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# Going Out

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## INTRODUCTION

We are misfits. The most developed sociology in the world exists in a country inhospitable to it. The related U.S. traits of individualism, jingoistic arrogance, and lack of a labor party tradition combine to make sociology a suspect endeavor. There is little incentive to understand our underlying assumptions nor attend to the progress of our work. The ideas of social structure, of social facts, of situated behavior are hard sells in a land where the way to win an election, become head of the PTA, or make a living in consulting is by pushing wars on drugs and locking people up. Our national traditions support forms of making sense that run counter to ours. Absent a strong hunch that social conditions affect individual behavior, you get a blameful and dangerous society, and certainly one with little use for sociology.

The symptoms appear with a glance at the media — high or low — or down the corridors of power. Magazines like *The New Yorker* and the *New York Review* seldom review the work of sociologists, nor use us as commentators or writers. Few bookstores, even in academic environments, stock much real sociology on their shelves. While in France, a blink in a theorist's nervous system snaps neurons down the Paris boulevards, we can throw public fits and nobody much cares. Our American Sociological Association (ASA) passes resolutions on the most important problems of our time (some endlessly debated in-house), and the press ignores them. Confused with social work, socialism, or perhaps the putatively failed programs of the 1960s, our work is unknown or trivialized.

There have been no sociologists in the cabinets of any president (political scientist Moynihan was the closest call) and our current leader —

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policy wonk though he be — would be politically foolhardy to treat the credential of a Ph.D. in sociology as anything but a confirmation hearing liability. Some sociologists have taken to obscuring their disciplinary connection by calling themselves "social scientist" or using some other self-deforming subterfuge to pass. In the Scandinavian countries, to take a strong contrasting case, the policy world turns heavily on sociological concepts and the craft of empirical research connects to receptive government agencies and supportive publics. In the Czech Republic, President Havel makes speeches and writes essays rich in sociological substance, without apology. The other social sciences fare somewhat better imagewise in the United States: economics supposedly makes people rich, psychology helps with angst or at least a nicer day, and anthropology amuses with exotic tales. Our attentive public, at any level, is thin. All of this has major consequences, many of them negative, for the way we conduct ourselves. One of the things wrong with sociology, in our country, is that we need a better country — a point to which to return.

The interdependence of sociology and its society is an old story. In some sense all societies have a sociology in that there are shared ideas of social boundaries and some notions of how the group can influence the individual and roles, shape attitudes and behavior. Aristotle said, "Poverty is the parent of revolution and crime." Mme. Cornuel (1605–1694) observed "no man is a hero to his valet" and Shakespeare not only wrote that "all the world's a stage . . ." but put grist for the sociological mill across page after page. Sociological homilies come from across the globe, all elements of folk sociologies — micro and macro — some as sharp as tacks. The popular circulation of these proto-sociologies affected, more or less, the societies they purported to describe, just as those social conditions shaped the "work" of the analysts. Sociology and society are deeply "in" one another, mutual in their determination. Society and its sociology, of whatever form, are not two "things," but an evolving process of knowledge, action, and substance.

In the modern context, the sociology–society process has speeded up, as description and analysis more rapidly feed back into the social order from which they come. That professional sociology is a part, however small, of this two-way enterprise is one of the reasons why it (or any of the social sciences, as Stephen Toulmin has pointed out) can never be normal science. If we report divorce rates are up, divorce rates may go down as folks and government become concerned about "the family." Or the same information may cause divorce rates to rise still higher because, after all, "everyone is doing it." Survey results alter the behavior of those polled — and not just the sample but the universe as well. The Heisenberg principle is radically in play; our very talk alters the object under our microscope. Our

work is never done, not just because there are always new realms to conquer and "history" marches on; our work is never done because it self-destructs, if it is any good. If we learn new things and communicate them quickly and effectively they will have a chance of entering other discourses and lead to the changes that undermine their adequacy. Our work needs to be good enough to become untrue.

Rather than capturing a fixed reality, the goal of a substantive sociology necessarily becomes participation through description-alteration. We are professional structurators, paid to observe, study, report, and even if only inadvertently, change the context that changes us. This is just what most everyone else does too, but we do it as occupation rather than as byproduct of other activities. What a deal; what a privilege!

Sociology's initial rise after World War II as another positivist endeavor was part of the heady American optimism that a "science of man" could solve our social problems just as a science of matter could solve our defense and health problems. That was our doing and our undoing. Sociology, of course, could never deliver the goods, something most everyone now knows. Rather than lament that we fail by the lights of science, or much worse, play at being one, we should competently embrace the creative task at hand: buzz around like gadflies, gathering up bits of news from here and there; develop systematic techniques for looking at one thing intensively or many things extensively. Putting aside scientific magic, we need to be the shamans who act as clearinghouse for what everyone is up to and how it is turning out. Many feel parts of the elephant, together we feel more of it. We are society's kibitzers, moving from table to table, watching the hands in play. By ourselves not having a hand to play, we have the time and inclination to reflect on how the game itself is conducted. This is not "mere journalism," but deep journalism.

Some others will be just as good as we at the task and indeed more than a few of the most important contributors to "our" field are free-lance, people like Betty Friedan, Jane Jacobs, and Harry Braverman — individuals we need to celebrate in our circle and in the disposition of our awards. We will never have a monopoly, but we can aspire to be reliable and useful. Timing is of the essence or our message will come when everyone knows it or — what may be the same thing — when it is too late to do any good. We needed (and eventually got) a good notion of cultural lag before nuclear Armageddon, not after; we needed (and eventually got) data on the social networks of AIDS transmission to save millions from the fatal theory that the disease spread through gay "lifestyle" rather than a virus (Silence = Death, and so does No Data).

World events have given new life to the sociological project, even if sociologists only play small roles in them. While clever entrepreneurs are

celebrated, markets are understood to thrive through state intervention and productivity to turn on specific kinds of "corporate culture." Increasing recognition within the economics discipline, including the Nobel Prize, goes for work that demonstrates the sociological elements of markets — like transaction costs, neighborhood effects, and organizational behavior. Rather than "sociological factors," the economists call them "market failures." But a sociological framing is their key, no matter what they call it. The country's social problems do not go away with policies of revenge. By listening to the sociologists the nation could learn why its antidrug repression will not work, why more cops will not stop crime, or why tax breaks for industries will not improve communities or build the commonwealth. The irony, once again, is that our professional fate, and the use of our learning, is strongly influenced not just by what we know or how much the problems of the day require our skills, but by ideological currents that discourage an explicitly sociological approach. We really have to change America to change us.

No small task, as I am not the first to indicate. But we can, in this *Forum,* constructively address how elements of our current sociological practice are a response to these conditions and how they function *vis-à-vis* such a grandiose goal. In other words, we can start by understanding how, as misfits, we are led into practices that do not help our cause of building discipline and country. We are caught within a classic sociological tension that is a recipe for deviant outcomes: a disparity between aspirations and opportunities. It is stressful and dysfunctional. More than most professionals, we got into this business not just to make a living, but to make a mark — a contribution to the greater good. Frustrated with our lot, some retreat, some innovate; many get mad or rebel.

Suffering does not always make people pretty; operating from an oppositional stance may not encourage the most effective practices. I take up some of the traits within sociology that I think come from living in a wrong country. It is not our fault but only we can lift our own bootstraps. Avoiding the pitfalls that follow may make the process less gawky.

## TOTALISTIC CLAIMS

Although our journals are full of pro forma caveats and humble pie modifiers, when it comes to the role of sociology, we are megalomanic. Even Comte's designation of sociology as "queen" of the sciences pales next to our assertiveness. We hurl the word "psychological" as damning epithet for a colleague's work. Any mention of "personality" is a "reduc-

tion," or in the more loaded lingo of today, "essentialist." These are easy scores. At the other social science border, economists provide an alternate negative marker of crimes against humanity — an argument that turns on ordinary market behavior (like buying at the lowest price) is obviously backward because it does not consider the social relations of production or the embedded quality of economy in society. Biological difference is clearly beyond the pale.

Our goal often becomes not to solve the problem, but rather to prove that only sociology can solve the problem. Even when we allow that sociology "is one of the important factors," we do not show how the other factors might count in light of the sociological contribution. Instead, we commit sociology and run. As I heard Erik Wright say at the 1993 Miami meetings, the real scientists do not act that way, or at least not so brazenly. I have watched marine biologists, for example, greet their counterparts in geology and chemistry with welcome arms. While competitive with other research *teams,* they understand they need other disciplines to solve their problem. Not us. As a result, we learn less from other disciplines and those in other disciplines learn less from us. This is one source of our general difficulty: since no one but the sociologists are interested in our Advance-the-Discipline agenda, outsiders lose interest or find us ridiculous.

It would be better if we could relax a bit into a multidisciplinary imagination and grant psychology, the market, and even biology their due. This does not mean, for one moment, giving up the importance of sociology or accepting the imperialistic forays into our field by the sociobiologists or Beckerite vandals from economics. We need to keep our sociological imagination and our pride. But we can enhance the standing, the utility, and even the thrill of sociology by understanding how the social intersects with other aspects of human systems. It would make the world come alive to confront it in its full complexity rather than worrying — as we look at a problem — whether our answer will be good for sociology.

Like ethnic studies, women's studies, and gay studies, sociology has perhaps needed the breathing space to construct its own agenda and solidarities, insulated from the depredations of the more established fields. Even now the other disciplines may devour us if we allow their nose under our tent. The Chicago economists proclaim their "imperialism" with the kind of crude gusto that their tall standing in the hierarchy makes dangerous. Risking for psychology may invite the crowds to charge past us for the individualistic explanations they seem to love. Calm and gracious sharing of the stage does not come easy to those struggling for bit parts. But moving forward as *though* we were self-confident would move us closer to the kind of assertive stance that would make us more effective.

## LITERATURE DISEASE

Among the secondary adjustments we have made to our plight is retreat into our own paraphernalia, including the consecration of our literatures into the stuff of science. The best retreat is always an invisible one and our literature cocoons keep many of us safely out of view. The Literature becomes topic *sui generis;* "progress" in refining, testing, elaborating what the last journal article refined, tested, or elaborated becomes a sign of salvation, or at least tenure. In too much of our writing, author's names perform the role of narrative tension: Jones was wrong but Schwartz is right, rather than education counts more than father's income. The plot line revolves around which scholar holds the trump, rather than which version of the world holds the wisdom. This allows underlying substance to be lost; after a while, somebody has to ask "what was that we were talking about?"

Journal referees act as insatiable citation cops — demanding more, more, more ("feed me, feed me") and never saying there are too many. As with other cops they sometimes go bad, corrupted by their desire to make sure their own names or those of their pals are among the references. Seldom do they have a sense that many different bodies of thought could be the relevant literature, and that their own version is idiosyncratic to a specific intellectual-biographic niche. The author has the challenge to cater to the citation world of the reviewers they draw, and hope a revise and resubmit will not switch the manuscript into still other citation firmaments.

Emphasis on citations rather than substance pushes the literature itself into ever more abstract and formal realms. With successive iterations through the journals, veins of scholarship lose whatever practical or theoretical thrust they might once have had. Besides Type I and Type II error, there is Type III error: nobody cares about the null hypothesis anyway. No matter how quantitative or rigorous the presentation, work can cease to be empirical at all, taking the word "empirical" back to its root (as psychologist David Bakan once proposed) to mean "experience." Empirical knowledge relies on direct observation of the real world, as opposed to armchair reflection, intuition, or logical deduction. A clean observation of two people eating dinner is surely more empirical than indexes of affinity, dependence, or prestige run as regressions with dozens of other variables — some surrogate measures for still other measures — dubiously scaled and weighted. With disengagement from the real world, but armed with statistical machinery (however bogus its application sometimes may be), the literature is undisciplined by ordinary human sense making.

The justification for citations and related sounds of science is a cumulative knowledge base in which one's own work could not have been done

without the prior labors of specific, namable others. But our citations often do not indicate the work upon which we relied; they show we "know the literature." Rather than display the route to our findings, they pay homage. I often cite people who other people think *should* have been important to my work, but in fact I had never read. And some free-lancers who have made powerful contributions probably did not even know we had literatures.

That we can do our work without using others' conceptions of the literature we "need" signals how idiosyncratic our routes to knowledge actually are. Despite the existence of ASA sections, named "specialties," abstracting services, key word searches, and consortia data storage, we cannot find what we need in any mechanistic way — even when we try. Fields are not distinct; findings in one specialty drift into another, and reshuffle in light of new knowledge or fashion, perhaps disappearing for a decade only to be later rediscovered. Key words that can be used in searches do not correspond to any agreed upon system of notation or hierarchy. It is hard to be cumulative if there is really no practical way to know what has been done before or what the trajectories of development have been.

Even some of the easiest challenges do not get met. I offer the career of one of my own little lost findings as a case illustration, chosen because it falls so squarely within a well-worked empirical tradition and because I have no particular passion about it (I swear). I learned, based on a case study, that neighborhood racial segregation is not due to white flight. This kind of thing is important to the human ecology types as well as for urban and racial studies. It was reported 25 years ago in one of the most mainstream, abstracted, indexed, assigned, and reprinted sources, as well as in a book I wrote on racial change. More methodologically sophisticated scholars replicated the finding on large data sets — again well reported. But not only has this finding made no dent on the popular view, even the most important scholars in the field ignore it, because they do not know it. I bring this up not out of sour grapes (maybe just a tad), but because it is the kind of finding with which our system should have been best able to deal. That it does not get in shows the crudeness of our retrieval system and the extraordinary noise — indeed cacophony — that surrounds any one element in what we "know."

While we cannot retrieve an ordinary finding, the science model encourages us nevertheless to mark each published observation with support from authority and past studies. We sort of spray on citations like the white stuff that makes snow on indoor Christmas trees. Thus our literature is full of statements that begin "As X has shown . . ." or "as has been demonstrated by . . . ." Tracing back, one too often finds only an assertion in the first place, or the results of a casual empirical inquiry — but cited like it was from Blau and Duncan, Incorporated. More status anxious than even

their mentors, graduate students create their own citation mayhems, beginning dissertations with a summary of the literature, moving through a review of "findings" — as though back in chemlab happily solving for unknowns. Whether in theses or journals, our system limits ways to read (or portray) the difference between vague assertion and well-reinforced documentation. Our adoption of the scientific notation system (Author, year), as opposed to substantive footnotes, communicates verification like a kosher stamp on a piece of meat, with no mister-in-between. The standing ban on use of first-person voice, on saying "I think but I'm not sure" or the use of similarly human qualifiers, promotes spiraling scientistic overstatement.

All these findings do cumulate, but as debris on the shop floor. We trip over our own stuff because, just as our departments have few ways to fail graduate students, the discipline has few ways to purge findings — either because they are wrong, or more importantly, because they have been replaced by more evolved formulations. The end of a line of sociological work does not result in a punch line ("it is a virus"), settling a string of inquiries and thus embalming them as fixed on the shelf and out of the way. That would allow researchers to go on to their next stage (e.g., identifying the virus) without having to deal with an army of genies that keep coming out of the old bottles. But for us so little gets settled, even as practical consensus: we can learn, 60 or so years after Durkheim, that we were working with an "over socialized conception of man" or that, with Marx dead even longer, it was time to "let the state back in." In a few more years, if it has not happened already, Durkheim will be back in and the State will be out.

Symptoms of the same awkward incoherence play out at the undergraduate teaching level, where departments generate curricula and systems of prerequisites that presume "building blocks" and interchangeablility from one sociology department to another. But apart from certain subfields (like statistics), such assumptions are illusory. Courses in, say "urban," are totally different if the approach is political-economy vs. demography-ecology vs. community studies. "Stratification" can build on the Blau–Duncan mobility approach, a Marxian–Braverman sociology of work orientation, or a world-scale comparative and/or feminist outlook. The sociology of culture can be akin to literary studies and discourse analysis, or can treat culture as an industry, or as a problem in formal organization. These are not different spins on the same topic, but as different from one another as each is from courses with completely different titles. So how can there be hierarchical curricula within a department — much less across departments — when what a student knows by taking the course depends not on its name but on who taught it? The curricula quandary reflects the essential nature of the discipline, not the need for another series of committee meetings to shape a shared conformity — that is a cult, not a science.

The real divisions in the discipline are based in cultural and biographic commonalties: where people went to school, their politics, and general intellectual sensibilities. So many people today can be "into culture," just as so many in the past were "urban" or "critical," because the terms delimit so little — mostly a mood shift and the willingness to put up four bucks for section dues. If we organized ourselves as cultures, we would do better — e.g., "Sociologists Active in the Sixties 'n' Such" or *The Journal of Wisconsin Quantoids* (as opposed to *American Sociological Review*). Naming this way might really unite people who approach research and teaching in a similar manner and link those who need each other to advance their work. It would look bad for the gentiles, but it would capture something real.

## WRITING MOUNT EVEREST

Howard Becker, in his "how to write" book for social scientists, gives many good reasons for clear exposition. It makes our work accessible to colleagues, students, and outsiders; it has a better chance to influence the world. The advice is simple: use small and easy words and first person. He doesn't say to write "in a more professional tone" — advice I have seen from a journal editor.

Becker is not an enforcer, so let it be me who says that the profession needs to guard itself and its students against obscurantist and difficult writing. Precisely because the boundaries of our various literatures are so indistinct and because we must read what our subjects read to understand their world, we have to read a lot of stuff. Its a zero-sum game out there. I greatly admire Harold Garfinkel, but he cost me. If it takes seven hours to read an article by scholar X, we may never read three articles by other scholars (or learn something in a bar). It may be that X is wonderful, but is X three times as wonderful? Parsons was very smart, but was he 10 times smarter than Mills? or Goffman? or Merton? Maybe "yes" and maybe "no." It is a question to ask.

It is not just that we need clear writing so that we can better express knowledge; the accomplishment of clarity *is* the accomplishment of knowledge. Even if something is already "known" in some sense, finding the words that concisely formulate it and make it memorable adds to the stock of what can be manipulated in the mind and made part of working sociology and of the culture. Notions like Merton's "self-fulfilling prophecy" or Giddens' "structuration" were, in some sense, already there — but making the underlying thought available advances what we actually "know." "Problems with no name," to borrow from Betty Friedan, are known in a radically different way "merely" through naming them (sort of "perfoma-

tives," in the linguistic lingo: utterances that accomplish something just by saying them).

Sometimes this process results in specialized terms and a distinctive conceptual apparatus. I am not preaching a know-nothing ridicule of scholarship, in the manner of Senator Proxmire. Some very worthwhile sociology can appear, especially to the uninformed, trivial and empty. Two very different fields in which I am not a practitioner but a user — demography and conversation analysis — come closer to the model of cumulative science than most others in sociology, and they utilize an arcane set of terms and methods, some of which are necessary. They make their gains through papers with titles that can seem ridiculous. The urgent goal for sociologists (and the outside world) is to know the difference between specialized language entailed by an authentic problem, rather than language entailed by a retreat from one.

To help others make the distinction, we can begin by not treating "hard to read" as benign challenge, like climbing Mount Everest, with honor going to those with the temerity to take it on. We should not defer to exotic creatures who create Mount Everests of needless jargon and complex sentence structures that others will have to scale. It does not build our character, it only takes our time and tests our patience. The gatekeepers should treat bad writing as a threat; it better be terrific to be let through. The guardians of journals, yours truly included, need extra gumption to warn against a paper, that while full of citations and other marks of the trade, may be using bad writing to obscure the fact that it has no important problem at its core. Like others, I suspect, I do my peer reviewing in off-hours (usually at night when I am half dead). How can I be certain that it is not fatigue that keeps me from "getting it" rather than the weakness of what is before me? How can I urge rejection for a paper that drugs with such rich professional paraphernalia? To these pushers of such downers I must — we all must — just say "no."

## STAYING INSIDE

In explaining the arcane writing style of professors, the historian Patricia Limerick says the key is that academics are the people nobody would dance with in high school (or choose-up early for gym class ball teams, I would add to her remembrance). Nobody, the therapists will tell you, gets over that so easy. Limerick may be wrong in her blanket characterization (what is her sample?), but it is certainly less threatening to come out only under full protection. For professors, that means the classroom and scholarly meetings, where vulnerability to real life can be kept at a minimum.

For experimental physicists this probably does no harm and indeed protects against skin cancer. For sociology, it is not good.

My first image of sociology was through the writing of C. Wright Mills, whom I also imagined as an album cover. He merged with Jack Kerouac, Lenny Bruce, and Henry Miller in my mind; they were all heroes who knew the world through its edges — deviant, strident, and/or dirty mouthed. I figured all sociologists were something like that, all the names between the parentheses in the journals I gobbled up. The only way to know the society that surrounds us is by understanding its margins, it seemed to me. But that means going outside: the taxi-dance hall, the housing projects, the protest march, the youth gang, and the dark places that most of us know only as haunting hints of the possible.

Staying in conventional settings (or as properly adjusted into an academic lifestyle of spicy food and ethnic clothing accessories) has substantive implications. Gouldner warned against the "congenial sentiment" through which comfortable sociologists come to see the world: tenured with families and health care coverage — not too rich but not poor either. It is not just that they aren't Kerouacs, they are not Louis Wirths or Herbert Gans, or others who can sustain a pattern of taking on even the ordinary outside settings. Sociologists often know no world outside their own academic and family daily round; they do not hang around commodity trading floors, or holy roller churches, or exclusive golf clubs. Committee meetings, teaching loads, peer reviews, and writing essays like this are the occupation, leaving little space for walking through the world. Going outside means "for extramural funding," not for extraordinary experience. Russell Jacoby blames the university system for coopting the "public intellectual," enervating the country's civic life, and he devotes several pages of his important book to the specific case of sociology.

My point is more narrowly methodological. The sociologists' taken-for-granted world, like everyone else's, is partial. That is why it is important what that taken-for-granted world might be. Our ranks, more so than the other disciplines, must be diverse — not just to satisfy affirmative action requirements or moral urgings, but to have common sense that encompasses a broader range of senses. Our point of departure for a study comes overwhelmingly from our own lives. Having only a thin slice of human experience weakens the sociologists' crucial first order of data, the thickness of one's own biography. If what the sociologists know best is what is also commonly known to the rest of the educated public, they are naive to all other settings — both substantively and methodologically — tabula rosa all over again. We are not ahead of the game, as we are paid to be.

Our lives tell us where the evidence is needed. Every assertion, indeed every word, of a written research report potentially "needs" evidence. The

most thoroughly documented article contains infinitely more unsupported assumptions than it is possible to back up. The action turns on which assertions lack the "ring of truth" — the vastly important arbiter of what can pass, unexamined, through the gate of sociological custom. The looseness of our subfields and ambiguity of which literatures ought to be cited for what makes for a murky situation. Given that the more ambiguous the stimulus, the more that social factors intervene in perception (there is a lot of citation for that), the stage is set for the subjective folk knowledge of the gatekeeper to operate — invisibly and hence irresponsibly — as the basis for discriminating. This again brings up the conventionalized yet idiosyncratic "literatures" that are invoked, in part as rationales, for determining which little items need the science regalia and which we can, as Garfinkel says, simply "let pass" so that there can be literatures at all.

So now we are back to the biographical ranch. Unless sociologists really do live rich lives, engage the world, or at least read widely, their research and writing are emaciated by misplaced demands for evidence — misplaced compared to what other folks further along the learning curve would demand. Without a broad collective sensibility, those who do have distinctive knowledge are held back by those who do not. Not knowing what "everybody" knows in the world of a specific study (on the streets, in the Army, at the boardroom), the gatekeeper turns into a judgmental dope who cannot distinguish between what needs documenting compared to what should be self-evident. Does an assertion that big cities are rife with corruption require, in the halls of social science, "evidence" before it can be used in formulating an argument? Does corruption simply pass, or is the baseline one of civics course "democracy?" Do we presume that campaign contributions fix politicians or is the reverse the working common sense? Or are we supposed to have no brain whatever until the data comes in, and comes in on everything?

Just as we cannot rely on the literature of sociology to equip us up for knowing what to doubt, we cannot use the adages of political correctness to do the job — no matter how strongly they take hold among progressive academics. They are as conventionalized (and detached) in their way as the table of contents of Sociology 1 textbooks. People in real life know, even if its hard to acknowledge, that when it comes to sex and romance, the young have power over the old, and that women *sometimes* indeed say "no" when they mean "yes" — just to bite a few harsh bullets. It is all part of the dance that makes life tense and interesting and, yes, sometimes criminally brutal. Sociologists disarm common sense knowledge of the embedded idiom of daily life to assert, as principle, that matters are more simple.

Again, the antidote is going out — not as leader but earnest listener who stays awhile. The academic calendar and routine push us to draw more and more of our cultural knowledge from within our workplace. Many of

us blow our sabbaticals by writing up what we have been doing, rather than immersing ourselves in new worlds. The anthropologists have a richer tradition of going into the field with body and soul. They used to be helped by an absence of phones and the presence of an ocean surrounding their little islands. For us, and increasingly for them, the temptations of E-mail and good fax conspire to keep things normal. Sociology would be better off if we, for sustained periods, could get out of it.

## STAYING SHY

Many sociologists are shy. Some of my best colleagues and my best parts are shy. It is not a sin; shy people make important contributions to sociology. But it takes nerve to hang out in a strange fieldwork setting (one's own therapy group, housing coop, or political movement is at least a start). It is moxie to interview people (arrogance, we are now told), especially ones who lead different lives than we, whether because they lack the marks of middle-class identity or have the trappings of power and wealth. Sociology does not select for fieldwork verve: I have rarely seen it mentioned as criterion for admitting a graduate student, hiring an assistant professor, or granting tenure. Most people who do good interviews are people for whom it is an easy social maneuver; they go confidently forward and their subjects love to tell them what is going on. Being able to go with ease is important to our knowledge base and should count. Instead, personality is prized more for what it can do within the department (as friendly colleague) than for what it can mean for scholarship. A certain pushiness, perhaps even crudeness, can — depending upon the research setting — make for effective work. But we use the same basis for judging personnel as in any other field, adjusted a bit for the moral stance and interactive style we mostly share. Colleagues reproduce the "congenial sentiment" that Gouldner (who did not always suffer from this ailment) warned us against.

That many of us are shy is another one of the dirty little secrets in sociology, somewhat obscured by our routine contentiousness and assertiveness in hotel lobbies. All of us were not meant for public roaming, but more of us could be made ready with a little help from our friends for interaction beyond the department and the Hilton. Perhaps even a bit of training that recognized shyness as a life-long challenge would be in order. No matter what kind of sociology we do, whether number crunching or comparative historical, there is nothing like varied life to launch ideas, facilitate interpretations, and help us speak well to others. We need to lessen our shyness if we are to get our act together and take it on the road.

## METHODS

### One Size Fits All

While the science model, crudely applied, holds back all varieties of sociology, it is especially deleterious for qualitative sociology. It takes away the very advantages that such methods permit: the ability to change one's mind, learn as it goes, turn on a dime. Design and its accouterments (hypotheses, operationalizations, etc.) counts with survey research and small groups experiments because they involve so much up-front investment. The wrong wording on a question that will be asked 300 people, failure to include what later turns out to be an important variable — these are costly errors. Funding agencies and graduate mentors are wise to ask for a lot of advance specification.

But the same idea carried into qualitative sociology — where research involves few sunk costs — undermines empirical accomplishment. In qualitative work, design is disaster: the researcher will not notice the serendipity bouncing all around and stay the course through a field experience that shifts with new possibilities. The National Science Foundation (NSF) says it wants to support qualitative work, but insists on the instrumentation of sunk-cost sociology in the proposal (hypotheses, operationalizations, falsifiables). If they can get that kind of stuff, they can accept the lack of hard numbers they associate with real science. In effect, they want the positivist method laid on "bad data." Because qualitative data is not really appreciated, the research process that would best lead to it is also not appreciated.

The humanities have a different way of forecasting good outcomes. In the arts, the way you get a gig, a grant, a professorship is to "do a little" of it; show your art by opening your portfolio, dancing across the stage, thinking a bit on paper. Grant applications in the humanities read like bits of finished articles and books; applicants provide a sampling of the quality of their mind by what they write. The granting agency says, in effect, we want more of this. But in sociology, the weight is on how one states what will be done, not on displaying an instance of doing it. Social science juries have a strong tendency — and I have felt it when serving — to reward the craft of proposal writing itself. By convention, the best qualitative proposals simulate the quantitative research method. But good simulation (deception?) is not an adequate predictor of a quality outcome, and certainly does not display *the kind of dedicated intellectual honesty that good fieldwork does require.*

The meaninglessness of much quantitative sociology has been well vetted, including by its most distinguished contributors (Stanley Lieberson, for example, and most demanding of all, Otis Dudley Duncan). Number soci-

ologists may screw up their own thing by the way they mimic science, but they also damage, almost as side effect, the rest of sociology. Their influence over the main journals, prestige systems, and foundation grants deforms qualitative work by forcing it into the same procrustean bed. Qualitative sociologists imitate the imitators, yielding a version of faux science that goes one step farther along the road of caricature.

## SOCIOLOGISTS EAT EACH OTHER

Sociologists formerly at NSF have told me that sociology has trouble justifying higher funding levels because the higher-ups perceive our proposals as "low quality" compared to the other disciplines. The NSF folks apparently count up the numerical scores from peer reviews in each discipline to measure overall quality. Because the sociology scores are worse than others, sociology proposals are seen as weak. But those of us on the scene are right to suspect that these relative scores probably measure contentiousness. Sociologists — critics by disposition and occupation — freely take issue with one another, often ungenerously. If you combine this with the science-rigor criteria, add in the reality of ambiguity and diversity, and spice with the personal frustrations of thwarted ambitions, you have a recipe for peer cannibalism.

Qualitative workers have enough trouble to give one another, without the added liability of holding up the science canon as test mechanism (usually a science hit man will be on the job anyway). Add in the strong moral and political concerns of many qualitative sociologists, and the threshold of acceptability becomes difficult to meet indeed.

## REACTION FORMATION

### Interventions of the Avant Garde

Some sociologists, not entering the science mimicry business, take their frustration into other modes of expression, rebelling with all heart. This takes a number of forms. One is the aversion to numbers—since science sociology, real and ersatz, tends to come with numbers—numbers no good. Some scholars — and especially some graduate students — get caught up in this number-hating game. Positivism sucks and so does counting up anything. Silly.

Others in rebellion, push their politics — not as a method for changing the world or as heuristic device for sensing a problem and developing

evidence — but as template for judging the outcome of any data manipulation, concept, or turn of phrase. Such sociologists overstate conclusions and patrol the discourses; they inhibit the open empirical or theoretic wondering that really could be a ticket to effective political action. Although their language is rich in critique, they too are often lost in their own discourse, reluctant to go out. They have trouble actually observing in the field, whether among the oppressed, in the demimonde, or — most unfortunately — the settings of deep power where privilege and wealth are actually centered. Few, from my experience, engage in off-campus political struggle where a natural laboratory of applied social change never closes. The social movements of the last decades and the sociological movements that have been both their cause and consequence have done an enormous amount to invigorate the discipline (feminist studies, gay studies, race and ethnic analysis). But the payoff for sociology and society can only continue with clear talk and, even when the results are disquieting, analytic openness.

Alternative epistemologies, such as versions of poststructuralism, urge new voice for the oppressed and encourage self-reflexivity that makes the scholars' role as observer an important topic of investigation. In embracing such doctrines, I believe, the sociologists are not leaving sociology, but buying into it via the sociological insights that have come into literary theory in the last decades. Many of these ideas have been around for quite awhile in nonmainstream versions of sociological practice — invited by Mannheim's sociology of knowledge and in the whole social construction school and ethnomethodology (where reflexivity, indexicality, intersubjectivity, have long been topic). The contemporary proponents seem unaware of this heritage, its richness as well as how it handled (or could not handle) some of the blind alleys (problems of solipsism and infinite regress) that stymie the current thinkers.

Discourse analysis does usefully invite attention to the linkages among different expositional forms: talk, writing, and physical objects of architecture, clothing, furniture, and the body — all treated as interwoven texts. But practitioners toss out babies with bath water. The inanimate texts (writings and physical objects) are rightly seen as fateful means of communication, "liminal" in what they "give off," as Erving Goffman might have said. But in the zeal to see "life" in what used to be treated as passive detritus (fetishes, for Marx), the new scholarship gives short shrift to the humans who make these products and who *manage* to interact with and through them. "Agency" to be sure is featured, but through assertion, rather than precise study. If the sociologist is going to add anything to what the literature people already do, it is through empirical study of just how humans operate as the thinking, behaving, interacting intermediaries that link texts to one another and give them their power in behavior. We might even

learn a lot from market research, a field we helped create, but which — in certain respects — has passed us by in determining how social consumption of goods and texts actually works.

We need to learn, in general, just how all the various modes of expressivity, animate and inanimate, manage to cohere; how a message, an artifact, a hegemon operate through their social settings. The temptation to stay inside often triumphs under the rationale (unstated to be sure) that since there is life in the inanimate, we can just "address" or "interrogate" it. Today's pioneers of a new professional orthodoxy, for all their self-conscious marginality and solidarity with the downtrodden, recapitulate the esoteric stance of their most inaccessible predecessors. In addition to the newer terms (simulacrum, liminal, subaltern), specialized use comes to old terms, like Other. Each scholar cleverly dis-members ordinary words to portray their etymology (at least plausible etymology), thereby dis-playing nefarious linguistic groundings of current social practices. Given that it is hard enough to make oneself understood when just working garden variety prose, these dashing little tripwires make comprehending any sentence a difficult move. When the obstreperous sociologists of the 1960s denounced their elders, whether in print or in person, it was ugly and sometimes wrong, but at least they could be understood. Today, it is more like a premise for a Monty Python skit.

The insight that gives rise to much of this twisting in empty space, the fundamental reflexivity of all human accounts, must be honored in a different way. Reflexivity must concern itself not just with a tortured self-examination of the privilege of the observer compared to the observed, but of the more general system bias imposed by not going out. Universities valorize something other than simple privilege; they represent a specific cultural niche that exists in contradistinction to both rich and poor, and most elements of the middle class. Those on our valued avant garde must not churn fundamental insights of phenomenology, communication theory, and literary criticism into a solipsistic nightmare that alienates sociology both from empirical reality and any potential public.

## SOLUTION

The diversity of sociology should be respected, not just through lip service but through the discipline's judgmental infrastructure. We need to retreat from the idea of professional literatures that adhere, just so, to a given project and adopt a more open sense of making use of whatever can advance an interesting and informing narrative. Writing "in a professional manner" should not consist of a particular style — such as associated with

the traditional American Sociological Review format. Different kinds of sociology should read and sound differently. If we could climb out of the science trap, we could acknowledge different modes and write in many voices. Indeed, the very computer that so helped sociology's science mania can now be used to express textures of truth, beyond the mechanics of validity tests. Fonts, formatting, and character size invite new manipulation of text to connote differing degrees of authorial certitude, evidence base, alternative means of knowing, and varying emotional sensibilities that correspond to specific words, sentences, and paragraphs. This is a way to nuance our writing with the kind of interactional thickness present in face-to-face talk, whether in everyday life or at conference hobnobbing. Whether through the computer or use of the first-person voice, this is a kind of mimicry worth striving for.

Striving is what it is all about. I have been guilty, increasingly over the years, of most all the sins and find a need to urge myself toward clear disclosure. The struggle is also social and political, given that our professional stresses come, in part, from the kind of country in which we live and the folk theories of human behavior that capture its peoples. It is never clear how one can simultaneously shape self and society when both are in reflexive relation to one another. But that is the way all change works and is as much the condition of sociological work as any other. There is a saying that if what we change does not change us, we are playing in a sandbox. How profound an anticipation of late modern insights into the construction of reality. To seriously engage in such efforts frightens precisely because success comes back, like karma, as part of one's being.

There are simple principles with which to start. There was another slogan I used to go by a lot, in part because it made me so fashionably obnoxious: "Speak truth to power." Increasingly I realize that can only work if we can speak *clearly* to *anybody* and about matters important on their face, or for which we can at least lay a trail of relevance. I am not speaking here only of Beckers' writing principles, but of the self-knowing clarity through which direct truth can be told, and the "scared employees" (Mills' term) among and within us be released from their chains.

The risks are those of self-revelation, of acknowledging the role of other disciplines, other parts of our own, as well as of the social movements and folk wisdoms that have shaped almost all our agendas, personal and scholarly. But it is the way to win respect from those who want what we really have to offer as opposed to what we can manage to concoct. Isn't that always both the risk and reward of integrity? Dangerous and thrilling, we need to think about those who will like us for what we are and, once again, ask them to dance.

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