EXTREME MAKEOVER:
THE ETHNOGRAPHIC EDITION

2  Introduction
   Dianna J. Shandy and Jon Poehlman

4  Commentary: The History and Purpose of Methods Camp
   H. Russell Bernard

6  Tibetan Identity and the Cultural Consensus Model
   Amy Mountcastle

12 Masculine Identity and HIV/AIDS Risk Behavior among African-American Men
    Jon Poehlman

18 Measuring Married Women “Opting Out”
    Dianna J. Shandy and Karine S. Moe

22 Using NVivo with Grounded Theory and other Qualitative Methods
    Elizabeth England Kennedy

27 Thinking Problem: Rethinking Ethnographic Methods in Relation to a Study of Students’ Cultural Models of Drinking
    Robert Philen

31 Students as Subjects and Scholars: QualQuant Tools in the Classroom
    Emily Stovel

36 Using Cultural Consensus Analysis to Improve a Safety Campaign for Farmworkers
    Paul Monaghan

40 Reflections on Teaching SCRM: Survey Research Methods in Anthropology
    William W. Dressler and Kathryn S. Oths

FOUR-FIELD APPLICATION

42 Applying Anthropology in Information Technology Deployment and Implementation Efforts: Business Processes Redesign in the Israeli Defense Forces
    Yonathan Mizrachi

45 The Making of Ritual Specialists: What Cultural Anthropologists Can Contribute to Educating Funeral Service Practitioners
    Sherlynn Briller and Allison Kabel

50 Stealth Language Teaching: A Preschool Foreign Language Pilot Program
    Anna S. Cohen-Miller

54 Pitfalls in Archaeological Fieldwork: On Gender, Ethics, and Danger
    Carleen D. Sanchez
By Dianna J. Shandy and Jon Poehlman

The Summer 2006 National Science Foundation Short Course in Research Methods (SCRM) is the original impetus behind this special issue of Practicing Anthropology. All of the paper authors were participants in one or more of the SCRM courses that summer. One evening over dinner, we asked ourselves how what we learned in these courses could be used to transform and improve current or past research projects. In other words, what would happen if we subjected our ethnographic research to “an extreme makeover”?

The SCRM short courses offer doctoral level anthropologists an opportunity to deepen and expand their knowledge in key methodological areas. SCRM courses consist of readings, lectures, discussions, as well as practical and technology-based exercises, with much of the learning revolving around the interests and research of the course participants. For instance, in the course on Survey Research Methods in Anthropology, taught by Bill Dressler and Kathy Oths, participants learned about research design, data collection techniques, and methods of statistical analysis. These lessons were grounded in application through on-going discussions of our own research projects.

In many ways, as course participants, we did develop the feelings of relocation that one might experience as a result of “an extreme makeover,” as new perspectives emerged for us in relation to our old research topics as a result of our participation in the course. Out of this reflection and further conversations grew a double panel session at the 67th Annual Meeting of The Society for Applied Anthropology (SfAA), in Tampa Bay, Florida, in March 2007. This session was jointly sponsored by SfAA and the Society for Urban, National, Transnational/Global Anthropology of the American Anthropological Association that was meeting in conjunction with SfAA.

We now continue this conversation in this current issue of Practicing Anthropology as we explore issues in applied anthropology through a focus on the experiences of anthropologists utilizing quantitative and qualitative research methodologies in a variety of research settings. Prefacing the issue, Russ Bernard provides a commentary that outlines the history and purpose of methods camp, the value of methods to anthropology, and what this all means for the professional development of current and future generations of anthropologists. His commentary sets the stage for a series of papers that showcase how past and current ethnographic research projects can be improved through more systematic research design and data analysis. The authors write about actual research that highlights the lessons learned at “methods camp.” The projects, spanning labor force participation, health, identity, education, and urban studies, illustrate the negotiation of qual-quant measurement issues. Cultural domain analysis, cultural consensus analysis, multivariate data analysis, and the use of Atlas.ti, Anthropac, and SPSS are some of the specific techniques discussed in these papers.

Our first paper by Amy Mountcastle uses a qualitative approach to elaborate on the production of a global Tibetan identity grounded in Tibetan cultural values that appeared to interface seamlessly with the transnational discourses of human rights, women’s issues and environmentalism. In this paper, Mountcastle considers how quantitative methods, including cultural consensus modeling, might contribute toward understanding the degree to which the global Tibetan identity resonates with Tibetans’ views, on the ground, of themselves.

Our next paper examines masculine identity and HIV risk behavior among heterosexual African-American men. In HIV prevention research, there is a need for research that contributes to the understanding of how culturally defined meanings of gender and expectations about men’s sexual behaviors influence HIV risk. Recent theory and methods from the field of cognitive anthropology...
may be able to facilitate more systematic investigations of gendered culture. In particular, research involving cultural domain analysis and cultural consensus analysis, together, form a potentially robust investigational technique to examine masculine roles and expectations. Jon Poehlman discusses the use of structured ethnographic techniques to identify concepts of masculine identity among a community sample of African-American men.

Dianna Shandy and Karine Moe, working together as a cultural anthropologist and a labor economist, explore why college-educated, married women with children are opting out of the labor force and the implications of doing so. Their project provides an example of an applied, interdisciplinary collaboration that seeks to inform public debate and to demonstrate how both qualitative and quantitative research can be enriched through combining their complementary strengths. In particular, in building on concepts shared in the short courses, they developed two surveys designed to enhance and expand on the findings from traditional ethnographic research, in the form of interviews and focus groups.

Emily Stovel, in her paper, explores the use of qual-quant methods in teaching and curricular development. She discusses the use of these tools to demonstrate key topics in anthropology such as cultural norms and deviation and to increase student exposure to methods training workshops in the discipline. Her efforts reflect institutional and departmental changes in emphasis toward more hands-on learning experiences and global pedagogy.

Researchers often analyze semantic illness networks (SIN) to better apply emic definitions to data and “tease apart” overlapping emic definitions of terms. Often, lists of relevant terms are intuitively created during ethnographic research. Liz England Kennedy’s analysis of semantic illness networks compares instruments created by an ethnographic team and a sequence of software programs and analytic techniques including Atlas.ti, KWIK, and MS Word using data generated by this team, to create a SIN team instrument.

England Kennedy considers whether a more systematic approach such as this can enhance protocol validity and efficiency and/or affect the number of terms included in instrument creation. Robert Philen’s paper describes his work over the past year, as part of an interdisciplinary research team focusing on public health issues at the University of West Florida, including students’ conceptions and patterns of drinking. As the cultural anthropologist, Philen has been the qualitative foil to quantitative emphasis of other social scientists on the team. After attending the NSF summer seminar on ethnographic survey methods, however, Philen has transitioned to a more pragmatic framework of thinking which de-emphasizes the quantitative-qualitative distinction but emphasizes precision in measurement (whether qual or quant) and interpretation. This paper examines how this transition has modified and enhanced the research on students’ cultural models of drinking.

Our last paper describes the work of Paul Monaghan in his efforts over the past four years evaluating an eye safety program targeted at citrus harvesters in Florida. In this setting, a variety of field methods have informed the development of a social marketing campaign to convince workers to wear safety glasses when they pick. His paper revolves around trying to better understand the reasons for the success of the program. We are fortunate, as part of this issue, to have William Dressler and Kathryn Oths share more about their approach to teaching survey research in anthropology. Their careful consideration of the use of survey research in the service of anthropology offers an original perspective that can inform both our practice and teaching.

In addition to our series concerning the expansion of knowledge in methodological areas, this issue of PA also contains four articles that explore the intricacies of anthropological practice across the subdisciplines of the field. In the first of these, Israeli anthropologist Yoni Mizrachi describes how the anthropological perspective is being fruitfully applied in a relatively recent human endeavor: using information technology to redesign/restructure/reengineer organizational processes, structures and culture. Next, Sherlynn Briller and Allison Kabel take us to another untapped area of practice, discussing why they believe greater teaching collaboration between the fields of cultural anthropology and mortuary science is both possible and timely. Teaching is also the subject of Anna Cohen-Miller’s article, although this time the focus is on language, and creating a foreign language-learning environment on the preschool level. Finally, Carleen D. Sanchez provides a thoughtful examination of the practical and ethical challenges of archeological fieldwork, especially outside one’s home country.
COMMENTARY: THE HISTORY AND PURPOSE OF METHODS CAMP

By H. Russell Bernard

The papers in this section of PA are by colleagues who attended one or more weeks of NSF’s Short Courses in Research Methods (SCRM) program in 2006. The SCRM program is part of a larger, long-term project (popularly known in the discipline as “methods camp”) to help cultural anthropologists develop skills in research design, data collection, and data analysis. I’m constantly delighted at how similar the social sciences all are with regard to the big research questions they ask, like: Why are some people early adopters of innovations? Why do some work groups develop good morale while others go nova? Why has romantic love replaced arranged marriage in some societies, but not in others? What accounts for variations in fertility within a society and across societies?

With the right tools, cultural anthropologists can provide precious, comparative data on all these questions and more. Every basketball coach knows that you can’t teach height but you can teach tall kids the fundamentals of the game. In the social sciences, you can’t teach anyone to go out for a year, risk serious illness, and learn another language just to collect some data. But you can teach the fundamentals of social science. The goal of methods camp is to provide cultural anthropologists—“otherwise sensible people who don’t believe in the germ theory of disease,” as Roy Rappaport (1990) called them—with fundamental skills in data collection and analysis.

Some History

To strengthen anthropologists’ research skills, NSF in the 1950s and 1960s supported a series of field schools in Mexico, Peru, the United States, Ireland, and elsewhere. Many of today’s senior scholars in anthropology were trained in those programs. When the bottom fell out of the academic market in 1971, NSF stopped funding field schools. In 1985, NSF sponsored a conference to assess the state of the art in methods in cultural anthropology. That conference (which Pertti Pelto and I convened) produced a joint article, published in Current Anthropology (Bernard et al. 1986), and a call for training programs in research methods for graduate students and faculty in cultural anthropology.

In 1987, with support from NSF, Pelto and I established the Summer Institute for Research Methods in Cultural Anthropology (SIRM), a three-week, summer training program in research methods for university teachers of cultural anthropology. Lee Sailer joined us for one year, and in 1988 we were joined by Steve Borgatti. Pelto, Borgatti, and I ran the SIRM through 1995. Some 130 colleagues were trained in qualitative and quantitative methods, particularly methods of cognitive anthropology.

This emphasis on cognitive anthropology was largely the result of Borgatti’s (then) new program, Anthropac (Borgatti 1992). That program made it easy to collect and analyze free lists, pile sorts, triad tests, and paired comparisons. These methods have been attractive in anthropology since the 1960s, but the data they produced were difficult to analyze in the era before personal computers. Anthropac supported a renewed interest in the methods of cognitive anthropology (see D’Andrade 1995; DeMunck and Sobo 1998; Handwerker 2001; Ross 2004).

Intuitive software for running statistics, analyzing texts, and processing complex network data have since made it easier to teach and to learn all these methods. Of course, learning to use software is not a substitute for learning the basics of any method (you can’t learn the basics of good writing by learning to use a word processor), but the existence of all the new software has made the collection and analysis of mountains of data (whether numbers or words or images) less intimidating and, I believe, has stimulated interest in research methods among cultural anthropologists.

In 1991, Carol Ember, Michael Burton, and Robert Munroe established a three-week summer program on systematic cross-cultural and comparative research. That program, also funded by NSF, ran for six years, training 72 faculty members, along with several post-docs and graduate students in anthropology.

In the early 1990s, as a panelist for dissertation grants in the cultural anthropology program at NSF, Jeffrey Johnson identified training in research design as a priority for graduate students. Johnson founded the Summer Institute for Research Design in Cultural Anthropology (SIRD) in 1996 to help graduate students who are preparing proposals for field research. He continues to direct the SIRD, with Susan Weller and me as co-directors. The focus of the SIRD is on integrating the objectives, theory, and methods for research into a solid grant proposal. Nearly 200 graduate students have been through the SIRD over the last 12 years.

In the late 1990s, panelists for senior grants in the cultural anthropology program at NSF identified training in research methods as a priority. In 2003, NSF held a Planning Conference for NSF Summer Workshops on Research Methods in Cultural Anthropology at the Belmont Conference Center in Elkridge, MD. Two programs in methods training came out of that conference: the Summer Field Training in Methods of Data Collection in Cultural Anthropology (SFTM) and the Short Courses on Research Methods in Cultural Anthropology (the SCRM).
The SFTM involves graduate students directly in collecting and analyzing data collected in the field on major research projects. In 2004, 2005 and 2006, there were two SFTM field schools: one in the Bolivian Amazon, among Tsimane’ Amerindians, and one in Zambia, among Chitonga-speaking peoples of Southern Province. The program in Zambia was run by Lisa Cliggett, with participation by Deborah Crooks. The Bolivian program is run by a team including Ricardo Godoy (the program director), Victoria Reyes-García, Clarence Gravlee, J. Richard Stepp, William Leonard, Thomas McDade, and Susan Tanner and is scheduled through 2009.

Among the suggestions at the planning conference was that workshops on specific methods be developed and managed by a committee of colleagues who have experience in providing training in research methods. The proposal for the SCRM was in response to that suggestion. The board of directors for the SCRM program includes Jean Ensminger, Eric Smith, Carmella Moore, Susan Weller, and Jeffrey Johnson.

The SCRM offers three five-day courses each summer. During the first three years (2005–2007), the SCRM offered four different courses two times each: text analysis (taught by Gery Ryan and Clarence Gravlee), survey research (William Dressler and Kathryn Oths), direct behavioral observation (Raymond Hames and Michael Paolisso), and methods of ethnecology (Gary Martin and J. Richard Stepp). In 2008, the SCRM will offer text analysis again (this time taught by Clarence Gravlee and Amber Wutich) and will add two new courses: one on network analysis (taught by Christopher McCarty and another on systematic techniques for gathering and analyzing video data (Elizabeth Cartwright and Jerome Crowder). Information on the SCRM courses is on the Methods Mall at http://www.qualquant.net/training/.

Finally, beginning in 2007, the SCRM began offering one-day workshops at the annual meetings of the AAA and the SFSA. These workshops are on the use of various kinds of software (for text analysis, for statistics, and for network analysis) and on principles of research design.

Do Cultural Anthropologists Need Their Own Methods Courses?

One might legitimately ask whether cultural anthropologists need their own methods courses. After all, courses on statistics, questionnaire design, probability sampling, database management, and statistical data analysis are offered in departments of statistics, psychology, education, political science, and sociology. Unfortunately, students of cultural anthropology are not encouraged to invest their time in learning systematic methods of research (Cohen 2003; Plattner 1989), and may even be actively discouraged from doing so.

Cultural anthropologists could also attend short courses on research methods that are available in the United States (at the University of Michigan: http://www.isr.umich.edu/src/si/courses.html—now in its 60th year), in England (at the Essex Summer School in Social Science Data Analysis and Collection: http://www.essex.ac.uk/methods/*—in its 40th year) and, as of 2006, in Croatia (at the University of Ljubljana: http://www.essex.ac.uk/ecpr/events/summerschools/ljubljana/index.aspx).

These programs offer courses (open to graduate students and faculty alike) in both qualitative and quantitative methods and are well known in the social sciences. Students and faculty in cultural anthropology rarely take advantage of these opportunities. Obviously, participants in the SCRM are willing to devote time and energy to learning more about research methods. In talking to participants, I find that many have actually been looking for methods courses taught by people who understand the exigencies of fieldwork—that is, other anthropologists. Disciplinary homophily, it turns out, is still an important consideration in the decision to learn more about methods.

The SCRM offers anthropologists training in the methods that are most useful to them. By the end of 2008, with six different courses in place, the SCRM will have many pieces of a methods curriculum. Every course has a web site with a detailed syllabus and all the readings (in full text) for the course. Our goal is to make these sites available to support all who teach these courses.

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TI BETAN IDENTITY AND THE CULTURAL CONSENSUS MODEL

By Amy Mountcastle

Anthropologists have typically been polarized in their preferred methods of data collection and analysis. Quantitative research is characterized by its detractors as being reductionistic and positivist, while qualitative research is viewed as being non-scientific because of issues of reliability and validity. In the Survey Methods course at SCRM, participants were primarily from qualitative backgrounds. It quickly became apparent that we represented a small, but perhaps growing, subgroup of ethnographic researchers who are interested, for a variety of reasons, in bridging this qual-quant divide.

Pioneers in this endeavor, among them our course instructors William Dressler (2005a,b) and Kathryn Oths (2001, 1994), have strong quantitative backgrounds and have applied mixed methodologies primarily in the area of health. The goal is to incorporate local meanings into quantitative tests of data reliability and to maintain culturally meaningful categories in devising quantitative instruments. In my area of study, identity, however, the why and the how of incorporating quantitative methods are less obvious. In this article, I explore some ideas about why it might be worthwhile for qualitative researchers of identity to consider applying quantitative methods, in particular, the cultural consensus model and cultural competence and consonance analyses. I first examine some of the limitations of the interpretive method and then I explore in hypothetical terms, how I might construct a model of cultural consensus from my data on Tibetans in exile. I conclude with some ideas about how the consensus model and measures of cultural consonance could enhance studies of identity.

One of the striking aspects of Clifford Geertz’s essay, “Deep Play: Notes on the Balinese Cockfight” (1973a), perhaps the quintessential elaboration of the interpretive method, is the authority that the author assumes in the narration. Given that the interpretive method stands in contrast to the positivist scientific model used in “classic” ethnographies of the early and mid-20th century (Rosaldo 1993), it is interesting that Geertz relinquishes no control of his data to the voices of the Balinese villagers that he studied. This should not be surprising, however, given that the interpretive method is centered on the interpretation and the interpreter. Emic analysis, says Geertz, in his treatise on interpretive theory (1973b) “means that descriptions of Berber, Jewish, or French culture must be cast in terms of the constructions we imagine Berbers, Jews, or Frenchmen to place upon what they live through, the formulae they use to define what happens to them. What it does not mean is that such descriptions are themselves Berber, Jewish, or French—that is, part of the reality they are ostensibly describing; they are anthropological—that is, part of a developing system of scientific analysis.” I will take issue with this definition of “emic analysis” a little later.

Culture, for Geertz, is context. It is a backdrop against and within which actions take place. The job of the anthropologist is to “clarify what goes on…to reduce the puzzlement…” about a given social practice (1973b:16). Geertz’s main point of contention in this article is the culture-as-knowledge definition of culture, for which he takes Ward Goodenough to task. Understanding cultural practices cannot be reduced to a glimpse into the cognitive world of individual actors, argues Geertz. Rather, we must gain insight into “the imaginative universe” within which actions take place or meanings are exchanged. The best way, the most holistic way, to get at these meanings is rhetorically, through thick descriptions wrought out of long stays in the field and the building of rapport with key informants. The anthropologist thereby becomes a cultural interpreter.

In re-reading this Geertz essay, many of these ideas continue to resonate. For example, our craft is an imperfect one: Cultural analysis is (or should be) guessing at meanings, assessing the guesses, and drawing explanatory conclusions from the better guesses” (Geertz 1973b:20). And further, “Ethnographic findings are not privileged, just particular: another country heard from” (Geertz 1973b:23). In short, undertaking ethnographic research is a complicated and imperfect process.

Yet, something is missing here. Geertz, in “Deep Play,” proffers a rather sweeping analysis of Balinese culture, one that suggests a great degree of cultural coherence in this Balinese village. In “Deep Play,” we are asked to grant the generalizability of Geertz’s interpretations of what a cock fight

Amy Mountcastle
signifies largely on the basis of trust and faith in him that his reading/description/interpretation of the world "of cocks and men" accurately conveys something about the Balinese world of meaning. But much of his interpretation strikes me as classic Freud: the play of oppositions and internal contradictions expressed in the ritual and symbolic drama of life. "In the cockfight, man and beast, good and evil, ego and id, the creative power of aroused masculinity and the destructive power of loosened animality fuse in a bloody drama of hatred, cruelty, violence, and death," (Geertz 1973a: 420-421). Cocks signify to a man his "ideal self...but also...what he most fears, hates...and is fascinated by-The Powers of Darkness." (Geertz 1973a:420).

How are we to assess whether the way Geertz imagines Balinese constructions of the significances of cocks is a good, or at least a better, guess? Perhaps when the essay was first published in 1972, and even for a decade or more afterward, those guesses may have seemed like good and plausible ones. But do they stand the test of time? Or have the guesses grown tarnished and timeworn by outdated constructs? If the stories that we tell do not endure, then how do we view these ethnographic accounts—are they part of the historical record (of a culture)? Do they serve "to reduce the puzzlement" (Geertz 1973b:16) as Geertz hoped, or are they so much fodder of the imaginative universe of their authors and do they serve to confound?

What would have the "Notes on the Balinese Cockfight" looked like had Geertz, at some stage, explicitly addressed the question, "to what extent am I, as an investigator, justified in making inferences from a set of observations?" That is, the question of distributive reliability (Dressler 2001). Had the question of distributive reliability been made explicit at some point, would that not have led Geertz to consider more richly the various constituencies of a Balinese village, male, female, rich, poor, young, old, etc.? Instead he silences these subjugated, or at least non-dominant participants in favor of the dominant males who are the prime participants in major cockfights. Had he considered these various constituencies more seriously, how might his essay have then been written differently?

The most vocal critiques of positivism and ethnographic authority come from feminist ethnographers and postmodernists, pre-saged by the work of such methodologists as Bert Pelto in the 1960s. The questioning of ethnographic authority is, implicitly or explicitly, a questioning of generalizability. Some ethnographic researchers have abandoned the whole notion that any one can speak authoritatively about any Other. They may resort to solipsism, emphasizing subjectivity and self-reflexivity, sometimes abandoning cultural analysis for self-analysis. Others have been able to strike some semblance of balance, interspersing subjective musings with interpretive analysis. One of the more productive forays into this was that of Renato Rosaldo (1993 [1989]) in "Grief and the Headhunter’s Rage." Here he demonstrates how his attempts over a period of years to understand and interpret Ilongot headhunting were unsuccessful. Headhunting, his informants kept telling him, was about the rage they felt when they lost a kinsman. Rosaldo couldn’t accept this explanation and sought to elaborate through his own etic account. He then consulted his Ilongot informants and they would politely tell him no, his interpretation was not correct.

It was not until Rosaldo experienced personal tragedy with the sudden death of his wife, Michelle Rosaldo, that he began to understand what the Ilongot perspectives that might be convincing to oneself and one’s colleagues, but that are not really substantiated by the data.

Here, I take exception to Geertz’s use of the term “emic.” Emic analysis, the native’s point of view, the verstehen—he glosses these as anthropologists’ depictions of their understanding of how cultural insiders view something—is quite the opposite of the definition to which Rosaldo subscribes. When Rosaldo called upon his informants to corroborate his “reading” of the cultural practice, he was seeking out the emic perspective.
An interpretivist narrative may be a good read, rhetorically eloquent and perhaps convincing, but how are we, the readers, to know that the authority of the narrative is warranted? How, in other words, does one arrive at some level of confidence that the interpretations one is rendering do accurately reflect the views and sentiments of a significant portion of the research group during a given slice of time, that they really are better guesses?

**Tibetans in Exile**

My work on Tibetan refugees began as a classic anthropological exploration of the forces of continuity and change of what I thought of as Tibetan culture. How, in the face of their dislocation and exile, had Tibetans maintained a sense of cultural identity and continuity? After a short time in the field, my work began to take a different turn.

Tibetans have been dubbed one of the most “successful” refugees of the 20th century. They have successfully formed viable political and cultural communities in exile and at least for the first 30 years of their exile, had avoided much of the anomy that plagues many displaced communities. Yet conversations with a few differently placed individuals in the exile community also revealed that alternative narratives of identity existed.

At the time, these had not been well described in the literature, if acknowledged at all. Following the tradition of Geertzian thick description, I set out to present a balanced depiction of Tibetanness in exile that acknowledged the shared Tibetan identity but that did not ignore the fact that this was contested. I aspired toward giving voice to this other country as yet unheard from. But I did not attempt to enumerate any of this data. And furthermore, upon completion of my dissertation I was surprised to find that the voices of Tibetans were not as prominent as I had thought they would be. What had happened to their voices? How am I to judge now, a decade on, how well my narrative data stands the test of time?

In hindsight, and after the SCRM Survey Methods course, I realize that the questions I ended up asking about Tibetan identity are questions about cultural consensus, particularly regarding the juxtaposition of official narratives of Tibetanness and alternative accounts. I was questioning the assumption of an unproblematic consensus. However, there was, I did agree, something that could be called “Tibetan identity.” What comprises that identity? How do we know what are significant disagreements about aspects of Tibetanness and what are relatively minor? What do we mean by the term “Tibetan identity”? Had I employed quantitative methods in addition to my ethnographic methods, I believe that my data would be clearer and perhaps easier to further analyze at this time.

### The Shape of Cultural Consensus of Tibetans in Exile

Before I explore a hypothetical attempt at conducting cultural consensus analysis on Tibetan exile identity, I’d like to review what some of the advantages in using a cultural consensus model might be. Drawing from Dressler et al.’s work (2005a: 335):

1. a formal method for testing shared meaning that is derived from cultural insiders
2. quantification of shared meanings making formal (intra-cultural) comparisons possible

<table>
<thead>
<tr>
<th>TIBETAN GROUP</th>
<th>SAMPLE SIZE</th>
</tr>
</thead>
<tbody>
<tr>
<td>Old Amalas and Palas (over 60)</td>
<td>4</td>
</tr>
<tr>
<td>New Arrivals (since 1990)</td>
<td>4</td>
</tr>
<tr>
<td>Government official (cabinet member)</td>
<td>4</td>
</tr>
<tr>
<td>Tibetan Youth Congress (leadership)</td>
<td>4</td>
</tr>
<tr>
<td>Tibetan Women’s Association (leadership)</td>
<td>4</td>
</tr>
<tr>
<td>Amdo Association</td>
<td>3</td>
</tr>
<tr>
<td>Dhotoe Association</td>
<td>3</td>
</tr>
<tr>
<td>U-Tsang Association</td>
<td>3</td>
</tr>
<tr>
<td>Gelugpa, Sakyapa, Kharmapa, Nyingmapa, Bon (leadership)</td>
<td>5 [one from reach religious group]</td>
</tr>
<tr>
<td>Youth raised in exile (ages 15-22)</td>
<td>4</td>
</tr>
<tr>
<td>Rural/agricultural residents</td>
<td>4</td>
</tr>
<tr>
<td>Petty business owners</td>
<td>4</td>
</tr>
<tr>
<td>Urban dwellers (major city of India)</td>
<td>4</td>
</tr>
<tr>
<td><strong>17 Groups</strong></td>
<td><strong>50</strong></td>
</tr>
</tbody>
</table>

*Table 1*
3. a means of formally testing qualitative data regarding the degree to which a given individual or group of people shares in a particular cultural model, i.e. their cultural competence
4. a way of depicting the distribution of respondents around an objectively determined set of “culturally best” responses.
5. greater transparency in data collection and measurement of responses.

The questions I was asking about Tibetan identity are questions that could, with relative ease, be posed in the language of cultural consensus theory: “What is the knowledge base out of which individuals draw their understanding of who they are as Tibetans, in the exile context?” Or perhaps, “What is the cultural model of an ideal Tibetanness?” And further, “to what extent do individuals of the different constituencies of the exile community meet the cultural ideals of Tibetan identity?

From my ethnographic research, several important ideational nodes of Tibetan identity emerged. First, there was a notion of “the pure Tibetan” form of the arts (song, dance, thangka painting), of understanding and undertaking religious practices and devotional acts, of language, festivals, and foodways. In some respects these notions reflect an imagined ideal type Tibetan. In other ways, though, this reflected the recognition by Tibetans that with the moving away of each Tibetan to the West or the death of each elder, a tangible and palpable loss of cultural knowledge occurred. Not only that, but this cultural knowledge and purity was also being traded or given up for the vicissitudes of modern life: modern schooling, partaking of popular culture of the West and of India, even the adoption of modern democratic attitudes and practices, and increasing participation in the consumer economy. The first step in my imagined cultural consensus analysis is to identify a set of items and behaviors that Tibetans see as culturally relevant to an ideal type (emically determined) “Tibetan identity.” I also need to identify the key constituent groups from which to draw representative samples. These groups should have a high likelihood of internal consistency and high degree of likely inter-group variation. My research suggests that these would consist of the categories in Table 1.

Sampling is not random, but draws from individuals in leadership positions as likely “spokespersons” or opinion shapers of their memberships. The categories Old Amalas and Palas, New Arrivals, and Younger generation should be selected by gender and by “typicalness” as determined by the researcher. Structured interviewing would follow in Tibetan language or English, depending upon the preference of the interviewee, in which views of informants on the importance of the following items (for example) would be attained. A survey sheet might look like Table 2.

Answer the following question: To be a “true” or “real” Tibetan, how important is it for someone to:

<table>
<thead>
<tr>
<th>ITEMS LIST</th>
<th>Not Imp</th>
<th>Important</th>
<th>Very Imp</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Speak the Tibetan language</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2. Read and write the Tibetan language</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3. Attend Dalai Lama’s teachings</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4. Believe in/worship Dalai Lama</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>5. Believe in/worship some other lama</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>6. Be a Buddhist</td>
<td></td>
<td></td>
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</tr>
<tr>
<td>7. Do regular circumambulation</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>8. Have household altar</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>9. Do daily devotional practice: fill water bowls, burn incense, doing prostrations, recite mantras</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>10. Possess mala beads</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>11. Exhibit religious images, objects at home</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>12. Give offerings to monastery/nunnery or lama regularly</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>13. Rely on one’s own lama for guidance</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>14. Go for pilgrimages</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>15. Attend March 10 and March 12 rallies each year</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>16. Have strong feeling for freedom and independence (rangzen)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>17. Have strong wish/yearning to return to Tibet</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>18. Retain refugee status</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>19. Receive sponsorship (chin-dak)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>20. Work for the exile government</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>21. Work for TYC, TWA or some other organization sanctioned by the Dalai Lama</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>22. Live in shija (rural, agricultural community) in India</td>
<td></td>
<td></td>
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</tr>
</tbody>
</table>

Table 2 (continued on next page)
Through the survey, we should be able to determine, based on how items are ranked across sample sets, whether or not there is a shared cultural model and meaning of the differences of viewpoints expressed in interview data concerning what it means to be a Tibetan. Vague concepts such as attributes comprising true or ideal Tibetanness. My ethnographic data could be triangulated with this data and I could consider the significance we are able to determine which factors are most important for the ideal type Tibetan, then we could ascertain the degree of cultural competence of different individuals and constituent Tibetan exile groups. This would be a separate process in which we ask informants about their actual habits and behaviors regarding the items on the survey. The degree to which informants’ responses are in consonance with the model of cultural consensus will provide a potential opening for other insights or avenues for exploration. For example, the power of the consensus model and cultural consonance measurement could really be demonstrated in longitudinal studies of the Tibetan diaspora particularly as applied to Tibetan communities outside of South Asia. Questions that might fruitfully be addressed are: To what degree do Tibetans resettled across the globe share in this model? How does the model apply as the length of exile extends for decades? How does it apply to different generations of Tibetans, whose values are impacted by global culture and by the experience of secondary and tertiary migrations? Periodically, the consensus data would have to be reevaluated and new studies done to see what new items should be incorporated in the cultural consensus model and changes over time could be documented for both cultural consensus and cultural consonance for various constituent groups. There are implications here for applying some of the anthropological insights that could be gained and for additional intra-cultural research. How are Tibetan immigrants adapting to their new lands of secondary and tertiary migration? Are there mental health implications correlated to cultural consonance measures in different settings? Do these vary across the different diasporic Tibetan communities? What about Tibetan communities in Tibet? These models and data could potentially be useful for Tibetan immigrant groups as they establish secondary communities of exile and consider where and what to invest in the areas of cultural and community support.

<p>| | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>23. Live in Dharamsala</td>
<td></td>
<td></td>
</tr>
<tr>
<td>24. Do work to help others</td>
<td></td>
<td></td>
</tr>
<tr>
<td>25. Know how to play traditional Tibetan instrument</td>
<td></td>
<td></td>
</tr>
<tr>
<td>26. Know how to dance traditional dances</td>
<td></td>
<td></td>
</tr>
<tr>
<td>27. Know how to sing traditional songs</td>
<td></td>
<td></td>
</tr>
<tr>
<td>28. Prefer for Tibetan foods</td>
<td></td>
<td></td>
</tr>
<tr>
<td>29. Eat tsampa</td>
<td></td>
<td></td>
</tr>
<tr>
<td>30. Drink Tibetan tea</td>
<td></td>
<td></td>
</tr>
<tr>
<td>31. Observe/celebrate Losar</td>
<td></td>
<td></td>
</tr>
<tr>
<td>32. Follow astrological (lunar) calendar</td>
<td></td>
<td></td>
</tr>
<tr>
<td>33. Consult Tibetan (traditional medicine) doctors</td>
<td></td>
<td></td>
</tr>
<tr>
<td>34. Take Tibetan pills (medicinal)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>35. Marry another Tibetan</td>
<td></td>
<td></td>
</tr>
<tr>
<td>36. Have (fully Tibetan) children</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Table 2 (continued from previous page)

“...the power of the consensus model and cultural consonance measurement could really be demonstrated in longitudinal studies of the Tibetan diaspora particularly as applied to Tibetan communities outside of South Asia."

“Tibetan values” might also be given a stronger point of reference. The analysis of cultural consensus could then comprise the basis for a study of cultural consonance. Once...
Conclusion

There are some important potential drawbacks to cultural consensus analysis. Studies such as those conducted by Dressler, Oths, and others are time and labor intensive and very costly. They require a level of expertise and proficiency in statistical theory and statistical software. These problems may be overcome, however, through research collaborations. Another problem is more structural. Quantitative research calls for a very different kind of presentation than qualitative research and these approaches are usually published in different kinds of journals. The choice we make in how we conduct our research—our methodological choices—may be a function of our writing predilections as much as anything else. Can a narrative writer be turned into a quantitative researcher, and vice-versa? Furthermore, what outlets for publishing are there for ethnographic-quantitative articles?

These are not insurmountable problems, but they do require a different kind of thinking, and training, than most of us are accustomed to or have received in our graduate programs.

Another potential objection to the quantitative approach is, especially as it applies to something like identity, are we not reducing identity (Tibetan, in this case) to a list of features, behaviors, and attitudes? Wouldn’t this reductionism detract from our attempt to grasp the insider’s point of view, to make those better guesses and elaborations of cultural meaning? After all, identities and culture are more than the sum of their parts, more than aggregates and coefficients. In using cultural consensus, are we falling into thinking of culture or identity as “arranging abstracted entities into unified patterns”? (Geertz 1973b)

If we were arguing that consensus analysis should replace other methods of analysis, then there might be reason for concern. But consensus and cultural consonance analyses, derived at and placed in context, can be powerful tools for elucidating issues, concepts, propensities, and potentialities of groups. Consensus and consonance data should not be viewed narrowly. Items on a list are not themselves the shared cultural models. Cultural models or schemas are constructs. Lists share with narrative data the fact of being artificial—that is, they are substitutions, proxies, representations of an ungraspable and perhaps ultimately unknowable condition of the mind. Neither should be reified. Culturally appropriate and salient lists that informants react to, and the stories that informants tell in interviews are indexical—they are approximations of what an informant understands to be true, or wishes to represent as true, at a given moment in time. Each is subject to its limitations. But in combination, these types of data can help us to “reduce the puzzle-ment,” to do so secure in their reliability, and perhaps to tell powerful stories.

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2001 “A Prospective Study of Psychosocial Job Strain and Birth Outcomes,” Epidemiology, 12: 744-746.

Oths, Kathryn S.

Rosaldo, Renato

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MASCULINE IDENTITY AND HIV/AIDS RISK BEHAVIOR AMONG AFRICAN-AMERICAN MEN

By Jon Poehlman

Introduction

There is a need for research that contributes to our understanding of how culturally shared gender ideals and expectations about men’s behaviors influence HIV/AIDS risk. In the U.S., there has been a particular focus on masculinity among heterosexual, African-American men (see Wright 1993). African-American men, between 2001 and 2004, accounted for the greatest percentage of new cases of HIV/AIDS among males (44%), as well as 66% of all cases of heterosexual contact, with heterosexual contact being the second most common route of HIV transmission among this group after male to male sexual contact (CDC 2006).

Of note, in terms of responses, is Tony Whitehead’s (1997) detailed ethnographic consideration of African-American men and HIV risk. Whitehead’s work offers a compelling description of the influence of social factors on the production of HIV risk, with gendered culture serving as an important mediating factor. He suggests that men who are unable to live in accordance with the behaviors locally defined as the ideal masculinity may act out this stress in alternative or dysfunctional ways that increase HIV risk. Further, he locates the inability of some men to achieve the ideal in the pervasive structural and economic stressors that exist in society for African-American men.

As compelling as Whitehead’s ideas are about the production of risk, to my knowledge, his model has not been articulated in an empirical research or systematically evaluated. This may in part be because of inherent challenges to studying cultural conceptions of gender in relation to individual behavior. However, recent theory and methods from the field of cultural anthropology may be able to facilitate such an investigation. In particular, Dressler, et al. (2005) have created a measurement model for examining the relationship between culture and individual behavior. Their “Theory of Culture Consonance” is based on identifying and quantitatively establishing shared meaning within a population on elements of a cultural domain and then using this information to develop a survey scale to measure the congruence between an individual’s behavior and the shared meaning or local cultural model.

In this article, I briefly review Whitehead’s discussion of African-American men and masculinities. I then present a potential strategy for evaluating Whitehead’s model that builds on methods and theories developed in cognitive anthropology. This is followed by review of pilot research being conducted to identify concepts of masculine gender among a community sample of African-American men.

Whitehead’s Model of Fragmented Masculinity

Based on extensive ethnographic research, Whitehead evokes a multidimensional conception of masculinity among African-American men. In re-formulating the work of Peter J. Wilson (1973), Whitehead suggests that African-American men hold in a balance the gendered domains of respectability (e.g. providing for family, winning in competition, law abiding) and reputation (e.g. sexual prowess, gamesmanship, defiance of authority). Whitehead also suggests that social marginalization, racism, and economic disenfranchisement, as experienced by many African-American men, may cause a shift in the balance of these domains, as men increasingly turn to the reputational domain as they seek power in their daily lives.

As a result of this rebalancing, men may increase their engagement in marginalized and risk producing behaviors, including those that lead to exposure to HIV and other associated morbidities. Significant to Whitehead’s description is the intermediate role he ascribes to masculine gender ideals in both creating stress and in shaping men’s response to this stress in such a manner as to increase HIV/AIDS related risk behaviors.

Investigating Masculine Models as a Mediating Factor for HIV Risk

For purposes of policy and prevention, it would be of benefit if we could demonstrate an association between specific social conditions and individual risk for HIV/AIDS. To do so, however, requires a clear understanding of how our bodies experience their social conditions and turn those experiences into stress, as well as an understanding of the adjustments our bodies make in response to such stress.

Whitehead, through his research, offers masculine gender models as one potentiality important linkage or
mediating factor between experience and behaviors in African-American men. Nonetheless there are challenges to systematically investigating these connections, particularly when considering culturally formed masculinities. Such investigation requires: (1) defining and operationalizing culture in a manner that it can be empirically identified, (2) demonstrating that it is shared among a group and (3) possessing a means of examining the relationship between culture, individual behavior, and HIV risk (Dressler, et al. 2005).

As previously suggested, Dressler, et al.’s theory of culture consonance offers a potential approach to such investigations. It starts by using structured ethnographic techniques, such as free-lists and pile-sorts, to identify elements of a cultural domain. After having identified the elements of the domain, cultural consensus analysis (Romney, Weller, Batchelder 1986), a non-probabilistic data reduction procedure that provides a means for evaluating intra-cultural variation within groups, is used to determine if the identified domain is shared among a group and can be considered cultural. If one finds consensus, a measure of congruence can then be developed by using the weighted model for the domain created through consensus analysis to create a scale that can compare an individual’s actual behaviors or beliefs to those identified as representing the cultural ideal.

Having created such a measure, it can be employed in survey research with a larger, random sample of community members and used in regression analysis to understand the relationship of the measure of congruence to an outcome variable, such as risk behaviors for HIV, as well as to competing theories—such as one’s locus of control or an individual’s self-esteem, and control variables—such as socioeconomic status, HIV knowledge, religious affiliation, health, and age—that potentially contribute to incongruities between the shared model and behavior. Through this approach, one can explore the linkages between culture, behavior, and risk.

Research Activities for Developing a Measure of Congruence between Masculine Gender Ideals and HIV Risk Behaviors

Through an award from the University of North Carolina-Chapel Hill Center for AIDS Research, I am conducting research that explores issues of HIV risk among heterosexual African-American men. One element of this research has been to investigate whether a measure of cultural consonance (Dressler et al. 2005) can be developed in order to better understand differences between African-American men’s shared, masculine gender concepts and lived experience. The development of this measure is a first step in potentially evaluating the utility of Whitehead’s model of stress and risk for African-American men, as related to a masculine gender concept. Here, I report on two phases of research conducted in developing this measure. In Phase 1, free-list activities were used to determine the domain of ideal or prototypical masculine gender concepts for “being a man” among a sample of African-American men. In Phase 2, pile-sorts were conducted with men to organize items from the domain of ideal, masculine gender concepts. In each phase, twenty African-American men
<table>
<thead>
<tr>
<th>Domain Item</th>
<th>Number of Times Listed</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Providing for family</td>
<td>19</td>
<td>95%</td>
</tr>
<tr>
<td>Working (or looking for work)</td>
<td>17</td>
<td>85%</td>
</tr>
<tr>
<td>Taking Responsibility</td>
<td>10</td>
<td>50%</td>
</tr>
<tr>
<td>Being a father to one’s children</td>
<td>10</td>
<td>45%</td>
</tr>
<tr>
<td>Giving support to others</td>
<td>9</td>
<td>45%</td>
</tr>
<tr>
<td>Hanging out with friend</td>
<td>9</td>
<td>45%</td>
</tr>
<tr>
<td>Watching or playing sports</td>
<td>9</td>
<td>45%</td>
</tr>
<tr>
<td>Owning a home</td>
<td>8</td>
<td>40%</td>
</tr>
<tr>
<td>Having many women</td>
<td>8</td>
<td>40%</td>
</tr>
<tr>
<td>Paying your bills</td>
<td>8</td>
<td>40%</td>
</tr>
<tr>
<td>Making good decisions</td>
<td>7</td>
<td>35%</td>
</tr>
<tr>
<td>Getting an education</td>
<td>7</td>
<td>35%</td>
</tr>
<tr>
<td>Going to church</td>
<td>7</td>
<td>35%</td>
</tr>
<tr>
<td>Drinking alcohol</td>
<td>7</td>
<td>35%</td>
</tr>
<tr>
<td>Working to better your community</td>
<td>6</td>
<td>30%</td>
</tr>
<tr>
<td>Putting food on the table</td>
<td>6</td>
<td>30%</td>
</tr>
<tr>
<td>Taking care of oneself</td>
<td>6</td>
<td>30%</td>
</tr>
<tr>
<td>Having a car</td>
<td>6</td>
<td>30%</td>
</tr>
<tr>
<td>Dressing well</td>
<td>6</td>
<td>30%</td>
</tr>
<tr>
<td>Being a role model</td>
<td>5</td>
<td>25%</td>
</tr>
<tr>
<td>Knowing what is going on in your community</td>
<td>5</td>
<td>25%</td>
</tr>
<tr>
<td>Take care of women</td>
<td>5</td>
<td>25%</td>
</tr>
<tr>
<td>Selling drugs</td>
<td>5</td>
<td>25%</td>
</tr>
<tr>
<td>Being satisfied with oneself</td>
<td>5</td>
<td>25%</td>
</tr>
</tbody>
</table>

Exhibit 1. Free-List of Masculine Domain Items

Participants in Phase 1 ranged in age from 24-59; with an average age of 38. The majority of the men was single (16), either having never married or were separated or divorced. The remaining four men were married. Fourteen of the men report having attended or graduated from high school. Three men reported having some college and another three reported having graduated from a 4 year college. Twelve of the twenty men report making less than $20,000 a year in income. One man reported making between $20,000 and $39,999. Five men reported making between $40,000 and $59,999.

Phase 2 participants were slightly older, with an average age of 40 (range 24-57) and more of the men were married (7 of the 20). Similar to the Phase 1 group, most of the men (14) reported having attended or graduated from high school. Two men reported only receiving a grade school education and two men reported attending or graduating from college. A greater number of men in Phase 2 reported earning more than $20,000 a year (10 < $20,000, two report not knowing their income); five reported earning $20,000 and $39,999 and three reported earning between $40,000 and $59,999.

Phase 1: Free-Listing to Determine the Domain of Ideal Masculine Gender Concepts

In Phase 1, after being consented and filling out a brief demographic information sheet, men were asked to orally provide as many answers as possible to the following two questions:

1. What makes someone a man in your community?
2. What kinds of things do men do to show that they are a man?
In answering the free-list questions, men were told that their responses did not have to reflect only their own views, but should also include the views of other men in their community. With each participant, after completing an initial list of answers to each question, the interviewer went back and probed each response, asking if there were any other related items. In addition to the free-list, the participants were asked to describe some of the challenges men face in their community.

From the free-listing, a total of 93 separate terms were generated. When related terms were collapsed together based on the judgment of the researcher, the number was reduced to 80. The number of concepts reported by an individual ranged from 30 to 7, with 18 responses being the average. Among the items listed, 51 items were listed more than once. Exhibit 1 displays the twenty-four most common items listed by the men in the interviews.

Highlighted (shaded in grey) in the list are several gender concepts that contrast the list of mostly positive attributes provided by the men. These items were used in describing men in their community, and usually posited following some form of social distancing on the part of participants. For instance, a man might say, “Some men think it is all about drinking and getting women”, suggesting the presence of alternative masculine ideals separate from their own.

A difference between “boys” and “men” was also used commonly to describe positive and negative male attributes. This dichotomy may reflect elements of the central concepts of “respectability” and “reputation” as suggested by Whitehead, with “boys” engaging in a “corner culture” that values the rewards of illegal activities and “getting over on women.” By contrast “men” are thought of as having put those activities behind them and now taking responsibility for themselves and others.

Free-list data also provides a cursory weighting of the values placed on each of the masculine gender concepts, based on the number of times each item is mentioned. From this sample, we see

<table>
<thead>
<tr>
<th>#</th>
<th>Domain Item</th>
<th>MDS Title</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Provides for family</td>
<td>FAMILY</td>
</tr>
<tr>
<td>2</td>
<td>Father to children</td>
<td>FATHER</td>
</tr>
<tr>
<td>3</td>
<td>Takes responsibility</td>
<td>RESPONSIBILITY</td>
</tr>
<tr>
<td>4</td>
<td>Has a house</td>
<td>HOUSE</td>
</tr>
<tr>
<td>5</td>
<td>Pays the bills</td>
<td>BILLS</td>
</tr>
<tr>
<td>6</td>
<td>Hangs out with friends</td>
<td>HANGSOUT</td>
</tr>
<tr>
<td>7</td>
<td>Drinks alcohol</td>
<td>ALCOHOL</td>
</tr>
<tr>
<td>8</td>
<td>Educated</td>
<td>EDUCATE</td>
</tr>
<tr>
<td>9</td>
<td>Goes to church</td>
<td>CHURCH</td>
</tr>
<tr>
<td>10</td>
<td>Dresses well</td>
<td>DRESS</td>
</tr>
<tr>
<td>11</td>
<td>Has a car</td>
<td>CAR</td>
</tr>
<tr>
<td>12</td>
<td>Works to improve the community</td>
<td>COMMUNITY</td>
</tr>
<tr>
<td>13</td>
<td>Is a role model</td>
<td>ROLE-MODEL</td>
</tr>
<tr>
<td>14</td>
<td>Takes care of his wife/girlfriend</td>
<td>TAKESCARE</td>
</tr>
<tr>
<td>15</td>
<td>Married</td>
<td>MARRIED</td>
</tr>
<tr>
<td>16</td>
<td>Has money</td>
<td>MONEY</td>
</tr>
<tr>
<td>17</td>
<td>Stands up for himself</td>
<td>STANDS-UP</td>
</tr>
<tr>
<td>18</td>
<td>Talks trash with friends</td>
<td>TRAHTALKS</td>
</tr>
<tr>
<td>19</td>
<td>Goes on dates</td>
<td>DATES</td>
</tr>
<tr>
<td>20</td>
<td>Has a child</td>
<td>CHILD</td>
</tr>
<tr>
<td>21</td>
<td>Shows respect for others</td>
<td>RESPECT</td>
</tr>
<tr>
<td>22</td>
<td>Works on things (fixing cars/cutting grass)</td>
<td>HOUSEWORK</td>
</tr>
<tr>
<td>23</td>
<td>Show his feelings</td>
<td>FEELINGS</td>
</tr>
<tr>
<td>24</td>
<td>Pride</td>
<td>PRIDE</td>
</tr>
<tr>
<td>25</td>
<td>Competes with others</td>
<td>COMPETES</td>
</tr>
<tr>
<td>26</td>
<td>Makes own decisions</td>
<td>DECISIONS</td>
</tr>
<tr>
<td>27</td>
<td>Independent of others</td>
<td>INDEPENDENT</td>
</tr>
<tr>
<td>28</td>
<td>Military</td>
<td>MILITARY</td>
</tr>
<tr>
<td>29</td>
<td>Knows what is going on in the community</td>
<td>KNOWS</td>
</tr>
<tr>
<td>30</td>
<td>Looks out for others</td>
<td>LOOKSOUT</td>
</tr>
<tr>
<td>31</td>
<td>Has a Job</td>
<td>JOB</td>
</tr>
<tr>
<td>32</td>
<td>Watches or plays sports</td>
<td>SPORTS</td>
</tr>
<tr>
<td>33</td>
<td>Has many women</td>
<td>SEXUAL</td>
</tr>
</tbody>
</table>

Exhibit 2. Pile Sort Items

Phase 2: Pile-Sort to Assess Domain Structure

It is through the research techniques of pile-sorting and multi-dimensional scaling (MDS) that we can better appreciate the structure of domain items, including the dimensions by which it is organized and the clustering of similar concepts.

In Phase 2, research participants were randomly presented, on index cards, 33 of the domain items identified from the free-lists and asked to sort the cards into piles based on the similarity or “likeness”...
of the items. Exhibit 2 shows the list of 33 domain items the men were asked to sort.

In conducting the pile-sort, participants were instructed that they could make as many piles as they wanted, as long as they made more than one. At the conclusion of sorting the cards, they were also asked to label or describe the contents of each pile. In doing the pile-sort, men made as few as two piles and as many as twelve. The average number of piles was five.

Following data collection, a non-metric, multi-dimensional scaling of the pile sort data was conducted using the ANTHROPACK software package. The two dimension MDS solution it created showed acceptable stress (.133) and is presented in Exhibit 3.

In visually inspecting the MDS solution for dimensionality and clustering, one is likely to first notice the left and right sided clustering of items. On the left side of the graph, we see many of the more positive items that men used in describing what it meant to be a man in their community, such as taking care of others, having a job, and being responsible. Within this cluster, there may even be further division, with attributes or characteristic of a being a “man” appearing towards the bottom and more concrete actions or status items closer to the top.

On the right side, we see what might be described more traditional masculine attributes, such as being competitive, drinking, and having many sexual partners. Note that military, which appeared relatively low in the free-list frequencies (listed just once), does not appear to fit well into most men’s conception of being a man, evidenced by its solitary placement in the lower left-hand side of the graph.

What is interesting about this clustering is the congruence of these groups with Whitehead’s concepts of “respectable” and “reputational” attributes of men. This research suggests that the categories of African-American masculinity proposed by Whitehead reflect in many ways how these men actually interpret and reason about what it means to be a man.

In terms of a dominant dimensionality reflected in the men’s ordering of the domain items, this is harder to interpret given the strong bi-polar clustering. Nonetheless, in looking at the men’s explanations for the piles they created, we find some clues. Men, in naming the piles, often referred to them in terms of either an age or experience gradation. Certain behaviors were associated with youth or inexperience; and as one matures or learns, he develops a different set of perspectives by which his life takes on meaning. Given this insight, the right to left progression in the items may parallel some kind of perceived life-course. This conclusion is at this point speculative and requires further research to resolve, through additional interviews with men to learn their interpretations of this phenomenon.

**Future Research and Conclusion**

Having conducted the MDS to better understand the semantic domain identified for being a “man,” in developing a measure of cultural consonance between men’s ideal or prototypical masculine gender concepts, the next step is to assess the extent that this domain, as identified in this research, is shared among men and can be considered cultural. To make a determination of shared culture, cultural consensus will be used to assess agreement across a set of responses to structured data collection techniques, as can be derived from the rankings of domain items on a single attribute or responses to individual item scales for each domain item.

If consensus is found, I will then have the necessary pieces to develop an item scale that can be used to assess men’s actual behavior or attitudes in relation to the identified and shared ideal masculine gender model that make up the domain of being a “man.” In following Whitehead’s lead, we can hypothesize that incongruencies of this nature produce a kind of psychosocial stress, which can now be measured, as well as assessed in terms of
its impact on one or multiple independent variables, including risk behaviors for HIV/AIDS. This measure will hopefully be the product of additional ethnographic research with African-American men in the Durham community.

In reporting on this current research, I should acknowledge that this data is derived from a small sample of men. And while the research methods being used are fairly robust in terms of sample size, the project could be strengthened with more participants, or expanded by stratifying the sample to include different social groups of African-American men (e.g. college students, members of fraternal organizations).

Further, while the data collected are thought to be representative of the perspectives of African-American men in Durham on gender related meanings, a bigger question is how representative are the perspectives of Durham men to other African-American men and to men in the U.S. in general. Are the phenomena reported in this article, in terms of masculine gender concepts, localized or part of broader cultural system? These are questions that require further research and comparison.

In closing, I want to reflect on a conversation I had with one of the men I interviewed. As I asked him about the challenges that men face in his community, he gave me a particularly detailed causal model for why drugs and alcohol use are common behaviors among men in his community. It started with men being unemployed and lacking opportunities in the community, which then meant an inability to provide for their families. In turn, men feel a lack of control, low self-esteem, as well as a feeling of loss. Use of alcohol and drugs then becomes a way to forget these feelings.

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“...
MEASURING MARRIED WOMEN “OPTING OUT”

By Dianna J. Shandy and Karine S. Moe

Women in America wield governmental and corporate power at levels never before seen in the history of this country. They have unprecedented access to education, jobs, and income. Having gained a foothold in formerly male-dominated positions, many of these highly educated, accomplished women are opting out of their careers, often as a consequence of becoming mothers. This presents a paradox in the negotiation of women’s roles, one whose implications have not been fully examined in social research.

Working together, as a cultural anthropologist and a labor economist, we set out to explore why these women opt out and to probe the implications of doing so. This article describes the resulting “qual-quant” research design of an on-going project, focusing in particular on the specific impacts of two different NSF-sponsored Short Courses in Research Methods in Cultural Anthropology, Methods in Survey Research (taught by William Dressler and Kathryn Oths in 2006) and Methods in Behavioral Observation (taught by Raymond Hames and Michael Paolisso in 2007) in shaping the direction this project took. This project provides an example of an applied, interdisciplinary collaboration that seeks to inform public debate and to demonstrate how both qualitative and quantitative research can be enriched through combining their complementary strengths. In particular, in building on concepts shared in the short courses, we developed two surveys designed to enhance and expand on the findings from traditional ethnographic research, in the form of interviews and focus groups. We discuss the use of surveys to expand our sample size, test hypotheses, and overall enhance the power of our research findings.

The Project: Married Women with Children Opting Out of the Workforce

Around the time we became interested in this research topic, the media started to buzz regarding these same sets of issues. Lisa Belkin’s (2003) New York Times Magazine piece on the “Opt-out revolution” kick-started a series of similar articles relaying anecdotes about highly educated mothers escaping the rat race. These stories in Time, Business Week, and Fortune, among others, sparked a passionate debate on blogs, at the dinner table, and in book clubs across the country. Not surprisingly, the responses ranged dramatically from those who felt affirmed by the recognition of their life choices as a growing trend to those who dismissed the existence of the phenomenon as neo-conservative hype.

We were inspired to look beyond media reports to see for ourselves what the numbers said. What we found, by drawing on U.S. Department of Labor data, convinced us that the significant outflow of women from the top tiers of the U.S. labor force is not only anecdotal. In fact, the full time labor force participation of married women with professional degrees and children under 18 fell from nearly two-thirds to just over a half between 1998 and 2004. To find this information, we analyzed data from the Current Population Survey (CPS), which is a monthly survey of roughly 50,000 households conducted by the Bureau of the Census for the Bureau of Labor Statistics. The CPS is the primary source of U.S. labor force information, and the data is readily accessible on their website (www.bls.gov/cps). Once the data are downloaded, it is fairly straightforward to sort the information by marital status and education using a standard statistical package, such as STATA or SPSS. From a labor market perspective, this is a significant and remarkable shift. This quantitative finding
related to change over time enhanced our ability to frame subsequent research questions. Once we had clarified for ourselves what we read in the media and what we could discern from our own social networks had some quantitative traction, we wanted to know why. And here, our research questions dictated a transition from quantitative to qualitative methods. While the labor data could tell us how many women were employed, it couldn’t explain why women make such different choices about career and family. What was drawing these women out of the labor force? And what kept other women tethered to their jobs? What did women see as the advantages and disadvantages of their decisions regarding career-family balance? We knew from our own social networks of high-earning women who had opted out that it was far more complicated than just a simple economic calculation of whether the wife’s earnings exceeded the family’s expenses incurred by foregoing her “services” (e.g. child care, managing the household, etc), as the husband of one of our informants suggested was the case. Yet we knew very little about how women who had “opted out” framed their experience.

**Our Core Research Questions and Activities**

To explore preceding questions, as well as other potential issues or ideas, we started by developing two similar semi-structured interview schedules—one for in-person individual interviews and another for focus group interviews. To give an idea of the kinds of information elicited, here we describe the individual interview. The interviews began by the woman telling us the “story” of her education and employment background with particular attention to when she had children, took family leaves, and had a change in employment status. We followed up with probes to ascertain her current employment status and for those who had had a change in employment status (e.g. from full-time to part-time, or from part-time to unemployed) to describe their employment situation before the change. We inquired about her spouse’s occupation and schedule. Using Spradley’s (1979) model, we also asked for details about how an average day in her household would unfold and whether there were any variations to that cycle. The interview then moved on to questions about what she saw as benefits and challenges in her current situation and the strategies her family used to make their situation work. We also asked about ideal child rearing and employment situations, i.e., what she would change about her situation if she could, as well as the key factors that shaped the choices her family had made about child rearing and employment. The interview concluded with a series of questions about re-entering the work force, such as whether she anticipated doing this at some point, what kind of job she would seek, and if she anticipated any obstacles. We wrapped up by asking informants for guidance in determining questions for future interviews. To contact women for the in-person interviews and the focus group interviews, we used a snowball sampling technique.

In conducting the interviews and focus groups, we appreciate that our position as “working moms” would make this research somewhat sensitive. Mothers in the United States often implicitly align themselves in what are called “the mommy wars” that pit women who stay at home to raise children and women who pursue careers while raising children against one another. Therefore, in our research we sought to avoid any perception that we were judging these women or the choices they had made about family and career. One strategy we used to avoid the pitfalls in this dynamic was to ask open-ended questions and to pay particular attention to the “folk” or insider terms these women used to describe their lives (see Spradley 1979.) Where possible, we used the language and terms the women themselves employed in describing their lives to frame follow-up questions and probes.

We used female, college-educated, a mother, and married as screening criteria. For the focus group interviews, to minimize any potential “mommy war” effects, we attempted to group the women according to what we understood was their employment status, appreciating that this might be a fluid status for some of the women. In addition, we also did a focus group with college-aged women using an adapted interview schedule to explore possible generational differences. This project was approved by the Macalester College institutional review board. All interviews were tape recorded and transcribed with the written consent of interview participants.

To date, we have interviewed nearly 100 women. These individual and focus group interviews yielded very rich data. The women spoke eloquently regarding the “mommy war” effects, we attempted to group these findings elsewhere (see Moe and Shandy, forthcoming.) Despite the compelling nature of the material we had collected, however, we wondered to what extent our findings might be generalizable to a broader population. What separated our work from journalists who were also conducting interviews with women about these topics? How would our findings be received by the publics we sought to inform, specifically the executives and policy makers responsible for structuring the space in which women make decisions regarding family and career? How could we pre-empt having our work dismissed as anecdotal?

**Enhancing Our Research: Lessons Learned from NSF Short Courses on Research Methods**

Here, it is useful to recap the sequences of decisions regarding research methodology in this project. An observation made by journalists based on interviews (or qualitative research), prompted us to test empirically their observations using macro-level quantitative data. The findings from the quantitative data raised questions that could not be answered by existing available datasets. We determined that these questions could be answered best...
through qualitative inquiry. Our desire to determine whether our interview findings were generalizable and our desire to pack more “power” into our findings led us back to quantitative methods. And this is where the NSF sponsored Short Courses on Research Methods come in. In this section we describe two on-going surveys whose design was facilitated through participation in two SCRM short courses—*Methods in Survey Research and Methods in Behavioral Observation*.

**The Survey Sample**

To complete our research for this project, we have opted to conduct two surveys using the professional version of the web-based survey tool, Survey Monkey (www.surveymonkey.com). We are in the process of drawing our sample by working with the alumni associations at select colleges and universities nation-wide. For our first survey, we are working with the alumni office at one institution to draw a random sample of approximately 2,000 alumnae, living in the United States, who graduated between 1970 and 2007 (which will give us a sample size of approximately 50 women per graduating class). This will allow us to administer Survey 1 to approximately 40 students and Survey 2 to approximately 10 students per graduating class.

We have asked the alumni office to contact the alumnae (using a letter we drafted for them) initially and to provide the link to the survey. We intend to repeat this process by working with alumni offices at other institutions. To our pleasant surprise, this institution reported having current email addresses for 98.5% of alumnae fitting our sample criteria.

**Survey Methods: The Life Trajectory Survey**

The aim of the Survey Research Methods course was to introduce anthropologists to the uses of social survey techniques in ethnographic research. The course covered survey research design, sampling strategies, linking ethnographic and survey data, construction of interview schedules, interviewing techniques in survey research, training interviewers, data quality control, coding and entering data, and analytic strategies for survey data. The most valuable part of this course, however, was its emphasis on teaching quantitative methods, with special emphasis on survey methods, *in the service of anthropology*. In this respect, the course was uniquely tailored to the needs and concerns of anthropologists. By combining qualitative with quantitative approaches to research projects, the course stressed how doing so could enable ethnographic researchers to test hypotheses. Participation in this course, therefore, contributed to the development of our next and on-going phase of research—the life trajectory survey.

The life trajectory survey looks at the sequencing of education, career, and family for three generational cohorts of women, Baby Boomers, Gen Xers, and Millennials. Through this survey we seek to test the hypothesis, developed from our qualitative research, that younger college graduates are not being “pulled” or “pushed” out of the workforce, but are planning to take time off from their careers to have families in ways Baby Boomers and older Gen Xers did not. This directed survey is divided into four sections: educational background, employment history, family history (whether a respondent has children, when the children were born/adopted, and how having children intersected with education and career trajectory), and demographics. We include a question asking women to categorize their plans for career arrangements for parents with young children that will allow us to test our hypothesis. Women without children will serve as a comparison group for this survey.

**Methods of Behavioral Observation: The Time-Use Recall Survey**

The Methods of Behavioral Observation course sought to teach ethnographic researchers, familiar with participant observation, ways to make their observations more useful in answering specific questions of anthropological interest. One of the areas of focus in this course was time allocation. The second survey, therefore, is adapted directly from readings and discussion for this course. This is a time-use recall survey in which we examine how college-educated, married, employed mothers allocate their time, compared to the time allocation of at-home mothers. Women without children will be screened out of this survey. One of the hallmarks of ethnographic research is participant observation. One of the drawbacks of participant observation is that it is very time-intensive, and, as a result, means that one or two researchers can only conduct continuous observation of a relatively small sample size (unless they are working with funding that allows employment of a research team). Another difficulty appears in urban and suburban settings when the researcher attempts to conduct continuous observations of people on the move and who engage in most of the activities of interest in the privacy of their homes or vehicles. We attempted in our individual interviews to elicit information about what women actually *do* by asking Spradley’s (1979) average day question: “Please describe what you did yesterday from the time you woke up until the time you went to sleep at the end of the day.” While this provided useful descriptive information that emphasized women’s *actions* and not just their opinions, we still felt like there was another layer of activity that we were unable to access. One of the obstacles we encountered was that these women were highly attuned to the American cultural ideal that it is desirable to be busy (or to present the appearance of being busy); this effect may have been intensified because of our own role as working moms asking at-home moms about how they spend their time. For example, one informant claimed that the only time she ever sat down during the day was when she was in the car driving kids from point A to point B. While this statement provides interesting insight into how this informant sees her role as an at-home mother, it likely does not reflect the actual time allocation of her day. Therefore, we sought a more systematic
means of eliciting how women spend their time.

When we piloted the time-use survey with some informants, we were surprised to see how much more detailed information we elicited than with the average day question. We cannot claim that this information is more accurate per se, but it does seem to generate a more systematic reflection on actual time use. The method still, however, is limited by the effect of recall bias in ways that direct behavioral observation would not be. We told women that we were interested in better understanding women’s lives by looking at how they spend their time. We asked them to tell us about their activities the previous day (a week day after the start of the school year), for 24 hours, beginning at 4 a.m. We asked them to record this information in a chart that separated out their main activity from any other activities. For example, from 10-11 a.m., their primary activity might be “taking the kids to the park.” Their other activities might be “used cell phone to returned call to son’s school and to confirm airline reservations for vacation.” Figure 1 is an excerpt of a completed time-recall chart to illustrate this survey technique. Both the life trajectory and the time-use survey include a question asking the respondent if they would be interested in being contacted by telephone for a follow-up telephone survey. We intend to sample randomly from a stratified sample of all positive responses to this question to conduct follow up interviews. The sample will be stratified according to employment status to allow us to speak with equal numbers of mothers who are employed outside the home and mothers who are not.

### Conclusion

When economist and anthropologist colleagues who know we are collaborating on this project ask one of us independently how the work is going, it is often accompanied by a meaningful tilt of the eyebrows. Implied perhaps in these comments is the suggestion that economists and anthropologists see the world in quite different ways. We agree; they do. In our project thus far, however, we have seen this as a strongpoint and not a stumbling block.

An outcome of the anthropologist-economist collaboration has resulted in a research design for this project that might most accurately be described as “qual-quant-qual-quant-qual.” In our experience working together, we have attempted to let the research question at hand determine the methods we employed. In this respect, the SCRM courses provided a set of tools for how to do anthropological research more systematically in ways that allow the researcher to test hypotheses. On another level, however, the SCRM courses helped bridge the gap between qualitative and quantitative research by facilitating the interdisciplinary conversation on a research team.

Currently the public debate on the topic of work and motherhood is fueled primarily by the work of journalists and other scholars who rely on qualitative inquiry. We, too, rely on interviews and focus groups with women; however, in our effort to impact public debate more effectively, we combine our qualitative research with hard data and statistical analyses that allow us to test hypotheses. We deem this a critical element of our work if we wish to advance the public conversation about women, work, and family.

### References

Belkin, Lisa

Spradley, James P.

**Karine Moe and Dianna Shandy** are working on a book about well-educated married women with children opting out of the labor force. Karine Moe is Professor of Economics at Macalester College. A labor economist, Moe’s research and teaching interests include economic growth, labor and demographic economics and economics of the family. Dianna Shandy is Associate Professor of Anthropology at Macalester College. Shandy is a sociocultural anthropologist whose work engages questions related to gender, forced migration, qualitative research methods, and applied social research. She is the co-author (with David W. McCurdy and James P. Spradley) of the second edition of *The Cultural Experience: Ethnography in Complex Society* (Waveland Press, 2004).
USING NVIVO WITH GROUNDED THEORY AND OTHER QUALITATIVE METHODS

By Liz England Kennedy

Introduction

This paper demonstrates the potential usefulness of the NVivo7 software for developing grounded theory through semantic analysis and for making grounded theory more accessible to students and researchers. Use of qualitative data analysis software early in the research process can impact research design, including the creation of interview protocols and survey instruments. It can also be useful later in the process for content and discourse analyses.

NVivo is a software program designed to facilitate coding and analysis of qualitative data; it includes “query” functions, which are specific searches that the software can perform on data.

The newest NVivo7 service packs introduce a new software feature for analyzing word frequency: the word frequency query (WFQ). The WFQ simultaneously and easily imports multiple documents and compiles comprehensive word frequency listings. Here I describe a project in which I tested the effectiveness of the WFQ feature. To accomplish this, I used data from two projects and from different points in the research process: before interview protocol design and after initial data collection. Project 1 drew on my dissertation research, which used life history interviews and ethnographic research in two sites to study the lived experiences of undergraduates diagnosed with learning disabilities and/or attention deficit disorders. Project 2 drew on data gathered early in an ongoing five-year, six-region ethnographic team project (NIMH R01 MH76084-01, C. Willging, Principal Investigator), briefly described below. In conducting these analyses, I found several uses for NVivo’s WFQ function. I first illustrate the importance of linguistic analysis for applied anthropology and provide a brief overview of grounded theory. Next, I discuss my uses of NVivo, then discuss my research findings, their possible applications, and limitations of the WFQ function.

Linguistic Analysis

Social scientists gather a great deal of linguistically-based data: e.g., narratives, naturally-occurring speech, and written texts. They acknowledge that words are multivocal, multivariate, and polysemic: They have different implicit and/or explicit meanings that depend on factors such as social context, linguistic context, and speakers’ social positions and intentions. Uses and semantics of words have become objects of study in and of themselves in anthropological research.

The usefulness of linguistic analysis has been demonstrated in a number of important practical settings. For example, the study of words is important in understanding cultural differences in medical systems, beliefs, and communications. How people use and interpret words creates and reflects their understanding of symptoms, causes of illness (etiology), systems of diagnosis (nosology), prevention and treatment activities, and other aspects of medicine. Whether a pattern of signs is labeled as “flu,” “tuberculosis,” or “the sniffles” affects how people respond to and care for the afflicted individual. Diagnosis involves attempts to choose a “correct” label from a set of interlinked terms: individuals compare items with varying semantics (meanings) to determine which of a network of interrelated words best “fits.” Which label is selected (if any) determines whether a person is allowed to take on the “sick role,” what care the person is given, and how the “sick” person understands her or his difficulties.

The use of the same word by people in different social positions also impacts medical care. De Morgan, et al. (2002) found that the term “carcinoma” held markedly different meanings for doctors and women diagnosed with ductal cancer. Such differences led to confusion and miscommunications that increased the women’s fears and made it harder for them to participate in treatment decisions or understand treatment recommendations.

In previous research, I demonstrated the importance of semantic analysis in regard to understanding the notion of “school.” I found that the definition of “school” was highly consistent, regardless of the type of “school” that interviewees had attended (e.g., public, private, homeschooling). “School” is a physical place with a regimented timeframe; formalized and enforced behavior codes; an age-based hierarchy; a single adult who serves as authority, gatekeeper, and information provider to an attentive group of younger people; and an emphasis on rote learning. Other learning modalities such as tutoring and seminars were not included in this framing. Thus, through semantic analysis, I identified a central ideal for the concept of “school,” which could then have been used as a basis for researching cultural consonance regarding “schools.” (For a discussion of cultural consonance, see Dressler, et al., 2005).
Grounded Theory

Grounded Theory (GT) is a popular research paradigm for developing theory based in systematically-obtained social science data (Glaser & Strauss 1967). Its processes are designed to produce theory that accurately reflects the lived realities of the people studied, clarifies relationships between theoretical concepts, and indicates practical applications of findings.

"Grounded Theory (GT) is a popular research paradigm for developing theory based in systematically-obtained social science data (Glaser & Strauss 1967). Its processes are designed to produce theory that accurately reflects the lived realities of the people studied, clarifies relationships between theoretical concepts, and indicates practical applications of findings."

Methods

I used data from two different projects to test the utility of NVivo 7’s WFQ feature. The use of WFQ was not part of either project’s research design. I tested the WFQ at two points within an overall research timeline: before interview protocols were created (Project 1) and after an initial field test of a protocol (Project 2).
to the large number of newspaper articles, which were often short in length and referred to localized situations, the media analysis was limited to magazine articles. I also omitted book reviews, assuming they would focus more on the book than on the topics.

I limited the article list to mass-distributed titles by creating a list of locations where the magazine should be easily found by members of the general public and sampling the available titles. Sites included were airports, common stores, major grocery chains, and doctors’ offices. Specific titles included were *Scientific American*, *Discover*, *Ladies’ Home Journal*, *Good Housekeeping*, *US News & World Report*, *Business Week*, *Psychology Today*, *People, Family Life*, and *Health*.

Ultimately, I only relied on article abstracts to represent the articles for three other magazines (*Working Woman*, *Woman’s Day*, and *Natural Health*) because full texts were unavailable to me. In total, I found 62 relevant articles or abstracts.

**Project 2**

Project 2 used texts from a team-based longitudinal ethnographic study of the effects of New Mexico’s “behavioral health reform” on services for people with severe mental illness, substance dependencies, or both. The reform, which was intended to enhance use of scant resources and provide quality care, re-organized 17 state service agencies into one “purchasing” collaborative. The purchasing collaborative contracts with a single billing entity (SBE), which then contracts with individual agencies. This project relies heavily on ethnographic interviews and observations. Interviewees include statewide policymakers, agency administrators and staff, frontline service providers, consumers, and consumers’ social supports. Interviews and analysis began with upper-level administrators.

In Project 2, I compared researcher-generated lists of key words with the results of a WFQ on texts created early in the project (the original contract between the SBE and the state, and a letter of concern written by frontline service providers to the SBE), and texts of three interviews with upper-level administrators. I selected these materials because they were created early in the ethnographic study and reflect diverse voices of the participating stakeholder groups.

**Procedure**

In both cases, I imported the relevant documents into NVivo Projects, then created a file of words to be omitted from the resultant frequency list. Because I wanted this method to be applicable with non-English languages, I decided to omit syntactic categories of words rather than words from “common words” lists. Specifically, I omitted these categories:

- articles
- conjunctions
- pronouns
- verbs related to speaking
- phrasal verbs
- auxiliary verbs, including conditionals
- referentials
- positionals (in English, prepositions)
- other locatives
- words specifying geographic location
- quantity-related words (enumerations and ordinals)
- interjections (e.g., “oh”)
- (for non-English languages, other grammatical particles, e.g., “wa”)

I also omitted terms that were specifically legal (e.g., “party,” “contract”) to avoid biasing the results toward legal aspects of the documents. Then I ran the query: “New”—“Word Frequency Query”—“Add to Project”—“Display 1000 most frequent” and considered the top 100 words.

Since NVivo is not designed to allow word culling unless every word in every document is separately coded, I exported the results to Excel (in NVivo: “Project”—“Export list”—[location]) and eliminated the relevant words. I also eliminated fragments such as “non-” and “co-” and combined the counts of singular and plural forms of nouns. This took about one hour.

I next compared the WFQ-generated word list with researcher-generated lists. For Project 1, I compared the WFQ-generated list to my dissertation semantics list. For Project 2, I asked my fellow Ethnographic Team members to send me lists of 10-15 terms whose meaning they would like to have defined by interviewees. Team members produced their lists independently. In the seven lists created by my fellow researchers, one word appeared in five lists, one in four, four in three, and six in two. All other words were unique.

**Results**

The results from Project 1 gave the closer match between software-generated

<table>
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<tr>
<th>school</th>
<th>medication</th>
<th>disorder</th>
<th>learning</th>
<th>teach</th>
<th>disability</th>
</tr>
</thead>
<tbody>
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<td>Ritalin</td>
<td>tutor</td>
<td>deficit</td>
<td>class</td>
<td>school</td>
</tr>
<tr>
<td>talent</td>
<td>diagnosis</td>
<td>resilience</td>
<td>gifted</td>
<td>dysfunction</td>
<td>remediation</td>
</tr>
<tr>
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<td>drug</td>
<td>education</td>
<td>at-risk</td>
<td>smart</td>
<td>dyslexia</td>
</tr>
<tr>
<td>hyperactivity</td>
<td>grade</td>
<td>lazy</td>
<td>work</td>
<td>cheating</td>
<td></td>
</tr>
</tbody>
</table>

Table A. Words from Dissertation Semantic Interview:

Bold-faced: in NVivo’s top 100 words. Underlined: in NVivo’s top 25
and human-generated lists. Seventeen of the 29 words on the original protocol were in the top 100 words from the NVivo search; 12, in the top 25 (see Table A). Many of the differences in the lists can be explained by (1) the origins of my own word list (partly grounded in a literature review of related concepts such as “giftedness”), and (2) inclusion of some common words in the top 25 of NVivo’s list that my system did not exclude (e.g., “new”).

Had I done a WFQ before my interviews, I potentially would have added “problem/s” and “study” to the list of words I asked interviewees to define, and a question asking how “brains” are related to LD and ADD. My results emphasize the point that even a researcher experienced on the topic and in the field can overlook important variables. The use of a dispassionate research tool such as a WFQ can bring terms and questions to light early in the research process and impact the creation of interview protocols and surveys.

In addition, analyzing items on the WFQ-generated lists suggested other lines of inquiry and possible hypotheses. For example, “boys” (95) was used almost twice as often as “girls” (51), and “child/ren” and “kid/s” (938) were used about 12 times more often than “adult/s” (77). There was also a wide array of medically-based terms in the first half of the list. This suggests that there are core cultural beliefs about causation and epidemiology; a researcher could explore whether these are a dominant belief system within the culture or if other understandings exist. While the single-word format of the list could mask other dominant systems of belief, starting points for research emerge from the list that are grounded in data about cultural representations of the disabilities.

The results from Project 2 were less obviously useful. The language of the contract and letter accorded better with each other than with word lists generated by the interviews or the ethnographic team. The team’s list best accords with the interview texts. These results could reflect team members’ emphasis on interviews and observational data rather than on more formal documents. Also, some team members generated terms to be asked of consumers and their identified supports (rather than administrators). Consumers and social supports were the last groups interviewed. Thus a recent effect may have affected list creation,

leading team members to create lists reflecting these groups rather than the broad array of stakeholder groups.

Some words found in the top 100 listing of the data sets (i.e., the contract, letter, and transcripts) were also non-unique items in the team’s list, e.g., “community” and “support.” Additionally, several words were found in the top 100 listing of two of the three data sets and the non-unique items in the team’s list, (e.g., “family”). If a WFQ were performed on literature or samples of naturally-occurring speech from different stakeholder or social groups before interview guides were created, guides could better represent perspectives that are both grounded in the overarching culture and held by multiple interviewee groups. Lists of words to be defined could be included to explore systematic differences in connotations and denotations. Another potential method would be to periodically run WFQs with increasingly large sets of data as it is gathered, to see if the WFQ improves in flagging words and highlighting lines of inquiry for further exploration as the amount of data increases. Such a technique would increase the continuous interaction between data collection and analysis emphasized in GT.

**Conclusion**

The WFQ is a highly promising piece of software that complements ethnographic research. The software does not replace fieldworkers, but can point researchers in new and under-

“The use of a dispassionate research tool such as a WFQ can bring terms and questions to light early in the research process and impact the creation of interview protocols and surveys.”

explored directions. Also, because researchers can analyze texts so early in the research project, analysis can begin before interview and survey protocols are developed. Their development can be informed by this type of initial data analysis. This agrees better with Glaser’s model, making this model more usable for anthropologists.

For the WFQ to be most useful, it is critical that text selection accurately reflects the researcher’s objectives. Someone interested in overarching cultural representations that might serve as “models of” and “models for” beliefs and actions related to the phenomenon of interest (Geertz 1973) could use popular culture texts early in the research process to identify salient cultural frames. A person interested in perspectives of medical personnel would more profitably use the WFQ with articles from medical and related professional journals. A researcher interested in avoiding biases caused by immersion in the professional literature could query popular text sources before beginning a literature review or could query popular and professional texts and compare the lists. Researchers in areas with little or no popular written culture could use samples of naturally-occurring speech collected in relevant contexts or popular
media such as songs. The use of the WFQ feature increases the usefulness of collecting naturally-occurring speech within appropriate contexts, especially when choice of the context has high face validity (e.g., data collected in a teacher’s lounge for a study of LD).

Researchers interested in using GT’s constant comparative method with the smallest possible text size (i.e., the smallest unit of analysis) could code according to a WFQ result list before moving on to longer units. This query takes little time and can be run multiple times in a longitudinal project, providing a “check” for researcher bias and opportunities to see shifts in the data.

Researchers interested in multiple groups might use this method to identify words used frequently by one social group but not others. They could also identify keywords used often by two or more social groups to see if their connotations, denotations, and pragmatics differ systematically.

A researcher who has been in the field for some time could also profit from using WFQs. For example, the connection between ADD and “gifted and talented” is explicit in systems of education, and in interviews and conversations with parents, students, and teachers. However, it is noticeably rare in the articles I analyzed. This could generate additional lines of inquiry, e.g., Why is a dominant educational setting discourse missing in popular media discourses? A seasoned researcher could also this method to ensure that assumptions and biases had not resulted in an important facet of a topic being overlooked.

Professors can use this technique to help students better understand and utilize the principles and techniques of GT and content analysis. Students could create and analyze samples of popular materials or naturally-occurring speech. Using the WFQ results, they could generate hypotheses, interview questions, and themes for coding or content analysis, based on word frequencies and clusters formed within the list.

This feature can also be used to help students become more comfortable with the GT processes of distancing oneself from one’s data while remaining sensitive to meaning and understanding, and of constant comparative analysis. Students can immerse themselves in the WFQ list to compare and contrast terms before moving to larger, more complex units of data to gain familiarity with and confidence in using GT methods. Last, using the WFQ can help students understand how qualitative methods (e.g., interpretive analysis) and quantitative methods (e.g., using counts to guide analysis) can be fruitfully combined in understanding bodies of data.

“...using the WFQ can help students understand how qualitative methods (e.g., interpretive analysis) and quantitative methods (e.g., using counts to guide analysis) can be fruitfully combined in understanding bodies of data.”

References


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In reviewing my own approach to ethnographic methods following attendance at a National Science Foundation supported summer seminar on mixed qualitative-quantitative ethnographic survey methods taught by William Dressler and Kathryn Oths, I have begun to think that the use of the terms “qualitative” and “quantitative” distracts from rather than facilitates scholarly communication and would be better replaced by an emphasis on “interpretation” and “measurement.”

There is neither pure quantitative nor pure qualitative research. There is always a qualitative element in quantitative work—you might be counting things, but the choice of what’s relevant to count is an inescapable qualitative decision. Likewise, there is always a quantitative element in any qualitative research, even if only in the rudimentary sense that it makes a difference whether there is a lot of something or only a little, whether something is always occurring, occurs every day, or once a year.

Many social scientists, when asked, pay lip service to this notion that there is no pure quantitative or qualitative research, but then go on acting as if there were. This is done largely uncritically and at least partly out of mutual contempt for number-fetishizing quantitative types and muddle headed, fuzzy thinking qualitative types. Many scholars of both stripes are committed to particular research methods they have typically been trained to use, e.g. survey driven quantitative work and participant-observation heavy qualitative work, without necessarily thinking through whether their chosen methods are really best suited for the research question at hand. If we chuckled the qualitative and quantitative labels, perhaps we could better focus on things that all decent research has in common: measurement and interpretation.

**Measurement:**

**There is no Immeasurable**

A lot of “qualitative” social scientists, including most cultural anthropologists, tend to be wary of “quantitative” research because they perceive it as ignoring things that are not easily counted and uncritically or simplistically counting things that seem easy to count. Frankly, a lot of “quantitative” work does do these things, though there are also a lot that do not. What could be better recognized by some quantitative types is the interpretive nature of choosing what it is important to count, but what qualitative types could recognize is that we are all engaged in measurement. There are phenomena that are not easy to count, but there are no observable phenomena that are not measurable.

There are different sorts of measurement. Some things can be measured only in fairly basic and imprecise terms—the binary measurement of the simple presence or absence of a phenomenon or trait, or rough measurement of quantity, e.g. something is absent, present in small quantity or frequency, or present in high quantity or frequency. Other things can be very precisely measured. So, highly “qualitative” ethnography involves measurement just as much as the most “quantitative” of studies. Once we recognize that we are all involved in measuring, we all ought to measure things as precisely as possible—sometimes that might involve quantification and in other cases might simply involve notation of the presence or absence of something. There is no reason to be wary of measurement, but good reason to be wary of measurement that is less precise than it reasonably could be or purports to be more precise than it can be.

**Interpretation: What’s the Significance of Statistical Significance?**

As with measurement, all research involves interpretation whether we realize it (or like it) or not. I alluded above to the interpretive quality of measurement—knowing what it makes sense to measure is an interpretive maneuver. Further, it is always necessary to interpret the results of measurement. Just as quantitative researchers are often more aware than qualitative of the need to measure, qualitative researchers are often more aware of this fact than quantitative. Measurements, and even basic analyses, alone never mean anything, e.g. an analysis indicating statistical significance of a set of data does not indicate at all what
the meaning of the data is, but simply that it is unlikely that that particular assemblage of data resulted from random chance whatever it might mean. There is no reason to be wary of interpretation, but good reason to be wary of uncritical interpretation not based on sound logical argumentation and good measurement or of interpretation by those not aware they are engaging in interpretation.

Rethinking Ethnographic Methods

What the shift to viewing research through the lens of “measurement” and “interpretation” has done for me is to free up my thinking about research methods in relation to styles of scholarly thought. Like many cultural anthropologists, my ethnographic methods training was mainly in highly qualitative participant observation—which is a fine methodology to use for many research purposes. For example, for my dissertation I studied the practices of HIV prevention organizations along the U.S.-Mexican border. For that research, participant observation served me quite well—in fact, for the particular research context I cannot think of another method more richly detailed, useful, or even efficient for answering my research questions. But now, rather than approaching any topic as a “qualitative cultural anthropologist” and attempting to fit participant observation to any and every topic at hand, I have begun to think of myself more as a researcher with anthropological interests and to ask myself what methods will work best and how I might best measure and interpret data for a specific research question.

Cultural Models of Drinking

These findings are drawn from an ongoing research project I am conducting with Dr. Debra Vinci, Department of Health Education at the University of West Florida. In this project we are interested in students’ cultural models of drinking and related activities, contexts, and concepts, and we are interested in this as a topic of interest in its own right and as an applied anthropological subject, where we hope that our research will contribute both to a safer campus for students and efforts to reduce risks of dangerous drinking patterns among students by helping to elucidate the models and patterns which reflect and shape students’ thinking about drinking. First, what do I mean by cultural models? And Second, what do I mean by drinking and related activities, contexts, and concepts?

By cultural model I suggest something along the lines of what Sherry Ortner (1989) discusses as cultural schemas. In her discussion, she argues that culturally significant schemas are built up out of important cultural symbols. Anthropologists have long focused on symbols as something that makes humans unique and as the basic building block of culture, though this alone doesn’t explain how culturally important symbols are related to one another nor to practice. C.S. Peirce’s discussion of the argument as one particularly complex type of symbol built up out of more basic symbols is useful here. The argument, as Peirce defines it, “is a sign whose interpreter represents its object as being an ulterior sign through a law, namely, the law that the passage from all such premises to such conclusions tends to the truth.” Or, as he puts it elsewhere, the argument is a sign of reason, building upon propositions to enact overarching logical systems (which is to say that argument involves theorization broadly understood) and is always composed of simpler symbols (specifically schematic and dicent symbols; see Peirce 1992 for detailed discussion of his typology of signs).

Cultural, I would argue, is not just the learned and shared lifeways of minimalist definitions of culture, but also an encompassing mesh of symbols, premises, and arguments. The argument, for human culture, is akin to what Ortner calls key scenarios or cultural schemas. She defines these “as preorganized schemes of action, symbolic programs for the staging and playing out of standard interactions in a particular culture. In her own analysis of Sherpa Buddhism in Nepal, she identifies such a cultural schema (Rivalry, Acquisition of a Protector, Departure of the Loser) which recurs in Sherpa myth and ritual and which provides a prototype for culturally typical interaction situations—which is to say that (using Clifford Geertz’s terminology; 1973) cultural schemas or arguments provide both “models of” and “models for” cultural action, and further that culturally significant arguments or cultural models are grounded in practice and simultaneously function to ground practice.

What do I mean by cultural models of drinking and related activities, contexts, and concepts? Essentially, we are interested in ascertaining the basic assumptions and premises of students’ conceptualization of drinking, places associated with drinking, behaviors typically associated with drinking, and notions of responsibility or irresponsibility with relation to drinking. Further, we are interested in how such basic premises are related and combined to form larger arguments or cultural models.

There are a variety of methods and techniques that could have addressed this research topic, such as participant observation or the collection of life history narratives, but given the primary interest in understanding the key terms and premises of students’ models and conceptualizations of drinking, methods that could directly elicit such material seemed clearly most useful.

Sample and Methods for Initial Data Collection

The first stage of this project (and the one which I will report on here) involved the collection of free lists from 101 students from three classes at the University of West Florida (UWF) during the Fall 2006 semester, two sections of Introduction to Anthropology of 46 and 33 students. This class was chosen for convenience, but also because of the representation of many different student major interests in the classes. The project also involved one section of an upper level “Nutrition and Health” course of 22 students; this class was
chosen again for convenience, but also for contrast—this was a course with mainly upper division students with an interest in nutrition and health, and who had already been asked in a variety of ways to think critically about health and nutrition, including alcohol-related issues, in class. Students were asked to generate five free lists: (1) types of alcoholic drinks or beverages; (2) types of places or settings in which people drink; (3) activities people engage in when drinking; (4) the characteristics of someone with a drinking problem; and (5) the characteristics of someone who drinks responsibly.

**Interesting Trends and Reactions**

**Drinking Discourse and Drinking Behavior**

Overall, the results of the first list are consistent with the findings of two other surveys that have been conducted on the UWF campus that indicate that the vast majority of UWF students either do not drink or drink infrequently and in low quantities (or at least claim to not drink or not drink in high frequency or quantity). The relationship between students’ discourse about their drinking and their drinking behavior (or lack thereof) is clearly something difficult to ascertain, though this free-listing exercise offers at least a slightly different window on the situation. A small handful of students provided us extremely comprehensive lists of drink types (so, in fact much of the variety of items listed came from just a few students), but the rest provided much shorter lists (generally 10—15 items at most) with mostly general categories of drink that many non-drinking students would probably be familiar with from simply having grown up in the culture. This, of course, is still another example of drinking discourse and doesn’t prove that most UWF students in fact do not drink or do not drink much (i.e. the lists don’t indicate clear lack of familiarity with drinking, but they also don’t indicate clear familiarity with drinking), but it also is consistent with those other findings and doesn’t give any contradiction to students when they respond to surveys indicating low frequency and quantity of drinking. These free lists offer a window into the distribution of cultural knowledge, with a small number of students indicating high degree of familiarity with specialized knowledge and a much larger number of students reporting many fewer items that were more thoroughly shared throughout the sample.

**Drinking and Food**

This and the following examples address the relationship between public health discourse and students’ discourse in the ways in which students’ conceptions and expressions of thought on drinking correspond (or do not) to institutional messages. (By institutional messages I simply mean discourse about drinking coming from institutions like UWF, such as public health information provided on posters or pamphlets that students might encounter.)

In our study, many students associated drinking with eating. Fourteen students mention eating as an activity associated with drinking (with three more mentioning dinner), and “eating contexts” were even more associated with drinking—with restaurants mentioned by 25 students as a setting associated with drinking (and 11 more mentioning “at dinner” or “at a dinner” as settings). At the same time, “eating before drinking” was listed by only two students as an attribute of responsible drinkers (with both in the upper level nutrition and health course). This is not so surprising given the (understandable) greater emphasis in public health campaigns on not drinking and driving, though at the same time, eating before and during drinking is often mentioned in public health campaigns, brochures, etc., as one strategy to reduce potential dangers of over-consumption of alcohol. This pattern of associating eating with drinking in one set of domains, but not within the context of thinking of responsible or appropriate behavior, is possibly the result of one message being swamped by another (and in this case more important) one.

**Drinking and Sex**

Students also associate sex and drinking. Aside from dancing, sex was...
mentioned by the largest number of students as an activity associated with drinking. At the same time that sex is clearly part of the model of drinking generally, responsible sexuality does not seem to be a significant part of students’ model of responsible drinking. “Promiscuity” was mentioned by one student as a trait of those who drink irresponsibly, and one student mentioned “has condoms” as a characteristic of someone who drinks responsibly. This, and the previous example to a lesser extent, is indicative of the fact that as we engage in further research to fine-tune our understanding of students’ cultural models of drinking, we will almost certainly be dealing not with a single or unitary model, but multiple overlapping models. For example, students’ models that not driving drunk and/or having a designated driver are responsible thinks to do when drinking: “Doesn’t drink and drive” (36 students), and “chooses a designated driver beforehand” (24 students) were common responses.

When it comes to what makes a person irresponsible, however, drunk driving seems to have fallen out of consideration—only five students mentioned anything to do with drunk driving or not having a designated driver as being a characteristic of someone drinking irresponsibly.

Ongoing Research

The goal of this first stage of research was to elucidate the common terms of students’ models of drinking, which it has done. A second stage of research will ascertain relationships between the terms to understand the shape of the larger model(s). Those items commonly listed in the free lists were used to produce cards (with one item listed on each card) to be sorted by another sample of 30 students, with students in my Applied Anthropology course conducting this research. The results from this stage of research are still being tabulated and will be analyzed using non-metrical multi-dimensional scaling techniques to provide a two-dimensional graphic representation of the relationships between sorted terms. This in turn, along with the results of another ongoing survey, will be used to develop a more finely tuned survey to ascertain degree of cultural consensus on various elements of the model.

Concluding Thoughts

Like many ethnographic anthropologists, as a graduate student I was trained in the use of highly qualitative research methods. Moving beyond a priori commitments to either qualitative or quantitative orientations or to particular research methods or tactics has a liberating effect. For me, in this instance, I was interested in eliciting the specific terms of students’ understandings of drinking, related contexts, and behaviors. Rather than falling back on what I’ve always done before and turning to participant observation combined with open-ended qualitative interviews (a fine, rich, and useful way to study some things), I used a set of methods that, in the specific research context, were a far more efficient use of my time and got at what I wanted to study much more directly and precisely.

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Geertz, Clifford

Ortner, Sherry

Peirce, Charles S.

Robert Philen is an Assistant Professor of Cultural Anthropology at the University of West Florida. He is interested in ethnographic theory and methods. Much of his research focuses on health and culture, as well as the relationship between discourse and practice. He writes a blog related to anthropology and culture commentary, “Robert Philen’s Blog,” http://robertphilen.blogspot.com/.
By Emily Stovel

My experience attending two NSF Short Courses on Research Methods in Cultural Anthropology prompted me to use free list and pile sorting activities as teaching exercises with interesting and informative results that could be useful for others grappling with teaching research methods to undergraduates. My immediate goal in attending the course was inform my teaching of anthropological research methods. However, I also gained a new pedagogical tool that I have implemented in a wide range of classes to help students experience and understand the various qualitative and quantitative methods undertaken in social science research, while also redirecting their role as passive students to active scholars. The following paper outlines the implementation of this Museum Studies’ free list and pile sorting exercises, considers the resultant data and impact on students in their own words, and provides some caveats for future implementation.

In the fall of 2007, I developed and implemented classroom activities that used free-listing and pile sorting in both of my 100-level classes: Introduction to Anthropology and to Museum Studies. Here I focus on the Museum Studies course. In this exercise, student engagement with these methods and resulting data catapulted them from undergraduates into a role as museum employees. This allowed students to reflect on “the public” thinks about museums and how to manage those ideas, even though they had provided the data for the student and thus constituted “the public” they were analyzing. This type of structured reflection is an important step in helping students recognize commonly shared preconceptions and breaks the usual North American focus on the individual self. Students see opinions as their own, mostly untouched by cultural mediation, and this can lead to the idea that people in other cultures are more embedded, less able to think independently. When they capture the shared nature and structure of their views, they see themselves as cultural beings, think more powerfully about culturally mediated perceptions (such as negative views of museums), and begin to see the internal variation in other communities to undermine generalizations and encourage dialogue.

With respect to Museum Studies, then, students begin to reflect on generally held ideas about museums rather than serving specific interest groups (such as historians, school groups, etc.). Or in other words, students can think about developing a dialogue with public perceptions of museums rather than simply serving presumed patron groups, and thus take a more active role in defining and redefining what a museum is. This ultimately leads to students taking a more engaged role in their response to course materials. To be sure, this exercise enlivened the class and set the tone for the rest of the semester by moving us away from traditional lecture-based formats from the very first day of class onward.

The First Day of Class

The first day of my new Introduction to Museum Studies course I began by asking students to give examples of their experiences in museums, and explaining the nature and contributions of free-listing and the concept of cultural domains. I then asked students to list all terms that come to mind when asked “What is a Museum?” This class of approximately 30 students provided 258 terms, 215 (83%) of which were provided only by individuals. The students averaged 7.8 terms per

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The first day of my new Introduction to Museum Studies course I began by asking students to give examples of their experiences in museums, and explaining the nature and contributions of free-listing and the concept of cultural domains. I then asked students to list all terms that come to mind when asked “What is a Museum?” This class of approximately 30 students was populated by a wide range of undergraduate students from all class levels. Our Museum Studies program is still nascent and thus attracted interested students from many departments who sought to take advantage of the new 100-level course. Some sample free-list answers are provided in Table 1. I then solicited some of their terms to put up on the blackboard and facilitated a discussion on the different ways the public perceives museums, using the students’ free-list items as representative of the views of a generic public. We thus treated the concept of Museum as a cultural domain and, through mutual consideration of the terms generated, were able to identify unexpected reoccurring concepts within the domain such as: superiority, cold, and elitism as well as some contradictory pairs: fun/boring, beautiful/odd, interactive/dusty.

Results

In all, 33 respondents provided 258 terms, 215 (83%) of which were provided only by individuals. The students averaged 7.8 terms per

Table 1

<table>
<thead>
<tr>
<th>Free-list Terms</th>
<th>Frequency</th>
</tr>
</thead>
<tbody>
<tr>
<td>Superiority</td>
<td>12</td>
</tr>
<tr>
<td>Cold</td>
<td>10</td>
</tr>
<tr>
<td>Elitism</td>
<td>9</td>
</tr>
<tr>
<td>Fun</td>
<td>18</td>
</tr>
<tr>
<td>Boring</td>
<td>15</td>
</tr>
<tr>
<td>Beautiful</td>
<td>14</td>
</tr>
<tr>
<td>Odd</td>
<td>12</td>
</tr>
<tr>
<td>Interactive</td>
<td>16</td>
</tr>
<tr>
<td>Dusty</td>
<td>13</td>
</tr>
</tbody>
</table>

In conclusion, the use of free list and pile sorting activities as teaching exercises has been both informative and interesting, providing students with a new perspective on museum studies and the ways in which people think about museums. The results of these activities suggest that students are engaged and ready to think critically about the role of museums in society, and that they are able to reflect on their own experiences in museums as a way to understand the experiences of others.
Table 1. Sample Free List Terms in Answer to the Query “What is a Museum?”

<table>
<thead>
<tr>
<th>Item</th>
<th>Frequency</th>
<th>Percentage of Respondents</th>
<th>Average Rank</th>
<th>Smith’s S</th>
</tr>
</thead>
<tbody>
<tr>
<td>ART</td>
<td>19</td>
<td>58</td>
<td>3.42</td>
<td>0.467</td>
</tr>
<tr>
<td>HISTORY</td>
<td>15</td>
<td>45</td>
<td>3.47</td>
<td>0.344</td>
</tr>
<tr>
<td>EXHIBIT*</td>
<td>10</td>
<td>30</td>
<td>2.8</td>
<td>0.258</td>
</tr>
<tr>
<td>CULTURE</td>
<td>9</td>
<td>27</td>
<td>5.56</td>
<td>0.151</td>
</tr>
<tr>
<td>QUIET</td>
<td>6</td>
<td>18</td>
<td>6.5</td>
<td>0.104</td>
</tr>
<tr>
<td>LEARNING</td>
<td>5</td>
<td>15</td>
<td>7</td>
<td>0.068</td>
</tr>
<tr>
<td>SCIENCE</td>
<td>5</td>
<td>15</td>
<td>4.4</td>
<td>0.109</td>
</tr>
<tr>
<td>ARTIFACTS</td>
<td>5</td>
<td>15</td>
<td>6.6</td>
<td>0.085</td>
</tr>
<tr>
<td>PRESERVATION</td>
<td>4</td>
<td>12</td>
<td>10.5</td>
<td>0.035</td>
</tr>
<tr>
<td>OLD</td>
<td>4</td>
<td>12</td>
<td>5.75</td>
<td>0.084</td>
</tr>
<tr>
<td>COLLECTIONS</td>
<td>4</td>
<td>12</td>
<td>6.5</td>
<td>0.068</td>
</tr>
<tr>
<td>TOURS</td>
<td>4</td>
<td>12</td>
<td>7.75</td>
<td>0.055</td>
</tr>
<tr>
<td>INTERESTING</td>
<td>4</td>
<td>12</td>
<td>3.75</td>
<td>0.088</td>
</tr>
<tr>
<td>DINOSAURS</td>
<td>3</td>
<td>9</td>
<td>3.67</td>
<td>0.069</td>
</tr>
<tr>
<td>EDUCATION</td>
<td>3</td>
<td>9</td>
<td>6.33</td>
<td>0.04</td>
</tr>
<tr>
<td>SCULPTURES</td>
<td>3</td>
<td>9</td>
<td>11.33</td>
<td>0.033</td>
</tr>
<tr>
<td>FUN</td>
<td>3</td>
<td>9</td>
<td>8.67</td>
<td>0.047</td>
</tr>
<tr>
<td>BORING</td>
<td>3</td>
<td>9</td>
<td>3.67</td>
<td>0.053</td>
</tr>
<tr>
<td>TOUR GUIDES</td>
<td>3</td>
<td>9</td>
<td>5.67</td>
<td>0.063</td>
</tr>
<tr>
<td>COLD</td>
<td>3</td>
<td>9</td>
<td>6.33</td>
<td>0.062</td>
</tr>
</tbody>
</table>

* collapsed with “exhibits”

Table 2. Top 20 Free List Terms for “What is a Museum?”

person. There is a certain degree of coherence to this cultural domain in that frequency and rank are somewhat associated for the three or four top terms which constitute the core of the domain for a large portion of the sampled population. Table 2 provides a list of the top twenty terms in descending order of frequency of occurrence. Charts 1 and 2 provide a visual representation of the same data, demonstrating the existence of a core of key concepts on the left hand portion of the graphs including: art, history, exhibit, culture and an additional series of less common terms including: quiet, learning, science, artifacts. The line of the frequency chart is low and quite smooth. There is not obvious break between the general core of concepts and the remaining low frequency, idiosyncratic terms.

The coherence of the central series of terms, however, seems supported by the average rank or relative salience of the terms provided. As we can see from Chart 2, art, history, exhibit, and culture share relative high frequencies and somewhat high average rank (3 - 5). Terms such as quiet, learning, science, and artifacts not only show lower average ranks (4 – 7), but there is more variation in the ranking of these terms and beyond, suggesting that they are not only mentioned by fewer but have more idiosyncratic salience for the participants, thus do not form part of the central core of concepts belonging to the cultural domain shared by all.

The average number of terms per person (7.8) is high in comparison to results produced in other introductory anthropology classes (results in preparation), indicative perhaps of the wider range of student ages and experience (several participants had studied abroad and/or are upper-year students) and enhanced commitment to the subject matter. Like that of many institutions, the general student body at my institution attends 100-level anthropology classes at first to fulfill general education requirements as they have had little exposure to the discipline. The Museum Studies students from the class sampled were more likely to be sophomores or above and actively engaged in museum studies or thinking about museums and anthropology.

The Second Day of Class

During the next class, I re-introduced the topic “What is a Museum?”, presented the quantitative analysis provided above to the students, and formed student working groups. These groups were charged with sorting the terms previously provided in the free-list exercise with the goal of identifying students’ perceptions of museums and what this reflects of widely-held cultural templates concerning the concept of the museum. Each group sorted 258 terms, identifying and collapsing singular and plural forms of the same words as they went.

Students discussed, and at times struggled, for the next hour and a half and came up with sets of categories that...
summarized and collapsed all free-list items. The goal of this exercise was to have students understand their own underlying assumptions about museums as members of the public (i.e., consumers of museums). Finally, I asked them to complete a survey about the pedagogical effectiveness of the exercise (results in Table 4).

**Results II**

Many questions remain. We did not have time to compare pile lists or wrestle with synonyms. Students, however, discussed the realization that cultural domains are structured and that everyone carries such organized logical ideas around in their heads whether they agree with them explicitly all the time or not. This was a profound realization for my students. It exposed them to differing perceptions of museums, allowed them to interact with new classmates, argue and break-out of passive learning modes, and discover the breadth of knowledge on the topic they already had. It pulled them from the realm of personal feelings and opinions, and asked them to see themselves as part of a community. They were intrigued by the structured nature of the shared understanding and the differences evident in the cultural domain.

**Feedback from Students**

Some students were frustrated with the excessive number of terms they had to sort and recommended culling the data set for synonyms beforehand. Others disagreed with their group mates about the membership of terms and method of subdivision. In the future, a collective eradication of agreed-upon synonyms would lead to further understanding of underlying concepts while whittling down the data set to a more manageable amount for sorting. Students could even carry out their own free-list and pile sorting exercises with their peers outside of class. In general, however, students were very positive about their experience, favored the collaborative nature of the exercises, and enjoyed exploring their own data as evidence of public opinions.

Students express interest in the implementation of the exercise in that it seemed to hamper clarity for some. In subsequent iterations, I will collapse synonyms in collaboration with students, thus reducing the number of terms to be sorted, and ensure there is time to discuss the results of the pile sort. I would add a third class to the sequence and go over the similarities and differences in categories, explore the major debates that
<table>
<thead>
<tr>
<th>Group 1</th>
<th>Group 2</th>
<th>Group 3</th>
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<tbody>
<tr>
<td>Adjectives</td>
<td>Business aspects</td>
<td>Creation of exhibits</td>
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<td>Education</td>
<td>Focuses of museums</td>
<td>Culture</td>
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<td>Experiences</td>
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<td>Issues</td>
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<td>Feelings they provoke</td>
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<td>People</td>
<td>Museum people</td>
<td>Financial</td>
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<td>Physical structure attributes</td>
<td>People</td>
<td>Locations</td>
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<tr>
<td>Places where museums are</td>
<td>Physical aspects</td>
<td>Negatives provoked or complaints</td>
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<tr>
<td>Possible collections objects in museum</td>
<td>Physical characteristics of museums</td>
<td>People</td>
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<tr>
<td>Rules authority</td>
<td>Points of view</td>
<td>Physical aspects</td>
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<td>Types of museum</td>
<td>Politics of museums</td>
<td>Related to actual physical building</td>
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<td></td>
<td>Reasons for visiting</td>
<td>Signs</td>
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<td>Representation</td>
<td>Things exhibits can be made up of</td>
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<td>Restrictions</td>
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<td>Specific places</td>
<td>Travel</td>
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<td>Subjects within a museum</td>
<td>Types of museums</td>
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<td>Things done</td>
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<td>Group 5</td>
<td>Group 6</td>
<td>Group 7</td>
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<td>Complaints</td>
<td>Concepts associated with museums</td>
<td>Adjectives</td>
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<td>Controversy</td>
<td>Departments of a museum</td>
<td>Atmosphere</td>
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<td>Descriptions</td>
<td>Frustrations</td>
<td>Business administration</td>
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<td>Education</td>
<td>Funding</td>
<td>Education</td>
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<tr>
<td>Functions</td>
<td>Group activities</td>
<td>Famous people</td>
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<tr>
<td>Ideas</td>
<td>Intellectual words educational</td>
<td>Locations</td>
</tr>
<tr>
<td>Kinds of museums</td>
<td>Navigational tools</td>
<td>Maintenance</td>
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<tr>
<td>Objects people exhibits</td>
<td>Objects found in museums</td>
<td>Parts of a museum</td>
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<tr>
<td>Restrictions rules</td>
<td>Parts of a museum</td>
<td>People contributors audience</td>
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<tr>
<td>Structure</td>
<td>People who work in or visit museums</td>
<td>Signs</td>
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<td>Travel</td>
<td>Places</td>
<td>Things in exhibits</td>
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<td>single word descriptions</td>
<td>Types of exhibit</td>
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<td>Specific people events</td>
<td>Types of museums</td>
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<td>Time related</td>
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<td>Types of museums</td>
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Table 3. Student Group Categories (one group omitted due to illegible results)

erupted, and begin to diagram the components of the Museums cultural domain in class.

Conclusions

I used the pile sort exercise to demonstrate that although the free-list exercise included many useful ideas, there were key topics, issues, and characteristics missing. By engaging the students in this exercise, I hoped to help students identify what they already knew and expose them to gaps in that knowledge, thus facilitating their ability to recognize and incorporate new material. At the end of the exercises, I clarified that one of the goals of the class was to shift their extant perceptions, help organize them, and demonstrate complementary and conflicting understandings of museums. This led me to a lecture on the currently accepted types and services of museums defined by the discipline which was somewhat broader and less contradictory than the student perceptions. Moreover, we were able to discuss the negative conceptualization of museums (i.e., boring, dusty, intellectual, irrelevant, old fashioned) in terms of future museum employment and as the reflection of a general anti-intellectualism in the student body. Our class bonded strongly around the exercises and students were subsequently more engaged and active in the exploration of course material from this point forward. These exercises demonstrated how important cultural understandings are
Table 4: A Sample of Students Comments on the Pile Sort Activities

<table>
<thead>
<tr>
<th>A Sample of Positive Student Pile Sort Evaluations</th>
<th>Sample of Negative Student Pile Sort Evaluations</th>
</tr>
</thead>
<tbody>
<tr>
<td>• Didn’t know there were so many different perceptions people have.</td>
<td>• It made me feel overwhelmed.</td>
</tr>
<tr>
<td>• Now have a greater understanding of society’s perceptions and mental constructs of museums.</td>
<td>• Dozens of words being thrown at me without any understanding of where they should go.</td>
</tr>
<tr>
<td>• Good interactive way to get us involved in our own education.</td>
<td>• Helpful to compare with other age groups or cultures to see differences.</td>
</tr>
<tr>
<td>• Really makes you think and actually be aware of what you are learning.</td>
<td>• My group spent more time arguing over categories than analyzing the cultural basis for said categories. It was, at best, chaotic and superficial. But I can see it was meant to be a reflection of our own perceptions, but this didn’t happen. True, we had a good time doing it, but I can’t help thinking that I am missing out on some grand point.</td>
</tr>
<tr>
<td>• Interesting to learn how our class views museums.</td>
<td>• Frustrating group work.</td>
</tr>
<tr>
<td>• It forced you to argue and defend why you would place a certain word in a category.</td>
<td></td>
</tr>
<tr>
<td>• The activity made me think of how we as a group and society already have an understanding of how we organize things, without actually vocalizing it.</td>
<td></td>
</tr>
<tr>
<td>• Interesting to see which words I had trouble categorizing.</td>
<td></td>
</tr>
<tr>
<td>• Better than lectures to make a point.</td>
<td></td>
</tr>
<tr>
<td>• Opened my eyes to how other people feel about museums that I don’t necessarily agree with or feel the same way about.</td>
<td></td>
</tr>
<tr>
<td>• I really enjoyed discovering that I share more ideas with my classmates than I thought. It also provided a means for our class to bond.</td>
<td></td>
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</table>

created, interpreted and recapitulated by all the actors involved, including students, patrons, museum staff, and finally the class as a whole as they undergo this analysis. We witnessed how knowledge is distributed across a body of people and how actors are engaged at various levels in its production, storage, and transformation. We moved from a static world of facts describing specific institutions (i.e., What is a museum?), to a dynamic and active set of mediated ideas; a set of ideas students could understand and potentially change.

I prefer explicit discussion with and between students about how learning occurs. In class, I show them how they can evaluate their own changing perceptions, encourage them to become more concerned with their own process of learning, and explore how college level courses should be dialectical rather than unimodal experiences. In my previous experience, many students find this idea of learning challenging; some even see it as even anxiety provoking, because it lacks a single accepted answer to a question. This, however, is a common product of anthropology classes: to move students beyond their accepted roles and cultural spaces. This cultural displacement, which causes students to explore their own cultural embeddedness, also frees them to change, relate to other cultural paradigms more easily with practice, and incorporate new ideas as conscious learners. The active exercises employed in my introductory classes in 2007 represent one small part of a process students hopefully wrestle with throughout their college careers.

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USING CULTURAL CONSENSUS ANALYSIS TO IMPROVE A SAFETY CAMPAIGN FOR FARMWORKERS

By Paul Monaghan

Introduction

For the past four years, researchers at the Florida Prevention Research Center, along with community partners that include the Farmworker Association of Florida, and the Collier County Health Department and several large citrus companies, have been evaluating an eye safety program targeted at citrus harvesters. We have used a variety of field methods to inform the development of a social marketing campaign that will attempt to convince workers to wear safety glasses when they pick. It is difficult to convince workers to wear safety glasses because many believe it slows down the rate of picking, which has an impact on earnings. We have achieved the most success so far by using trained community health workers called “promotores” who are citrus pickers themselves and who work alongside their peers, dispensing safety glasses, advice and first aid. In our research, we found that harvesting crews that have one of these “promotores” have increased their use of safety glasses from zero to over 30%.

While our research has discovered many reasons why pickers do not wear the glasses, it has been more difficult to identify exactly why the 30% who have adopted wearing the safety glasses have done so. This paper will review some of our findings on the ethnography of citrus work and propose using the data to create domains to model cultural consensus on issues such as self-efficacy, risk, and comfort. We hope that by discovering what motivates some citrus workers to begin using eye protection in the groves, we can develop an effective social marketing campaign to increase the percentage of workers who opt to wear safety glasses.

Background

The Partnership for Citrus Worker Health (PCWH) in southwest Florida is a community-university collaboration involving the University of South Florida’s College of Public Health, the Farm Worker Association of Florida (FWAF-a statewide advocacy group) and a community advisory board which is composed of employers, health professionals, and citrus workers. As part of the intervention conducted by the project, there is also a network of more than 30 trained community health workers (CHWs) promoting injury prevention and providing first aid in the citrus groves for hundreds of harvesters in the region. The research team of community partners (university researchers, board members, employers and FWAF organizers) has collected data on the CHW intervention and conducted repeated surveys with more than 500 workers.

The overall strategy of the PCWH is to combine the methods of social marketing with the skills and knowledge of the community members. Working alongside our community partners and citrus harvesters, we have used a variety of research methods to collect ethnographic data on “picking,” including worker perceptions of risk and safety. Citrus workers have detailed knowledge of at least two dozen ways to receive an eye injury in the groves. They understand changing conditions from one day to the next and from one grove to another. From our research with workers, we understand the reasons for not wearing safety eyewear while picking in the hot, humid, dusty environment. Safety glasses can be uncomfortable due to fogging lenses and the increased heat around the face and as a result, none of citrus companies we work with have been successful at changing worker behavior. We are reasonably certain that we have been successful in influencing behavior change, but we are not sure how workers overcome the barriers and make that decision to change their behavior. Cultural consensus analysis provides one possible model for understanding how citrus workers calculate risk, measure the discomfort of wearing the glasses compared with the discomfort of injuries, and determine their sense of self-efficacy in avoiding injuries. These data could then be incorporated into making our social marketing campaign more effective by appealing to a worker’s decision-making process.

The Dangers of Citrus Harvesting

Citrus harvesting is one of the most physically demanding jobs for agricultural laborers in Florida. Pickers stand on an 18-foot aluminum ladder that is leaning on the branches deep inside the canopy of an orange tree. Starting at the top of the ladder, with a large canvas bag across one shoulder, they descend, snapping off oranges using both hands and filling the bag. A full bag can weigh 90 pounds. Once on the ground, the picker drags the full bag over to a large bin, lifts it to the edge, and empties it. Each bin holds up to 600 pounds of fruit and, when full, can be worth $9.00 to $15.00, depending on the set price that day. An experienced worker can fill 8-10 bins (more than two tons of oranges) in a day and sometimes more if the fruit is plentiful and easy to harvest. By way of comparison, citrus pickers make a higher average wage than vegetable harvesters in the same area.

The relatively high wages for pickers come at a cost: injuries are frequent and can be quite serious when they involve a fall from the ladder. In focus groups and surveys, workers have reported falling from the top of the ladder with the bag around their neck, twisting their ankles by missing the bottom step.
of the ladder, muscle cramps in their hands, pain in their feet and knees from standing and leaning on the steps of the ladder all day, eardrum punctures from branches, and headaches and respiratory symptoms from the smell of chemical residues.

The most common injuries to citrus pickers, however, are not always considered the most serious, and these include bruises, abrasions, foreign objects in and general irritation (redness, burning and tearing) to the eyes. Our research shows at least a quarter of all citrus pickers experience these injuries, a finding similar to other studies of agricultural workers. Yet for the most part, these more common injuries are not reported as on-the-job accidents to supervisors or employers, or if they are reported, there is often a delay and it is unlikely the worker is examined or treated at a clinic. Citrus pickers are expected to bear the burden of the pain and possible long-term suffering of most eye injuries as just a part of the job. In focus groups, the question, “How does one avoid eye injuries while picking?” was often answered with laughter and the advice, “Don’t work in citrus.”

The general attitude towards eye injuries as less serious than other injuries or an accepted part of the business of picking oranges, is influenced by the way pickers are paid. Pickers are paid by the piece rate system, not by the hour. The faster they work, the more they make. Yet if they are slow and do not pick enough to equal the minimum hourly wage, they will eventually be let go. The crew leaders are also paid by the rate at which pickers work, receiving a percentage for how much the crew picks each day. The crew leader on a citrus crew often makes 2 to 3 times the average daily wages of a harvester. If a worker is slowed down or stops work for an injury, both he and his crew leader will invariably lose money.

**Research with Citrus Workers**

Research began in late 2002 with a team composed of an anthropologist, several graduate students bi-lingual in Spanish, and community members. Surveys, focus groups, and participant observation were conducted in workers’ homes and in the orange groves. The questions focused on the risks to health from citrus work, experiences with eye injuries, and perceptions of the usefulness of wearing safety glasses. A limitation of this approach was that workers had rarely tried wearing safety glasses but had already formed an opinion about not wanting to wear them because of the potential for increased heat and fogging.

Our community partner (FWAF) recruited participants for focus groups that were conducted in the yards and homes of workers in the evenings and on weekends. According to most of the 56 focus group participants, eye injuries, including scratches and foreign objects, are accepted as part of the job of citrus work and are usually treated by workers themselves. Most participants did not report their eye injuries to supervisors. For those who sought clinical treatment, some did so on their own. When questioned about the use of safety glasses, most had a negative opinion about them although they had never actually tried picking while wearing them. A frequent response was that picking by the piece rate was incompatible with safety glasses, as it slowed down the rate of picking. Safety glasses, they said, would fog up and get dirty in the orange groves. If they had to stop and clean them, it would cause them to slow down and earn less money. Many workers thought that safety glasses could be worn if workers received an hourly wage.

Focus groups, along with key informant interviews in the citrus industry and participant observation in the orange groves, provided a good ethnographic description of citrus work and the hazards for eye injuries. The two main risks for eye injuries can be divided into the foreign objects that fall in the eyes and cause scratches or irritation, and the numerous ways the branches and leaves can lacerate or bruise the eyes and cause trauma.

While the trauma injuries are less common, workers recognize the many risks that contribute to them. A strike or poke to the eyes (golpe, piquete, or ramalazo) can be caused by many conditions: branches that snap back from behind the ladder, blunt tipped branches from trees that have been trimmed, older trees with brittle branches that are less flexible, and trees that are spaced closely together or have dense undergrowth, making it difficult to walk around while carrying the heavy bag. In addition, trauma can also result from slips and falls from the ladder and ladder instability when it is not securely placed in the soft ground or in older groves with taller trees that require the picker to extend his body beyond his balance.

The more commonly experienced injuries of foreign objects lodging in the eye are caused by a variety of factors that are much more difficult to avoid. There is polvo (dust) kicked up by the trucks driving through the groves and coating everything until it is released by the snap of an orange off a branch, basura (trash) a generic term for mold, mildew and dirt from the leaves and branches, espray (residue) from the chemicals, pesticides and solvents sprayed on the trees, the granular Florida sand that sticks to gloves and shoes and the steps of the ladder, citrus flowers and pollen that dry out and crumble into dust, and a variety of insects--gnats, citrus flies and mosquitoes--which can cause pain and burning. Workers can do certain things that make it more likely that foreign objects would fall in the eyes, such as picking on the ground using a hooked stick (gancha) to pull the fruit down while looking up into the canopy, or wiping the sweat off their face with a dirty shirt sleeve or a with a glove that has sand on it. Conjunctivitis, sties, and various infections are also common due to the unsanitary conditions of the groves and some homes.

At the end of the first stage of research, we concluded that workers had detailed knowledge of eye injuries and sought to avoid them. The reason they often ignored the risk was because they...
did not see any alternative. They did not perceive safety glasses as a viable solution, because they thought the glasses would slow picking rates and thereby lower wages. At this time, we began working with members of a research project at the University of Illinois, Chicago, who had recently developed their own eye safety intervention for farmworkers in the Midwest. The UIC team had created a curriculum for eye safety education with the help of Migrant Health Promotion, a non profit with expertise in community health worker programs.

The PCWH research team adapted the eye safety program for the conditions of citrus harvesters in Florida and hired health promoters that were currently working with harvesting crews. During the first season, we field tested more than two dozen styles of commercially available safety glasses and, using worker feedback, determined the characteristics which would minimize discomfort and fogging. We finally chose one style of glasses which were lightweight, adjustable, and had a lens that had low distortion and a tint compatible with work in the shady tree canopy. The design reduced fogging through a ventilating gap in the top of the frames. A short strap was added to keep the glasses from being knocked off by the tree branches.

For the past four citrus harvesting seasons, we have fine-tuned the promotores program, which consists of training one person on each participating crew to be the eye safety specialist. That person attends all training sessions and is required to wear safety glasses himself and conduct educational outreach to everyone on the crew. He receives a first aid kit and enough glasses for the whole crew. In addition, he collects data during the week on his activities, meets with the field supervisor, and receives follow-up training. We have collected process evaluation data, measured outcomes in the field and surveyed participants in order to make the curriculum and the educational program relevant. The program is difficult to implement because of the close supervision required of the promotores and the logistics of working in the orange groves and labor camps. In addition, crew leaders and company policies do not always support eye safety.

By all measures, the PCWH promotores program is a success. A comparison of nine crews receiving the intervention and four control crews resulted in our highest measured success in four seasons. Of the nine crews that participated in the program, there was a range of 8% to 66% of crew members that adopted safety glasses with a median of 32% using them across all intervention groups. Workers on the control crews remained at zero percent usage over the course of the season, except for one crew, which increased to 9% without program intervention. In this case, the employers had influenced adoption of the use of safety glasses. The promotores are successful because they model behavior for the rest of the crew, demonstrating that it is possible to pick successfully while wearing safety glasses. They also provide encouragement and education about the dangers of eye injuries. They also provide first aid, mostly by washing out foreign objects lodged in workers’ eyes. The relationship they establish with their crew helps them to increase the number that use safety glasses and they also get support from the company which encourages safety glasses’ use and provides incentives to those crews that reduce eye injuries.

Gaps in Our Knowledge, Can Cultural Consensus Help?

Our survey of workers in the field was conducted once before the intervention began and then repeated eight weeks after the promotores had begun their activities. The survey took only a few minutes, collecting demographic data, work history, experience with eye injuries and an open-ended question on the worker’s opinion about using safety glasses. The interviewer also noted whether the worker was wearing safety glasses at the time of the survey. Of those surveyed after the intervention began, about 80 citrus workers were using safety glasses and this contributed to our profile of the target audience.

The survey data shows that older workers, (most of whom are married with children and families back home in Mexico and Central America) do have higher use rates as a group. There is possibly something motivating them to protect themselves in greater numbers, something we hypothesize to be related to life cycle and greater responsibility to protect their health and remain good providers for their families while they are working in the United States. Younger workers might be expected to be a little more reckless or to evaluate their risk differently, but even among the youngest workers, there are 30% who use safety glasses. There was little difference in use according to years of experience picking, although second-year pickers had higher use rates, but many had been exposed to the program the previous year. We were encouraged by the fact that repeating the program does result in higher use rates in the second season; it helps change behavior when workers see use of safety glasses as normative.

The participant’s answer to the survey question, “Why do you wear safety glasses?” was not entirely conclusive and it is one reason that cultural consensus modeling may provide better answers. Most workers gave general answers about “protecting eyesight” although a significant number specified whether it was the annoyance of foreign objects (basura or polvo) or the risk of branches (ramazos) hitting them. We hypothesize that the main reason is due to avoiding pain from fewer foreign objects irritating the eyes, with a secondary benefit that of reduced strikes by branches. If this hypothesis proves to be correct, the minor injuries caused by foreign objects (irritation, burning and redness) would actually be the motivating factor to adopt safety glasses and this would reduce the more severe eye injuries too. The most minor annoyance of citrus picking could be the major factor to change behavior and reduce all
eye injuries. As earlier research showed, a traumatic injury was less common and fear of losing one’s eyesight was not enough of a motivating factor for adopting safety glasses use. Greater comfort during the day from less dirt, dust and insects, would be a better benefit to offer workers because their perception initially is that safety glasses are less comfortable due to the increase in heat, fogging and dirt on the lenses.

The PCWH intervention with citrus crews focuses on three main points; eye injuries are serious; they need to be both reported and treated; and they are preventable. The idea that they are serious builds on the knowledge of workers who understand the many different ways to experience an eye injury but makes sure they understand the potential consequences for eye injuries in terms of pain, earning potential and long-term damage to vision. The idea that eye injuries need to be reported and treated is based on their rights as workers and the process that covers their treatment under workers compensation rules. Treatment refers to both the first aid steps taken in the field and the possible follow up treatment in a clinic or pharmacy. The third point, that most eye injuries are preventable, is made through our promotion of tested safety eyewear that is effective in the humid and dirty conditions of the Florida orange groves. Our presentation of each of the three points could benefit from the knowledge gained from cultural consensus modeling.

For example, we know that we can motivate workers to at least try picking citrus while wearing the safety glasses. They will put them on for a period of 15 minutes to an hour and usually report back that they did not experience distorted vision or uncomfortable fogging or increased heat around the face. Beyond that initial experiment, how long does it take for workers to really adjust to the new way of picking? This adjustment period, which the early adopters call acostumbrar (getting used to something), is still unknown to us and we can’t determine how long it has to last (days or weeks) and what is experienced during this time that changes their behavior.

We are proposing that a cultural consensus approach will help us to understand if workers construct a cognitive domain that classifies eye injury risks, their sense of control over each of these risks, and how they might calculate the financial costs of wearing or not wearing safety glasses or calculate different perceptions of comfort or discomfort. In the social marketing framework, a target audience is asked to “exchange” one behavior for another while calculating the costs and benefits of each behavior. Among citrus workers, we would like to know if they measure the discomfort of wearing the glasses (fogging, annoyance with dirty lenses, increased heat and sweating) and compare it with the discomfort of foreign objects (dust, dirt, chemicals and insects) and the potential risks of more serious injuries from branches.

Our strategy for the 2007-2008 harvest season is to create a pile sort exercise and administer it to citrus workers that have not been exposed to the PCWH promotores intervention. Our previous ethnographic research has produced the equivalent of a “free list” of all the risks that lead to eye injury to approximately two dozen. The cards will feature hand drawn pictures of each of the eye injury hazards and will be tested for reliability to make sure workers understand what is depicted on each one. Participants will sort the deck into different piles of their own choosing. We will ask them to provide a name or a description of each pile and explain why some cards (types of injuries) belong in each pile. Then we will ask them to repeat the pile sort and provide direction by asking them to group the injury cards by those that are most serious to them (risk) and then finally by categorizing those that are easiest to avoid (self-efficacy). In addition, demographic variables will also be collected such as age, years of experience picking, and personal experience with an eye injury. Through analysis of how citrus harvesters categorize these risks for injury we hope to create a cultural consensus model of what injuries are most serious, and which are most likely to happen, or easily avoided.

The second stage of research will be conducted once the PCWH promotores program is implemented and workers who have been observed wearing the safety glasses will be asked to participate in the pile sort exercise. By focusing on just the population of workers who choose to wear safety glasses, we hope to discover whether this group exhibits higher cultural consensus with their peers or if they have a different outlook altogether about risk, personal comfort, and productivity.

**Conclusion**

The PCWH has conducted extensive ethnographic research on citrus harvesting and the factors that lead to eye injuries. Through a successful intervention that uses community health workers, we have improved safety conditions in the orange groves and motivated a significant percentage of workers to try a new safety technology. Through the use of cultural consensus modeling, we hope to uncover the cognitive reasoning by which workers make choices about adopting safety eyewear and use these factors to improve our social marketing program.

Paul Monaghan is an Assistant Professor in the College of Public Health at the University of South Florida in Tampa. He is the project director for the Partnership for Citrus Worker Health, a pilot program of the Florida Prevention Research Center. The FPRC is implementing a model of prevention called Community-based Prevention Marketing which combines social marketing methodology with participatory approaches to health and safety among underserved communities. Since 1998 he has been conducting research with migrant farm worker communities in Florida on topics that include workplace safety, pesticide exposure and chronic disease such as hypertension and diabetes. He has a Ph.D. in Anthropology from the University of Florida. ■
REFLECTIONS ON TEACHING SCRM:
SURVEY RESEARCH METHODS IN ANTHROPOLOGY

By William W. Dressler and Kathryn S. Oths

First, we want to emphasize that we are pleased and honored that the students from our first Short Course in Research Methods (SCRM) in Anthropology, taught during the summer of 2006 at the Duke Marine Laboratory in Beaufort, North Carolina, have shown such commitment and perseverance in terms of continuing their professional development with respect to research methods. Through the organization of the session at the 2007 Society for Applied Anthropology Annual Meeting in Tampa, and their continuing work on those papers for publication here in Practicing Anthropology, they are demonstrating clearly what we learned about them during that week at ‘methods camp’: they have a clear devotion both to their own ongoing maturation as scholars and to the development of anthropology as a rigorous field of inquiry.

Second, we want to acknowledge and thank these students for their contributions to our own growth and development in this respect. As we emphasize in our regular university teaching, and it is even clearer here, we are all, technically, students of anthropology. It’s just that some of us have been around longer than others. Being around this group helped us to grow as anthropologists.

Our charge in the SCRM was to develop and offer a short course on ‘Survey Research Methods in Anthropology.’ This charge could be read two ways, as follows: (a) Survey Research Methods in Anthropology; or, (b) Survey Research Methods in Anthropology. We chose the latter course, for several reasons. First, if somebody wants to learn survey research per se, there are ample opportunities around the country every summer for studying with accomplished sociologists, political scientists, social psychologists, and epidemiologists (to name only the most prominent academic disciplines that also employ survey research methods). We are not about reinventing wheels.

Second, we don’t see survey research methods as something odd, unusual, or otherwise out of the ordinary for anthropologists to be doing. Quite the contrary, survey research methods are an integral part of ethnographic research, because we in anthropology are in the business of testing hypotheses about human behavior and social life, derived from anthropological theory. Testing hypotheses involves the collection of quantitative data, and survey research is an excellent way of doing that.

Third, we are thoroughly committed to what is generally referred to as the “qualitative-quantitative mix” in anthropological research, or what many in other fields have begun to refer to as a “mixed methods approach.” We say it is generally referred to in this way because it is increasingly difficult for us to see this as a mix of different kinds of data collection, but rather as a rough way of saying that there is a continuum of data-collection activities in which anthropologists engage. Some of these activities are more directly allied with describing variation within populations; others are less directly concerned with describing that variation. But drawing a line and saying that “this is qualitative and that is quantitative” is both logically and practically difficult.

As a result of all these suppositions, our course would be only partially recognizable as a course in social survey research methods to a social psychologist or epidemiologist. Many of the routine elements were there, to be sure. We began the course with a discussion of the formulation of hypotheses and the selection among different research designs for testing those hypotheses. These are among the most difficult issues in the development of any research project, because if you cannot state clearly the objectives of your research, especially in terms of how you view the relationships among those factors you believe to be causally antecedent and those you see as outcomes, then the entire endeavor is likely to fail. Research design follows naturally from the statement of hypotheses, because what kinds of data you collect and the sequence in which you collect it—in many respects the essence of research design—will be determined in part by what it is you are trying to test.
This is the stuff of introductory texts in survey research, and the place where we began to depart from what is standard. For example, because of our background in epidemiology, we placed more emphasis than is usual on specialized kinds of research designs, especially the classic case-control study, that are not normally associated with survey research, but that we feel can be particularly suited to the testing of hypotheses within the context of ethnographic research. The classic case-control design developed in part as a means to study relatively rare phenomena, hence using modest sample sizes. Such a specialized research design lends itself well to ethnographic research.

We began to depart even more from the standard approach to survey research by incorporating immediately into our discussions the systematic link of survey work to other ethnographic methods, including open-ended interviewing, participant-observation, and focus groups. Survey research in ethnography is not useful without the contextualization that is the hallmark of anthropological research. We understand humans to be thinking, feeling, and acting in relation to specific social and cultural contexts, and our survey work must be embedded in an understanding of that context in the same way that other kinds of data must be. But this is just another way of saying that there is not a sharp dividing line on the qualitative-quantitative continuum.

One step in ethnographic survey research that is of critical importance is measurement. We talk about measurement in anthropological survey research using the same terms that are commonly employed in discussions of research methods, especially the terms of validity and reliability. But, often, we mean something different, particularly by the term validity. The ultimate arbiters of validity in social psychology or political science are other researchers who assess the validity of measurements employed in a study by how well they reflect their—the other researchers’—understanding of theoretical constructs, and how well those measurements link to other, accepted measures in the field.

Sometimes in anthropology we make use of such a thoroughly etic approach. But (we think) more often, validity in ethnographic measurement rests on the fidelity of a construct to the shared understandings that the participants in our research have, or a thoroughly emic approach. We often, then, must systematically link our measurements to our description of shared understanding, while at the same time adhering to the requirements of reliability in measurement, as well as meeting the demands of a jury of our scientific peers. This is a daunting task that demands a new ethnographic measurement model and, hence, a place that survey research in anthropology diverges substantially from its use in other fields.

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One reason that anthropologists are so committed to the traditional qualitative approach of ethnography is its sensitivity to the subtle and nuanced expression of meaning that can be captured with that approach. This stands in contrast to the conventional understanding of interviewing in survey research that replaces the ethnographer with an automaton expected, like Sgt. Joe Friday, “to just get the facts, ma’am.” We have never practiced the craft of survey research in that fashion, based on our ethnographic intuition, and practitioners of survey research in other fields are now providing the empirical justification for that intuition. New approaches in “cognitive interviewing” are showing that validity in survey interviewing is enhanced by taking into account the context of communication between interviewer and respondent, and these approaches encourage the interviewer to do what it takes to maximize the likelihood of valid information being exchanged in each individual interview.

Finally, of course, any survey research requires that those data be analyzed, and anthropologists are sometimes characterized as inveterate quantiphobes. We believe that this stems mostly from the way in which statistical analyses are taught, emphasizing the arcane over the conceptual. In our course, there are “five things you need to know about statistics, and the rest is gravy.”

We hope that this brief overview of our approach to the SCRM survey research methods course underscores one major point: when an anthropologist is out there asking and trying to answer anthropological questions, it’s all ethnography.
By Yonathan Mizrachi

Introduction

One of the most fundamental assumptions held by applied anthropologists is the view that the anthropological perspective can be fruitfully applied in any human endeavor. As stated by P. Higgins and A. Paredes in their introductory notes to Classics of Practicing Anthropology 1978-1998 (Society for Applied Anthropology, 2000), this has been the underlying philosophy of Practicing Anthropology since its first publication.

The following paper describes how the anthropological perspective is being fruitfully applied by the author in a relatively recent human endeavor: using information technology to redesign/restructure/reengineer organizational processes, structures and culture. Practitioners of Information Technology and Management consultants use the cover terms Business Processes Reengineering (BPR) and more recently Business Processes Management (BPM) when they refer to this type of activity. Despite the extensive literature on BPR since the early 1990’s and on BPM starting in 2000, I do not know about any explicit attempt to apply anthropological method and theory to BPR-related efforts. In this paper, I wish to state some of the generic potential benefits of anthropology to any BPR-related efforts and demonstrate this using concrete examples from a three years applied research effort conducted by the author during 1998-2000 in the Israeli Defense Forces.

Business Processes Reengineering: Concept, Assumptions and Problems

Business Processes Reengineering combines the adoption of a workflow and process view of organizational activity with the application of advanced Information Technology in planned organizational change efforts. Central to any worthy BPR effort is acting on the basis of the conviction “Don’t automate, Obliterate”, coined by Michael Hammer, the father of BPR. In his words: “It is time to stop paving the cow paths. Instead of embedding outdated process in silicon and software, we should obliterate them and start over. We should ‘re-engineer’ our businesses: use the power of modern information technology to radically redesign our business processes in order to achieve dramatic improvements in their performance” (Hammer 1990:104). At about the same time, Thomas Davenport and James Short published another landmark article in the field of BPR entitled “The New Industrial Engineering: Information Technology and Business Process Redesign.” While the author names were different, the message was the same: IT should be used to creatively re-wire organizational entities and processes. The ability of IT to bend time and space would allow new business structures, business processes, and business models. The practice of BPR became even more common with the publication of Hammer and Champy’s 1993 book Reengineering the Corporation: A manifesto for Business Revolution and Davenport’s same year publication of Process Innovation: Reengineering Work through Information Technology and see also: R. L. Manganelli and M. Klein, The Reengineering Handbook. Over the second half of the 1990’s and at the beginning of 2000 there has been criticism over the radical/total approach as advocated by the founders of the BPR approach as well as substantial achievements, fine tunings and methodological improvements (For a detailed review see, H. Smith and Peter Finger’s Business Process Management: The Third Wave as well as improvements, Tampa: Meghan-Kiffer Press [1994] and Paul Harmon’s, Business Process Change: A Manager’s Guide to Improving, Redesigning, and Automating Processes, New York and Oxford: Morgan Kaufmann Publishers [2003]).

Today, due to its potential impact on key performance criteria such as delivery and response speed, low cost per output unit, and high process quality,
business processes reengineering is on the agenda of many large and mid-sized organizations in the private/business, government and NGO sectors. As a top-management issue, business reengineering projects are typically initiated from the top down and implemented from the bottom up, within the hierarchy of a given organization. BPR/BPM - if implemented successfully - not only transforms processes, but ensures that the structure, the work-place contents, the information technology in use and the culture of the organization successfully assimilate the improved processes. And yet, due to the amount of potential change affecting people and their work environments, business reengineering projects are often accompanied by a considerable amount of resistance from the organization’s members and outside forces. Additionally technical obstacles may prevent business reengineering implementation success. In the worst case the barriers to business reengineering implementation may cause a failure of the whole effort. Sources talk about high failure rates of BPRs, some running as high as eighty percent.

**Anthropology and BPR in the Israeli Defense Forces**

During the years 1998-2000, the Israeli Defense Forces hired the author to examine ways to optimally deploy a major command and control information system at a headquarters level of an active duty division. The division was the first to adopt the new information system and lessons drawn from the project were to be implemented in the other divisions that were about to adopt the system later. Although not explicitly phrased in BPR terminology, the IDF was interested in leveraging the opportunity of the introduction of the new system to optimize and improve organizational processes, structures and work culture in the division’s headquarters. In the following three years, the consulting work has been integrated and embedded in a longitudinal formative (as opposed to summative) applied research project. Insights and lessons gained in this study in reference to the value of anthropology for such endeavors are discussed below.

Our study focused on three related, chronologically-ordered research questions: *What* happened in the course of the transition to the new Information system? *Why* did it happen the way it did? And a final, retroactive and applied-oriented question: *How* could it be done differently to maximize the effort? A qualitative research design framework vectored along three core anthropological principles (holistic, comparative and emic views of reality) and powered by interviews and observations led the research effort and gave it its applied anthropology “edge.” The first research question was a documenting-oriented one: What happened to the organizational processes, structures and culture of the division headquarters in the course of implementing the new information system? This was a classic before and after “snapshot” documentation. We were interested to know whether core business processes in the headquarters were improved as a result of the transition to the new information system, whether positions and roles of staff were altered (structural changes), and if changes in “corporate culture” can be documented and related to the new command and control system. In short, we wanted to know if the transition to the new system was “automation” or “obliteration” oriented and in each of these scenarios, whether it touched and improved core business/organizational processes.

In order to do this, we conducted a series of in-depth interviews with key organizational players in the division’s headquarters before the introduction of the new system. The interviews were aimed to record (and hence, to “freeze” in time) the state of things before the introduction of the new information systems. Senior officers and key staff in three distinct command centers within the headquarters (operations, intelligence and logistics commands) were asked to describe: (1) core competencies and responsibilities of their command center within the division headquarters (2) core business processes that are used within their command in order to implement these key responsibilities. Next, the interviewed officers were introduced to the notion of BPR/BPM and to the organizational transforming potential of information systems. The interviewers were then asked the following question: Given that Information Systems can be used to redesign structures and processes in your command and the entire headquarters, how would you use them to improve and transform your current business structures and processes in the most innovative and productive way? By the end of this phase of the research, we had a clear “emic” documentation of the perceived current reality by headquarters officers. These materials were then used together with the analysts and the planners of the information system to determine problems and opportunities, to better analyze and understand existing roles and processes, to determine information requirements, and to “calibrate” their perceptions of the future system with those of the users. Furthermore, and perhaps more importantly, we had the most important yard stick to measure the success of the BPR process: that of the users.

By recording user expectations of BPR/BPM levels in advance (that is, “what would constitute a success of the project?”), we could measure actual BPR/BPM transformation levels at the end of the project and “calculate” the effectiveness of the project in terms of organizational transformation. In this sense, the ultimate judgment of the success of the process was not the outside specialist (me) or some other professional outside agent but the users themselves. Surly, I had my own observations and professional assessments of the projects, but in terms of authority in answering the question of BPR success, nobody could argue with the users. I believe that my (anthropological) view that any documentary effort is always interpretative effort (in the soft, non-post-modern sense of things), contributed to an improved understanding and evaluation of the process of introducing the information system. In practice, we were able to introduce the “native” (user) view of what will constitute a success and what needs to be done into
the process of designing and implementing the information system. We have embedded in silicon the emic view of users in the division’s headquarters.

Anthropological perspectives and methods proved valuable also in dealing with our first (already discussed above) and second question: Why did the project developed the way it did? First, the anthropological holistic approach to understand social process related very well to the notion of business processes as cutting through organizational boundaries. Units, commands, divisions, are linked by organizational process and intertwined and weaved together to create a unified organizational entity. Accordingly, adopting holistic view of the business processes in our interviews with headquarters’ officers reduced their tendency to express current and desired future states of the organization in compartmentalization-oriented perspective. The users saw themselves as part of a whole. The holistic approach also helped us in retroactive explanation of why things happened they way they did. For example, we looked at aspects of technology (e.g., gaps between user desire and technical constrains), organizational politics (e.g., internal power struggles over authority and resources among officers) and finally, economic look at Return on Investment (ROI)—(e.g., how ROI considerations influenced the level of BPR application).

We have conducted textual analysis to study operational manuals of the headquarters before the introduction of the new Information system and compared them with the software operational manuals in the post-information system state of things. We have also analyzed all the administrative exchanges and literatures related to the project (including meeting protocols, exchange of letters and other forms of correspondence between developers, decision making military authorities and documented internal discussions within the division) in order to provide an accurate historical review of what happened and why things happened the way they did from a multiple perspective.

Lastly, we turn to our final retroactive and applied-oriented question: How could the introduction of the new information system done differently to maximize the effort? Once again, anthropological perspectives influenced our approach in dealing with this question. By empowering users via our emphasis on the emic view of things, I, as a consultant, was able to avoid the situation in which I had to provide directly the organization with an external yardstick to BPR success. Instead, I did it through its own people who expressed their own (high) BPR expectations independently, even before the project started. The users set their benchmark of project success at the beginning of the project (by stating what they think should be done with the introduction of the new system) and these statements became the yardstick by which the division management evaluated the success of the project in terms of BPR levels. At analysts of the information system etc.) and added our professional knowledge and (external, industry standard) benchmarking values from best BPR practices conducted elsewhere to generate a complex, yet clear, picture of what happened, why it happened the way it did, and finally how could it be done better.

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THE MAKING OF RITUAL SPECIALISTS: WHAT CULTURAL ANTHROPOLOGISTS CAN CONTRIBUTE TO EDUCATING FUNERAL SERVICE PRACTITIONERS

By Sherlynn Briller and Allison Kabel

Introduction

For the past five decades, anthropologists have taught health practitioners about how our discipline studies and addresses health-related issues. Important curricular reforms in the education of health professionals in the late 1960’s and early 1970’s greatly expanded such teaching roles for social scientists (Chrisman and Johnson, 1996). While anthropological teaching of certain types of health practitioners such as physicians and nurses are well-known, anthropologists’ growing roles in teaching other kinds of practitioners are also being documented today. For example, the burgeoning intellectual, collaborative teaching and practice relationships between anthropology, occupational therapy and occupational science have been discussed by Frank (2007) and Frank & Zemke (2005). One allied health field where the potential for cultural anthropological contributions in teaching has been far less investigated is mortuary science. While physical anthropologists sometimes interact with mortuary science programs in providing anatomy and forensics instruction, cultural anthropologists have not yet fostered teaching partnerships with this allied health field to the same degree. One possible reason that such teaching collaborations are not more common is that mortuary science education still largely occurs in vocational trade schools versus university settings where more cultural anthropologists regularly teach. We conducted a year long ethnographic study of the education and professional socialization of students in one of the few U.S. university-based mortuary science education programs. The purpose of this paper is to highlight several key areas where we believe cultural anthropological input could be more fully utilized for educating practitioners to direct the important societal task of publicly acknowledging the end of life. Two of these key areas include teaching about becoming a ritual specialist charged with the cultural “management” of end-of-life experiences and how to consider human variation in doing this work. In considering these key areas, we will discuss why we believe greater teaching collaboration between the fields of cultural anthropology and mortuary science is both possible and timely.

Ethnographic Study of a University-Based Mortuary Science Education Program

The mortuary science education program that we studied has been in existence since 1939 and is considered nationally to be a premier program in this field of study. It is one of only a handful of four year university-based mortuary science programs and is located on a large urban research campus. At this university, mortuary science is an allied health education program that is part of a College of Pharmacy and Health Sciences. Over the course of an academic year, we observed eight required classes attended weekly by undergraduate mortuary science students pursuing a Bachelor of Science degree. We also regularly “hung out” with students and instructors informally on campus. Voluntary informed consent was obtained from all study participants (N=35) for participant observation, including 29 students and 6 instructors. Separate informed consent was obtained later for conducting in-depth interviews in which we discussed multiple aspects of funeral service education, including mortuary science teaching. Six departmental instructors as well as seven students voluntarily participated in this in-depth interviewing process. All participant observation and interviews were conducted either by the study’s principal investigator or graduate.

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research assistant who are cultural and medical anthropologists with special research interests in the anthropology of death and dying.

A major ongoing debate in mortuary science education centers on what is the “right” model for educating funeral service professionals. A focal point of discussion is what advantages exist in having this training take place in a university setting vs. a vocational trade school. Key aspects of this discussion include how mortuary science education should be structured, what curricular needs are, what skills practitioners must acquire, and how professional socialization should occur. These issues are not new. In The Culture of Professionalism, the historian Bledstein (1976) uses funeral directing as an example of an occupation that clearly strove to “professionalize” itself over time. He shows how this continuing quest for further “professionalization” and a closer relationship to the realm of higher education has been a part of mortuary science education since its development in the late 19th century.

Deliberation among this program’s faculty about the “right” contemporary model for mortuary science education parallels the ongoing national discussion about these issues in the field today. It is important to recognize that most mortuary science educators are active practitioners as well. Even in this university-based program, only three faculty members teach full-time and approximately 20 teach part-time. For example, this program regularly employs instructors who are active practitioners with specific areas of expertise such as a psychologist with a specialty in grief counseling, a chaplain to teach religion classes, and so forth. These practitioners are expected to have knowledge of the funeral service industry and tailor their teaching for this student population and their specific educational needs.

While there is intra-group variation among faculty here, the majority viewpoint is that the university provides greater opportunities for multi-disciplinary education and many benefits associated with a “traditional” liberal arts and sciences undergraduate education. In this program, a widely held belief among faculty is that students who receive a university education will become more broadly trained practitioners who can use their enhanced education to further “professionalize” the field. Several faculty who are especially strong proponents of this stance spoke passionately in the in-depth interviews about why and how they see this program’s mission as being an educational leader that needs to reform or even revolutionize this field’s educational approach. In these conversations, they also emphasized why they believed a university setting is most conducive to making these broader curricular changes in the field as a whole and how they are beginning to implement these ideas in their own educational program. To illustrate how this process is occurring, this core group of faculty discussed plans to develop a new master’s degree program at length. Since the time of our study, they have moved ahead with this pioneering initiative and are now getting ready to launch this new graduate degree which will be the first M.A. program in the nation specifically training funeral directors.

We have seen from the discussion above that much conversation about mortuary science education centers on the further “professionalization” of the field, what this training should entail, and in what kind of settings it should be provided. While there are currently very few U.S. university-based mortuary science education programs, we contend that studying these out of the ordinary programs offers an excellent opportunity to more fully contemplate what the relationships of these mortuary science education programs to broader university communities could and should be. Our ethnographic study gave us a chance to explore in-depth one such program and conceptualize how cultural anthropologists could potentially contribute to mortuary science education in this university setting.

Educational Requirements

Before discussing how cultural anthropological insight could be infused into the mortuary science curriculum, it is first useful to summarize what the current educational requirements are for getting a B.S. degree in this program. To obtain a bachelor’s degree, students are first required to complete 68 university general education credits. These general education requirements include at least one course in each of the following areas: life sciences, physical sciences with lab, historical studies, American society and institutions, philosophy, social science, foreign culture, and visual and performing arts. In addition to these university general education requirements, students must take an additional set of prerequisite courses before they can be formally admitted to the mortuary science professional training program. These additional prerequisite courses include several basic biology courses including human anatomy with a lab section, a series of relevant chemistry classes, computer literacy, writing/composition, oral communication and general psychology.

Upon completing these required courses, a student must then take 52 credits in the mortuary science professional program curriculum. The required mortuary science professional program courses are divided among several areas that are classified according to the scheme presented in Table 1.

- Sciences: Chemistry, Human anatomy and physiology, Introduction to disease, Medical microbiology, Embalming, and Restorative arts
- Funeral Service Management: Funeral directing, Mortuary management and administration, Mortuary and business law, Small business financial management, Past and future trends in funeral service, Senior seminar, and Field practicum.
- Humanities/Social Sciences: Religions, Values and death, Psychology of death and dying, Psychosocial aspects of grief, Applied grief counseling.

Table 1. Required Classes in the Mortuary Science Professional Program
Although the official language describing the program’s curriculum treats these three main areas as being of similar importance, we observed that both faculty and students tended to prioritize things somewhat differently “down on the ground.” For example, most students reported that they spent the greatest amount of time studying for the science classes. Typically, the students next concentrated their efforts on the funeral service management classes and then spent their remaining time on humanities/social science classes. We will next consider how the pedagogical emphasis in the program may contribute to this patterning.

To a large degree, the teaching approach in the required professional program classes focuses on ensuring that students will do well on the national board exam administered by the International Conference of Funeral Service Education. Students must pass this exam in order to be licensed. This university program prides itself on having an 85% pass rate for first-time test takers which contributes to the program’s prestige in the field. Faculty consequently spent much instructional time in all classes covering what kinds of items will be on the national licensure exam and how the questions will be asked. As one instructor for several core science classes emphatically stated, “I don’t want them to have any doubts going into the test what they need to know. In essence, the whole year is preparation for them going in there and being able to do well.”

We observed that this teaching approach, emphasizing memorization skills and direct preparation for the licensing exam, seemed much better suited for some types of classes than others. Whereas this strategy appeared to work well for the required science and funeral service management courses, it was often far less compatible with the material being covered in the humanities/social sciences portion of the curriculum. Indeed, the instructors of the humanities and social science courses spoke at greatest length in interviews about the need for students developing critical thinking skills to use in their professional practice and how the content that they were teaching was designed to promote gaining these skills. They also spoke most explicitly about why they perceived that a university was the appropriate educational setting for obtaining these skills. However, these instructors also complained that a major teaching challenge was that many students entering this practice-oriented training program did not necessarily fully recognize or value the importance of these critical thinking skills in their foundational training. As a psychology instructor said, “There are some students who say ‘Why do I even need to take this class?’ They don’t realize how much they will be drawing upon what they learn here in their professional practice.”

In short, we have now provided some context for understanding the faculty’s perspectives about their program’s role as an educational leader in the field, ongoing discussion about the “value-added” of a university education for mortuary science students, current educational requirements, pedagogical approaches used, and some discussion of how effective these teaching approaches are in light of the program’s mission. With all of these elements in mind, we can now move on to specifically considering what potential teaching contributions cultural anthropologists could make to mortuary science education.

**Teaching About Becoming a “Ritual Specialist”**

An important area where we believe cultural anthropologists could enhance the existing curriculum involves what it means to take on the highly socially meaningful role of being a ritual specialist…. We wish to put forward the idea here that cultural anthropologists could deepen mortuary science students’ fundamental understandings of how ritual specialists participate in socially constructing different types healing processes in communities.

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work already address this issue to some degree. However, more time is currently spent in these courses discussing the “nuts and bolts” issues of thinking about how funeral ceremonies should be constructed and run and how to appropriately tailor them for various constituencies rather than explicitly considering the important social meanings associated with this role.

We wish to put forward the idea here that cultural anthropologists could deepen mortuary science students’ fundamental understandings of how ritual specialists participate in socially
constructing different types healing processes in communities. For these students who must become the cultural “managers” of death, such discussions could be greatly enhanced, especially regarding how funerary practitioners can become effective ritual specialists and their roles in interacting both with people directly involved in healing processes relating to death as well as their broader communities. The educational program we observed did recognize the need for training future practitioners about the needs of a diverse client base. However the instructors mainly relied on the so-called “ethnic cookbook” manual approach that anthropologists have long critiqued for teaching health-care practitioners about cultural issues. Our discipline could certainly assist in better providing these future funeral service practitioners with the conceptual tools they need to adapt their services in culturally sensitive ways.

Anthropologists could conceivably use some of the same sources that we use to teach our cultural and medical anthropology students about ritual healing processes and their social meanings for teaching mortuary science students.

for practitioners on these critical topics. For instance, the well-regarded book *Celebrations of Death: The Anthropology of Mortuary Ritual* by Metcalf and Huntington (1991) is frequently used in various types of social science classes to teach about cross-cultural study of death-related rituals. Students often find this book’s comparative examples about how such rites are performed and socially interpreted in diverse cultural contexts to be thought-provoking. Teaching with these types of cultural anthropological resources could similarly aid mortuary science students in thinking more deeply about how death rituals are socially constructed events and their roles as practitioners integrally involved in these processes.

While we are recommending that these aspects of the curriculum could be enhanced for all mortuary science students, it seems especially important that the new graduate students in the MA program for funeral directors receive an enriched education in these areas. It is reasonable to assume that mortuary science students trained in a university setting and especially those in the advanced graduate program are likely to become educators in this field themselves. Since these graduates will then be teaching the next generations of practitioners, it would be highly beneficial for them to be comfortable teaching about important interactions between ritual specialists, mourners, and their wider communities in socially recognizing and acknowledging death.

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Anthropologists could conceivably use some of the same sources that we use to teach our cultural and medical anthropology students about ritual healing processes and their social meanings for teaching mortuary science students. For instance, a classic writing that is often used to orient our students to thinking about ritual healing as a socially constructed process is “The Sorcerer and the Magician” by Levi-Strauss (1963). In the anthropology of death and dying literature specifically, numerous accessible writings exist that would have much utility in teaching expanded units

Teaching About Human Variation

Another significant area in which anthropologists can contribute to mortuary science education is teaching about human variation. During our study, we observed many inconsistencies in teaching about race, culture, and ethnicity between various instructors and in different courses. Even the ages of the textbooks and materials used to describe these key concepts astonishingly ranged from the 1940s to the 21st century. Whereas some faculty treated race as a “scientific fact,” others presented race as a socially-constructed category. Thus, there appeared to be no overarching philosophy about teaching of these critical subjects in the current curriculum; instead these topics were discussed quite differently in various types of science and social science oriented courses. Due to the centrality of critiquing the “race concept” in anthropological thinking and teaching over time, we believe our field has much to offer in thinking about how to coherently present this crucial subject matter.

Once again, we believe that having a solid foundational training regarding issues relating to human variation is important for all mortuary science students who will become practitioners and is likely most significant for those who will themselves go on to become educators in the field. In a context where mortuary science students are receiving a four year undergraduate education instead of a shorter vocational education, it seems especially important to broadly educate them in these areas. In this way, the program’s graduates will be prepared to take on leadership roles within a funeral service profession that will need to respond to the complex demands of an ever-changing and increasingly diverse society. The hope would be that these graduates could also help with bringing about much needed curricular updating in teaching about this crucial subject matter in the field as a whole.

Conclusion

Building on a well-established tradition of anthropologists teaching health
practitioners about our disciplinary approaches, this article highlighted several vital areas in which we believe cultural anthropological involvement in mortuary science education could be highly beneficial. Anthropologically framing topics such as the “making of ritual specialists” and making concepts of culture, race, and ethnicity more distinct will facilitate training of practitioners who have good foundational understandings of these social science concepts and why they are important. While cultural anthropologists have a more prominent teaching role in other allied health education domains, a similar connection with mortuary science education has not yet been created. Our recommendations are informed by observing the kind of training already taking place in an innovative university-based mortuary science program that prides itself on educational leadership in the field. The recommendations discussed here were shared with the program upon completion of this study. We hope that these ideas will help to stimulate further discussion with this program as well as others interested in launching new and valuable teaching partnerships between the fields of cultural anthropology and mortuary science education.

References


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“While cultural anthropologists have a more prominent teaching role in other allied health education domains, a similar connection with mortuary science education has not yet been created. Our recommendations are informed by observing the kind of training already taking place in an innovative university-based mortuary science program that prides itself on educational leadership in the field.”

Notes

1 The following classes were regularly observed: Psychology of Death and Dying, Psychosocial Aspects of Grief, Religion, Values and Death, Mortuary and Business Law, Restorative Arts, Medical Microbiology and Mortuary Science Senior Seminar. Classes were selected for observation based on a combination of factors: subject matter (prioritization of social science related courses due to our research interests, instructor interest in participation, and scheduling logistics. All required program courses were also discussed with faculty and students in the in-depth interviews. While it would have been ideal to observe all program courses, we believe that the quality of the information we obtained about the training program generally and the social science related components of the program specifically, enable us to comment knowledgeably about these aspects of the curriculum as well as what anthropologists can potentially contribute to teaching in these areas.

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STEALTH LANGUAGE TEACHING: A PRESCHOOL FOREIGN LANGUAGE PILOT PROGRAM

By Anna S. CohenMiller

Introduction

In 2003, I worked at the Deacon Park, a non-profit private preschool in Atlanta, Georgia, to create a foreign language-learning environment that used second language (L2) as a medium for the curriculum instead of as a separate subject matter. With a background in Montessori education, through first hand experience as a student as well as professional experience as a Montessori teacher, I was interested in the possibility of creating a Montessori inspired content-based environment where early learners (ages ranging from months to 5 years old) could be exposed to Spanish as a natural part of their day.

Even though the director of Deacon Park was open to the idea of implementing a Spanish program, she seemed a little surprised when I proposed a format that varied from the rest of the “enrichment” courses offered, such as art, drama, and music, which typically took place outside the regular classroom (in a separate room), in fifteen to thirty minute increments, thus allowing the regular teachers a time to themselves (for planning, organizing, setting-up an activity, etc.) and allowing the students to focus on the special activity thus not being distracted by the students’ normal classroom. For the Spanish classes, the idea was to incorporate the lessons into the regular classroom—both physically and as part of the curriculum—so as to have a L2 be taught in a similar fashion to the students’ first language (L1), through hands-on activities and learning.

The focus was to teach Spanish within a foreign language environment that was as natural as possible for learning language, without the ability to create an ideal environment of a complete immersion program/dual language program². I realize now that this type of natural environment for language learning is similar to content-based programs that use the foreign language as the basis for the education and not as a separate subject matter. However, my work was a bit different in that this pilot program was created with the concept of Montessori pedagogy, which focuses on learning language through experiencing and absorbing the language of her/his environment both naturally and unconsciously as well as teaching when the children have the greatest urge to learn the new material, allowing students to elicit their own learning. The end result was that many students appeared to learn Spanish without realizing that they were acquiring a L2.

Spanish became a part of the regular day, meaning that as the Spanish teacher (and researcher), I would approach the children throughout their day and incorporate Spanish into whatever else they were already doing. Therefore with a Montessori-based concept of integrating the language into the classroom and regular life, the learning of a separate language was not a subject matter to be mastered or sectioned off from the rest of the class activities (as with the other enrichment class) but instead incorporated into the children’s regular activities, such as in the classroom, outside on the playground, in the halls, during snack/lunch, as well as the “in-between” times (e.g. in the hallway, going to the bathroom). In teaching Spanish, the children did not go into a separate classroom for learning, instead I went into each class and interacted with the children during the regular activities of the day letting the students approach me when they were interested. This new approach to teaching an enrichment class, however, was not immediately accepted by the teachers or parents³. Instead, I initially heard concerns about the Spanish classes not teaching the children enough language, which seemed based upon the fact that I only spoke Spanish to the children and tried never to translate any words or phrases. Therefore the children did not come home saying that they learned how to say dog in Spanish class because there was no such time designated as “Spanish class.”

By focusing on Spanish as a medium for education, the preschool pilot program helped to foster students’ language acquisition by imitating children’s natural learning style. With the intersection of cultural analysis, educational implementation and ethnographic fieldwork, this preliminary study was planned as a foundation for graduate study that draws from the applied disciplines of anthropology and linguistics. Framed within the context of bilingual education, the research from this topic was constructed in order to take the next logical step in program development for educators and applied researchers in the fields of anthropology, education and language; thus adding to the theory underlying the praxis of creating and implementing L2 acquisition programs for majority language students.
Methods

This study used a qualitative methodology that included ethnographic research involving participant observation, teaching, curriculum development, informal interviews and insider information. As an action-oriented ethnography, I was one of the approximately twenty-five part-time teachers who worked with the preschool children. On any particular day, I would move between three to ten classrooms, working with the children during their typical activities, which could include snack time, circle time, free time, recess, etc. As a researcher and participant observer, notes were taken multiple times per week. As a teacher and researcher, informal interviews were conducted with parents, teachers and staff both in and outside the classroom. Using a qualitative methodology, the data is analyzed at four levels: student, parent, teacher, and researcher.

The preschool caters to a predominately middle to upper class European American community in Atlanta, Georgia, and is located in an upscale, trendy area of the city. The school day for the preschoolers is shorter than a traditional full-day program; they arrive at 9am and leave at 1:30pm. Students can be enrolled for anywhere between two days per week to five days per week, depending on their age and maturity level. The classrooms that I focused on were the four-five day per week students.

Before beginning the Spanish pilot program, there was no foreign language component of the classroom curriculum or school. The director of the preschool approved the development of the language program, which was based upon Montessori methodology. I taught four days per week, for an average of four and a half hours a day during the regular school hours. As a teacher, I was able to gather information on the culture of the school as well as the dynamic (and culture) of each individual class. Interaction with the parents occurred on a daily basis primarily during pick-up and drop-off times, although additional opportunities were afforded when parents volunteered as well as the letters created to inform, update and maintain the Spanish program.

The structure of the program progressed based upon my educational background and teachings in Montessori. The overall utility of the program was not initially foreseen, yet developed bit by bit, with the general hope of creating a framework for foreign language programming. On average, I spent approximately twenty-five minutes in each of eight classrooms. Depending on the classroom culture and structure, the number of children I would work with varied. Generally, I would work students one-on-one or with groups of two, switching from group to group, following the student’s interest. Separate from the stealth language teaching I was able to provide, the children also supplemented their learning (as is typical in a Montessori classroom), in speaking with one another, in particular correcting one another in proper Spanish terminology.

Revealing the Child and Language

Sheldon

Unlike the other two-year-old boys in the class, Sheldon used his verbal skills more than his physical ones. He talked all the time; managing to have a conversation with himself about anything, whether it was the particular type of tiles on the floor, or the thought of the pet turtle that he might eventually have. In Example 1, Sheldon shows that he understands a verbal directive and responded in both action and verbal response. So, although Sheldon did not use Spanish in his response, he was showing a distinct listening comprehension for the L2.

Maurice

Maurice spoke three languages fluently and could understand Spanish comfortably. However he did not realize his abilities consciously. Instead, he was able to switch between languages with his parents (one spoke French, the other Farsi); at school he could switch into English, and then in speaking with me, he could effortlessly carry on a conversation. Even when it was clear to me that there was some misunderstanding in his comprehension, he was nevertheless able to continue chatting (which I suspect is based upon the general language ability and understanding of linguistic conversational cadence).

Herbert

The children responded differently depending on their own individual comfort level with Spanish as well as their academic and intellectual. Three-year-old Herbert is the perfect example of a student who had trouble with adjusting to a language that was different from what he expected. His background
During recess on the playground, talking with 2 year old, Sheldon:

Anna: [pointing to the truck and leaves Sheldon was playing with] Pones las ojas en el camion. [Put the leaves in the truck.]
Sheldon: [looks at the leaves and the truck, and pauses briefly, then begins putting the leaves into the truck] They don’t fit, they’re too big.

Example 1: Creating a Spanish Foreign Language Environment in an Everyday Environment

showed that he had a bit of trouble both socially and academically which his parents often worried about. When the order of events in the class did not follow their pre-prescribed order (e.g. lunch time following directly after recess), Herbert would start to break down. In Example 2, Herbert was showing that the order of events (the language expectation) is not what he was used to and did not fit within his comfort zone. Unfortunately for Herbert, it took him almost a year to get comfortable with me using varying words for objects he knew. Eventually (I suspect out of intellectual and emotional growth), Herbert was able to listen, and even interact with me in Spanish, without being upset by the language difference.

Lucy
Herbert was in direct contrast to four-year-old Lucy who was not only an incredibly precocious four-year-old, but also wanted to know the names of everything around her. She wanted me to translate anything (and everything) she pointed to, into a Spanish equivalent. Although one thing that was interesting about Lucy was that she spoke to me as if I did not understand her. She rarely (if ever) used verbs when speaking with me. Instead, she would just point toward what she wanted translated and with an exaggerated rising intonation say “table?” Then she would wait for my response. If my response was more than one word, she would ask me again louder and a bit more emphatically. After a few months, it was revealed that Lucy’s full-time nanny did not speak English as her L1. This proved interesting because the exposure to a non-English speaker helped to enculturate Lucy in how to toward what she wanted translated and with an exaggerated rising intonation say “table?” Then she would wait for my response. If my response was more than one word, she would ask me again louder and a bit more emphatically. After a few months, it was revealed that Lucy’s full-time nanny did not speak English as her L1. This proved interesting because the exposure to a non-English speaker helped to enculturate Lucy in how to

Discussion
From the initial analysis, it was clear to see the variances in the responses of

Anna: “Este es un dinosaurio” [This is a dinosaur.]
Herbert: “NOOO, nooo, that’s a dinosaur!!” [pointing emphatically at the picture]
Anna: “Muy bien, sí, es un conejo” [Very good, yes, it’s a rabbit.]
Herbert: “NOOOOOO, nooooo, that’s a DIIINOSAUUUUU!”

Example 2: Reading a Book to a Small Group of Children, Pointing Out the Different Animals

the children to learning Spanish as a “stealth” medium for education. Some of the students wanted direct translation, in the case of four-year-old Lucy, some were able to converse easily without acknowledgement of the linguistic difference, in the case of Maurice and Sheldon (which is the ideal means for natural language acquisition), whereas others had varying degrees of difficulty in accepting alternative ways of speaking, in the case of three-year-old Herbert.

Parent and teacher responses closely reflected the varying feelings of the children. Some of the adults were supportive of the concept of stealth language teaching from the beginning. Other parents were disappointed, confused or even upset that their children did not consciously know that they were learning Spanish.

Although varying in level of acceptance of the stealth language program at the onset, there were definite changes throughout the fifteen-month study including: distinct improvement in the level of language acquisition in the children was noted as well as a quick move from hesitancy to enthusiastic participation; an initial parent/teacher resistance that was overcome through explanation and advocacy; profound change was noted amongst parents and teachers/staff in terms of understanding a content-based foreign language program based upon Montessori concepts of language acquisition. Additionally, as the teacher and researcher, my initial curiosity (and slight trepidation brought on by parent/teachers concern) was followed by increased confidence as the students, and parents/staff, began to get used to the additional teacher (participant observer) and interaction on a regular basis as well as the obvious level of language acquisition. Although initially intending to focus language learning on the unconscious level, it turned out that the children both acquired Spanish unconsciously and consciously (e.g. in teaching one another).

Looking back, I would have ideally gathered additional information before being the pilot program. For instance, formal interviewing could have added important information in terms of what
obstacles I might face within the school community. Additionally, testing of the children’s level of exposure to Spanish previous to the follow-up post-study could have provided important background information (although this does not address additional outside influences that could affect the child, such as a family member who spoke the L2, regularly watching shows such as Dora the Explorer, etc.).

Looking forward, I will be working to incorporate more than ten hours of follow-up interviews with teachers, the director of the preschool, and parents, in order to re-evaluate the research in terms of current educational concepts of bilingual education, looking to create a way to improve foreign language programming. This analysis will be utilized to create a recommendation for the future of the L2 program at the preschool.

Appendix

Using the commonly used/requested words to indirectly teach Spanish, stories and toys were an expected type of learning used by young children. However, the importance of trying on hats came as a surprise. Each classroom had a variety of fabulous hats (large felt hats, small brightly colored ones with feathers sticking out, police caps, etc.) that the children consistently used in order to take on varying roles. Not only did the students talk about food both at snack and lunchtime, but also food talk could occur when playing tea-party or grocery store.

An interesting category of commonly used words was the “container” category. It turned out that many facets of a preschooler’s life involves different types of containers, including the above examples as well as cubbies, lunchboxes and boxes. This category provided an opportunity to follow suit with the children’s interest of describing what was inside a container, what could be taken out, or who was in control of each particular container.

Although many teachers did not interact with the kids much during recess, I made it a point to attempt to relate with the students as much as possible. Just as the classroom is an (indoor) environment for the children, the outdoors is an environment for learning and processing information as well. I saw it as ecologically important to speak about the outdoors and use it as a similar opportunity as classroom time to use stealth language teaching. The concept of integrating the outdoors with

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<th>Commonly used/requested words</th>
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<td>Category</td>
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<td>1. Stories/Play</td>
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Stories/narrative (reading a book, coming up with stories…)

Imaginative play (indoors and on the playground, e.g tea party, grocery store)

While eating (snack time, lunch time)

Artistic expression (music, drama, art)

In-between times (in the halls, lining up to go outside, cleaning up the classroom; comforting kids when they fall down or miss their parents, basic conversation, directives, commentary)

Different Techniques for Teaching Spanish

1Fortunately, the director almost immediately understood the possible advantage of incorporating second language within the classroom. So despite some initial lack of support from teachers and parents, I felt supported throughout the program.

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PITFALLS IN ARCHAEOLOGICAL FIELDWORK:
ON GENDER, ETHICS, AND DANGER

By Carleen D. Sanchez

Independent field work is considered a rite of passage towards advanced degrees in archaeology—particularly for the Ph.D. Through graduate coursework and mentoring from advisors, students prepare for fieldwork by their reviewing the literature, preparing grant proposals, and defending the thesis. Through a period of apprenticeship and/or field-schools archaeology students go on to learn various methods and techniques. However, little explicit guidance is provided to prepare students, particularly women, for the types of problems and issues that can be encountered in the field. Further, there is no published literature that directly addresses the challenges of fieldwork, especially outside one’s home country. How is it, then, that archaeologists learn about the pitfalls of the field? It is largely via trial and error, which can be a costly and dangerous mode of learning.

It is notable that a quick review of archaeological literature reveals that text books and articles specifically address archaeological theory, field methods, site reports, and archaeological ethics. But I was surprised to find no information about the doing of archaeology—and how gender affects fieldwork. This was all the more unexpected in that over the past 25 years, gender archaeology has become an important area of archaeological writing. I was surprised that gender archaeology does not explicitly address the implications of being a woman in the field. I know anecdotally, that as women archaeologists, we often bemoan the lack of bathroom facilities in the field and other personal issues. However, these problems have not entered the literature, as if to acknowledge that women who experience weaknesses and inferiority.

Doing Archaeology in Central America

I began archaeological fieldwork in Honduras in 1991 under the direction of the late Dr. LeRoy Joesink-Mandeville, Professor of Anthropology at California State University, Fullerton (CSUF). At the time I was a graduate student at CSUF and had previously participated in two semester-long field school courses taught by Joesink-Mandeville. In the summer of 1991, Professor Joesink-Mandeville directed the Yarumela Project in central Honduras and I spent two months at the project. Over the course of several years, I accompanied Joesink-Mandeville to Honduras—variously for field or laboratory work—-at Yarumela. Later, I began my own field project in western Honduras as part of my doctoral research. Between the years of 1991-1998, I was actively engaged in archaeological fieldwork outside of the U.S. While I had many experiences that might be seen as exciting “adventures” to some, I narrowly escaped serious injury or danger on a few occasions by being prepared and sheer good luck.

For me, I found that fieldwork in Honduras was facilitated by my Spanish language skills and familiarity with Central American cultures. I am Chicana (US born Mexican American) and had since the early 1980s worked with the Central American community in the Los Angeles area. From my personal experiences and studies, I had acquired a great deal of cultural knowledge about Central American society. During much of the time that I worked in Honduras, many of the highest positions of the Honduran Institute for History and Anthropology (IHAH) were held by women. Through my work with the Yarumela Project I became acquainted with several Honduran professional women in the IHAH—and I believe that as a Spanish-speaking woman I was able to establish myself as a professional foreign archaeologist in Honduras. Several times I was asked to supervise salvage work for the IHAH and report findings of the excavations. These “emergency archaeology” situations not only provided me opportunities to demonstrate my abilities, but also proved to be important for networking. Once so established, when it came time to begin my doctoral research, the necessary permits were issued under my own name rather than a supervising archaeologist, the standard practice at the time. Thus, I believed that I was well prepared to undertake archaeological fieldwork and had a number of advantages.

Nevertheless, I would find that I also had a number of disadvantages—primarily due to my gender. Honduras, as a Latin American country, is a largely patriarchal society—and one that valorizes machismo—a hyper-idealized masculinity. Gender roles are more traditional and women experience daily discrimination and violence. Yet, gender roles in Honduras are not set in
stone and social status is also influenced by class and race. Since I was a foreigner, I was generally exempt from prescribed gender roles. As a foreigner and light skinned Latina, I was classified as white, upper class, and educated—all characteristics that mitigated my gender. My gender worked alternatively to my favor or detriment, depending on the situation. Generally, difficulties that arose with respect to my gender were minor and easily rectified. For example, I usually found that Honduran men typically did not want me to do heavy lifting, digging, or carrying. This was because as a foreign “white” woman, it was expected that I be waited upon, much like a colonial memsaab. In such cases, I often felt the need to “prove” that I could work like a man to be taken seriously as a woman, but also to actively work against ingrained notions of colonialist privilege (both mine and theirs).

At other times, I felt that my gender put me at a disadvantage and exposed me to greater danger. I was particularly aware that I was not free to walk about at night and at all times needed to be aware of my environs to avoid assault.

Traveling and working in foreign contexts can present any number of challenges—from poisonous snakes to revolution. This article draws on incidents that occurred while undertaking archaeological research in Central America and will highlight a few serious hurdles that arose that I did not anticipate. I will present three problems, how I (mis)managed these problems, and more importantly, what I learned from these experiences. Rather than ignoring that ethical, physical, and political problems can and do arise during fieldwork, this article is intended to expose a few problems I have had in order to increase awareness, provide points for discussion, and some possible solutions.

**Challenge 1: Finding Landowners Before They Find You**

Working in rural areas of Honduras has offered me an opportunity to learn hard lessons about the importance of social networking, patience, and approaching people from a humble perspective. On two occasions I failed to take the above in mind, and was met with unwelcoming, suspicious landowners—both times with weapons drawn. I believe my gender may have saved me from more hostile encounters. The problem arose when I took at face value, from IHAH representatives, that archaeological sites are national patrimony and under the purview of the Institute. According to Honduran law, registered archaeological sites are part of the national trust. There are various national laws regarding archaeological sites that establish ownership, access, cultivation of lands on and around archaeological sites, and protection. Legislation addressing archaeological resources is predicated on the notion that the past must be protected from destruction and deterioration.

In 1996, I was searching for an archaeological site appropriate for a dissertation project in central Honduras. I had identified a number of possible sites that had been registered, but not excavated. I was given permission by IHAH to inspect a number of sites in order to determine which site I might research for my doctoral work. Armed with maps and naïve enthusiasm, I began visiting archaeological sites. One site appeared promising from the literature—not too large, not too small; sizeable mounds and near the regional center. I inquired around town about the landowner and was told that he lived in a more distant village, had numerous landholdings, and was not easily located. Not able to contact the owner and notify him of my intended visit (the notion of permission was not operable in my imagination), I proceeded to the archaeological site for an on-site inspection. I had previously been informed that land owners could not prevent research authorized by the IHAH.

The site proved interesting, and I decided that it would be useful to make a sketch map for future reference. As I was mapping the site with an assistant from the archaeological museum, we were approached by a very angry man—with a drawn pistol. It was the landowner, and he informed me that I was trespassing and should get off his property. I attempted (in Spanish) to explain that I had a permit from the IHAH—but to no avail. The landowner was uninterested in my credentials (and he could see that I was driving a Government vehicle) and explanation. I was summarily ejected from the land with the threat that if I trespassed again I would be shot. I did not doubt his sincerity.

Eventually, I was able to procure the landowner’s permission to enter his land and conduct research at this site. It took, however, a great deal of effort, much lost time, and I had to call in a very big favor in order to get that permission. Fortunately for me I had, a few years earlier, given a lecture at a local Casa de Cultura (culture house) and was on good terms with Señor X, the director. Señor X was a highly respected poet, local intellectual and patron of the arts. I consulted Señor X after being thrown off the site at the point of a gun and explained that my plan was not going well. Shaking his head in disbelief, Señor X expected that it would be very difficult to get permission from the landowner who was renowned for being obstinate, foul-humored, and paranoid. Señor X admonished me for my gringa hubris. I had failed to realize that archaeological politics can be quite contentious in Honduras. No matter the location, there are inherent conflicts between national patrimony and private property rights. In this particular case, the landowner was likely concerned that any attention given to an archaeological site on his property could result in the state declaring the land for the state and acquiring it under public domain laws. Justifiably or not, the landowner was afraid that he would lose his land and receive inadequate or no compensation at all. Señor X paid a personal visit to the landowner to plead my case. After what seemed like hours of cajoling and negotiations, Señor X was able to obtain permission for me to enter the property for preliminary work. I completed a sketch map and seriously considered conducting excavations at the site. However, since another opportunity arose, I did continue working at this particular location.
What I Learned from this Experience

Perhaps the most important lesson learned about the above experience is that not everyone sees archaeology and archaeological sites in the same manner. Archaeological sites are contested terrain for numerous reasons. In particular, I learned that in cases of the public good and private property—someone may, in fact, experience damages whether financial, family legacy, or rights of access. As an archaeologist, it is important for me to recognize that archaeology is not a universal value, shared equally by or among all people. Issues of eminent domain are often quite contentious whether in the United States or Honduras. Resources claimed by the state for the public good can come at the cost of a specific individual. In this particular case, my lack of understanding complicated my research—and put me at risk of injury. While in the long run I was able to procure permission, it was costly in terms of time, social capital, and patience. However, the most important lesson learned is that it is important to take one’s time when approaching new situations. In retrospect, I assumed too much, and assumed, with hubris, the authority of the state. I took my privilege and unthinkingly expected that all members of Honduran society would be open to my research and appreciate it as beneficial to them.

Challenge 2: Working a Chain Gang?

Ethical dilemmas do arise during archaeological fieldwork—often times one is forced into situations beyond one’s control. I experienced such a situation in Honduras. In 1996 I was engaged in lab work with the Yarumela Project in Comayagua, Honduras. I was contacted by a representative of the IHAH and asked to supervise a salvage excavation in a neighboring town. Workers digging water pipelines had come across what was believed to be large pre-Columbian pottery vessels. By the time that I was notified of the find, several local officials had mobilized a “salvage” operation to excavate the vessels. I was retrieved by a local school teacher and taken to the site. Already on location were the town mayor, who was so excited about the excavations that he got into the trench himself, the director of the local culture house, the director of the regional museum, and numerous neighborhood residents. The town is poor, with few resources, yet with enormous pride—there are numerous large pre-Columbian sites in the vicinity. Decisions regarding the excavations were being made by the local authorities (e.g., mayor, a respected school teacher, and museum director) that were older, male, and Honduran citizens. My role in this event was ambiguous and I was ambivalent about it. First, I was considered an “expert” since I was a college-educated archaeologist. Yet, I was also a woman and a foreigner. The position of foreigner can be simultaneously privileged and precarious. My authority in this instance was limited to the technical aspects of archaeology—how the excavations would proceed. I did not have the authority, power, or means to influence any other aspect of the excavations. It was decided that excavations would proceed under my general direction, but I would only direct, not excavate. Furthermore, there were no municipal funds to pay an excavation crew; so the town mayor decided that two “volunteers” would be “recruited” from the nearby prison. Much time was spent waiting for the prisoners to arrive with an armed guard. I never found out what crimes these men had committed, their names, or whether they had “volunteered” or not. The situation, for me, was highly objectionable. The prisoners were not motivated to dig in the excavation trench; their initiation to archaeology was not romantic, adventurous or exciting. Rather, they were being forced to dig in a trench in direct sun with no pay. The prisoners were even jeered and ridiculed by the onlookers. At one point, I jumped into the trench to excavate a delicate area myself. The townspeople immediately began to voice their disapproval that the gringa archaeologist worked harder than faster than the men. I was embarrassed by this situation for myself and the prisoners, yet felt powerless to alter the circumstances.

Ethical guidelines for research projects on human subjects explicitly recognize the vulnerability of incarcerated persons. Prisoners cannot “volunteer” to participate in research studies because they are susceptible to manipulation and exploitation. Further, prisoners may not be used as a population of convenience. Research on human subjects conducted by academics generally is evaluated by IRBs (insert full word). Archaeological research, however, is generally exempt from IRB review since the “human subjects” are no longer living and part of the archaeological record. Consequently, I was unfamiliar with IRB guidelines regarding prisoner populations—and had I been, the guidelines would not have clarified my dilemma. Graduate programs and courses frequently cover ethical issues in archaeology. However, these issues are generally constrained to issues about archaeological sites, artifacts, and policies/politics of archaeology. I do not recall ever reading or discussing the issue of human subjects in archaeology.

What I Learned from this Experience

In this case, I could not have anticipated that convict labor would be used in an excavation that I was involved with. I was faced with an ethical issue; I was uncomfortable and realized that I would be complicit in the exploitation of vulnerable people. Yet, I felt powerless to act. As a foreign archaeologist, I rely on the Honduran government for permission to engage in research in the country. De facto, I must be mindful of the situation and act in a manner coherent with the needs of the state. I had been engaged by a governmental agency to oversee what should have been a fairly straightforward excavation. I was granted no other authority than to ensure that the excavations proceeded in an archaeologically sound manner. In retrospect, I am not sure I could have been prepared for this situation. Further, my options were limited. I could have refused to participate on ethical grounds. Potentially, however, a refusal might have resulted in having no one supervising the excavations leading to loss of data or the destruction of an
important site. I could participate or not, but truly had no other option. What I learned was that real life archaeology can present ethical challenges that have no clear moral standpoint. The value in this experience was gaining awareness that there are gray zones in ethical conduct in the field.

Challenge 3: That Belongs in Our Museum!

The final day had arrived—my field work was completed and I was leaving the town of La Unión, Honduras. I had spent months in the small town while conducting my dissertation research. During those months, I had remained largely a gringa to the local populace. I had served as an interesting feature in town—as a gringa, I could violate expected norms of behavior without any serious repercussions. I was not, however, welcomed to more than one or two homes, and even then the invitations had been offered more out of interest than hospitality. On one occasion I was invited to a large public ceremony and seated with local authorities due to my social status as an educated foreigner. Although I experienced many problems during my stay, there were times that I was treated deferentially. On this, the last day of my residence in town, I was busy packing up the truck with the fruits of the project’s labor; bags of ceramic sherds, a number of ground stone implements, and equipment. I was relocating to a larger town that had facilities for archaeological lab work as well as more amenities. The day of the move was hot, dusty, and nearly a three-hour drive to a hot shower, cold beer, and a “real” bed. I was pleasantly surprised when one of the municipal councilmen arrived at the door to invite me to “city hall.” This councilman actually worked on my project in the first couple of months. He was a hard worker in between jobs—with a little extra time on his hands willing to work on the project. He later quit—he had asked for a larger raise than I could afford. I considered the issue moot—there were other more lucrative opportunities at hand so I wasn’t surprised when Don A. decided to quit. But, on that last day, Don A. appeared at my door in the capacity of councilman—unbeknownst to me—he had the power of the municipal council to call me to a town meeting. I mistakenly regarded him as a former employee—an engaged town citizen calling me to a meeting with the town council to congratulate me on a job well done. I was well mistaken—again, I was very, very mistaken. The town council had convened in order to issue a demand that any artifacts uncovered in excavation were property of the municipality—and that they would be confiscating the materials. I was in shock.

I was now in the middle of a local versus national conflict that I had not anticipated. On my arrival to town several months prior, I had shown my permit issued by the IHAH in the national capital to the mayor. I had assumed that since my permit was in order and I had IHAH employees on my project that there would be no problems with excavations. What I had not counted on was that the local municipal government resented the national government. The municipal council apparently feared that once the excavated artifacts left the town they would never return. On the local level, “national patrimony” became the property of the state which was controlled from the national capital city. I learned that small rural communities like La Unión generally believed that the “best” archaeological specimens were taken away for placement in the museum in Tegucigalpa—several hours away from town by bus. Even though I was in compliance with national law regarding the transfer of artifacts from one town to another, the townspeople objected. Taking the artifacts out of La Unión represented a loss of their local patrimony, control, and power. As I ascertained what the problem was—I had to quickly formulate a response and new plan. Clearly I was not going to be able to simply drive out of town. Ultimately, I was able to resolve the issue, but nevertheless had to beat a hasty retreat. The council was demanding a complete inventory of all the artifacts to be taken out of the town. I explained that since I had not been previously notified of this condition and that I had not yet compiled a complete inventory, I could
not meet their demand. Rather, I invited the council to examine the materials that had been excavated. My impression was that the council members believed that valuable artifacts, such as jade, gold, and elaborate pottery had been excavated. Instead, the excavations had rendered nothing of “value”—no complete pots, jade, or other artifacts that could be displayed in a local (and non-existent) museum. Once the council members recognized that there were no fancy artifacts, they seemed to lose interest in keeping the collection. While the members debated the issue, I returned to the truck which was now loaded, got into the car and drove off. The municipal council had neither the legal standing nor police force to detain me.

**What I Learned from this Experience**

From this experience, I learned that there can be differences of opinion regarding who “owns” and benefits from archaeological sites and materials. In this case, the local populous believed that the designation of artifacts as “national patrimony” was a legal means for the capital city (core) to appropriate resources from the rural inhabitants (periphery). This was not merely an ideological conflict, but one with potentially significant economic ramifications as well. The municipal council reckoned that the archaeological site of La Union had the potential of becoming a tourist attraction. If the local authorities lost control of the site and its artifacts, then they would also lose any revenues that might directly result from tourism development. But, the council was ill prepared to address this possibility. The town had no secure location to house the artifacts and no legal basis for demanding that they stay in the town. Further, once it became apparent that the artifacts had no monetary or expository value (that is, not fit for museum display) the council members ceased to be invested in a fight with the national government.

In retrospect, I believe that I should not have been so naïve in my dealings with the municipal authorities. I did not recognize that conflict existed between local communities and national governments. I knew what my rights and responsibilities were vis a vis the national government, but had not anticipated that I would be held accountable (contradictory to the national edicts) by the local officials. A possible solution to this situation would be to meet with local authorities and representatives from the national office early in the field season to directly address legal issues, as well as allow for questions and discussion. Had such a discussion been held in this instance, I would not have been in the difficult position of telling municipal officials that they were in violation of national policy.

**Conclusions**

I have presented three different situations that arose while I was conducting archaeological fieldwork. These problems may have been made easier or more difficult for me based on my national origin, race/ethnicity, gender, education, or class origins. I am convinced that being fluent in Spanish facilitated my research and helped me to better explain myself when confronted with a volatile situation. But, as a woman, I had to overcome gender based obstacles in Honduras, many already encountered in my life in the US. Yet, as a woman, I likely avoided being shot as illustrated in challenge 1. Pitfalls in archaeological fieldwork are bound to happen, yet situations will vary based on one’s gender, race/ethnicity, nationality, class, or social status. My goal in writing this article was not to provide absolute answers, but rather begin a dialogue about the challenges inherent in the “doing” of archaeology. My hope is that these vignettes will prove thought-provoking for anyone, student or professional, planning on conducting archaeology in any setting.

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The Society for Applied Anthropology (SfAA) announces its search for the next editor of its journal, *Practicing Anthropology*. The current coeditors, Professors Jeanne Simonelli and Bill Roberts, will complete their term on December 31, 2008. The new editor(s) will assume responsibilities for a three-year term on January 1, 2009.

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