FROM
TRICKS OF THE TRADE:
HOW TO THINK OF
YOUR RESEARCH
WHILE YOU'RE
DOING IT
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SAMPLING

What to Include

Sampling and Synecdoche

Sampling is a major problem for any kind of research. We can't study every case of whatever we're interested in, nor should we want to. Every scientific enterprise tries to find out something that will apply to *everything* of a certain kind by studying *a few examples,* the results of the study being, as we say, "generalizable" to all members of that class of stuff. We need the sample to persuade people that we know something about the whole class.

This is a version of the classical trope of *synecdoche,* a rhetorical figure in which we use a part of something to refer the listener or reader to the whole it belongs to. So we say "The White House," and mean not the physical building but the American presidency—and not just the president, but the whole administration the president heads. Synecdoche is thus a kind of sampling, but meant to serve the purpose of persuasion, rather than that of research or study. Or perhaps it would be better to say that sampling is a kind of synecdoche, in which we want the part of a population or organization or system we have studied to be taken to represent, meaningfully, the whole from which it was drawn. Logics of sampling are arguments meant to persuade readers that the synecdoche works, because it has been arrived at in a defensible way. (I only discovered the discussion of sampling and synecdoche in Hunter 1990, which parallels mine in several ways, as this book was being readied for publication.)

The problem with synecdoche, or sampling, seems at first to be that the part may not represent the whole as we would like to think it does, may not reproduce in miniature the characteristics we are interested in, may not allow us to draw conclusions from what we do know that will also be true of what we haven't inspected ourselves. If we pick a few men and women off the streets of Paris and measure their height, will the average we calculate from those measurements apply to the whole population of Paris? Can we compare a similar average, computed from the heights of a few people picked off the streets of Seattle, to the Parisian average? Will the average height of all the inhabitants of each of these cities be the same, more or less, as the average height of the few we did measure? Could we, with these samples, arrive at a defensible conclusion about the comparative height of people in France and people in the United States? Can we use the sample as a synecdoche for the population? Or will our research be open to the kind of carping criticism students soon learn to address to any finding, the one that announces triumphantly "your sample's deficient!"?

Random Sampling: A Perfect Solution (For Some Problems)

The procedure of random sampling, so beloved of those who want to make social science into "real science," is designed to deal with this difficulty. Suppose we want to know what fraction of a city's population think of themselves as Democrats, or voted Democratic in the last election, or think they are going to vote for the Democratic candidate in an upcoming election. For the sake of efficiency, we don't want to ask every inhabitant about their identifications or actions or intentions. We want to ask some of them and reason from the some we talk with to the whole population of the city. If 53 percent of those we talk to say "Democrat," we'd like to be able to say that if we had asked everyone, the proportion would have been pretty much the same.

Statistical sampling procedures tell us how to do that. We can choose the people we will interview by using a table of random numbers, numbers arranged in an order guaranteed not to contain any bias. That is, there aren't any patterns in the numbers that will give some people a greater chance of being chosen. We have to use such an arcane procedure because almost any other way of picking cases you can think of will turn out to have such a bias built in.

Here's a frightening example of the kind of mistake you can make. Hatch and Hatch (1947) decided to study "criteria of social status" by gathering biographical data about the participants in weddings announced in the Sunday *New York Times*, on the assumption that people whose weddings got into the *Times* probably occupied "a superior position in the New York social system." Well, maybe so; it's the kind of thing sociologists are always assuming in order to get on with their research. The researchers further decided (it seems a reasonable way to get a large number, though the researchers did not make an argument for it) to study all the weddings announced in June over a period of years. They reported (this was only one of many findings) that "no announcement acknowledged marriage in a Jewish synagogue or gave any indication of association with the Jewish faith." They don't comment on this result, although they do make some interpretations of other findings, mostly pointing to the social characteristics of their families people thought worth emphasizing in their announcements. Still, it's quite striking that, in a city with as large a Jewish population as New York then had, no Jewish weddings were announced in the place where such announcements were customarily made.

The explanation was not long coming. A "Letter to the Editor" (Cahnman 1948) reported replicating the study, at least with respect to the proportions of Jewish weddings, in Sunday editions in October and November (because that was when Professor Cahnman read the offending article). In this sample, he reported, "[Of the] 36 marriage announcements [in these editions] no less than 13, that is 36.1% of the total, were performed by a rabbi. (The rabbi, to be sure, is labeled 'The Reverend So-and-so,' but there is a way to find out who is a rabbi, for one who knows.)"

Why the discrepancy? Cahnman explains:

> [T]he fact which the authors could easily have ascertained from any rabbi or otherwise Jewishly informed scholar is that Jewish weddings are not performed in the seven weeks between Passover and the Feast of Weeks and in the three weeks preceding the day of mourning for the destruction of the Holy Temple in Jerusalem. Almost invariably, June falls into the one or the other period. All orthodox and conservative rabbis, and the great majority of reform rabbis, adhere to the observance.

Cahnman concludes that the authors should have, on getting such a seemingly unusual result, looked into the matter further, become more knowledgeable, or at least gotten some expert advice—in short, done something to undo the effects of their ignorance of this feature of Jewish practice.

But Josephine Williams, from whom I was taking a course in statistics at the University of Chicago when the article and letter appeared, drew a different and in some ways more practical conclusion. Recognizing that (a) there might be many such problems buried in the data, and (b) not all of them would produce "amazing" conclusions of the kind that alerted

Cahnman, she showed us that any and all problems of this general type would have been avoided had the authors used a table of random numbers to pick their months, instead of the cute device of studying June weddings.

Using such a method, we pick our cases (usually people, but they could easily be issues of the *New York Times*) in such a way that every member of the population has a known (usually, but not necessarily, equal) chance of being chosen for the sample. Then existing formulae, whose mathematical logic is thoroughly defensible, can tell you how probable it is that the proportion of Jewish weddings reported in the issues you looked at (or the proportion of Democrats you found in your sample of interviewees) could have come from a population where the "true" proportion of Jewish weddings (or Democrats) was different.

Such a result is well worth getting, but only when it is what you want to know. That's why I said above that the problem *seems* to be that the part might not accurately represent the whole, faithfully reproducing its important characteristics: average height, proportion of Democratic voters, proportion of Jewish weddings. The relation of a variable's value in the sample to its value in the population is a problem, but it isn't the only sampling problem, because the average or proportion of some variable in a population might not be what you want to know. There are other questions.

Some Other Sampling Problems

We might, to take another kind of problem social scientists often try to solve, want to know what kind of an organization could be the whole of which the thing we have studied is a part. Using "the presidency" to refer to the whole administrative apparatus of the executive branch of the United States government raises the question of what kind of phenomenon that apparatus is. If we talk about the executive in charge, does our synecdoche communicate anything meaningful or reliable about the rest of it? We're not interested in proportions here, but in the way the parts of some complicated whole reveal its overall design (see the discussion in Hunter 1990, 122–27).

Archeologists and paleontologists have this problem to solve when they uncover the remnants of a now-vanished society. They find some bones, but not a whole skeleton; they find some cooking equipment, but not the whole kitchen; they find some garbage, but not the stuff of which the

garbage is the remains. They know that they are lucky to have found the little they have, because the world is not organized to make life easy for archeologists. So they don't complain about having lousy data. Instead, they work on getting from this thigh bone to the whole organism, from this pot to the way of life in which it played its small role as a tool of living. It's the problem of the Machine Trick, of inferring the organization of a machine from a few parts we have found somewhere.

We might want to know a third thing social scientists often concern themselves with: the full range of variation in some phenomenon. What are all the different ways people have organized kinship relations? What is the full range of variation in the ways people have organized keeping records or designing clothing? We ask those questions because we want to know all the members of the class our generalizations are supposed to apply to. We don't want our synecdoche to have features that are specific to some subgroup of the whole, which the unwary (among whom we must include ourselves) will take as essential characteristics of the class. We don't want, simple-mindedly, to assume that some feature contained in our example is just "naturally" there in every class member and thus does not require explanation. Is it just "instinctive" and "natural" that people don't have sexual relations with close relatives? If it turns out that that "natural" restriction didn't hold for the royalty of ancient Egypt, then we have to revise our conclusion about how "natural" the restriction is. We have to recognize that its existence requires a more detailed and explicit explanation.

Where Do You Stop? The Case of Ethnomusicology

Before considering some tricks that will help us arrive at synecdoches that will be helpful and withstand the "bad sample" criticism, let's return to an alternative approach I dismissed out of hand above, an approach that, though not practical, is something most social scientists have now and then dreamed about: to forget about sampling and, instead of relying on synecdoche, just get "the whole thing" and present it to our colleagues as the result of our work. This produces such chimeras as "complete description" and "reproducing the lived experience of people," among others.

We can investigate the result of trying to have it all by looking at ethnomusicology, that interesting, and usually happy, hybrid of anthropology and musicology. As a discipline, it aims to improve conventional musicology by getting rid of its ethnocentrism, and to improve anthropology by giving it access to a subject matter nonmusicians find hard to describe and discuss. In pursuit of these worthy goals, it sets out to solve the sampling problem by describing, as I will explain, all the music there ever was or is.

But such an inclusive goal immediately creates a terrible problem. If you don't limit the scope of your discipline—the range of material whose explanation and understanding its ideas and theories are responsible for—to conventional Western music (that's the usual solution), what do you count as the music you ought to be studying and theorizing and generalizing about? (Remember that this is only a special case of a problem all the social sciences share, whether they recognize it or not. Try it for yourself with religion or economy or any of the standard social science objects.)

An outsider approaching ethnomusicology can't help noticing the ambitious nature of the enterprise. The simple but unsatisfactory answer the discipline long gave to itself, and to anyone else who asked, was a list of all the things that were patently music but had usually been left out of musicological thinking and theorizing. It thus proposed to study and take intellectual responsibility for all the world's musics, all the music made anywhere by anyone in any society. Not just Western symphonies and operas, and Western popular music, but Javanese *gamelan* and Japanese court music and Native American musics and African drumming and Andean pipe playing, and anything else an exhaustive survey could uncover. Later ethnomusicologists added to the list: folk musics of all kinds, jazz, the transformations of Western pop music found in other parts of the world (as in Waterman 1990). But a list is not a definition.

In addition to taking all that on, ethnomusicology, as the plural form—"musics"—implies, proposed to treat all those musics on their own terms. Every music has an aesthetic ethnomusicological researchers have enjoined themselves to take as seriously as the people who perform it and listen to it do. Researchers therefore do not treat other musics as degenerate or incompletely realized versions of "our" music; rather, they give each one the same serious consideration musicologists give to music in the Western ("our") tradition. If you accept this view of the job, there isn't anything that might be considered music that one shouldn't, in principle, be studying. Such catholicism has been traditional in comparative studies of the arts, and comparative musicology has always been omnivorous, collecting instruments and sounds and compositions and performances from anywhere a practitioner could get to with notebook, still camera, movie camera, and state-of-the-art sound recording equipment.

This definition of the job, of course, has never been completely honored in ethnomusicological practice. The discipline has always had to struggle against a chronic highbrow prejudice, a tendency to give greatest attention to what counts as art music in other "high" cultures, musical traditions we think as aesthetically worthy as our own: Indian *ragas* or Japanese *gogaku*. The discipline has often overcome that prejudice, but practicing ethnomusicologists always feel a strong obligation to go beyond such parochialism. Their worries about that obligation show up in the general statements about the field made in textbooks and on such ceremonial occasions as the presentation of presidential addresses.

Such a definition of ethnomusicology's domain creates terrible problems because, in practice, the comprehensiveness can't really be honored. You can aim at collecting all the music, but then collecting takes precedence over everything else. You never get beyond the collecting, because there is so much music to collect. Surely there has to be a principle of selection. What music can we safely leave out? How about children's nursery rhymes? Can we ignore them? Well, no, we wouldn't want to leave those rhymes out. They're so important in understanding how children are taught the ways of thinking and feeling and acting characteristic of their society—how they are, in a word, socialized. And the way children learn music, their "mistakes," the salience of one or another aspect of music to them, that's all interesting and important. Look what John Blacking (1967) did with such material, or at Antoine Hennion's study (1988) of the way French children are taught (whether they learn, as he shows, is another matter) music in school.

Can we leave out what isn't "authentic"? Authenticity has off and on been a problem for ethnomusicologists, at least some of whom used to have that sort of bias, a predilection for what people used to do rather than what they're doing now—a greater interest, let's say, in the remnants of authentic Polynesian musics than in the "Hawaiian" songs like "Sweet Leilani" that Don Ho was singing in a hotel on Waikiki Beach. Ethnomusicologists have often wished people wouldn't change their musical habits and tastes the way they do, that they would keep their music "pure," unadulterated by the inexorable spread of Western (mostly North American) rock and roll, jazz, and the rest of it. Ethnomusicologists have in this way resembled those naturalists who want to save endangered creatures so that the earth's gene pool will contain maximal variety.

These complaints often merge with those of musical nationalists, who want to preserve the "traditional" music of their people or country, even when that tradition is newly invented. Hermano Vianna (1995) has described how the samba, itself a mixture of a variety of musics from Europe and Africa, became the "traditional" national music of Brazil, a claim to which it had no more right than many other musics played and heard in Brazil at the same time.

Preserving all these changing musics sounds like a noble idea, but the world seldom accepts such noble ideas as guides to action. People pick up on the music they like, the music that seems attractive to them, that represents, however inchoately, what they want represented, the music that will make a profit for those who do the producing and distributing, and so on. So it seems wiser, even more practical, if you're interested in the world's musics, to study what people are playing and singing now, no matter what bastard combination of raw materials it comes from, as well as whatever you can recover of those musics they are deserting.

But, far from solving the problem of what to study, that really opens the door. I worked my way through graduate school playing piano in taverns and strip joints in Chicago. Should ethnomusicologists study what every tavern piano player (the kind I was) plays in all the joints on all the streets in all the world's cities? No one would have thought it worthwhile to do that around 1900, when a definitive study could have been done, say, of the origins of ragtime. But wouldn't it be wonderful if they had? And had carried that study through with the same care and attention that have been devoted to Native American music? Of course it would.

But why limit ourselves to the professionals who make music as a job? Should we study, as we might study the similar musical rituals in a Melanesian society, every singing of "Happy Birthday" in the United States or, to be a little reasonable, a sample of such singing? And if not, why not?

I won't continue with the examples because the point's clear. We'd like, in retrospect, to have everything, because all of it will fit the definition and all of it could be made the object of serious study. (By now it should also be clear that I'm not just talking about music.) But we can't have everything, for the most obvious practical reasons: we don't have the people to collect it and we wouldn't know what to do with the mass of detail we'd end up with if we did. It resembles oral history in that way. The "new" historians (see McCall and Wittner 1990) have convinced us that everyone's life is important; but we can't collect *everyone's* life, and if we did we'd drown in

the detail of all those lives. And no computerized database could help us, because the drowning is conceptual, not mechanical.

Social science has no simple answer to this problem. A social scientist might put it in comparative perspective and note that every global definition of a field creates just such an undoable job, certainly in the social sciences. A sociologist of science and scholarship might note further that the practical answers to these unanswerable questions—and practitioners always have practical, everyday answers to unanswerable questions—do not come from logic or argument, but are based in solid social facts of organizational resources and competition. Ethnomusicology's scope has, I assume (though I haven't done the work to justify saying this), been determined by its position in the academic hierarchy and the resources for research and other scholarly activities that position makes available. That's a topic ethnomusicologists might want to confront directly, rather than continuing to debate the proper boundaries of the field, taking as a model the discussions of the effect of anthropology's position in the academy on anthropological work in George Marcus (1986) and Paul Rabinow (1986, esp. 253–56).

Other social scientists might at this point be feeling superior to these benighted ethnomusicologists, who haven't grasped the impossibility of "getting it all," and haven't understood that the point is to find ways to avoid having to do that. But they needn't feel superior. Every field of social science has its own yearnings for completeness. For some it is the archive that will contain all the data from all the polls ever made; for others it is the will-of-the-wisp of "complete description" made possible by such new machines as audio or videotape recorders. We all know better, but we all lust after "getting it all" just the same.

Harold Garfinkel, the founder of ethnomethodology, has made generations of researchers of every methodological tribe uneasy by insisting that social science is, after all, a "practical activity," which is to say, among other things, that the work has to get finished sometime. No one can spend forever doing their study, so short cuts have to be taken and these invariably lead to violations of "the way research is supposed to be done."

This long example is just one version of how and why we are stuck with the synecdoche of sampling. Let's return to the idea of sampling understood in this extended way, as a question of what we can say about what we didn't see on the basis of what we did see, keeping in mind that there are several reasons for doing that, and not just the conventional one of estimating, within a given range of confidence, a measure of something in a population from a sample of that population.

Having just given up on the idea of completely describing everything, I now want perversely to return to it, to use it as a benchmark, to consider every way of creating the synecdoches of sampling as methods whose results we should assess against the "ideal" of full and complete description of everything that might be or is relevant to whatever we want to say with assurance about some social phenomenon. I suggest this not because I think such description is possible, but because such a benchmark shows us what choices we make when we do, inevitably, leave things out.

What, then, would "full and complete description" mean?

How Much Detail? How Much Analysis?

When I teach field research, I always insist that students begin their observations and interviews by writing down "everything." That is, I claim that I don't want them to sample but rather to report the universe of "relevant" occurrences. This generally leads to a good deal of foot dragging by them and nagging by me. They say they can't do it, or can't do it "honestly" (by which they mean that what they write will be neither complete or fully accurate). I say they will never know whether they can do it unless they try and that their attempts to write everything down will be no less accurate than an account that leaves a lot out. I suggest that they buy a rubber stamp that says "This transcript is not complete or fully accurate" and stamp every page of their notes with it, to assuage the combination of guilt and sloth that attacks them. Though I make fun of them, beneath their reluctance lies a healthy wariness at being asked to do what we have already seen is, on the large scale, undoable.

The job is, of course, undoable on the small scale as well. You can't write down "everything." That doesn't mean that you can't write down a good deal more than students ordinarily do. But the students are right, they can't write it all down.

I also insist that what they think is straight description is usually nothing of the kind, but rather a sort of analytic summary of what they have seen, designed to evade the requirement not to sample, but to report it all. Thus: "Patients came into the office, and waited impatiently for the doctor to see them." That sentence contains no report of an observation of someone actually exhibiting impatience, no sample of such descriptions on which a conclusion might be based. Instead, it summarizes and interprets many things its author surely did see: people walking in and out of the office, fidgeting, looking at their watches or the clock on the wall, making ritualized sounds of impatience aimed at no one in particular, perhaps soliciting an expression of similar feeling from others, and so on.

What would straight, uninterpreted description—supposing one did it—actually look like? Granting that it is, in principle, impossible to avoid all interpretation, you can still go a lot farther in the direction of pure description than most of us ever go. Georges Perec, the French novelist, was a great experimenter with "plain description," and conducted one of his experiments for a French radio network, the experiment here described by his biographer, David Bellos:

> On 19 May, 1978, a mobile recording studio drew up outside L'Atrium (Perec usually called it L'Acquarium) at Place Mabillon, on Boulevard Saint-Germain. One of the strangest experiments in radio history was about to begin. A writer well known for his attention to detail and to the "infra-ordinary" was to spend an entire day describing what passed in front of his eye, into the microphone, in real time. Obviously, Perec took a few breaks for coffee and meals and so on, and the experiment was brought to a close with about five hours of tape in the can. This was later edited by Perec and René Farabet, the producer, into a hallucinatory aural experience some two hours in length, broadcast in February 1979 as *Tentative de description de choses vues au carrefour Mabillon le 19 mai 1978* (An attempt at a description of things seen at Mabillon Junction on 19 May 1978).
>
> What does the experiment prove? That trivia can become poetry when pushed beyond reasonable limits; that repetition can become rhythm. That there is a thin borderline between punishment and intoxication. And perhaps no one but Perec could have had the combination of self-restraint (he never comments on what he sees, he just says, *another 68 bus, three red cars, a lady with a dog . . .*), modesty, and sheer gall to carry on for hours on end, to the end.
>
> The art of enumeration is not easy. (Bellos 1993, 640)

Right. The art of enumeration is not easy. Understand what enumerating without ever commenting implies here. Perec did not say "He looks like he's in a hurry to get home with his shopping" or "Those two look like they're gossiping about someone they know slightly," the kind of thing you might expect a novelist to say, the kind of thing you might expect anyone to say. Here's what he did say (and this quotation comes from a published fragment drawn from a different occasion of such observation and recording, the material from the day Bellos talks about not being available in print):

> Saturday, June 12, 1971, Around three o'clock.
> Cafe l'Atrium.
> A gray police car just stopped in front of Lip's clothing store. Three women cops got out, their traffic ticket books in their hands.
> Next to Lip's, a black building is being repaired or torn down. On the wooden enclosure hiding its ground floor, three advertisements, one for the "House Under the Trees" (the title hidden by a row of yellow portraits under which I believe I can read "Pasionaria"), the second for "Taking Off," the third for "You're Always Too Good to Women" (the title hidden by violet and white question marks which I know, because I saw them from much closer up a second ago, belong to a poster for a public discussion with Laurent Salini (Communist Party)).
> At the intersection of Buci and Saint-Germain, a pole with a French flag and, a third of the way up, a banner announcing the Roualt exhibition.
> In the foreground, chains which prevent crossing the boulevard. Someone has hung small placards for the magazine CREE "The First French Magazine for the Design of Art and the Contemporary Environment" on them; the cover of the magazine represents a fence.
> Light traffic.
> Not many people in the café.
> Pale sun coming through the clouds. It's cool.
> The people: generally alone, sullen. Sometimes in couples. Two young mothers with their young children; girls, in twos and threes; very few tourists. Long raincoats, a lot of army (American) jackets and shirts.
> A newspaper stand across the street:
> Automobile: Le Mans
> Romy Schneider charged!
> Week-end: A camera shows the winners
> (I still have a good view!)
> Another police car (the third since I got here)
> A friend who I often see strolling along the streets shuffles by.
> (Sketch of a typology of walking? Most of the passersby stroll, shuffle, seem to have no precise idea of just where they are).
> A couple on the terrace block my view.
> It begins to rain. (Perec 1980, 33–34)

This is description without the interpretations that, we might say, make sense of the simple facts of observation, the interpretations the students in my fieldwork classes so often want to substitute for sheer observation.

Social scientists, like those students, ordinarily expect to be given such interpretations in what they read and to rely on them in what they write. They think of the details of their work as the basis for generalizations, as samples whose interest lies in their generalizability, in the interpretations that explain what the details stand for. But perhaps these interpretations aren't as necessary as we think. We can get a lot from simpler, less analyzed observations. The appropriate ratio of description to interpretation is a real problem every describer of the social world has to solve or come to terms with.

(Everyone knows that there is no "pure" description, that all description, requiring acts of selection and therefore reflecting a point of view, is what Thomas Kuhn said it was, "theory laden." That it is not possible to do away entirely with the necessity of selection, and the point of view it implies, does not mean that there aren't degrees of interpretation, that some descriptions can't be less interpretive (or perhaps we should say less conventionally interpretive) than others. We might even say that some descriptions require less inference than others. To say that someone looks like he is hurrying home with his shopping requires an inference about motivation that saying that he is walking rapidly doesn't.)

So social scientists expect interpretations from themselves and each other. They typically want to reduce the amount of stuff they have to deal with, to see it as examples of and evidence for ideas they have, not as something to be dished up in quantity for its own interest. They don't want a lot of (what is often labeled "mere") description, or a lot of detail. John Tukey, the statistician, once remarked that most tables contain far more information than anyone wants or needs, that mostly what we want to do is compare two numbers and see if they are the same or if one is bigger than the other; the rest of the numbers in all those cells are just noise, drowning out the message we are looking for.

Still, massive detailed description has something substantial to recommend it, beyond the possibilities of poetry and rhythm to which Bellos alluded, which we can't expect social scientists to take seriously. An occasional researcher still finds the accumulation of enormous detail to be just the ticket. Roger Barker, in a wonderful but never imitated book (Barker and Wright 1966), described one Kansas boy's day in that kind of detail. Gregory Bateson and Margaret Mead (1942) described the psychological life of Balinese villagers in something like such detail, adding several hundred photographs to the verbal descriptions. A well-known example of such description is *Let Us Now Praise Famous Men*, by photographer Walker Evans and writer James Agee, from which I will take an extended example.

In 1936 James Agee and Walker Evans, writer and photographer, went to Alabama to do a story, text and pictures, for *Fortune* magazine. Their book, *Let Us Now Praise Famous Men: Three Tenant Families* (Agee and Evans 1941), was not successful when first published but has since been recognized as a classic of—well, it isn't exactly clear what genre it's a classic of. Literature, perhaps. I would be glad to claim it for sociology, although I think a lot of sociologists would be unhappy about that (bad sample, not very scientific, etc.). In any event, one thing it is certainly a masterpiece of is minute, detailed description, the kind of description that lets you see how much summary, how much generalization, is contained in the most exhaustive social scientific descriptions. So it raises the question of sampling in an even stronger form than Perec's description of the Paris street corner. This is what description would look like if it were a much more detailed and complete sampling of what is there to describe.

The book's extended table of contents gives an idea of this detail. A section called "Shelter: An Outline," in the subsection devoted to "The Gudger House," contains the following headings, each referring to a substantial (that is to say, several printed pages) description of the kind I will shortly quote:

> The house is left alone
> In front of the house: its general structure
> In front of the house: the façade
> •
> The room beneath the house
> •
> The hallway
> Structure of four rooms
> Odors
> Bareness and space
> • •
> I. The Front Bedroom
> General
> Placement of furniture
> The furniture
> The altar
> The tabernacle
> II. The Rear Bedroom
> General
> The fireplace
> The mantel
> The closet
> The beds
> III. The Kitchen
> General
> The table: the lamp
> IV. The Storeroom
> Two essentials
> In the room
> • •
> In the front bedroom: the Signal
> The return

Fifty-four pages are devoted to this description of a sharecropper family's shack, which the reader already knows from the portfolio of photographs by Walker Evans that precedes the book's text. Here are the two pages devoted to "the altar" (already pictured in one of the Evans photographs, so the reader can check the words against the picture):

> The three other walls [of the front bedroom] are straight and angled beams and the inward surfaces of unplaned pine weatherboards. This partition wall is made of horizontals of narrow and cleanly planed wood, laid tightly edge to edge; the wood is pine of another quality, slenderly grained in narrow yellow and rich iron-red golds, very smooth and as if polished, softly glowing and shining, almost mirroring bulks: and is the one wall of the room at all conducive to ornament, and is the one ornamented wall. At its center the mantel and square fireplace frame, painted, one coat, an old and thin blue-white: in front of the fireplace, not much more than covering the full width of its frame, the small table: and through, beneath it, the gray, swept yet ashy bricks of the fireplace and short hearth, and the silent shoes: and on the table, and on the mantel, and spread above and wide of it on the walls, the things of which I will now tell.
>
> On the table: it is blue auto paint: a white cloth, hanging a little over the edges. On the cloth, at center, a small fluted green glass bowl in which sits a white china swan, profiled upon the north.

On the mantel against the glowing wall, each about six inches from the ends of the shelf, two small twin vases, very simply blown, of pebble-grained iridescent glass. Exactly at center between them, a fluted saucer, with a coarse lace edge, of pressed milky glass, which Louise's mother gave her to call her own and for which she cares more dearly than for anything else she possesses. Pinned all long the edge of this mantel, a broad fringe of white tissue pattern-paper which Mrs. Gudger folded many times on itself and scissored into pierced geometrics of lace, and of which she speaks as her last effort to make this house pretty.

On the wall, pasted or pinned or tacked or printed, set well discrete from one another, in not quite perfected symmetric relations:

A small octagonal frame surfaced in ivory and black ribbons of thin wicker or of straw, the glass broken out: set in this frame, not filling it, a fading box-camera snapshot: low, gray, dead-looking land stretched back in a deep horizon; twenty yards back, one corner of a tenant house, central at the foreground, two women: Annie Mae's sister Emma as a girl of twelve, in slippers and stockings and a Sunday dress, standing a little shyly with puzzling eyes, self-conscious of her appearance and of her softly clouded sex; and their mother, wide and high, in a Sunday dress still wet from housework, her large hands hung loose and biased in against her thighs, her bearing strong, weary, and noble, her face fainted away almost beyond distinguishing, as if in her death and by some secret touching the image itself of the fine head her husband had cared for so well had softly withered, which even while they stood there had begun its blossoming inheritance in the young daughter at her side.

A calendar, advertising ________'s shoes, depicting a pretty brunette with ornate red lips, in a wide-brimmed red hat, cuddling red flowers. The title is Cherie, and written twice, in pencil, in a schoolgirl's hand: Louise, Louise.

A calendar, advertising easy-payment furniture: a tinted photograph of an immaculate, new-overalled boy of twelve, wearing a wide new straw hat, the brim torn by the artist, fishing. The title is Fishin'.

Slung awry by its chain from a thin nail, an open oval locket, glassed. In one face of this locket, a colored picture of Jesus, his right hand blessing, his red heart exposed in a burst spiky gold halo. In the other face, a picture by the same artist of the Blessed Virgin, in blue, her heart similarly exposed and haloed, and pierced with seven small swords.

Torn from a cheap child's storybook, costume pictures in bright furry colors illustrating, exactly as they would and should be illustrated, these titles:
The Harper was Happier than a King as He Sat by His Own Fireside.
She Took the Little Prince in Her Arms and Kissed Him. ("She" is a goose girl.)
Torn from a tin can, a strip of bright scarlet paper with a large white fish on it and the words:
SALOMAR
EXTRA QUALITY MACKEREL
At the right of the mantel, in whitewash, all its whorlings sharp, the print of a child's hand.

No one will read this description without arriving at a conclusion about the misery of lives lived in these surroundings, but we have the data to arrive at that conclusion ourselves, and at much else besides. We don't need Agee to tell us explicitly. That is the kind of thing massive description can do.

Beyond the Categories: Finding What Doesn't Fit

Description and the "Categories"

What does all that description do for us? Perhaps not the only thing, but a very important one, is that it helps us get around conventional thinking. A major obstacle to proper description and analysis of social phenomena is that we think we know most of the answers already. We take a lot for granted, because we are, after all, competent adult members of our society and know what any competent adult knows. We have, as we say, "common sense." We know, for example, that schools educate children and hospitals cure the sick. "Everyone" knows that. We don't question what everyone knows; it would be silly. But, since what everyone knows is the object of our study, we must question it or at least suspend judgment about it, go look for ourselves to find out what schools and hospitals do, rather than accepting conventional answers.

We bump up against an old philosophical problem here, the problem of "the categories." How can we know and take account in our analyses of the most basic categories constraining our thought, when they are so "normal" to us that we are unaware of them? The exercises of Zen and other meditative practices, as well as creativity training, brainstorming, and similar exercises designed to get people to redefine vague or undefined common subjects, often have as their goal the elimination of the screen that words place between us and reality. Robert Morris, the visual artist, says "Seeing is forgetting the name of the thing we are looking at." John Cage's notorious composition "4' 33"," which consists of a pianist sitting at a piano, but not playing, for that length of time, calls attention to all the sounds that go on as an audience sits and listens to . . . to what was there to hear all along, but not listened to because it wasn't "music." Names, and the thoughts they imply, prevent us from seeing what is there to see.

You might think that any social scientist would, as a matter of course, expect a social law or general theory to cover all the cases it was supposed to cover, and would, again as a matter of course, systematically investigate the full range of possible applications, taking whatever steps were necessary to do that and to discover every subkind that might exist. You might think the problem of the categories would be an ever-present worry. Social scientists speak of this problem from time to time, but usually dismiss it as a philosophical conundrum ("How can we escape the constraints of our own culture?" "Too bad, looks like it's logically impossible").

In fact, social scientists seldom treat the problem of the categories as a practical research problem you could expect to solve. They usually do just the opposite, concentrating their efforts in any particular field of study on a few cases considered to be archetypal, apparently in the belief that if you can explain those, all the other cases will automatically fall into line. If we are going to investigate revolutions, we study the American, French, Chinese, and Russian (sometimes the English), which is not to say that historians and others ignore the hundreds of other revolutions around the world and throughout history, but rather that these few become what Talcott Parsons, in a felicitously misleading phrase, used to call "type cases," whose study is central to that area of work.

Consider: in the study of work, for quite a long time, people concentrated on investigations of medicine and the law. Though other varieties of work have since been studied intensively, these (and other kinds of work likely to be called professions) are still favorites, far out of proportion to something as simple as the proportion of all work they make up. In the study of deviance, violations of certain criminal laws (the ones usually violated by poorer people) are much more likely to be studied than those committed by business people and other middle-class folks. This disparity persists, even though Edwin Sutherland founded an entire field of study

around what he called "white collar crime." (I'll consider these examples at greater length in chapter 4, on concepts.) If we study social movements, we typically study those that succeed rather than those that fail.

One way of avoiding being trapped in our professionalized categories like this is, exactly, massive detailed description of the kind Agee and Perec produced. Careful description of details, unfiltered by our ideas and theories, produces observations that, not fitting those categories, require us to create new ideas and categories into which they can be fitted without forcing. This is one of the "other" sampling questions I spoke of earlier. If we call the choice of things to describe a sampling problem—which, of all the things we can observe about a person or situation or event, will we include in our sample of observations?—then we can see that the general solution of the problem is to confront ourselves with just those things that would jar us out of the conventional categories, the conventional statement of the problem, the conventional solution.

This brings up another paradox, due to Kuhn (1970, 18–22). Science can only make progress when scientists agree on what a problem and its solution look like—when, that is, they use conventionalized categories. If everyone has a different idea about what kinds of entities the world is made up of, what kinds of questions and answers make sense, then everyone is doing something different and it won't add up to anything. This is the situation Kuhn describes as having plenty of scientists, but no science. But scientists can only reach agreement on what to look at and study by ignoring practically all of what the world actually shows them, closing their eyes to almost all the available data. It's best to see this paradox as a tension. It's good to have a common conventionalized way of doing business, but it's also good to do whatever it takes to jar that agreement from time to time.

How do we go about finding cases that don't fit? We can do it by paying attention to all the data we actually have, rather than ignoring what might be inconvenient or otherwise not come to our attention. Or we can see what gets in the way of our finding such cases—whether the obstruction be conventional techniques or conceptual blinders—and, having identified the obstacles, manufacture tricks for getting around them.

Everything Is Possible

The simplest trick of all is just to insist that nothing that can be imagined is impossible, so we should look for the most unlikely things we can think of and incorporate their existence, or the possibility of their existence, into our thinking. How do we imagine these possibilities? I have been insisting on the necessity of choosing carefully, rather than ritualistically, what sort of data to go after, record, and include in our analyses; and on the further necessity of systematically using what we have so far gathered to avoid the traps conventional categories set for us. Random sampling won't help us here, or will help us only at an exorbitant cost. Remember that random sampling is designed to *equalize* the chance of every case, including the odd ones, turning up. The general method for sampling to avoid the effects of conventional thinking is quite different: it consists of *maximizing* the chance of the odd case turning up.

Look at the problem Alfred Lindesmith (1947) confronted when he wanted to test his theory about the genesis of addiction to opiate drugs. The theory, briefly, said that, to begin with, people became addicted to opium or morphine or heroin when they took the drug often enough and in sufficient quantity to develop physical withdrawal. But Lindesmith had observed that people might become habituated to opiates in that way—in a hospital, say, as the sequel to injuries from an auto accident that were painful and took a long time to heal—and yet not develop the typical behavior of a junkie: the compulsive search for drugs at almost any cost. Two other things had to happen: having become habituated, the potential addict now had to stop using drugs and experience the painful withdrawal symptoms that resulted, *and* had to consciously connect withdrawal distress with ceasing drug use, a connection not everyone made. They then had to act on that realization and take more drugs to relieve the symptoms. Those steps, taken together and taken repeatedly, created the compulsive activity that is addiction.

W. A. Robinson, a well-known statistical methodologist of the day, criticized Lindesmith's sample (Robinson 1951). Lindesmith had generalized to a large population (all the addicts in the United States or in the world) from a small and haphazardly drawn sample. Robinson thought Lindesmith should have used random sampling procedures to draw a sample (presumably from populations in prison or identified by having arrest records for narcotics offenses) of adequate size. Lindesmith (1952) replied that the purpose of random sampling was to ensure that every case had a known probability of being drawn for a sample and that researchers use these procedures to permit generalizations about distributions of some phenomenon in a population and in subgroups in a population. So, he ar-

gued, the procedures of random sampling were irrelevant to his research on addicts because he was interested not in distributions but in a universal process—how one became an addict. He didn't want to know the probability that any particular case would be chosen for his sample. He wanted to maximize the probability of finding a negative case. (Here he anticipated the procedure Glaser and Strauss [1967] described, years later, as "theoretical sampling.")

The trick, then, is to *identify the case that is likely to upset your thinking and look for it.* Everett Hughes taught me a wonderful trick for doing just that. He liked to quote the hero of Robert Musil's novel, *The Man without Qualities,* saying, "Well, after all, it could have been otherwise." We should never assume that anything is impossible, simply could not happen. Rather, we ought to imagine the wildest possibilities and then wonder why they don't happen. The conventional view is that "unusual" things don't happen unless there is some special reason for them to happen. "How can we account for the breakdown of social norms?" Following Hughes's lead, you take the opposite view, assuming that everything is equally likely to happen and asking why some things apparently don't happen as often as this view suggests. "Of course social norms break down. How can we account for their persistence for more than ten minutes?"

What you invariably learn from such an exercise is that all the weird, unlikely things you can imagine actually have happened and, in fact, continue to happen all the time, so that you needn't imagine them. Oliver Sacks, the neurologist, tells of seeing his first case of Tourette's Syndrome, the neurological disorder that leads people to burst into loud and uncontrollable cursing and dirty talk, in his office and being thrilled at having encountered such a "rare" phenomenon (1987, 93–94). He left his office to go home and, on the way to the subway, saw two or three more people whom he now recognized as Touretters. He concluded that those cases had been there in profusion all along; he just hadn't been ready to see them.

So, though they might not be where you thought they would turn up, if you keep your eyes open you have real cases to investigate. But even cases that come from fiction or science fiction can serve the same theoretical purpose, which is to imagine under what circumstances "unusual events" happen, and what obstacles prevent them from happening all the time.

We might, instead of saying "everything is possible," instruct ourselves to "look at the whole table, not just a few of the cells," or "find the full range of cases, not just the few that are popular at the moment." Each of those names points to another way of talking about this trick that Hughes thought so essential. Let's explore some of the obstacles to seeing the full range of cases and using it to theoretical advantage, and look for some ways of surmounting them. The problems are usually conceptual, arising because we believe something to be true and as a result have not investigated the situation it refers to. If we do investigate it, we will invariably find the odd cases we can use to advance our thinking. But the problems are also social, or sociological, in the sense that our reasons for not seeing the obstacles and doing something about them lies in some feature of the social organization they are embedded in and the social organization of our own work lives.

Other People's Ideas

A world of unlimited possibility is confusing and threatens to overwhelm us with a mass of fact and idea that can't be handled, so we are happy whenever we can persuade ourselves that we already know enough to rule out some of the possibilities the trick of exhaustive description might alert us to. The reasons for that are various, but they invariably involve researchers accepting the ideas other people have about what's important, what's interesting, what's worth studying. But other people have reasons for making those judgments that aren't our reasons. We can respect their opinions, but needn't and shouldn't accept them as the basis for our own decisions about what to include in our samples of cases and data. That's true even when the others involved are our own professional colleagues.

"Everybody Knows That!"

Scientists of every variety want to find something "new," rather than the same old stuff. This can be seen in the persistent misreading of Thomas Kuhn's (1970) idea of a "scientific revolution." Everyone wants to make a scientific revolution in his or her field. Heaven forbid that we just find out something routine, something that fits into the body of social science understanding we already have. Every finding, every tiny development in a field is hyped as a "revolution." That ignores Kuhn's analysis, mentioned above, which tells us that scientific revolutions are rare, that it is only by continuing to work on the same problems that workers in a discipline make any progress on anything.

Most of us, however, do not expect to make a revolution. But we do want, at least, not to study "what is already known," what has already been studied (or so we think). We think we can justify any research topic with the argument that no one has ever studied that particular thing before. Why study restriction of production? Donald Roy had already done that (Roy 1952, 1953, 1954). But Michael Burawoy, undeterred, went to study the same topic again (1979). He pushed understanding of the problem forward by doing so. Quite accidentally, Burawoy went to do his research in the very shop Roy had studied. It was still in the same building, but conditions had changed. No longer independent, the shop was now part of a larger firm. As a result, it no longer had to make its way in a competitive marketplace, because the larger corporation was now an assured market for its products. The shop was now unionized. And so you could study the same problem—how workers bought into management objectives—again. It was the same problem, but now it was occurring under new conditions.

That's a general point. Nothing stays the same. Nothing is the same as anything else. We do not operate in the world of physicists, where we can take a sample of a pure substance off the shelf and know that it is, near enough as makes no difference, the same substance any other scientist in the world will be handling under that name. None of our "substances" are pure anything. They are all historically contingent, geographically influenced combinations of a variety of processes, no two of the combinations alike. So we can never ignore a topic just because someone has already studied it. In fact—this is a useful trick—when you hear yourself or someone else say that we shouldn't study something because it's been done already, that's a good time to get to work on that very thing.

"That's been done" very often does get said to people, however, most often to students searching for a dissertation topic. "No sense doing that, Jones just published an article on it." Such remarks rest on a serious fallacy: that things with the same name are the same. They aren't, at least not in any obvious way, so studying "the same thing" is often not studying the same thing at all, just something people have decided to call by the same name. Just because someone studied the culture of prisoners somewhere doesn't mean you shouldn't study it somewhere else. I will not pursue this thought here, since it's taken up (and the example of prisons gone into at length) in chapter 4, under the heading of "enlarging the reach of a concept."

The Hierarchy of Credibility

Very often social scientists don't study the full range of phenomena because the people who run the organization we are studying define some of what should be included in our sample of cases and topics as not requiring study. They assure us that if we need to know anything beyond what they've outlined as "the problem," they can tell us all about it and there's no necessity to look further. If we accept that premise, we are letting their ideas dictate the content of our research.

I have elsewhere defined this phenomenon as the "hierarchy of credibility":

> In any system of ranked groups, participants take it as given that members of the highest group have the right to define the way things really are. In any organization, no matter what the rest of the organization chart shows, the arrows indicating the flow of information point up, thus demonstrating (at least formally) that those at the top have access to a more complete picture of what is going on than anyone else. Members of lower groups will have incomplete information, and their view of reality will be partial and distorted in consequence. Therefore, from the point of view of a well socialized participant in the system, any tale told by those at the top intrinsically deserves to be regarded as the most credible account obtainable of the organization's workings. And since, as Sumner pointed out, matters of rank and status are contained in the mores, this belief has a moral quality. We are, if we are proper members of the group, morally bound to accept the definition imposed on reality by a superordinate in preference to the definitions espoused by subordinates. (By analogy, the same argument holds for the social classes of a community.) Thus, credibility and the right to be heard are differentially distributed through the ranks of the system. (Becker 1970, 126–27)

So the presidents and deans of colleges, the managers of businesses, the administrators of hospitals, and the wardens of prisons all think they know more than any of their subordinates about the organizations they run.

That's only a problem for researchers if they accept the idea. If we turn to the leaders of organizations and communities for the final word on what's going on, we will inevitably leave out things those people think unimportant. We think we are being sophisticated and knowledgeable when we accept the ideas suggested by the hierarchy of credibility. It's

tempting to accept them, because we are, after all, well-socialized members of our society—we wouldn't have gotten where we are if we weren't—and it feels distinctly odd and unsettling to question so obvious an allocation of respect and interest. Educators, to recur to an example mentioned earlier, think sociologists studying school problems should study students because it's students' failure to work hard enough that makes problems; there's no point, if you talk to them, in studying teachers, let alone administrators, since they can't, by definition, be the problem. And we think to ourselves, "These people run the schools, they must know plenty, why shouldn't I accept their definition of the reality they work in?" Of course, we also know that leaders don't always know everything; that's one reason they let us do research. (They will, however, know if you come up with an answer they don't like.)

The trick for dealing with the hierarchy of credibility is simple enough: *doubt everything anyone in power tells you.* Institutions always put their best foot forward in public. The people who run them, being responsible for their activities and reputations, always lie a little bit, smoothing over rough spots, hiding troubles, denying the existence of problems. What they say may be true, but social organization gives them reasons to lie. A well-socialized participant in society may believe them, but a well-socialized social scientist will suspect the worst and look for it.

One way to make sure you exercise the proper skepticism is to look for "other opinions"—for people placed elsewhere in the organization who will give you another view, for statistics gathered by others than the officials. If you study a school, you will, of course, gather information from the principal and the teachers and the students; but try talking to the janitors and the clerks and secretaries too (and don't forget the people who used to work there).

Another way to get around the hierarchy of credibility is to search for the conflict and discontent whose existence organizational leaders usually deny. Everett Hughes had a wonderful way of doing this. When he interviewed members of an organization, he would ask, with his best innocent Midwestern look, "Are things better or worse around here than they used to be?" It's a wonderful question: almost everyone has an answer to it, it brings up the issues that are salient in the organization, and it prejudges nothing—neither what things might be better or worse, or what the appropriate measure of better and worse might be.

It's Trivial, It's Not a "Real Problem"

This criticism has been made of my work more than once. Just as some people think tragedy is somehow more important than comedy (you can tell I don't), some problems are seen as inherently serious and worthy of grownup attention, others as trivial, flyspecks on the wallpaper of life, attended to only for their shock value or prurient interest, mere exotica. Paying attention to these common ideas is a common reason for social scientists to study less than the full range of social activity that merits their attention.

I must have been immunized against this idea early, because my own research moved back and forth between "serious" and "nonserious" topics without causing me any anxiety. I first studied, for my master's thesis, the musicians who played in small bars and clubs in Chicago neighborhoods, for weddings, bar mitzvahs and other social affairs, and so on. These musicians, of whom I was one, did not belong to so socially worthy a profession as medicine or law. Nor were they workers in major industries, whose behavior (for instance, in restricting production) might have been a source of concern to the managers of those firms. Nobody cared about them, one way or the other. They weren't doing any particular harm (other than smoking marijuana, and no one cared if they damaged themselves that way), they didn't upset anyone powerful, they were just minor cogs in the entertainment industry. Everett Hughes found them interesting precisely because they were social nobodies with no reputation to protect, and so able to voice the conviction that was the major finding of my thesis: that the people they played for were stupid, unworthy clods. Hughes was interested because my finding, extending the range of kinds of work that had been studied, gave him a new hypothesis: that all members of service occupations hated the people they served, but members of high-prestige groups (the doctors and lawyers most people studied) wouldn't say that because it wasn't an appropriate thing for such high-class folks to be saying.

My dissertation research, however, was about the careers of public school teachers. Not a very prestigious group, but engaged in the culturally valuable activity of socializing the young, and respectable enough to satisfy anyone who thought sociology should deal with socially worthy topics. My more conventional friends applauded this choice, though my reason for it was mundane: Hughes paid me a dollar an hour to interview school

teachers and I decided I might as well write my dissertation about what I was doing anyway.

This fluctuation continued. I next studied marijuana users, not at the time considered a major problem (this was in 1951, long before dope-smoking became a standard middle-class activity that landed some nice kids in trouble with the police), therefore mere exotica. When it achieved the status of a real "social problem" some years later, my research was redefined as having dealt, after all, with a serious problem.

After a stretch of "serious" topics—studies of medical education and undergraduate collegiate life—Blanche Geer and I then studied trade schools, apprenticeships, and a variety of other educational situations working-class youth often attended. And my friends who thought I had "gone straight" were displeased. But then the federal government declared war on poverty and part of that war was a serious effort to teach more people trades and my research was "relevant" again.

So: recognize that your peers often judge the importance of a research problem by criteria that have no scientific warrant, criteria you might not accept. Knowing that, ignore these common-sense judgments and make up your own mind.

Why Them?

The hierarchy of credibility has, as a corollary, that certain people or organizations aren't really worth studying at all. That pervasive bias in the study of higher education at the time Hughes, Blanche Geer, Anselm Strauss, and I did our study of medical students (Becker et al. [1961] 1977) led to researchers studying only the "best places." Robert Merton and his colleagues were then studying medical education at Cornell and Columbia, commonly recognized as two of the "best" medical schools in the country. When we said that we were going to study the medical school of the University of Kansas, knowledgeable experts in research on higher education would ask us, solicitously and as if we perhaps didn't know any better, why we were doing that. "Why not?" "Well," they said, "after all, it's not one of the best schools, is it? I mean, if you're going to go to all the trouble of a big research project, why not study the best? You know, the University of Chicago or Harvard or Stanford or Michigan or some other 'eastern' school?" ("Eastern" was a well-known euphemism for "top-ranked," so 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 with independents; Hughes studied the faculty. We each learned things that "our" group knew, but others didn't. A "secret" society, dominated by the fraternities, operated a machine that organized campus political life; its leader told Geer all about it and she told me. But the independents I hung out with didn't know about it and I didn't tell them. Conversely, when independents mounted political actions they shared their plans with me and I told Geer, but she didn't tell the fraternity members. So our team, and each of us individually, knew more than any of the participants in campus political life.

Knowing these things didn't mean that we felt superior to the people we studied or that we thought we could find meanings in the events they participated in that were too subtle for them to understand. That would indeed be disrespectful. But it did mean we knew obvious things that the people involved would have understood quite well, had they had access to them. The reason they didn't know them was not that they were stupid or uneducated or lacking in sensibility, but that campus life was organized so as to prevent them from finding out. Saying that does not indicate disrespect for anyone's experience, but rather respect for the reality of the differential distribution of knowledge Simmel described in his essay on secrecy (1950, 307–76).

The message for researchers is plain. When the people studied know what they are doing and tell you about it, listen and pay attention. That doesn't mean to be gullible, because people will tell you things that aren't true from time to time. It does mean to use ordinary channels of organizational communication the way participants do, as a source of information

Jean Peneff makes a specific version of this point when he recommends that researchers do more counting in the field than they ordinarily do. He points out that most areas of social life involve a lot of

> counting, calculating, and enumerating. Factory workers count constantly: how many pieces have I made, how many operations have I done, how long have I worked? Office workers classify, file, count, and inventory. Measurement and calculation are ever-present on hospital services: how many beds are available? How long do I have to wait for a radio? How much time do we have? How many patients are waiting to be treated? How many hours of work do I still have to do? Workers are obsessed with time: the time already passed, the time to make a decision and, of course, how long until we can go home? It is surprising that researchers so seldom use and discuss this incessant preoccupation and evaluation of time, in the form of timekeeping, controls, and planning, even though it is at the center of workers' interactions. (Peneff 1995, 122)

Since people use that sort of information and take it seriously, we should too. Geer, Hughes, and I did when we noticed that undergraduates, preoccupied with grades, spent a great deal of time calculating and recalculating how their grade point averages would vary under differing allocations of effort to different courses. "Let's see, German's a five-hour course, so if I spend time on that my average will go up more than if I study anthropology, which is only three hours." (See the example in Becker, Geer, and Hughes [1968] 1994, 89–90).

So . . . don't ignore things because the people you're studying do. But don't ignore things that they pay attention to either. This may be as good a place as any to remark that it's not as contradictory as it seems to recommend tricks that seem to be at cross purposes, as these last two seem to be. Remember that the point of the tricks is to help you find out more, and that each may work in its own way, pointing you in a direction the other might ignore. Consistency in the midst of the search is no great virtue.

Using Other People's Information

Social scientists very often use information other people and organizations have collected and, as a result, leave out of account whatever those people left out. We don't have the resources of time, money, and personnel available to the United States Census Bureau and have to rely on them for all sorts of information. As a result, we leave things out because the people whose information we're using don't think it's important, even if we do. Or the constraints on their activities prevent them from getting something we want. As Bittner and Garfinkel (1967) explained, people and organizations collect information for their own purposes and under their own system of assessing practicality. They don't gather information so that social scientists can do research with it. So they don't collect all the facts we'd like to have and it's a lot of work for us to do it. Ever since the 1920s, when a lawsuit based on the religious establishment clause of the Constitution put an end to the collection of data on religion by the U.S. Census, estimating membership in various religious groups has been a research nightmare. Much ingenuity and great effort have gone into devising indirect methods of finding out how many Jews or Catholics or Baptists there are, but none of them can approach the breadth and comprehensiveness of the Census. Too bad for us.

Sometimes collecting the data that others haven't collected for us is so expensive and requires so much work that we just don't do it. They don't get it for us, and we don't get it for ourselves, not because it isn't worth having, but because having it is "impractical"—that is,more expensive than the people who pay for such things are willing to pay for.

Following the lead of Bittner and Garfinkel, and of those who have been worried about the inaccuracies of police statistics (a favorite source of data for studies in criminology) and medical records (a favorite source of data for investigators of health problems), a field of sociological research has grown up that deals, exactly, with the sociology of record keeping. This research looks into how records are kept, not as a way of correcting their deficiencies as data sources, but because keeping records is a commonplace activity in most contemporary organizations; to understand how the organizations work you have to know how the records are kept. But knowing that means that you know too much to take them as accurate sources of information for social science purposes. We want full description. What we get is partial description for practical organizational purposes. If we know that police statistics are kept with one eye on how insurance companies will use them to set the price of household theft insurance, and that householders complain to elected officials when their insurance costs more for that reason, we know that police statistics on theft will probably reflect such political contingencies to some degree.

The inaccuracy of every sort of data gathered by others is a very large area of scholarly activity, and I will not try to cover it here. That's another book. Some work deals with the simple fact of inaccuracy: for instance, Morgenstern's (1950) classic dissection of errors in economic statistics. Some of it deals with conceptual problems, as in Garfinkel's questioning of Census data on sex on the basis of his study of a transsexual: how do you classify someone who does not exactly fit into any of the standard categories? Garfinkel, of course, dealt with a rare situation, though he was correct to say that the Census had no idea how many people wouldn't fit into the categories, since they made no independent investigation. Some researchers describe the way the information is not what it ought to be as a result of the work routines of the data gatherers (for instance, Roth 1965, Peneff 1988).

All these investigations of problems with "official" or quasi-official data

interest us here because every such problem means that we are losing some information that, if we knew it, would help us recover the cases we need for the complete descriptions that help us get around conventional categories. Since we often rely on such data, no matter what our criticisms and distrust of it (no social scientist can do without the Census, for all its faults), we need a trick for dealing with it. The trick is easy. Ask where the data come from, who gathered it, what their organizational and conceptual constraints are, and how all of that affected what the table I'm looking at displays. It makes rather more work out of consulting a table than you might think necessary, but there is too much trouble built into other people's data to run the risk of not making that effort.

Bastard Institutions

All these obstacles to researchers seeing what is there to see, and using it to enlarge the range of their thinking, can be remedied, and I have suggested a lot of tricks for doing that. The best way of avoiding these errors is to create a more general theoretical understanding of the sociology of making distinctions between what's appropriate and necessary for social scientists to include as they construct their synecdoches. Everett C. Hughes's classic paper on "bastard institutions," a small masterpiece of sociological theorizing (Hughes [1971] 1984, 98–105), shows how conventional choices of appropriate material for sociological analysis rule out a whole range of phenomena that ought to be included in our thinking, and thus make our sample of collective human activity a less accurate synecdoche than it ought to be.

Hughes begins by defining a very general problem of social organization: how institutions define what will and won't be distributed within a given category of service or goods:

> Institutions distribute goods and services; they are the legitimate satisfiers of legitimate human wants. In the course of distributing religion, play, art, education, food and drink, shelter, and other things—they also define in standard ways what it is proper for people to want. The definition of what is to be distributed, although it may be fairly broad and somewhat flexible, seldom if ever completely satisfies all kinds and conditions of men. Institutions also decide, in effect, to serve only a certain range of people, as does a shop that decides not to carry out-sizes and queer styles of shirts. The distribution is never complete and perfect.

Some institutions result from collective protest against these institutionalized definitions—the protest, for instance, that a religious sect makes against the definition of acceptable religion promoted by an official clergy or the protest made by the variety of groups which established new kinds of educational institutions as a reaction to the conception of education established by the classical New England colleges. But there are also:

> . . . chronic deviations and protests, some lasting through generations and ages. They may gain a certain stability, although they do not have the support of open legitimacy. They may operate without benefit of the law, although often with the connivance of the legal establishment. They may lie outside the realm of respectability.
>
> Some are the illegitimate distributors of legitimate goods and services; others satisfy wants not considered legitimate. . . . All take on organized forms not unlike those of other institutions. (Hughes [1971] 1984, 98–99)

Hughes suggests calling these *bastard institutions.* They take a variety of forms. Some are not formally legitimate but are not necessarily illegitimate either, though they may be. They are highly conventional and supported by popular opinion, but only within a subcommunity. He has in mind here such informal forms of justice as kangaroo courts in prisons and armies or tong courts in the Chinatowns of another era, but also the institutions Orthodox Jewish communities developed to insure a supply of properly slaughtered kosher meat for their members.

Some are marginal to more legitimate distributors of services. So, right alongside the schools that teach law and accounting are cram schools that teach people how to pass the examinations the state uses to decide who will be allowed to practice those professions. These schools don't pretend to teach law; they teach test-passing. Hughes puts in this category the communities that make available what nearby communities forbid. He loved to point to George Pullman's model community in Chicago, built in the 1880s for the men who worked for him making sleeping cars for railroads. Pullman, who took his version of religion seriously, allowed no taverns in his model town. No problem for the workers. Just across South Michigan Avenue, Pullman's western border, lay Roseland, a mile or so of taverns that provided the cigarettes, whiskey, and wild women unavailable to the east (a specialty that continued into the 1940s, when I occasionally played piano in those same taverns).

In the clearest cases, well-established institutions provide forbidden

goods and services for which there is a permanent and substantial market, such as illegal gambling casinos, speakeasies in areas where alcohol cannot be sold legally, and whorehouses of various kinds. Or it might be that there are things that are fine for other people to have, but not available in any appropriate way for people like you. Transvestites who wish to dress in women's clothes find it easy to shop where the clerks expect to sell dresses, pantyhose, and garter belts to six-foot-tall, two-hundred-pound men. As Hughes says of establishments like this:

> They are in direct conflict with accepted definitions and institutional mandates. [They offer] a less than fully respectable alternative or allow one to satisfy some hidden weaknesses or idiosyncratic tastes not provided for, and slightly frowned on, by the established distributors. Still others quite simply offer a way to get something not easily available to people of one's kind in the prevailing institutional system. They are corrections of faults in institutional definition and distribution. ([1971] 1984, 99)

Social scientists have typically studied such phenomena as "deviance" as pathological, abnormal behavior whose special roots have to be uncovered, so that "society" can act effectively to rid itself of the "problem." Hughes, however, wants to include them as "part of the total complex of human activities and enterprises . . . in which we can see the [same] social processes going on . . . that are to be found in the legitimate institutions" ([1971] 1984, 99–100). He connects the legitimate and illegitimate forms of activity this way: "The institutional tendency is to pile up behavior at a modal point by definition of what is proper, by sanctions applied against deviating behavior, and by offering devices for distributing only the standardized opportunities and services to people. But while institutions cluster behavior, they do not completely destroy the deviations."

So, for example, marriage is the modal way of organizing sex and procreation, but some people don't marry and some who do don't confine their sexual activity to legitimate mates. Every society defines a form of marriage (among other things, a device for distributing men among women, and women among men) as involving people whose specific social attributes (for instance, race, class, and ethnicity, but there are others) make them "appropriate mates." But people's ability to take care of mates varies, and the way people move around and often congregate in relative isolation creates situations in which, for many people, there are no suitable marriage mates available. The classic examples are the heroines of Jane

Austen novels, on the one hand, and the men who work in logging camps or ships or mines far removed from the conventional communities in which they might find appropriate mates, on the other. Prostitution and temporary homosexual relationships have been common solutions to the male version of the problem, as the quietly lesbian relationships of middle-class women who "shared an apartment" were at one time for the female version.

So far, the analysis is interesting but not surprising. Other social scientists (e.g., Kingsley Davis 1937) have used similar examples to make similar points. Now Hughes produces a surprise. Deviation moves in two directions, takes two forms, and the social scientist should look at and discuss not only the illegitimate and frowned on deviation (he calls it the direction of the devil) but also the angelic form. Prostitution works to provide scarce women to men, but there is no corresponding device to supply men for women when the imbalance is the other way. So many women who would prefer not to be in that situation have no legitimate male partner (in whatever way legitimacy is defined).

The point, for Hughes, is that the workings of conventional institutions put some people in a position where they are required to be "better" than they want to be or than anyone has a right to expect them to be. "It would be especially important to find out at what points there develops an institutionalizing of adjustments to the position of being better than one wishes" ([1971] 1984, 103).

The institutionalizing of celibacy in the name of religion is the

> realization in institutional form of deviation from marriage in the direction of the angels—a deviation rationalized in the terms of supposedly supreme values, the higher-than-normal ideals of human conduct. For the individual in such an institution the function may be clear; these institutions allow one to live up to some ideal more nearly than is possible out in the world and in marriage. I emphasize the word *allow,* for the world would merely think a person queer to so live without special declaration, without attachment to an ongoing body devoted to this special deviation. . . .
>
> . . . The institutions of celibacy offer a declared, established, and accepted way of not accepting the modal norm of behavior; perhaps a nobler and more satisfying way of accepting the fate that a fault of distribution in existing institutions condemns one to. They may be considered also as institutional provision for those highest lights of idealism that, although engendered by the established teaching of the virtues, are not provided for in the modal definitions to which institutional machinery is generally geared. Let it be noted, however, that society very often accepts such deviation in an organized institutionalized form, when it would scarcely accept it as isolated individual behavior. . . . The individual deviation may appear as a threat to the whole accepted system; the organized deviations, however, may appear as a special adaptation of the system itself, perhaps as a little special example of what humans are capable of. ([1971] 1984, 103–4)

So, Hughes points out, a classic form of heresy is the demand that everyone live up to some commonly proclaimed virtue:

> Society idealizes, in statements and in symbolic representation, degrees of virtue that are not in fact realizable by all people or are not realizable in combination with other virtues and in the circumstances of on-going real life. It appears that society allows some people to approach these levels of one virtue or another in some institutionalized form that will at once provide the spiritual lift and satisfaction of seeing the saintly example before one, without the personal threat that would come from mere individual saintliness offered as something that all of us should seriously emulate and the social threat of a contagious example. ([1971] 1984, 104)

Sociological analysis should then, according to Hughes,

> take some matter, some aspect of human life, which is highly institutionalized and is the object of much moral sanctioning, and . . . treat the whole range of behavior with respect to it: the institutionalized norms and the deviations in various directions from the norm. . . . We have seen the norm, the institutionally defined and distributed relations between adult males and females, as a special point in the fuller range of possible and actual behavior, and have at least indicated some possible functional relations between the instituted and the deviation in both the bastard and the angelic directions. ([1971] 1984, 105)

Treating the full range of cases, then, means including what we might otherwise leave out as in some way too weird or raunchy for proper sociologists to consider. It also means using such cases to define and point to the other end of the scale, those activities that are too good to be true, the angelic deviations. In Hughes's hands, this often takes the form of comparisons that seem shocking or highly improper. He liked, for instance, to compare priests, psychiatrists, and prostitutes, noting that members of all three occupations have "guilty knowledge," that they know things about their parishioners, patients, or customers that have to be kept secret. Hughes was interested in a comparative study of the means by which, under the differing conditions in which the members of each profession worked, those secrets were kept.

Leaving cases out because they seem tasteless or politically discomfiting is equally guaranteed to be a mistake. Good taste is a potent form of social control. Nothing is easier than to get someone to stop doing something we don't like by suggesting that it is "cheap" or "not cool" or "gauche" or any of a hundred similar put-downs. The Russian literary critic Bakhtin pointed out that Rabelais told his tales of Gargantua's carryings on in common vulgar language precisely because it was politically offensive to the educated folk who would have preferred a "more elevated" tone. We are likely to be responding to someone's exercise of social control when we unthinkingly accept such criticism, and social scientists often do.