounts" (one discourse type) about why the women had returned to school, and that these would shed light on their model of schooling and success, I had not been expecting life stories or forms of "personal narrative" (yet another discourse type).[10]

[9] Linde defines the life story as consisting "of all the stories and associated discourse units, such as explanations and chronicles and the connections between them, told by an individual in his/her lifetime" (1993:21).

[10] See Luttrell (2003) for a detailed discussion of life story as a taken-for-granted interpretive device. In my research with pregnant schoolgirls I found it particularly difficult to elicit life stories, for a variety of reasons. The girls with whom I worked did not narrate their pasts

I had been trained as a sociologist in fieldwork methods most closely aligned with symbolic interactionism. From this perspective, the information people give about themselves always serves more than one purpose, and tends to "add up," in one way or another. Borrowing on the language of mathematics, these theorists examined how people "account" for their actions, commonly explaining in advance what others might perceive as unexpected or inappropriate behavior. "Indeed, the giving and taking of accounts in everyday life represents one of the most fundamental characteristics of the social order" (Weinstein 1980:591). So, while I was prepared to elicit and analyze "accounts," I was not yet aware that collecting, interpreting, and narrating life stories was a common tool in the anthropological kit (it has since gained prominence in both sociology and anthropology since the oft-noted postmodern turn). Falling somewhere between autobiography and biography, the narration of these stories is meant to provide the listener a sense of what life is like or what it means to be a member of a particular culture.[11] James Peacock and Dorothy Holland (1993) divide the use of life-story analysis within anthropology and the social sciences into two types: "One emphasizes the 'life,' the other the 'story' " (1993:368). As my research evolved, I utilized both types. I elicited the women's life stories to learn something about the women and the structural as well as psychological processes that their stories were presumed to mirror—the "life-focused" approach. I was also using the "story-focused" approach (1993:370), paying close attention to the structure, coherence, and forms of discourse the women used to tell their stories. This dual approach enabled me to see that the women were constructing their identities and forging social relationships as part of the story-telling process. As I discuss in detail later, I also turned to a psychoanalytic approach to life-story telling and *listening*, to help me understand the divided sense of self I noted in the women's discourse.

Collecting and then transcribing the women's life stories was the most comfortable part of the research for me—it was when I felt most at ease as

---

in light of the present as the older women had. They didn't claim they could "write a book" about all the things that had made them who they are today. Indeed, several girls said they didn't know where to start or what to say. Mostly, their relationship to the past was told in bits and piece, was not unified or linked as the older women's retrospective accounts had been. Thus, my realization of the girls' unique discourse types (including their performative style of storytelling) lead to yet other methods for reconstructing culture from discourse. See Luttrell 2003, chapter 6 for more discussion of these methods.

[11] As Naomi Quinn mentions in the introduction, there is, of course, much debate about the use and meaning of life stories collected by anthropologists, particularly when anthropologists bring certain expectations about what should constitute the life story of any particular person. Ruth Behar provides one, among many, examples. She writes that her subject, Esperanza refused to talk about certain matters, for example, sexuality, which is a key subject that we have come to expect to find discussed in women's lives. Behar says, "her life story, as she told it to me, was not a revelation of the 'real truth' of her inner life but an account of those emotional states (which were also often bodily and religious states) that she construed as worth talking about—physical suffering, martyrdom, rage, salvation" (1993:12).

a researcher listening and responding to what I heard. During this stage I felt that my mistakes could be corrected. If I listened to a taped interview and realized I had not followed through on a topic or missed an opportunity to probe for information and understanding, then I could go back and ask more questions.[12] The next stage of research—the classification and winnowing of the interview material—was more anxiety-ridden for me. I worried about the enormity of the analytic task (more than 500 pages of transcribed interview material) and the fear that I would "get it wrong." Meanwhile, at this stage of my research, increasing numbers of scholars had begun writing about the highly constructed nature of oral testimony and personal narratives. The more I read, the more I questioned my epistemological premises. I found myself moving between two ways of thinking about life stories. On the one hand, I saw these stories as factual accounts of the women's experiences, views, and values about schooling. On the other hand, I also understood that these stories represented what the women wanted me most to know and what they construed as being worth talking about (which is not to say that these stories were "fictions," but that they were told with particular points in mind). I decided against taking an either–or position on these two forms of life-story analysis—"life" and "story"; realist and reflexive.

While I believe there is an important distinction, I don't believe that researchers must choose to do one or the other. Part of the challenge of my research was finding a way to do both—to make realist claims about school culture and organization, the material conditions of the women's lives and their cultural beliefs, *and* to make reflexive interpretations of the ethnographic exchange between the women I studied and myself.[13] I designed a three-step coding procedure, in part to relieve my anxieties and in part to sustain what I saw as a necessary tension in the analysis of life stories.[14]

Decision #2: Developing the Coding Procedures

I provided each woman a copy of her interview transcript to see whether she wanted to make any changes. Most women told me they weren't interested in reading the transcripts; they wanted to read what I wrote

[12] Indeed, this is a stage of research in which getting feedback from more experienced interviewers is key. It is for this reason that I advocate teaching courses in research methods so that ethnographers/interviewers can develop and practice skills before entering the field. I disapprove of the "sink or swim" approach to fieldwork.

[13] It is this either–or thinking that has, in my mind, fueled the long-standing debate about whether there can be such a thing as a "feminist ethnography." I think reframing this question to ask, "In what ways is or is not a particular ethnography 'feminist'?" is a more useful approach.

[14] I have seen this anxiety in my own students who get to this stage. After collecting extensive and rich interviews, they feel overwhelmed about what to do next, hoping that their coding scheme will ease their worries.

about them (which only increased my anxiety level).[15] After gaining their permission to proceed, I read through each individual woman's set of transcripts and looked for an overall point, the gist of a woman's life story. During this reading I took note of recurring images, words, phrases, and metaphors as each woman talked about growing up. I listened for what sort of person I thought the interviewee wished to present, not just in school, but also at work, in the community, in her family, and to me.

During my second reading of each woman's set of transcripts, I selected out all the passages referring to school and educational views and values—what I called their school narratives. I took note of the sequence of the string of stories the women told (stories about teachers' pets and teachers' discipline routinely came first followed by stories of rebellion or acquiescence). I also looked for any coherence among the stories—what theme(s) tied them together. I paid particular attention to how each woman named her difficulties in school and how she sought resolution. Then I grouped together stories related to these problems.[16] My main aim was to glean insights about how each woman understood herself in school from the stories she told. For example, Doreen described herself having an "attitude problem" in school. I grouped this story together with one she told about being the class clown which won her the respect of her friends but not of her teachers (thus, she was never a teacher's pet). Later she told about having a "problem with authority" and "resenting school rules and regulations," which she explained was part of her decision to leave school when she became pregnant at age 16. She made it a point to say that being pregnant in school was not a problem for her, but the school regulations against pregnancy were. Still later she talked about her dilemma as a mother raising children who she hopes will "speak their minds," but not get in trouble with the teachers. She said she couldn't decide whether to laugh or to cry about some of the disciplinary problems her children bring home from school because "they've been taught to speak up when they think something is unfair." I viewed these stories as being related, tied together by her concern with/conflict about authority relations. Needless to say, a story could fit into more than one category. For example, the class clown story also fit into a string of stories Doreen told about how she often used humor to ease tensions at home or on the job.

During my third reading I looked for patterns across all 15 women's school stories. All 15 women identified three conflicts without any prompting on my part:

1. They all talked about having common sense, but not being "school-smart." This distinction emerged in each woman's narration of

[15] Two women did read and comment on their transcripts—the first said she was surprised, if not a bit embarrassed about how much time she had spent talking about her mother and the second asked for an additional interview so that she could fill in the gaps of her life story—events she had "short-shrifted," like how she had met her husband.

[16] This was before the availability of computer programs for doing qualitative research. I have yet to become proficient in the use of such programs and prefer a more "hands-on" approach that includes colored highlighting, cutting and pasting, filing and re-filing.

her childhood schooling, albeit not in the same way. It was as if my request for school memories was a catalyst for critical self-appraisal. The unstated assumption seemed to be that there was a story to be told, and this story was about comparing oneself to others through school-based eyes, as to who was better, "smarter," and more worthy. At times, the women sounded defensive as they described the gap between having common sense and being school-smart. The women would often make it a point to say that "real intelligence" has nothing to do with schooling (in fact, too much schooling could ruin a person's common sense). The smartest people they knew were those (working-class men) who could "make things work," who were "good with their hands." The fact that the women cited only men as examples of those people with "real intelligence" unsettled me. And as I began to ask more directly about this gap between common sense and "real intelligence" I discovered that gender and class were wedded together in the women's definitions of and values regarding knowledge. The women were aware that they held values about knowledge that differed from what they believed to be school values. But they were not necessarily aware that their explanations of these opposing genres of knowledge (school-smarts and common sense) could be seen as a critique of class and gender relations.[17]

2. All 15 women told stories about teachers' pets, describing why they had or had not been chosen as a pet. Some women could vividly recall the names, demeanors and outfits of the girls who had been chosen as pets. Others told about how they didn't like the girls who "acted cutsie" and that they had refused to join in the pet contest. Each woman also offered her own version of how teachers selected their pets—but all agreed that teachers liked girls who were both smart and submissive. These stories were told with strong feelings of envy and guilt, and sometimes with flashes of embarrassment for expressing so much emotion. In Gloria's words, "I still remember that girl's name; can you believe all these years and I still can't get over how the teacher treated that girl?" As in their stories about common sense, I heard the women blaming themselves for not being "pet material" and defending themselves against what they felt were unfair school judgments or expectations about working-class femininity. As Debra put it, "you couldn't be prissy and make it on the streets."

3. The third conflict they all mentioned revolved around aspirations. Each woman named the same set of career options that she saw available—"you could either be a secretary, nurse, nun (if you were Catholic), or mother." Nevertheless, each woman told about her childhood dream of "becoming somebody" (such as a judge, fashion model, or lawyer). Each then proceeded to explain difficult and often discriminatory circumstances and events in school that interfered with the realization of these aspirations. For example, one woman talked about having been placed into a

[17] This is an example of the women's "cultural penetration"—a knowledge of the way the world worked that they didn't know they held.

vocational track called "kitchen practice—the lowest of the low" despite her success in junior high. She lamented that at the time she had not challenged this school placement.

These conflicts about aspirations and obstacles resonate with Charlotte Linde's observation that "people show enormous zest for discussing their experiences in high school, however horrific the stories they tell may be" (1993:25). She attributes this zest to the prohibition within American culture against talking about *class* as a legitimate explanation for why people end up in the particular position that they do. Instead, there seems to be an unspoken assumption that important life decisions are made in schools, decisions that people feel compelled to incorporate and explain as part of their life story. The women's stories also echoed the work of Sennett and Cobb (1972), whose interviews with white, working-class men had produced what they called "confessions" and "defensive" stories about the role of formal education.[18]

Symbolic interactionists might have analyzed the women's school stories as "accounts" and looked to explain different face-saving strategies, such as "credentialing" (a strategy for establishing qualifications for one's actions so they won't be viewed negatively) or "disclaimers" (a strategy used to counteract in advance the possibility of being seen as not exercising good judgment).[19] Across the interviews, the women used the same accounting strategies in their school narratives. First, they would explain that, as young girls, they had often thought about going to college, but that financial difficulties had made it impossible to consider seriously (a disclaimer if applying an accounts framework). But the discussion would not stop there. As if an explanation based on lack of financial resources was not enough, each woman would go on to describe how she also did not "feel comfortable" with "those" kinds of students—students who were "real smart" or had money or acted superior because their parents were professionals. These feelings of discomfort were described in unique ways, from feeling a lump in one's throat when walking on a college campus, to being worried about not wearing the "right" clothes or saying the "wrong" things among people with higher class status. Those "other" kinds of people (including me? I wondered) were "different from" them. They felt more affiliated with people who had common sense, who had graduated from the "school of hard knocks" and shared common interests and concerns (a credentialing strategy.)[20]

[18] Linde discusses the evaluative property of life stories—that life stories have confessional qualities, allowing the narrator to reflect upon whether his or her self is (or was) good, proper, worthy, etc. "Confession may be good for the soul, but it is also excellent for the self-image" (1993:124).

[19] See Arlene McLaren (1985) for her use of the notion of accounts in her study of British working-class women's pursuit of education.

[20] See Luttrell (1997:15) for a discussion of an exchange with a Philadelphia interviewee in which the issue of our class difference was crystallized.

Paying attention to the women's accounting strategies made me return to the school narratives one more time, taking note of how often the stories were framed in class-based, "us" and "them" terms. For example, knowledge was presented in us–them terms. Thus, being "schoolsmart" was associated with middle-class, "professional" people, who were not necessarily better, just "different from us." In talk about school, middle-class, female teachers from the suburbs (understood as "them") were pitted against working-class girls (understood as "us") who shouldered considerable family and work responsibilities and resented being treated as if they "had nothing to do but go to school" (which also explained why stories about work were salient in their school narratives).[21] These same middle-class teachers only chose those girls as their pets who met traditional standards of femininity (i.e., those who could afford nice clothes, acted "cutsie" and obediently). The teacher's pet contest was an us-against-them struggle—and for the women I interviewed a key conflict was about choosing which side they were on.[22]

It struck me that these us–them stories about school served dual purposes. On the one hand, the stories explained the women's failed social mobility (disclaimers). Yet, at the same time, the stories affirmed their working-class affiliations and identities (credentialing). I argued that the women's accounts served to illustrate their shared class-based views and values about education and success. They shared the view that school is a sorting mechanism, dividing students into class-based, divided types—those who will succeed and those who won't. And they shared a set of personal experiences of school as a place in which they measured themselves against others in terms of divided types: those who were and were not pets; those who were and were not "college material"; those who were and were not "school" smart. But lacking in this explanation was the power of the women's feelings, especially their sense of ambition and envy that I detected in their affirmations of their "common sense," "street-smarts" and "mother-wise knowledge" that set them apart from their teachers. I wrestled with how to write about the emotions these stories evoked, what role schooling played in producing such feelings, and where these emotions fit into a larger argument about the women's gender and class identities and self-understandings.

Decision #3: Designing a Comparative Study

As I wrote about the interview material, I felt torn between reporting individual life stories (which could not so easily be reduced to a main point)

[21] In Luttrell (1997) I show that the women spoke "in the voice of" and "in the image of" their peers, often referring to themselves in the plural, as in "we knew what we wanted."

[22] Aspirations were also described in us against them terms, as in "We knew we were going to become secretaries. We weren't like those girls who were going to college."

and building a case for the patterns I was detecting.[23] It was at this point that I made a decision to focus on patterns and not individuals and something was lost and something else gained. I lost the capacity to see each woman primarily as an individual with her own story to tell, but gained clarity on what I came to understand as links between the social and the psychological in the women's narratives. I think the alternative approach (developing more complete and holistic case studies of individual women) would have had certain benefits. And some might argue that such an approach is more consistent with feminist epistemologies.[24] The trade-off, as I saw it, was that insofar as the women's individuality (the personal context and emotional force of their stories) would be lost, building a theory about the links among school structure, culture, identity, and self-understandings would be gained.

To trace and explain these links, I would need to cast the ethnographic net farther and investigate at least one other kind of school and another group of working-class women. I had moved to North Carolina and was working in a workplace literacy program that presented unique opportunities for a comparative analysis. When I learned that most of the North Carolina literacy students had grown up in rural counties and had attended one-room schoolhouses, I thought the urban–rural contrast of school context would be especially fruitful to examine. Both school contexts were also racially segregated, with the urban school being white-only and the rural school being black-only, and this provided another rich, if not complicated, layer of comparison.

My decision to do a comparative study came at a particularly contentious time in the historical development of feminist anthropology.[25] While some people were focusing on *commonalities* in women's experiences of gender inequality, others were focusing on the *differences* among women, and the context-bound nature of gender inequality. Still others were arguing against using the analytic category "women" altogether. I was taken with

[23] "Was my focus on the *women* or on the *schools* they attended?," one kind critic asked after I had completed writing up the Philadelphia study in the form of my dissertation. I wanted to do both, I said, but was not sure how to achieve this.

[24] I had this same conversation (about my decision to focus on the Philadelphia women as a group and the patterns that emerged throughout their stories and not on individuals) with some of the Philadelphia interviewees. One interviewee, after hearing me speak about the interview material at a conference, said she wished I had focused more on her individual life story. Another interviewee, who was also at the conference disagreed and said she didn't want me to write her story; if she wanted her personal story told she would write it herself. She preferred her experiences being used to illustrate larger points about "working-class women's lives" (her words). See Luttrell (1997:17–18) for more discussion of this conversation.

[25] See Micaela di Leonardo's introduction to *Gender At the Crossroads of Knowledge* (1991) for an excellent review of the feminist project in anthropology, which she rightly says has "flown under several flags." In this piece she traces the shift from the anthropology of women, to the anthropology of gender, to feminist anthropology, to feminist-inspired anthropological research and writing on gender relations, and how these shifts reflect changes in the academy, political economy and the critique of ethnography (1991:1–48).

Ruth Behar's (1993:301) observation of the opposing tendencies within feminism—"to see women as not at all different from one another or as all too different." She warned, "to go too far in either direction is to end up indifferent." Meanwhile, as Naomi Quinn (2000) has argued, feminist anthropology had reached an impasse, in part because of academic politics, and in part because of the feminist turn from universalism to particularism. Right at the time when feminist anthropology was close to explaining gender inequality (having identified many "near-universals"), it became unfashionable to develop explanations that could be tested or confirmed. In the context of these debates, my decision to focus on institutional, psychological, and cultural patterns and not individual particularities meant taking a position favoring one side over the other.

My responsibilities directing a workplace literacy program at a North Carolina state university included teaching, curriculum development, and research. This program had traditionally served the predominantly African American housekeeping, landscaping, and maintenance staff, mostly female housekeepers. I followed the same research protocol as in the Philadelphia study. I conducted short interviews with 50 program participants about why they were returning to school. Then I selected 15 women to observe and interview. These women had all been raised in southern rural communities, growing up mostly on tenant farms. Their past schooling had been sporadic, in part because of the demands of farm work and in part because of racial discrimination. The women were all employed as housekeepers; most had also worked as domestics, and some had been waitresses and factory workers.

My decision to label the two groups of women as the Philadelphia and North Carolina women rather than as the white, working-class and the black, working-class women (as I did in an early publication of my research)—was another research decision, one that I write about elsewhere (Luttrell 2000).[26] Here, my focus is on my approach to finding the structural features and cultural meanings embedded in the women's school stories and linking these to their personal conflicts.[27]

Decision #4: Attending to Variations

I used the same three-step procedure to analyze the North Carolina women's interview material, only this time I had the Philadelphia women's stories at the forefront of my mind.[28] I quickly learned that the North Carolina women offered the same skeletal school story as had been offered

[26] See Luttrell (1989).

[27] See Nancy Chodorow (1999) for her discussion of the underexamined links between cultural and personal meanings within feminist research on gender identity and within much psychological anthropology on self and identity.

[28] It would be interesting to know how my analysis would be different had I conducted the North Carolina study first.

by the Philadelphia women. They too cast school as a battleground, used us–them terms, and identified a series of divided types who peopled their school stories. Their stories also evoked powerful emotions, including defensiveness, self doubt, and a critical self-appraisal. But there were key variations on the theme that demanded explanation. Paying close attention to these variations enabled me to develop a more nuanced argument about the links between school structure, culture, and identity.

Variation A: Knowledge Types

Like the Philadelphia women, the North Carolina women divided knowledge into two types: common sense (often referred to as mother wit) and school-smarts. The North Carolina women also held those with common sense in high esteem, arguing that being educated was not the same as having "real intelligence." Indeed, "book learning" could not always match the wisdom of those who had actually seen or experienced life events, most especially slavery and the effects of Jim Crow laws. Moreover, the North Carolina women expressed a great deal of suspicion about much that is written in books, newspapers, and other printed material that did not reflect the "truth" of black life in white America. (I returned to the Philadelphia women's interviews to discover a similar distrust of the written word, albeit expressed in different terms. They were suspicious of "forty dollar" vocabulary, either spoken or written, that served to exclude people like themselves from public discourse.) But, unlike the Philadelphia women, the North Carolina women did not privilege the knowledge of men over women, nor did they associate "real intelligence" with skilled manual labor. Their definition of "real intelligence" was more all-encompassing, most often associated with "making ends meet," overcoming natural disasters (e.g., droughts and hurricanes), and avoiding racial conflict. Black men, as often as black women, were identified as having "real intelligence" when they successfully negotiated and survived racism, even when the emotional cost was great. Black women who were self-sufficient and raised a family without the support of a man were held in the highest regard as those having "real intelligence." And, yet, it was black men who could most easily cause a woman to "lose" her common sense, a recurring theme across the North Carolina's life stories. Like the Philadelphia women, the North Carolina women did not seem to be aware that their explanations of these opposing genres of knowledge could be seen as a critique of race, class, or gender relations. Both groups of women described knowledge in terms of divided types of people, not as a set of abstract categories.

Variation B: Teacher Types

Like the Philadelphia women, the North Carolina women pitted their middle-class, black female teachers against students like themselves (in this case, students who had "country ways" and darker skin color). But

embedded in these us–them stories were examples of "good" teachers who had treated them with special care and "bad" teachers who had belittled or made them feel ashamed of themselves and/or their parents. These stories had to do with teachers' efforts to either make up for or correct the women's "country ways" (in terms of speech, deportment, or clothing), which I learned was a cornerstone of the "racial uplift" mission of the all-black rural schools the North Carolina women had attended. Teachers were "good" if they provided students the means for uplift (such as clothes, books, hair ribbons, and other traditionally feminine accessories) and "bad" if they made fun of students' deficiencies. The link between the definition of goodness in teachers and the school's uplift mission led me to return to the Philadelphia women's interview material and reexamine what criteria they had used to assess their teachers. I discovered that the Philadelphia women also divided the teachers into good versus bad types. Teachers were "good" if they were strict but fair; teachers were "bad" if they were arbitrary or cruel. In light of the urban-bureaucratic school mission to prepare students for the industrial workforce, this emphasis on discipline made sense.

I began to realize that each school organization and mission had its own forms of authority and discipline and produced its own divided types. In the rural school, black female teachers' knowledge and authority were embedded in a web of community and school relationships, and discipline focused on enforcing "personal" attributes like manners and habits. In the urban school, white, middle-class teachers' authority was more distinct from working-class parental authority, and while these two forms of authority were not necessarily in conflict, they didn't stem from the same source. ("if I got in trouble at school, there was no question about what had happened. First I got it from the teacher and then I got it from my mother for getting caught in the first place.") The disciplining practices in the urban school had more to do with rigid rules and imposing "public" (i.e., work) attributes, like punctuality and obedience. Each group of women's critical self-appraisals reflected the disciplinary code of each school context. The North Carolina women characterized their problems in school as stemming from personal deficiencies and described themselves as "slow learners," whereas the Philadelphia women characterized their problems in school as stemming from rigid rules and their own "bad" attitudes toward authority.

Variation C: Feminine Types

The teacher's pet contest was an organizing theme in the North Carolina women's school stories, as it had been in the Philadelphia women's stories. But, rather than seeing themselves as potential competitors for the teacher's approval and affection (as had the Philadelphia women) the North Carolina women said that because of their dark skin color they would not have been chosen as a teacher's pet, even if they had wanted to be.

The teachers' pets—the light-skinned girls with the "good" hair and nice clothes and professional parents—were set against dark-skinned girls who were "passed over" by the teachers and made to feel "invisible." The fault lines dividing feminine types were distinctive for each group of women. For the North Carolina women, the color line was tightly woven into definitions of the "ideal" schoolgirl.

The North Carolina women's use of the metaphor of visibility (who was seen and recognized by the teachers) made me look for the metaphors the Philadelphia women had used to describe their relationships to and conflicts with teachers. I discovered a pattern that, again, I had not been attuned to before. When describing their schooling, the Philadelphia women drew upon terms related to voice—how they were "talked down to" by the teachers and how they had a "mouth," and resented not being able to speak their minds, and so on. These contrasting metaphors of visibility and voice provided yet more insight into the relationship between school structure, culture, and identity. One the one hand, these metaphors reflected the grammar of each school context, disciplinary code, and student response. In the case of the rural school context, where "bad" black teachers used public shaming and humiliation to enforce the school mission, making oneself "invisible" was an effective means to defend against personal attack. In the case of the urban school context, being silent in the face of abusively enforced rigid rules could provide protection from even further trouble. On yet a deeper level, these distinct metaphors spoke to a constellation of emotions about self–other relationships that were being forged in school settings.

Decision #5: Turning to Psychodynamic Understandings of Self–Other Relationships

My argument—that each school context produced its own set of divided student identities, critical self-appraisals, and anxieties about measuring up to school standards—was based on an analysis of variations in the themes that organized the women's school narratives. Yet, striking similarities, in the women's self-narration, especially their split sense of self, deserved examination. Both groups of women made persistent references to feeling torn about how best to present themselves in school. They talked about their different sides—for example the side that could "con" the teacher or "put on the dog" versus the side that resented having to play the game; or the side that wanted the teacher's approval versus the side that sought peers' approval and felt disloyal if chosen by the teacher as a pet. Moreover, it was not easy to resolve these two sides. In Helen's words, "I felt bad because I felt like I had conned them, like it wasn't really me who they (*teachers*) liked."

Initially, I had understood the women's "us versus them" stories about school in terms of class and race conflict. Drawing on a culturalist Marxist

tradition, most specifically E. P. Thompson, I argued that the women's "us–them" view of knowledge—their pitting common sense against school knowledge—reflected their class and race consciousness.[29] The women spoke about knowledge in the same way that Thompson defines class consciousness, as the way people "feel and articulate the identity of their interests as between themselves, and as against other men (*sic*) whose interests are different from (and usually opposed to) theirs" (Thompson 1963:9). I understood the women's notion of class (and race) consciousness to be embedded in relationships that are "embodied in real people in a real context" (Thompson ibid.). Real people and real school contexts were at the heart of the women's stories about knowledge and schooling and the explanations they gave for why they did or did not measure up to whatever they construed as the ideal. But there was another layer of emotionally charged talk that did not seem to fit into this class- and race-consciousness framework. This layer had to do with the women's split sense of self—the tenacious pattern whereby they would break themselves or others into two parts, one side idealized ("good") and the other devalued ("bad"). To help me understand this phenomenon I turned to feminist relational psychoanalysis for guidance.

Splitting is a psychoanalytic concept that is used to explain the meanings we make of and the feelings we attach to our relationships with others. In object–relations terms, it is our earliest experiences with those upon whom we are dependent for survival (usually maternal caregivers) that shape what we take in as meaningful, important, fearful or fanciful about ourselves (introjection) and what we project onto others (projection). These early emotional or fantasy meanings about a person, object, or idea become part of who we are and how we relate to others. Whereas splitting most commonly refers to individual psychic conflicts, I began to reconsider the range and scope of references to splitting throughout the women's self-narrations that were suggestive of personal conflicts *and* larger cultural processes. I took note of just how frequently the women cast aspects of the schooling process in divided, either–or terms, from their relationships to teachers (good versus bad); to the subject matter (academics versus vocational); to the division between school-smarts and street-smarts. And, as a consequence of paying new attention to varied forms of splitting that were embedded in the women's school stories, I realized that there were stories about mothers that I had not previously probed with the same fervor as I had the stories about teachers and students. Taking up a psychodynamic understanding of the women's life stories meant rethinking each speaker's identifications, conflicts and emotions, including those conflicts I might have preferred to avoid for myself. This decision brought me full circle from focusing on individual lives, to focusing on overriding patterns, back to focusing on individual lives (including my own).

[29] I developed this argument in Luttrell 1989.

Decision #6: A Psychoanalytic Listening to Life Stories

Within psychology, and particularly psychoanalysis, life stories (referred to as case histories) are elicited and tied together by the analyst as insight into a person's development or illness, as clues to both the care and cure of the patient. The foundation for this notion of the life story is that there is a "normal" developmental path against which everyone's life can be evaluated. Many critics of psychoanalysis, including some psychoanalysts themselves, have pointed out the "class-blind, politically pacifying and apparently ahistorical myth of human development" (Phillips 1998:39). Still, what appeals to me about psychoanalysis is the attention paid to how individuals make sense of their own story of development and how they infuse their life stories with fantasies, images, feelings, and other associations.[30] A description of the psychoanalytic life story I found useful was Juliet Mitchell's:

> [T]he patient comes with the story of his or her own life. The analyst listens; through an association something intrudes, disrupts, offers the "anarchic carnival" back into that history, the story won't quite do, and so the process starts again. You go back, and you make a new history. (1984:288)

Intruding associations—especially about mothers—disrupted many of my interviews. There were times when the women would be taken back to images and feelings from childhood, realizing that the story would not quite do. In one particular case, Kate, a North Carolina woman asked that I turn off the tape recorder before telling about a memory that she said still "has a hold on me." In telling her memory Kate tried unsuccessfully to keep from crying, apologizing several times for the fact that she couldn't tell her story without emotion. At the end Kate thanked me for being willing to "hold" her memory, but then said she wished we had tape-recorded it so she could hear it for herself. We agreed that I would return the next week to record the story. Not surprisingly, the taped version took some new turns, but again she focused on her regrets about not having been nicer to her (now-dead) mother. Initially, I filed this entire exchange as tangential—a disruption unrelated to my research topic.

In another case, Tina, from Philadelphia, was remembering an incident at school in which she had felt compelled to make up a story to cover for her mother's negligence. As she was telling the story she started to cry and then angrily said, "why did you make me remember all this? I don't like thinking about my mother this way." Feeling guilty and wanting to avoid conflict, I apologized, turned off the tape recorder and said I knew how

[30] George Rosenwald (Rosenwald and Ochenberg 1992) writes that no other science has paid such close attention to the life story as does psychoanalysis, whatever its limitations. I found the following work on narrative in psychoanalysis most useful: Steedman (1986); Rosenwald and Ochenberg (1992); and Mishler (1986).

much she cared for her mother. I suggested that we should stop for the day. This only made Tina angrier. She turned the tape recorder back on and said she had a lot more to say about her mother and that I wasn't getting off that easily. We talked for over an hour about how certain of her mother's actions still haunt her to this day and how she continues to feel anger and guilt about herself in relation to her mother. At the end, she said that I shouldn't "feel bad if people get angry" and that I shouldn't be "afraid to ask more about mothers." "I'm the first to say that I love my mother and that she did the best to raise me, but. . . ."

Realizing my reluctance to deal with strong emotions and mixed feelings about mothers—what could be called countertransference in my fieldwork relationships—marked a major breakthrough in my thinking and in my research process.[31] Becoming attuned to my feelings about and avoidance of what the women were saying about their mothers made me rethink the practice of ethnography and the role of interpersonal encounters. I like the way Nancy Chodorow puts it:

> There is no psychoanalysis or anthropology apart from the interpersonal encounter, an encounter that draws unavoidably on the investigator's power of empathy as well as observation. Both fields have come increasingly to emphasize the central participatory role and influence of the practitioner, who is no longer seen as a detached scientific observer. They now recognize that what comes to be understood about the subject (culture or psyche) is a product created within a particular encounter or set of encounters. In this encounter, both fields have increasingly emphasized the real emotional effects on the investigator: anthropologists who are not psychologically inclined write of culture shock; those who are psychologically attuned, of anxiety, fear and anger discovered through self observation, introspection, and observation of transferences that emerge in relation to informants or to the culture as a whole; psychoanalysts speak of the widened field of countertransference and the role of the analyst's subjectivity as well as objectivity in the analytic encounter. (1999:134–135)

I returned again to all the interview material, and with more self-awareness discovered a range of maternal images and mixed feelings that the women had expressed, but that I had minimized in my analysis of the links between school structure, culture, and identity.

A new line of questioning emerged from this finding: Why had the request for school memories evoked these maternal images and conflicts? Feminist relational psychoanalysts would argue that these ever-present,

[31] See Sherryl Kleinman and Martha Copp (1993) for an excellent discussion of emotions and fieldwork, particularly the section on researcher identifications with those being researched entitled, "The Costs of Feeling Good." Jennifer Hunt (1989) also writes about the role of transference and countertransference in fieldwork relationship in a way that I found helpful. And Barrie Thorne's (1993) realization that her own memories of being a schoolgirl made her overidentify with a particular girl she was observing also enabled me to rethink my reactions.

yet varying maternal images and memories reflect women's identificatory struggles in the gender- and self-making process (Chodorow 1978, 1989, 1995; Benjamin 1988, 1994; Flax 1987, 1990).[32] But, what structural or cultural aspects of schooling might produce such associations? And what might be the connection between the women's split sense of self, the particular school context, and their stories about mothers? As D'Andrade (p. 90) points out in this volume, "it is better not to ask informants directly about their models, but rather to ask something that will bring the model into play; that is, something that will make the person *use* the model." What model of schooling was being "brought into play" as a result of these interviews and how might this model address the women's identificatory struggles?

Two theories about the gendered organization of schooling helped me answer these questions. The first was Alison Griffith and Dorothy Smith's observation (1987) that schools are organized around the unacknowledged and devalued work of women (as teachers and mothers). The second was Madeleine Grumet's (1988) observation that teaching and learning is unconsciously associated with mothering despite great efforts on the part of schools, teachers, and students to deny and repress this connection. Both helped to explain why the women might express mixed feelings about the role of women (as mothers and teachers) in their life-story telling. The women's stories, which drew upon a vivid world of women (teachers, mothers, and daughters) revealed a knowledge about how the school world works that the women themselves did not know they knew.[33] One insight to be drawn from their descriptions and associations about school was that each school context had, in its own way, fostered personal conflicts and anxiety about a host of related issues, about school-wise knowledge, about themselves as female objects of desire ("pets"), about their class and race affiliations, and about their future lives as mothers, to name a few.[34] While the process of splitting (specifically the idealization and devaluation of women's work and knowledge) had helped the women deal with these

[32] See Chodorow (1995) for her observation that "guilt and sadness about mother are particularly prevalent female preoccupations and as likely to limit female autonomy, pleasure, and achievement as any culture mandate. . . . Similarly women's shame *vis-à-vis* men, whether of dependence or of discovery in masculine pursuits, is certainly situated in a cultural context in which such pursuits are coded as masculine in the first place. But this shame is also experienced in itself, inflected with many unconscious fantasies that stem from a time in development well before such coding could be known. It is a conflict in itself, and it inflects the general sense of self and gender as well as interacting with specific cultural expectations and meanings" (1995:540). I argue that the women's stories were tied together by (a) their social/cultural analysis of motherhood and (b) their personal/psychological preoccupation with their own mothers.

[33] Again, Willis would call this a cultural penetration. But I am arguing that the women's knowledge stems from psychodynamic processes that fill in a necessary piece of the puzzle about social and cultural reproduction in school settings.

[34] See Jenny Shaw (1995) about the relationship between school organization, anxiety, and gender. I have recently written about schools as sites of profound anxiety (Luttrell 2003). I argue that schooling evokes deep-seated personal feelings, and that institutional mechanisms

mixed emotions, it had also deflected the women's attention away from the effects of social inequality in their lives.[35]

A psychoanalytic *listening* to women's narratives made me realize how much more mutually engaging and intersubjective the process of fieldwork is, as well as the extent to which my own subjectivity shapes the research.[36] When I entered the field I was best prepared to think about issues of access and rapport, and to negotiate my multiple roles as a researcher, a teacher, and as a mother (I often brought my children to interviews). I saw these multiple roles as positive because I believed they enabled me to tap into the women's life worlds and perspectives.[37] In retrospect, I can also see that I was hearing the women's stories through several layers of identification—not the least of which were my own complicated feelings as a daughter and schoolgirl.[38]

Reducing Researcher Anxieties

The epistemology of social science demands a distinction between researchers and researched, observer and observed, and at the most abstract level, between subject (self) and object (other). But, this distinction need not break down into research relations of domination and submission, or idealization and devaluation, or detachment and (over) involvement.[39] This is why I agree with George Devereux (1967), who has argued that traditional research methods help researchers to overcome threats posed

---

that are often designed to contain such feelings can themselves create more anxiety and disaffection (e.g., depersonalized routines that allow teachers and students to distance themselves from feelings of failure). The convergence of institutionally created and individually felt emotions has been under-examined in the anthropological and sociological literature on identity.

[35] See Luttrell (1997:91–107) for a full discussion of how the cultural and personal myth of maternal omnipotence and idealized images of the perfect mother work in the context of schooling. Also see Griffith and Smith (2005).

[36] In graduate school I had studied symbolic interactionism as a theoretical and methodological approach to fieldwork. While I was fortunate to take courses with Nancy Chodorow, who exposed me to psychoanalysis and feminism, when I entered the field I did not see this perspective as guiding my research questions or approach. It is only in retrospect and in revisiting parts of my field notes that I have been able to see the connections between the theoretical traditions I have just outlined.

[37] What I like about this (classical) sociological view of the researcher as negotiator of roles and actor learning a social script, is its emphasis on consciousness and intentionality. Insofar as I made conscious choices and acted in certain ways based on what I learned in the field, then I could extend this notion of consciousness and intentionality to the subjects of my study.

[38] See Luttrell (1997:13–23) for a detailed discussion of my multiple identifications with each group of women. And see Luttrell (2003:147–170) for a discussion of how my position as a "daughter" was created by the women interviewees, whereas, in my work with pregnant girls, my position as a "mother" was reinforced in ways that imbued my analysis.

[39] This is the research version of splitting. See Luttrell (2003) for a more explicit discussion of how ethnographers engage in and defend against feeling "split at the root" in their work.

by making connections and relationships with the objects/subjects of their research. For example, he says that methods of detachment are a way for researchers to bind their anxieties and not necessarily a means to discover truth about an "other." Ruth Behar takes this observation one step further:

> Because there is no clear and easy route by which to confront the self who observes, most professional observers develop defenses, namely "methods" that "reduce anxiety" and enable us to function efficiently. . . . This is especially the case for situations in which we feel complicitous with structures of power, or helpless to release another from suffering, or at a loss as to whether to act or to observe. (Behar 1996:6)

But how do we prepare ourselves to effectively negotiate self-other ethnographic relationships? I propose that we strive to develop "good enough" methods in the same spirit as pediatrician and child psychoanalyst D. W. Winnicott (1965) called for "good enough" mothering. He argued that the perfect mother (like the perfect researcher) is a fantasy, a projection of infantile wishes that cannot be met by any *real* person. Moreover, efforts to attain perfection (either for one's self or for another) can stand in the way of healthy tensioned human relations.

It is possible to be a "good enough" researcher—that is, a person who is aware that she/he has personal stakes and investments in research relationships; who does not shy away from frustrations, anxieties, and disappointments that are part of any relationship; and who seeks to understand (and is able to appreciate) the difference between one's self and another. The "good enough" researcher tries not to get mixed up between fantasies, projections, and theories of who the "others" are and who they are in their own right. "Good enough" researchers accept rather than defend against healthy tensions in fieldwork. And they accept the mistakes they make—errors often made because of their blind spots and the intensity of their social, emotional, and intellectual involvement in and with the subject(s) of their research. These mistakes can be compensated for by the many times that they will do it right.

I believe that students of ethnography during anthropology's "double crisis"—in terms of the challenges to ethnographic writing and feminism—need both reassurance and direction. Students need practice in order to learn how to be attuned to questions of relationship, position, and social complexities, and how to turn resulting tensions into data, analysis, and eventually theories. Being reflexive is something to be learned in terms of degrees rather than absolutes (a "good enough" ethnography is more or less reflexive, not either–or in my view). I think of being reflexive as an exercise in sustaining multiple and sometimes opposing emotions, keeping alive contradictory ways of theorizing the world, and seeking compatibility, not necessarily consensus. Being reflexive means expanding rather than narrowing the psychic, social, cultural, and political fields of analysis.

Applying the "good enough" credo to gathering life stories, I suggest that researchers concentrate on the specifics of life events. Frigga Haug (1987)

calls this "memory work" and argues that it is not so much a question of respondents "having a good memory," as practicing it. she writes:

> Once we have begun to rediscover a given situation—its smells, sounds, emotions, thoughts, attitudes—the situation itself draws us back into the past, freeing us for a time from notions of our present superiority over our past selves; it allows us to become once again the child—a stranger—whom we once were. With some astonishment, we find ourselves discerning linkages never perceived before: forgotten traces, abandoned intentions, lost desires and so on. (47)

In this way I side with the "life-focused" side of life analysis, believing that buried memories can be recovered. I have listened countless times to respondents say, "I haven't thought about this in years. Now that we are talking about it I can remember . . . ." and then proceed to describe in rich detail a scene they had long forgotten. The passion with which the women recalled (and in some ways, emotionally relived) their pasts, makes me an advocate for facilitating interviewees' "memory work."

Two listening strategies are particularly useful toward this end. The first is paying attention to and pointing out interviewees' self-evaluative language and second is taking note of interviewees' meta-statements. Examples of the first are statements like, "I guess you could say I just didn't measure up," "I didn't work to my potential," "I didn't try hard enough," "I gave up on myself." Asking for details and examples—that is, "Can you tell me about a time when you felt like you didn't measure up? What happened?"—often opened up the space for the women to draw conclusions about their past selves (e.g., reconsidering what they valued and what they thought others valued, or how they were told to act and how they felt about themselves when they did or did not act that way). Examples of the second, or meta-statements, occurred when the women spontaneously commented about what they had just said, as if they were considering their statement from an outsider's (my own?) perspective. It was common for the women to say things like, "I know this might strike you as strange . . . .," or "that was a crazy thing . . . ," or "there's no telling why such and such happened." Paying attention to these meta-statements clued me into aspects of the women's model of schooling and knowledge that I might need to probe further (e.g., why I thought it might be strange that a Philadelphia woman chose not to attend an honors program into which she had been selected, a story which opened a storehouse of painful memories about the role her mother played in this decision). The frequency of the women's meta-statements made me realize the extent to which they held their own images of me as the researcher and audience of their stories. Perhaps these meta-statements (ways of hooking me as a listener) would not have been necessary had I not been viewed as an outsider, or as a "college-educated" person.[40] Nonetheless,

[40] See Catherine Kohler Riessman (1993) for her discussion of the teller–listener relationship in narrative analysis. Her work (and her comments on this chapter) have been very influential in my thinking.

I often found it difficult to ask why they thought I might consider something strange. At times it seemed improper (i.e., not the kind of question a researcher asks) and at other times I felt vulnerable. But if I was to be a narrator of the women's life stories, I felt it was necessary to take the risk to find out who they thought I was. When I began to ask—or perhaps I should say when it became impossible for me not to ask more about how the women were meant to feel about their mothers and how they actually felt (thanks to interviewee Tina who pushed me on this point)—only then was I able to tap into a range of complicated feelings and to connect these feelings to the gendered organization of schooling.

Finally, in collecting the women's life stories I learned to worry less about whether the women were "telling the truth" than in listening for gaps, inconsistencies and associations. Noting these out loud allowed both the women and me to rethink and reconstruct our stories. Here, the psychoanalytic, "story-focused" side of my ethnographer self predominates. For whatever else can be said about it, I believe that ethnographic research and life story analysis is about making meaningful connections with others who may or may not be like us. Karen McCarthy Brown puts it this way:

> Ethnographic research is a form of human relationship. When the lines long drawn in anthropology between participant–observer and informant break down, then the only truth is the one in between; and anthropology becomes closer to a social art form, open to both aesthetic and moral judgment. This situation is riskier, but it does bring intellectual labor and life into closer relation. (1991:12)

Many things influence how a researcher shapes her/his social art form—one's research questions, study design, and theoretical–explanatory approaches, coupled with one's particular temperament, personality, and intended audience. I strongly endorse the use of a variety of discourse methods (some of which are presented in this volume) to bring intellectual labor and life stories into closer relation. At its core, ethnographic research and cultural discourse analysis is creative, inventive, emotionally charged and fraught; and "good enough" researchers find ways to sustain all these aspects.

References

Behar, Ruth. 1993. *Translated woman: Crossing the border with Esperanza's story.* Boston, MA: Beacon Press.

———. 1996. *The vulnerable observer: Anthropology that breaks your heart.* Boston, MA: Beacon Press.

Behar, Ruth and Deborah Gordon. 1995. *Women writing culture.* Berkeley, CA: University of California Press.

Benjamin, Jessica. 1988. *The bonds of love: Psychoanalysis, feminism, and the problem of domination.* New York: Pantheon.

———. 1994. The omnipotent mother: a psychoanalytic study of fantasy and reality. In *Representations of motherhood*, ed. Donna Bassin, Margaret Honey, and Meryle Mahrer Kaplan, pp. 129–146. New Haven, CT: Yale University Press.

Brown, Karen McCarthy. 1991. *Mama lola: A vodou priestess in Brooklyn*. Berkeley, CA: University of California Press.

Chodorow, Nancy. 1978. *The reproduction of mothering: Psychoanalysis and the sociology of gender*. Berkeley, CA: University of California Press.

———. 1989. *Feminism and psychoanalytic theory*. New Haven, CT: Yale University Press.

———. 1995. Gender as personal and cultural construction. *Signs: Journal of Women in Culture and Society* 20(3):516–544.

———. 1999. *The power of feelings: Personal meaning in psychoanalysis, gender and culture*. New Haven, CT: Yale University Press.

Devereux, George. 1967. *From anxiety to method in behavioral sciences*. The Hague: Mouton.

di Leonardo, Micaela. 1991. *Gender at the crossroads of knowledge: Feminist anthropology in the postmodern era*. Berkeley, CA: University of California Press.

Flax, Jane. 1987. Postmodernism and gender relations in feminist theory. *Signs: Journal of Women in Culture and Society* 12(4):621–643.

———. 1990. *Thinking fragments: Psychoanalysis, feminism, and postmodernism in the contemporary West*. Berkeley, CA: University of California Press.

Griffith, Alison and Dorothy Smith. 1987. Constructing cultural knowledge: mothering as discourse. In *Women and education: A Canadian perspective*, ed. Jane Gaskell and Arlene McLaren, pp. 87–103. Calgary, Alberta: Detselig Enterprises.

———. 2005. *Mothering for schooling*. New York: Falmer Press.

Grumet, Madeline. 1988. *Bitter milk: Women and teaching*. Amherst, MA: University of Massachusetts Press.

Harrison, Faye. 1993. Writing against the grain: cultural politics of difference in the work of Alice Walker. In *Women writing culture*, ed. Ruth Behar and Deborah Gordon, pp. 233–245. Berkeley, CA: University of California Press.

Haug, Frigga. 1987. *Female sexualization: A collective work of memory*, translated from German by Erica Carter. London: Verso.

Hunt, Jennifer. 1989. *Psychoanalytic aspects of fieldwork*. Newbury Park, CA: Sage Publications.

Kleinman, Sherryl and Martha Copp. 1993. *Emotions and fieldwork*. Newbury Park, CA: Sage Publications.

Linde, Charlotte. 1993. *Life stories: The creation of coherence*. Oxford, England: Oxford University Press.

Luttrell, Wendy. 1989. Working-class women's ways of knowing: effects of gender, race and class. *Sociology of Education* 62(1):33–46.

———. 1997. *Schoolsmart and motherwise: Working-class women's identity and schooling*. New York: Routledge.

———. 2000 "Good Enough" methods for ethnographic research. *Harvard Educational Review* 70(4):499–523.

———. 2003. *Pregnant bodies, fertile minds: Gender, race and the schooling of pregnant teens*. New York: Routledge.

Mathner, Natasha and Andrea Doucet. 1997. Reflections on a voice-centered relational method: analysing maternal and domestic voices. In *Feminist dilemmas in qualitative research: Public knowledge and private lives*, ed. Jane Ribbens and Rosalind Edwards, pp. 119–146. London: Sage Publications.

McLaren, Arlene. 1985. *Ambitions and realizations: Women in adult education*. London: Peter Owen.

Mishler, Elliot. 1986. *Research interviewing: Context and narrative*. Cambridge, MA: Harvard University Press.

Mitchell, Juliet. 1984. *The longest revolution: Essays in feminism, literature and psychoanalysis*. London: Virago.

Peacock, James and Dorothy Holland. 1993. The narrated self: Life stories in process. *Ethos* 21(4):367–383.

Phillips, Adam. 1998. *The beast in the nursery*. London: Faber and Faber.

Quinn, Naomi. 2000. The divergent case of cultural anthropology. In *Primate encounters: Models of science, gender and society*, ed. Shirley Strum and Linda Fedigan, pp. 223–242. Chicago, IL: University of Chicago Press.

Riessman, Catherine Kohler. 1993. *Narrative analysis*. Newbury Park, CA: Sage Publications.

Rosenwald, George C. and Richard L. Ochenberg. 1992. *Storied lives: The cultural politics of self-understanding*. New Haven, CT: Yale University Press.

Sennett, Richard and Jonathan Cobb. 1972. *The hidden injuries of class*. New York: W. W. Norton and Co.

Shaw, Jenny. 1995. *Education, gender and anxiety*. London: Taylor and Francis.

Steedman, Carolyn. 1986. *Landscape for a good woman*. New Brunswick, NJ: Rutgers University Press.

Thompson, E. P. 1963. *The making of the English working-class*. New York: Vintage Books.

Thorne, Barrie. 1993. *Gender play: Girls and boys in school*. New Brunswick, NJ: Rutgers University Press.

Weinstein, R. M. 1980. Vocabularies of motive for illicit drug use: an application of the accounts framework. *Sociological Quarterly* 21:577–593.

Willis, Paul. 1977. *Learning to labour: How working-class kids get working-class jobs*. Westmead, England: Saxon House, Teakfield.

Winnicott, D. W. 1965. *The maturational processes and the facilitating environment*. New York: International Universities Press.

Index

abstract, 165, 172, 173, 175, 176, 179, 185, 190, 191, 195
accounting strategies, 252–3
agency/agentivity, 22, 159, 161, 178, 181, 222, 244, 244n4
  suppression of, 173, 175, 181
ambivalence, 25, 223, 224, 227, 230, 231, 231n24
anonymity, guarantees of, 90
antagonist, 114, 117, 122, 123, 125, 126, 128, 129, 131, 137, 141, 145, 146, 147
argumentative discourse, 8, 160, 186, 190, 194
associations, 17–18, 109n5, 206, 216n11, 220, 236, 260, 262, 266
  associative links, 209
  emotional/motivational hot spots, 18, 209, 214, 219–21, 239, 240
  free, 17
  intruding, 17, 260
  self-relevant vs. non-self-relevant, 209, 213, 218–19, 220–1
  shared, 28, 220
  strong vs. weak, 209, 212, 218
associative nets, 83
autobiography, 30, 248

Bakhtin, Mikhail, 6, 209, 232
Bayes' theorem, 88, 100–2
Behar, Ruth, 30, 243n1, 248n11, 255, 264
beliefs, 10, 15, 19, 24, 25, 26, 27, 29, 30, 32, 63, 77n36, 87, 93, 95, 98, 99, 132, 139, 148, 152, 203–4, 231, 231n24, 251
  cultural, 32, 220, 244, 245, 249
Bellah, Robert, 228, 228n19, 229
Benedict, Ruth, 90
Bialostok, Steven, 40, 56n21, 79
bias, 2n1, 17, 18n9, 20, 94, 150, 195
Bourdieu, Pierre, 6, 204, 233, 233n27
Briggs, Charles, 7–9, 13, 17, 18, 161, 180n11, 182, 192
Brown, Karen McCarthy, 32, 266

Capps, Lisa, 14, 15, 44n11, 158n2, 160, 161, 182
cargo, 108, 133
causality, 62n29, 65, 67–9, 71, 120, 122
  causal chain, 53n18, 67, 96
  causal contingencies, 87
  causal link, 66, 67, 69, 71, 75
  causal relation, 61n26, 64, 66, 67, 68, 69, 76, 87
  causal sequence, 69
Chinook/Chinookan, 105n2, 111, 159n4
chronicle, 27n14, 247n9
chunking/chunks, 83–4, 83n2, 84n3
Clark, Andy, 45–6
classification, 47–8, 51, 54, 55, 56, 86, 249
clauses
  coda, 182
  conditional, 176
  descriptive, 162, 174
  embedded, 177, 238
  evaluative, 165–6
  event, 162, 163, 164, 171, 172, 173, 175
  flashed, 162–3
  main-line, 171, 172, 173
  narrative, 157, 162, 165, 170, 171, 172, 174, 176, 182, 191
  negative, 166
  progressive, 172

clauses—*continued*
relative, 173, 176, 177
stative, 162, 172
subordinate, 175, 181
temporally-ordered, 162
cluster, 24, 25, 28, 108, 220
analysis, 108
coda, 8, 63, 146, 174, 175, 176, 182, 186, 190, 193, 195
cognitive anthropology, *see* psychological anthropology
cognitive science, 35, 38, 60, 141
cognitive structure, 83, 83n2, 89, 98, 108n5, 109n5, 141
analysis, 108n5
coherence, 27, 30, 42, 157, 160, 167, 169–70, 169n9, 179, 186, 192, 196, 222, 247, 248, 250
presumption of, 169
common sense, 98, 250–1, 252, 253, 256, 259
communicative hegemony, 13
compartmentalization, 22, 25, 223–7, 223n15, 230, 231, 231n24
horizontal, 223n15
vertical, 223n15
complicating action, 172, 173–4, 176, 181, 182, 191
connotation, 203, 206, 206n6
consent form, 162, 197–8
constituents, 115, 117
beginning, 115n6, 117, 120
development, 115n6, 117, 120
ending, 117, 120
contexts of discovery, 32, 85, 86, 89
contexts of justification, 85
contexts of verification, 85, 87, 89
contextualization cue, 158, 166
contiguity, 208, 209, 223
conversational exchange, 5, 27n14
conversational habits, 58, 90
conversational turns, *see* turn-taking
coraje, 121, 145, 146
corpus, 2, 10, 24, 33, 41, 43, 49, 60, 71, 113, 150
countertransference, 17, 27n14, 261, 261n31
cross-cultural research, 29–31
cultural analysis, 1–7, 10–11, 13, 15–16, 18–19, 22, 26, 27, 29, 31, 33, 44n11, 135, 159, 163, 167, 180, 183, 203, 220, 222, 232, 262n32
cultural assumption, 8, 10, 16–17, 204–5, 207, 208
cultural cognitive complexes, 89, 98
cultural imaginaries, 203
cultural models, 2n2, 3, 15, 22–3, 25, 40, 47, 61, 79, 87, 89, 99, 100, 105–7, 112–13, 115, 121, 126, 132, 133, 134, 139, 142, 150, 151, 152, 203, 204–21, 223, 223n25, 233
analysis, 207, 208
cultural patterns, 65, 231, 255
cultural penetrations, 244, 251n17, 262n33
cultural presupposition, 14–15, 17, 23, 28
cultural schemas, *see* schemas
cultural standing, 19, 22, 24, 25, 32, 204, 232, 232n26, 233, 233n27, 235, 236, 238, 239, 240
cultural story, 60, 61, 62, 87, 120
cultural studies, 1
cultural understandings, 1, 3–4, 7–9, 13, 19, 22, 27, 38, 38n5, 40, 41, 42, 45, 47, 79, 96, 106, 152, 203; *see also* shared understandings
culture-laden, 44, 49, 61, 72

denotation, 206, 206n6
dimensions of organization, 167
disciplinary boundaries, 1, 15, 35, 38, 107, 112, 158n2
discourse, definition of, 4–5
discourse communities, 157, 163, 166, 167, 174, 196
discourse constraints, 158, 159
disfluencies, 25, 178, 185, 186, 187, 190, 194, 195, 228
false starts, 25, 178, 194, 231
hesitations, 25, 31, 194, 235, 236, 239
incoherencies, 157, 169
pauses, 25, 91, 157, 170, 179, 194, 205n3, 228, 231, 234, 235, 239–40
schema boundaries, 228
silences, 157, 158, 196

dispreferred second, 235
domain
  of culture, 14, 22, 45, 99
  target, 48, 54, 56
dramatis personae, 111, 122, 136
dukha, 13
Durkheim, Émile, 97, 98

education, 12, 92, 94, 96, 97, 212, 214, 217, 220, 244–5, 247, 250, 252, 252n19, 253, 256, 265
Ehrenreich, Barbara, 238–9
embedded clauses, 238
embedding, 8, 13, 16, 22, 27, 37, 39, 42, 45, 53, 63, 158n3, 160, 173, 176, 177, 186, 187, 190, 191, 195, 245, 255, 256–7, 259
emotional tone, 18–19, 209, 220, 222, 228
emplotment, 15
enactment, 21–2, 115, 117, 134n8, 159, 160, 180
enduringness, 24, 30
entextualized discourse, 11–12, 159n4, 160, 160n5
episodes, 112, 114, 115, 115n6, 117, 120, 143, 145, 151, 158n3, 160, 165, 175, 179
  embedded, 114, 117
  of mediation, 143
epistemology, 249
  feminist, 254
  of social science, 263
ethnopsychology, 207–8, 221; *see also* folk psychology
ethnoscience/ethnosemantics, 38
evaluation, 13, 44, 106, 108, 114, 133, 137, 152, 163, 165–6, 175, 176–8, 179, 181, 182, 183, 208
explanation, 8, 27, 61n26, 63–4, 74, 247n9
expository discourse, 8, 9, 11, 27–9; *see also* interview
extension, 206n6

Fairclough, Norman, 5, 5n4, 222, 233, 238
Farrer, Claire R., 105
fatherhood/fathering, *see* parenting
femininity, 251, 253, 257–8
feminism, 243, 243n1, 244n3, 254–5, 255n27, 263n36, 265
  feminist anthropology, 254, 254n25, 255
  feminist ethnography, 249n13
  feminist relational psychoanalysis, 6, 244n3, 259, 261–2
Fillmore, Charles, 37–8, 37n4
flashing (back or forward), 162–3, 165, 166, 176
folk psychology, 52, 67, 100, 207, 222, 228; *see also* ethnopsychology
folktale, 2n3, 3, 6, 11, 15–16, 19, 23, 26, 27, 28, 30, 105–52, 157
Foucault, Michel, 5, 222
frequency, 2n3, 92, 98, 212, 259, 265
Freud, Sigmund, 77, 77n37, 78n40, 222
functional analysis, 125, 143
functional assemblies, 37n3, 38

gender
  models of, 105, 106, 107, 132, 134, 135, 141, 142, 150
  roles, 120n7, 122, 126, 135, 150, 181, 204
genre, 3, 5, 8, 12, 13, 15, 16, 17, 30, 105, 105n2, 110–11, 113, 121, 137, 148, 149, 150, 152, 157, 158, 159, 162, 171, 178, 194, 196, 251, 256
  constraints of, 110, 158, 159–60, 179
gestalt, 83
gist propositions, *see* propositions
Goffman, Erving, 186n14, 233, 235n29
grammatical analysis, 132; *see also* story grammar
grammatical categories, 84, 84n4
Grice, Paul, 238n34
grouping, 83, 93

Harrison, Faye, 2n1, 243n1
hedges, 233, 235
high concordance code, 99
honor, 23, 139
Horowitz, David, 236n31

Hutchins, Edwin, 45, 46, 61n26, 87, 173, 233n27
Hymes, Dell, 105, 105n2, 110–11, 149–50, 152, 159n4, 170–1

identity, 15, 24–6, 28, 59, 208, 213, 214, 218, 219, 220, 244, 247, 248, 253, 254, 255n27, 256, 258, 261, 262, 263, 263n34, 363n38
ideology, 5–6, 203, 221, 221n13, 222
  dominant, 108, 151–2
  gender, 105, 108, 135, 148, 149
  public, 135, 148, 149
indexing, 8, 16, 44n11, 185, 190
individualism
  American, 245, 245n5
  hard vs. soft, 228n18
instrumental analysis, 158, 161, 179
integration, 22, 30, 169, 223, 224, 228, 229, 230, 230n21, 231
intension, 206n6
internalization, 3, 46, 47, 84, 203, 222, 223, 223n15, 225, 232, 239
intertextuality, 222
interview, 1, 3, 7–14, 9n5, 16–19, 20, 29, 31, 40–3, 44, 45n12, 89–91, 106n3, 109n5, 132, 134, 161, 180, 183, 187, 190, 191, 195, 197, 203, 204, 239, 246, 255
  data, 12
  focus group, 10–11, 90
  in-depth, 106n3, 109n5, 134, 204
  interviewee-interviewer relationship, 17, 24, 26
  semi-structured, 90, 204, 212, 245
involvement, 182, 191

jargon, 221, 223, 224, 227
Johnson, Mark, 60, 60n25

Kataoka, Kuniyoshi, 167–8, 168n8, 170, 171, 173–4, 177, 178, 181, 184, 186
key units, 114, 115, 150
key word/keyword, 18, 36, 42, 44, 45, 47, 71–2, 76, 78, 192, 193, 204–7, 209–10, 210n9, 212, 221, 229
  analysis, 36, 71–9, 205
Kohli, Marti, 30
Kuna, 163, 166

Labov, William, 6, 161, 162, 163, 166, 167, 171–6, 178, 180, 180n12, 181, 182, 184, 191, 194
Lakoff, George, 6, 60, 60n25, 61, 121
La Llorona, 3n3, 13, 26, 27, 28, 105–52; *see also* folktale
lamination, 233, 234, 235n29, 239
layering, 95, 233–4
Lévi-Strauss, Claude, 6, 107, 111, 135–6, 143, 147, 151
lexical (contrast) set, 37, 39
life experience, 210, 247
life-focused approach, 248, 265
life story/life history, 6, 12–13, 27–31, 27n14, 40, 42–3, 106n3, 158n2, 160, 175, 204, 213, 234, 247–9, 247n9, 247n10, 248n11, 250n15, 252, 252n15, 253, 254n24, 256, 260, 260n30, 262, 264, 266
  analysis, 244, 248, 249, 266
Linde, Charlotte, 6–8, 9n5, 12, 27, 27n14, 29–31, 40n8, 42, 43, 61n26, 63, 160, 169, 176, 180, 194, 222, 245n5, 247, 247n9, 252, 252n18
linearity of language, 174, 175
linguistic anthropology, 6, 14–16, 157, 159, 180, 194
linguistic discourse model, 179; *see also* Polanyi, Lydia
linguistics, 19, 35, 84, 85, 87, 110, 167
listening
  psychoanalytic, 17–18, 25–6, 248, 260, 263
  strategies, 265–6
literacy, 40, 41n9, 254, 255
Longacre, Robert A., 162, 171, 178, 181, 182, 191

main line, 162–3, 166, 170–4, 176, 181, 182, 186, 193
Maranda, Elli Kongas, 111, 113, 136–7, 143, 149
  four models of, 136–7, 143, 149
Maranda, Pierre, 111, 113, 136–7, 143, 149
  four models of, 136–7, 143, 149
marriage
  arranged, 77n36, 106n3, 134n8

models of, 15, 23, 25, 47, 61, 71, 79, 86–7, 100, 115, 122, 132–3, 134, 135, 137n9, 141, 142, 150
Marx, Karl, 97, 98, 258
masculinity, 23, 244, 244n3, 262n32
meaning
cognitive, 221
cultural, 2, 3, 4, 5–6, 18, 21, 23, 27, 35, 38, 39, 45, 45n12, 79, 149, 196, 255, 262n32
emotional, 221, 259
linguistic, 35
mediating structures, 45–6
mediation, 107, 111, 135–45, 146, 147–9, 151–2
memory, 19, 83–4, 83n2, 112
affect-laden, 209, 213, 214, 219, 260
childhood, 214
literacy-related, 40, 41n9
school, 251, 261, 261n31
memory work, 265
mestizos, 106n3
meta-communicative norms, 13
metaphor
analysis, 36, 49–59, 60n25, 61–2, 65, 72, 76, 77, 78, 86
classes, 47–8, 49n17, 50–4, 53n18, 53n19, 55, 56, 57, 59, 60, 60n25, 65, 78
conventional, 56, 58, 60n25, 76
of visibility, 258
methods
correlational, 85, 108
of discovery, 85, 89
eclectic, 6, 32, 107, 150, 152
ethics of, 161–2
experimental, 36n2, 85, 87
good enough, 244, 264
humanistic, 1
mechanistic, 32, 35
opportunistic, 6, 15, 36, 43, 47, 79
organic, 35–6, 43
scientific, 1
structural, 107, 111, 113, 114, 122, 135–6, 151
systematic, 1–2, 2n1, 32, 37, 45, 79, 105, 107, 122, 151, 152
of verification, 85
minimal narrative, 162, 191
models
basic building blocks of, 92
cognitive, 87, 121
see also cultural models
Moffatt, Michael, 71–2
morality tales, 15, 27, 106, 108, 113–14, 117, 121, 125–6, 137, 149, 151
motherhood/mothering, *see* parenting
motivation, 3, 5–6, 18, 21, 23, 24–6, 27n14, 47, 64, 72, 74–5, 77–9, 78n38, 78n39, 93, 95, 96, 97, 106, 112, 114, 120, 120n7, 121–2, 126, 129, 131, 132, 137, 150, 208, 209, 214, 219
multidimensional scaling, 108n5

narrative
accounts, 247–8, 249, 252, 252n19, 253
confessions, 252, 252n18
conversational, 14, 15, 27, 32, 44n11, 158, 161, 180n11
credentialing, 252, 253
defensive stories, 252
dialogic dimensions of, 27, 180, 185
disclaimers, 252, 253
entextualized, 11, 159n4, 160, 160n5
of the law, 160
personal, 234, 239, 247, 249
school, 30, 250, 252, 253, 258
socially occurring, 9, 9n5, 161, 167, 183
narrative competence, 12, 157–8
narrative evaluation, 208
narrative icon, 157, 162, 163, 165, 166, 170, 179
narrative privilege, 8–9, 183
narrative urgency, 12, 25, 247
notational systems, *see* transcription
notes on notes, 246n8
Nuckolls, Charles, 26, 78n38

Oaxaca, 11, 13, 26, 27, 106n3
Ochs, Elinor, 14–15, 18, 18n9, 44n11, 158n2, 160, 161, 182

opinion, 8, 18, 22, 24, 27, 96, 194, 195, 220, 235
  common, 8, 22, 24, 194, 203, 232–3, 234, 235, 236–8, 237n33, 239
  community of, 232, 236
  controversial, 8, 24, 204, 232, 233–5, 236, 239
  debatable, 8, 232, 233, 235–6, 236n31, 237, 239
  moral, 120n7
  public, 203, 232
  statements of, 239
opposition, 107, 111, 114, 122, 135–7, 141, 142, 143, 145, 146, 147, 148, 149, 151
  binary, 136, 141
orientation, 172, 175, 176, 177, 181, 191
overlay, 163–4, 166

parenting, 210, 212, 213, 214, 218, 220, 221
  fatherhood/fathering, 133, 143, 219
  motherhood/mothering, 23–4, 25, 132, 134n8, 143, 180–1, 218–20, 227, 245, 246, 250, 260–1, 262, 262n32, 264
partonomies, 83, 83n1, 86
pattern number, 167, 170–1, 179
pena, 109, 121, 145, 147
personal semantic networks (PSN), 10, 18, 22, 23, 28, 208, 208n8, 209, 214, 220, 223, 224, 231, 239, 240
  analysis, 28, 208, 209, 220–1, 231, 239, 240
phraseology, 221–2, 223–4, 227
plot, 15, 111, 113, 117, 120, 122, 126, 137, 141, 178, 181, 182, 183, 191
  moves, 113, 114, 115, 119, 122, 132, 150
  trajectory of, 181, 182
  units, 107, 113, 114, 122
Polanyi, Lydia, 162, 179
  linguistic discourse model, 179
power, 5–6, 96, 133–4, 134n8, 146, 148, 169, 182–3, 203–4, 221n13, 232, 264
  black, 5, 227
  political, 134n8, 218
  relations of, 5, 243
pragmatics, 121
pregnancy, 210, 211, 244, 247n10, 250, 263n38
presupposition, 4, 7, 14–15, 16–17, 23, 28, 29, 95, 110, 157–8, 167, 184, 238
presupposition triggers, 238; *see also* cultural presupposition
propositions, 8, 19, 27, 61n26, 63–4, 85n7, 87, 89, 91–3, 94–5, 96, 98, 99, 117–19, 233, 235, 236, 238
  gist, 2n3, 28, 91
  grouping of, 93
Propp, Vladimir, 6, 111, 113, 114, 122, 135
protagonist, 114, 117, 122, 123, 125, 126, 128, 129, 131, 132, 137, 141, 145, 146, 234
prototype worlds, 37–8, 37n4, 38n5
PSN, *see* personal semantic networks
psychic conflict, 23, 24, 30, 223, 259
psychoanalysis, 6, 21, 24–6, 110, 194, 244n3, 248, 259, 260, 260n30, 261, 263n36, 264, 266
psychoanalytic anthropology, *see* psychological anthropology
psychological anthropology, 3, 4, 14, 15, 16, 46, 255n27
  cognitive anthropology, 3–4, 25, 35, 38, 39, 78n38, 107, 112, 207
  psychoanalytic anthropology, 3, 26, 78n38

qualitative analysis, 240
quantification, 2, 2n2, 89

race, 94, 187, 204, 234, 236n31, 244, 246, 246n7, 254, 255, 256, 257, 258–9, 262
realism, 243n1, 249
reasoning, 3, 16, 23, 36, 42, 44–6, 47, 49, 53n18, 59–71, 61n26, 62n28, 72, 74, 75–6, 78, 87, 98, 160
  fragmentary, 64–5
  overlapping, 66–7

recording, 3, 18–21, 24, 40, 43, 44n11, 90–1, 161, 168n8, 179, 197, 233, 238, 246, 260, 261, 246n8
digital, 161
objections to the use of, 19–21, 197
video, 20, 161, 168n8
reflexivity, 244, 249, 264
Reichenbach, Hans, 85
repairs, 73, 90, 239
reportability, 73, 176, 180, 181, 182–3, 190
norms of, 9, 183
reported speech, 166, 182, 186, 186n14, 191, 192
direct discourse, 191
result, 174, 191
rewrite rules, 114–17
rhetorical argument, 236n32
rhetorical structure components, 178–9, 181
Riessman, Catherine, 18, 265n40
Rosaldo, Renato, 29

salience, 46, 49, 51, 72, 183, 221, 226, 245, 246, 247, 253
sampling, 2, 11, 19, 28, 98, 106, 106n3, 108, 180, 204, 206n7, 245, 246, 246n6
sample size, 2, 98–100
scaffolding, 45
scenario, 109n5, 115n6, 117, 120, 125, 126, 181
scenes, 37n3, 38, 43, 171
schemas, 3–4, 15, 22, 38, 45, 57, 63, 67, 71, 74, 76, 77–8, 79, 107, 109n5, 112, 113, 114, 115, 119, 120, 120n7, 121, 132, 141, 149, 150, 152, 208, 213, 216n11, 221, 223n15, 224, 224n16, 227, 228, 235
cultural, 3–4, 15, 22, 38, 39, 40, 42, 44, 51, 52, 56, 57, 60, 61, 62n28, 62n29, 64, 66, 67, 72, 78n38, 112, 113, 115, 120, 121, 150, 152
emotion, 121
mental/cognitive, 112, 149–50, 203, 223
shared, 44, 45, 48, 50, 71, 72, 79, 112, 113, 115, 119, 120, 152, 203
schema theory, 3–4, 38–9, 79, 114, 120
schematic analysis, 117, 121, 125, 126, 132, 134, 151
Schiffrin, Deborah, 8, 223n25, 233n28, 236n32, 237
school-smart, 244, 250, 251, 253, 256, 259
scripts, 38n5, 83, 263n37
self, 12, 24, 28–30, 120, 132, 209, 228, 234, 247, 248, 251, 252n18, 253, 254, 255n27, 256, 257, 258, 259, 261, 262, 262n32, 263, 264, 266
self-image, 28, 209, 213, 214, 218–19, 220, 229, 230
semantics, 121, 207
setting, 115, 115n6, 117, 120, 125, 126, 179
sexuality, 23, 71, 77n37, 108, 129, 133, 248n11
shared expectations, 48–9, 74
shared experience, 3, 38, 79
shared understandings, 3–4, 8, 22–4, 39–40, 45, 46–7, 159, 170, 193
stability of, 39, 43, 47
see also cultural understandings
Skinner, Debra, 13, 13n7, 108n5
Skolnick, Arlene, 30
Smith, Adam, 97
Utilitarian model of, 97, 98
social discourse, 5, 5n4, 9n5, 203, 221–32, 221n13, 222n14, 225n17, 233, 236, 239, 240
analysis, 222, 222n14
social equality/inequality, 22, 26, 27, 28, 90, 91, 93, 94, 95, 96, 97, 243
socially occurring discourse, 9, 9n5, 161, 167, 183
sociolinguistics, 5, 6, 15, 16, 42, 157, 161, 180, 180n12, 208, 247
speech community, 11, 161, 180n12, 232
splitting, 259, 262, 263n39
spontaneous discourse (spontaneously occurring discourse/talk), 3, 9–12, 9n5, 11n6, 14, 14n8, 15, 28, 41, 44n11

stanzas, 171
sterilization, 218, 220, 221, 237
story focused approach, 248, 266
story grammar, 83, 107, 113–15, 120, 121
storytelling, 14, 105n1, 139n9, 247, 248n10
stress (word), 177, 205
structural margin (of narrative), 173, 175, 185
structure, 22–3, 28
  analysis, 105n1, 111, 114, 122, 136, 141
  cognitive, 83, 83n2, 89, 98, 108–9n5, 141
  narrative, 27, 126, 158n3, 161, 167, 168, 171–2, 178–9, 195–6
  of plot, 15, 106–7, 111, 182
  serially organized, 84
  shared knowledge, 95
subculture, 5, 24, 29, 38, 42, 167, 203, 208, 221, 231
subjectivity, 1, 5, 261, 263
suicide, 26, 126, 128, 129, 131–2, 134, 134n8, 135, 137, 139, 141–2, 143, 146, 146n10, 147, 148, 150, 151
survey, 7, 85, 99
  telephone, 18, 204
suspense, 181, 182–3
  trajectory of, 181, 182
syntax, 32, 56, 58, 121, 164, 173, 175, 176, 179, 180n12, 194
systematicity, 1–2, 2n1, 32, 37, 45, 60, 60n25, 72, 79, 105, 107, 110, 122, 134, 151, 152, 169–70, 179

Taggart, James M., 105, 105n1, 135, 137n9, 146n10
taken for granted, 3, 17, 19, 22, 76, 96, 109n5, 180–1, 203, 208, 232–3, 235, 238, 247, 247n10
taxonomies, 83, 83n1, 86, 176
themes, 15, 18, 23, 24, 28, 32, 49n17, 52, 105n1, 134n8, 192, 194, 218, 223, 226, 250, 256, 257, 258
Tohono O'odham, 14n8, 158, 163–4, 179
tone of voice, 18–19, 162–3, 168n15, 179, 209, 220, 222, 227, 228
transcription, 2, 3, 10, 18–19, 20–1, 25, 91, 239–40
  conventions, 18, 161, 168n8, 196–7, 205n3
  ethics of, 161
  notational systems, 18, 161
  politics of, 161
  web storage, 161
transference, 27n14, 261, 261n31
transgression, 117, 123, 126, 128, 129, 131, 132, 135, 137, 139, 141, 142, 146, 148
transitions, 190, 192
transitivity, 167, 171, 173, 181
tristeza, 121
turn-taking, 10, 239

unconscious compromise, 223n15
Uto-Aztecan, 163

values, 13, 27, 70, 108, 132–3, 145–6, 206, 207, 214, 217–18, 228–9, 228n19, 244, 249, 250, 251, 253, 265
variation, 16, 21–3, 27, 31, 32, 65, 74, 105n2, 106, 108n4, 110, 111, 113, 114, 122, 123, 126, 128–9, 134, 135, 136, 149, 150, 151, 166, 204, 255, 256, 257, 258
  cross-cultural, 208
  cultural/autobiographical, 30–1
  dialect, 14n8, 164
  idiosyncratic, 126
  intra-cultural, 111, 204, 208
  sub-cultural, 221
Vélez Ibañez, Carlos, 192, 193, 194
verbal molecules, 221, 237
video, *see* recording
voice, 5, 9, 19, 20–1, 30, 32, 74, 163, 169, 179, 183, 187n15, 195, 209, 213, 220–1, 222, 225n17, 227, 228, 228n18, 232, 234, 237, 238, 243, 253n21, 258
  analysis, 222

welfare, 5, 15, 23, 24, 26, 29, 204, 205, 205n4, 206, 207, 209, 210, 212, 213, 214, 217, 218, 220, 221, 221n12, 224, 225, 226, 230n22, 237, 238
Wierzbicka, Anna, 83, 89, 232n26
winnowing, 2n3, 86, 91, 249
Woodbury, Anthony C., 171, 178–9, 181
work ethic, 206, 206n7, 217

Zamosc, Leon, 99–100
Zapotec, 106n3, 148
zooming (in or out), 32, 181–2, 191