4

CONCEPTS

Having worked on our imagery, and having looked for a proper sample of cases to investigate, a sample that covers the full range of types of the phenomenon we want to learn and think about, we're ready to start the thinking in earnest. That means using concepts, generalized statements about whole classes of phenomena rather than specific statements of fact, statements that apply to people and organizations everywhere rather than just to these people here and now, or there and then. Many social scientists work at these problems deductively, treating concepts as logical constructs that can be developed by the manipulation of a few basic ideas. I'm not very sympathetic to these efforts, which are too divorced from the empirical world to keep my attention. I recognize this as, in some respects, an issue of taste.

A fruitful and more empirical mode of conceptual analysis has been to develop ideal typical models, which consist of "a systematically related set of criteria surrounding a central issue" that is "sufficiently abstract to be applicable to a variety of national and historical circumstances" (Freidson 1994, 32). Using this method, for instance, Freidson solves the thorny problem of defining the concept of "professional power" by creating a model in which "the central issue of professional power lies in the control of work by professional workers themselves, rather than control by consumers in an open market or by the functionaries of a centrally planned and administered state."

But my favorite way of developing concepts is in a continuous dialogue with empirical data. Since concepts are ways of summarizing data, it's important that they be adapted to the data you're going to summarize. The discussion that follows describes tricks for doing that, ways of using your data to create more complex ideas that will help you find more problems worth studying and more things about what you have studied worth thinking about and incorporating into your analysis.

Concepts Are Defined

We all work with concepts. All the time. We have no choice, as Herbert Blumer pointed out in a critique of what was called, when he wrote, "operationalism." He noted that you could not have a science without concepts. Without concepts, you don't know where to look, what to look for, or how to recognize what you were looking for when you find it. Psychologists, in their heyday when Blumer wrote, thought they could do without concepts, at least concepts defined in abstract theoretical terms. They thought they could avoid such chronic troubles as arguments over definitions by defining concepts simply, as what they measured by the operations they used to study the phenomenon they were investigating. In the classic example, they said that "intelligence," whose definition was hotly debated then as now, was what intelligence tests measured.

Sociologists equivocated in the same way about the concept of *attitude.* Many researchers assumed that people had thoughts or dispositions or ideas (or something)—summarized as attitudes—inside them, waiting to be released by the appropriate stimulus or situation. What an attitude was wasn't clear. Scientists argued about the definition. But their inability to define an attitude didn't prevent them from inventing attitude measurement, a procedure in which people's answers to a long list of questions produced a number that "measured" their attitude toward movies or foreigners or schools or political parties. The scientists measured the reliability and validity of attitudes, and concocted statistics that described the relations of attitudes to one another and to other facts about people. They thought they could show that people differed with respect to attitudes about this or that, and that those differences correlated with other differences in ways that seemed meaningful.

Critics complained that there was no general understanding of this thing that was being measured. Operationalists evaded those complaints by denying that they had said anything about the actual content or meaning of the measured attitudes: attitudes were just what the tests measured, nothing more. No one believed that. If they had, there would have been much less research on attitudes or intelligence or the other important ideas that were defined operationally. Because, after all, no one really cares about test measurements in themselves—only about intelligence or racial attitudes or propensities to violence or whatever the test is supposed to measure.

A favorite reply to attacks on attitude or intelligence tests was, "You

don't want to call it intelligence? Fine! Call it X. OK?" You could deflect this irritating and unsatisfactory riposte by actually referring to the item in question as X. "I see, you've shown that children of different racial groups differ by ten points, on the average, on something called X. So what?" But, of course, no one cares about the differential scores of black and white children on X. Without content, X has no relevance to any question of theory or policy. But people do care about differences in intelligence because, if they really exist, they have serious political and moral consequences of a kind something that is just X could never have. By the third time a critic called X what everyone involved knew was really intelligence, the discussion would get more serious.

This critique may seem quaint and out of date, since few contemporary social scientists would admit to being operationalists of the kind Blumer criticized. But many contemporary researchers act as though they'd accepted a variant of that position. In this sense: they choose, as an "indicator" of the phenomenon they want to talk about, something that has an imperfect, sometimes a highly imperfect, relation to the phenomenon itself, and then treat the indicator as though it were that phenomenon. They ask people their occupation and treat the answer as a measure of social class, by locating the occupation named in a list of jobs whose prestige has been measured or placing it in a Census classification of major occupational groups. They may say that they are measuring what Karl Marx or Max Weber or W. Lloyd Warner or C. Wright Mills meant when they spoke of "social class," but that's neither obvious or particularly believable. People who make such measurements don't insist that a person's occupation *is* social class in the Marxian or Weberian sense, since they haven't demonstrated any relation between the two empirically, but their analyses and discussions implicitly assert the identity. Important as measurement may be, it doesn't do much for our understanding of the concepts we use.

Another way of defining a concept is to collect examples of things we recognize as embodying what the concept refers to, and then look for what the inevitably messy and historically contingent ideas people routinely use have in common. Some common sociological examples of such conceptual work are skill, crime, or profession. We try to formulate a definition that includes all the things we think are alike and leaves out those that are different. We are embarrassed if someone can show that something we didn't think belonged in our collection in fact fits the terms of the definition. Thus, researchers tried to define a "profession" as a special kind of

FOUR

work, different from other occupations. What they wanted to include in the aggregate their definition collected were such highly respected and well-paid occupations as medicine and law. So they framed their definition by listing the traits that characterized those occupations. (Freidson 1994 gives a careful account of these problems, and offers realistic and useful solutions to them.)

Invariably, an industrious and clever critic would find an occupation that fit all the definitional requirements (long years of training, a body of esoteric knowledge, state licensing, and so on) but clearly "didn't fit." Plumbing used to be good for this bit of theoretical skullduggery. Plumbers have the attributes included in standard definitions of a profession: an esoteric body of knowledge (try fixing your own drains), long years of training, state licensing, and the rest. But "everyone knows" that plumbing is not a profession. The seeming paradox arises because the items in the collection the definition is framed to cover have been chosen on the basis of an unacknowledged variable: the social prestige of the occupation. If prestige correlated perfectly with the other criteria, there would be no problem. But it doesn't.

Such problems arise in many areas of sociological work. The theoretical trick that helps solve the problem is to recognize that what goes into the collection the definition has to cover governs the kind of definition we come up with. And collecting the examples is the kind of sampling problem considered in chapter 3. So we look for answers to such questions as: How do we make up those collections? What do we typically leave out? And what harm does it do to be selective in our choice of examples? Definitional problems arise exactly because we have chosen these collections in ways that ignored the injunction of chapter 3 to include the widest possible variety of cases of a phenomenon in our sample. Here are two further examples where the harm is more substantial, or at least more easily seen, than in the case of "profession" (which is, at least on the surface, mainly a conceptual embarrassment, though the policy implications of the definition of that term are quite serious, as Freidson [1994: 149–216] shows).

Skill

Sociologists, economists, and other social scientists rely, implicitly or explicitly, on the idea of "skill." They argue that differences in pay, for instance, result from the scarcity of real skills, so that people who have rare

skills get paid more. What would make a skill scarce? One thing would be the differential distribution of natural talent to exercise the skill. People who are tone deaf would find it difficult to learn to play hundreds of songs by ear, as I had to do to hold a job playing the piano in taverns. Some people can manipulate numbers easily and might be especially good at accounting, keeping books, or managing people's money. Some are very skilled with a needle and can sew or knit or crochet excellently. Some have a way with people, know how to ease their fears or make them feel at home. Some have learned decisiveness and are good at it; they can make up their minds in a difficult situation while the rest of us stand around sucking our thumbs.

Another contributor to the scarcity of a skill might be how long you have to work or how much you have to pay to acquire it. People, on this theory, wouldn't invest so much time and energy they might invest elsewhere if it weren't going to pay off. So the number of people willing to acquire the skill will go down if the rewards for selling it are low. If everyone acts in this economically rational way, the number of people in each occupation will reach an equilibrium at a price users will pay for the skill and practitioners will accept.

We can certainly make a long list of skills people have had over the centuries. It's clear, when we inspect such a list, that not all skills are equally rewarded. Skill alone does not produce big rewards. You need a skill that someone else, who can and will pay for it, wants. If you have a very rare skill that very rich people want badly, you will be rewarded handsomely. If, for instance, you are one of the few people who can repair damaged art works owned by wealthy people who prize them highly, you will be paid well to exercise that skill. If you have a skill many other people have—if you are one of the millions who can quickly be taught to cook hamburgers at a fast-food franchise, a group of whom there are more members than anyone needs—you will be paid the lowest legal wage (or less if the bosses think they won't get caught). But even a very rare skill won't do you any good unless people wealthy enough to pay for it at the rate you'd like really want and need it. My ability to play hundreds of tunes wasn't worth much, because the only people who wanted it were band leaders and tavern owners who could, if it got too expensive, manage with pianists who knew a lot less than I did.

The demand for skills varies historically. Temporary conjunctions of circumstances can raise the value of skills ordinarily not worth much.

Hobsbawm (1964) described the unlikely victory of a group of "unskilled" laborers in the great London gas strike of 1896. London, at that time, was lit largely with natural gas, manufactured by coking coal—that is, by heating coal in large furnaces so that the gas it contained would be released to be captured and piped to households and factories. Running the furnaces—shoveling the coal in and keeping it burning—was unskilled labor. Anyone could do it. It had never required any special training, other than what you got on the job. So, when the laborers who did this work went on strike, conventional wisdom and economic theory alike said it was unlikely they could win.

But they did win the strike, and got a handsome settlement from their employers, who were as greedy as capitalists are supposed to be. How did the workers win? Hobsbawm shows that these unskilled laborers actually had some very important skills and that an unusual conjuncture of circumstances at the time of the strike had made those skills more valuable to the employers than they ordinarily were. Put the question this way: why didn't the employers just go out and hire some other unskilled men to shovel coal into the furnaces? Why didn't they just wait the strike out, manipulating public opinion to make their stubborn employees look responsible for the discomfort householders were suffering and thus bring them to heel?

There were several reasons why employers didn't take these obvious steps. The sellers of gas were facing new competition in the form of electricity. Still a novelty, electricity was potentially just as good a way to light your house and, if a strike went on for a while, customers might be tempted to experiment with the new form of energy. The longer the strike went on, the more customers the purveyors of gas would lose to electricity.

Further, the employers couldn't replace these unskilled laborers as easily as you might have thought. To be sure, what they did required no great schooling. But the machines they tended, while not highly technical and thus not requiring, say, engineering knowledge to run, were old and crotchety. The gas manufacturers had been coasting, collecting their profits and not maintaining the machinery any more than absolutely necessary. So the machines worked but, like all old machines, had to be coaxed. You had to know when to give the furnace a good kick, and where to kick it. These might not be skills in the conventional sense, but if the men who shoveled the coal didn't have them the furnaces didn't work. The bosses could hire other unskilled workers but, lacking that special knowledge, the new men couldn't do the job.

That combination of circumstances gave these unskilled laborers some skills that were at least temporarily valuable, and they used their advantage skillfully to win higher wages. The important lesson for us is that the identical ability may be skilled or unskilled, depending on circumstances. The meaning of the concept of skill depends on which cases you have in mind when you define it.

So skill, if you want to raise your wages by withholding it, must be a skill that someone with money wants. Suppose you have the skills, and they are scarce and people want them, but those potential purchasers of your services would rather not pay you as much as your skills might be worth on the open market. This, I take it, is the point of research and work on what is called "comparable worth." Here's the problem: many people think women have been historically, and still are, discriminated against in the labor market. A great variety of statistical studies show that employers pay women less than men any time they can get away with it. And who can blame them? Capitalism, as Marx said, is a tough system and employers who pay more than they need to for components of their products will soon be driven out of business by shrewder manufacturers who can sell the same product cheaper.

The gas worker example sheds some light on this problem. Suppose the law finally forbids out-and-out discrimination on the basis of gender; women must be paid what men doing the same job are paid. Women will still make less. Now why? Because the distribution of men and women across occupations is skewed. No women play major league baseball and very few nurses are men, and ball players make a lot more than nurses. A disproportionate number of schoolteachers are women; a disproportionate number of corporate executives are men. If you pay all nurses, whatever their gender, the same, and pay all executives, men or women, the same, but pay nurses less than executives, women will end up making less on the average because more of them are in jobs that don't pay as well.

How can that inequity be remedied? Some reformers have attacked the way pay scales are set (it is primarily governmental agencies that are vulnerable to such attacks), noting that salaries are set with reference to the skills allegedly required to do the work, but that skills important in "women's occupations" (that is, occupations most of whose members are women) are either ignored or not valued highly in such evaluations. If technical skills are valued more highly than the skills necessary to deal with complex social situations, and the jobs women are more likely to have—

like nursing and teaching—require fewer technical skills and more "human relations" skills, then women will be paid less even though they are just as highly skilled, although in different areas.

Of course, proponents of the status quo will argue that it can't be shown that the skills are commensurable. But that, of course, is the point. If they aren't, it's because we haven't agreed on how to measure skill. And if that's true, then how do we know that men's skills are worth more? And it's just that judgment that is embodied in the very wage scales being attacked.

I've been a long time getting to the conceptual point, because the point lies in the kind of examples I've given, not in abstract talk. The point is that concepts presuppose that you have inspected the full range of things they cover when you formulate and define them. Now we can see one of the reasons for my earlier emphasis on methods of sampling that produce examples of that range. If you leave some phenomena out because of conventional prejudice or for any of the other reasons I discussed there, your concepts will be flawed. Generalizations of which those concepts are components will contain a lot of noise, random variation that isn't random at all, but rather the result of systematic social biases in the selection of cases you used to define your concepts.

Crime

The same reasoning applies to the well-known phenomenon of white-collar crime. Why did Edwin Sutherland find it necessary to devote his presidential address to the American Sociological Association (1940) to the subject of white-collar crime? Because he wanted to accuse his colleagues of a conceptual error that had a similar basis in inadequate sampling based on conventional, socially approved prejudice. Criminological journals and books, at the time Sutherland delivered his blast, were filled with theories about crime and research on crime. What was crime, this thing all the theories and research were about? Activity that violated the criminal law. That seemed fair enough. The mountains of research that had been done showed that crime was highly correlated with poverty, with broken homes, and all the other conventional indices of what was then called "social pathology." Sutherland asked a simple question: how can that be true when there are crimes being committed by very well-to-do people who do not exhibit the conventional signs of social pathology, and by the largest

and most respected corporations in the country, which similarly did not come from broken homes?

The answer to that was simple enough. No one, no conventional criminologists certainly, thought the crimes well-to-do people and corporations committed were, in some fundamental way, "really crimes." Besides, the culprits involved were seldom convicted of criminal violations, because these cases were often settled as civil suits. If there were no criminal convictions, how could there be any criminals? The government was typically more interested in getting the bad guys to stop their mail frauds and security swindles and forcing them to pay off those who had been cheated than in sending anyone to jail. But that was not a natural consequence of the nature of the crimes, which could just as well have been prosecuted under criminal statutes, and occasionally were. It resulted from judgments made by prosecutors, who exercised the discretion the law gave them as to whether to pursue criminal or civil remedies.

Prosecutors had other reasons for not pushing for criminal convictions. As Katz's later (1979) research showed, white-collar crime and crimes of the more conventional kind differ in another important way. In ordinary crime, there's no question that a crime has been committed. Someone has been robbed or assaulted. The question is: who did it? In white-collar crimes, on the other hand, there's no question about who did it. The big grocery chain *did* label meat that weighed 14 ounces as weighing one pound. The question is not who did it but rather is it a crime or not? Such a thing, after all, might have happened because a scale was faulty and the company didn't know about it, or because a crooked butcher was skimming some of the profit for himself, or for any of a number of reasons that would show that the company lacked criminal intent. So, for both sets of reasons, white-collar criminals are convicted of crimes far less often than common criminals.

Sutherland's impeccable reasoning was that if you decided not to include the crimes rich people and corporations committed when you calculated your correlations, you guaranteed the result that crime was correlated with poverty and its accompaniments. Not because it really was, but because you were using a flawed concept, one that pretended to contain all members of a given class, but actually left out a large number of those members on the uninspected grounds of social prestige. You didn't have an empirical finding, you had a definitional artifact.

Defending against Sutherland, conventional criminologists argued, essentially, that "everyone knew" that those rich people and corporations weren't "really criminals." That is, if you accepted the conventional notion of what a criminal was—a tough guy with a mask who jumped out of the bushes, stuck a gun in your ribs and took your money, a guy who made a career of crime, lived a life of crime, shared the culture of crime with others like him (and these criminals were, in conventional thought, male, of course)—then it was clear that the nice people who wore suits and ties and took your money in broad daylight over a desk in a fancy office, and the organizations in whose buildings those offices were situated, didn't look like that at all. They might take your money, but not with a gun; in fact, the way they did it you might not even know you had been robbed unless someone pointed it out to you.

Sutherland arrived at his understanding of white-collar crime by using a trick based on a common feature of organizational life. As I suggested in the discussion of sampling, organizations typically tell lies about themselves. If that's too harsh, we might just say that they like to put their best foot forward, and prefer not to mention things that would make them look bad, especially when those events and activities can plausibly be interpreted as random deviations or character flaws attributable to individuals, things that are in any case beyond what anyone could reasonably expect the organization to guard against. It's the general explanation police departments give when any of their officers get caught misbehaving: "There's a few bad apples in every barrel." This explanation is designed to counter any suggestion that would accept the more sociological hypothesis that the barrel makes the apples rotten—that is, that the department's organization and culture might lead officers who would otherwise be law-abiding into bad ways.

Social scientists will be led astray if they accept the lies organizations tell about themselves. If, instead, they look for places where the stories told don't hold up, for the events and activities those speaking for the organization ignore, cover up, or explain away, they will find a wealth of things to include in the body of material from which they construct their definitions. Sutherland's trick was simple. He looked for facts corporations might not put in their annual reports: the civil suits against them and the settlements they had made of such claims; and the violations of criminal law sociologists did not count because corporations had managed to avoid criminal prosecution, instead settling them as matters of civil law.

When you find events and facts that are not accounted for in the stories conventionally told about a class of organizations, you have usually found a new element or "variable" that needs to be incorporated into the definition of the phenomenon under study. A more general version of Sutherland's trick produces the labeling theory of deviance (see for example Becker 1963). In this way: the conventional story about deviance is that the organizations responsible for dealing with it actually do deal with it effectively. They may not prevent it from happening—police departments may not be able to control every rogue cop—but once it is known to have occurred they find it and punish it. Corporations may not be able to prevent employees from cheating customers, but they track down and punish the cheaters. And so on.

But when you discover that not all deviations are tracked down, and that the selection of which ones to track down is not random, you have good reason to think that you have found another element in the puzzle—namely, a step in the process of detection and punishment that consists of not detecting some people or not punishing some that have been detected. You thus know that "deviance" includes both a possible infraction of a law or rule, and a process of acting in some fashion against whoever might be thought to have committed the infraction. When Sutherland saw that some who committed crimes were not treated the way others were, he knew he was onto something.

Keep in mind that what Sutherland saw was not much of a secret. Every organization enforces the rules it is responsible for in a partial and discretionary way. Sutherland's originality consisted in making that discretion the subject of study. (I'll return to this separation of rule-breaking from the perception and punishment of rule-breaking in chapter 5, when we look into the uses of combinatorial logic for social research.)

All these examples show that concepts that don't cover the full range of cases to which they allegedly apply are flawed. Generalizations that include flawed concepts as terms in the explanatory equation will not explain everything they claim to apply to, as explanations of crime based on juvenile delinquents' activities could not explain the crimes of large corporations. Including the full range of cases forces us to revise our generalizations, make them more complex and more interesting. Then, containing less noise and less unexplained variance, they will explain more of what they are supposed to explain.

The trick here, to repeat, is recognizing that the definitions of concepts

rest on what the examples they are based on have in common. However abstract (or "theoretical") the resulting definition is, it bears the marks of that often uninspected selection of cases. That's why I've insisted on the necessity of striving for imagery that enlarges our ideas about what might be present in the world we study. If our imagery is based on a biased sample we will have trouble. If we systematically look for excluded cases, our work will improve.

Defining Concepts: Some Tricks

To review our results to this point: we define concepts (as opposed to discovering their true nature), and our definitions are shaped by the collection of cases we have on hand with which to think about the problem. Suppose we have gathered a good collection of cases and want to proceed with creating a useful concept. How do you do that? It's true that it takes some imagination and some free associating and some consulting of what others have said in the past, but you can do all that and still not know how to create a concept. What do you actually do?

Social scientists ask themselves this question when they begin to gather data without having much sense of what the problem they are studying actually is. That happens more often than we would like to admit. It happens, for instance, when we agree to study a "practical" problem, a problem defined by its importance to the people involved in it. (Since so much research is funded because the problems are practically and politically important, this situation is common.) "Are black students getting a fair shake in education?", however any of those terms is defined, is not a question framed in sociological terms. That's not to say it isn't important or interesting, but rather that when we study it we will have to turn it into a sociological question before we have anything distinctive to say about it. But we don't know what that question will be, not yet. We only know it after we see what kinds of organizations, institutions, and processes are involved in the production of that problem (what kind of a machine is operating to make things happen that way), and only our research will tell us that.

So we find ourselves with a pile of data, trying to figure out what it could be about, sociologically. Students who find themselves in this fix often say they want to "narrow their problem down," a ritual phrase some teacher taught them to say to ward off getting in over their heads. For students, but

not only students, that also means finding a way to say something that will be defensible against all attacks; if they make the "problem" narrow enough they can find out all about it, nail it down, and none of the vague enemies they sense around them can get them. (I've discussed those fears in Becker 1986b.)

Students learning to do fieldwork commonly suffer from this disease. They finally get their nerve up to interview someone and then don't know what to ask. When they observe some social situation, they aren't sure what constitutes their "data," which of the things they see and hear they are supposed to write down. That's because they don't know what their problem is, what they're studying. They know they have to do it, so they put anything down. Or so it seems. As a result, their notes are scattered, essentially incoherent; their interviews wander because they don't give the people they are talking to any systematic guidance about what they would like to know.

But there is some order to what they have done, because you can't make the simplest decisions unless you have some idea as to what you are doing. The students' imagery of people and places and situations like the one they're examining has led them to do whatever they did, ask what they did, attend to what they did, ignore what they did. They now have to find out what they had in mind that led them to do all that. The problem is to uncover the imagery that got them into this fix.

My trick here is a version of an old parlor game. In the game someone says, for instance, "Nine Wagner." The object of the game is to imagine the question that is the answer to. In this case, the question that elicits that answer is "Who wrote that piece? Mozart?" And the answer (I took liberties with the spelling) is "Nein! Wagner!" So, trying to figure out what you are doing, you say to yourself, "The data I have here are the answer to a question. What question could I possibly be asking to which what I have written down in my notes is a reasonable answer?" I ask students to reread their notes with this in mind, to pretend that they did everything they did purposefully and have succeeded in doing just what they set out to do. Now they will find out what they did.

The exercise generally makes students unhappy. They see that, whatever vague idea they had in mind when they began their work, they didn't get anywhere near doing it. Unspoken assumptions and unacknowledged imagery—about the problem, but more likely about what they can reasonably expect in the way of cooperation from people—have led them to

investigate topics they didn't have in mind and didn't care about, usually very minor and superficial matters whose virtue was that they came to mind during a lull in the conversation. The students wanted to know about patterns of social organization but, under the pressure of performing as knowledgeable researchers when they knew they weren't any such thing, they asked the people they interviewed and participated with about trivia. They want to know about unrest among the factory workers they are observing, but they have only talked to them about the food in the company cafeteria or last night's football game on television. And they know that's not it. They didn't do what they should have done to find out what they wanted to know.

I tell them not to be unhappy. Now they know what they were "actually investigating," what their first attempts actually asked about, and they know that what they learned wasn't what they wanted to know. Knowing that, they can change direction, reformulate their questions, and have something different to put in their notes. Their data are now more likely to be about what they want to be investigating. And, if it appears they may not be able to see something they think it's important to see or ask about something they think it's important to ask about, they can consider alternate ways to get at what interests them.

Their reformulated questions constitute the beginnings of conceptual construction. They see what they aren't interested in and don't want to know about. They usually don't find this very thrilling and think they have just wasted their time on a wrong lead. But they haven't. They can only say that X *doesn't* interest them by having some notion of what *would* interest them. Naming the object of interest is the beginning of conceptualization.

I've made it sound as though this trick could only be done by sociologists who work with qualitative data, unfettered by research designs, able to keep changing their minds as they do their research. In fact, the introduction of microcomputers into everyday sociological life has freed quantitative sociologists from their dependence on mainframe computers, from the long waits those machines inserted between getting an idea, thinking how to test it on your data, and actually getting the results. Freed from the mainframe, quantitative analysis is much more interactive. People run off factor analyses that once took a year of hand calculation in the time it takes to refill their coffee cup. The cost of calculation having been lowered so dramatically, researchers can do analyses just for the hell of it, to see if there is anything to a hunch (Ragin and Becker 1988). And that in turn means

that quantitative researchers too can inspect the answers they have to see what questions they imply. The same tricks will work for them.

Let the Case Define the Concept

This is a slightly different way of exploiting the recognition that concepts are defined. Sociologists, concerned to generalize, want to establish that what they have studied is not the only one of its kind. What good would it be to get sure knowledge about something when you couldn't apply that knowledge anywhere else? This concern is enshrined in the well-known distinction between idiographic and nomothetic sciences. Students especially, I think, want to put their case (the thing they studied) into some conceptual category, for the very good reason that if they can do that then all the justifications for why you should study such things are ready-made and easily available to them.

But there's a problem with that. It's not clear that you can say anything very useful if you focus only on what is common to your case and other cases with which it shares membership in some class. The more seriously you take the case, the harder you try to understand it fully so that there's nothing about it that you have to hide or ignore, the harder it is to see it as being "just like" any other case it might superficially resemble.

Consider this as a choice between letting the conceptual category define the case and letting the case define the category. We let the category define the case by saying that what we have studied is a case of x, let's say of bureaucracy or modernization or organization or any of the other common concepts we use to understand the social world. Doing that leads us (not necessarily, but often enough in practice) to think that everything that is important about the case is contained in what we know about the category. Analytically, then, we just have to inspect the case to see that it has all the attributes a member of that category is supposed to have and thus is one of the things described by that concept. We check, say, to see that our case has all the features Max Weber said a bureaucracy should have. Our analysis is complete when we show that it does have all (or most) of those things, and have explained why it doesn't have the ones that aren't there. We ignore those elements of the case whose presence or absence the category description ignores. This strategy helps us develop theory by adding cases to the collection of examples of the type, and variations to ideas and principles others have developed to explain them.

This is something like the normal science work of articulation described by Kuhn (1970, 27–34).

The more the world, as exemplified in our case, includes just what our concept includes and no more, the better our analysis works. But the world is hardly ever just as we imagined it. In fact, such a rare similarity probably occurs only under some very special circumstances. It occurs, for instance, when we have tailored our concept to fit a particular instance. If I construct a theory of revolution by generalizing from the American or Russian Revolution, then my theory will fit the case I based it on. The world and our concept resemble each other, too, when we have enough control over the world to *make* it exactly fit our categories. Latour explains that science "works," which is to say that its predictions are verified in practice, because scientists change the world until it is just like the setting in which they made their discoveries (1987, 249–50). Louis Pasteur could protect cows from anthrax by vaccinating them only when he could persuade farmers to replicate the essential features of his laboratory on their farms. He says: "Facts and machines are like trains, electricity, packages of computer bytes or frozen vegetables; they can go everywhere as long as the track along which they travel is not interrupted in the slightest" (1987, 250). It is extremely difficult to lay the tracks on which social science can travel. Too many other people have conflicting ideas about how the social world should be arranged to let us arrange it so that our theories will work. So such tracks are best laid in computer simulations and sometimes in laboratory experiments. Unlike Pasteur, social scientists can rarely persuade anyone to turn their real (not simulated) homes or communities into the tracks on which our theories might run.

So the strategy of letting the concept define the case accomplishes a lot, but at a price: we don't see and investigate those aspects of our case that weren't in the description of the category we started with. The things we leave out, however, come back to bother us. Whether we include them in our investigation or not, they are still there and continue to operate in the situation we're studying, almost surely influencing the phenomena we want to understand. It makes sense to include them in our analysis even if our concept doesn't make room for them. Which is the argument for the alternative strategy: letting the case define the category. As in the earlier example, take the American revolution as the model and define a category that has all the attributes (every single one, because we don't know what to leave out) of that case. Anything we find out about the case becomes a cru-

cial part of the concept. What does that accomplish? Can we ever create any generalizations working that way?

Letting the case define the concept lets you define dimensions you might see varying in other cases. You discover that the executives of savings and loan associations sometimes steal money by manipulating banking regulations whose complexity makes it difficult for prosecutors to decide whether what they indisputably did is a crime. That identifies an aspect of "crime" you would not see in cases of assault, where no one doubts that hitting someone with a club is a crime. The generalization that results from your study is that the clarity or ambiguity of an action's criminality, and the things that affect that, are something to include in all future studies of "crime." In a way, the result of working like this is not more answers, but more questions.

Generalizing: Bernie Beck's Trick

I snuck in a move in the above analysis, when I said its outcome was a new aspect of crime to be included in future research—the clarity or ambiguity of an action's criminality. I'll explain what's involved in that move now. Sociologists often know no intermediate stops between the raw facts of the case they studied and the largest, most general categories of social analysis. Thus, they may describe the findings of their research on, say, drinking alcohol, and jump from that to talk about identities or self-conceptions or some other highly abstract aspect of social organization or interaction. As a rule, our research does not have anything very new to say about self-conceptions or identity. Researchers usually use such general ideas to orient their work, to suggest an overall approach and a very general set of questions they might ask. The ideas serve as what Lewontin refers to as "informing and organizing metaphors" whose role is "to bring order into confusion" (1994, 509). What the researchers who use them discover will probably not lead to any reformulation of those general ideas or questions. At worst, the researcher announces triumphantly that what was studied was indeed a case of the development of identity or the adaptive character of social organization. That kind of result isn't useful to anyone. It doesn't add much to whatever warrant the very general theories it is attached to already have. And the general theories don't add much to the specific studies. The advice they offer is too general.

What is useful is the description of something more general than the

particular facts we discovered, but less general than notions of identity or social interaction. Something in between, something like what Robert Merton alerted us to as "theories of the middle range." I moved from the savings and loan convictions to the idea of the clarity or ambiguity of an action's criminality, but I didn't explain how I did that. When I teach field-work, I often make that kind of jump in discussing the possible extensions of a student's findings. This is the aspect of what I do that most often pro-vokes the feeling that some kind of magic trick is being performed, that the way I get from A to B isn't something a person can learn to imitate.

During the twenty-five years I taught at Northwestern, my office was always next door to that of Bernard Beck, one of sociology's great teachers and thinkers, whose qualities are less well-known than they ought to be. I learned more from him than I will ever repay, a lot of it from eavesdropping on his conversations with graduate students about their work in progress. None of what I heard has been more useful to me than his trick for getting to this intermediate level of thinking about a research result. Since he has never published this trick, which has the elegance of simplicity, I'm taking the liberty of borrowing it from him.

Beck says to a student who has gathered some data and now is trying to understand what his or her dissertation research is about, "Tell me what you've found out, but without using any of the identifying characteristics of the actual case." I'll use my own dissertation, a study of the careers of schoolteachers in Chicago, as an example (the results are reported in Becker 1970, 137–77). Had I been a student asking Beck for help figuring out what generalization my research could produce, he probably would first have asked me what I had actually found out about Chicago teachers. I could have offered this conclusion:

> These teachers make their careers by moving from school to school within the Chicago school system, rather than trying to rise to higher, better paid positions, or moving to other systems in other cities, and their moves between positions in the school sys-tem can be understood as trying to find a school in which the people they interacted with—students, parents, principals, other teachers—would act more or less the way the teachers expected them to.

Had I told Beck all that, he would, using this trick, have said to me, "Tell me what your research is about, but now you are forbidden to use the words 'teacher,' 'school,' 'pupil,' 'principal,' or 'Chicago.'" To answer such a

question, I would have to choose words more general than the specifics of my case, but not so general as to lose the specificity of what I found. If I started talking about "identity" or "rational choice" or similar high-level abstractions, I would lose what I had learned about career movements resulting from choices between more and less comfortable work situations. So I might have answered that my study showed how people in bureaucratic systems choose between potential positions by assessing the way all the other participants will treat them and choosing places where the balance will be best, given whatever they are trying to maximize.

That's how I made the move from the fact that banking executives steal to the statement I made about the clarity or ambiguity of an action's criminality. I restated the assertion that "the executives of savings and loan associations sometimes steal money by manipulating banking regulations whose complexity makes it difficult for prosecutors to decide whether what they indisputably did is a crime" without using any of the specifics. I didn't say "executives" or "savings and loans" or any of the other specifics. I said what class each of those belonged to and so ended up talking about the ambiguity of an action's criminality, a dimension that could be useful in the study of any criminal activity. And I could take another step and talk about something less specific than criminal law—rules in general—and that would let me introduce such interesting cases as whether the ball the pitcher throws is a "ball" or a "strike," the rules for deciding that being as ambiguous as any in the criminal law.

You could argue that, after all, baseball and banking don't have much in common. Right. Every time we make such a comparison and find such a similarity, we will also immediately find such a difference. Both the similarity and the difference give us general categories to think about and use in our analyses. The similarity says, by way of generalizing, "Every set of rules is clear to some degree and ambiguous to another degree." The difference says, by way of a different kind of generalization, "Within the organizations (like baseball and banking) in which rules are made and enforced, there are other things going on, such that those rules will vary along a dimension running from clarity to ambiguity." Making such comparisons reveals further complexities in the creation and application of rules, complexities that can be attended to in future research.

The immediate consequence of that result is that every study can make a theoretical contribution, by contributing something new that needs to be thought about as a dimension of that class of phenomena. The only time

that wouldn't be true would be when the two cases studied were identical in every respect—something so unlikely as not to be worth worrying about.

Concepts Are Generalizations

Here's a different approach to the same point. Although we think about them and speculate about them and define them, concepts are not just ideas, or speculations, or matters of definition. In fact, concepts are empirical generalizations, which need to be tested and refined on the basis of empirical research results—that is, of knowledge of the world.

We commonly have difficulty applying concepts to real cases of social phenomena: they sort of fit, but not exactly. That's because we seldom define phenomena by one unambiguous criterion. We don't say "If it has a trunk, it's an elephant, and that's that," or "If people exchange goods on the basis of price, that's a market." If we talked that way, we would know for sure whether a case was or wasn't one of the things we were interested in. (That's something of an exaggeration. We would still have all the problems associated with deciding what a trunk or an exchange on the basis of price is.)

Concepts that interest us, however, usually have multiple criteria. Max Weber didn't define bureaucracy by one criterion. He gave a long list of characteristic features: the existence of written files, jobs defined as careers, decisions made by rules, and so on. Similarly, social scientists usually define culture with multiple criteria: it consists of shared understandings, handed down from one generation to the next; of coherent propositions that embody the basic values of a society, and so on.

In the world we live in, however, phenomena seldom have all the attributes required for them to be, unambiguously, members of a class defined by multiple criteria. An organization has written files, and makes decisions by strict rules, but has no career paths for functionaries. Is it a bureaucracy, or not? An organization has, on paper, all the attributes Weber attributed to a bureaucracy, but is the kind of organization in which such things happen as this incident, reported by Gordon and his colleagues in a study of the public's access to information that was legally supposed to be available from city, county, and state offices in Illinois under various freedom of information laws:

> When a professor from the Center for Urban Affairs at Northwestern University sought some voting data in Chicago, for ex-

ample, he was clearly and repeatedly told, in person, by a clerk with an Irish surname, that those data, while legally public, were not available. While he was arguing to the contrary one day, an Italian surnamed clerk glanced at the professor's name on the written request, and interrupted to say: "Masotti. You Italian?" Dr. Masotti said, "Si," and spoke briefly in Italian to the clerk, who then called to another fellow Italian who labored for 30 minutes to produce a complete set of the initially "unavailable" data. (Gordon et al. 1979, 301)

Even if it has files and rules and all the other Weberian criteria, is that a bureaucracy?

A first reason these quarrels over definitions are important is that the descriptive titles that embody these concepts are seldom neutral, but rather are terms of praise or blame. "Culture," for instance, is almost always a good thing ("bureaucracy," as in the above example, is almost always bad). So we care, beyond technical theoretical considerations, whether we can say that a group has culture or not. We do not wish to reward with the approbation signaled by the honorific title some bunch that doesn't deserve it. Suppose a group's members share understandings, an element I mentioned above as often included in definitions of culture, but invent those understandings on the spot, instead of handing them down from generation to generation. Is that culture, or not? Some social scientists will not want to give a "bad" group that does such things (for instance, a delinquent gang) the honor of having real "culture"; they want to save such a good word for praiseworthy organizations (Kornhauser 1978). (An interesting problem arises here when historians discover that what seemed to be just such handed-down traditions embodying primordial values, etc., were actually invented not so long ago, the way they have discovered that Scottish culture, as embodied in the traditions of the ancient clans and their customary tartans, was invented by woolen merchants with excess stock on hand.)

Another problem can be put more technically. Suppose you have x criteria for an object, and you call objects that have all x criteria O. What do you call the objects that have $x - 1$ or $x - 2$ or $x - n$ of the criteria? The simple solution is to call them *not-O* and ignore all the differences among them—that is, treat them as though the only thing that is important about them is what they aren't. But that is often unsatisfactory because hardly any of the objects we study have all the criteria; instead they have varying mixtures of them—what Wittgenstein called "family resemblances." The bureaucracies we study are similar, but they aren't identical the way molecules

of copper are. We can, of course, give every combination of possibilities a name. In fact, we seldom do that, because these devices quickly generate a very large number of possibilities we aren't prepared to handle theoretically or practically. (Methods for handling that complexity exist, and I will discuss them in chapter 5.)

So concepts like bureaucracy are really, as we ordinarily use them, generalizations that say: "Look, these x criteria actually do go together, more or less, all the time, enough so that we can pretend that they are all there in every Object O even though almost all Os in fact just have most, not all, of them." That makes a problem because many of your cases don't act as your theory says they will, precisely because they are missing an important attribute that is responsible for that aspect of the behavior of O.

We can often finesse these difficulties, because the number of cases is small or because the objects we collected don't lack attributes that are important for the problem we are pursuing. But when we can't, we should recognize that our "concept" was not just an idea but an empirical generalization that said that all those criteria always went together.

A good example from the world of practical affairs has to do with the concept of "living" somewhere. When the 1960 Census failed to count a large number of young black males, the political consequences forced statisticians and survey researchers to take the problem seriously. The practical question confronting the research committee considering this problem was how to conduct the next Census so as to count the people who had been missed the last time (Parsons 1972, 57–77). The U.S. Census must count people where they live, for purposes of political representation, so the question became a double one: how can we find them *where they live* so that they will fill out our forms, and what does it mean to live somewhere (because if we understand what it means to live somewhere we will know how to reach them)?

The expert committees' discussions revealed a profound ambiguity in the notion of living somewhere. What does it mean to live somewhere? For every criterion proposed, you could imagine a perfectly reasonable exception. You live where you sleep: if I'm on vacation in Mexico do I live in Mexico? It's where you usually sleep: I'm a traveling salesman, I don't usually sleep anywhere in particular. It's where you get your mail: many people get their mail at General Delivery or the City Lights Book Store in San Francisco, but they don't live in those places. It's where you can always be reached: for me, at the moment, that's the Sociology Department at the

University of Washington, but I certainly don't live there. It's where you keep your clothes, it's where. . . .

For most people, most of the time, all those places are the same place. They usually sleep in the place they get their mail, which is also where they have their clothes and can most easily be reached. But for most people sometimes, and for some people all the time, these are different places: they keep their clothes one place and sleep in another. For them the concept is just not adequate and, if we want to take them into account, we have to break the concept down into its component indicators and treat each one separately. In other words, we have to realize that the empirical generalization embodied in the concept is not true: all those criteria don't go together all the time.

You can make this failure of the indices of a concept to stick together as we'd like them to the jumping-off point for expanding and complicating your theory of the world. Marisa Alicea (1989) did that in her study of return migrants to Puerto Rico—people who, having moved to New York or Chicago from San Juan or Ponce, then go back to the island. She showed that, in fact, they move back and forth between their two homes frequently. Thus, it's misleading to think of them as migrants and far more realistic and useful to see them as people who have, as she says, "dual home bases." Taking that result seriously means that another "fact" built into the concept of "living somewhere"—that people can only "live" in one place—has to be seen as simply another possibility that may or may not be true in a given case.

I have sometimes upset listeners with such examples, which seem to entail an extreme constructivism that makes it impossible to do any research at all. They are especially upset if I follow the "living somewhere" example with a mention of how Harold Garfinkel (1967) confounded demographers by describing the case of Agnes, a transsexual who had changed genders socially and then physically, and then asking how the Census could be sure it had correctly classified someone as male or female. Did you have to take down everyone's pants in order to be sure of the classification, he wanted to know? If you couldn't use even so simple an idea as living somewhere or being male or female, how could you observe or count anything?

Alicea's research shows that seeing the concept as an empirical generalization helps you to avoid analytic errors. We conventionally think that migrants live in only one place at a time and, when they move, stop living in

the place they used to live in and go to live somewhere else. Well, of course, they do go somewhere else. But they actually have some sort of home (what sort, of course, is the researchable question that makes it worth getting into these complications) in two places, both the mainland U.S. and their home town in Puerto Rico. You can't assume that living in the second place means exactly what it meant when they lived where they used to live before they migrated. Before moving, they might have thought of Home$_1$ as the only home they had. But having acquired Home$_2$, they might decide that they needn't give up the first one, and then might move back and forth between the two the way people with a little more money go to their summer cottages every year. The pathos of the story is that these people may not have, in either place, some of the nice things a "real home" gives you, such as a secure economic base or an affectional base of people who know you and love you. (But having two homes isn't necessarily a deprivation either. Carol Stack's research shows how poor children who can "run away" and live for a while with a neighbor or relative two doors down the street can profit from having multiple homes.)

The trick of seeing concepts as empirical generalizations helps solve the problems created by an unthought-through insistence that all the properties of a concept always go together. Uncoupling them, and treating them as capable of varying independently, turns a technical problem into an opportunity for theoretical growth and articulation.

Concepts Are Relational

I once taught a class called "Classics of Social Research." One of the books we read for the class was Jane Mercer's *Labeling the Mentally Retarded* (1973), a study of the way the label "mentally retarded" was applied in the Riverside, California schools. This study proves, as well as anyone but an ideologue would want it proved, that borderline retardation (as opposed to the "real" retardation that is accompanied by obvious physical handicaps, etc.) is a disease Mexican and Black kids get when they go to school, and are cured of when they leave school.

I was moved in class one day to give a lecture on the idea that all terms describing people are relational—that is, that they only have meaning when they are considered as part of a system of terms. This is not a new idea. I think I first saw it put that way by a Marxist historian (perhaps E. P. Thompson or Eric Hobsbawm) who said that class was a relational term:

terms like "middle class" or "working class" only have meaning in relation to one another or to "upper class," and the meaning is the character of the relationship. "Working class" means that you work for people who are members of the "owner class."

That seems obvious enough. But it's one of those obvious things that people acknowledge and then ignore. How do they ignore it? By imagining that a class, by having a characteristic culture or way of life, would be what it is no matter what system of relations it was embedded in. That's not to say that there aren't class cultures, but rather to insist that such cultures result from some group of people being related to some other group in a way that creates, at least in part, the conditions in which their distinctive way of life develops.

A similar meaning has been attached to the idea of a country being "underdeveloped." In this case it was done by the simple device of treating "underdevelop" as a verb, "to underdevelop," which made it obvious that there were some other countries or organizations that were making that underdevelopment be what it was. In this case, there are obviously two separate things: to be underdeveloped only has meaning in relation to other places that are developed, and the distribution of "development" as a trait is created by the deliberate actions of some of those other organizations.

I took this up in class when one of the students, a clinical psychologist who found Mercer's conclusions hard to accept, insisted that mental retardation was, after all, real, not just a matter of definition or relations. At least, she said, there are some cases in which children are profoundly retarded. I started my reply by asking the students whether they thought I was tall or short. (If you measured me, I would be about 5'10", which, these days, is not particularly tall, but not short either.) They looked confused and waved their hands as if to say that I was medium. I insisted on an answer and, of course, they couldn't give it. I said that I used to be a shorter member of the faculty, when one colleague who measured 6'9" and another one who was 6'6" were around, but that I had gotten taller since they left. I asked a visiting Japanese student if it wasn't true that I would be tall in Japan. She laughed uncomfortably and finally said yes. I said that when I was in high school I would have been a reasonable height to play basketball but not anymore, and went on to point out that height was about as real a fact as you could hope to know about anyone—certainly as real, say, as retardation or intelligence.

The trick here is to place any term that seems to describe a trait of a per-

son or group in the context of the system of relations it belongs to. That shows you that the trait is not just the "physical fact" of whatever-it-is, but rather an interpretation of that fact, a giving of meaning to it, that depends on what else it is connected to. The first thing it is connected to is other traits, which have similarly been given meaning, so that they constitute a system of possibilities. The graded series that runs from "profoundly retarded" to "retarded" to "normal" to "gifted" to "genius" is a good example.

But, the analysis can go on, what else is this system connected to? Why do these distinctions seem "natural" to a no more than ordinarily reasonable person? Why do they seem reasonable enough and important enough to act on? I pointed out that I myself was "profoundly retarded"—in the area of drawing. I could never draw a tree or a dog the way the "good drawers" in my class could. As a result, I had always felt ashamed. This disability had affected my life in nontrivial ways. Another student owned up to being "profoundly retarded" in the area of music, so unable to carry a tune that she had always been told to just mouth the words when her grammar school class sang in assemblies.

Why were these statements ironic, not serious? Because, obviously, these disabilities "don't make any difference." Nothing really bad happens to you if you can't draw or carry a tune. It may be unpleasant and mildly shaming. You may wish you could do these simple things with as little trouble as others. But our world is not so organized as to require us to be able to sing or draw.

Our world, however, *is* so organized that people must be able to do some things that "retarded" people can't do easily or well or at all. To get along, at least at a level some people and institutions define as minimal, you have to be able to read a little, do a little arithmetic, "catch on" to what's going on and pick up various kinds of ideas and skills within a certain length of time, read maps, tell time, understand directions, and so on. Otherwise, you are "slow."

Lewis Dexter (1964), writing about "The Politics of Stupidity," pointed out that all those skills result from our ancestors and contemporaries having built and maintained a world that makes those skills more or less necessary. You could build another kind of world where a similar necessity for physical grace and dexterity would be built into its physical appurtenances. In such a world, it might be necessary, in order to open a door, to perform some rather complex physical movement awkward people would have

trouble with; some very awkward people wouldn't be able to open it at all. We might call these people "gawkies" and have special entrances to places built for them, perhaps give them special remedial classes in the hope of reclaiming them for a productive life, although we might have to conclude sadly that their genetic endowment made it impossible.

So there is a great difference between a physical trait and its social importance. We all have all sorts of traits, only a few of which are socially marked as important because of the way they are embedded in a system of relations. They become important when the organization of physical and social arrangements makes them "necessary." Take height. If you are above or below a certain range of height our physical arrangements make it awkward. If you're short your feet won't reach the floor when you sit in standard chairs; if you're tall you'll bump your head on doors if you aren't careful. Our social arrangements are somewhat more forgiving; but still, very tall women and very short men are exposed to troubles finding partners the rest of us don't have.

All this has a historical dimension. Several centuries ago, people's average height was less than it is now—so doorways built in the fifteenth and sixteenth centuries, unless they have been rebuilt, will catch careless contemporary people and bump their heads. Or take the skill of doing simple arithmetic. Anyone, these days, who cannot do addition, subtraction, and other simple arithmetic operations is certainly "slow," maybe even "retarded." But those skills were not always required. Patricia Cline Cohen's *A Calculating People* (1982) showed that it wasn't until well into the nineteenth century that the ordinary American really needed such skills; before that storekeepers and clerks might need them, but not the average person. She calls these skills "numeracy," in analogy to "literacy." The term emphasizes that it is because these are socially valued skills now, built into our everyday operations, that we see them as such important human abilities; at an earlier time such skills might have been interesting cultural ornaments just as singing and playing the flute were, but certainly not "important."

Skills and traits not only become more important, they also become less important. Diana Korzenik's book *Drawn to Art* (1985) describes the changes, back and forth, in the importance of skill in drawing in American society. In the middle and late nineteenth century some important people decided that the reason the United States was falling behind in industrialization was that Americans did not know how to draw. Much invention

and adaptation of machinery took place on the floor of industrial shops, where workmen dreamed up improvements and inventions based on their detailed experience of the operations involved. For that to happen efficiently, workmen had to be able to draw plans from which the necessary parts and equipment could be built. But American workmen had not been trained in mechanical drawing and were not as good at it as were, for instance, German workmen. Steps were taken: a movement to have remedial classes for adults, so that workmen could acquire this necessary skill; a push to have drawing taught more systematically in the elementary schools. But that emphasis on drawing was relatively short-lived; other developments made drawing not so important after all, which meant that in the 1930s I could go through elementary school and be thought a bright student even though I couldn't draw (and had, in addition, terrible penmanship, which would have been a severe handicap in the pre-typewriter era).

Who gets to say which traits are important enough to be made the basis for serious and fateful distinctions? Sometimes it is our immediate associates who will decide for themselves whether my inability to draw or your inability to do arithmetic or her inability to carry a tune are serious enough to warrant special negative treatment, or whether my ability to remember and be ready to play one thousand popular songs on the piano or your ability to imitate Cary Grant or Groucho Marx or Judy Garland warrant special rewards. Sometimes, and this is where Mercer's results are so important, the decision is put in the hands of specialized professionals, who possess special esoteric methods for making these determinations. One of Mercer's truly shocking findings is that gross racial and ethnic disproportions in labeled retardation do not appear when teachers recommend children in their classrooms for intelligence testing—the children recommended display the same proportion of Mexicans, Blacks, and Anglos as the general school population. No, the gross overrepresentation of Mexicans appears *only* when intelligence tests are given, when the decision to classify a child as retarded is made by someone who has no experience of the child in the real life of the classroom and cannot interpret the bare test scores in the light of other knowledge of the child. So the professionalization of these decisions, through the development of occupational specialties and monopolies, is another important historical variable affecting how "individual traits" come to be embodied in a set of social relations that make them important.

Politics and power similarly affect how systems of relations make some

traits important. If a negative trait is being assigned to people, powerful people can often prevent that from happening to them or theirs. If something good is being passed out, they will do their best to see that they and theirs get it. In the 1980s, the U.S. Congress (presumably trying to give middle-class people something to balance the special resources being allocated to the education of poorer, so-called "underprivileged" children) authorized a program for "gifted and talented" children. I suppose that the distinction mirrors, on the positive side, the distinction between "profoundly" and "mildly" retarded.

This program created a problem for teachers of visual art in the public schools: how do you choose the children who are gifted or talented and thus deserve extra training and opportunities? Even though middle-class parents are, by and large, more interested in other kinds of skills and talents than they are in visual art, still, if it's there to get, they want it. They want it to the degree that the people who decide who gets such special treatment need a scientifically defensible way of making the choices involved. Which is how I ended up at a conference that was labeled as being about "creativity" in the arts but turned out in fact to be about "Can you devise a test of some ability such that I can tell parents that children got into the Gifted and Talented Program on the basis of this test score and please leave me alone, I can't do anything about it if your child's score was low?"

So the teachers' problem became a testers' problem. What do you measure to assess ability in visual art? This was a serious problem because it is much more difficult to agree on a criterion in art than it is in arithmetic or reading. There is, however, one thing that "everyone knows" is important for visual art, and that happens to be the thing I can't do: drawing. Unfortunately, it isn't obvious that the ability to draw, even supposing that it might be relatively easy to test, is closely related to, say, success as a visual artist, any more than such conceptual abilities as ability to visualize spatial relationships or color sense or you name it. Furthermore, it's obvious that if you use a criterion such as success as an artist you might want to include such social and business skills as hustling. Still further, some visual arts, notably photography, don't require any drawing ability at all, so any test based on drawing would necessarily make some gross errors.

What's the point of this lengthy digression about "gifted and talented"? That the power of middle-class parents can affect the way this system of relations is set up and thus make it more or less important, and more or less available to people of different kinds. But their power may not be sufficient

to overcome the power of the entrenched professionals into whose hands these determinations have fallen.

A second point to this example is that there are at least two kinds of systems of relations involved. In one, the reputationally desirable position is in the middle, at the mean of whatever is being measured, like height. This recalls Everett Hughes's suggestion, discussed earlier, that we inspect deviations from the average in two directions, looking both for people who have more of whatever it is and people who have less. In his example, one doesn't want to deviate from the modal way of organizing sexual relations, either by being "worse" than others (in ways that produce labels like "rake" or "slut") or by being "better" (being, say, a "goody-goody"). In other relational systems, however, reputations and their results for one's life get "better" the farther you go in one direction, and worse the farther you go in the other direction. Intelligence is like that, as are other traits like artistic ability.

To summarize this set of tricks: Put terms into the full set of relations they imply (as "tall" implies "short" and "gifted" implies "not gifted"). Then look at the way that set of relations is now organized and has been organized at other times and in other places (as in understanding that not knowing arithmetic has a different meaning and different consequences than it did 150 years ago). And, finally, see how things came to be organized the way they are here and now, and what connections to other social arrangements sustain that set of relations.

The Wittgenstein Trick

I've owned a copy of Ludwig Wittgenstein's *Philosophical Investigations* for years, but I read it the way Everett Hughes told me to read the sociological writings of Georg Simmel: not to get a full understanding of what the author might have meant, but rather as a way of generating ideas I could use in my own research and thinking. One of Wittgenstein's ideas has become a standard part of my repertoire. Because it was provoked by a passage in the *Investigations,* I think of it as the Wittgenstein trick.

Discussing the philosophical problems of intention and will in one of the numbered paragraphs that make up the book, Wittgenstein makes this remark: "Let us not forget this: when 'I raise my arm,' my arm goes up. And the problem arises: what is left over if I subtract the fact that my arm goes up from the fact that I raise my arm?" (Wittgenstein 1973, §621). That's the

essence of the trick: if I take away from an event or object X some quality Y, what is left?

This trick helps us strip away what is accidentally and contingently part of an idea from what is at its core, helps us separate what's central to our image of a phenomenon from the particular example it is embedded in, as Wittgenstein isolates the core of our intuitive image of intention by separating the contingent physical action from it. Here's an example. I was once part of a panel organized to talk about modern art. One of the other panelists had become a serious, big-money collector of contemporary art three years earlier. When it was his turn to speak, he talked knowledgeably and at length about his "collection," which of course consisted of a large number of paintings, sculptures, and other objects. As I listened to him, I thought, "I have a house full of paintings and other objects, just as he does, but I don't have a collection. Why not?" So I did the Wittgenstein trick. I asked myself: "What is left over if we subtract from the idea of a collection the fact that this collector has a large number of paintings and other art objects in his house?" I turned to my data—the talk the collector was giving—for the answer. He immediately gave me part of the solution to my problem: his collection, as opposed to my mere mass of objects, had, he said, a "direction." It was not just an aimless assortment of stuff, the result of whim and caprice; less pejoratively, it did not represent the untutored application of his own taste. Rather, it resulted from and embodied knowledge and trained sensibility (his own and that of his advisers), and thus had a concrete and explicit aim and structure. Likewise, his collection had a "future." It was headed somewhere. It would be the object of repeated evaluation by knowledgeable experts. It was part of a world of artistic activity and progress, its very accumulation an act of substance in that world. My stuff, in contrast, was just that: stuff I had bought because I liked it, stuff I had traded my photographs for; accumulating it was just a private act that had no significance to anyone but me and mine. (The word "just" is important here, signifying as it often does in philosophical talk "merely" or "no more than.")

In fact, as the collector talked, I realized that having the objects in the house (or the office or any place he actually lived or worked) was not really necessary to his having a collection. Accumulating the objects in one place is not necessary to the *idea* of a collection. Why not? If you are a dealer specializing in new, trendy art (the kind the collector collected), you insist before you sell a piece (the dealer who was the third panelist explained this to

me) that the purchaser make the work available for loan to museums for exhibitions. If you, an art dealer, are trying to build an artist's reputation, it does neither you or the artist any good to have an important piece sitting in someone's living room in the Midwest, no matter how much you sold it for. The piece must be where it can be seen by "important people" (that is, people who are important actors in the world in which such paintings are exhibited, bought, and sold) and thus contribute to the development of a career. Many museums have shows that are part of this process, and the purchaser of a work must make the purchase available for them. In fact, I had been in Amsterdam a few months earlier, and had seen, in a show of work by New York artists at the Stedelijk Museum, many pieces by the artists the dealer on the panel represented, some of them in the collector's collection. Truly "with it" collectors thus might not see sizable fractions of their collections for long periods of time. In fact, of course, some people's collections, or portions thereof, are often on more or less permanent loan to museums (which hope to be left those works in the lender's will).

Using the Wittgenstein trick, then, what is left when you take away from "collection" the idea that you have a lot of art stuff in the house? What seemed to be left (in this situation at least, but I think it would be a common view of the problem) was the idea of the collector as a person who has the financial and cultural resources (the latter what Pierre Bourdieu has called "cultural capital") to choose and acquire objects that represent what will eventually turn out to have been major trends in modern art. In his talk, the collector said something like this: "The idea is to find out how to get the best work of an artist who will be historically significant, works that will turn out to be a major part of art history. Your reward is to have your judgment approved by history." On this view, where the objects are is irrelevant, and having objects in itself doesn't make you a collector. The objects are merely the visible symbols of the decisive action the collector has taken by staking big money and a reputation for sagacity and sensibility on the choice of art works, and it's that action that is crucial to understanding what a collection is. (Which is why some members of the art world dispute the characterization of Joseph Hirshhorn, for whom a major art museum in Washington, D.C. is named, as a great collector. Can you, they complain, be a great collector if, as he is said to have often done, you just walk into an artist's studio and after a quick look around buy everything in it? Where's the sagacity and sensibility in that? This, of course, is an art world complaint, not a sociological judgment.) And it's not just the action the collec-

tor takes that's important for understanding the idea of a "collection," obviously; it's also the action the rest of the world takes by making what the collector has accumulated significant in art history or not. (I've drawn on Raymonde Moulin's analyses [1967, 1992] of the French and international art markets for some of these ideas. An attentive reader will see, too, that this trick is another way of describing what you've studied without using any of the specifics, which is what the Beck trick does.)

Enlarging a Concept's Reach

The Wittgenstein trick, then, lets us isolate the generic features of a series of cases we think have something in common, the features out of which we can construct the generalization that is a concept. Once we have isolated such a generic feature of some social relation or process and given it a name, and thus created a concept, we can look for the same phenomenon in places other than where we found it. The study of prison cultures furnishes a nice example.

Students of prisons (e.g., Sykes 1958) had demonstrated that the inmates of men's prisons developed an elaborate culture. Inmates created convict governments that took over many of the functions of keeping order in the joint; they developed informal but orderly markets in cigarettes, drugs, tailor-made prison uniforms for the snappily dressed convict, and a variety of personal services; they organized sexual activity; they enforced a strict code of convict behavior emphasizing the necessity of never giving information about other prisoners to prison guards and officials.

Analysts of prison culture attributed these inventions to the deprivations of prison life: deprived of autonomy, prisoners carved out a governmental structure that got some autonomy back for them, and a convict code (of which the prohibition on snitching on other prisoners to prison staff was a major component) that preserved that autonomy; deprived of drugs, sharp clothes, and other goods they were used to in civilian life, they organized markets to provide those things; deprived of sex, they improvised a system of predatory prison-specific homosexual relationships that did not threaten their self-conceptions as macho men. The sociological generalization, a specification of a more general set of ideas that goes back to William Graham Sumner, was that prisoners collectively develop a culture that solves the problems created by the deprivations of prison life.

So far, so good. With this theory in mind, Ward and Kassebaum (1965)

studied a women's prison. They found none of the things the theory of prison culture had led them to expect. Quite the opposite. Even the officials of the prison complained about the lack of a convict code: the women were forever snitching on each other in a way that made a lot of trouble for them and thus for the prison staff. No real underground market existed. Sex life was not organized in the predatory style of the men's prison; instead, the women developed pseudo-families, with butches acting as the husbands and fathers of a collection of wives and daughters. (See also Giallombardo 1966.)

Did these differences—the absence of any of the things predicted by the available theory of prison life—invalidate the generalization that the deprivations of prison life lead to the creation of a prison culture? And did that in turn mean that no generalizations about prisons were possible? Not at all. They meant that the generalizations are not about how all prisons are just the same, but about a process, the same no matter where it occurs, in which variations in conditions create variations in results (which is actually a much classier form of generalization anyway).

In this case, the theory wasn't wrong, but you had to put in the right values of the variables, so to speak, to see how it was right. You could still say that the deprivations of prison life led to the creation of prison culture, but that was true only if you understood that these deprivations were different for men and women. Women were not deprived of autonomy because, as they explained to the researchers, they had never been autonomous; they had always lived under the protection of and been subject to the authority of a man: a father, husband, or lover. What prison deprived them of was exactly that kind of protection. So, rather than develop a convict government to replace the autonomy they didn't miss, they developed a system of homosexual relationships in which one woman stood in as the masculine protector.

New women prisoners were especially afraid because, due to variations in the gender distributions of crime, men's prisons have a lot of professional criminals serving time for robbery, burglary, and other less violent crimes, while most women prisoners are in for drugs and prostitution, and for the typically amateur "crime of passion"—that is, murder. Since there are thus more murderers in them, women's prisons sound like very dangerous places to be, even to the murderers who know that they themselves aren't dangerous (they just wanted to kill that one person who done them wrong). So even the murderers are looking for someone to take care of

them. Similarly, women's prisons typically allow inmates to buy things they want, like cosmetics and clothes, so there is no need for an underground market.

In short, women prisoners are deprived of different things than men, both because their lives on the outside and, therefore, their needs on the inside, differ, and because the prison is run differently for them. Their culture responds to that difference. The generalization is still true, even though the results are quite different.

The general lesson here, the trick to be applied elsewhere, is not to mistake a specific instance of something for the entire class of phenomena it belongs to. Deprivation probably leads to the collective development of cultural practices designed to relieve it in all sorts of settings, but what constitutes deprivation may vary considerably.

We are most likely to confuse part of a class for the whole in this way when the class has a well-known name that applies to an equally well-known set of instances. That's why people who study "education" almost always study schools. That's where education takes place, isn't it? Everyone knows that. Education, conventionally defined, consists of knowledgeable people teaching people who are less knowledgeable, and typically, not surprisingly, less powerful and less well-placed (children or immigrants, for instance), and doing it in schools. That's what education *is*.

If, however, we think of education and learning as generic social processes, there's no reason to think that those processes take place only in schools. We might try to redefine the subject matter as people learning things, wherever and however that activity happens and whoever does it. Then we could include in our collection of cases the way thieves teach other the latest techniques of their trade, or the way young people teach other to use drugs or engage in sexual activity. But that's just cheap irony, because everyone knows that those activities aren't "education," at least not what any reasonable layperson means by that. Education means schools.

But there's no reason to assume that learning takes place in schools at all, even though that is the story schools tell about themselves and the story well-socialized members of our society believe, or at least pretend to believe so that they won't appear to be nuts. You can study, as an example of learning, how young people learn to use marijuana. You may find, as Schaps and Sanders did in 1970 (and it might be different at another time), that young women typically learn from their boyfriends, while the boyfriends learn from each other. By ignoring the conventional instances

that define the concept, you have enlarged its reach. You have discovered new people who do the job of teaching and new relationships in which it is done.

It's quite likely that the process by which boyfriends teach their girlfriends to smoke dope has a lot in common with other activities in which knowledge, skill, and ideas are passed on. It might, for instance, resemble the system described by Gagnon and Simon (1973), in which young women teach their boyfriends to engage in romance, which they have been practicing in solitude for quite a while, while the boyfriends are teaching them to engage in sex, which *they* have similarly been practicing in solitude. If the process works, and each learns what the other knows, they can manage to fall in love in the more or less standard way.

These processes of peer teaching and mutual learning may, in turn, have their counterparts inside schools and other so-called educational institutions. Personal computer users often teach each other how to use their machines, despite or perhaps because of the more conventional standardized instruction available here and there. Students in conventional educational institutions have repeatedly been shown (e.g., Becker, Geer, and Hughes [1968] 1994) to teach each other how to deal with the constraints, requirements, and opportunities those places embody: how much of the assigned work you really have to do, for example.

To take another variation on the standard model of education, some kinds of teaching and learning are, unlike the elementary and secondary education that form the archetypal instances that define the concept, totally voluntary: lessons in piano playing, tennis, and French are all like that. They take place in profit-making establishments, are often if not always individual, and have no fixed term. The students get no credits and no degrees. They just take lessons until they feel they aren't getting anything out of it any more. The distribution of power between student and teacher is so different from the stereotypical school that this is bound to be a somewhat different generic type. (See the discussion in Becker 1986a, 173–90.)

An excellent, perhaps the best, way to enlarge the reach of a concept is to forget the name entirely and concentrate on the kind of collective activity that is taking place. A good example of this strategy is Erving Goffman's analysis (1961) of what places that had the generic features of "total institutions" had in common with respect to the way their inmates (be they nuns, sailors at sea, or mental patients) had to live and the kinds of adjustments they made to living that way. Or his analysis of the characteristic so-

cial forms that grew up around people who had stigmas of various kinds (Goffman 1963). The brilliance of these analyses was to show that, in the generic sense he had in mind, everyone had some sort of stigma, not just people who were blind or missing a limb, and every institution was, in some respects, a total institution. Exchanging the conventional contents of a concept for a sense of its meaning as a form of collective action enlarges its reach and our knowledge.

It's time in the next chapter to consider some more formal ways of working with concepts, ways that use the devices of serious formal logic.