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Howard S. Becker

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that Stanford and Michigan and Chicago became "eastern" schools). Our professional colleagues asked us the same question when we compounded the sin by going on to study undergraduate student culture at the same institution.

Our sampling choice offended an uninspected credo which held that, when you studied one of the major social institutions, you studied a really "good" one so that you could see what made it good. That would make it possible for other institutions of that type to adopt the good practices you had detected, and that would raise the standard of that segment of the organizational world. Such an approach rested on several untested and not particularly believable presumptions. To take just one, such an approach assumed that the supposed difference in quality really existed. No one had demonstrated such a difference, and one major study (Petersen et al. 1956) had shown that it didn't much matter where doctors went to school, because after five years the main determinant of the quality of medical practice (defined as practicing the way medical schools taught you to) was where you were then practicing, not where you had gone to school. If you practiced in a big city hospital, especially one affiliated with a medical school, where a million people looked over your shoulder as you worked, you got a pretty high score on the quality scale. If you practiced alone, in a rural setting, where no one knew what you were doing, your score dropped steeply.

All these reasons lead to people studying a small part of the total range of practices and behaviors Hughes had insisted was our business. Social scientists tended to study successful social movements, the best colleges and hospitals, the most profitable businesses. They might also study spectacular failures, from which of course there is much to learn. But such a sampling strategy means that they pretty much ignored all the organizations that were thought to be so-so, medium, nothing special. And remember that the so-so quality is reputational. So generalizations meant to describe all the organizations of a society have rested on the study of a nonrandomly selected few, with the result that sociology suffered from a huge sampling bias. As Hughes ([1971] 1984, 53) remarked: "We need to give full and comparative attention to the not-yets, the didn't quite-make-its, the not quite respectable, the unremarked and the openly 'anti' goings-on in our society."

To say that we should pay attention to all these marginal cases is by no means a plea for random sampling. I've already suggested that we ought to

deliberately seek out extreme cases that are most likely to upset our ideas and predictions. But we ought to choose them for our reasons, not because other people think they are something special.

"NOTHING'S HAPPENING"

A typical obstacle to finding the odd case arises out of our belief that some situation is "not interesting," contains nothing worth looking into, is dull, boring, and theoretically barren. Though the following example comes from my experiences doing a documentary photographic project, the general point applies to all sorts of social science problems, as I will later make clear.

Some years ago I started photographing the Rock Medicine unit of the Haight-Ashbury Free Clinic in San Francisco, as they attended to the medical needs of people who came to the big outdoor rock concerts impresario Bill Graham put on at the Oakland Coliseum. I knew that what I photographed was what I found interesting, not a function of the intrinsic interest of events and people but rather of my ability to find a reason to be interested in them. Everything could be interesting, was interesting, if I could just get myself interested in it.

But after attending a number of these events (which went on from nine or ten in the morning until well after dark) with the Clinic team, which numbered as many as 125 volunteers (a few doctors and nurses, but mostly civilians), I found myself getting bored. I couldn't find anything to photograph. I felt that I had photographed every single thing that could possibly happen, that nothing interesting was going on most of the time. My finger wouldn't press the shutter button any more.

I finally realized I was picking up and accepting as my own a feeling common among the volunteers of the Rock Medicine unit. They knew what was interesting: something medically serious, maybe even life-threatening. They got excited and felt that "something was happening" when, as in one classic tale they told over and over again, someone fell out of the upper grandstand in the baseball park where the concerts took place, and broke a lot of bones; or when someone experienced a severe adverse drug reaction; or when (another classic event) someone had a baby fifty feet in front of the bandstand. Those events were "something happening," but they were very rare. Most "patients" wanted an aspirin for a headache or a bandaid for a blister, and long periods went by when no one wanted

anything at all. Most of the remainder had had too much beer and dope, too much hot afternoon sun, and had passed out, but were not in any real danger. When those things were what was "happening," the volunteers sat around and complained that "nothing was happening." Infected by their mood, I concluded that nothing was happening and therefore that there was nothing to photograph.

One day I realized that it couldn't be true that nothing was happening. Something is always happening, it just doesn't seem worth remarking on. (Just as the John Cage piano piece I mentioned earlier forces us to realize that there is always some sound going on, though we may not identify it as music.) So I set myself the problem of photographing what was happening when nothing was happening. Not surprisingly, a lot was happening when nothing was happening. Specifically, the volunteers, who were mostly in their twenties and early thirties and mostly single, were mostly still looking for Mr. or Ms. Right. Volunteering for this event was like going to a big party with some of your favorite bands playing, free beer, an organic lunch, and a lot of nice-looking young men and women who shared some of your tastes. Once I instructed myself to photograph what was happening when nothing was happening, I found hundreds of images on my contact sheets of these young folks dancing, conversing earnestly, coming on to each other, and otherwise socializing. This added an interesting and important dimension to my sociological analysis and photographic documentation, showing me that there was more to recruiting the medical team than providing some interesting medical experience.

The more general statement of the problem, as I've already suggested, is that we never pay attention to all the things that are going on in the situations we study. Instead, we choose a very small number of those things to look into, most obviously when we do research that measures only a few variables, but just as much when we do fieldwork and think we're paying attention to everything. And, having looked at what we've decided to look at, we pretty much ignore everything else that's going on, which seems routine, irrelevant, boring: "Nothing's happening."

The idea that we should only attend to what is interesting, to what our previous thinking tells us is important, to what our professional world tells us is important, to what the literature tells us is important, is a great pitfall. Social scientists often make great progress exactly by paying attention to what their predecessors thought was boring, trivial, commonplace. Conversation analysis provides a classic example. How, for instance, do people

decide who will speak next in a conversation? Conversation analysts suggest that there is a rule, the "turn-taking rule," that requires people to alternate turns and speak only when it is their turn. Well, who cares? Is that worth paying attention to? Harvey Sacks (1972, 342) went on to suggest a major subcategory of this phenomenon: questions. Generally accepted rules governing conversation constrain anyone who asks a question to listen to the answer their question has solicited. Again, so what? Well, that provides an understanding of the annoying habit children have of beginning a conversation with adults by saying "You know what?" Conversation analysis explains this commonplace event as a shrewd exploitation by children of the rule about questions. It is hard to avoid answering "You know what?" with "What?" But once we have asked "What?" we have to listen to the answer, and that was what the child was after all the time, getting our difficult-to-secure adult attention. Suddenly, this "silly result" about turn-taking has explained something about the uses of power, and given us a rule we can take elsewhere, to more adult and "serious" phenomena.

So we can generalize the procedure I used at the rock medicine concerts to cover all the variations of other people's ideas shaping what we choose to study. Researchers pick up, not very consciously, the ideas of the people they're studying and working with. If they think something is trivial, you (as researcher) are likely to think that too. These young people liked the sociability that went with the rock concert. But that wasn't "serious," it wasn't what you especially looked forward to, it wasn't what you included when you wanted to impress someone else about your participation in the event. (The comedian Mort Sahl used to explain that, when he was in college, he got involved in left-wing causes for the same reasons other guys did: he wanted to save the world and meet girls.) Everyone shares these ideas, and it doesn't occur to you to look beyond them. After all, there's plenty to be interested in in the provision of medical services to a young drug-using population, isn't there?

It's not just common sense and the prejudices of our companions that blind us to what's there to see. We often decide what to include and what to leave out on the basis of an imagery and its associated theory that settles all those questions for us a priori. All our theories specify something about what we should look at and, by implication, what we needn't bother with (whatever the theory doesn't bother with). That's the very solid core of feminist complaints that many, if not most, sociological theories are sexist. Those theories aren't openly, or necessarily, male-oriented; they just don't

routinely include, in their systematic exposition of topics and problems, some concerns feminists think important, part of what you routinely ought to look for. The male-dominated study of chimpanzee social life, as Donna Haraway has shown, went on and on about dominance and all that boy stuff, without paying attention to the food-gathering and childrearing the females did. There's no good scientific reason for that emphasis and, of course, the males could never have spent all their time trying to push the other guys around if someone wasn't bringing home the bananas and taking care of the kids. The theories that focused on dominance could, in principle, encompass these other matters, but they didn't enjoin researchers to do it in a regular way.

On the Other Hand . . .

I insisted earlier that researchers must learn to question, not accept blindly, what the people whose world they are studying think and believe. Now I have to say that at the same time they should pay attention to just that. After all, people know a lot about the world they live and work in. They have to know a lot to make their way through its complexities. They have to adjust to all its contradictions and conflicts, solve all the problems it throws their way. If they didn't know enough to do that, they wouldn't have lasted there this long. So they know, plenty. And we should, taking advantage of what they know, include in our sample of things to look at and listen to the things the common knowledge and routine practice of those studied make evident.

I don't, however, mean that we should treat "people's" knowledge as better or more valid than ours. Many social scientists, justifiably leery of the contention that we know more about the lives and experience of the people we study than they do themselves, have argued that our work should fully respect the superior knowledge social actors have of their own lives and experience. These researchers want to leave the "data" pretty much as they found it: people's stories in the words in which they were communicated, uncut, unedited, "unimproved" by any knowing social science commentaries and interpretations. Science, these researchers think, really has nothing to add, because people, who know for themselves what they have lived through, are the best source of information about it.

This argument has the kernel of truth suggested in the discussion of imagery: social scientists, who have ordinarily not had the experiences of the

people they're learning about, must always rely on the accounts of those people to know what it's like from the inside. (An important exception occurs when the analyst participates in the activities being studied.) But that doesn't make them unconditionally usable for research purposes. Since people ordinarily give us these accounts in a "research situation" that differs substantially from the ones they are describing, the accounts cannot be taken at face value. We, for instance, guarantee our interviewees a confidentiality they could never be sure of in their ordinary lives. This can only make the account of an event something less, and perhaps quite different, than what we might have seen had we been there to see for ourselves.

Social scientists who propose that people necessarily know more than we do about their own lives often add that we must respect the dignity of other people by refusing to appropriate their lives and stories for our own selfish uses, simply presenting, unchanged and uninterpreted, what they have told us. The warrant for this is less obvious: It is not self-evident that everyone social scientists study deserves such respect (the usual counterexamples are Nazis and sadistic police). Further, fully accepting this position might reasonably lead us to conclude that we aren't entitled to make any use at all of the material of other people's lives. Contemporary anthropology is caught up in this dilemma, as are contemporary documentary photography and filmmaking (particularly over the blatantly exploitative nature of many "slumming" documentaries).

I disagree. Sociologists do know some things the people they study don't know. But that's true in a way that makes the claim neither unwarranted nor disrespectful, a way that suggests some sampling tricks we can use. The argument is an extension of one Everett C. Hughes used to make.

Briefly, sociologists and other social scientists do not ordinarily study the life and experience of just one person (even when they focus on one person, in the style of Douglas Harper's study [1987] of a rural jack-of-all-trades, they usually include all the people that central character comes in contact with regularly). Rather, they (at least some of them) study the experiences of a great many people, people whose experiences overlap but aren't exactly the same. Hughes used to say, "I don't know anything that someone in that group doesn't know but, since I know what they all know, I know more than any one of them."

When Blanche Geer, Everett Hughes, and I studied college students (Becker et al. [1968] 1994), we divided our attentions in the field. Geer studied fraternity and sorority members, while I spent most of my time