

SPIES LIKE US



WHEN SOCIOLOGISTS DECEIVE THEIR SUBJECTS

By CHARLOTTE ALLEN

THEY CALL THEMSELVES GUINEAMEN. For more than two hundred years, they and their forebears have fished, hunted, raised livestock on, and otherwise made their living from a broad peninsula of marshland in a corner of the Virginia tidewater region, where the York River meets the Chesapeake Bay. Although many Guineamen now work outside the peninsula, a large number still ply the traditional Chesapeake waterman's trade, generating a distinctive local culture centered around the outboard skiff, Ford pickup, rubber wading boots, snap-brim cap, and plug of tobacco.

When I visited them one afternoon this past summer, members of two Guinea families were sitting in the yard in front of their trailer homes. When I explained to them why I was there, they began jeering and trading jibes. The target of their mockery was not another local but Carolyn Ellis, a sociologist at the University of South Florida, whose prizewinning 1986 book about the Guineamen, *Fisher Folk* (Kentucky), transformed her in their eyes from a beloved outsider and frequent guest into a traitor.



For nine years, from 1972 (when she was an undergraduate at the nearby College of William and Mary) to 1981 (when she completed her doctoral dissertation at SUNY Stony Brook), Ellis spent her weekends and summers researching a "kinship network" among a particular group of Guinea watermen. Her theory was that the Guineamen lacked the external social mechanisms--strong churches, economic cooperation, a sense of community beyond the extended family--necessary for them to prosper. The conclusions of the book were not flattering to the region, which already had a reputation for white-trash backwardness and marshland criminality.

In her writing, Ellis used pseudonyms to conceal her subjects' identities--a standard practice in sociology. Guinea became "Fishneck," and members of the local families she described were given plausible-sounding made-up names. But that didn't stop her words from causing hurt. Ellis's "Fishnecks" were often illiterate, obese, poorly dressed, and ignorant of basic hygiene. "Scarcity of plumbing meant baths were infrequent," she wrote. "That combined with everyday work with fish produced a characteristic fishy body odor, identified by outsiders as the 'Fishneck smell.'" What most riled the fishing families who had taken Ellis into their homes, fed her meals, and let her stay over on many nights, however, was that she never once let on that she was using them for sociological research. "I thought she was nice," fumes one Guinea woman whose family hosted Ellis often over the years. "But she turned out to be a liar."

SOCIOLOGISTS have argued over the propriety of deceptive research for decades. But in 1995, the debate took a decidedly heated turn. In April of that year, Ellis published a remorseful essay in the *Journal of Contemporary Ethnography* enumerating the ways she had deceived her subjects. And her essay, which provoked much discussion among her colleagues, was not the only controversial confession that year: *The American Sociologist* published a far less remorseful account by a sociologist who some felt had used deceptive techniques to research police interrogation procedures. Finally, this spring, after two years of raging debate on the topic, the American Sociological Association (ASA) approved a set of stringent new ethics guidelines for professional conduct

More starkly than ever before, these events illustrate the degree to which the profession is caught in an uneasy bind between fulfilling research objectives and honoring ethical obligations. Sociological deception can take many forms, some more subtle than others, but all equally entangled in moral dilemmas: A researcher might not tell his subjects that he is using them for research purposes; or he might misrepresent the motives of his research; or he might violate a pledge to keep the identities of his subjects fully anonymous. In recent decades, researchers have practiced these forms of deception, and each has been earnestly defended and attacked within the profession. Ellis's behavior, it turns out, was unusual but not unique; in some ways, her deception was simply easier to see because--as she herself admitted--it was so blatant.

ACCORDING to her confession in the *Journal of Contemporary Ethnography*, Ellis secretly tape-recorded conversations with her Fishneck subjects, eavesdropped on their small talk, and coaxed data out of them while pretending to be visiting socially or doing favors, such as writing letters, baby-sitting, and driving them to doctor's appointments.

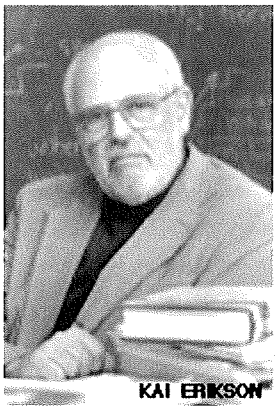
"Initially, I told a number of the Fishneckers who knew I was a college student that I was writing a paper on fishing," she writes. As the years passed, nearly everyone forgot about the college connection, until finally, Ellis writes, "I was just Carolyn coming to visit."

When he read her essay, veteran sociologist Herbert J. Gans of Columbia University was concerned. He wrote Ellis a reproofing letter. "I told her that I'm old enough to be her Dutch uncle and that what she did was wrong," says Gans. "She told people she was their friend. I told her, 'Yes, you use friendly methods, but you're always a researcher. You arrange to tell people every so often, I'm not your buddy. I'm a researcher.'"

Ellis agrees that she committed a sociological sin, and she said so with admirable candor in her essay--albeit after she had published her book, received a prize for it from the ASA, and won tenure at the University of South Florida. Still, she's convinced that deceiving her subjects was indispensable to her project's success. "I know I did them an injustice," she says from her Tampa office. "But I couldn't have done the study any other way. My study was predicated on my getting close to them, and if you're constantly reminding people that you're not one of them, you can't do that. They're afraid of the IRS, and I didn't want to make people suspicious of me."

Unlike Ellis, a significant number of sociologists who have engaged in deceptive research remain unrepentant. This group insists there is nothing unethical about deceiving one's subjects to a greater or lesser extent in the name of scientific research. Those who defend deceptive techniques claim subterfuge is sometimes the only way to elicit information from deviant and marginal groups--or from socially powerful groups that can otherwise justify secrecy. Defenders of deception typically use a cost-benefit analysis: If the deception doesn't hurt anyone very much and the payoff in data is high, covert research is worth doing.

Richard Leo's essay in the Spring 1995 issue of *The American Sociologist* made precisely this argument--in defiant, provocative language. Leo, then an assistant professor of sociology at the University of Colorado, boasted that he "consciously reinvented" his "persona" in order to gain admission into police interrogation rooms for research on his UC-Berkeley dissertation. Leo's larger point was that sociologists should have an evidentiary privilege--like doctors and lawyers--so they are not obliged to testify in court about what they see and hear in the field. But what struck many of his readers was his ardent defense of certain deceptive techniques. Leo declared that he had feigned conservative views (support for the death penalty, opposition to abortion and homosexuality) and had described his intimate relations with women in the same crude language he heard the cops use. In describing the ideological mask he had donned in order to study the ways officers question suspects, Leo proudly compared himself to "confidence men who wish to set up their marks." Leo's article had a crusading tone: He depicted police forces as deviant groups analogous to criminal gangs who broke laws (in the case of the police it was the Miranda rule and other constitutional protections) and required extreme measures to infiltrate.



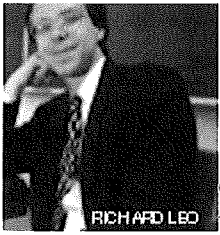
ELLIS SECRETLY TAPE-RECORDED
CONVERSATIONS WITH HER FISHNECK
SUBJECTS AND EAVESDROPPED
ON THEIR SMALL TALK.

Leo's essay provoked an angry counterblast from the eminent Yale sociologist Kai Erikson, who accused him of engaging "in a degree of deceit that is more widely known in espionage than in social research." As a graduate student during the 1950s, Erikson's own ethical standards had been less rigorous. He had applied for, but failed to receive, a position on a team of undercover social investigators led by the sociologist Leon Festinger whose mission was to infiltrate a doomsday cult by lying about their professional identities and pretending to be believers. (The project resulted in a famous 1956 book by Festinger and two colleagues, When Prophecy Fails.) Soon after, Erikson changed his views about deceit and took an absolutist stance against it, a position he has held ever since. His arguments are both ethical and practical: It is morally wrong to lie, and it also tends to distort research. (By assuming a false persona, for instance, the sociologist forecloses opportunities to collect more complete information through direct questioning.)

Yet, as even Erikson was forced to acknowledge, Leo's case hardly constituted the most egregious example of deceptive fieldwork. After all, Leo had informed the police department (in a city that he calls "Laconia") that he was a sociologist, and he had provided the officers with an accurate written description of his project. However, he also cut his hair short, shaved off a budding beard, and put on a coat and tie before he headed for the station--which for him was decidedly out of character. He might simply have been following the dictate of Erving Goffman, who declared in his 1959 sociology classic, The Presentation of Self in Everyday Life, that everyone is always role-playing and there is no such thing as one's true self. "I didn't lie to them about my views," insists Leo, who now teaches at UC-Irvine. "I just didn't try to argue with them when they raised the question of abortion or homosexuality. They'd say, 'You're not against the death penalty, are you?' And I'd just laugh. I know I gave the impression that I agreed with them. I just wanted them to think I was a normal person. From their point of view, a normal person was a conservative."

Of course, it is possible that Leo, a self-identified Berkeley graduate student in sociology, fooled no one on the Laconia force with his Joe Sixpack impersonation. "These guys have fantastic bullshit detectors, if you'll pardon my French," says Robert Jackall, a sociology professor at Williams College who spent more than five years prowling crime-ravaged precincts with New York City detectives as he researched his latest book, Wild Cowboys (Harvard, 1997). Jackall maintains that he did not need to use deception to go where he wanted, including interrogation rooms. "I just adopted the persona given to me by the police," he says. "They dubbed me the professor. They were teaching me, and they loved the symbolic reversal. I didn't have to penetrate anything."

Leo's response is that Jackall, a middle-aged tenured professor at a well-endowed liberal arts college, had time and job security on his side, which enabled him to dispense with



deception, whereas he, Leo, a penniless doctoral candidate working on a law degree at the same time, could not afford to spend more than the five hundred hours he gave to his fieldwork in Laconia. "I was a full-time student in my twenties, and I just didn't have that kind of time," says Leo. "I had to get inside those interrogation rooms."

But many of Leo's colleagues aren't buying this kind of reasoning. The Leo-Erikson debate, which continued through several issues of *The American Sociologist*, resulted in a panel discussion on the morality of deceptive research when the thirteen-thousand-member ASA met for its annual convention in August 1996. Then, this past May, the ASA voted in favor of a new code of ethics that specifically addressed deceptive research techniques for the first time. The new protocol requires sociologists to obtain their subjects' informed consent "when behavior of research participants occurs in a private context where an individual can reasonably expect that no observation or reporting is taking place." Further, it explicitly bans tape-recording and videotaping without subjects' permission, as well as the use of assumed identities. Despite its hard-hitting rhetoric, however, the ethics code contains a loophole: A sociologist may obtain a waiver (from his university or the ASA) for all these constraints.

AFTER the publication of *Fisher Folk*, Carolyn Ellis was wholly unprepared for her subjects' backlash. She hoped that the Guineamen would never learn of the book's existence. Although she traveled annually to the tidewater to update her research and to visit friends among the residents, she kept mum about her monograph. "They can't read," she says. "I never took the book to them. I didn't know how to deal with it, and I hoped they would never see it."

Perhaps she underestimated the literacy rate in Fishneck. (In *Fisher Folk*, she puts it at 50 percent.) She certainly underestimated the wrath of Victor Liguori, one of her former professors at William and Mary. A specialist in maritime sociology, Liguori has spent thirty years or so working on a still-unfinished magnum opus about Guinea. He knows many residents on the peninsula, and it was he who introduced Ellis to her first Guinea contacts, as part of his custom of taking interested students with him on his research excursions. Ellis sent Liguori a copy of *Fisher Folk* upon its publication--with an acknowledgment of his help, for he had shared his research notes with her. What happened after that is a matter of some dispute.

Ellis contends that Liguori, perhaps in a fit of professional jealousy because she had poached on his academic preserve, read the most damning passages of *Fisher Folk* aloud to the Guinea unlettered, suppressing everything positive she had to say about them and generally stirring up trouble. In her 1995 article, Ellis gave Liguori a pseudonym, "Professor Jack." Comparing him to a Pentecostal preacher on a Bible-thumping binge, she speculated: "Was he envious because he never finished his manuscript? Was any of his outrage justified? Or had he gone mad?" Liguori maintains that several Guineamen had obtained copies of the book, and others--who heard about it--contacted him and asked him to send them particular sections. Most of the Guineamen, he insists, read the book on their own--and then "went ballistic."

**ERIKSON ACCUSED LEO OF ENGAGING "IN A
DEGREE OF DECEIT THAT IS MORE WIDELY
KNOWN IN ESPIONAGE THAN IN SOCIAL RESEARCH."**

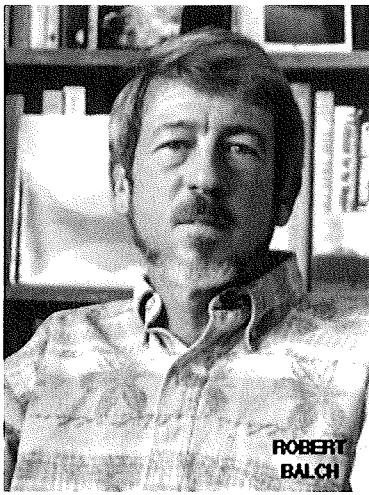
In any event, a friend eventually tipped off Ellis that several Guineamen were upset about her book, and she hastened to the marshes to beg for forgiveness. According to Ellis, after some angry exchanges about factual errors, geographical discrepancies, and broken confidences, nearly all her favorite Fishneakers forgave her. Liguori, however, contends that the Guineamen are unlikely to pardon Ellis so quickly. "One woman came up to me a month or two ago and asked, 'Is it true that she had bad things in there about the girls?'" Liguori told me in September. "And I still can't take one of my students into the marshes, especially if she's a young, attractive woman. Someone would say, 'Is she going to be another Carolyn Ellis?'" The Guineamen aren't the only ones who may be permanently shaken. Ellis herself hasn't done any fieldwork since completing her book. Her remorseful 1995 essay is representative of the work that has occupied her for the last twelve years: auto-ethnography--in which the subject is primarily the sociologist herself.

MODERN American sociology dates back to the 1920s, when the field, just beginning to get its bearings in this country, was regarded as a dubious European import. At the time, the discipline's stronghold was at the University of Chicago, where Robert Park eschewed the largely theoretical musings of his European predecessors--such as Max Weber--in favor of fieldwork based on long-term observation of one's subjects as they engage in social interaction. In Park's day, the possibility that a researcher at a local pub or political meeting might disguise his identity was virtually unthinkable.

Indeed, the ethics of deceptive research did not become a controversial topic in the profession until 1958. The occasion was a massive Cornell University study of participatory democracy in a local community and its unanticipated spin-off book, Small Town in Mass Society, co-written by a former project employee, Arthur J. Vidich. The project sent teams of graduate students into Candor, NY, pop. 2,500, to gather statistics. Vidich moved to Candor in order to oversee data collection and supply a friendly human face that would encourage village residents to cooperate with the survey. Now a professor emeritus at New York City's New School for Social Research, Vidich says that Cornell even advised him to join a local church. Although he had no interest in religion, he gamely taught Sunday school.

As part of the study, Vidich was also supposed to gather material for a more qualitative analysis of Candor's social structure. When he was hired, his supervisors showed him the code of ethics they had drafted. Vidich read it but "found nothing in it," he says today, "that related to the practical exigencies of day-to-day fieldwork. The code of ethics was a statement of intent, not a guide to conduct." (There was no provision for a participant observer like Vidich himself, for example.)

After living in Candor for two and a half years, he took a job in Puerto Rico, and, together with Joseph Bensman, another sociologist, used what he had learned to write *Small Town..* The book, which referred to Candor by the pseudonym "Springdale" and



read like a Sinclair Lewis novel, exposed the political machinations of a clique of Springdale businessmen who ran the town behind its facade of folksy democracy. Springdale was supposed to be proudly self-reliant and scornful of urban ways, but Vidich and Bensman pointed out that the town relied heavily on federal and state intervention and was pervaded by mass culture. As they elaborated Springdale's political and social structure, Vidich and Bensman described specific townspeople and the roles they played. Although the sociologists did not use anyone's real name, it was clear to everyone in Candor who these figures were. The book became a local best-seller, à la *Peyton Place*--and a source of general outrage among residents. Vidich was hanged in effigy, and the village's Fourth of July parade featured a float carrying an image of him bending over a manure spreader.

For many years afterward, sociologists, who feared that Vidich's conduct had jeopardized the field's newfound respectability, argued over whether he had done anything wrong. On the one hand, everyone in Candor knew he was the field director of a Cornell research project. On the other hand, many Candor residents might have thought (and been encouraged by Cornell to think) that the project consisted solely of the field-workers' demographic survey.

In the end, sociologists failed to resolve the ethical questions that Vidich's course of action raised. "You have to remember that things are never quite all they seem," says Jackall, a close friend of Vidich's. "Research subjects are also trying to use the research for their own agendas and aggrandizement. People simply forgot that [Vidich] was a researcher." Vidich himself remained unrepentant. In a 1964 essay (reprinted in later editions of *Small Town*), he railed against imposing ethical restraints on social scientists. "It would be dangerous for the freedom of inquiry," he wrote, "if the formalized ethics of bureaucracy prevailed or predominated in all research."

THE deception debate shook the profession a second time in 1970, and this time the fallout left permanent damage--at least in one well-regarded sociology department. That year, Laud Humphreys, an Episcopal priest-turned-sociology graduate student at Washington University in St. Louis, published Tearoom Trade, a study of homosexual encounters in men's rooms (called "tearooms" in gay slang) at public parks. To gather data for his doctoral dissertation on rest-room sex, Humphreys pretended to be gay, and assumed the role of voyeur and "watchqueen"--or lookout--for the police. He also wrote down the license-plate numbers of participants in order to obtain their names and addresses. Then he waited a year, disguised his appearance, and interviewed about fifty of the tearoom regulars at their homes (sometimes in the presence of their wives and children), on the pretext of administering a social health survey. His descriptions of this second encounter made it possible that many of the men and their families would recognize themselves once the dissertation was published as a book. Humphreys cited situation ethics--the application of rules of conduct on a case-by-case basis, a popular topic at theology schools during the late 1960s--as a justification for his *modus operandi*. The controversy over Humphreys's covert techniques ultimately spelled the end of sociology at Washington University. There was talk of revoking Humphreys's doctorate,

and one well-known member of the department, Alvin Gouldner, delivered a blow to Humphreys's head that hospitalized him overnight. As a result, Gouldner was stripped of his title, Max Weber Research Professor of Social Theory. The sociology department never recovered from the demoralization brought on by the Humphreys incident, and, in 1989, the university disbanded the program.

Alarmed by increasing reports of unethical research practices on campuses, the Department of Health, Education, and Welfare (HEW) issued a stern report on sneaky bio-medical and behavioral research in 1978. The report came in the wake of the St. Louis scandal and adverse publicity over the filmed experiments that Yale psychologist Stanley Milgram carried out between 1960 and 1963. In his most famous work, Milgram told volunteers they were participating in a learning experiment in which they would "punish" (by means of remote-control electric shocks of ever increasing voltage) students in another room who failed to match word pairs correctly. The shocks were imaginary; Milgram was actually testing the volunteers' willingness to follow orders, which many of them did punctiliously. Milgram's film of his experiments, grainy black-and-white footage aptly titled *Obedience*, depicts its unwitting subjects as analogous to Nazi concentration-camp guards. It is shown to this day in many undergraduate classrooms.

The HEW report led to federal regulations requiring all scientists who use government funds to conduct research on human beings to clear their procedures with institutional review boards or human-subjects' committees at their universities. The boards are supposed to ensure that subjects give informed consent and to approve any exceptions to this rule. (Richard Leo, for example, got permission from UC's Human Subjects' Committee for his dissertation research on police forces.) The new ASA ethics code advises sociologists seeking waivers of its informed-consent and deceptive-research guidelines to clear their projects even when they are not using federal money.

THE CREATION of human-subjects' committees and the ASA's ethics protocol may force researchers to think twice before using deceptive techniques on a project. But neither innovation addresses the bigger questions that have dogged sociologists for years: When is deception of subjects permissible in social-science fieldwork? Should it ever be?

"We do cost-benefit analyses to justify deception," says Yale's Kai Erikson. "But most often it's we who get the benefit and they who pay the cost. There have been sociologists who have gone into religious groups or Alcoholics Anonymous. We don't know how much harm it does to research subjects. There are some people who say, 'I'm doing it for the sake of science.' They're doing it for themselves. One of the things that I've noticed is that people who disguise themselves are always looking at groups less powerful than they are. If a doctor pretends to be a patient, that's all right, we say. But if a patient pretends to be a doctor, he'll get arrested."

ROBERT BALCH INFILTRATED A UFO CULT. "WE HAD TO TAKE NOTES IN BATHROOM STALLS, SO WE HAD TO GET UP EARLY AND WRITE THEM ON LITTLE SCRAPS OF PAPER," HE SAYS.

Erikson's observation clearly applies to Ellis's relationship with the generally less educated, rural Guineamen, but not all researcher-subject relationships favor the more powerful party. Before starting to work on *Wild Cowboys*, for example, Robert Jackall published *Moral Mazes* (Oxford), a 1988 study of managerial ethics at a large (and pseudonymous) chemical-manufacturing company. Jackall ran into trouble starting his research because thirty-six corporations had flatly turned down his request to study ethics on their premises. As a desperation measure, he worked with a public-relations expert to devise a project description that would sound acceptable to a CEO. Eventually, he found his way into a chemical company that encouraged him to study the effect of chlorofluorocarbon regulation on corporate practices. Jackall took a crash course in chemistry from a fellow Williams professor, and he was soon inside the corporate doors asking questions about ethics.

His findings appeared first in a 1983 article in the *Harvard Business Review* and later in his book. Jackall concluded that the main "ethic" governing managerial practice was self-interest: protecting one's derriere and furthering one's career. He also found that organizational life was indeed a maze, a thicket of never-ending status jockeying and euphemistic doublespeak. (He included a glossary of job-performance-evaluation lingo, in which "quick thinking" meant "offers plausible excuses," and "requires work-value attitudinal readjustment" meant "lazy and hardheaded.") Jackall started receiving phone calls from managers deep within the company (and other companies) congratulating him for his acuity, but the top dogs demanded to know why he had been allowed on the premises. "All the managers had to do was pull my proposal out of the file and say, 'We thought he was here to study chlorofluorocarbon regulation,'" explains Jackall, adding that what looks like deception can sometimes be part of an elaborate linguistic code in which no one is really fooled and nearly everyone is satisfied--not least because there is always someone else to blame for the researcher's unflattering revelations.

Nonetheless, Kai Erikson maintains that deception of any kind is bad for the profession. "It jeopardizes the reputation of all the rest of us when some of us sneak around," he says. "And it's also very poor research." In her 1995 essay, for example, Ellis conceded that some of her book's ribald facts about the Fishneckers' sex lives might have been tall tales. There are other, more horrifying stories of deception gone awry: sociology graduate students who checked themselves into mental hospitals or joined cults--only to discover that the people they were observing were other sociology graduate students.

After infiltrating the UFO cult that evolved into Heaven's Gate, Robert Balch, a sociology professor at the University of Montana, came to conclusions similar to Erikson's about the morality and practicality of undercover research. Ironically, Balch's concern was not about unfairly harming his subjects but about inadvertently helping them advance an

ethically dubious cause. In 1974, he became intrigued by the flying-saucer-obsessed organization, which he thought might be linked with the disappearance of twenty young people in Oregon. The following year, he and a graduate student approached members of the group as researchers with some general questions. When the cult refused to cooperate, Balch and his student spent two months posing as members, traveling with the cult from town to town in the West as it promoted its beliefs to susceptible crowds. "We were expected to do things that we didn't want to do," Balch recalls. One evening he was obliged to promote the cult to an audience of ninety people. On another occasion, he found himself talking to a couple who had driven thousands of miles looking for the UFO group. "They had a kid," Balch recalls, "and I had to tell them, 'If you join the group, you have to leave your kid behind.' That was enough to persuade them not to join--but what if they'd decided to give up the kid?"

**DESPITE TROUBLING EXPERIENCES USING
DECEPTIVE TECHNIQUES, FEW SOCIOLOGISTS
BELIEVE COVERT RESEARCH SHOULD BE BANNED.**

As undercover investigators, Balch and his student were subjected to the same unwritten rules that bound everyone else in the cult: no idle socializing (all references to one's past life were forbidden). "We had to take notes in the bathroom stalls, so we had to get up early and write them down on little scraps of paper," says Balch. "I came away with the feeling that it wasn't ethical, and it wasn't the best way to get accurate information. I wouldn't trade the experience for anything, but on every other study that I've done, I've identified myself as a sociologist."

In the end, despite troubling experiences using deceptive techniques, few sociologists believe in hard-and-fast bans on covert research. Erich Goode, a sociology professor at SUNY-Stony Brook who sat on the ASA's deceptive-research panel in 1996 with Erikson and Leo, says that the decision boils down to a trade-off: "Less-than-complete honesty versus getting the information. Do you announce up front that you're a sociologist, say, when you're studying drug dealers?" Goode believes social scientists should be free to make the trade-off at their own discretion. Accordingly, he has not sought federal funding (with its accompanying constraints) for one of his favorite covert research projects: placing bogus personal ads in order to study the sociology of mate selection. In one experiment, he placed four different ads in four different publications, two purporting to be from women seeking men and two purporting to be from men seeking women. To do this, he invented four personae: a beautiful waitress, an average-looking female lawyer, a handsome taxicab driver, and an average-looking male lawyer. One need not be a sociologist to guess the breakdown of the nearly one thousand responses, the majority from men, that Goode received (and tabulated in several scholarly articles). The beautiful waitress was the overwhelming favorite for male respondents; women preferred the average-looking male lawyer (but not by so great a margin). Originally, says Goode, "I tried to do this kind of research aboveboard. I wrote to a couple running a newsletter focusing on personal ads and explaining that I was a sociologist, but I got no reply."



GOODE'S attitude--that the knowledge gained can sometimes justify the deceitful means--may not dominate the profession today, but it represents a powerful challenge to absolutists like Kai Erikson. And it represents a faction of sociologists who are unlikely to be content with the ASA's stringent professional guidelines or with guilty, after-the-fact conversions like Carolyn Ellis's. As for Ellis, she has switched her main appointment at South Florida to communications (although she has retained a joint appointment in sociology). Her current projects fall under the rubrics of either auto-ethnography or "emotional sociology"--a brand-new subfield in which, as she describes it, the "emotionality of the researcher" plays a central role in the study.

In her recent essays, Ellis puts many of the emotional events of her life on display, including her abortion and her brother's death in the Air Florida crash of 1982. In 1995 she published her most ambitious piece of auto-ethnography to date, Final Negotiations (Temple). Nearly twice as long as *Fisher Folk*, the book is a grim, often poignant account of her tempestuous nine-year-plus relationship with Eugene Weinstein, the late chairman of the SUNY-Stony Brook sociology department. Weinstein was already dying of emphysema when Ellis met him in 1975 at a faculty party, where he passed her a toke and a kiss even though he had arrived with another woman. He had a tangled marital and romantic past (he and Ellis collaborated on an article on jealousy in open relationships). Two months after their marriage, in 1985, he died.

Ellis's book chronicles many details that might seem too tragic or intimate for other writers: LSD trips, sex with an oxygen tank in the bed, Weinstein's gradual mental decline, and his painful difficulties with elimination during his last days. Besides being fearfully ill, Weinstein was a demanding, complaining patient who could not stand to be alone. Ellis gritted her teeth and endured it--and then told it all in her book. Weinstein, she says, fully supported the project.

The sociologist who once practiced her profession by telling the secrets of people she had deceived in order to get close to them is still telling secrets. This time, however, the secrets are mostly her own or belong to those closest to her. For Ellis, auto-ethnography is a solution to the ethical quagmire surrounding deceptive research. But many sociologists are likely to find it an impractical one. Is researching oneself instead of observing others rather too high a price to pay for ethical purity?"

Charlotte Allen is a contributing editor of *Lingua Franca*. Her book, The Human Christ: The Misguided Search for the Historical Jesus, is forthcoming from The Free Press.