A Case in Case Study Methodology

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Articles

What We Learned from a Photographic Component in a Study of Latino Children's Health
Lauren Clark and Lorena Zimmer 303

A Case in Case Study Methodology
Christine Benedichte Meyer. 329

Another Complex Step: A Model of Heroin Experimentation
Michael H. Agar 353

Research Uses of the U.S. Freedom of Information Act
Raymond M. Lee 370

Learning How to Find Out: Theories of Knowledge and Learning in Field Research
Cati Coe. 392

Reviews

Narrative Inquiry: Experience and Story in Qualitative Research, by D. Jean Clandinin and F. Michael Connelly
Judith Preissle. 412

Grounding Grounded Theory: Guidelines for Qualitative Inquiry, by Ian Dey
Janet W. Salaff. 414

Index 418

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What We Learned from a Photographic Component in a Study of Latino Children's Health

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This article reviews the contributions of three different kinds of photographs taken in a study of Latino children's health. The three photographic methods were photos of children taken by their mothers who were given disposable cameras, photos taken by research staff during regular home visits, and photos taken in a day-long period of intense observation. Using qualitative and quantitative comparisons of the photos generated by these methods, the authors conclude that the Day in the Life method—although the most expensive—also provided more new information. Specifically, the authors learned more about the children's family relationships, feeding patterns, and the safety and stimulation of their home environments.

John Collier, Jr. and Malcolm Collier (1986:5) wrote that "the critical eye of the camera is an essential tool in gathering accurate visual information because we moderns are often poor observers. Its sharp focus might help us see more and with greater accuracy."

To accomplish exactly what the Colliers suggested, we added a photographic component to a study of Latino children's health.[1] Rather than aiming for one "truth" about the children's lives and health, we hoped to see more of their home life, see it from different perspectives, and be able to talk about what we saw by having photographs that captured their home environments

This project was funded through a grant to Lauren Clark from the National Institutes of Health (R29 HD32366) and a companion grant, called a Minority Graduate Student Supplement, that funded Lorena Zimmer's participation in the study and the photographic component of her independent mentored research. Lisa Hofsess, who was employed on this grant during the same time period, worked with us on this and other research projects and offered valuable insights throughout the study. Kristin Leonardi and Jennifer Whalen offered their services in photographic cataloging, and we thank them for their efforts. We also thank Oswald Werner and Margarita Kay, who talked with us about the photographs described in this article and offered suggestions about photographic methods in general and suggested interpretations of the photographs taken by the mothers.

Field Methods, Vol. 13, No. 4, November 2001 303–328

and activities. We were particularly curious to learn what more we would see and what greater accuracy we would find by bringing a camera into a home with families we already knew and had studied. Specifically, we wanted to explore how photographic data could expand, confirm, or challenge information collected using other research methods, such as interviews with mothers and photographs taken by mothers.

In this article, we describe each of the three photographic methods we used and the challenges they presented. Then we compare and contrast the results generated from the different methods, particularly that of the Day in the Life of a Toddler, to discuss what we learned about Latino children's health. In the conclusion, we show how this photographic component to an ethnographic research project enhanced our ability to see more and with greater accuracy children's home life and health.

Many researchers have generated photographic banks of data for fieldwork documentation and descriptive purposes. A classic example of detailed fieldwork and photography is the work of Margaret Mead, Gregory Bateson, and colleagues in Bali (Mead and Bateson 1942; Mead and Macgregor 1951). More recently, Johnson and Griffith (1998) demonstrated quantitatively oriented ways of gathering and analyzing photographic data. They worked with a sample of fishermen who were instructed to shoot two rolls of film on anything they would like. The researchers wanted to test hypotheses about how the ethnic background of fishermen affected the content of their fish camp photographs.

The innovation and emotive power of research photography is exemplified in health-related research. Using photographs taken by disabled people, Highley and Ferentz (1988) studied the lived experience of disability. Other health-related photographic studies include Highley's (1967) work on maternal role identity with young mothers and their firstborn infants, Magilvy et al.'s (1992) research on rural aging, and Wang, Burris, and Ping's (1996) study of women's health and development in rural China.

The photo novella research described by Wang, Burris, and Ping (1996: 1395) was an explicit tool for empowering Chinese women to "express their vision, literally and figuratively, to policy makers" and it "put a human face on the data" that shapes public policy. Their work did, in fact, change three different public policies related to women's health in rural China. Later, the photo novella approach was called photovoice, since it gives voice to people who "can identify, represent, and enhance their community through a specific photographic technique" (Wang and Burris 1997:369).

Unlike the photo novella or photovoice research Wang described, we did not set out to reach educational or empowerment goals. Instead, we pursued a photographically descriptive process for women to communicate their lives

to the researchers and for the researchers to record the lives of the women and children in the study.

Photographic research is hermeneutic in that photographs are amenable to interpretation and allow observers to view the human experience from a particular perspective (Hagedorn 1994). The photographs collected about health for Latino children can be considered a "visual diary or raw data bank" for thematic and pattern analysis (Highley and Ferentz 1988:123). We used three different ways of collecting photographs to supplement interviews in this study of Latino children's health.

The study, conducted in Denver, Colorado, between November 1995 and April 1999, involved twenty-eight women who were interviewed about child health issues from pregnancy through their child's nineteenth month of age. The population of Hispanics in Denver was approximately 450,000 at that time, and the families included in the study were selected by convenience to participate if they met inclusion criteria (Latina, not a first-time mother, not a teen mother, pregnant at time of recruitment) and were willing to be involved in a longitudinal child health study.

PHOTOGRAPHIC METHODS

We chose mother-generated photographs of their children as our initial photographic method. We were particularly interested in what we called the *index child*, meaning the infant born during the course of the study. Starting with a visit soon after the index child's birth, we began giving women cameras. Thirteen women were given disposable cameras at three-month intervals to record events or situations they considered relevant to children's health in general or the health of the index child specifically. This resulted in 1,018 mother-generated photographs.

Then we added photos taken by both authors and a research assistant. Each of us carried cameras to home visits, and we took 943 photographs during the study. A final photographic source was a Day in the Life of a Toddler Project, or Day in the Life, as we came to refer it. We added this documentary approach in the final months of the study to capture the daily activities of three babies for one day. A total of 1,234 photographs were generated from the Day in the Life project.

Photographs taken by mothers, by the researchers at home visits, and by researchers during the Day in the Life project were all cataloged into a Microsoft Access (1998) database. Access is one of many database programs that can be adapted for cataloging photographs by multiple fields of interest. ProCite (1998), a bibliographic database management program, and Atlas/ti

(1997), a qualitative data analysis program, are other programs that can be set up for cataloging and annotating photographs. Atlas/ti is also equipped to import and analyze computerized graphical data in twenty different file formats, including Bitmap, TIFF, and Kodak Photo CD formats. One advantage of using Microsoft Access is its ability to display a digitized thumbprint-sized photograph corresponding to each data entry form.

Before taking any photographs of the families, we told the parents about the study's purposes and procedures. We used a protocol and consent form approved by the Colorado Multiple Institutional Review Board to assure families' protection as research participants. Each adult or adolescent photographic participant was asked to sign an additional consent form before being photographed, and permission to photograph minor children was granted by parents. We were also dedicated to providing the families with copies of all the photographs in which they appeared.

The same team of researchers was involved throughout the study. Lauren Clark, the principal investigator, directed the study. As a nonnative Spanish speaker, former public health nurse, and mother, Lauren felt comfortable talking about and photographing maternal child health issues in young mothers' homes. Lorena Zimmer was a graduate research assistant when this study was undertaken. As a native Spanish speaker, Lorena conversed more easily in the households that were only Spanish-speaking. Lisa Hofsess, a nonnative Spanish speaker, was also involved as a research assistant throughout the project. All of us were comparable in age to the women in the study, whose mean age was 28 years. There were few barriers to communication with families involved in the research; since we all spoke Spanish, the language used for interviews and photographic sessions was English, Spanish, or a mixture of both, depending on the mother's preference. Overall, we spoke slightly more often with mothers in Spanish than in English during the study.

MOTHER-GENERATED PHOTOGRAPHS OF CHILDREN

When the Latino Children's Health study was planned, a schedule of interviews with mothers about child health was to be supplemented with mothers' photographs of their children in health-related situations. Mothers were given disposable cameras and asked to take the twenty-seven frames during the three-month interval between visits. At the next visit three months later, we would collect the disposable camera and order two sets of prints. At the following visit, we would return the duplicate prints to the mother and ask her to tell us more about what was happening in the pictures.

Unforeseen factors complicated this plan. Of the 2,300 photographs possible from this method, only 1,018 were developed and cataloged into the research database. There are four possible causes for this disparity.

First, some of the newer immigrant mothers had little or no experience with a camera. The workings of a disposable camera, although not sophisticated, were a barrier to their photographic impulses. Paloma[2] reported that her camera was broken and that she could not take any photographs. One of us (Clark) inspected the camera and found she had simply failed to advance the film to the first frame. We quickly learned that demonstrating this to all mothers when we delivered their first camera saved frustration on all sides. After Paloma learned how to advance the film, she reported that the camera was broken again at the next visit. This time, the button controlling the shutter had been depressed with such eagerness that it was mashed down irreparably.

A second challenge mothers faced in taking snapshots of children's health was their need to use the camera for other purposes. For example, Marisa used her research camera to document the damage her vehicle sustained in a car accident. Marisa's family owned no other camera and the research camera was on hand to record information critical to her insurance claim. This recording of a car accident that affected her family's health stretched our initial preconceived notions about what "health-related" photography would end up looking like. On another occasion, Delores's elementary school-aged son wanted to take a camera on a field trip, so the research camera was given to him to record a train ride with his school class. Research cameras were used by families to meet their own needs at various times, making us aware that our goal of generating health-related photographs was a low priority in certain circumstances. We began to view mothers' photographs of daily life, their neighborhoods, and their car accidents as unanticipated windows into the context of health for the children in the study.

A third challenge to collecting informative maternal photographs about children's health was that mothers were the predominant photographers. This meant that they were rarely in the photographs, eliminating the opportunity for a photographic record of interactions between mothers and children. Also, because mothers were the primary photographers, some of them reported being too busy to remember to take photographs in the midst of running a household and caring for children. "Oops, I forgot to take any photographs this time" was heard on occasion, as well as, "Luckily, I remembered you were coming and took the whole roll yesterday." Ideally, all twenty-seven photographs would have been spread out over the three-month interval. This was seldom possible.

Finally, mothers were often anxious to see photographs soon after taking them. Some felt waiting three months for the pictures to be developed and

returned by the research team was impractical. Consequently, mothers sometimes developed the film themselves without making a copy for the researchers. All of these problems contributed to a photographic database of maternal snapshots of child health that was interesting, but partial and inconsistent.

RESEARCHERS' PHOTOGRAPHS OF CHILDREN

Anticipating that mother-generated photographs would be limited, we planned to supplement them with photographs we would take on every home visit to document the settings of the home visits and the people present at each visit. On average, we took about four pictures per visit.

The challenge we faced using this method concerned the representativeness of people and settings. Nearly all the home visits were scheduled during the day, so school-aged siblings and working fathers were often not at home. It seemed most comfortable to stage a photograph toward the end of the home visit with the mother and child (and sometimes other children) sitting on the couch and smiling for the camera. Photographic settings were limited, too, in that most were posed in the living area of the home. Daily activities like bathing, diapering, feeding, and family interactions were occasionally pictured, but inconsistently. Even more limited was the time span represented by the photographs, since the researcher was only in the home for two hours every three months and took a small number of photographs. This provided an interesting visual record of the home visit participants and setting that was comparable across all families, but it was not a systematic record of any one child's home or family environment.

When reviewing the limitations of the mother- and researcher-generated photographs, we concluded that more productive photographic data could be gathered if we spent more time with each family and overcame the sense of formality in our photographs. Given that time and money were limited, we pursued these objectives with three families in what we called the Day in the Life of a Toddler Project.

THE DAY IN THE LIFE OF A TODDLER PROJECT

Of the three methods we used to gather photographic data, this was the most intensive in cost, time, and photographic output. Our goal was to systematically capture the events that occurred during each child's typical day,

focusing particularly on health-related happenings. We considered health-related happenings to be any number of daily tasks that contributed to keeping the child fed, clothed, clean, content, safe, and stimulated.

For consistency and comparability, we carefully considered which families to include in this project and based inclusion on certain criteria. First, we planned to select families to represent different levels of acculturation. Before initiating this project, all the mothers in our study had completed the twenty-seven-item Los Angeles Epidemiologic Catchment Area (LAECA) acculturation rating scale (Burnam et al. 1987). Acculturation rating scales are far from foolproof indicators of fixed levels of acculturation, but can be useful in indicating language skill and preference and social interactions with other Latinos and non-Latinos.

Second, by looking at the acculturation scores (ranging from 1 to 5), we developed three acculturation categories based on LAECA acculturation score and our knowledge of each woman's life situation and our perceptions of her acculturation. The acculturation scores and our independently derived subjective impressions of acculturation were congruent in the majority of cases, and resulted in the least-acculturated group containing thirteen women (LAECA score < 2.0), the bicultural group containing seven women (LAECA score 2.0–3.0), and the highest acculturation group containing eight women (LAECA score > 3.1). Knowing we ultimately wanted one family from each acculturation group, we began to consider other areas of comparability between the families, such as family structure and age and gender of the index child. By reviewing the families on our roster, we were left with maximum inclusion if we selected families with live-in fathers and similar numbers of siblings.

Third, we invited families to participate who we believed posed no safety issues. In other words, if we knew the family was actively involved in gang, drug, or domestic violence situations that could prove explosive and threatening (for the family or for us) through a day of constant companionship, we did not want to provoke problems. Safety did not become an issue, since each of the families identified as suitable for inclusion to this point also met this safety criterion.

Finally, to protect the right to privacy of the families we would work with, we discussed our selections as a research team and agreed that—to the best of our knowledge—the families we wanted to recruit were not housing undocumented family members or family members who were known to be at risk of legal action if their photographs were seen publicly. At the conclusion of this selection process, we invited three families to be participants in the Day in the Life project. All three had female children from twelve to seventeen months of age with one to two older siblings, all had live-in fathers, none had known

safety or privacy issues, and each family represented a different acculturation group. All three families accepted the invitation to participate in this portion of the study.

Once we arrived in their homes, we clarified with each adult who came in contact with the family that day that the photographs we were taking could be publicly viewed at a future professional meeting or in a professional journal. Each person photographed completed a photo consent. To recognize each family's effort to accommodate this research endeavor, we offered them $100 in cash at the end of the day.

For this project, a two-person team went to each household. We took with us a field notebook and two cameras. The primary camera contained black-and-white film for photographs of the family, and a secondary camera contained color film for photographs of the home environment and was a backup camera, if needed. Having two researchers was an advantage, since one could take pictures while the other kept notes of the events that were occurring, record the approximate time of day the photographs were taken, and maintain an orderly record of the sequence of the exposed rolls of film. Preserving the sequence of events was important, and each roll of film was carefully labeled to allow verification of events in the photographic record with notes if the photographic record was confusing. In addition to the record of events in the field notebook, one of the cameras had a date/time stamp that appeared on each photograph.

To be certain we captured the full "day" in the child's life, we planned to be at the family's home at the time the parents indicated as their child's usual waking time. Typically, we arrived at 8:00 A.M. and stayed until the index child went to sleep at night. On average, we stayed at each house 13.5 hours. The total number of pictures and the types of photographs taken were driven by the actions of the child. Capturing sequences of photographs and recording events as they unfolded was critical to amassing analytically worthwhile visual evidence (Collier and Collier 1986:163).

Some children's activities (such as eating or bathing) resulted in a flurry of photographs, whereas other happenings, like repetitively playing with a toy for several minutes or watching a television program, generated less-frequent photographs. Averaging these bouts of intense photography with periods of relative quiet, we took approximately one picture every 1.75 minutes; no pictures were taken during the child's nap time. For Family A, the least-acculturated family, 410 photographs were taken. Time spent with the bicultural family (Family B) generated 422 photographs, and the family with the highest acculturation (Family C) had 402 photographs taken of them. As with the other photographic methods, the families were given copies of all the photographs taken during the Day in the Life project.

Taking photographs and observing each family for a full day added substantially to our understanding of the families and our photographic record of their children's lives. During our day with the families, we were free to leave the formal, structured environment typical of the home visit interview and follow the mother and child throughout the house during their normal activities. This freedom to move about the house with the family helped us see various events—like children's temper tantrums and sibling quarrels—that we didn't see during more formal visits. We became bolder, and possibly better, photographers once we left the couch and stayed with the families for an entire day.

The limitations of the Day in the Life project were of a different scope than those of the informant and researcher photographs previously described. The challenges we faced were related to the project's intensity. Since this project was added late in the research, some financial limitations had to be considered. Purchasing and developing approximately forty-five roles of film and paying families for their participation brought the total cost of this project to approximately $2,500. This cost limited the number of families we could include, which, in turn, limited our opportunities to compare daily events and children's activities across families. Although we chose three families, each from different acculturation groups, this information does not hold external validity. We were able to spend quite a bit of time with each of the families, and the photographic data we gathered added substantially to the interview data. Still, one day is, after all, just one day, and we must be careful about how we extrapolate Day in the Life patterns of behavior.

We had to take great care in sequencing the reloading of film to make sure that the film was kept in the correct order from roll to roll. Even more challenging was coordinating the field notes with various photographic sequences. By dating and timing all entries into the field notebook and using the time-and-date stamp on the camera, we were able to coordinate the photographic and field note records with relative ease. The time of day the events on film were captured was important if we ever wanted to identify feeding times or calculate frequency of feeding, number of naps, or number of minutes watching television.

ANALYZING PHOTOGRAPHS FOR CATEGORIES AND THEMES

Photographs saved in the research archives were all cataloged with the family identifier, date, and age of the child using Microsoft Access. All photographs were also digitized at the time of development and stored on photo-

graphic CD-ROMs or disks. A digital camera was not available for this research, so the film submitted to the developer was saved in a digitized format on CD-ROM. Using a digital camera would simplify some aspects of this process and reduce the cost.

All photographs collected from each of the three photographic methods were cataloged and entered into the Access database with information recorded in nine different fields. The database was designed to prompt us to list the number of the photograph, family code, age of the index child at the time of the photograph, approximate date of the photograph, activity of the index child, location where the photograph was taken, and the type of photograph (color, black-and-white, or slide). In photographs with many children, we recorded the position of the index child so that he or she could be identified at a later date. We also filled in a general notes section if we recalled important events or discussions that had taken place at the time of the photograph. In the child activity section of the database, we created a coding system to label the child's activity in the photograph. Table 1 lists these activity codes and descriptors.

We planned to conduct two different analyses using information from the photographic database to compare and contrast the different photographic methods. The first analysis was a qualitative assessment of the similarities and differences in photographic content between the researcher- and informant-generated photographs. The second analysis was a quantitative comparison of content between mothers' photographs, researchers' photographs, and Day in the Life photographs.

In the qualitative content analysis, we made visual comparisons between mothers' and researchers' photographs, examining two content categories: the activity portrayed in the photograph and the people present in the photograph. This analysis began by spreading out successive groups of about 50 researcher and 50 mother photographs with the same activity code on a large table. When we did this for the activity code Eat, 100 photographs were simultaneously visible of children eating, snacking, self-feeding, or breastfeeding. By examining researchers' and mothers' photographs side by side, it was easier to make visual comparisons about content and style, and we hypothesized about the photographic intent of specific photographs. As Collier and Collier (1986:9) state, "The camera, however automatic, is a tool that is highly sensitive to the attitudes of the operators," and we saw differences in the perspectives of the photographers and differences in technique and attitude between researcher and mother photographs.

Of the photographs taken by researchers and mothers of children eating, mothers had more pictures of their child eating at a table and many more pic-

TABLE 1
Codes Applied to Photographs to Indicate Activity of Index Child

Code	*Activity of the Index Child*
Action	Sitting, crawling, movement (excluding pose or play)
Area	Locations without people
Baptism	Baptismal ceremony
Baby equip	In swing, car seat, playpen, highchair, walker
Bath	In bathtub, sponge bath (excluding simple grooming such as face and hand washing)
Clinic visit	At a medical clinic
Cry	In the act of crying
Diaper	Having diaper changed
Dress	Changing clothes, dressing, undressing (excluding diaper changes)
Eat	Self-feeding, breastfeeding, bottle feeding, snacks (excluding time in highchair without food [see baby equip])
Measure	Measuring anthropometrics for study
Play	Touching or using toys or objects (excluding playing with people [see interact])
Party	All family-oriented activities or parties, including birthdays, holidays (excluding baptism)
Pose	Formal photograph
Sick	Resting or crying during a known illness episode, possibly with visible rash or indication of illness
Interact	Playing or otherwise engaged with another person, subcoded as mother, father, grandfather, grandmother, sibling, or unknown individual
Other	Any other activity not included above, with description provided in additional notes

tures of the child with a messy face. Researchers' photographs more often portrayed snacking and the mother feeding the child. Comparisons like this were repeated for other codes, and after comparing all photographs of mothers and researchers on similar topics, we conclude that the photographic intentions of mothers and researchers were most similar in instances where both recorded the child doing something clever or cute. Children wearing sunglasses or hats or generally "hamming it up" for the camera reflected the affection of the photographer and emphasized the endearing character of the child. Both mothers and researchers took these kinds of photographs, conveying an appreciation for the personality and loveableness of the child.

The people in the mother and researcher photographs were similar but not identical. The index child was the most common participant in both sets of photographs, but mothers' photographs contained more family members and

friends. In one case, the researchers may not have ever seen the index child's father if not for maternal records of the child and father. Because this particular father lived in a separate residence and had a turbulent relationship with the mother, the photographic record provided a visual image of the father. Fathers were also portrayed by mothers playing with their children or interacting with their adult male friends. One provocative photograph by a mother showed the index child's father with several other men posing in very masculine ways in front of a car. What looked like gang paraphernalia was also in the photograph, providing another view of the family's neighborhood and daily life.

The quantitative analysis compared the relative emphasis of content categories across photographic methods. Using the Access database, we tabulated the raw frequency and percentage of photographs by code taken by the researcher, the informant, and those photographs taken during the Day in the Life project. Then we aggregated activity codes into four larger categories. Action, cry, play, sick, and sleep codes were combined to form the category of Independent Baby Action. The new category, Childcare, included the codes of bath, diaper, dress, eat, and groom. The six codes for interaction with grandparents, siblings, mothers, fathers, researchers, and others were all grouped together as Interaction, and the code, pose, was made into a category by the same name. The result of this comparison of four categories by three photographic methods is seen in Figure 1.

The relative distribution of photographs by content category are presented in percentages in Figure 1 to give an accurate representation of the photographic content by photographic method, since the raw number of photographs per method varied substantially. As shown in Figure 1, there were differences in the proportions of photographs taken in the Childcare and Pose categories. Comparing the percentage of photographs portraying Childcare between the Researcher and Day in Life methods, for example, it is clear that the day-long photographic method captured more of the routine activities of diapering, dressing, grooming, and eating. By getting into the homes of the families when the child woke up, we were present for bathing, dressing, and feeding, whereas during our regular interviews the babies were usually prepared for our arrival looking clean and neatly dressed.

The low percentage of photographs in the Pose category for the Day in the Life method suggests families were desensitized to the photographers and their cameras and lost interest in posing for each photograph. Although posed photographs are scripted events that are familiar to the photographer and the family being photographed, they offered little substantive information on children and their daily health. During the home visits, we found that the

social norms about taking photographs of people resulted in more posed photographs than we anticipated.

In the Day in the Life project, posing was dramatically decreased, although the least-acculturated family still smiled and paused for the camera well into the project. Both the mothers' photographs and researchers' photographs showed more posed shots than the Day in the Life project. Despite our preference for fewer posed shots, we realize that the posed photographs speak to the kinds of images families wanted recorded of themselves and tell us something of their perceived role in the research endeavor as photographic participants. In the future, we plan to analyze each of the different content categories of photographs more fully.

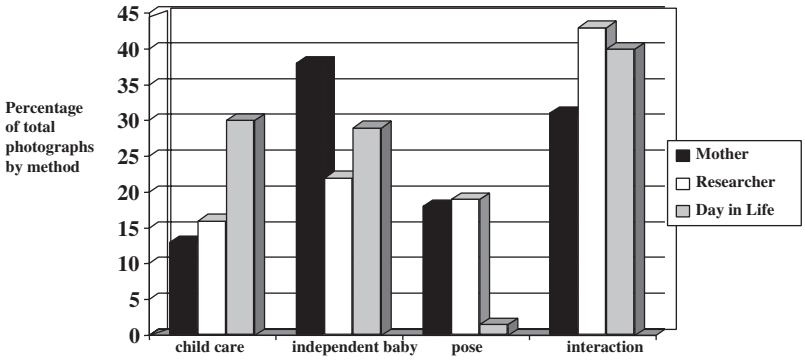
WHAT MORE DID WE LEARN FROM THE DAY IN THE LIFE PROJECT?

In considering the Day in the Life photographs as a whole, the predominant question we asked was, "What more did we learn from this project than from the interviews alone or the other photographic methods?" The answer was that we learned more about three child health-related categories: family relationships, feeding patterns, and the safety and stimulation of the home environment.

Family relationships, including fathers' relationships with children and extended family relationships, were better represented in this photographic method. Fathers changing diapers and feeding babies showed their own style of accomplishing these childcare tasks, and the difference between maternal and paternal patterns of caregiving brought a few amused chuckles from mothers who viewed the photographs later. "See, he always watches TV while he's giving her a bottle," quipped one mother. By capturing *los cariños*, or small gestures of affection exchanged between adults and their children, the Day in the Life project also provided more information about how parents care for children. Like the photographs of caregivers' hands taken by Mead and Macgregor (1951), we saw examples of mothers and fathers caring for children with competence and affection. Their hands and eyes and gestures sometimes spoke volumes about their comfort and familiarity with the index child and their intentions and feelings for their caregiving tasks. The sequence of daily activities (like bathing and eating) and the settings for these activities were well represented.

Methods of communication between the extended family and the index children were also clearer in the Day in the Life project. We photographed

FIGURE 1
Distribution of Photographs Taken by Mothers, Researchers, and the Day in the Life Project among Four Categories of Activity and Interaction



children talking on the telephone with grandparents and their fathers at work. Older children were pictured helping with their younger siblings, kissing, playing, and fighting with them. Nonverbal communication between extended family members and the index child was visible across several photographic frames. For example, the grandmother in the bicultural family was the primary caregiver for Nina during the day while Nina's parents worked. The grandmother gave Nina a bath the morning we visited for the Day in the Life project. The comfortable routine enacted by Nina and her grandmother, their eye contact, playfulness, and way of communicating with each other, was captured in thirty photographs of the bath routine. These photographs emphasize the affection characteristic of their relationship and their easy flow of communication (see Figures 2A through 2D). Contrary to our observation, the interview data we had from Nina's mother did not lead us to expect a compatible, tender series of interactions between the grandmother and granddaughter. According to Nina's mother, the grandmother was overwhelmed and did not particularly enjoy taking care of the children. She certainly had her good days, but Nina's mother suspected that caregiving was too much for her mother:

> There are days when my mother is ready to scream with bloody murder after taking care of the kids. And I tell her, "You encouraged me to have another

FIGURE 2A

NOTE: Field notes that accompany this photograph state: "10:55 A.M. Nina begins her bath in kitchen sink. She pees in water and they start with fresh water. '*Sientate*,' grandmother says over and over to get her to sit and rinse. [Nina] cries and struggles against rinsing." Despite Nina's resistance, her grandmother proceeded calmly and resolutely through the bath time ritual. (Field notes by Lisa Hofsess, photograph by Lauren Clark.)

> one." And she'll say, "Yeah, well I don't know what I'm going to do." I was an only child and she's not used to kids. Even though you would think she's young enough to handle kids, she really isn't. She's had a rough life. A rough life. It just depends on her mood and healthwise how she's doing.

The photographic sequence of the bath was representative of the entire Day in the Life with Family B, in that the grandmother and children appeared to have better rapport and working relationships than would have been assumed from the maternal interview data alone.

Feeding patterns were also better assessed through the Day in the Life photographs than through either maternal interview alone or the other photographic methods. Photographers recorded the kinds and amounts of food the children consumed and where and when they ate.

The most acculturated family (Family C) went out to lunch at a Mexican restaurant during our day together, and gave their one-year-old daughter soda pop with lunch (see Figure 3A). The least acculturated family (Family A)

FIGURE 2B

NOTE: From both visual inspection of the grandmother's gestures and facial expression and the field notes taken throughout the day, there is no sign that she has difficulty coping with the daily activities of caring for Nina and her brother. On the contrary, she appears competent and calm, perhaps even fulfilled. (Photograph by Lauren Clark.)

encouraged their daughter to eat watermelon and corn on the cob for snacks (see Figure 3B), and served a traditional, homemade *sopa de pollo* (chicken soup) for lunch. The bicultural family served their daughter a high-calorie breakfast of frozen waffles, grapes, cantaloupe, tomatoes, ham, and cheese (see Figure 3C).

Acculturation may be one factor explaining the replacement of high-fiber fresh fruits and vegetables with prepared foods eaten at home, and ultimately

FIGURES 2C AND 2D

NOTES C and D: These photographs depict the loving and expressive nonverbal communication between Nina and her grandmother. Recorded in the field notebook is this part of the bathing sequence: "11 A.M. Grandma offers a cold bottle from refrigerator to comfort Nina, who whines a little bit at end of bath." (Field notes by Lisa Hofsess, photograph by Lauren Clark.)

FIGURE 3A

NOTE: Family C is pictured here eating at a Mexican restaurant after taking Filomena to a doctor's appointment at the HMO and filling an antibiotic prescription for her diagnosed ear infection. This photograph depicts Francisca, the child's mother, offering Filomena sips of Coke from a straw. Each of the families drank caffeinated soda pop during the Day in the Life project, including the least acculturated Family A, who served Mountain Dew with a traditional *sopa de pollo* (chicken soup) for lunch. (Photograph by Lorena Zimmer.)

replacing those foods with fast food eaten in restaurants. The "nutrition transition" (Popkin 1994) in developing countries describes these kinds of shifts from traditional, healthy diets to modern, unhealthy ones and associates this shift with the growing problem of obesity. Although perhaps a matter of family preference and habit as much as a function of acculturation, these kinds of differences in feeding patterns were discernable and comparable. Because we also collected anthropometric measures at each home visit, we have compared the feeding patterns we observed during the many home visits and the day of photography to growth patterns over time. The bicultural child's growth chart showed a steady rise in age- and sex-adjusted weight-for-length to the 99th percentile on the National Center for Health Statistics (NCHS)[3] growth chart, meaning that the child weighed more than 99/100 of other little girls of her same age and height. This is a clear indication of obesity consistent with the overfeeding we observed. In this case, the anthropometric data

FIGURE 3B

NOTE: Family A, the least acculturated family, is pictured here sitting on the couch and snacking on cold corn on the cob. As recorded in field notes, "5:50 P.M. Vilma is calmed by bottle Mom makes with strawberry milk. Dad feeds Vilma, then she eats cold corn on the cob." Not all of their eating habits were as exemplary as a low-fat, high-fiber corn snack. From the field notebook we learn that "Mother gives Vilma a cookie while she warms lunch (12:05 P.M.)." Note, too, the ever-present bottle (this one of strawberry milk) sitting on the back of the couch. (Field notes by Lauren Clark, photograph by Lorena Zimmer.)

and photographic data were mutually supportive, with the photographic data recording how the child's obesity may have been fostered.

After looking at the Day in the Life photographs across all families, a consistent finding was the ubiquity of the bottle. Each child was photographed with at least one bottle of milk during the day, and at least one was photographed with a bottle at nap time. Bottles of juice were also common. They appear to be everywhere in the photographs. There are photographs of a bottle dangling from a child's mouth as she watched television, a bottle dripping onto the carpet as a child used it to "paint," and bottles being prepared by mothers and grandmothers.

For children over one year old, as all of these were, bottle feeding is currently discouraged by physicians and nurses because of potentially health-damaging effects of prolonged bottle feeding. Weaning from the bot-

FIGURE 3C

NOTE: The field notebook records the preparation of the high-calorie breakfast and some of the more dogged attempts of the grandmother to get Child B to eat the entire meal. For example: "9:15 A.M. grandmother calls Nina in to kitchen to eat. Grandma makes French toast with toaster waffles and egg, cooked in vegetable oil. 9:23 A.M. Grandma adds ham and cheese to plate, also chopped tomatoes, cantaloupe, and Karo syrup over toast. 9:45 A.M. Nina drops ham on floor; Grandma puts it in kitchen sink and gets another piece from fridge, cuts up with knife. Grandma scolds Nina for playing with food. Nina eats ham." (Field notes by Lisa Hofsess, photograph by Lauren Clark.)

tle should be initiated between twelve and fifteen months of age (Frazier, Countie, and Elerian 1998), and bottles should never be given at nap time or bedtime to avoid a host of problems ranging from obesity to dental decay (Serwint et al. 1993; Bruerd and Jones 1996; Elli and Atkinson 1996). Although we were aware that the children in the study were still using bottles from our maternal interview data, we would have underestimated the extent of bottle use if we had not seen and photographed so many bottle-feeding episodes during the day.

A third health area highlighted through the Day in the Life was the home environment. Because these little girls were toddlers, they showed an amazing ability to explore the environment and sometimes get into trouble. Overall, the children's environments were safe and families used safety precautions appropriately. One family drove their child to a baby shower and a

doctor's appointment and used a child car seat. The same family also bathed the child using a bathtub ring to keep her from tipping over and never left her unattended. Other families showed a similar awareness of safety and attention to environmental risks. The most risky situation we observed was when a child spent several minutes standing on the toilet and leaning precariously forward to play in the sink. Her older sister ended this questionable activity by picking up the child and hauling her back to the living room where she could be more closely supervised.

The children's environments stimulated their cognitive and physical development through the availability of age-appropriate toys, games, and activities. In each of the three households, we photographed books and bookcases. Only the least acculturated family was photographed looking at a book with their child on the day of observation. The other families had bookcases with children's books so neatly arranged that we questioned whether the families read to their children much at all. The least acculturated family displayed a wide range of activities for their child, who was photographed playing with newspapers, purses, a ball, her sister's braid, dolls, an umbrella, her tricycle, and Nintendo. The most acculturated family had a computerized spelling game that the children used, and more than one family played in the yard with plastic toys (see Figure 4A). Other activities stimulating children's development included magnetic letters on the refrigerator and a host of manipulative fine- and gross-motor toys. Although child development and stimulation were evident across all three acculturation groups, the lack of a strong reading ethic for families with toddlers engaged in language acquisition is unfortunate.

The environmental strengths recorded in the Day in the Life project were tempered by the identification of an environmental risk: excessive television viewing. In the two households where the child stayed home all day, both had the television set on at all times, including during mealtimes (see Figure 4B). The third family completed some errands and then returned home and immediately turned on their television set. All-day television viewing as a normal activity for toddlers was an unexpected finding. Research on the outcomes of television viewing for young children has suggested associations between television viewing, obesity (Robinson 1999), and other negative outcomes. The documented negative effects of television viewing on children recently prompted the American Academy of Pediatrics (1999) to issue a strongly worded statement urging pediatricians to counsel parents to avoid television viewing for children under the age of two years.

Although certain television programs may be promoted to the age group, research on early brain development shows that babies and toddlers have a critical need for direct interactions with parents and other significant care-

FIGURE 4A

NOTE: In this photograph, Filomena (the child from the most acculturated family) plays outside on her plastic bike with her mother and sister. This episode on the bicycle, like several instances of children's play across all families, involved many family members and precipitated a situation that elicited a disciplinary response from a parent. The field notes record the episode: "7:00 P.M. Kids ride battery-operated car on sidewalk. Mother smacks the older sister's bottom for not sharing." In Family B (bicultural), sharing outdoor toys was also an issue. As recorded in the field notes, "7:35 P.M. The mother states that Nina won't share, the little car is hers. Nina fell out of the car onto her bottom. Didn't cry. Grandma jumped up and laughingly said, '*Te cayiste*?' [Did you fall?] several times. Nina got up by herself and got back into her car. Then Grandma came and helped her turn the car around." Natural play scenarios, like the two recorded here, identified the kinds of toys and play environments available and the interaction of family members around play incidents like getting hurt or sharing with siblings. (Field notes by Lisa Hofsess, photograph by Lorena Zimmer.)

givers (e.g., childcare providers) for healthy brain growth and development of appropriate social, emotional, and cognitive skills. Therefore, exposing such young children to television programs should be discouraged (American Academy of Pediatrics, Committee on Public Education 1999; www.aap.org/policy/RE9911.html).

For the three families in the Day in the Life project, the strength of their extended family relationships and daily use of environmental protections were health-promoting behaviors emphasized in the photographic record. The children's constant bottle feeding and television viewing were health

FIGURE 4B

NOTE: As a researcher unused to having the television on all day in my own home, I found the constant noise and barrage of visual images to be distracting and irritating. At 11:40 A.M. I wrote: "The TV has been on all day." By that evening, the television was part of the backdrop in a photograph that showed the father feeding the child, the table set with the traditional *sopa de pollo*, and the caffeinated soda pop. The field notebook recorded the events of the meal: "6:50 P.M. Dinner was the chicken soup from lunch but we had rice and chile [this time]. Dad gave Vilma some rice. 7:00 P.M. Mom has Vilma on her lap, feeding her occasionally. 7:05 P.M. Vilma is eating soup. 7:10 P.M. Dad cut up a watermelon and gave out pieces to everyone. [Parents report that] Vilma likes watermelon, melon, and mango." (Field notes by Lorena Zimmer, photograph by Lauren Clark.)

risks apparent only after the Day in the Life project, because neither maternal interviews nor periodic home visits elicited convincing data on these activities.

CONCLUSION

Each of the three different photographic methods we used in this research on Latino children's health had advantages and disadvantages in terms of intensity, cost, quality, representativeness of daily life, and comparability of results across families. We included a photographic component because of our need to better understand and document the household behaviors that contributed to children's health outcomes. For our purposes, the Day in the Life project best met that goal.

Triangulation has been touted as one way of strengthening the robustness and completeness of research data and data interpretations. "The most effective way to ensure reliability and validity of ethnographic data is to obtain comparable, confirmatory data from multiple sources from different points in time, and through the use of multiple methods. This is the process of 'triangulation' " (Trotter and Schensul 1998:719). Including a photographic component in a research study replete with other investigative methods is one way of accomplishing methodological triangulation (Denzin 1978).

In this study, research on the household health practices in Latino families that contribute to children's health already relied on a variety of methods: ethnographic interviewing about child health, a review of children's medical records, and anthropometric measures of each child's growth over the first nineteen months of life. Adding a photographic component was one more way to triangulate methods and strengthen findings.

NOTES

1. For this study, Latino children are defined as children whose mothers self-identified as Mexican, Mexican American, American of Mexican descent, or Chicana, Latina, or Hispanic of Mexican descent. Because of the variability within groups of Latinos or Hispanics, we focused on only Mexican-descent mothers and their children. The women ranged from first-generation (meaning they were immigrants from Mexico) to fifth-generation U.S. residents. To determine women's ethnic identity, we asked them screening questions before enrolling them in the study. If they self-reported a Mexican origin category and enrolled in the study, we asked them to complete the comprehensive Los Angeles Epidemiologic Catchment Area (LAECA) acculturation rating scale (Burnam et al. 1987) to ascertain their level of acculturation and generation of residence in the United States.

2. All names reported in this paper are pseudonyms.

3. The National Center for Health Statistics produces a widely used clinical growth chart with percentiles for physical growth for boys and girls from birth to thirty-six months using data from the Fels Longitudinal Study as a reference (see Hammill et al. 1979). The Centers for Disease Control have raised children's growth charts available at www.cdc.gov/nchs/about/major/nhanes/growthcharts/clinical_charts.htm.

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A Case in Case Study Methodology

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The purpose of this article is to provide a comprehensive view of the case study process from the researcher's perspective, emphasizing methodological considerations. As opposed to other qualitative or quantitative research strategies, such as grounded theory or surveys, there are virtually no specific requirements guiding case research. This is both the strength and the weakness of this approach. It is a strength because it allows tailoring the design and data collection procedures to the research questions. On the other hand, this approach has resulted in many poor case studies, leaving it open to criticism, especially from the quantitative field of research. This article argues that there is a particular need in case studies to be explicit about the methodological choices one makes. This implies discussing the wide range of decisions concerned with design requirements, data collection procedures, data analysis, and validity and reliability. The approach here is to illustrate these decisions through a particular case study of two mergers in the financial industry in Norway.

In the past few years, a number of books have been published that give useful guidance in conducting qualitative studies (Gummesson 1988; Cassell and Symon 1994; Miles and Huberman 1994; Creswell 1998; Flick 1998; Rossman and Rallis 1998; Bryman and Burgess 1999; Marshall and Rossman 1999; Denzin and Lincoln 2000). One approach often mentioned is the case study (Yin 1989). Case studies are widely used in organizational studies in the social science disciplines of sociology, industrial relations, and anthropology (Hartley 1994). Such a study consists of detailed investigation of one or more organizations, or groups within organizations, with a view to providing an analysis of the context and processes involved in the phenomenon under study.

As opposed to other qualitative or quantitative research strategies, such as grounded theory (Glaser and Strauss 1967) or surveys (Nachmias and Nachmias 1981), there are virtually no specific requirements guiding case research. Yin (1989) and Eisenhardt (1989) give useful insights into the case study as a research strategy, but leave most of the design decisions on the table. This is both the strength and the weakness of this approach. It is a

Field Methods, Vol. 13, No. 4, November 2001 329–352

strength because it allows tailoring the design and data collection procedures to the research questions. On the other hand, this approach has resulted in many poor case studies, leaving it open to criticism, especially from the quantitative field of research (Cook and Campbell 1979). The fact that the case study is a rather loose design implies that there are a number of choices that need to be addressed in a principled way.

Although case studies have become a common research strategy, the scope of methodology sections in articles published in journals is far too limited to give the readers a detailed and comprehensive view of the decisions taken in the particular studies, and, given the format of methodology sections, will remain so. The few books (Yin 1989, 1993; Hamel, Dufour, and Fortin 1993; Stake 1995) and book chapters on case studies (Hartley 1994; Silverman 2000) are, on the other hand, mainly normative and span a broad range of different kinds of case studies. One exception is Pettigrew (1990, 1992), who places the case study in the context of a research tradition (the Warwick process research).

Given the contextual nature of the case study and its strength in addressing contemporary phenomena in real-life contexts, I believe that there is a need for articles that provide a comprehensive overview of the case study process from the researcher's perspective, emphasizing methodological considerations. This implies addressing the whole range of choices concerning specific design requirements, data collection procedures, data analysis, and validity and reliability.

WHY A CASE STUDY?

Case studies are tailor-made for exploring new processes or behaviors or ones that are little understood (Hartley 1994). Hence, the approach is particularly useful for responding to *how* and *why* questions about a contemporary set of events (Leonard-Barton 1990). Moreover, researchers have argued that certain kinds of information can be difficult or even impossible to tackle by means other than qualitative approaches such as the case study (Sykes 1990). Gummesson (1988:76) argues that an important advantage of case study research is the opportunity for a holistic view of the process: "The detailed observations entailed in the case study method enable us to study many different aspects, examine them in relation to each other, view the process within its total environment and also use the researchers' capacity for 'verstehen.' "

The contextual nature of the case study is illustrated in Yin's (1993:59) definition of a case study as an empirical inquiry that "investigates a contem-

porary phenomenon within its real-life context and addresses a situation in which the boundaries between phenomenon and context are not clearly evident."

The key difference between the case study and other qualitative designs such as grounded theory and ethnography (Glaser and Strauss 1967; Strauss and Corbin 1990; Gioia and Chittipeddi 1991) is that the case study is open to the use of theory or conceptual categories that guide the research and analysis of data. In contrast, grounded theory or ethnography presupposes that theoretical perspectives are grounded in and emerge from firsthand data.

Hartley (1994) argues that without a theoretical framework, the researcher is in severe danger of providing description without meaning. Gummesson (1988) says that a lack of preunderstanding will cause the researcher to spend considerable time gathering basic information. This preunderstanding may arise from general knowledge such as theories, models, and concepts or from specific knowledge of institutional conditions and social patterns. According to Gummesson, the key is not to require researchers to have split but dual personalities: "Those who are able to balance on a razor's edge using their pre-understanding without being its slave" (p. 58).

DESCRIPTION OF THE ILLUSTRATIVE STUDY

The study that will be used for illustrative purposes is a comparative and longitudinal case study of organizational integration in mergers and acquisitions taking place in Norway. The study had two purposes: (1) to identify contextual factors and features of integration that facilitated or impeded organizational integration, and (2) to study how the three dimensions of organizational integration (integration of tasks, unification of power, and integration of cultures and identities) interrelated and evolved over time. Examples of contextual factors were relative power, degree of friendliness, and economic climate. Integration features included factors such as participation, communication, and allocation of positions and functions.

Mergers and acquisitions are inherently complex. Researchers in the field have suggested that managers continuously underestimate the task of integrating the merging organizations in the postintegration process (Haspeslaph and Jemison 1991). The process of organizational integration can lead to sharp interorganizational conflict as the different top management styles, organizational and work unit cultures, systems, and other aspects of organizational life come into contact (Blake and Mounton 1985; Schweiger and Walsh 1990; Cartwright and Cooper 1993). Furthermore, cultural change in mergers and acquisitions is compounded by additional uncertainties, ambi-

guities, and stress inherent in the combination process (Buono and Bowditch 1989).

I focused on two combinations: one merger and one acquisition. The first case was a merger between two major Norwegian banks, Bergen Bank and DnC (to be named DnB), that started in the late 1980s. The second case was a study of a major acquisition in the insurance industry (i.e., Gjensidige's acquisition of Forenede), that started in the early 1990s. Both combinations aimed to realize operational synergies though merging the two organizations into one entity. This implied disruption of organizational boundaries and threat to the existing power distribution and organizational cultures.

The study of integration processes in mergers and acquisitions illustrates the need to find a design that opens for exploration of sensitive issues such as power struggles between the two merging organizations. Furthermore, the inherent complexity in the integration process, involving integration of tasks, unification of power, and cultural integration stressed the need for in-depth study of the phenomenon over time. To understand the cultural integration process, the design also had to be linked to the past history of the two organizations.

DESIGN DECISIONS

In the introduction, I stressed that a case is a rather loose design that requires that a number of design choices be made. In this section, I go through the most important choices I faced in the study of organizational integration in mergers and acquisitions. These include: (1) selection of cases; (2) sampling time; (3) choosing business areas, divisions, and sites; and (4) selection of and choices regarding data collection procedures, interviews, documents, and observation.

Selection of Cases

There are several choices involved in selecting cases. First, there is the question of how many cases to include. Second, one must sample cases and decide on a unit of analysis. I will explore these issues subsequently.

Single or Multiple Cases

Case studies can involve single or multiple cases. The problem of single cases is limitations in generalizability and several information-processing biases (Eisenhardt 1989).

> One way to respond to these biases is by applying a multi-case approach (Leonard-Barton 1990). Multiple cases augment external validity and help guard against observer biases. Moreover, multi-case sampling adds confidence to findings. By looking at a range of similar and contrasting cases, we can understand a single-case finding, grounding it by specifying how and where and, if possible, why it behaves as it does. (Miles and Huberman 1994)

Given these limitations of the single case study, it is desirable to include more than one case study in the study. However, the desire for depth and a pluralist perspective and tracking the cases over time implies that the number of cases must be fairly few. I chose two cases, which clearly does not support generalizability any more than does one case, but allows for comparison and contrast between the cases as well as a deeper and richer look at each case.

Originally, I planned to include a third case in the study. Due to changes in management during the initial integration process, my access to the case was limited and I left this case entirely. However, a positive side effect was that it allowed a deeper investigation of the two original cases and in hindsight turned out to be a good decision.

Sampling Cases

The logic of sampling cases is fundamentally different from statistical sampling. The logic in case studies involves theoretical sampling, in which the goal is to choose cases that are likely to replicate or extend the emergent theory or to fill theoretical categories and provide examples for polar types (Eisenhardt 1989). Hence, whereas quantitative sampling concerns itself with representativeness, qualitative sampling seeks information richness and selects the cases purposefully rather than randomly (Crabtree and Miller 1992).

The choice of cases was guided by George (1979) and Pettigrew's (1990) recommendations. The aim was to find cases that matched the three dimensions in the dependent variable and provided variation in the contextual factors, thus representing polar cases.

To match the choice of outcome variable, organizational integration, I chose cases in which the purpose was to fully consolidate the merging parties' operations. A full consolidation would imply considerable disruption in the organizational boundaries and would be expected to affect the task-related, political, and cultural features of the organizations. As for the contextual factors, the two cases varied in contextual factors such as relative power, friendliness, and economic climate. The DnB merger was a friendly combination between two equal partners in an unfriendly economic climate.

Gjensidige's acquisition of Forenede was, in contrast, an unfriendly and unbalanced acquisition in a friendly economic climate.

Unit of Analysis

Another way to respond to researchers' and respondents' biases is to have more than one unit of analysis in each case (Yin 1993). This implies that, in addition to developing contrasts between the cases, researchers can focus on contrasts within the cases (Hartley 1994). In case studies, there is a choice of a holistic or embedded design (Yin 1989). A holistic design examines the global nature of the phenomenon, whereas an embedded design also pays attention to subunit(s).

I used an embedded design to analyze the cases (i.e., within each case, I also gave attention to subunits and subprocesses). In both cases, I compared the combination processes in the various divisions and local networks. Moreover, I compared three distinct change processes in DnB: before the merger, during the initial combination, and two years after the merger. The overall and most important unit of analysis in the two cases was, however, the integration process.

Sampling Time

According to Pettigrew (1990), time sets a reference for what changes can be seen and how those changes are explained. When conducting a case study, there are several important issues to decide when sampling time. The first regards how many times data should be collected, while the second concerns when to enter the organizations. There is also a need to decide whether to collect data on a continuous basis or in distinct periods.

Number of data collections. I studied the process by collecting real time and retrospective data at two points in time, with one-and-a-half- and two-year intervals in the two cases. Collecting data twice had some interesting implications for the interpretations of the data. During the first data collection in the DnB study, for example, I collected retrospective data about the premerger and initial combination phase and real-time data about the second step in the combination process.

Although I gained a picture of how the employees experienced the second stage of the combination process, it was too early to assess the effects of this process at that stage. I entered the organization two years later and found interesting effects that I had not anticipated the first time. Moreover, it was interesting to observe how people's attitudes toward the merger processes changed over time to be more positive and less emotional.

When to enter the organizations. It would be desirable to have had the opportunity to collect data in the precombination processes. However, researchers are rarely given access in this period due to secrecy.

The emphasis in this study was to focus on the postcombination process. As such, the precombination events were classified as contextual factors. This implied that it was most important to collect real-time data after the parties had been given government approval to merge or acquire. What would have been desirable was to gain access earlier in the postcombination process. This was not possible because access had to be negotiated. Due to the change of CEO in the middle of the merger process and the need for renegotiating access, this took longer than expected.

Regarding the second case, I was restricted by the time frame of the study. In essence, I had to choose between entering the combination process as soon as governmental approval was given, or entering the organization at a later stage. In light of the previous studies in the field that have failed to go beyond the initial two years, and given the need to collect data about the cultural integration process, I chose the latter strategy. And I decided to enter the organizations at two distinct periods of time rather than on a continuous basis.

There were several reasons for this approach, some methodological and some practical. First, data collection on a continuous basis would have required use of extensive observation that I didn't have access to, and getting access to two data collections in DnB was difficult in itself. Second, I had a stay abroad between the first and second data collection in Gjensidige. Collecting data on a continuous basis would probably have allowed for better mapping of the ongoing integration process, but the contrasts between the two different stages in the integration process that I wanted to elaborate would probably be more difficult to detect. In Table 1 I have listed the periods of time in which I collected data in the two combinations.

Sampling Business Areas, Divisions, and Sites

Even when the cases for a study have been chosen, it is often necessary to make further choices within each case to make the cases researchable. The most important criteria that set the boundaries for the study are importance or criticality, relevance, and representativeness. At the time of the data collection, my criteria for making these decisions were not as conscious as they may appear here. Rather, being restricted by time and my own capacity as a researcher, I had to limit the sites and act instinctively.

In both cases, I decided to concentrate on the core businesses (criticality criterion) and left out the business units that were only mildly affected by the integration process (relevance criterion). In the choice of regional offices, I

TABLE 1
Periods of Time for Studying the Two Combinations

	Bergen Bank–DnC	*Gjensidige-Forenede*
Announcement of intention to merge/acquire	October 1989	December 1991
Government approval	February 1990	June 1992
First data collection	Autumn 1991/Winter 1992	Winter 1993/Spring 1994
Second data collection	Spring 1994	Autumn 1995

used the representativeness criterion as the number of offices widely exceeded the number of sites possible to study. In making these choices, I relied on key informants in the organizations.

SELECTION OF DATA COLLECTION PROCEDURES

The choice of data collection procedures should be guided by the research question and the choice of design. The case study approach typically combines data collection methods such as archives, interviews, questionnaires, and observations (Yin 1989). This triangulated methodology provides stronger substantiation of constructs and hypotheses. However, the choice of data collection methods is also subject to constraints in time, financial resources, and access.

I chose a combination of interviews, archives, and observation, with main emphasis on the first two. Conducting a survey was inappropriate due to the lack of established concepts and indicators. The reason for limited observation, on the other hand, was due to problems in obtaining access early in the study and time and resource constraints. In addition to choosing among several different data collection methods, there are a number of choices to be made for each individual method.

Interviews

When relying on interviews as the primary data collection method, the issue of building trust between the researcher and the interviewees becomes very important. I addressed this issue by several means. First, I established a procedure of how to approach the interviewees. In most cases, I called them first, then sent out a letter explaining the key features of the project and out-

lining the broad issues to be addressed in the interview. In this letter, the support from the institution's top management was also communicated. In most cases, the top management's support of the project was an important prerequisite for the respondent's input. Some interviewees did, however, fear that their input would be open to the top management without disguising the information source. Hence, it became important to communicate how I intended to use and store the information.

To establish trust, I also actively used my preunderstanding of the context in the first case and the phenomenon in the second case. As I built up an understanding of the cases, I used this information to gain confidence. The active use of my preunderstanding did, however, pose important challenges in not revealing too much of the research hypotheses and in balancing between asking open-ended questions and appearing knowledgeable.

There are two choices involved in conducting interviews. The first concerns the sampling of interviewees. The second is that you must decide on issues such as the structure of the interviews, use of tape recorder, and involvement of other researchers.

Sampling Interviewees

Following the desire for detailed knowledge of each case and for grasping different participant's views the aim was, in line with Pettigrew (1990), to apply a pluralist view by describing and analyzing competing versions of reality as seen by actors in the combination processes.

I used four criteria for sampling informants. First, I drew informants from populations representing multiple perspectives. The first data collection in DnB was primarily focused on the top management level. Moreover, most middle managers in the first data collection were employed at the head offices, either in Bergen or Oslo. In the second data collection, I compensated for this skew by including eight local middle managers in the sample. The difference between the number of employees interviewed in DnB and Gjensidige was primarily due to the fact that Gjensidige has three unions, whereas DnB only has one. The distribution of interviewees is outlined in Table 2.

The second criterion was to use multiple informants. According to Glick et al. (1990), an important advantage of using multiple informants is that the validity of information provided by one informant can be checked against that provided by other informants. Moreover, the validity of the data used by the researcher can be enhanced by resolving the discrepancies among different informants' reports. Hence, I selected multiple respondents from each perspective.

TABLE 2
Distribution of Interviewees

	DnB		*Gjensidige*		
	Phase 1	*Phase 2*	*Phase 1*	*Phase 2*	*Total*
Level of organization					
Top management/board	13	7	10	5	35
Middle management	4	10	8	2	24
Union representative and employees	3	3	11	2	19
Organizational affiliation					
Acquirer	10	7	15	4	36
Acquired company	9	11	12	5	37
Neither	1	2	2		5
Locales					
Oslo	13	12	11	3	39
Bergen	6	2			8
Trondheim			10	3	13
Locales outside headquarter cities	1	6	8	8	18
Total	20	20	29	9	78

Third, I focused on key informants who were expected to be knowledgeable about the combination process. These people included top management members, managers, and employees involved in the integration project. To validate the information from these informants, I also used a fourth criterion by selecting managers and employees who had been affected by the process but who were not involved in the project groups.

Structured versus unstructured. In line with the explorative nature of the study, the goal of the interviews was to see the research topic from the perspective of the interviewee, and to understand why he or she came to have this particular perspective. To meet this goal, King (1994:15) recommends that one have "a low degree of structure imposed on the interviewer, a preponderance of open questions, a focus on specific situations and action sequences in the world of the interviewee rather than abstractions and general opinions." In line with these recommendations, the collection of primary data in this study consists of unstructured interviews.

Using tape recorders and involving other researchers. The majority of the interviews were tape-recorded, and I could thus concentrate fully on asking questions and responding to the interviewees' answers. In the few inter-

views that were not tape-recorded, most of which were conducted in the first phase of the DnB-study, two researchers were present. This was useful as we were both able to discuss the interviews later and had feedback on the role of an interviewer.

In hindsight, however, I wish that these interviews had been tape-recorded to maintain the level of accuracy and richness of data. Hence, in the next phases of data collection, I tape-recorded all interviews, with two exceptions (people who strongly opposed the use of this device). All interviews that were tape-recorded were transcribed by me in full, which gave me closeness and a good grasp of the data.

Documents

When organizations merge or make acquisitions, there are often a vast number of documents to choose from to build up an understanding of what has happened and to use in the analyses. Furthermore, when firms make acquisitions or merge, they often hire external consultants, each of whom produces more documents. Due to time constraints, it is seldom possible to collect and analyze all these documents, and thus the researcher has to make a selection.

The choice of documentation was guided by my previous experience with merger and acquisition processes and the research question. Hence, obtaining information on the postintegration process was more important than gaining access to the due-diligence analysis. As I learned about the process, I obtained more documents on specific issues. I did not, however, gain access to all the documents I asked for, and, in some cases, documents had been lost or shredded.

The documents were helpful in a number of ways. First, and most important, they were used as inputs to the interview guide and saved me time, because I did not have to ask for facts in the interviews. They were also useful for tracing the history of the organizations and statements made by key people in the organizations. Third, the documents were helpful in counteracting the biases of the interviews. A list of the documents used in writing the cases is shown in Table 3.

Observation

The major strength of direct observation is that it is unobtrusive and does not require direct interaction with participants (Adler and Adler 1994). Observation produces rigor when it is combined with other methods. When the researcher has access to group processes, direct observation can illumi-

nate the discrepancies between what people said in the interviews and casual conversations and what they actually do (Pettigrew 1990).

As with interviews, there are a number of choices involved in conducting observations. Although I did some observations in the study, I used interviews as the key data collection source. Discussion in this article about observations will thus be somewhat limited. Nevertheless, I faced a number of choices in conducting observations, including type of observation, when to enter, how much observation to conduct, and which groups to observe.

The are four ways in which an observer may gather data: (1) the complete participant who operates covertly, concealing any intention to observe the setting; (2) the participant-as-observer, who forms relationships and participates in activities, but makes no secret of his or her intentions to observe events; (3) the observer-as-participant, who maintains only superficial contact with the people being studied; and (4) the complete observer, who merely stands back and eavesdrops on the proceedings (Waddington 1994).

In this study, I used the second and third ways of observing. The use of the participant-as-observer mode, on which much ethnographic research is based, was rather limited in the study. There were two reasons for this. First, I had limited time available for collecting data, and in my view interviews made more effective use of this limited time than extensive participant observation. Second, people were rather reluctant to let me observe these political and sensitive processes until they knew me better and felt I could be trusted. Indeed, I was dependent on starting the data collection before having built sufficient trust to observe key groups in the integration process. Nevertheless, Gjensidige allowed me to study two employee seminars to acquaint me with the organization. Here I admitted my role as an observer but participated fully in the activities. To achieve variation, I chose two seminars representing polar groups of employees.

As observer-as-participant, I attended a top management meeting at the end of the first data collection in Gjensidige and observed the respondents during interviews and in more informal meetings, such as lunches. All these observations gave me an opportunity to validate the data from the interviews. Observing the top management group was by far the most interesting and rewarding in terms of input.

Both DnB and Gjensidige started to open up for more extensive observation when I was about to finish the data collection. By then, I had built up the trust needed to undertake this approach. Unfortunately, this came a little late for me to take advantage of it.

TABLE 3
Documents Used in Analysis of the Combinations

Gjensidige	*DnB*
Strategic plan for 2000	McKinsey reports from the premerger phase
Reports from the integration project groups	Reports from the integration project groups
Report from the recruiting committee	Guidelines for selecting and positioning in 1990
Internal letters	Minutes from top management integration groups
Letters to the Norwegian authorities	Written submissions for the government approval application
Declaration of intent	Merger prospect
Internal job announcement magazines	Articles from the press
Report from employee survey	Annual reports
Internal newsletters	Publications and documents from the banking crisis projects
Articles from the press	
Annual reports	
Acquisition prospectus	
Union magazines	
Publication from the Association of Norwegian Insurance Companies	

DATA ANALYSIS

Published studies generally describe research sites and data-collection methods, but give little space to discuss the analysis (Eisenhardt 1989). Thus, one cannot follow how a researcher arrives at the final conclusions from a large volume of field notes (Miles and Huberman 1994).

In this study, I went through the stages by which the data were reduced and analyzed. This involved establishing the chronology, coding, writing up the data according to phases and themes, introducing organizational integration into the analysis, comparing the cases, and applying the theory. I will discuss these phases accordingly.

The first step in the analysis was to establish the chronology of the cases. To do this, I used internal and external documents. I wrote the chronologies up and included appendices in the final report.

The next step was to code the data into phases and themes reflecting the contextual factors and features of integration. For the interviews, this implied marking the text with a specific phase and a theme, and grouping the para-

graphs on the same theme and phase together. I followed the same procedure in organizing the documents.

I then wrote up the cases using phases and themes to structure them. Before starting to write up the cases, I scanned the information on each theme, built up the facts and filled in with perceptions and reactions that were illustrative and representative of the data.

The documents were primarily useful in establishing the facts, but they also provided me with some perceptions and reactions that were validated in the interviews. The documents used included internal letters and newsletters as well as articles from the press. The interviews were less factual, as intended, and gave me input to assess perceptions and reactions. The limited observation was useful to validate the data from the interviews. The result of this step was two descriptive cases.

To make each case more analytical, I introduced the three dimensions of organizational integration—integration of tasks, unification of power, and cultural integration—into the analysis. This helped to focus the case and to develop a framework that could be used to compare the cases. The cases were thus structured according to phases, organizational integration, and themes reflecting the factors and features in the study.

I took all these steps to become more familiar with each case as a individual entity. According to Eisenhardt (1989:540), this is a process that "allows the unique patterns of each case to emerge before the investigators push to generalise patterns across cases. In addition it gives investigators a rich familiarity with each case which, in turn, accelerates cross-case comparison."

The comparison between the cases constituted the next step in the analysis. Here, I used the categories from the case chapters, filled in the features and factors, and compared and contrasted the findings. The idea behind cross-case searching tactics is to force investigators to go beyond initial impressions, especially through the use of structural and diverse lenses on the data. These tactics improve the likelihood of accurate and reliable theory, that is, theory with a close fit to the data (Eisenhardt 1989).

As a result, I had a number of overall themes, concepts, and relationships that had emerged from the within-case analysis and cross-case comparisons. The next step was to compare these emergent findings with theory from the organizational field of mergers and acquisitions, as well as other relevant perspectives.

This method of generalization is known as analytical generalization. In this approach, a previously developed theory is used as a template with which to compare the empirical results of the case study (Yin 1989). This comparison of emergent concepts, theory, or hypotheses with the extant literature

involves asking what it is similar to, what it contradicts, and why. The key to this process is to consider a broad range of theory (Eisenhardt 1989). On the whole, linking emergent theory to existent literature enhances the internal validity, generalizability, and theoretical level of theory-building from case research.

According to Eisenhardt (1989), examining literature that conflicts with the emergent literature is important for two reasons. First, the chance of neglecting conflicting findings is reduced. Second, "conflicting results forces researchers into a more creative, frame-breaking mode of thinking than they might otherwise be able to achieve" (p. 544).

Similarly, Eisenhardt (1989) claims that literature discussing similar findings is important because it ties together underlying similarities in phenomena not normally associated with each other. The result is often a theory with a stronger internal validity, wider generalizability, and a higher conceptual level.

The analytical generalization in the study included exploring and developing the concepts and examining the relationships between the constructs. In carrying out this analytical generalization, I acted on Eisenhardt's (1989) recommendation to use a broad range of theory. First, I compared and contrasted the findings with the organizational stream on mergers and acquisition literature. Then I discussed other relevant literatures, including strategic change, power and politics, social justice, and social identity theory to explore how these perspectives could contribute to the understanding of the findings. Finally, I discussed the findings that could not be explained either by the merger and acquisition literature or the four theoretical perspectives.

In every scientific study, questions are raised about whether the study is valid and reliable. The issues of validity and reliability in case studies are just as important as for more deductive designs, but the application is fundamentally different.

VALIDITY AND RELIABILITY

The problems of validity in qualitative studies are related to the fact that most qualitative researchers work alone in the field, they focus on the findings rather than describe how the results were reached, and they are limited in processing information (Miles and Huberman 1994).

Researchers writing about qualitative methods have questioned whether the same criteria can be used for qualitative and quantitative studies (Kirk and Miller 1986; Sykes 1990; Maxwell 1992). The problem with the validity

criteria suggested in qualitative research is that there is little consistency across the articles as each author suggests a new set of criteria.

One approach in examining validity and reliability is to apply the criteria used in quantitative research. Hence, the criteria to be examined here are objectivity/intersubjectivity, construct validity, internal validity, external validity, and reliability.

Objectivity/Intersubjectivity

The basic issue of objectivity can be framed as one of relative neutrality and reasonable freedom from unacknowledged research biases (Miles and Huberman 1994). In a real-time longitudinal study, the researcher is in danger of losing objectivity and of becoming too involved with the organization, the people, and the process. Hence, Leonard-Barton (1990) claims that one may be perceived as, and may even become, an advocate rather than an observer.

According to King (1994), however, qualitative research, in seeking to describe and make sense of the world, does not require researchers to strive for objectivity and distance themselves from research participants. Indeed, to do so would make good qualitative research impossible, as the interviewer's sensitivity to subjective aspects of his or her relationship with the interviewee is an essential part of the research process (King 1994:31).

This does not imply, however, that the issue of possible research bias can be ignored. It is just as important as in a structured quantitative interview that the findings are not simply the product of the researcher's prejudices and prior experience. One way to guard against this bias is for the researcher to explicitly recognize his or her presuppositions and to make a conscious effort to set these aside in the analysis (Gummesson 1988). Furthermore, rival conclusions should be considered (Miles and Huberman 1994).

My experience from the first phase of the DnB study was that it was difficult to focus the questions and the analysis of the data when the research questions were too vague and broad. As such, developing a framework before collecting the data for the study was useful in guiding the collection and analysis of data. Nevertheless, it was important to be open-minded and receptive to new and surprising data. In the DnB study, for example, the positive effect of the reorganization process on the integration of cultures came as a complete surprise to me and thus needed further elaboration.

I also consciously searched for negative evidence and problems by interviewing outliers (Miles and Huberman 1994) and asking problem-oriented questions. In Gjensidige, the first interviews with the top management revealed a much more positive perception of the cultural integration process

than I had expected. To explore whether this was a result of overreliance on elite informants, I continued posing problem-oriented questions to outliers and people at lower levels in the organization. Moreover, I told them about the DnB study to be explicit about my presuppositions.

Another important issue when assessing objectivity is whether other researchers can trace the interpretations made in the case studies, or what is called intersubjectivity. To deal with this issue, Miles and Huberman (1994) suggest that: (1) the study's general methods and procedures should be described in detail, (2) one should be able to follow the process of analysis, (3) conclusions should be explicitly linked with exhibits of displayed data, and (4) the data from the study should be made available for reanalysis by others.

In response to these requirements, I described the study's data collection procedures and processing in detail. Then, the primary data were displayed in the written report in the form of quotations and extracts from documents to support and illustrate the interpretations of the data. Because the study was written up in English, I included the Norwegian text in a separate appendix. Finally, all the primary data from the study were accessible for a small group of distinguished researchers.

Construct Validity

Construct validity refers to whether there is substantial evidence that the theoretical paradigm correctly corresponds to observation (Kirk and Miller 1986). In this form of validity, the issue is the legitimacy of the application of a given concept or theory to established facts.

The strength of qualitative research lies in the flexible and responsive interaction between the interviewer and the respondents (Sykes 1990). Thus, meaning can be probed, topics covered easily from a number of angles, and questions made clear for respondents. This is an advantage for exploring the concepts (construct or theoretical validity) and the relationships between them (internal validity). Similarly, Hakim (1987) says the great strength of qualitative research is the validity of data obtained because individuals are interviewed in sufficient detail for the results to be taken as true, correct, and believable reports of their views and experiences.

Construct validity can be strengthened by applying a longitudinal multicase approach, triangulation, and use of feedback loops. The advantage of applying a longitudinal approach is that one gets the opportunity to test sensitivity of construct measures to the passage of time. Leonard-Barton (1990), for example, found that one of her main constructs, communicability, varied across time and relative to different groups of users. Thus, the longitu-

dinal study aided in defining the construct more precisely. By using more than one case study, one can validate stability of construct across situations (Leonard-Barton 1990). Since my study only consists of two case studies, the opportunity to test stability of constructs across cases is somewhat limited. However, the use of more than one unit of analysis helps to overcome this limitation.

Construct validity is strengthened by the use of multiple sources of evidence to build construct measures, which define the construct and distinguish it from other constructs. These multiple sources of evidence can include multiple viewpoints within and across the data sources. My study responds to these requirements in its sampling of interviewees and uses of multiple data sources.

Use of feedback loops implies returning to interviewees with interpretations and developing theory and actively seeking contradictions in data (Crabtree and Miller 1992; King 1994). In DnB, the written report had to be approved by the bank's top management after the first data collection. Apart from one minor correction, the bank had no objections to the established facts. In their comments on my analysis, some of the top managers expressed the view that the political process had been overemphasized, and that the CEO's role in initiating a strategic process was undervalued. Hence, an important objective in the second data collection was to explore these comments further. Moreover, the report was not as positive as the management had hoped for, and negotiations had to be conducted to publish the report. The result of these negotiations was that publication of the report was postponed one-and-a-half years.

The experiences from the first data collection in the DnB had some consequences. I was more cautious and brought up the problems of confidentiality and the need to publish at the outset of the Gjensidige study. Also, I had to struggle to get access to the DnB case for the second data collection and some of the information I asked for was not released. At Gjensidige, I sent a preliminary draft of the case chapter to the corporation's top management for comments, in addition to having second interviews with a small number of people. Beside testing out the factual description, these sessions gave me the opportunity to test out the theoretical categories established as a result of the within-case analysis.

Internal Validity

Internal validity concerns the validity of the postulated relationships among the concepts. The main problem of internal validity as a criterion in qualitative research is that it is often not open to scrutiny. According to Sykes

(1990), the researcher can always provide a plausible account and, with careful editing, may ensure its coherence. Recognition of this problem has led to calls for better documentation of the processes of data collection, the data itself, and the interpretative contribution of the researcher. The discussion of how I met these requirements was outlined in the section on objectivity/subjectivity above.

However, there are some advantages in using qualitative methods, too. First, the flexible and responsive methods of data collection allow cross-checking and amplification of information from individual units as it is generated. Respondents' opinions and understandings can be thoroughly explored. The internal validity results from strategies that eliminate ambiguity and contradiction, filling in detail and establishing strong connections in data.

Second, the longitudinal study enables one to track cause and effect. Moreover, it can make one aware of intervening variables (Leonard-Barton 1990). Eisenhardt (1989:542) states, "Just as hypothesis testing research an apparent relationship may simply be a spurious correlation or may reflect the impact of some third variable on each of the other two. Therefore, it is important to discover the underlying reasons for why the relationship exists."

Generalizability

According to Mitchell (1983), case studies are not based on statistical inference. Quite the contrary, the inferring process turns exclusively on the theoretically necessary links among the features in the case study. The validity of the extrapolation depends not on the typicality or representativeness of the case but on the cogency of the theoretical reasoning. Hartley (1994:225) claims, "The detailed knowledge of the organization and especially the knowledge about the *processes* underlying the behaviour and its *context* can help to specify the conditions under which behaviour can be expected to occur. In other words, the generalisation is about theoretical propositions not about populations."

Generalizability is normally based on the assumption that this theory may be useful in making sense of similar persons or situations (Maxwell 1992). One way to increase the generalizability is to apply a multicase approach (Leonard-Barton 1990). The advantage of this approach is that one can replicate the findings from one case study to another. This replication logic is similar to that used on multiple experiments (Yin 1993).

Given the choice of two case studies, the generalizability criterion is not supported in this study. Through the discussion of my choices, I have tried to show that I had to strike a balance between the need for depth and mapping

changes over time and the number of cases. In doing so, I deliberately chose to provide a deeper and richer look at each case, allowing the reader to make judgments about the applicability rather than making a case for generalizability.

Reliability

Reliability focuses on whether the process of the study is consistent and reasonably stable over time and across researchers and methods (Miles and Huberman 1994). In the context of qualitative research, reliability is concerned with two questions (Sykes 1990): Could the same study carried out by two researchers produce the same findings? and Could a study be repeated using the same researcher and respondents to yield the same findings?

The problem of reliability in qualitative research is that differences between replicated studies using different researchers are to be expected. However, while it may not be surprising that different researchers generate different findings and reach different conclusions, controlling for reliability may still be relevant. Kirk and Miller's (1986:311) definition takes into account the particular relationship between the researcher's orientation, the generation of data, and its interpretation:

> For reliability to be calculated, it is incumbent on the scientific investigator to document his or her procedure. This must be accomplished at such a level of abstraction that the loci of decisions internal to the project are made apparent. The curious public deserves to know how the qualitative researcher prepares him or herself for the endeavour, and how the data is collected and analysed.

The study addresses these requirements by discussing my point of departure regarding experience and framework, the sampling and data collection procedures, and data analysis.

DISCUSSION

Case studies often lack academic rigor and are, as such, regarded as inferior to more rigorous methods where there are more specific guidelines for collecting and analyzing data. These criticisms stress that there is a need to be very explicit about the choices one makes and the need to justify them.

One reason why case studies are criticized may be that researchers disagree about the definition and the purpose of carrying out case studies. Case

studies have been regarded as a design (Cook and Campbell 1979), as a qualitative methodology (Cassell and Symon 1994), as a particular data collection procedure (Andersen 1997), and as a research strategy (Yin 1989). Furthermore, the purpose for carrying out case studies is unclear. Some regard case studies as supplements to more rigorous qualitative studies to be carried out in the early stage of the research process; others claim that it can be used for multiple purposes and as a research strategy in its own right (Gummesson 1988; Yin 1989). Given this unclear status, researchers need to be very clear about their interpretation of the case study and the purpose of carrying out the study.

This article has taken Yin's (1989) definition of the case study as a research strategy as a starting point and argued that the choice of the case study should be guided by the research question(s). In the illustrative study, I used a case study strategy because of a need to explore sensitive, ill-defined concepts in depth, over time, taking into account the context and history of the mergers and the existing knowledge about the phenomenon. However, the choice of a case study strategy extended rather than limited the number of decisions to be made. In Schramm's (1971, cited in Yin 1989:22–23) words, "The essence of a case study, the central tendency among all types of case study, is that it tries to illuminate a decision or set of decisions, why they were taken, how they were implemented, and with what result."

Hence, the purpose of this article has been to illustrate the wide range of decisions that need to be made in the context of a particular case study and to discuss the methodological considerations linked to these decisions. I argue that there is a particular need in case studies to be explicit about the methodological choices one makes and that these choices can be best illustrated through a case study of the case study strategy.

As in all case studies, however, there are limitations to the generalizability of using one particular case study for illustrative purposes. As such, the strength of linking the methodological considerations to a specific context and phenomenon also becomes a weakness. However, I would argue that the questions raised in this article are applicable to many case studies, but that the answers are very likely to vary. The design choices are shown in Table 4.

Hence, researchers choosing a longitudinal, comparative case study need to address the same set of questions with regard to design, data collection procedures, and analysis, but they are likely to come up with other conclusions given their different research questions.

TABLE 4
Choices and Steps in Case Study Design

1. Selection of cases	Single or multiple sampling	
	Unit of analysis	
2. Sampling time	Number of data collections	
	When to enter	
3. Selection of data collection procedures	Interviews	Sampling interviewees
		Structured versus unstructured
		Use of tape recorder
	Documents	Sampling documents
		Use of documents
	Observation	Choosing method
		When to enter
		How much
		Which groups

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Another Complex Step: A Model of Heroin Experimentation

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In a recent article in this journal, the author explored complexity theory as a potential resource for anthropological research, using ongoing research on heroin trends as the example. In this article, the research on trends among a sample of youth in Baltimore County is again used. A preliminary computer simulation of the research results is reported. The simulation was written in the Starlogo *language, a language designed to introduce agent-based modeling. The results show that key elements of the explanation of experimentation can be modeled and that the outcomes show potential use in intervention as well as validation of the anthropological research. However, the exercise also reveals limits of the model and problems in translation into the programming language.*

A while ago, *Field Methods* published my article reviewing John Holland's work, my way of learning something about complexity theory (Agar 1999). Michael Patton (1999) wrote a commentary on that article in the same issue. As it turns out, he is more skeptical about the anthropological use of complexity than I am. He pointed out that the anthropological proof was in the computational pudding. Absent an actual application, what did we have aside from yet another list of formal metaphors to label age-old problems without solving them? It's a good question.

Since then, I've had the chance to actually program a complex model of heroin experimentation trends. My colleague Heather Schacht Reisinger and I are trying to figure out how to explain why such trends occur, drawing on contextual data that feature interactions among population history, drug policy, and systems of production and supply. So far, we've looked at the recent increase in use among white suburban youth (Agar and Reisinger 2000) and the historical case of African American use in the 1960s and 1970s (Agar and Reisinger forthcoming).

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To conduct a first experiment with complexity, I reduced the problem to heroin experimentation only. When we began our project in 1998, Reisinger and I were surprised at how quickly experimentation had spread during the early and mid-1990s. We interviewed youth and adult experts and consulted media and other studies of contemporary youth to model how and why it happened (Agar and Reisinger 2000). I wondered if I could translate at least some of what we had learned into a complexity program based on agent-based adaptation and see if it produced an epidemic of experimentation as well.

This article is a report and evaluation of that preliminary effort. The work presented is only a first step; it continues at the moment with the assistance of a trained complexity programmer, but more on that later. My intent here is to serve the same audience I did with the first *Field Methods* article—those who are interested in learning about complexity. There will be more in the future. Perhaps I'll write the first academic serial around the theme of old dogs trying to learn new tricks. At any rate, I hope the article is useful to readers curious about this interesting intellectual trend.

ARTIFICIAL SOCIETIES

John Holland's books *Hidden Order* (1995) and *Emergence* (1998) were reviewed in the previous article. They remain, in my view, excellent introductions to agent-based adaptive models. Since then, I have consulted several other sources. They are a varied and interesting lot. Complexity and chaos are clearly growth industries. Sometimes the reading gets downright disorienting. For instance, complexity is offered as a solution to quantitative and postmodern research. Byrne (1998) argues that complexity reframes the quantitative sociological paradigm; for example, he thinks of statistical clusters as system "attractors" that then need to be explained. Cilliers (1998), on the other hand, shows how neural nets clarify and make more precise the postmodern program, with shifting patterns of distributed knowledge and action that adapt to changing circumstance.

Most useful for the research problem at hand is Epstein and Axtell's (1996) work on artificial societies. Epstein is a policy analyst and an affiliate of the Santa Fe Institute, the Jerusalem of complexity. He developed a model called "sugarscape," which consists of a world of sugar resources and agents who want to eat it. Epstein and Axtell build their model in layers, chapter by chapter. Initially, there are just agents and sugar, agents who, by the way, rapidly generate inequitable distributions of "wealth." Then complications are

added, bit by bit—reproduction and death, ethnic groups, trade, new commodities, conflict, and so forth.

Here, I'm less interested in the details of sugarscape and more in Epstein and Axtell's (1996:6) comments on the general nature of artificial societies:

> Indeed, the defining feature of an artificial society model is precisely that *fundamental social structures and group behaviors emerge from the interaction of individual agents operating on artificial environments under rules that place only bounded demands on each agent's information and computational capacity*. The shorthand for this is that we "grow" the collective structures "from the bottom up."

In a nutshell, agents—who know some things and can do other things—interact with an environment—which also knows some things and can do other things. Agents and environments will both change as a result of these interactions. The macrostructures that these local interactions produce over time—maybe they're surprising, certainly they're variable from time to time—are the phenomena that we are trying to explain. The program, then, represents a candidate explanation for the phenomenon.

> Clearly, agent-based social science does not seem to be either deductive or inductive in the usual senses. But then what is it? We think *generative* is an appropriate term. The aim is to provide initial microspecifications (initial agents, environments, and rules) that are *sufficient to generate* the macrostructures of interest. We consider a given macrostructure to be "explained" by a given microspecification when the latter's generative sufficiency has been established. (Epstein and Axtell 1996:177)

It's worth noting that the purpose isn't to create a "simulation" of a world in the sense, say, of a flight simulator or computer game. Axelrod, a political scientist who has also made use of agent-based models, talks about a program as a way to conduct thought experiments, to tinker with intuitions. One shears away the detail of particular instances to "enrich our understanding of fundamental processes that may appear in a variety of applications" (Axelrod 1997:5).

Axelrod uses the prisoner's dilemma game as one example. He conducted a tournament among models for game strategy. Anatol Rapoport's simple tit-for-tat won, a strategy where the player first cooperates and then does whatever the other player did on its last move (Rapoport and Chammah 1965). Axelrod then used Holland's genetic algorithm (Holland 1995) to see if agents would figure this out for themselves. With a couple of important exceptions that are neglected here, they in fact did. The model helped under-

stand both foraging fish and divorcing, not because it represented either, but because it captured a feature of many situations, the conflict between "the advantages of selfishness in the short run versus the need to elicit cooperation from the other player to be successful in the longer run" (Axelrod 1997:6).

Epstein and Axtell and Axelrod talk about how agent-based modeling requires a transdisciplinary approach to social research, an issue that keeps reminding me of the writings of the late Eric Wolf, though he was more of a top-down theorist.

> When such multifaceted agents are released into an environment in which (and *with* which) they interact, the resulting society will—unavoidably—couple demography, economics, cultural change, conflict, and public health. All these spheres of *social* life will emerge—and merge—naturally and without top-down specification, from the purely local interactions of the individual agents. Because the individual is multifaceted, so is the society. (Epstein and Axtell 1996:158)

Epstein and Axtell present a "gedanken experiment" to show how artificial it is to isolate phenomena. Consider population—as one turns various rules on or off that have to do with movement, gender, culture, trade, disease, and so forth, the curve moves. Treating the curve in isolation would be ridiculous: "Is it sensible to study long-range population dynamics as though economic structure were irrelevant?" (Epstein and Axtell 1996:159).

So, one casts aside disciplinary boundaries and zeroes in on a macrostructure of interest—in my case, a wave of heroin experimentation. Why does it happen in different ways among different people at different times? Can I dig into our case studies and come up with a few simple characteristics of agents and their worlds and build an agent-based model? Will that model run on repeated occasions and produce an epidemic S-curve? What else might it produce?

The problem was, to answer these questions, I had to write a program. As a psychic vacation from the 1960s, I had learned Algol and Fortran and actually wrote some programs to analyze genealogical data for Alan Beals, with whom I'd worked in India. But times have changed. Agent-based modelers belong to something called the SWARM community (www.swarm.org). SWARM is a library of "objects" built to be called by object-oriented programming languages like C and Java. Indeed, I learned that an archaeologist was using SWARM to model changing Anasazi populations (Kohler 1995).

While the basic logic of object-oriented programming is simple to grasp if you are already thinking in terms of agent-based models, the thought of learn-

ing pages of new syntax was enough to make me consider active participation in the drug worlds I usually study. Fortunately, I attended the meetings of the New England Complexity Sciences Institute and learned about Starlogo, which is sort of object-oriented programming with training wheels.

The language has an interesting story behind it. Originally it was just "Logo," a turtle that painted lines to help teach kids geometry. Then along came Mitchel Resnick (1997). Resnick spent several years as a science journalist before returning to MIT for his Ph.D. He began to build Logo into Starlogo, making it a more sophisticated language, with the goal of teaching youth about decentralized thinking.

> When people see patterns in the world (like a flock of birds), they often assume that there is some type of centralized control (a leader of the flock). According to this way of thinking, a pattern can exist only if someone (or something) creates and orchestrates the pattern. Everything must have a single cause, an ultimate controlling factor. (Resnick 1997:4)

Resnick notes that centralized models are, of course, sometimes the right answer, at least in part. But they're seldom the whole story. They certainly aren't for evolution, epidemics, and economies, or for any anthropologist who uses the term *resistance* in his or her analysis.

The connected learning project at Tufts University, under National Science Foundation (NSF) sponsorship, has developed the most sophisticated version of the Starlogo language to date, *Starlogo 2000*. (The project, under its director Uri Wilensky, recently moved to Northwestern.) It can be downloaded at www.ccl.sesp.northwestern.edu. At the moment, it only runs on the MAC operating system. A Windows version is available from other sources, but—at the time of this project—it wasn't as well developed. By the time this article appears, other versions may be available.

Because of its history, Starlogo calls its agents *turtles*, although here I will continue to use the term *agents*. Like many complexity applications, the "world" is made up of a simple two-dimensional lattice. In Starlogo, one refers to a particular square in this grid as a *patch*, and I'll do the same. Starlogo is by no means a simple language, but many of the commands are intuitive, and the download comes with several samples and good documentation. Even in the simple application I am about to describe, though, I've run up against walls that will be described later. However, Starlogo is a great way to begin your education in agent-based models and how they work.

THE HEROIN MODEL

Recall that the problem is considerably simplified. All the model handles is the first use of heroin. The larger project that Reisinger and I are engaged in involves distribution systems, population histories, policy environments, and much more. And recall that I draw on what the youth themselves taught us when we did the original case study (Agar and Reisinger 2000). What I'll do here, by and large, is adapt what they taught us about experimentation to a Starlogo program and see what that program produces.

The program itself is simple. Starlogo sets up a default world that measures fifty by fifty patches, so I worked with that. The program selects one patch at random and heroin is put there.

Next, one hundred agents are created and told to move themselves to a random patch to spread themselves out.

Now, each of these one hundred agents has three variables. It has a risk variable, taken from diffusion of innovation theory (Rogers 1995), indicating how likely the agent is to try something new. It has an attitude variable, which represents its acceptance or rejection of illicit drug use. And it has an outlaw variable, representing its orientation to the rules and regulations of the institutions that make up its world. All three variables are assigned with Starlogo's random-normal function. The attitude and outlaw values can be skewed as well. The three values are assigned independently of each other. (Bear with me. I'll discuss and critique these assumptions shortly.)

Each agent, then, has normal-randomly generated values for risk, attitude, and outlaw. Each agent also has a variable called experiment, which is just the sum of risk, attitude, and outlaw. The general idea is, the higher the value of experiment, the more likely the agent is to try heroin should it run across it.

A second variable is also set before the program runs. It is called threshold. Threshold is a system variable rather than a characteristic of agents. Threshold is the value that an agent must match with its experiment value if it is to try heroin. Threshold was added because the youth distinguished between what we called edge users (kids who would try most anything that comes along) and ordinary users (kids who might use illicit alcohol, marijuana, and perhaps some other things experimentally, but who were otherwise more cautious). Other kids are abstainers, who don't use anything. Threshold is just a way to express how on the edge the world of the agents is. The assumption is that some groups of youth are more likely to experiment with illicit drugs than others. The higher the threshold, the less edgy a particular group.

Here's how the program works. The agents move around randomly, one patch at a time. If an agent lands on the heroin patch, it compares its experi-

ment value with the threshold value. If its experiment value is higher than the threshold, then the agent experiments. If the agent's experiment value is lower, then the agent continues on its way.

Once an agent has experimented, it can offer heroin to another agent. So the program also works like this: If an agent lands on the same patch as another agent who has already experimented, then the agent checks its own experiment value. If it's higher than the threshold, it then takes some heroin from the other agent who has already experimented.

If one agent takes some heroin from another agent, then the patch where the exchange occurred turns into a heroin hot spot. That patch now has heroin as well, so any agent who lands on it will check its experiment value and, if it exceeds the threshold, it will experiment. This feature models the fact that increasing use generates increasing supply.

To sum up so far, an agent can get heroin either from the original patch, from another agent who has already experimented, or from another patch that turned into a hot spot when one agent took some heroin from another. The agent decides whether to try heroin depending on its experiment value. If the agent's experiment score is higher than the threshold, the agent tries the heroin. If it is lower, the agent does not.

Now, a final critical piece of the program: The youth we interviewed taught us that their most important sources of information about illicit drugs were the narratives that traveled through personal networks. In the case of heroin, initial stories from the edge users are positive. With time, negative stories appear—about getting sick, about overdoses, and about addiction.

The program contains a simple function that changes the threshold value as the program runs. When the first few agents experiment, the threshold value drops. The stories are good, and this increases the chance that agents will experiment. As more and more agents try heroin, the bad stories start circulating, so the threshold begins to increase. Agents are now less likely to experiment.

That's the program to date. With each tick of the program clock, agents move and check for heroin patches or for other agents who have experimented. They experiment or not, and then move on with the next tick. The threshold drops as experimentation begins and then rises as it continues. After about one hundred or more ticks, agents who are going to experiment already have and nothing new happens.

Starlogo makes it easy to add observer windows to a program to track and graph key indicators. I put in a counter and a simple epidemiological chart that tracks total number of experimenters over time.

If one sets the threshold high and skews attitude toward "anti-illicit drug" and outlaw toward "follow all the rules," then when one runs the program

nothing happens. The graph is flat. No agents, or hardly any, experiment. I call this the "goody two-shoes" world. On the other hand, if one sets the threshold low and skews attitude toward "pro-drug" and outlaw toward "who cares about the rules," the graph takes off quickly and climbs steeply. I call this the "no good kids" world.

In between, at more realistic settings of the parameters given the contemporary youth we worked with, the graph produces a variety of S-curves of experimentation. I was sort of flabbergasted, to tell you the truth, when I first saw it work. The visual image of the agents moving around on the screen, turning from green to red as they experiment, the black background of the patch world adding more squares of blue as heroin becomes available at more and more locations—it started me thinking that the program might be useful for prevention programs as well. Why couldn't youth play with the program themselves, change the different values and see what a difference it made?

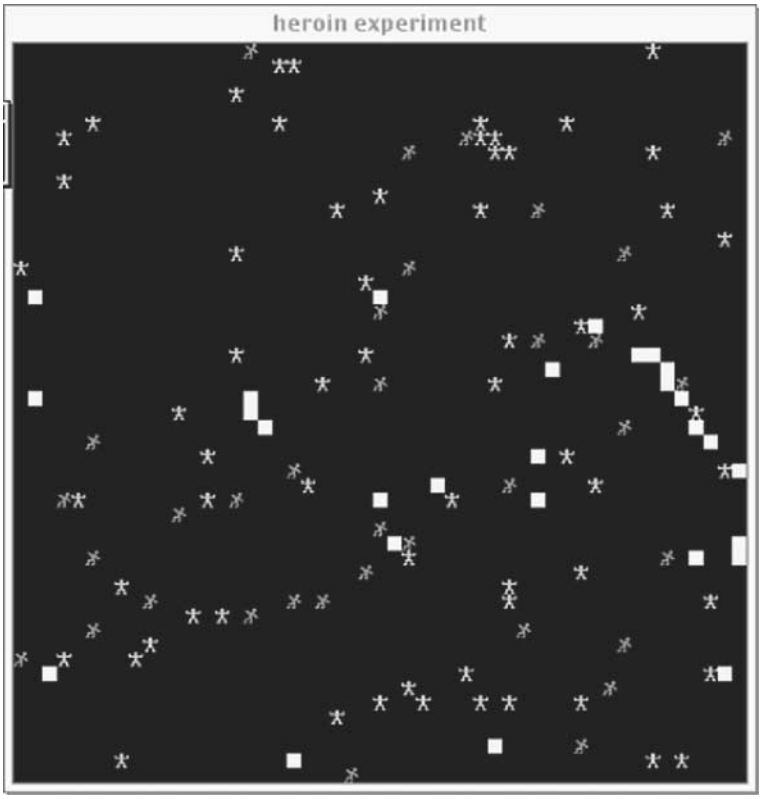
A sample screen at the end of a run is shown in Figure 1, and two different curves showing number of experimenters over time are illustrated in Figures 2 and 3. For all three figures the values were set as follows: The initial threshold value, which varies between 0 and 1, was set to .54. The setting is based on Parker, Aldridge, and Measham's (1998) longitudinal study of a general youth sample in the United Kingdom, where roughly one-third were regular users, one-third were experimenting on the way to regular use or on the way out, and one-third were abstinent. Threshold is guesstimated at the halfway point, the .54 value being an artifact of how the slider variable in Starlogo works.

Attitude varies from –1 to +1, with the minus value skewing attitude toward pro-illicit drug. For the runs illustrated in the figures, the value was set at –.33 because of Parker, Aldridge, and Measham's (1998) finding that one-third of the youth were regular users. The outlaw variable also runs from –1 to +1, with the minus value skewing toward a disregard for the rules. The value for these runs was set to –.54, again with Parker, Aldridge, and Measham's results and our own interviews and intuitions that many youth weren't too concerned with mainstream adult guidelines, especially in the area of illicit drugs.

In the next section, I'll discuss how the meaning of these numbers, especially attitude and outlaw, represent problematic issues, to put it mildly. But they have some intuitive plausibility, given Parker, Aldridge, and Measham's (1998) and our own work. So do the results shown in Figures 2 and 3.

I ran the program eight times and took the two curves most different from each other. Figure 1 was the experimentation incidence curve that took off most rapidly, in terms of onset and acceleration of the curve. (Total experi-

FIGURE 1
Sample Results of Experimentation Run

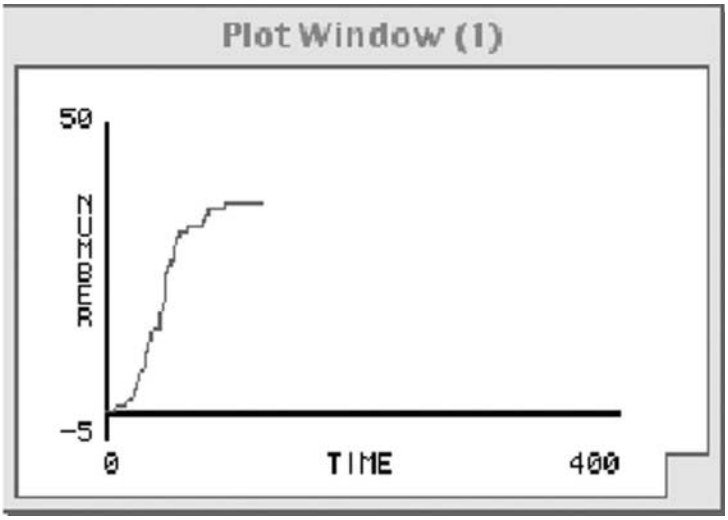


NOTE: Diagonal figures are experimenters while patches are heroin.

menters at the end of the run were thirty-seven out of one hundred.) Figure 3 was the slowest curve in terms of onset and also acceleration when compared to Figure 2. (Total experimenters at the end of this run were thirty-three.)

The *x*-axis in Figures 2 and 3 represent time, in the sense of number of program iterations. How this "program tick time" relates to the passage of clock and calendar time in the world of youth is another problem in the link between model and reality. Nonetheless, the different curves do show how the same starting conditions can result in different curves, and the differences

FIGURE 2
Rapid Onset, Rapid Acceleration ($N = 36$)



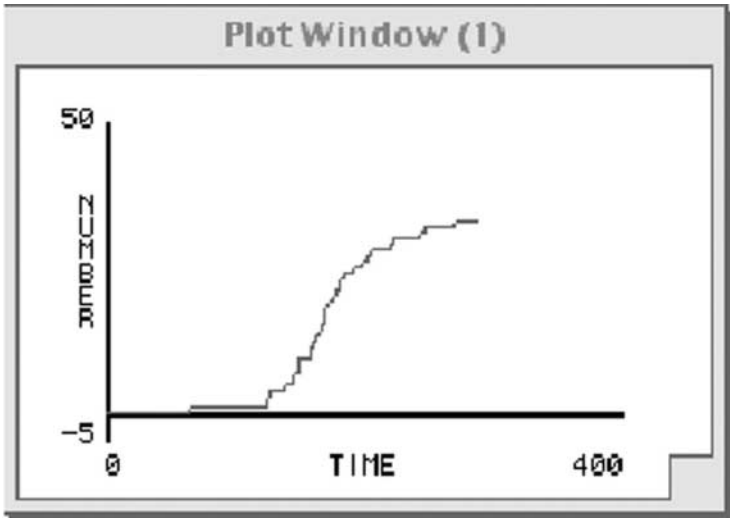
in overall shape of these example curves do appear plausible, given what we know from our own and others' research.

COMPLEX VARIATIONS

Recall that the model only deals with experimentation. In the drug-savvy world of suburban white youth, an epidemic of trying heroin should not be a surprise. But addiction, or physical dependence on heroin, is another matter. To date, addiction has only taken off in the area most severely affected by industrial decline and community deterioration. Future models will have to explain many variations on the theme—experimentation and nothing else, recreational use, addiction, and use of any kind followed by quitting. This model does not.

Even with the parameters fixed, not all incidence curves look the same, as exemplified in Figures 2 and 3. Much of the model is stochastic or contingent rather than deterministic. Probabilities play a role in several areas.

FIGURE 3
Slow Onset, Less Acceleration



- Risk, attitude, and outlaw variables are assigned with a random-normal function call, and they are assigned independently. The justification for independent assignment is intuitive and arguable. But I've known people who are high-risk but antidrug and conservative about law-breaking, or conservative outlaws, or low-risk people who use illicit chemicals, and so forth through the other possible combinations.
- Because of this, different runs of the program will differ in the distribution of experiment values, since experiment is the sum of risk, attitude, and outlaw parameters.
- The functions that skew attitude and outlaw variables are also stochastic. One sets a proportion, then the function picks that proportion of agents at random and changes their parameter values by that same proportion.
- Experimentation depends on the random movement of agents in the world and the random placement of the first heroin patch, and the changes in the threshold value will have more or less of an effect depending on the configuration of agents and patches that result from those moves.

In other words, even with parameters set at particular values, the model will generate different curves, depending on differences in contingencies and differences in the feedback processes that increase and decrease use.

This is a signature of complexity, that outcomes change with different runs of the program. Even with the model parameters fixed to correspond roughly to our case study, runs differ on how long before experimentation begins, how steeply the curve rises, and how many agents try heroin in the end. At this point, another level of analysis is called for, one that records several hundred runs of the model and then figures out the distribution of the distributions. Such an analysis would give us a picture of the attractor space of the system, the space that shows the different places the system might go, and just as important, the places it never goes.

It is clear that this program, given the parameters of our case study, usually produces an S-curve of experimentation of one sort or another. Even in the simple case of experimentation, though, it would be useful to have a picture of the attractor space from an intervention point of view. For example, suppose the attractor space shows a good chance of a slow increase after first experimentation occurs. Such a result suggests there is time for quick intervention. On the other hand, suppose the attractor shows that once experimentation starts, it usually goes to maximum in a brief time. In this case, prevention efforts would be better served by accepting that an epidemic of experimentation will have occurred once use comes to public attention. Energy in this case could be dedicated to preventative efforts to block physical dependence. Unfortunately, the Starlogo language can't handle this kind of attractor analysis, so work must await more sophisticated programming with the aid of a computer consultant.

SOME ANTHROPOLOGICAL ISSUES

Several issues come up in this exercise, ranging from the methodological to the epistemological. On a methodological level, one begins to worry about numbers in ways not captured in the usual quantitative/qualitative debates. Complex models require numbers. But it is not so much a question of how to measure phenomena; instead, it is a question of how to express qualities learned through anthropological research, using functions instead of words as the language for that expression. Epstein and Axtell (1996) write that they want to use their sugarscape model to tell the story of a civilization. What is different here, though, is this: Epstein and Axtell build a model instead of doing anthropological research; I'm trying to build a model after anthropological research to try out explanations based on it.

Some of the numbers I used seem more plausible than others. The risk value, for instance, is based on years of research in diffusion of innovation

theory (Rogers 1995) that shows how innovators do in fact distribute normally in the many populations studied. To take another example, the threshold parameter can be set to reflect differences in how many edge users might be in a particular population. So far, so good.

But what about the outlaw and attitude parameters? On what basis are they distributed with a random-normal function? And why do they have the same mean and standard deviation as the risk parameter? Are they all equally important? Wouldn't drug attitude and outlaw be more uniformly distributed in a particular world of potential experimenters?

I have no answer to these questions. On one hand, the issue signals limitations around the issue of whether numerical expressions exist that can translate something we've learned about the world. On the other hand, the issue also suggests looking at the extensive mathematical language of functions and their distributions, a language in which my training is dismal. Like many in social research, most of what I learned lies in the realm of statistics, a corner of mathematics that focuses on the linear causal models that complexity challenges with its initial assumptions. I have learned enough to know that if one can imagine something changing in almost any way, across a population or over time, one can probably write a function to express it.

Another issue connects anthropology and complexity. Complexity is a descendant of cybernetics, the invention of which involved Margaret Mead and Gregory Bateson at the Josiah Macy Foundation conference in 1948. In fact, Bateson's ethnography *Naven* (1936), with its concepts of complementary and symmetrical schismogenesis, pioneered the use of systems models in ethnography. Over the years, other classics, such as Rapoport's *Pigs for the Ancestors* (1967), have also drawn on the approach. As Ashby (1963:1) writes:

> Cybernetics was defined by Wiener as "the science of control and communication, in the animal and the machine"—in a word, as the art of *steermanship*, and it is to this aspect that the book will be addressed. Co-ordination, regulation and control will be its themes, for these are of the greatest biological and practical interest.

How is complexity different? Complexity treats the cybernetic situation, the dynamic equilibrium, as a special case in the history of a system. Complex systems coevolve with their environments. The emphasis is on change. Agents and environments conduct experiments and react to unexpected contingencies. The complex system changes as a result of these experiments and contingencies, but not as a result of a top-down control function. It self-organizes.

In this sense, the heroin experimentation program lies somewhere between the old cybernetics and the newer complexity. In a true agent-based model, the stories about heroin, and the values of attitude and outlaw, should change with agent experiences and contact with those who have had the experience. In this program, the stories change via a threshold function that simply reflects the passage of time, and the attitude and outlaw values stay the same. The program could then also be sensitive to different drugs in ways that reflect their characteristics. Heroin and cocaine are pleasurable but also dangerous in terms of possible dependence. PCP has never really taken off, probably because its negative effects are rapidly broadcast through stories based on experience. By comparison, marijuana is viewed as more benign, reflected in its more acceptable status and higher prevalence among youth (Parker, Aldridge, and Measham 1998; Agar and Reisinger 2000).

The agents don't change much in the heroin experimentation model, and they should in an agent-based approach. Variables like "attitude" and "outlaw" should change with experience. In fact, a long-range model should explain the increase and decrease in acceptability of illicit drug use in general over time. It should show how differences come about in different populations. Consider Canadian colleagues' speculation that there is something about the history and society of that country that might explain the different experiences of Canada with the crack epidemic in the 1980s when compared with the United States (Erickson et al. 1994). Ideally, agents should have a genetic algorithm, as first described by Holland (1995), where random strategies are generated and then reproduce according to fitness in some environment. Epstein and Axtell's (1996) sugarscape also lacks this feature, as they point out, although Axelrod's (1997) later work on the prisoner's dilemma game does draw on the approach.

At the same time, the heroin model is on the way to complexity. It cheats by asking for critical variables from the programmer rather than generating them itself. History is inputted rather than self-organized. But then it does act in complex ways. First, it is nonlinear, with numerous agents interacting in multiple ways and initial conditions that vary contingently. Second, it is dynamic, with a process that unfolds over time. Third, it is emergent, in the sense that the same initial conditions self-organize into different outcomes on repeated runs. It fits the overall complexity bill as the study of nonlinear dynamic systems, but it fails the agent-based adaptive model bill because the agents don't change, except to experiment (or not, as the case may be).

Another major issue has to do with the enduring question of the individual actor versus the characteristics of the world in which he or she acts. The issue has taken many forms, including the old fields of culture and personality and

social psychology. More modern versions include Giddens's (1979) structure/agency distinction, Sahlins's (1981) notion of culture as contingency, and Wolf's (1999) recent effort to integrate actor's meaning with broader issues of political economy.

The model does raise interesting structure/agency issues. First of all, I wrote the program, so all kinds of institutional presuppositions are introduced at the beginning. Heroin just appears, when, in fact, the shifts in systems of production and distribution play a major role independent of any agent, as we show in our case studies. Risk may be something that is distributed normally wherever you look; at least Rogers's (1995) work suggests this. However, attitudes toward illicit drugs and the rules and regulations of society clearly are a function of historical context, that context created by institutional processes. A lot of top-down assumptions are present in this decentralized model.

At the same time, the model does introduce decentralized thinking into the study of illicit drug use trends. In part, at least, drug trends occur because many agents using local rules—"local knowledge," in Geertz's famous phrase—take individual actions that produce the macrostructure at the societal level that we call an epidemic. The epidemic results from agents responding to an innovation based on their biographies, passing on the results of their experience to other agents, and doing so in a world where chance and contingency play a significant role.

At the end of this exercise, I remain interested in complexity models for explanation of illicit drug use trends in particular, or anthropological research in general. I use the term *anthropological* rather than *ethnographic* because it is not clear what ethnographic research in this day and age entails (Gupta and Ferguson 1997). Certainly our case studies were not ethnographic in any traditional sense of that term.

Complexity retains its appeal as a general theory that explains emergent macrostructures as a function of local interactions among agents over time. Furthermore, the model is neither linear nor deterministic. The notion of contingent translates into stochastic, although the challenge here is to acquire adequate literacy in the language of mathematics to accomplish the translation, or conclude on a knowledgeable basis that one is simply not possible at all. Issues of structure and agency are problems for complexity, just as they are for anthropological analysis.

When we tell each other about our work, we tell stories that reduce a complicated historical moment to characteristics of a world and characteristics of people in that world, then narrate how those characteristics interact to produce different outcomes. Complexity is another language in which to do the

same thing, with the added advantage that our explanation can be run several times to explore its variations and then it can be tinkered with to see what it might produce if things were different.

The effort does generalize those historical moments and link our explanations with others in many different fields. Complexity models that produce S-curves are legion, and diffusion of innovation theory also shows that such curves are the usual result of diffusion processes that take off. This sort of general claim is out of sync with current anthropological theorizing. But I've always thought of anthropology as making sense out of differences by linking them to similarities, a result of my early training that asserted cultural relativity and psychic unity in the same breath. Most social research begins and ends with assumed similarities and neglects the differences, a tendency that anthropology has always struggled and should continue to struggle against. However, once the differences are straightened out, links to similarities are in order, and it is here that complexity plays an interesting role.

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Research Uses of the U.S. Freedom of Information Act

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This article reviews the workings of the U.S. Freedom of Information Act, looks at its use by social scientists, and examines some of the implications of the U.S. experience for researchers interested in using freedom of information legislation in their own countries.

Gary Marx (1984) has argued that social scientists should make more use of what he refers to as "institutionalized discovery practices," the information generated by investigative, legislative, and judicial bodies. Among relevant sources is the information provided by freedom of information legislation. In Sweden, the basic principle of access to official information was first expressed in 1766. In its modern form, freedom of information legislation in Sweden dates from 1949. Similar legislation was passed in Finland in 1951. The United States has had a Freedom of Information Act (FOIA) since 1966. Subsequently, laws guaranteeing access to government information have been passed in more than forty countries. Denmark, Norway, Holland, and France have legislation dating from the 1970s. Australia, New Zealand, and Canada passed FOIAs in the 1980s. In the 1990s, Hungary, Ireland, Thailand, Korea, Israel, and Japan all enacted legislation. In the United Kingdom, a (widely criticized) FOIA received the Royal Assent on 1 December 2000.

The U.S. act is interesting, not just because of the length of time it has been in operation, but also because of its documented use by social scientists as a source of research data.[1] This article reviews the workings of the U.S. FOIA, looks at its use by social scientists, and examines some of the implications of the U.S. experience for researchers interested in using freedom of information legislation in their own countries. The article is a general one; it reviews the existing literature on the FOIA. It does not, however, seek to be an extensive guide to using the act. Nor does it explore in detail the workings of particular agencies.

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THE U.S. FOIA

The basic principle behind the U.S. FOIA is rather simple. Unless specifically exempt, records of U.S. federal government administrative agencies must be accessible to the public. Agencies include executive offices, departments, bureaus, councils, commissions, government corporations, government controlled corporations, and regulatory agencies (Hulihan 1983). The act encompasses the armed forces but not the Congress, the courts, or the president's immediate staff.

First passed in 1966, the act was amended in 1974 in the wake of the Watergate scandal. These amendments were designed to counter strategies that agencies had been using to discourage requests for information, including interpreting exemptions in a broad manner, charging excessive fees, using delaying tactics, and claiming not to be able to find records (Adler 1991:10).

During the Reagan presidency, the act was further amended to make access to law enforcement records more difficult. Provision was also introduced, however, for standardizing search fees and for waiving fees for certain categories of requesters, including scholars.

In 1993, President Clinton issued a policy memorandum urging federal agencies to comply with the spirit as well as the letter of the act. Simultaneously, the Department of Justice put in place a variety of policies to make easier disclosure of government information. An executive order signed by President Clinton in 1995 authorized the disclosure of records more than twenty-five years old. This policy has apparently encouraged a shift toward making heavily requested records available electronically. Critics note, however, that declassification of Central Intelligence Agency (CIA) records has proceeded at a painfully slow pace (Dempsey 1998), and that in the case of national security–related records, the public interest case in favor of disclosure has actually been weakened (Wiener 1998).

The act specifies that any person can make a request. This includes U.S. citizens, resident aliens, and foreign nationals, as well as corporations, voluntary associations, and organizations. As should become clear, although the overall process can sometimes be problematic, it is not difficult to make a request for information under the act. One simply sends a letter to the section of the relevant agency dealing with FOIA requests setting out what records are requested. There are a number of useful and easily accessible guides to the process that include advice on the drafting of the initial request (e.g., see Adler 1987).

Agencies can refuse to disclose records that fall under the scope of any one of nine specific exemptions permitted under the act. Exemptions include

information relating to national security and law enforcement, matters under litigation, and personnel or medical files, the disclosure of which might involve unwarranted invasion of privacy. While individuals can request files relating to themselves or those who are dead, they cannot do so for other living individuals without those individuals' permission.

In denying a request for disclosure, an agency must explain the reasons for claiming an exemption. Exemptions must be narrowly applied and are discretionary rather than mandatory. In other words, agencies can choose to waive exemptions unless prohibited from doing so by another statute. Even where part of a record is covered by an exemption, agencies are required to release nonexempt portions of the record. Requesters can appeal against the decision of an agency to deny disclosure of documents or the decision to withhold a fee waiver. When a request made under the FOIA is denied, the agency to which the request has been made must inform the requester of the reasons for the denial and of their right to appeal the decision. The requester can then file an administrative appeal to the head of the agency. As long as the appeal process continues, records subject to an appeal cannot be destroyed. In cases where an administrative appeal has been denied, the requester has the right of appeal to a court.

Agencies are required to file annual reports that show, among other things, the number of FOIA and Privacy Act requests they receive and the amount of time taken to process them. The Web site of the Office of Information and Privacy at the Department of Justice maintains links to most of these reports.[2] Inspection of these reports suggests that a few agencies, notably the Immigration and Naturalization Service and the Social Security Administration, receive very large numbers of requests.[3] Less than a dozen or so agencies, including the Federal Bureau of Investigation (FBI) and the large regulatory agencies, receive requests in the five-figure range. A long tail of agencies follows, at the bottom end, with some small agencies receiving few or no requests.

In some ways, a companion act to the FOIA, the Privacy Act of 1974, allows U.S. citizens and permanent resident aliens (but not noncitizens) certain rights with respect of information held about them by executive agencies of the federal government. These include: (1) the right to know how personal records are collected, maintained, used, and disseminated by the federal government; (2) the right to gain access to most kinds of personal information held about them; and (3) the right to amend information if it is found to be incorrect or incomplete. For the Privacy Act to apply, records have to be maintained in a system of records: In other words, records have to be grouped in a way that allows information from them to be retrieved by an identifier such as name or social security number. Information not held in this way is

not subject to the Privacy Act but may be subject to the FOIA. In practice, the Privacy Act and the FOIA are often used together in making requests for information from federal agencies.

PATTERNS OF USE

Very little is known about who uses the FOIA or how it is used. Relyea (1987:65) contends that academics "with the exception of historians, apparently make very little use of the FOI Act." According to Kessler (1993:375), in an unspecified year (probably in the early 1990s), 5.5% of FOIA requests received by the FBI were from scholars, with a further 4% coming from news organizations. Figures for the number of requests by category of requester obtained from the FBI under the FOIA show the following distribution for 1995: private individuals, 73.3%; prisoners, 14.6%; scholars and historians, 4.6%; news media, 3.3%; freelance writers and authors, 2.6%; organizations, 0.8%; and current employees, .08%. Since 1995, the volume of requests from scholars and historians has declined to around the 1% level, while requests from members of the news media show a decline followed by a subsequent rise (see Table 1).[4]

Among other agencies, the Environmental Protection Agency (EPA) is unusual in providing information about users' fee status in its annual reports. There are four categories of user for the purposes of fee assessment: representatives of the news media, requesters based in educational or scientific institutions, commercial requesters engaged in profit-making activities, and an "other" category that includes individuals, public interest groups, and nonprofit organizations. In 1999, the vast majority of EPA requests (87.9%) were from commercial requesters, 1.2% were from news media requesters, 0.8% from educational and scientific institutions, while the other category accounted for 10.1% of requesters. (The corresponding figures for 1998 are very similar: 88.6%, 1.0%, 0.7%, and 9.7%.) This level of request is likely to be similar for many other agencies (Michael Ravnitzky, e-mail communication, 16 July 2000).

Relyea is probably right, then, to argue that academics make up a relatively small proportion of FOIA users. Moreover, within social research the act seems rarely used as a data source relative to other sources. It seems, however, that academics use the FOIA at a level slightly less than that of journalists. Indeed, if one assumes that the number of journalists in the United States is larger than the number of social scientists and the volume of newspaper coverage is greater than the volume of academic output, research use is not entirely minimal.

TABLE 1
Freedom of Information Act (FOIA) Requests to the Federal Bureau of Investigation (FBI) by Requester Type, Fiscal Year 1995–2000 (in percentages)

	1995	*1996*	*1997*	*1998*	*1999*	*2000*
News media	3.3	2.6	1.6	2.3	4.8	6.5
Scholars and historians	4.6	2.5	1.1	1.2	1.0	1.4
Prisoners	14.6	14.5	9.9	14.4	13.7	14.0
Private citizens	73.3	76.9	84.6	80.7	78.5	76.7
Organizations	0.8	1.1	1.5	0.4	0.5	0.2
Current employees	0.8	0.5	0.3	0.5	0.4	0.7
Freelance authors and writers	2.6	1.9	0.7	0.2	1.1	0.6

SOURCE: FBI data released under the FOIA.
NOTE: Percentages do not sum to 100% due to rounding errors.

Among the range of topics social scientists in the United States have documented using the FOIA are: (1) the collusion of academics with intelligence-gathering activities, (2) the surveillance of social scientists by law enforcement agencies, and (3) the attempt by the United States to establish economic, cultural, and political hegemony over other countries.

Prompted by an exchange about his political beliefs with the then Harvard president, McGeorge Bundy, Diamond (1992) sought access to his FBI file. As a result of the information Diamond obtained, he then made further use of the act to look in more detail at the relationship between U.S. universities and intelligence agencies. In particular, Diamond documents how a close involvement between senior university officials and the FBI monitored and controlled political dissent in a way that seriously compromised university autonomy.

Diamond (1993) further reveals how the FBI took an unhealthy interest in a study of national character that involved the interviewing of Russian émigrés by the Survey Research Center at the University of Michigan. Given the names of interviewers on the study and the localities in which interviews took place, there is evidence that the FBI subsequently interviewed some of the respondents. (Diamond's wider discussion of the ethics of informed consent in interview studies raised by this case is beyond the scope of this article.) Using (among other sources) records obtained under the FOIA, Simpson (1994) documents how the development of communications research as an academic specialty in the aftermath of World War II depended heavily on overt and covert funding from defense and intelligence agencies in the United States.

Price (1998) used documents released by the CIA under the FOIA to examine the work Gregory Bateson carried out for the Office of Strategic Services during World War II. Among other roles, Bateson was a propagandist in Southeast Asia for the Allied cause, and was decorated for his part in a dangerous foray across enemy lines to rescue captured Allied agents. Price contrasts Bateson's success in his intelligence role with his later expressed dislike of applied anthropology. According to Price, Bateson's view of applied anthropology might have been affected by misgivings he later developed about his wartime role. Price also notes, however, that Bateson's experience points to the enduring quality of the ethical dilemmas inherent in applied anthropology.

In his book *Stalking the Sociological Imagination*, Mike Forrest Keen (1999) uses files released under the FOIA to document FBI surveillance of a number of prominent sociologists during the cold war era. Sociologists obviously of the left, such as Robert and Helen Lynd and C. Wright Mills, were targeted as subversive, as were African American sociologists such as W.E.B. du Bois and E. Franklin Frazier, who were critical of racism in American society. However, even a figure such as Talcott Parsons, now almost universally regarded as a conservative social theorist, was the target of surveillance (see also Keen 1993), as was his Harvard colleague Pitirim Sorokin.

Drawing a parallel with the suppression of sociology in the Soviet Union around the same time, Keen notes that even apparently innocuous research activities could place an individual under suspicion. A research interest in the Russian family made Chicago sociologist Ernest W. Burgess a target for investigation, as did William Fielding Ogburn's survey of industrial dispersion, information from which was deemed to be useful to an enemy power. Samuel Stouffer and Edwin Sutherland came to the FBI's attention because their work on civil liberties and on white-collar crime, respectively, were deemed critical of the bureau and its director, J. Edgar Hoover. Herbert Blumer came under investigation when he applied to attend a United Nations Educational, Scientific and Cultural Organization (UNESCO) conference in Paris in 1956.

Keen argues that such cases demonstrate the power of anonymous informants during the cold war to bring the unwelcome attention of the state to bear on unwitting subjects. He also speculates that FBI activities had a chilling effect on sociological scholarship in the United States, particularly in relation to a critical engagement with the writings of Karl Marx.

Pendakur (1985) obtained U.S. State Department documents under the FOIA to examine who controlled the revenues and content of U.S. films imported into India. Calavita (1992) used the FOIA to gain access to Immi-

gration and Naturalization Service (INS) documents relating to the Bracero Program, a foreign labor system in which laborers from Mexico were contracted to growers and ranchers in the United States. Calavita shows how, unless its own interests were at stake, INS served the interests of farmers at the expense of their migrant workers. She also documents battles with the agency's congressional opponents and with the Department of Labor. Theoretically, she shows how the activities of the state are in practice neither monolithic nor uncoordinated.

Coleman and Seligman (1988) were skeptical about a series of surveys conducted in Central America that were sponsored by the U.S. Information Agency and that showed large proportions in favor of U.S. foreign policy toward the region. Using the FOIA, they were eventually able to obtain a codebook for the study that contained the questions asked and a set of frequency distributions for each item. Although they concluded that the findings from the survey were not artifactual, they were able to point to potential sources of invalidity in the data that resulted from the exclusion of potential respondents and ambiguities in question design.

THE FOIA AS AN ARCHIVAL RESOURCE

From the point of view of research, agencies subject to the FOIA can be thought of as archival repositories. As Hill (1993) points out, archives have a number of structural characteristics that constrain and channel their use for research purposes. First, access to archives is restricted and their use is spatiotemporally specific (i.e., the researcher must travel to the archive and only at times when it is actually open for business). Second, producers and donors of archival material often retain property rights over it. Third, the keepers of an archive occupy a structural position vis-à-vis the researcher, which gives them considerable scope, if they wish it, for obfuscation and deception. Fourth, archival materials are unique in that materials are not necessarily duplicated elsewhere. Fifth, what is recorded, and how, once recorded, materials are sorted, organized, or inventoried reflects organizational priorities, practices, and interests (Garfinkel 1967; Hakim 1983; Hill 1993). Although empirically intertwined, analytically one can make a distinction between issues relating to the availability of archival materials and those relating to their accessibility.

From this point of view, the FOIA can be seen as mandating the increased availability of records by treating them as being publicly owned rather than being the preserve of individual bureaucratic fiefdoms and by removing spatial barriers to access through what is, in effect, a mail-order system. In use

rather than spirit, however, the FOIA as a source of research data is heavily constrained by accessibility issues. These issues include the erection by agencies of protective barriers against disclosure; "black box" problems hindering the identification, specification, and location of records relevant to a particular research purpose; and frictional barriers caused by the working of the act itself.

PROTECTIVE BARRIERS

Fuller (1988:99) draws attention to the role of the state in creating and maintaining what she calls "forbidden research terrains"—"whole areas of possible investigation, which may be geographically, intellectually, or institutionally defined, where social scientists are strongly discouraged from pursuing research." Existing sociological uses of the FOIA suggest that it has a role in illuminating forbidden research terrain. However, its use in such contexts is by no means unproblematic. In many cases, the exemptions built into the act sharply circumscribe the kinds of material available.

Under the terms of the FOIA, material relating to national defense or foreign policy that have been properly classified under a presidential executive order can be withheld. Material is exempt from disclosure under the FOIA where disclosure is expressly prohibited under the terms of another statute. An exemption is made in this way, for example, for census records, tax records, and grand jury deliberations.

The Supreme Court ruled in 1985 in relation to the CIA that under this provision information relating to sources and methods of intelligence gathering is exempt from disclosure. Records can be withheld where disclosure would interfere with law enforcement proceeding, deprive an individual of the right to a fair trial, constitute an invasion of privacy, compromise the identity of a confidential source, reveal techniques or procedures used by law enforcement agencies, or endanger the life or physical safety of an individual.

In the case of intelligence agencies, a so-called Glomar response to a request for disclosure of records is also possible. That is, the agency can refuse not only to disclose records on the basis of appropriate exemptions, but can also refuse to confirm or deny that they actually exist. According to Dempsey (1998), Glomar responses are routine when information is sought from the CIA about named foreign individuals or specific events overseas.

These exemptions and exclusions can seriously hamper scholarly work. Dempsey (1998) argues that since almost anything could be regarded as providing information about the sources and methods used by an agency, in effect, this exemption allows agencies like the CIA very wide latitude to

withhold information. In relation to exemptions dealing with confidential informants and law enforcement techniques, Nelson (1998) points out that it is difficult for a researcher to weigh the value of a particular piece of evidence if it is not known who provided it or how it was obtained. She adds that the CIA and the FBI have been rather diligent in protecting the identity of confidential sources, even when records requested refer to events taking place many years ago or where the source can be presumed to be deceased.

Moreover, there are grounds for supposing that occasionally claims that the privacy of individuals is being protected have been used to cover up the fact that information has been collected through the use of illegal investigative techniques. Scholars who study the U.S. intelligence community generally complain of legislative, judicial, and presidential timidity in challenging attempts by agencies to invoke national security exemptions in relation to FOIA provisions (Aid 1998; Charns and Green 1998; Dempsey 1998; Theoharis 1998; Wiener 1998).

Deliberately obstructive tactics appear to have been used by some agencies. Charns and Green (1998:99), describing their experience of obtaining FBI files, contend that "there is certainly some degree of internal bridling against the FOIA, and some degree of covert resistance against its requirements by the very officials responsible for its implementation." Agencies sometimes claim not to be able to find records, even when it is clear from other material released that the requested documents exist (Charns and Green 1998; Dempsey 1998). Writing of the CIA, Dempsey notes that requests on general topics that are somewhat nonspecific are often refused for being "burdensome" or "unsearchable," while Theoharis (1998:25) contends that the FBI has interpreted exemption provisions in the act "broadly and capriciously."

Agencies sometimes take steps to limit the usefulness of material released. According to Wiener (1998), these might include using "stick-on" notes for making annotations on documents. Annotations are releasable under the FOIA, but stick-on notes can be released separately from their parent files, making their context unclear.

As noted earlier, where an agency is not prepared to disclose records and the administrative appeal process is exhausted, litigation is possible. Calavita (1992) notes that FOIA staff at INS were overstretched and operated on the basis of crisis management. In this context, they welcomed litigation because it implicitly gave them a rationale for prioritizing litigated requests over others. However, compelling disclosure by legal means is time consuming and expensive, with no guarantee of success. Wiener's (1998) lawsuit seeking release of FBI files on John Lennon took thirteen years; even then some files were withheld on national security grounds.

According to Dempsey (1998), litigation will not necessarily secure full disclosure, but it does often force the agency to examine material in a more detailed and conscientious way rather than relying on blanket redaction. Theoharis (1998) makes the point, however, that scholars do not usually have the resources to litigate, and that only high-profile research projects are likely to attract pro bono legal assistance.

Even when one has obtained the material requested, further difficulties arise. A request can produce a great deal of superfluous or unusable material. Keen's experience is instructive; comprising almost half of the material on the American Sociological Association that he received from the FBI was a set of abstracts for papers given at the 1965 annual conference of the association. In addition, he received only "segregable" material, those parts of the records not covered by exemptions. What Keen (1992) describes as "significant portions" of the material were blacked out.

As Rosswurm and Gilpin (1986) point out, FBI files are often a source of materials such as personal histories of labor leaders, leaflets, union newspapers, and internal records that otherwise have been destroyed. They note, though, that in many cases, newspapers are often heavily excerpted in ways that overemphasize union radicalism. Sometimes material does not exist because it has previously been destroyed by the agency (Price 1997), although interagency variation can sometimes mean that material missed under one search can turn up under another (Dempsey 1998).

Keen (1992) also warns that material appearing in files can be inaccurate and unreliable. Pratt (1992), who has used the FOIA to conduct research on farm movements and the Left in Montana and the Dakotas, notes that disclosed material can sometimes provide information discreditable to other people. The FOIA only allows access to information about other people if they are dead. Pratt records that he is sometimes unwilling to approach the relatives of people potentially mentioned in FBI files to confirm their date of birth. The consequence is sometimes the need to spend considerable periods of time looking for gravestones in cemeteries.

BLACK BOX PROBLEMS

Information generated from sources such as the FOIA forms an important class of "unobtrusive measures" (Webb et al. 1966, 1981; Lee 2000). From the point of view of the social scientist, use of the FOIA as a means of generating data suffers from a limitation common to unobtrusive measures (Webb et al. 1966, 1981). Elicited data from interviews, questionnaires, and the like

tend to be generated with a particular research question in mind. Unobtrusive measures, on the other hand, depend on what is available to be found, captured, or retrieved (Lee 2000). There is little explicit guidance in the literature about how unobtrusive data relevant to a particular research problem can be generated (Sechrest and Phillips 1979; Webb et al. 1981). While a number of not entirely satisfactory solutions have been proposed, in fact, a close reading of Webb et al.'s work suggests that "underpinning their discussion of various data sources are a set of implicit heuristic strategies for finding data sources relevant to a particular research problem" (Lee 2000:13).

If anything, however, the heuristics surrounding the retrieval of archival data are less clear than those for other sources of unelicited data. This again relates to the structural characteristics of archives. The metaphors archival researchers use to describe archives or archival work are instructive. For Hill (1993:44), "Archives are essentially large 'black boxes' from which you must extract useful data without being able to look directly inside." Calavita (1992:13) likens archival work to an archaeological dig, and "as in archaeology, it is not possible to know in advance precisely what remnants to look for."

U.S. government records constitute a black box. Although a list of agencies, with descriptions of their functions and their addresses found in the *U.S. Government Manual*, there is no central list of records held by the U.S. government. Perhaps the nearest one comes to this are descriptions of records systems holding personal information that the Privacy Act requires agencies to make available in the *Federal Register*. Such descriptions, known as Privacy Act issuances, are published from time to time, and are now available on the Web.[5]

Faced with the black box problem, researchers have three broad choices. They can (1) gain cultural knowledge of the box's inner workings either for themselves or with the help of an expert insider; (2) rely on already codified information about the contents of the black box in the form of indexes, catalogues, and finding aids; or (3) bet on a certainty by looking for something already likely to be there.

The first strategy is time consuming and might pose its own problems of access. However, publicly available information on the history, mandate, goals, structures, and programs associated with a particular agency can be used to identify broad administrative purposes and the information needs likely to be associated with them.

The second strategy can also be time consuming if suitable material is not already in the public domain, in which case the black box problem might be reproduced in a different form. Having access to already available material

reduces the time spent searching, but is most fruitful if the concerns and interests of its producers are relatively close to those of the researcher. Speaking of "a level of file disorganization matched only in some people's attics," Calavita (1992:14) reports that INS records are "catalogued according to a variety of overlapping and unwieldy systems," none of which pointed unambiguously to her specific topic of interest.

The second and third strategies both risk the exploitation of resources already mined at the expense of new, richer, but less obvious sources. In sum, the black box problem discourages the opportunistic use of data, can channel researchers in the direction of well-worn problems, and potentially requires research skills that Marx (1984) claims are underdeveloped in the professional socialization of sociologists in the United States.

FRICTIONAL PROBLEMS

Researchers with experience of using the act are unanimous that it is unwise to rely on using its provisions to obtain information if you are working on tight deadlines or must base an entire project on the expectation that fully usable material will be available (Keen 1993; Noakes 1995; Price 1997). An agency has ten working days in which to comply with a request. In practice, however, requesters must often wait while a backlog of requests is processed. Keen (1992), for example, waited eight months to receive FBI records relating to the American Sociological Association. Diamond (1992) records that between 1977 and 1991 he exchanged more than 1,700 letters with the FBI in relation to FOIA requests.

Table 2 shows the number of FOIA requests received during Fiscal Year 1999 by a number of intelligence, law enforcement, and regulatory agencies. The list is meant to be indicative rather than exhaustive. Also shown for each agency are the number of staff who work full-time on FOIA matters, and the median processing time in days for complex requests, that is, those involving voluminous records or complicated searches. The CIA takes about six months to process complex requests to completion, while the National Security Agency and the FBI each take something on the order of three years. By contrast, the Drug Enforcement Administration processes complex requests in fourteen days, the Environmental Protection Agency takes twenty-five days, the Equal Employment Opportunity Commission eighteen days, and the Immigration and Naturalization Service twenty-one days. It is important to note that these figures reflect agency determinations of the outcome of a request, which might include among others denial or only partial granting of

TABLE 2
Freedom of Information Act (FOIA) Requests Received
During Fiscal Year 1999 by Selected Federal Agencies

Agency	*Number of Requests Received During Fiscal Year 1999*	*Full-Time FOIA Staff*	*Median Processing Time for Complex Requests (days)*
Central Intelligence Agency	5,485	27	187
Drug Enforcement Administration	2,452	21	14
Defense Intelligence Agency	1,215	5	302
Environmental Protection Agency	18,841	94	25
Equal Employment Opportunity Commission	15,838	11	18[a]
Farm Credit Administration	43	0	6[a]
Federal Bureau of Investigation	20,754	671	1,148
Food and Drug Administration	27,823	74	47
Immigration and Naturalization Service	168,944	175	21
National Security Agency	1,111	17	1,047

SOURCE: FOIA annual reports for all executive branches and agencies are available at http://www.usdoj.gov/04foia/fy99.html.
a. All requests.

the material requested. For example, in Fiscal Year 1999 the FBI denied in full fewer than 3% of requests, but only 6% were granted in their entirety. One-fifth of requests were granted only in part, while in 40% of cases the bureau claimed to have no records pertaining to the material requested.

Noakes (1995) points out that because of delays, researchers frequently have recourse to other sources. Previously released files are available in agency reading rooms, in other archives and repositories (see Price 1997:14), books, and microfilm collections. An increasing volume of material is being made available via the Internet, particularly in the form of frequently requested records. Advice from those who have extensive experience in using the FOIA stresses the importance of being precise about the records required, providing information that will make easier the identification of relevant material, and being patient in the face of delay.

Even when there is no deliberate obstruction, the request process often doesn't operate smoothly. Understaffing, underfunding, and bureaucratic disorganization all seem to play a part. Some writers have noted that changes in personnel or in administrative structure within an agency can dramatically shift levels of compliance with the FOIA (Aid 1998; Theoharis 1998).

Noakes (1995) makes the point that those who deal with requests under the FOIA are usually middle-ranking workers within a large bureaucracy. They have little involvement in the formulation of policy, nor do they actually review documents for security purposes. However,

> they can exercise a great deal of "street-level" power. Given their position in the hierarchy, one of their primary goals is to fill as many requests as possible in as efficient a way as possible so as to avoid angry phone calls from citizens, senators, and supervisors. (Noakes 1995:275)

In other words, while patience is a virtue and it is probably unwise to make enemies of the officials who deal with requests, those seeking information under the FOIA ought to make it clear that they are familiar with their rights, unlikely to be deterred by delay or obfuscation, and ready to seek judicial remedy if the need arises (Adler 1987).

THE WIDER USE OF THE FOIA

The work produced by social scientists using the FOIA is interesting and illuminating. In particular, writers like Keen tell an important story about the relationship between the social sciences and the state in the cold war era and beyond. From another point of view, however, it might be argued that sociologists using the FOIA have tended to focus on a narrow range of issues, agencies, and data sources. As noted earlier, the level of academic use of the FOIA is probably on par with that of journalists. Some evidence, however, suggests that journalists make much wider use of the FOIA than do social scientists.

The Center for Investigative Reporters and Editors, a nonprofit organization dedicated to investigative reporting based at the University of Missouri, maintains a database of news stories by investigative journalists. This database was searched for abstracts of newspaper articles in which the FOIA had been used to obtain information. Abstracts were categorized by broad topic and the target of the investigation (see Table 3).

As the table shows, 33% of the articles abstracted were concerned with official corruption, malfeasance, or financial mismanagement. Around 17% dealt with the regulatory issues surrounding environmental and consumer protection and gun control, and a further 15% dealt with gender or racial inequalities, child abuse, and human rights issues. Relatively few stories dealt with safety issues or business (mal)practice, while a scattering dealt with access to information, crime, U.S. involvement in foreign wars, or celebrities.

TABLE 3
Content of News Stories Involving
Use of the Freedom of Information Act (FOIA)

Type of Story	*Percentage*
Official wrongdoing (corruption, malfeasance, financial mismanagement)	22
Regulatory issues (environmental protection, consumer protection, gun control)	24
Health and medicine (medical malpractice, public health, health inequalities)	12
Civil and human rights (gender/racial inequalities, child abuse, human rights abuses)	10
Safety issues (transportation safety, public safety)	10
Business practices (business malpractice, working conditions, strike breaking)	6
Access to information (workings of FOIA, official willingness to provide information)	6
Crime and deviance (organized crime, underworld figures)	4
Foreign wars and veterans (Vietnam, Gulf War)	2
Celebrities (files of famous people)	2
Total ($N = 202$)	100

SOURCE: Investigative Reporters and Editors (IRE) Resource Center Database (http://www.ire.org/resourcecenter/).

One can look at story topics in a different way by focusing on who formed the subject of the news stories. The main categories are: corporate personnel, public officials, and/or employees (45%); people currently serving in the military (11%); and institutional populations of patients, pupils, or inmates (15%). There are obvious overlaps here with the concerns of criminologists, political sociologists, and those interested in social divisions or the workings of total institutions. Even issues concerning national pride and its related symbolism and the social meanings of fame can be addressed, as with stories that deal with foreign wars or celebrities (see Table 4).

Of course, this might mean very little more than that sociologists and journalists share a common interest in social issues. An alternative view might be to argue the utility of moving away from seeing the FOIA as a source of personal records or as providing a window on government and toward seeing it more generally as a tool for studying bureaucratic organizations. The lesson of journalistic use is that the FOIA potentially produces data relating to how organizations deliver services, make decisions and policies, process client populations, and relate to other organizations and stakeholders. In this context, the legal mandate of the FOIA provides a degree of leverage over the records produced by official bureaucracies not available when studying the private sector.

TABLE 4
Persons Investigated in News Stories Involving
Use of the Freedom of Information Act (FOIA)

Subject of Investigation	*Percentage*
Corporate personnel	20
Public officials	14
Public employees	13
Service personnel	11
Company employees	8
Institutional populations	16
Other/not clear	18
Total (N = 202)	100

SOURCE: Investigative Reporters and Editors (IRE) Resource Center Database (http://www.ire.org/resourcecenter/).

Writers like Keen (1992) and Marx (1980), who have urged social scientists to make more use of the FOIA, point to an apparent lack of awareness of what the act can provide. Obviously, however, we are not dealing with a matter of simple awareness since few educated Americans are unaware of the FOIA. As Keen and Marx both hint, the matter is more obviously one of methodological culture. During the 1970s, some sociologists appropriated the term *muckraking sociology* to describe a research style that had an "exposé, sacred cow-smashing, anti-establishment, counter-intuitive, even subversive quality" (Marx 1972:3).

This approach was designed to sustain social criticism and facilitate radical social change by self-consciously emulating investigative journalism of the kind that had unearthed the Watergate scandal and had formed part of an earlier tradition in American journalism, which, at the turn of the twentieth century, had sought out and exposed municipal corruption. Muckraking sociology,[6] then, sought to uncover what the powerful wanted to keep hidden for their own ends.

Underpinning this position was an assumption that the research relationship should be defined not by trust, but by distrust. People, especially the powerful, were assumed, as a matter of course, to have the motive, means, and opportunity to conceal information from researchers. To lack suspicion, therefore, is to collude with one's own deception. Such a stance required a methodological reorientation away from the survey-based methods dominant at the time. One alternative was to make use of the information generated by investigative, legislative, and judicial bodies.

There is little evidence that the radical methodological prescriptions associated with muckraking sociology produced a decisive reshaping of empirical research in U.S. sociology (Lee 1993). Speculatively, this was due to a number of factors. Reviewing the rise of what he calls the radical-critical theory group in U.S. sociology during the 1970s, Mullins (1973) noted that writers in this tradition tended to favor exegesis, programmatics, and polemic over empirical work. At the same time, when they did empirical work, radical-critical sociologists didn't completely abandon the social survey as a methodological tool (Wells and Picou 1981), or looked to field methods as a basis for reorienting methodological practice (Douglas 1976). More widely, we can observe, on the basis of contemporaneous survey data, that while sociologists as a group were further to the left than colleagues in other disciplines, they still tended to separate political orientation and methodological style (Janowitz 1972; Lipset and Ladd 1972).

Turning to today, Cappell and Guterbock (1992) and Ennis (1992) have mapped the structure of U.S. sociology by analyzing data on the degree of overlap between membership in special interest sections of the American Sociological Association. Using multidimensional scaling to represent this structure in a graphical way, they independently locate critical sociology some way from the mainstream of the discipline. Moreover, Cappell and Guterbock (1992:270) present a further analysis suggesting that "critical specialties are not as institutionalized in the discipline's graduate curriculum as applied specialties." Analysis of this kind suggests that social ties between scholars are stronger within clusters of specialties than between them. In other words, both the formal and informal structures of the discipline are likely to inhibit the diffusion of the kind of methodological innovation represented by the wider use of institutionalized discovery practices.

CONCLUSION

Theoharis (1998) carried out a survey of historians who had used the FOIA to study the FBI. Some respondents reported problems such as being unable to identify relevant material, having to pay substantial amounts in processing fees, or receiving very large volumes of material containing a high level of dross. In general, however, his respondents evaluated their use of the act in a positive way. The material they had obtained had been vital to the successful completion of their research. In many cases, they had received material that simply could not be obtained in any other fashion. Actors and actions that had previously been hidden were made visible in a detailed and specific way. The day-to-day activities, tactics, strategies, procedures, and

underlying assumptions of the FBI were laid bare. In some cases, the bureau had gathered in one place material that would otherwise have been completely scattered.

Records provided under the provisions of the FOIA often gave leads to new areas of inquiry and insights into matters previously thought obscure. And Calavita's (1992:16) observation was that the INS records she obtained, while in many ways partial and incomplete, are an invaluable source for understanding bureaucratic processes "relatively uncontaminated by self-censorship and official posturing." What is being described here, of course, are the strengths of archival research and of unobtrusive methods more generally.

What lessons are there in the U.S. experience for researchers in other countries who might want to use their own freedom of information legislation for research purposes? The first lesson is surely that, as Gary Marx (1984) suggests, institutionalized discovery practices can provide a viable source of data for social research. To take the United Kingdom as an example, there is clearly scope to investigate security service involvement in universities during the cold war in terms that parallel the work of U.S. scholars. Against this, the U.S. experience is obviously cautionary about the revelatory potential of research using the FOIA.

Although how the boundaries are drawn obviously differs from country to country, national security, law enforcement, and personal privacy exemptions are in general typical of FOIA legislation wherever it is found. The scope of such exemptions, how they are framed and interpreted, and their interrelations establish in part the contours of the secret state. Again to take the United Kingdom as an example, even with an FOIA in place, researchers are still likely to face barriers to the investigation of sensitive topics such as Northern Ireland and the policing of political and industrial dissent.

In this context, compliance regimes are important. Snell (2000) notes that one benefit of litigation surrounding freedom of information legislation is that the issues involved are explicitly tested in the courts. However, he points out that in Australia, which modeled its legislation on the U.S. act, lengthy and costly legal battles around requests for information are, as in the United States, not uncommon. Moreover, the legal framework has encouraged agencies to take and try to uphold a narrowly legalistic view of disclosure requirements. Snell contrasts this situation unfavorably with the ombudsman system in New Zealand (and often in European countries) in which an independent arbiter mediates between requester and agency and can enforce agency compliance.

Research methods are sometimes ill adapted to the study of their own accomplishment. In field research, for example, the access process is rarely

studied from the point of view of the gatekeeper to a research setting (Lee 1993), and there are difficulties in using survey methods to study nonrespondents to surveys (Goyder 1987). An analogous problem might operate with institutionalized discovery practices. How far ombudsman systems tilt the balance of power between requester and agency more firmly in the direction of the requester than the litigation driven approach found in Australia and in the United States is, of course, an empirical matter. However, speculatively, the potentially adversarial relations between requesters and agencies typical of the U.S. system might have inhibited a theoretical understanding of the internal organizational principles by which agencies respond to FOIA requests. The need to study organizational culture as a way of understanding what records are available and what is accessible for research purposes is an important lesson for researchers in countries outside the United States.

Nearly a century ago, Georg Simmel (1906:464) observed that the value placed on gaining access to restricted items of information "often enough falls into a significance entirely subordinate to the fact that others are excluded from knowing them." Form, in other words, does not always signal significant content. One downside of using nonreactive data is that the researcher is cast potentially into a rather passive role (Marx 1984). Using data from institutionalized discovery practices, it is rarely possible to produce a tailor-made research design. One must make do, so to speak, with what is available off-the-rack. Thus, as Keen and some of Theoharis's respondents found, the usability of disclosed data can be limited by a fairly high level of dross. From this perspective, the U.S. experience might be seen as arguing for a dampening of the expectations that sometimes surround freedom of information legislation. There might be important lessons here for researchers in countries like the United Kingdom or Ireland. Traditionally in these countries, official culture has been strongly secretive (on the U.K.'s Official Secrets Act see, e.g., Lee 1993:22–23), and expectations have been correspondingly high.

As indicated earlier, levels of FOIA use by categories of user are not well documented in the United States. The same seems to be true of other countries. What little scattered evidence there is suggests relatively low levels of media and academic use wherever FOIA legislation is found. The indication that, compared to social scientists, journalists in the United States have used the FOIA to investigate a wider range of topics is intriguing. It suggests a potential for the use of institutionalized discovery practices that has yet to be fully realized by social researchers anywhere.

NOTES

1. Michael Ravnitzky, a well-informed commentator on Freedom of Information Act (FOIA) matters, has pointed out to me that academics use the act for business as well as research purposes "to determine why their grant application was denied, or their project proposal was not selected, or to determine the funding levels for future years in a particular research area" (e-mail communication, 16 July 2000). This article deals only with research uses of the FOIA.

2. The URL is http://www.usdoj.gov/04foia/fy99.html.

3. The high number of requests to Immigration and Naturalization Service (INS) probably reflects a fairly routine use of the FOIA by immigration lawyers to obtain information about the status of visa applications. According to the Social Security Administration's annual FOIA report, a large number of requests that the agency receives are from genealogists asking for copies of the original application forms for social security numbers: http://www.ssa.gov/foia/foiareport99.htm.

4. Figures refer to requests, not requesters. An individual requester can, of course, make multiple requests. Quoting the official in charge of the Federal Bureau of Investigation's (FBI's) FOIA activities, Hernandez (1996) gives the following figures for 1995: private individuals, 74.6%; prisoners, 14.7%; scholars and historians, 4.1%; news media, 2.8%; freelance writers and authors, 2.4%; organizations, 0.8%; and current employees, 0.6%. It is not clear how the discrepancy between Hernandez's figures and those shown in Table 1 arise, although they might reflect the difference between Calendar Year figures and Fiscal Year figures.

5. At http://www.acess.gpo.gov/su_docs/aces/1997_pa.html.

6. The term *muckraking* in this context is derived from a speech by Theodore Roosevelt in which he attacked the increasing use of exposure tactics by journalists and the increasingly socialist tone of much of that reporting (Lindner 1996:24).

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Learning How to Find Out: Theories of Knowledge and Learning in Field Research

CATI COE
Institute for Community Research

In Learning How to Ask*, Charles Briggs argues that asking questions follows cultural conventions. Field workers carry assumptions about the nature of talk and knowledge, and their questions may elicit different kinds of information and relationships than expected. This article looks ethnographically at theories of knowledge in Akuapem, Ghana, and how they interacted with the author's own native theories in interviews. Learning local conventions of knowledge transmission thus becomes one of the major tasks for the field worker.*

> Mepɛ sɛ anka mihu wɔn amanne, nanso enye sɛ wubisabisa asɛm pii. (I would have wanted to see/understand their customs, but it was not appropriate to ask many questions.)
>
> —Diary of Basel missionary Andreas Riis of his journey to Kumase and stay in Fomana, 11 December 1839; originally in German and translated into Twi by N. Clerk, and printed in *Kristofo Sɛnkekafo* (1917:78–79) (translation to English mine)

In *Out of Our Minds* (2000), Johannes Fabian examines the practices of scientific inquiry of some explorers in Central Africa. In the former Gold Coast (now Ghana), these ancestors of some modern anthropologists included missionaries like Andreas Riis, who studied local customs and languages for the purpose of converting Africans to Christianity. Reading missionaries' reflections on their travails in the Gold Coast, I was surprised and shocked to see parallels to my own fieldwork difficulties in Akuapem,

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Field Methods, Vol. 13, No. 4, November 2001 392–411

Ghana. In the above statement made after eleven years of living and working in the Gold Coast, primarily in Akuapem, Riis reflects an awareness of the limits of asking questions, which may have been for him, as it was for me, a primary route to understanding.

This article explores the problems of interviewing in Akuapem and the mutually incommensurable modes of discourse and knowledge production invoked by my conducting life-history interviews as part of an ethnographic study of the production of national culture in schools in Akuapem, Ghana. My struggle with local genres of knowledge transmission made me look more closely at the construction of knowledge and practices of learning in schools and community contexts. I urge researchers to attend to the breaks, misinterpretations, and silences in interviews and conversations, not only because they help us to frame more appropriate and useful questions, but also because local theories of knowledge may themselves be more central to the topics we (as knowledge brokers and producers) study than they first appear.

The speech genre of interviewing was not a local genre of speaking or knowledge transmission. Anthropologists have been concerned with epistemological issues in terms of experimenting with more dialogic or more visibly subjective or multigenre representational practices (Clifford and Marcus 1986; Rose 1990; Behar 1996) and in terms of examining the power relations in which ethnographic knowledge is produced (Asad 1973; Fabian 2000). However, they have not been similarly engaged in examining and transforming field methods to reflect those epistemological concerns (but see Wolf 1996). Rather, within anthropology, field methods remain unexamined and mysterious. As a result, students may come back from difficult fieldwork experiences looking for greater rigor in field methods, feeling that the problem was in their training, not in the methods themselves. Anthropologists need to argue explicitly against behaviorist, naturalist, and reductionist perspectives of fieldwork, in which "the object of anthropological inquiry is the 'stuff out there,' " and the stuff has as two of its primary attributes stability and observability (Karp and Kendall 1982:251). The epistemological issues that anthropologists have been raising regarding representation should be brought to bear on field methods, both in considering local ways of knowledge production and in focusing on the co-construction of meaning, rather than its discovery, within fieldwork.

One exception to this lack of interest in field methods within anthropology is Charles Briggs's *Learning How to Ask* ([1986] 1992). He argues that interviews are a multifaceted speech event, encapsulating the native theories of communication of researchers, rather than those of respondents. As a result, interviews often involve miscommunication and misinterpretation. He describes how his questions to Mexicanos in northern New Mexico were

answered with, "Who knows?" Researchers come to the field with specific rhetorical modes that may or may not make sense to the people to whom we talk. Briggs argues that researchers should listen to and imitate local metacommunicative strategies before actively participating in such exchanges by asking questions; they first need to learn how to ask.

However, the difficulties go beyond a lack of correspondence in researcher and native communicative frames. Briggs ([1986] 1992) suggests this when he argues that researchers' folk belief of language foregrounds the referential or descriptive function of language, pointing to objects, people, events, and processes, and ignores the speech act (as speech act) and its context. Thus, researchers' native beliefs highlight the informational content of language rather than, for instance, its role in redefining the relationship between the speaker and listener. Briggs thus points out some of the assumptions behind researchers' native theories of knowledge.

What are researchers' native theories about knowledge? Michel Foucault (1990) describes the process by which sex, first the object of Christian confession, became medicalized during the nineteenth century. Truth must be told, and its telling liberates the one who confesses. However, confession requires an interlocutor, a person in authority, who can interpret: "The work of producing the truth was obliged to pass through this relationship if it was to be scientifically validated" (p. 66). Although Foucault is analyzing the roots of psychology, this route for the production of scientific knowledge may also hold for ethnography, in which recitation is therapeutic and knowledge is transformative. Knowledge in and of itself is considered desirable and essential to progress. Many of our field methods assume that knowledge has to be verbalized and explained to be valid (Mavanhão 1993); it does not reside in successful practice or bodily experience.[1]

As Foucault and others have shown, this desire to know the intimacies of people is connected to regimes of discipline and power. Those in power, both emperors and corporate managers, desire "systematic social knowledge written down" to best manage and direct (Rose 1990:31; Bendix 2000). Thus, the desire to know of those in power helps sustain ethnographers' use of language that expresses literal meanings (referential language) and representation of knowledge that is decontextualized and independent of the circumstances in which it was produced.

Recent studies of learning are important here. Rather than conceiving of knowledge or learning as internalized or something acquired, Jean Lave and Etienne Wenger (1991) talk about learning as situated practice. As a result, knowledge is never "out there," a stable and observable object, but is distributed throughout the complex structure of people acting in that context (Lave 1993). Knowledge is enacted, discussed, and negotiated by people who know

different things and speak with different interests and experiences as they constitute a situation together. So conflict is ubiquitous to human existence, including learning. I argue here that not only should we pay attention to local metacommunicative practices but also to local meta-theories of knowledge, which form the basis for respondents' interpretation of what our research activities mean and what we are seeking. We need to make sure our epistemological concerns, reflected in theoretical debates and representational practices, also govern our field methods in understanding how "knowledge of the socially constructed world is socially mediated" (Lave and Wenger 1991:51).

AKUAPEM AND THE NATION, CHRISTIANITY, AND CULTURE

Akuapem is a hilly area in the eastern region of Ghana. Its seventeen towns lie on a low-lying hill range, ranging from 1,200 to 1,600 feet high (Blay 1972). Numerous smaller villages are in the valleys near the farms, and migration has long pulled people away from their towns. However, mansions in the towns show that people are fiercely loyal to their hometown: They return home for festivals and funerals on weekends, and this is where they will build a house if they can. Towns in Akuapem compete with one another, and conflicts between them have been heightened by inhabitants' knowledge of their town's history, told with an awareness of ethnic identity and past injustices.

The first settlers of Akuapem were Guan-speaking.[2] However, since the seventeenth century, Akuapem has been conquered and ruled by Akan (Twi)-speaking peoples, of whom the first were the Akwamus, who engaged in linguistic imperialism, forcing accused persons detained in court to learn Twi to defend themselves. Local historians in Akuapem recount the atrocities of the Akwamus as a reason why Akuapems asked the rulers of Akyem Abuakwa for help in a war to overthrow their Akwamu rulers. After the Akwamus had been defeated and driven across the Volta River, the Akyem warriors asked Akuapem for a reward for helping them and, since the Akuapems had no gold, they asked the Akyems to rule them. The Akyems, also an Akan people, were given land to settle at the present-day towns of Akropong and Amanokrom. They brought in Akan political organization, appointing relatives and friends to important posts and organized the seventeen towns into a traditional Akan hierarchical arrangement that was based on military formations. Akropong became the capital of the new kingdom with the paramount chief. This history is reactivated today with Guan ethnic

consciousness and the de facto disintegration of the Akuapem traditional state into several independent states during the 1990s (Gilbert 1997).

I went to Akuapem to study the production of national culture in schools in Ghana. Ghana seemed to be an especially fascinating place to study the way that governments were attempting to jumpstart national development through the cultivation of heritage in the nation's youth. I sought to understand the forces—social, intellectual, and ideological—behind the promotion of Ghanaian culture in schools, and the reasons why people fought for and through culture. I also planned to look at how a national curriculum and cultural policy was transformed by social actors in schools in Akuapem. My research focused on how schools become sites for the production, invention, and objectification of national culture, as constituted not only by government officials and national elites, but also by local actors.

The Provisional National Defense Council (PNDC) government of Ghana that came to power in 1981 was especially interested in producing a national culture and rejuvenating tradition, writing a new cultural policy, promoting national festivals, and instituting the teaching of culture in schools (National Commission on Culture 1991).[3] The school-based program has two components. As part of a World Bank–sponsored education reform, a subject called cultural studies was added to the national syllabus for the first nine years of education in 1986, in which students studied music, dance, life-cycle customs, verbal art, and religion in classroom settings. Another, more popular method has involved cultural competitions between schools in which students perform through drum language, poetry recital, choral music, and dance-drama, and display their work in arts and crafts exhibits.

Interviews were only one method among many to understand the ideological reasons for and the unintended effects of this incorporation of cultural traditions into schools. I lived in the town of Akropong, Akuapem, for twelve months from August 1998 to August 1999 (a school year). I observed classroom lessons on cultural traditions and other subjects, rehearsals for cultural competitions, and the competitions themselves; attended church services, annual festivals, and Sunday school services; and did historical research in regional and national archives. In seeking to understand the complex effects of the production of national culture in Ghana, I focused on three areas: (1) historically, the process by which culture became an object of discourse, a selection of the complex flow of cultural practices, and associated with the nation; (2) contemporary competing discourses about culture and the way that students and teachers negotiated between government policy and Christian identifications in classrooms and performances; and (3) the impact of the teaching of culture in the schools on students' relationship to knowledge. Using these three foci, I documented the complicated, problematic effects of

cultural programming directed toward youth through schools in a postcolonial African nation. I examined how the practices and meanings of schools in one area (Akuapem) transformed the ideological intent of government cultural programming, even as that programming created different possibilities than were otherwise available in schools.

I was introduced to Akuapem by a retired intellectual who promoted his town to outsiders, especially to foreigners, thus serving as a broker of outside influences. A former language teacher, he had taught many of the teachers in the schools and thus facilitated my entry into schools. This serendipity also made sense from a research perspective. A hilly place, and thus considered healthier for Europeans, the Akuapem ridge—and Akropong in particular—became the headquarters for the Basel missionaries in the Gold Coast in the 1840s. Akuapem has a long history of exposure to Christianity and schooling and a high rate of literacy (Kwamena-Poh 1973).

The churches in Ghana are opposed to traditional customs, an antagonism vividly shown when Ephraim Amu, a music teacher at the Akropong Presbyterian Teacher's College, was expelled from the church and the college for wearing African cloth to church in 1931 (Agyemang 1988). Akuapem is not representative of Ghana, but it is a place that highlights certain tensions that have relevance beyond Akuapem: the ideological tensions between Christianity and "tradition," the structural tensions of an economy built on agriculture but which rewards urban dwellers and those close to the state, and the fluctuating loyalties to hometown, ethnicity, and nation.

ASKING MANY QUESTIONS: THE PROCESS OF DOING ORAL HISTORIES

When I began my fieldwork in Akuapem in August 1998, I decided that during the first few months, when I expected to be slowly gaining entrée into schools, I would concentrate on the historical incorporation of tradition in Akuapem schools. I wondered whether the teaching of culture in classroom lessons dated further back than the educational reform of 1986, perhaps to the late colonial days in the 1930s and 1940s or the heady days of early independence in the 1960s. Knowing of the paucity of written documentation in the archives from a two-month exploratory trip in 1997, I hoped to study the ideological reasons for interest in traditional culture through oral history interviews with teachers, asking them about their experiences as students and teachers from the 1940s onward. Although the teaching of culture was a national reform, I was curious whether teachers had been active in promoting Ghanaian heritage, which the government had then adopted.

From the few people I knew, I gathered an initial list of names of older teachers with an interest in culture. From there, I used snowball sampling, and ended up conducting interviews with ten people, stopping before I interviewed all those recommended to me by others. Those I did interview were elderly men, ranging in age from their sixties to their nineties; they were retired teachers or ministers who had been teachers early in their careers. Their English was quite good. Initially, I conducted interviews in English; later, we spoke Twi, with English occasionally used when I did not understand what had been said or when I had difficulty expressing myself.[4] Sometimes, those doing the recommending would accompany me on my first visit to introduce me; other times, they would tell me to mention their names. Needless to say, I had forgotten Briggs's ([1986] 1992) advice about first observing and imitating local speech genres before embarking on interviewing; I was concerned about accomplishing all I had to do and thus proceeded with what I saw as my work.[5]

I will describe what happened during a series of visits and interviews that took place during the course of a month with Mr. Asante (a pseudonym) as an example for what commonly happened during interviews. He was one of my first key informants, and thus the difficulties with the process were magnified. During the first visit to Mr. Asante, the visit of contact, as I thought of it, I introduced myself and described my project: I was interested in the teaching of culture (the local term) such as proverbs, music and dance, and arts and crafts in the primary and junior-secondary schools (the first nine years of education). However, I needed some context for the reform, so I hoped Mr. Asante could tell me about how these things were taught when he went to and taught school. I said that I was writing my *long essay*, the word used in teacher-training colleges and universities for thesis.

I explained that I wanted to tape-record and conduct the interviews in whatever language would be most comfortable for Mr. Asante. However, maybe he would say something in Twi that I would not understand well, and I could go back and listen to it again if it was taped. I told him I also wanted to remember what he said and that I would be happy to give him copies of the tapes. Mr. Asante said that he would prepare something to read for the tape. I assured him that that was not necessary, but he ignored my protestations. He seemed excited and eager about the process. He told me that he was very pleased with my visit and that he had a lot of documents to show me. He also talked about his life history and introduced me to his family. We then made arrangements for a day I should return.[6]

On the second visit, I again described my topic in Twi. Mr. Asante began telling me about how crafts were taught in his primary school in Akropong, and then recounted his biography and the state of arts and crafts as he contin-

ued training in different places and became a teacher, moving from school to school in different towns across southern Ghana. During this process, he emphasized dates and places. He was uncomfortable with my turning on the tape recorder but he allowed me to take notes as he spoke. When he came to the end of his biography, Mr. Asante asked me what I wanted to study, and I explained a little bit more, changing my question to reflect his interest in arts and crafts. He told me that one reason he was helping me was because he felt that he should be documenting his life to pass on all the things he had seen and done. Although he considered himself an artist rather than a teacher, he felt he should be thinking now about teaching art to young children because of my interests. He then told me the origin of his name, and about his former wife who had died some years ago and his children. He invited me over for an evening meal, an invitation that I accepted.

At the next meeting, about ten days after we had had the meal prepared by his new wife, he let me turn on the tape recorder while he read from notes that he had prepared for this occasion and from an autobiography that he wrote in the 1980s. The notes he had prepared seemed to be the beginning of a textbook he was writing about claywork for teachers in primary and junior-secondary schools, detailing the importance of claywork, how to prepare the clay, and methods of firing and decorating the clay pieces. At the end of this meeting, after an hour and a half of his reading into the tape, I went home utterly bewildered, wondering what had happened. What had I said that made him think I wanted a textbook on claywork? How had he misinterpreted my interest in the teaching of culture in the classroom as a request for a textbook on claywork? I looked back at what I had said and done, but finally realized that we could only continue with our conversations at cross-purposes.

At the fifth visit a week later, Mr. Asante continued his reading of the textbook he was writing and when he was finished reading what he had written, he continued reading into the tape a talk he had given at the Legon Festival of the Arts in 1977 on "Contemporary Forms of Artistic Expression." After forty-five minutes, I used the clicking off of the recording, signaling the end of the tape, to divert the direction of our talk. Without touching the tape recorder, I asked more detailed questions of what had happened when he was trained at Achimota as an arts and crafts teacher and the goals of arts and crafts education at that time. I was desperate to get away from the general to the more descriptive and from the practical to the ideological. The conversation became very choppy at this point, with many more questions and answers directed by me. As I went home, I felt deeply embarrassed that I had not simply turned over the tape and let him continue.

I did set up one more interview time, in which he asked me to turn on the tape recorder and began reading again from his textbook, saying he was

"happy to continue our discussion of art education." Again, when he had finished, I asked follow-up questions about his experience and about claywork, resulting in more choppy discussion. This was our last interview. However, I retained contact with Mr. Asante, returning every month or so when I was in the neighborhood. Sometimes, I simply came to greet him and ask how he and his family were doing; other times, I gave him an update of my research, to which he might respond and elaborate. I wrote in June 1999 after a visit, "It feels as if I have to tell people what I am finding and not finding and getting them involved in that way." Once he borrowed my tape recorder and some tapes to interview relatives about his family history.

These patterns in interviewing were similar with other key informants, although other informants talked about the history of their town, the history of the missionaries and church in Akuapem, and the development of church music instead of claywork. They all took great responsibility for the accuracy of the information they were giving me, often reading from documents or books in their possession, but Mr. Asante was the only one who created a book for my benefit.[7] Because of this, they told me information in which they were expert and used to giving out; I was not the only supplicant for information for many of my informants. Thus, I was given lectures; I was given codified information that had movement and order, in the sense of having a beginning, a middle, and an end, such as a historical narrative or the introduction to a subject in a textbook.

I was not sure what was going wrong. The people I talked to seemed kind, as if they wanted to help me, and the atmosphere felt positive. They were certainly working hard to help me. And yet our understandings and interpretations of our mutual interactions were quite different.

WHAT CULTURE MEANT, TO GHANAIANS AND TO ME

At the point that I began the interviews with Mr. Asante, I was not completely aware of local meanings of the word *culture* (or in Twi, *ammamerɛ*) that I was using as a starting point in these interviews, although I did know it was a local term. People in Akuapem felt that culture touched their lives only occasionally, at nonordinary events: ritual ceremonies, festivals, and school cultural competitions. Embodied by chiefs, who were its caretakers, culture provided connections to ancestors and to the past. Culture was associated with performance, especially music and dance, and with traditional religious practices. Thus, it did not encompass Mr. Asante's interest in arts and crafts. I had been operating under late colonial notions about African culture, which

did include local arts and crafts, because I had read about the founding of Achimota school in 1927, in which both arts and crafts and performance traditions were important as emblems of African culture. The World Bank–sponsored Education Reform of 1986 had mandated the subject of cultural studies, as well as vocational studies, which rejuvenated the teaching of the same arts and crafts as had been taught at Achimota. I assumed that there was ideological as well as practical continuity between Achimota and this reform. However, for people in Akuapem, vocational studies was considered preparation for employment (albeit manual) and had no connection to culture, which was considered somewhat frivolous to the main business of schooling.

I define culture differently from most Ghanaians. I see culture as encompassing the everyday, embodied and habitual, practices of people; this includes practices of Christianity and schooling and processes of appropriation of Western items and ideas. It is contextual and flexible: We enact it in specific settings among specific people. For instance, I would argue that there are school cultural traditions in Ghana in which certain practices and speaking patterns have become natural and expected; in fact, school ways of transmitting knowledge were what I was eliciting in my interviews. But for many Ghanaians, culture meant Ghanaian culture, which could be avoided if one wished, or in which one could participate on Saturdays or specific festival occasions. Through my interviews and other more mundane conversations, I did learn the referent for the term *culture* and used it more appropriately thereafter.

LOCAL NOTIONS OF EXPERTS

I also realized that part of the problem was that I was looking at the wrong era. The people recommended to me were elderly men who had grown up in the 1930s and 1940s, when schools were under the control of the church; they were not terribly interested in culture, which was considered antithetical to Christianity. Furthermore, the little information I was getting about the past was fragmented: As teachers, these men had moved around a great deal from teaching post to teaching post, so I could not reconstruct a history of education in Akropong or even Akuapem because they were teaching in one town (perhaps in the Central or Brong-Ahafo Region or the north) for three years and in another town for the next eight.

It was the people currently in their forties and fifties, who had grown up in the era of independence when the teaching of culture really took off, whom I realized I should be asking. Perhaps I should be less interested in teachers

who had moved around from place to place than in people who had been students in Akropong. However, it was those who had not succeeded in school who would still be living in Akropong, and these people were precisely those who were not respected and would not be recommended to me as experts. Thus, snowball sampling is problematic when one's object of study is different from local peoples' understanding of important knowledge.

I did have some success asking questions about the history of the teaching of culture with middle-aged teachers whom I met when I visited schools. These were usually not scheduled interviews, but conversations on the porch or in the teacher's lounge in the early morning or during breaks, elicited by my talking about my interests and what I was finding. They told more personal stories and were less concerned with accuracy, because there was no tape recorder, they knew me better at this point than my key informants did, and their stories were embedded in a back-and-forth conversation.

However, when I conducted interviews with them, even the middle-aged teachers would lecture about culture in the forms of various important customs, narrating to me the order of activities within a festival or ritual, for instance. Often, I felt that I was breaking the frame of the conversation to ask about their personal histories and specific events or curricula in their personal experience. One problem was that the teaching of culture in schools was not considered knowledge in their sense. So, even when I used the term *culture* in a culturally appropriate way, even when I was talking to people who had personally experienced cultural programs in their schooling, they still wanted me to give me a body of knowledge, this time about culture itself, whereas I was asking about the cultural programs in schools. To understand the reasons for this, it is necessary to look at local theories about knowledge and learning in Akuapem.

LOCAL KNOWLEDGES AND CONTEXTS OF LEARNING

In Maurice Bloch's (1993) ethnography of knowledge, he distinguishes between different kinds of knowledge and communicative styles corresponding to different stages and statuses in the life-cycle of the Zafimaniry people of Madagascar. Children are associated with practical and scientific knowledge of the wild, married people with practical agricultural knowledge, and the elders with wisdom and history, so that with age, knowledge has decreasing relevance to the immediate environment and becomes more abstract. Bloch argues that literacy among the Zafimaniry is seen as similar to elders' knowledge, despite the fact that the majority of literates are young men.

Drawing inspiration from Bloch's typology, I distinguish between three different kinds of knowledge that I saw operating in Akropong and in Akuapem more widely. One is a practical kind of knowledge, centered on ordinary, everyday tasks such as housework (cooking, washing, sweeping) and farming. Another is a specialized knowledge, such as carpentry, *kente* weaving, and drumming, which depends on having access to contexts of performance through kinship or apprenticeship. A third is a knowledge focused on history (family and chiefly genealogies, the history of a town) and ritual practice. As mentioned above, history (church and town history) is an important speech genre in Akuapem, recited in church sermons, in ordinary conversations with elders showing off their knowledge in the form of nuggets of history, and during funerals.

Only some of these kinds of knowledges are identified as cultural by people in Akuapem: Some of the specialized knowledges, especially drumming and dancing, are given this label, but more commonly, the abstract knowledge of history and rituals are so designated. These knowledges are gendered and also attest to the gerontocratic hierarchy operating in Akuapem: Practical knowledge is available to everyone but is especially performed by young people and women, while the knowledge of ancient events and rituals is the most prestigious, not widely available, and in the possession of certain elders.

Competent performance is highly valued for all three knowledges, and even the most abstract knowledge is enacted: During funerals, genealogy is discussed to sort out issues of inheritance, and rituals are performed at festivals. Enactment might involve a display of verbal artistry. Accuracy means a great deal in public enactment. Performing badly in public results in personal humiliation and loss of reputation (see Yankah 1995); mistakes in ritual performance might end in punishment (even death) from angry spirits or ancestors.

The most powerful and sacred knowledge is considered secret, which is used to bolster the status of certain elders and chiefs. Just as the chiefs are protected from the profane world by the mediation of their *akyeame* or spokesmen (Yankah 1995), so too are powerful objects and events kept hidden and protected by indirection and secrecy. Most cultural and historical knowledge is considered to be secret and held by the elders; thus, it is called *mpanyinsɛm* or elders' matters. The secret nature of this knowledge is noted by authors in books that make that knowledge public. In the preface to a popular book documenting the various festivals of Ghana, A. A. Opoku (1970) wrote that it is difficult to give acknowledgments "in a book dealing with what is sacred and to some extent, secret in our cultural heritage." In a review of two books documenting different Akan festivals, I. E. Boama (1954) wrote:

> Two Twi festivals which every Akan should try to watch are Adɛɛ [Adae] and Odwira. But there are many people who even if they have seen these festivals, they have seen only a part. Because only insiders have permission to see the true [or pure] activities. . . . If you are a citizen [child of the nation], buy [these books] to read, and if you know your nation's secrets, you won't avoid them. (Translation from Twi by Afari Amoako and myself)

Cultural knowledge, at its deepest or most pure, was thus considered hidden, not accessible to outsiders; books documenting them violated that secrecy by describing rituals to nonroyals and youth.

Some history was also considered secret. As we walked down the main street of Larteh one day, one of my key informants, Teacher Asiedu, told me about doing his senior thesis for Presbyterian Training College in 1957 on the history of Larteh, and he came to talk to one elder in his hometown.

> The elder told him he would not tell him anything unless he brought drinks, and by the time he returned, the elder had died. Another elder would not tell him anything, and Teacher Asiedu, then a young man, rebuked him, saying, "If you don't tell, then how will the children learn?"
>
> "Why wouldn't they say anything?" I asked Teacher Asiedu in Twi.
>
> "*Wosuro*" (they are afraid), he said.
>
> "What were they afraid of?" I asked. He said that they were afraid that they would reveal something secret and the *ɔbosom* (spirit) would punish them. (Field notes, 19 February 1999)

The secrecy of certain historical and cultural knowledge allows powerful elders to manipulate important decisions regarding property rights and political positions, which are entwined with family genealogy and local history. As William Murphy (1980) points out, the content of the hidden knowledge does not matter as much as the privileged society the secrecy creates.

Although all the forms of knowledge in the community highlight their enactment, whether during morning housework or festivals or rituals in sacred groves, schooling in Akuapem is notable for its teaching of knowledge that is not practical or useful, in which knowledge is made abstract and into a game of word reproduction, a litany to be learned and not questioned, with very little relevance to everyday life. Classroom teaching—and the pattern became increasingly clear and strong at higher levels of schooling—consisted of teachers having a discussion: Teachers asked directed questions in which the explicit goal was to elicit student knowledge, but students recognized that they were supposed to figure out the answer in the teacher's mind. The discussion would result in various lists and definitions being put on the board. Then, notes would be given, in which the teacher would write down sentences and paragraphs on the topic on the board, often duplicating the

points of the previous discussion, and students would copy these notes into their notebooks. These notes would form the basis of exercises, questions in school tests, and (it was assumed) the nationwide exams.

Sometimes, for homework or class work, the teacher would write questions on the board based on the notes, and students would write the answers in their notebooks.[8] Notes give students the opportunity to review information from the board by copying it down into their own notebooks. This is a labor-intensive and mechanical process. Notebooks are often the material objects around which lessons revolve: Students hurry to copy notes down from the board; the notes are collected to be graded by the teacher, who often has stacks of notebooks on his or her table; and then the notebooks need to be distributed again and corrections made. Notes are therefore an important mechanism for turning everyday knowledge into school knowledge, and verbalizing embodied knowledge through English words, definitions, and lists.

The decontextualized and abstract nature of school knowledge makes it akin to historical knowledge and knowledge of the elders, but since it is taught to children, that knowledge is simplified and flattened, something I discuss elsewhere in more detail (Coe 2000). Unlike historical and cultural knowledge, however, school knowledge is only useful in its reproduction on exam papers.

THE PERFORMANCE OF EXPERTISE

I discuss these kinds of knowledge because they structure how people responded and made sense of my questions. It seems to me that many of my key informants considered my requests to be requests for school knowledge: Their recitation from documents and my reproduction of those words and facts through the tape recorder or writing looked similar to the form of knowledge transmission from teacher to student, minus a blackboard. This resulted in formalized, codified knowledge, not personal experience. My questions about culture elicited descriptions of customs and rituals from middle-aged teachers who were then teaching those descriptions or who had been taught those descriptions in their youth. Accuracy was important in the sense of correct performance, where informants took care to present true information, an anxiety increased by my recording.

I was not the only one seeking information from them. Teacher Asiedu made sure that I knew that other people came to him for information about the history of his town. He commented, complimenting me at the same time, because people with wisdom came to visit him, his children will also think that he is wise. To a young teacher who stopped by in the middle of my visit,

Teacher Asiedu said that we had come to learn *mpanyinsɛm* or elders' matters from him. Thus, my informants felt that my questions contributed to their status and authority as knowledgeable elders in the public eye or within their families. Teacher Asiedu in particular had transformed himself from retired teacher to elder in the town, *ɔkyeame* to the chief, and he may have seen my interests as more in the nature of *mpanyinsɛm* than school knowledge.

To highlight their expertise, my key informants indexed their age and experience through their narration of their life history, listing the dates and places of their birth, schooling, teaching posts, further education, later positions, and retirement. Sometimes this narration would include an explanation of their names and discussion of family history, as we saw with Mr. Asante. After several experiences of this, I learned to ask for this kind of life history. The first time I did this, the informant's wife, who was listening to our interview, gave a satisfied grunt, showing the appropriateness of this question. Other questions that indexed my informants' age and experience proved fruitful, such as asking for comparisons between the past and present. Elders often compared the present with the past to highlight their knowledge and to critique present-day matters.[9] I learned to ask, "What was education like in your time?"[10] and as a follow-up, "What has changed since?" thus inviting critique of the present. Both these questions showed respect for their expertise and experience and elicited specific speech genres associated with elderly men, thus helping to construct the interview context as that of an elder teaching a young person.[11]

I was glad to be told their life stories because I saw it as an avenue for asking more specific questions about their schooling and teaching experiences. However, my informants did not understand why I was asking questions about it; they wanted to tell me their perspective and knowledge whole, as a package, seamless and smooth. Changing the direction of the conversation and interrupting their lecture on the topic was not respectful of their age and expertise. In fact, asking about their personal experience may have been interpreted as a challenge to their authority—Do you really know this? Were you really there?

My goal was a conversational, open-ended interview, free-flowing and informal (Jackson 1986; Rubin and Rubin 1995); my respondents' understanding of the interview turned out to be more formal, in which they were imparting codified, accurate knowledge as elders to a young person, or as teachers to a student. Therefore, my emic notion of what a conversational interview should be, supported by my professional training, elicited a genre of talk in Akuapem (*mpanyinsɛm*) that was more formal, systematized, and careful than I expected or wanted.

OTHER METHODS

I visited schools and observed classroom lessons on culture and other subjects. I continued attending church services around town, conversing with people who stopped me on the street, and going to festivals and other rituals. I talked to young adults in the family with whom I was living, visited teachers in their homes, and was adopted and protected by two older women teachers. I maintained contact with anyone I met and kept visiting them. I relied on informal conversations, letting others direct the conversation and asking a question if there was an opportunity. I gained entrée to situations accompanying acquaintances and friends to church, parties, and the market, and visiting them in their homes and schools.

As secondary schools began preparing for cultural competitions in February, I accompanied the district cultural studies officer as he gave workshops to teachers, and then picked three secondary schools to follow as they rehearsed and then performed. I interviewed judges of the competitions, with varying success, depending on their interest in culture. I also conducted focus group discussion with performers and some individual interviews after the competition. The group discussions worked better than the ones with individual students: My power was diminished in a group and students corrected each other and elaborated on others' statements. In all these interviews, people wanted to describe various traditional customs for me, and I slowly negotiated what I wanted, asking questions about their experience with cultural competitions in their primary and junior-secondary school, the rationale for their performances, and their mode of learning. However, because we had a common basis (the rehearsals and cultural competition) for discussion, I could ask more knowledgeable questions. I also felt that adolescents were more open with me than adults. None of these interviews lasted longer than one session of an hour or an hour and a half.

Watching the rehearsals for the cultural competitions and festivals, I noticed that children and youth learned to perform through observation followed by imitation. Children and adolescents practiced offstage, in backstage contexts, out of the view of critical adults, and only performed publicly when they felt confident that they would perform correctly under others' scrutiny. During a focus group discussion with boys, I asked how they learned to play the drum *fɔntɔmfrɔm*:

> We listen, you see. That thing, they don't drum *fɔntɔmfrɔm* one day only. If we hear *fɔntɔmfrɔm*, they don't just do it for a day or a minute and then stop. They do it continuously, so we listen all the time they are doing it. We listen to how it goes, like we listen to exactly how the sound goes, and we follow it. So if I hear

> that they are drumming it like that, I follow it exactly, the rhythm. Next time, if I go [to a place] and no one is there, I practice the way I heard them drum. That is how I saw how to drum. That is how I saw [learned] how to play the drums. (Taped discussion, 22 March 1999; translation from Twi by Afari Amoako, Kobina Ofosu-Donkoh, and myself)

I wish I had started out learning to ask questions the way this young man learned to drum. Instead, I learned through experimentation, listening for success or failure, for questions or statements that allowed people to open up and others that created silences or other topics. Instead, only after repeated frustration, did I step back and observe how people learned.

CONCLUSION: FROM INTERVIEWS TO OBSERVATION

I have argued that interviewing generated different kinds of speech genres and knowledge transmission than I expected. My frustrations with the interviewing process led me to observe and imitate interactions more closely, something Briggs advocated researchers should do before they begin asking questions. These struggles with the process helped me to see some things more clearly—the construction of knowledge, theories of learning, communicative strategies, and the importance of correct performance—that I came to see as central issues in the teaching of Ghanaian culture in the schools.

Interviewing, as every conversation, is negotiated, not only in its form but also in its interpretation. Both respondent and interviewer are signaling and interpreting the context for the interview through their talk. However, in my case, my respondents and I had different theories of knowledge and ways of transmitting knowledge. Whereas I came with a sense of knowledge being accessible and public, free of power relations, people in Akuapem saw knowledge, especially cultural and historical knowledge, as secret and protected, intricately interwoven with social status and power. I assumed that historical knowledge was based on personal experience, the standpoint or identity of my informant, whereas my key informants used life history as a marker of their age and expertise, but preferred to construct knowledge as reproducible (written), accurate, generalized, and descriptive, following the norms of school knowledge in Akuapem mixed with local notions of elders' knowledge and history. Without understanding the guidelines for this transmission of knowledge, I sought to control it through my own criteria for interviews.

Knowledge is socially mediated, bound up in my position as American researcher and their positions as elders imparting knowledge. Our social positions and understandings of knowledge production and transmission influenced what we said to one another and how we said it. All knowledge is socially mediated, but these interviews at cross-purposes make this more visible. My growing knowledge of local strategies of learning simply allowed me to more smoothly elicit elders' knowledge and to participate in that conversation as a respectful young person.

Thus, learning local conventions of knowledge transmission is essential to fieldwork. We need to understand the interpretive frames our questions and interviews elicit, as well as how our interests intersect with local notions of knowledge and expertise. In my case in Akuapem, this meant learning how to find out through experimentation, observation, and imitation.

NOTES

1. At the same time, there is attention being paid to bodily experience and embodiment as a route to knowledge (Sklar 1994; Young 1994).

2. The Guan may have arrived in the fifteenth century but, based on archaeological evidence, definitely by the end of the sixteenth century (Kwamena-Poh 1973).

3. Instability within the military caused a coup in 1979 by flight lieutenant Jerry Rawlings, who then allowed a civilian government to be elected. Two and a half years later, however, this democratically elected government was toppled by another coup led by Rawlings, on the common charge of corruption. The new government was headed by Rawlings and the Provisional National Defense Council (PNDC). Although at first socialist and populist, the PNDC later became the darling of the International Monetary Fund (IMF) and the World Bank and instituted reforms of structural adjustment and economic liberalization. Due to the international community's pressure, the PNDC government began a process of democratization and transformed itself into a civilian political party, the NDC, which won presidential and parliamentary elections in 1992 and 1996. To stress the continuity between the PNDC and the NDC, many political scientists who study Ghana, as well as newspaper columnists in Ghana, refer to the (P)NDC government, and I follow their example.

4. My fluency in Twi increased during the course of the year. By the end, I was able to understand most sermons and primary and junior-secondary school lessons conducted in Twi and have everyday conversations with people. I had difficulty reading the more archaic and proverbial language of poetry, chiefly courts, and the Bible. Generally, as with most second-language learners with a grammar and dictionary, my reading and writing ability was greater than that of my speaking or hearing.

5. Dan Rose (1990) discusses the form of life of ethnographers as a cultural practice embedded in the rise of corporations.

6. A human subjects review at the University of Pennsylvania was not customary for anthropological research. I personally was concerned about consent and use of materials, and I did have informants sign an informed consent form saying that I could use the information they

provided in publications, for which they could choose to be named or anonymous. If they wished for changes to this form, I acceded to their requests.

7. In a discussion about consent, one informant told me not to use the information "raw" but to check with other people and documents to determine its truth.

8. This teaching strategy is to some extent a response to the lack of textbooks; when textbooks are pulled out of their closets, five or more students share one book while huddled around a table, reading upside-down, sideways, or over another's shoulder.

9. For instance, one secondary-school teacher whose lessons I observed highlighted the difference between the celebration of funerals in his time and the present-day for his students. He was called *ɔpanyin* or elder. I am sure that this was not just for his age (in his sixties) but also because of his rhetorical style.

10. For "your time," I used the Twi terms, "wo bere so" and "saa bere no."

11. I was twenty-seven years old at the time of these interviews, but many people in Akuapem thought I was younger than I was.

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REVIEWS

Narrative Inquiry: Experience and Story in Qualitative Research, by D. Jean Clandinin and F. Michael Connelly. San Francisco: Jossey-Bass. 2000. 211 pages.

Clandinin and Connelly's introduction to narrative inquiry offers its readers examples of the stories and narratives the authors advocate and invites readers to respond with their own. I was tempted to frame this review as a story, but my stories differ from theirs and would take readers down other paths. Their stories and what they call "storying" are grounded in their experiences as educators, educational researchers, and research supervisors in the last decades of the twentieth century. This account of their research lives and experiences shares characteristics of an emerging postmodern literature of inquiry on the human condition.

Clandinin and Connelly emphasize that their book is neither an analysis of narrative nor a textbook of methods for narrative analysis: "It is a book that tells stories of how we do narrative inquiry. We hope that it is a book on thinking narratively, a book that tells something of what it is that narrative inquirers do" (p. 187). They build the book around accounts of doing and writing research and on researchers' reflections on these experiences.

Unlike other methodological work in the human and professional sciences, this approach is framed with concepts from two thinkers associated with education, John Dewey and Jerome Bruner. The authors appear to base their idea of narrative inquiry and thinking on Bruner's (1986) classic differentiation between narrative and paradigmatic knowing: narrative knowing being structured as stories and paradigmatic knowing being as structured as arguments. More explicitly they draw from Dewey's (1938) formulation of human experience as characterized by situation (occurring in specific place), continuity (occurring in past, present, or future), and interaction (personal and social). Clandinin and Connelly seek to understand human experiences as they occur in time and space and as they are simultaneously social and personal. They emphasize that the participant experience being studied and the researcher's experience of studying it also interact. The authors represent these experiences in narratives—narratives built by participants as well as narratives constructed by researchers from their own and their participants' accounts.

Field Methods, Vol. 13, No. 4, November 2001 412–417

The book is organized in ten short chapters, with a prologue and an epilogue offering the authors' reflections on what they present. The chapters cluster in three groups. Chapters one to three integrate the authors' research experiences with the conceptual frame they endorse as best suited to the study of human experience. They reject what they call the grand narrative of research, conventional notions of rational empiricism whether quantitative or qualitative. Their position is that the formal theories and reductionist thinking that most social scientists use preclude understanding of human experience as it occurs to people in their times and places and as that experience is always social and personal. Chapters four to nine address topics common to most qualitative research methods books, but from Clandinin and Connolly's narrative perspective. Thinking through the research purpose is lodged in the researcher's own history and experience. Entering, working in, and exiting the field are personal, social, and professional experiences conducted from the particular position a researcher takes. Collecting, analyzing, and interpreting data or "field texts" constitute stories themselves: Fieldworkers live a story as they are collecting others' stories and as they are composing a narrative to represent what they make of themselves studying others' lives. Chapter ten addresses such issues as ethics and quality of work that percolate throughout events and decisions in research.

The strength of this text is, first, its development from the lived experiences of two master scholars—from their own inquiries and from their work with student researchers. Anyone new to narrative inquiry and analysis will vicariously experience both research and the mentoring of research in these detailed, poignant accounts. Second, researchers in education and other professional fields like social work will find a conceptual frame grounded in practitioner concerns. Third, researchers seeking exemplars of what a postmodern inquiry might look like will welcome this work. Human relationship, human purpose, and human position are highlighted, and the tensions, ambiguities, and uncertainties of real work in and out of the field are viewed as indicators of representing well rather than conducting badly.

What postmodernists will not find here is much reflection on the constructs underpinning research projects. Throughout the text researchers reflect on their goals, their practice, and their relationships, using Dewey's experience in time-space-interaction frame. This construction itself is never probed for suitability, and reflection on what else researchers know from their professional and disciplinary educations—what Bruner would call their paradigmatic knowledge—is rarely represented. The authors describe the ideal narrative relationship to formal theory and conventional analysis as working on the boundaries, but the boundary-working stories they include lead only to rejection of theoretical ideas in narrative inquiry. Overall, how-

ever, the research examples provided indicate careful thought, but how the researchers "worked" those theoretical boundaries to achieve conceptual integration is invisible in this account. This would make an excellent chapter in the next edition of this fine introduction to narrative inquiry.

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Grounding Grounded Theory: Guidelines for Qualitative Inquiry, by Ian Dey. San Diego, CA: Academic Press. 1999. 282 pages.

Many remember the days when colleagues and sociology chairmen were skeptical at our claim to teach qualitative methods. In the 1960s and 1970s, few of the top Carter-rated institutions in the United States had formal sociology courses on qualitative methods. In Toronto (as elsewhere) in the past decade, our graduate program has moved from a qualitative course option—those who took it were looked at as sociological softies—to a required graduate course. The designation of qualitative sociology as soft has changed considerably. Much of this paradigmatic transformation can be traced to the spread of grounded theory.

Dey starts his dense book on grounded theory with similar observations. He is perturbed that grounded theory has been canonized. Now that there are software solutions to organizing qualitative methods, these methods and especially grounded theory have become an acceptable way to do and analyze qualitative research. His book challenges the widespread but not thought through acceptance of grounded theory from several directions.

The volume is a detailed and critical analysis of Glaser and Strauss's (1967) and Strauss and Corbin's (1990) classic manuals on grounded theory. Dey mainly discusses the contradictions embodied in their work and in the work of those that follow and write about grounded theory and qualitative theorizing. He focuses on a number of issues. One overriding issue is the balance between quantitative and qualitative methods.

Dey's underlying story line is that grounded theory has not clarified its relationship to quantitative methods. When Glaser and Strauss developed their several well-known forms and terms for coding and indexing, they challenged the supremacy of quantitative methods to be scientific. They advanced the view that coding qualitative data could be as rigorous as in the quantitative field. Dey devotes chapters 2 to 6 to assessing coding issues. He creatively shows how the mechanics of coding are not clear. (He prefers the term *categorization* to *coding*.) He reminds us that in their concern to conceptualize and theorize, grounded theorists have not conclusively addressed the issue of proof. We are comfortable when qualitative methods suggest relations between data and concepts, but proof eludes us. When have we collected enough data? As one of my engineering colleagues put it, "I stopped when the funds ran out." But most of us that do qualitative sociology "feel" when to stop. Like the notion of data saturation suggests, we may simply get bored, tired of getting more data. We are more interested in data that may challenge what we have found than repeat it. And so the constant comparative method is appealing. Here, grounded theory gives us few guidelines as to when to stop collecting data.

The issue of quantification is related. Dey contrasts terms like *magic* versus *mechanical* to note our unease with counting social phenomenon. Yet he also notes that qualitative sociologists include inveterate counters that try to find ways to embed and draw nominal measures from the data. Let's take the important concept of social capital that is spreading rapidly through our field. We can ask individuals about their useful contacts, and then use these materials in either a quantitative (Lin, Cook, and Burt 2001) or qualitative manner. But even qualitatively, we can find ways to count similar forms of helping, and learn of different situations in which help from one's contacts is never offered or is always extended. Grounded theory readings have not devised canons to tell us how we can go about the counting process.

Others think of doing qualitative sociology as mainly doing humanist and sensitive sociology (Denzin 1989). Dey talks about the tension between creative sensitive observations and the rigorous coding schemes, which have gained ground. One can contrast the samples given in the tutorials for the qualitative analysis computer program NUD*IST 3 and 4 and NUD*IST 5. The tutorials in NUD*IST 3 and 4 were sensitive to taking the role of the

other, as in coding respondents' varied views of "smoking at work." In contrast, the tutorial in NUD*IST 5 focuses more on analyzing the index system. Nuanced thinking about interaction is not easily fitted into grounded theory.

This tension between sensitive sociology and rigorous sociology spills over to interaction. Our anthropologist colleagues have raised the issue of researcher's involvement in research. Since the 1980s and the classic *Writing Culture* (Clifford and Marcus 1986), anthropologists have stressed that we must work into our analysis the understanding that the investigator is part of the investigation. Grounded theory's more rigorous approach to qualitative analysis cannot handle this issue to Dey's satisfaction.

Dey also raises the important issue of structural levels. Missing in grounded theory is a clear analysis of how we should categorize and take into account structural bases to data. Dey faults grounded theory for overlooking the hard structures that won't go away. It is generally recognized in sociology that we need to bring in the value of social structure. But there are no rigorous cannons of weighing the institutional context as we code.

The last part of the book (chapters 7 to 11) are on the processes of analysis and argument. Dey discusses the difficulties in grounded theory of analyzing causes, conditionality and responses, structure, and agency.

This book is an important and timely, and I should say past due, analysis of grounded theory. It is not light, easy reading, however. Perhaps because Glaser and Strauss felt that the single case did not provide enough theory, Dey presents no lengthy qualitative data set or examples that might ease the book's accessibility. There are some ad hoc examples, sketches, boxed dialogues, and lists of items that summarize the text make the text only slightly more accessible. Examples turn on practical images—ducks, dogs, and furniture—and humorous word play in the spirit of Lewis Carroll. However, if we think of this as a volume for those that want to teach about, more than do, qualitative sociology, it fills an important slot. Dey has systematized what many of us have felt. Many of us have been doing what he advocates, but have not considered how to fit it into grounded theory. He gives us a lot to think about.

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INDEX

to

FIELD METHODS

Volume 13
Number 1 (February 2001), pp. 1-112
Number 2 (May 2001), pp. 113-220
Number 3 (August 2001), pp. 221-300
Number 4 (November 2001), pp. 301-420

Authors:

AGAR, MICHAEL H., "Another Complex Step: A Model of Heroin Experimentation," 353.
BAER, ROBERTA D., see Weller, S. C.
BATES, CHRISTOPHER, see Trotter, R. T.
BERNHEIMER, LUCINDA, see Weisner, T.
CHIBNIK, MICHAEL, "*The Politics of Large Numbers: A History of Statistical Reasoning*, by Alain Desrosières" [Review], 214.
CLANCEY, WILLIAM J., "Field Science Ethnography: Methods for Systematic Observation on an Arctic Expedition," 223.
CLARK, LAUREN, and LORENA ZIMMER, "What We Learned from a Photographic Component in a Study of Latino Children's Health," 303.
COE, CATI, "Learning How to Find Out: Theories of Knowledge and Learning in Field Research," 392.
DRURY, JOHN, and CLIFFORD STOTT, "Bias as a Research Strategy in Participant Observation: The Case of Intergroup Conflict," 47.
GALLIMORE, RONALD, see Weisner, T.
GARRO, LINDA C., "*Ethnographer's Toolkit*, by Jean J. Schensul and Margaret D. LeCompte" [Review Essay], 103.
GOOSBY, ERIC, see Trotter, R. T.
GREEN, EDWARD C., "Can Qualitative Research Produce Reliable Quantitative Findings?" 3.
GREENE, PAUL D., "Handheld Computers as Tools for Writing and Managing Field Data," 181.
HELMERSSON, HELGE, and JAN MATTSSON, "Demonstrating Pertex: A New Method for Improving Text Interpretation," 115.
KROESEN, KENDALL, see Weisner, T.
KUZNAR, LAWRENCE A., and OSWALD WERNER, "Ethnographic Mapmaking: Part 1—Principles," 204.
KUZNAR, LAWRENCE A., see Werner, O.

Field Methods, Vol. 13, No. 4, November 2001 418–420

LEE, RAYMOND M., "*Documents of Life 2: An Invitation to a Critical Humanism*, by Ken Plummer" [Review], 297.
LEE, RAYMOND M., "Research Uses of the U.S. Freedom of Information Act," 370.
LEPKOWSKI, JIM, "*Analysis of Health Surveys*, by Edward L. Korn and Barry I. Graubard" [Review], 216.
LIST, DENNIS, "The Consensus Group Technique in Social Research," 277.
MALONEY, R. SHAWN, and MICHAEL PAOLISSO, "What Can Digital Audio Data Do for You?" [Short Takes], 88.
MATSUMOTO, GORDON, see Valentine, K. B.
MATTSSON, JAN, see Helmersson, H.
MELROSE, MARY J., "Maximizing the Rigor of Action Research: Why Would You Want To? How Could You?" 160.
MEYER, CHRISTINE BENEDICHTE, "A Case in Case Study Methodology," 329.
NEEDLE, RICHARD H., see Trotter, R. T.
PAOLISSO, MICHAEL, see Maloney, R. S.
PREISSLE, JUDITH, "*Narrative Inquiry: Experience and Story in Qualitative Research*, by D. Jean Clandinin and F. Michael Connelly" [Review], 412.
REESE, LESLIE, see Weisner, T.
ROCHA, JORGE M., "Using Emergy Analysis in Ethnographic Field Research," 244.
RYAN, GERY W., see Weisner, T.
SALAFF, JANET W., "*Grounding Grounded Theory: Guidelines for Qualitative Inquiry*, by Ian Dey" [Review], 414.
SINGER, MERRILL, see Trotter, R. T.
STOTT, CLIFFORD, see Drury, J.
SUTROP, URMAS, "List Task and a Cognitive Salience Index," 263.
TROTTER, ROBERT T. II, RICHARD H. NEEDLE, ERIC GOOSBY, CHRISTOPHER BATES, and MERRILL SINGER, "A Methodological Model for Rapid Assessment, Response, and Evaluation: The RARE Program in Public Health," 137.
VALENTINE, KRISTIN BERVIG, and GORDON MATSUMOTO, "Cultural Performance Analysis Spheres: An Integrated Ethnographic Methodology," 68.
WEISNER, THOMAS, GERY W. RYAN, LESLIE REESE, KENDALL KROESEN, LUCINDA BERNHEIMER, and RONALD GALLIMORE, "Behavior Sampling and Ethnography: Complementary Methods for Understanding Home-School Connections among Latino Immigrant Families," 20.
WELLER, SUSAN C., "Using Electronic Scanning Forms for Data Entry" [Short Takes], 198.
WERNER, OSWALD, "How to Reduce an Unwritten Language to Writing: III. Phonetic Similarity, Suspicious Pairs, and Minimal Pairs" [Short Takes], 97.
WERNER, OSWALD, and LAWRENCE A. KUZNAR, "Ethnographic Mapmaking: Part 2—Practical Concerns and Triangulation" [Short Takes], 291.
WERNER, OSWALD, see Kuznar, L. A.
ZIMMER, LORENA, see Clark, L.

Articles:

"Another Complex Step: A Model of Heroin Experimentation," Agar, 353.
"Behavior Sampling and Ethnography: Complementary Methods for Understanding Home-School Connections among Latino Immigrant Families," Weisner et al., 20.
"Bias as a Research Strategy in Participant Observation: The Case of Intergroup Conflict," Drury and Stott, 47.

"Can Qualitative Research Produce Reliable Quantitative Findings?" Green, 3.
"A Case in Case Study Methodology," Meyer, 329.
"The Consensus Group Technique in Social Research," List, 277.
"Cultural Performance Analysis Spheres: An Integrated Ethnographic Methodology," Valentine and Matsumoto, 68.
"Demonstrating Pertex: A New Method for Improving Text Interpretation," Helmersson and Mattsson, 115.
"Field Science Ethnography: Methods for Systematic Observation on an Arctic Expedition," Clancey, 223.
"Handheld Computers as Tools for Writing and Managing Field Data," Greene, 181.
"Learning How to Find Out: Theories of Knowledge and Learning in Field Research," Coe, 392.
"List Task and a Cognitive Salience Index," Sutrop, 263.
"Maximizing the Rigor of Action Research: Why Would You Want To? How Could You?" Melrose, 160.
"A Methodological Model for Rapid Assessment, Response, and Evaluation: The RARE Program in Public Health," Trotter et al., 137.
"Research Uses of the U.S. Freedom of Information Act," Lee, 370.
"Using Emergy Analysis in Ethnographic Field Research," Rocha, 244.
"What We Learned from a Photographic Component in a Study of Latino Children's Health," Clark and Zimmer, 303.

Short Takes:

"Ethnographic Mapmaking: Part 1—Principles," Kuznar and Werner, 204.
"Ethnographic Mapmaking: Part 2—Practical Concerns and Triangulation," Werner and Kuznar, 291.
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