

excitement in writing about paradigms and presuppositions, are, from the student's point of view, tools with which to generate hundreds of new ideas and arguments. An astonishing number of important pieces of social science have made their mark precisely by playing with these various debates in exciting ways. There is no reason the student should not use the same tools. You should get a sense of these debates and, above all, a sense of them not as something to get right or take a position on or otherwise etch in stone but as something to play with. These debates are the most sophisticated tools for producing new social science. And any good student can get in on the action.

A. Abbott, Methods of Discovery  
(W. Norton 2004)

## Chapter Seven

### IDEAS AND PUZZLES

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- I. TESTS OF IDEAS
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WE HAVE NOW BEEN THROUGH four chapters of heuristics that generate new ideas. But not all of these new ideas will be good ideas. How do we know which are good and which are bad?

Part of the answer depends on what we mean by a good idea. Sometimes "good idea" means an idea worth retaining for the moment. (And it's worth remembering "the moment" could mean a lot of different things—five minutes, an afternoon, until I think of something better, and so on.) But sometimes a "good idea" means good on some absolute scale. A good idea is good because it's right or because we really believe it. Obviously, an idea has to see some testing before we decide it's good in this second sense.

There are several different ways to recognize and develop good ideas when we see them. First come tests we set ourselves. Critique starts at home, as everyone knows. So we need to discuss some personal ways to test ideas, to get a personal sense of

whether they are worth elaborating and developing. Second come interactional tests, ways of trying the idea out on others. The usual ways of trying out our ideas on others are pretty wasteful. In the classroom and out of it, we often behave as if our ideas were weapons and others' ideas were targets. We dismiss them with the obligatory "that may be, but I think . . ." But intellectual life is neither a shoot-out nor a sequence of random opinions. It is a **mutual challenge**, with equal emphasis on *mutual* and *challenge*. Others' thoughts can help you see what's good and what's bad about your own.

Finally, we need to test our ideas with respect to existing scholarly writing on a topic. If you recall, I said at the outset that this book originated in the complaint I heard from many students that "I have nothing new to say." Now that you've read a book's worth of ways to find new ideas, the literature should no longer seem so frighteningly complete and comprehensive. So you're ready to use the literature in order to evaluate and develop your ideas. You have to understand how social scientific literatures work if you want to have ideas that make sense to the people who write them.

This leads us naturally to two broader topics: how we develop good taste in ideas and how we come to know our intellectual personalities. The question of taste is crucial. In the long run, good intellectual taste is the best passport to good ideas. But a passport is no good without a means of travel. So our personalities are equally important. Each of us has habits of thought that make certain ways of thinking more dangerous or more useful or more easy. These two topics, of taste and personality, bring me, finally, to the issue of puzzlement. **Having good ideas also means being able to see certain things in the so-**

**cial world as puzzling.** Cultivating puzzlement is my concluding concern.

# I. TESTS OF IDEAS

Obviously, the first test of an idea is to try it out, to run it past some data. **In practice, most ideas come from looking at data in the first place.** Only when one is using formal methods do ideas come from dataless thinking, and even with formal methods the ideas usually come more from **reflecting on commonsense knowledge** than from pure deduction. Most people get their stimulus from thinking about data they've already got or empirical things they already know.

Once you've got an idea, you need to try it out on some new data. So if you're an ethnographer studying welfare-to-work training programs and you've begun to notice that the trainer's rhetoric emphasizes getting rid of race-stereotyped mannerisms, you start looking for other indications of overt or covert race retraining in other parts of your data. If you are Barrington Moore studying the histories of the revolutions that led to modernity and you notice that in America and France the old rural aristocracy was undercut completely but in Germany it survived and even dominated politics, you start looking for other cases and see if you can predict whether a government turns fascist based on how its rural aristocracy fared during modernization.

It's not just a matter of looking for other cases of a phenomenon or a relationship you've identified. It's also a question of looking for other implications that your idea has for data. Suppose you're a survey analyst studying married women's labor-force participation and you suddenly get the idea that it's

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driven by a woman's need to guarantee a skill set and an experience record so that she can support herself in case of divorce. You can infer from that idea that the long-term overall trend in women's labor-force participation should correlate closely with the long-term overall trend in the divorce rate. That correlation follows logically from your new idea because if women aren't more likely to get divorced (and to suffer divorce's economic loss), then (on your argument) there's not the same necessity for them to have work skills as a precaution. You also know that your idea implies (at the individual level) that women with alternative resources unaffected by divorce (women with inherited wealth, say) won't have to get the resources through work, and so your theory implies that they will be less likely to work (which they will also be for other reasons, of course). Both of these empirical predictions can be tested, formally or informally.

We see this deriving of implications most clearly in formal methods, for these usually produce clear predictions. The formal arguments in Schelling's famous *Micromotives* book have clear implications for traffic jams, for social movements and riots, and so on. Indeed, one could say the greatest virtue of **formal methods** is their copious production of implications. 骚乱

But all ideas have implications for data, whatever the method used. You should get into the habit of continually generating these implications and of continually **moving your ideas on to new cases or data.** It should become a matter of second nature, something that goes on almost automatically when **you think up an idea.** My friend and colleague the late Roger Gould was a master at this. You would utter an idle truism, like "young people are always each other's harshest critics," and

he immediately would respond, "Well, if that's true, then it ought to be the case that dissertation defenses will be easier on 毕业论文 graduate students than having lunch with their friends" or "Do you really mean that people's harshest critics are always their peers, so that older people's harshest critics are other older people?" and so on. Note that just because an idea fails a few of these tests—makes a few bad predictions, doesn't work in a couple of cases—doesn't mean that we must throw it out. Most often, we get new wrinkles in our ideas that way; we learn how to move them around a bit, expand one part at the expense of another. (That's what Roger would have been suggesting by making the generalization that peers are always the harshest critics.) **It's rather like decorating a room; you try it, step back, move a few things, step back again, try a serious reorganization, and so on.**

This continuous monitoring and testing of your ideas rests more than anything else on a firm command of logic. The basic logical forms—implication, inverse, converse, and so on—need to be hardwired into your mind so that the process of monitoring goes on in the background, like the antivirus software on your computer. It is a matter of practice as much as anything else. If your logic software hasn't been updated recently, a review might be worthwhile. **Being able to quickly think up three or four implications (positive and negative) of a social theory is a crucial skill.**

In order to be tested, all of these ideas and implications must be framed in such a way that they can be wrong. It is great if your idea works most of the time, but if it works all the time, you should start to suspect it. It's likely to be a truism 自明之理 and therefore not terribly interesting. (Although sometimes it's

fun to turn a truism on its head, as we've seen.) It is quite surprising how many researchers—even graduate students in their dissertations—propose arguments that can't be wrong. For example, research proposals of the form “I am going to take a neo-institutionalist view of mental-hospital foundings” or “This paper analyzes sexual insults by combining a Goffmanian account of interaction and a semiotic approach to language” are not interesting because they do not propose an idea that can be wrong. They boil down to classifying a phenomenon or, seen the other way around, simply illustrating a theory.

Similarly, universal predicates are in general uninteresting, even if they are consequential. Thus, the idea that this or that aspect of reality—gender roles, say, or accountancy—is socially constructed is not particularly interesting. Everything is socially constructed in some sense, and probably even in a relatively strong sense. The interesting questions involve *how* gender roles are socially constructed or *what the consequences* of the constructed nature of accounting experts are. Watch out for universal predicates.

Another way to put this is to say that good ideas have real alternatives, not simple negations. It is better to be thinking “A is true or B is true” than “A is true or A is not true.” If you have a genuine puzzle, you want to solve it, not simply to know that one particular solution doesn't work. Thinking without alternatives is a particular danger in ethnography and historical analysis, where the natural human desire to develop cohesive interpretations (and the need to present a cohesive interpretation at the end of the research) prompts us to notice only those aspects of reality that accord with our current ideas. It's also surprisingly common in standard quantitative work,

which often tests ideas against things that are called, all too literally, null hypotheses. The majority of published quantitative articles do *not* have two real alternatives that are *both* dear to the writer. Most of the time, the writer's sympathies are clear well ahead of time, and the suspense is purely rhetorical. The writer's ideas are tested against random chance, even though nobody really thinks pure randomness occurs much in social life. All of this is wrong. An idea always does its best if it has a real alternative. Always maintain *two* basic ideas about your project, and try to be equally attached to both.

Truisms are not a lost cause, however. It is a useful challenge to try to make a truism into an idea that can be wrong. Suppose we wanted to make something out of the old joke that the leading cause of divorce is marriage. To make this meaningful, one has only to reconceptualize marriage as formalization of a relationship and divorce as breakup or damage, and we have the very interesting hypothesis that formalizing a love relationship decreases some aspect of its quality and hence makes it more likely to dissolve. This, too, is a platitude (not only in the nontechnical literature on romance but also in Weber's formal version of it as “routinization of charisma”), but it is not definitionally true and could be empirically right or wrong. It's a much better idea than the bald statement that “marriage is the leading cause of divorce,” if a little less amusing.

Not being able to be wrong is thus a sign of a bad idea. It goes without saying that having no empirical referent at all is also a sign of a bad idea. An idea of the form “The population-ecology theory of organizations is really just a version of conflict theory” is not very interesting. One could for various reasons want to write a polemical paper about it, but it's not a 争论的

powerful or exciting idea, unless we turn it into the empirical assertion that "the population-ecology theory of organizations *arose historically from* conflict theory." Although somewhat vague, this version has the beginnings of a good idea in it. The first version is just a classificatory exercise. The second is an empirical assertion about the history of social science.

A good idea, then, ought to have some referent in the real world. This is not to deny the utility of pure social theory, but the vast majority of social theory consists of relabeling. All real theory arises in empirical work, in the attempt to make sense of the social world, no matter how abstractly construed. A student is well advised to stay clear of writing pure theory. It's an open invitation to vacuity.

To pursue this argument a bit, we should note that it is also a bad sign if an idea works too well or too quickly. Usually this means that the idea is just *relabeling* something that is already known or accepted. When you have an idea—say, that a certain kind of behavior is guided by norms—most of the time you are simply relabeling the fact that the behavior is regular and consistent. The notion of norms doesn't add anything to the fact of regularity unless it involves the positive assertion that the regularity is produced by obligatory, emergent rules. But then you have the problem of demonstrating that these rules actually exist independent of the behavior they enjoin. It's this existence question that is crucial, and if you don't fight it out, your work is just providing fancy labels for something simple.

Relabeling is a general activity in social science because it's a way of appearing novel without having to do much. Often when you've just read a new theorist, that theorist's language will seem supremely compelling because of its novelty, but

then it will turn out to be the same old stuff with new names. Much of sociology fell in love with Pierre Bourdieu's word *practice*, for example, but most of the time when the term is used by others in sociology, it simply means "regular behavior." It's just a new word for something we have talked about for a long time. To the extent that it *is* new, it involves the assertion that the behavior involved is in some way self-perpetuating, that doing it regularly creates the possibility and the likelihood that we will do it even more. That is a stronger assertion—one that must be considered empirically—but of course it, too, is quite old and familiar. (Stinchcombe called this mechanism "historicist explanation," for example [1968].)

Ideas that reclassify something are also usually pretty uninteresting. "Social work is really a profession" is an interesting topic polemically, but as a research idea it is going to be interesting only if by seeing social work as a profession, we can understand something profoundly puzzling about it. For example, we might think that demonstrating that social work was really a profession might explain why its practitioners work for so little money. But then the strong form of the idea would be some more general statement, such as "People are always willing to exchange prestige for salary, and being thought professional confers high prestige." This is quite different from "Social work is really a profession." By themselves, then, classificatory ideas aren't interesting, but they often conceal an interesting question. So the proper challenge to present to a classificatory idea is Why do I think this classification matters? What is really at issue? Note, too, that in the largest scale, reclassifications are often analogies, which are among the most powerful of heuristic gambits. Saying that the family was really



a utility-maximizing unit like any other helped win Gary Becker the Nobel Prize in economics.

The criteria for good ideas discussed so far are short-term criteria. These are not the only ones. One of the most important tests of a good idea, needless to say, is that it still seems like a good idea when you get up the next day or when you've been doing something else for a few days and come back to it. This seems obvious enough, but in practice we often forget it. For from this obvious fact follows the corollary that no good paper is ever written at a single sitting, the practice of generations of college students (including me) notwithstanding. If you don't go away from an idea—really go away from it, so that you've forgotten important parts of it—you can't come back to it with that outsider's eye that enables you to see whether it's good or not. A good idea is one that stays faithful even when you go out with other ideas. There's no other way to test that than to do it.

In the long haul, the best personal criterion for a good idea is the one presented by the philosopher Imre Lakatos thirty years ago (1970). A good idea is one that is "nondegenerating." It is productive. It gives rise to more ideas, to more puzzles, to more possibilities. Its curve is upward. At the same time, it doesn't deceive us with the "suddenly everything is solved" feeling that comes from truisms and relabelings. A good idea is a little resistant to us. It sometimes doesn't work when we want it to and sometimes it works when we least expect it to.

Ultimately, one knows good ideas by the solid feeling they give over time. A good idea will make you feel secure while you do the grunt work that takes up the majority of research time: cleaning quantitative data, spending lonely time in

ethnographic settings, slogging through archival documents. When you do these things with a good idea in your head, you know why you are doing them. That gives you the confidence and endurance you otherwise lack. When you don't have a guiding idea, you feel desperate; you hope that somehow an idea will emerge magically from the next page of coefficients, the next incomprehensible document or conversation. Indeed, students often throw themselves into the detail work to hide from their feeling that there isn't a big idea. Don't. Work at the idea, and the grunt work will become much more bearable.

## II. OTHER PEOPLE

Once an idea has passed our own preliminary screening, it needs to be tried out on others. Sometimes this exercise will be formal, sometimes informal.

From the start, trying out ideas on others is different from trying them out on yourself. Others do not hear your ideas the way you hear them yourself. It's not just that they disagree or something like that. Rather, inside our own heads, our ideas are sustained by a lot of assumptions and things taken for granted that we are unaware of. It's like singing. Any instrument but the voice is heard by performer and listener in the same way: through the ear. But your voice reaches your ear as much through the inner passages of the head as through the outer ear, so it never sounds the same to you as to someone else. That's why singers are always listening to recordings of themselves, trying to hear what others hear.

So, too, with ideas. They never sound the same to others. And it is crucial to remember that for all save a handful of us, it is their sound to others that matters: to teachers, to readers,

to professional or popular audiences we may wish to persuade. The more arrogant among us find this a hard lesson to learn. You can say things in ways that *you* find perfect, insightful, brilliant. But if other people don't or can't hear them when you present them, you must find a better way to communicate. Otherwise, you will be ignored.

Saying that your own ideas don't sound the same to others is a way of saying that you will always find yourself leaving out crucial aspects of your idea when you talk to other people. Indeed, it is by carefully listening to what other people say in response to your idea—what they add, what they want clarified, what they misunderstand—that you will be able to figure out the essential and inessential parts of the idea. So listen carefully to others' demands for clarification.

At the same time, however, it is true that an idea that requires a *huge* amount of explanation is probably not a good idea. Most likely, it just doesn't work, and the need for explanation is telling you that. Note that these two arguments push in different directions. The first says you should figure out from others what you need to explain or add or remove in order to make your idea work. On that argument, the more problems others have with your idea, the more you can figure out about it. The second says that if you have to do too much explaining, your idea probably isn't good; the more problems other people have with it, the weaker your idea is. The skill of learning from other people—and it is a skill, just like any other—lies in figuring out how to read these two contradictory processes correctly.

The first is the more important of the two. No matter how smart you are, always assume that if other people can't under-

stand you, it's not due to their stupidity, disinterest, envy, and so on, but to your inability to articulate your idea properly. The reason for making this assumption is not that it is necessarily correct; they may well be stupid, disinterested, and so on. But the assumption enables you to get the maximum out of them. Every social scientist learns this from dealing with blind referees (people who review articles for publication in journals; usually they are unidentified colleagues at other universities). One's first reaction to their criticisms is to scream and yell in anger. But even if they *are* fools, the way they misunderstood you tells you how to write better for others.

Some of us don't get angry at negative comments. We find them overwhelming and collapse before them. But even if you *believe* someone who says your idea is junk, you should assume that the reason this smart person thought your idea was wrong was that you didn't say it right, not that the idea itself is bad. That enables you to use others' comments to improve your idea, to raise it to its highest possible level. It may turn out to be much better than you think.

The things you learn from this process of clearing up others' presumed misunderstandings are fairly specific. You learn first about intermediate steps that you left out of your argument; these are hidden stages you may not have noticed and may involve real difficulties. You also learn about the background assumptions that you make—often as part of your general way of thinking about the world—that others do not necessarily share. If you are careful, you will also learn a great deal about the specific (and often contradictory) meanings that people give to words. For example, I called my book about professions *The System of Professions*, more or less because I liked the sound of

that title, which I used for an early paper on the subject. Knowing my book had a title allowed me to feel it was more real somehow during the five years it took to write it. But I have since discovered that many people infer from the word *system* that the book argues that there is some kind of grand intention behind the way professions work, as if all of society's professions were part of a huge plan. In fact, the book says precisely the reverse of that, but I had forgotten what the word *system* means to most readers. Thus, one should remember that social science is a place where most of the basic concepts—identity, structure, culture, nation, and so on—do not have anything like generally accepted definitions. Indeed, this is *always* the first place to look for misunderstanding: the definitions of the words you are using to state your idea.

Note that I haven't said much yet about whether other people think your idea is good. I have talked only about the fact that they are likely to misunderstand it. It is important not to take other people's first reactions to your ideas at face value. This is true whether they think it's a great idea or a bad one. If they think it's great, it could easily be that they don't understand it any more than you do and that it's really a bad idea that you both have misunderstood. Or it could be that they don't really care much and are agreeing in order to be polite. Or it could be that you have an overbearing personality and they're agreeing because it's too much work for them to disagree. The same if they think it's a lousy idea: they could have misunderstood it altogether; they could have understood it but missed its greatness; they could be dismissive people who never agree with anyone but themselves. In sum, don't take the first few reactions seriously.

The first hint that you are past the stage of first reactions comes when you yourself feel confident that you can state your idea clearly, effectively, and *briefly*. The *crucial* moment comes when other people are able to *repeat* your idea to you in such a way that you recognize it and agree with their presentation of it. For an undergraduate trying out ideas for a course paper, this is going to happen after talking to four or five people and hammering out the details. For a graduate student writing a dissertation proposal, this is going to happen after many weeks and many drafts.

Whenever it comes, the ability of others to restate your idea clearly is the watershed. Then you can start to put some faith in their judgment. Of course, you still have to factor in their personalities. Arrogant people like only their own ideas. Negative people don't like anything. Pollyannas like everything. You have to reset your meter based on the person you're talking to. If the negativist thinks it is not the worst idea he or she has ever heard, maybe that's good news. This relativism is true, by the way, for faculty just as much as for anyone else; there are faculty of all types, from thoughtlessly arrogant to hopelessly negative to mindlessly supportive. Although only their own graduate students really know how to read particular faculty members, it's wise to be aware that each has a unique style. You can probably guess most of it, and you need to second-guess the rest.

You will find that it is useful to build up a small group of people who are sympathetic but thoughtfully critical. (The way to do this, of course, is to play the same role for them.) It's also important to keep peddling your ideas in many different places. Your friends get used to you (they start to know, and



make up for, your hidden assumptions) and will ultimately get too easy on you. Finding a group of people who will listen to, read, and reflect on your developing ideas is the most important thing you can do. It is also the hardest.

For those who become serious scholars, the ultimate test of a good idea is the taxi-driver test. If you are on your way somewhere to present your idea and you cannot *in five sentences* explain what you are talking about well enough so that your taxi driver or the person in the adjacent aircraft seat can understand it and see why it's interesting, you don't really understand your idea yet. You aren't ready to present it. This holds no matter how complex your idea is. If you can't state it in everyday terms for an average person with no special interest in it, you don't understand it yet. Even for those working in the most abstruse formalisms, this is the absolute test of understanding.

### III. LITERATURE

I have talked so far about submitting your ideas to your own judgment and your friends' judgment. But what about the relationship of a new idea to previous published work? For undergraduates, this is the hardest bit. It always seems that everything that could possibly be said has been said. There is no room to enter, no place to start. Moreover, when you do think up something startling and new, the literature's reaction (via the faculty) can be incomprehending or dismissive.

The first thing to realize is that it is probably true that everything that could be said has been said, at least at the level of generality at which an undergraduate is likely to be thinking. But this does not prevent faculty themselves from saying the same things again and again—but in new ways, with new

evidence, in new contexts. Indeed, that's what a huge proportion of excellent social science scholarship is: saying the old things in new ways. (If we didn't say them again and again, we'd forget them, which would be a bad thing.) What faculty know that students do not know—and what enables them to accomplish this turning of old things into new ones—is the conventional nature of the literature. They know which old things can be resaid and, indeed, which old things *need* to be resaid. They know how the literature defines the border between restating something and stating something new.

This system of conventions is mostly invisible to undergraduates and even to most graduate students. Suppose you take a stratification course. You read the stratification literature. There are a lot of questions that occur to you about that literature that most people writing in it don't seem to worry about. For example, why should we judge somebody's success by how well he or she was doing in a particular year? Why should we assume that everybody judges success by the same scale? Why do we think about a family's social status by asking the job of the husband? Indeed, why is measuring social status more important than measuring, say, personal judgments of well-being or satisfaction? And so on. Occasionally, these things do get written about, of course. But in the main, the stratification literature goes on happily envisioning new puzzles and issues without thinking about these questions for a second. They are ignored by common agreement. Yet they seem of burning importance to an undergraduate, and rightly so.

As I have said throughout, literatures work by making simplifying assumptions about some things so that researchers can

do complex analyses of other things. That's the nature of the beast. It's not possible to do social science—by any method whatsoever—without making simplifying assumptions. They facilitate research by preventing people from bogging down in preliminaries. So survey analysts make assumptions about how attitudes relate to behavior, and ethnographers make assumptions about how informants do and do not twist the truth. And such assumptions usually go well beyond the methodological preliminaries. They get into the very details of the substance, as I just noted in the case of stratification research.

Faculty know these conventions so well that they are usually quite unaware of them as conventions. As a result, many ideas that occur immediately to undergraduates seem ridiculous to faculty. "We showed years ago that that didn't matter," "That's more a question of method and technique than substance," and "That's really not what is central here" are typical reactions to what seem like obvious questions to a bright undergraduate. All of these may mean that the faculty member has forgotten that your idea is a legitimate question because it has been set aside conventionally by the literature. (These statements don't *necessarily* mean that, of course, but they may.)

Often, as we saw in Chapters Three through Six, a good idea is one that pushes one or another of these conventions. But a good idea doesn't try to push several conventions at once. So, to continue the stratification example, it would be interesting to ask what happens to the standard relationship between education and family social status if we used the wife's job prestige instead of the husband's as the indicator of family social status or if we used some average of both. Such a study would contribute to the literature precisely by opening up one of its

conventional assumptions to further analysis. But suppose one changed indicators on *both* sides of the relationship, not only moving to the wife's job prestige as the status indicator but also changing education from degrees or years in school (the standard indicators) to a true outcome variable, like SAT scores, for example (on the assumption that the SAT actually measures prior achievement and schooling more than it measures schooling-independent talent). This would restrict one's attention to the college bound, as well as changing one's conceptual idea of the meaning of education. And now the study begins to lose its relation to the traditional stratification literature, where it is conventional to think about stratification in terms of breadwinner employment and where it is customary to consider education in terms of credentials (with their more direct link to occupation and income) rather than achievement scores (which measure a less actualized but perhaps more general resource). So you would have done a doubly brilliant study, but one hanging in midair as far as literatures are concerned.

Conventions play an important role in all methods and literatures. A historically inclined student might be interested in changes in the patterns of lawyers' careers over the twentieth century and decide to approach it by reading twenty or thirty biographies of lawyers in order to develop a schematic model of lawyers' lives. But a faculty adviser would probably make the largely conventional judgment that the student should move either toward a quantitative analysis, digging up simple information for a much larger but random sample of lawyers throughout the period, *or* toward a detailed study of two or three lawyers suitably spaced through the century. The convention is either to be fully scientific, with a defensible strategy

and agreed-upon career measures, or to be deeply interpretive. Yet against the first plan, one could easily argue that changes in the nature of lawyers' jobs meant that coding categories, like "working for a law firm," meant something completely different in 1900 than they did in 2000; in that sense, there is no stable categorization of jobs that will enable meaningful coding over the century. And against the second plan, one could argue that its sampling is so arbitrary that any conclusions are spurious. Nonetheless, the conventions are that you probably can do the positivist version or the interpretive version, but you will have trouble writing about twenty to thirty lawyers' lives in the middle.

Dealing with conventions is another of these damned-if-you-do, damned-if-you-don't things. Everybody agrees that whatever else it does, the best work nearly always overturns some conventions. At the same time, the general preference is to obey conventions, especially when one is starting out. So you can obey the conventions and have people think you unadventurous or disobey them and have people reject or misunderstand what you are doing. For students, the best way to learn the research conventions is of course to look at current work, and the easiest way to generate feasible ideas is to clone an existing project by changing one detail: getting a new variable, changing the time period examined, adding some more cases. (This is the additive heuristic of Chapter Three.) But this invites the charge of timidity.

There is no way out of this dilemma, which is, after all, the dilemma of creativity in social science writ small. It is important, nonetheless, to know about the problem of conventions, because it is the key to understanding how the professionals in

your world—meaning people who know a given area better than you do, be they older students or faculty—will react to your ideas. Often, faculty push students toward following conventions for the very good reason that unconventional work is much harder. Students' research plans are often unrealistic in the extreme, and faculty are trying to encourage students' interests while helping make the research more feasible. Urging students to learn conventional research models and to write conventional papers is a way of doing that. A student needs to be aware of this complex tension between convention, originality, and feasibility—and to be willing to make some compromises if necessary.

#### IV. TASTE

Conventions and the problem of knowing them bring us to the matter of taste. Judging one's ideas becomes much easier when one begins to acquire scholarly taste. By taste, I mean a general, intuitive sense of whether an idea is likely to be a good one or not. It is of course important not to become a slave of one's taste, to try new things as one tries new foods. But developing a sense of taste makes things a lot easier.

The foundation of good taste—like the foundation of good heuristic—is broad reading. It is not necessary that all the reading be of good material, only that it be broad and that it always involve judgment and reflection. A musical metaphor is again useful. A good pianist always practices not only technique and repertoire but also sight-reading. Broad reading for social scientists is the equivalent of sight-reading for pianists. A pianist practicing sight-reading grabs a random piece of music and reads it through, playing steadily on in spite of

mistakes and omissions. So, too, should you just pick up pieces of social science or sociology or whatever and just read through them, whether you know the details of the methods, see the complexities of the argument, or even like the style of analysis. The obvious way to do this is to pick up recent issues of journals and quickly read straight through them.

You learn many things from such broad reading. You learn the zones of research in the discipline. You learn the conventions of each zone, and you figure out which you like and which you don't like. You learn what interests you and what does not. Of course, you should not let your interests dictate your reactions, just as you should disregard, when you are "sight-reading," conventions with which you disagree. When you find you don't like a paper's methodology and you think its concepts don't make sense, force yourself to go on and ask what there is that you *can* get out of it—perhaps some facts, a hypothesis, even (in the worst case) some references. In the best disciplinary journals, every article will have something to teach you, even those articles that lie completely outside your own preferences.

This is also a useful rule for seminars and lectures, which are another useful place to develop your taste. There is no point in sitting through a lecture or talk whose methods you hate, self-righteously telling yourself about the "positivist morons" or the "postmodern bullshit" or whatever. All that does is reinforce your prejudices and teach you nothing. Judge a talk or a paper with respect to what it is itself trying to do. This is hard, but by working at it, you will gain a much surer sense of both the strengths and the weaknesses of your own preferences. You will become able to gather useful ideas, theories, facts,

and methodological tricks from material that used to tell you nothing.

You will, of course, run into plenty of bad stuff: bad books, bad papers, bad talks. The symptoms are usually pretty clear: pontification, confusion, aimlessness, overreliance on authorities. Other signs are excessive attention to methods rather than substance and long discussions of the speaker's or writer's positions on various important debates. But even bad material can teach you things. Most important, it can teach you how to set standards for an article or talk on its own terms. What was the writer trying to accomplish? For the truly terrible, what should the writer have been *trying* to accomplish? This last is the question that enables you to judge material on its own grounds, by imagining the task it should have set itself.

Of course, it is also important self-consciously to read good work. Oddly enough, good work will not teach you as much as will bad. Great social science tends to look self-evident after the fact, and when it's well written, you may not be able to see what the insight was that instituted a new paradigm. What you take away from good work is more its sense of excitement and clarity, its feeling of ease and fluidity. Not that these are very imitable. But they set an ideal.

How does one find such good work? At the start, you ask people you know—faculty members, friends, fellow students. You also look at influential material, although—again oddly—there is plenty of influential material that is badly argued and opaque. Soon your taste will establish itself, and you can rely more on your own judgment. There is no substitute for practice and, in particular, for "sight-reading." You just need to learn to read and make judgments, always working around

your own prejudices to separate bad work from work you simply don't like.

Developing this taste about others' ideas is a crucial step toward judging your own. Even given all the hints scattered throughout this chapter, judging your own ideas is the hardest task of all. The only way to become skilled at it is to acquire general taste and then carefully and painfully turn that taste on your own thinking. The skill of learning to find good and bad things in the work of others can be the best help in finding the good and bad things in your own work.

## V. PERSONALITY

Part of developing a taste for good ideas is getting a sense of your own strengths and weaknesses as a thinker. You must eventually learn to second-guess your scholarly judgments. This second-guessing comes from understanding your wider character as a researcher and thinker: your intellectual personality. Your intellectual personality is based on your everyday character, of course, but builds on it in surprising ways. The strengths and weaknesses of your intellectual character decisively influence the way you evaluate ideas and, indeed, everything about the way you think.

It is important to realize from the start that every aspect of your intellectual character, like every aspect of your everyday character, is both a strength and a weakness. In the everyday world, what is precious loyalty in one context is mindless obstinacy in another. The same two-facedness is true in the research world. What is daring analogy at one point is dangerous vagueness at another. So let us consider some character traits as intellectual virtues and vices. You need to figure out for your-

self where you are on each scale. It is true, though, as Mr. Darcy says in *Pride and Prejudice*, that "[t]here is . . . in every disposition a tendency to some particular evil, a natural defect, which not even the best education can overcome." Each of us has at least one great weakness; understand it, and you come a long way toward controlling it.

Let us consider some important qualities of intellectual character. Take orderliness, for example. It is painfully obvious that orderliness is absolutely necessary for any major research project. A keen sense of research design, a mania about careful records and filing, a deliberate discipline of analysis—these are the avatars of orderliness necessary to undertake any major research enterprise, from an undergraduate paper to a multi-investigator project. But orderliness can also be important within thinking itself. It is very helpful to have an orderly mind. When you write out a big, long list of ideas, it's very useful to have the habit of rearranging the ideas every now and then into categories, changing the category system from time to time, to make it better and better. So in writing this chapter, I first wrote down dozens of free associations about judging ideas. Then I put them into a set of categories; there seemed to be some about talking to yourself, some about talking to others, and so on. Later (after adding some more ideas), I put those categories in an order for writing, figuring to move from the individual to the group and the literature and from the specific qualities to more general ones. Once I saw this emerging outline, I saw that I needed to split up one category and relabel a few others. I then sat down to write the chapter, creating categories *within* my headings (for example, the different types of personality qualities) and setting those in order as I came to

write each section. This is a useful strategy for me, because I get worried when I've got a long list of somewhat related ideas but no clear structure for it.

Obviously, orderliness of thought is a good quality in mild doses. But as a dominating characteristic, it has problems. It is at the root of the reclassification papers mentioned earlier, papers whose only aim is to pull some idea or phenomenon out of one pigeonhole and put it into another. Pigeonholers also have a hard time finding phenomena *genuinely* puzzling. Their main concern is getting things into the proper boxes. Even worse, sometimes the pigeonholer has a personal, idiosyncratic set of boxes that other people don't have. Such pigeonholers often take things and deform them considerably to get them into classifiable shape. They can't leave things ambiguous and open. Yet this ability to leave things unresolved is absolutely necessary to a serious thinker.

Thus, orderliness is a quality that can cut both ways. So, too, is loyalty, in particular, loyalty to ideas. On the one hand, a certain loyalty to ideas is a great strength. Often a good idea doesn't show its colors for a while. It resists or evades. Loyalty to your ideas in the face of various kinds of criticisms is a strength. At the same time, it can become a liability. You have to know when to give up on ideas, when to set them aside and move on. Most of us have a little museum of cherished notions that have had to be rejected for this or that reason, much against our will. It's OK to keep these ideas in a personal museum, but they should probably stay there.

Another quality that cuts both ways is habit. There are many habits that are very useful. It is useful to have the habit of automatically verifying the logical structure of one's ideas

before considering them further. It is useful to have the habit of listening to others as well as oneself. It is useful to know the conventions and usual disciplines of one's research area. At the same time, habit can become paralyzing. It can lead one to accept dead conventions. It can hide the paths of imagination completely.

Also two-faced is breadth of interest. There is something wonderful about a great breadth of interest, an ability to see the many things relevant to any given issue. Breadth of interest can open the doors to powerful analogies. It can bring distant methods to new uses. At the same time, excessive breadth (and depth) of interest can, like habit, be utterly paralyzing. In fact, the need to say everything one knows in every single paper is the most common single disease among young researchers. And excessive breadth of interest can lead to a variety of other pathologies: to pigeonholing, because only that can deal with such diverse interests; to arbitrary argument, because it will bring things together somehow; to sheer paralysis, because the range of topics is too great.

Related to breadth of interest is another quality with varying impact: imagination. It may seem odd at the end of a book aimed at increasing imagination to mention that it's possible to be too imaginative, but it is worth reflecting on imagination. There is more than a grain of truth in Edison's "genius is 99 percent perspiration and 1 percent inspiration." Ideas *do* need to be worked out. The working out is not easy. It is all too comfortable to avoid recasting one's ideas because "others don't see the imaginative links I have made," and so on. Most of the time when your ideas don't survive the tests presented earlier in this chapter, they're bad ideas. If they don't sustain—indeed,



call out for—careful elaboration, they're probably just flimsy analogies with nothing in them. So watch out for congratulating yourself on your imagination. It can be a cover-up for flimsy thinking.

There is also an underlying personality difference at issue here. Some people have a tendency to see things as alike (by making analogies); others see things as different (by making distinctions). Many years ago, the personnel directors of Bell Laboratories found these tendencies to be so strong that they tried to make sure that S (similarities) engineers worked for S bosses and D (differences) engineers for D bosses. This quality of seeing similarities or seeing differences is captured in the old mathematics joke that a topologist is a mathematician who can't tell a doughnut from a coffee cup. (A doughnut and a coffee cup are topologically equivalent, since a plane intersecting them can intersect two disconnected parts, something that can't happen with a pencil or a tennis ball, which are topologically equivalent to each other but not to doughnuts or coffee cups.) Topologists are *very* abstract mathematicians. Things that look utterly different to the rest of us look alike to them.<sup>1</sup>

As the Bell Labs reference makes clear, this quality of seeing similarities or differences takes on much of its color relationally, from the habits of others around you. To be an S person in the midst of a group of Ds can mean that you're treated as a visionary or a visionary crank. To be a D in a group of Ss can define you as a plodding pigeonholer or as someone with his or her feet on the ground. It is worth trying to figure out your general habit. Do you look for similarities? build down from abstractions? make strong assumptions? Or do you see differences? build up inductively? keep all the details straight? As

with so many qualities, it is best to alternate between these styles if you can.

We come now to the more publicly evident qualities of an intellectual personality. Of these, by far the most important is self-confidence. In general, everyone in academia thinks he or she can judge the self-confidence of others by noting how much they talk. In fact, there's much else involved in talking too much. People can talk a lot because they know a lot or because they come from talky cultures or because they are trying to persuade themselves that they have something to say or, in some cases, simply because they are arrogant.

There is probably nothing more important than coming to a good sense of your own degree of self-confidence. It's pretty easy to tell if you have too much self-confidence. If you can't quickly think of two or three people who have recently taught you something important about a topic you thought you knew well, you are probably too self-confident. If you do most of the talking in most of your classes or in groups of friends, you are probably too self-confident. If you don't have to rewrite most of your papers three or four times, you are probably too self-confident. If you can't take criticism, you are probably too self-confident. Generally, overconfident students are unaware of their overconfidence. If they do recognize their tendency to domineer, they may put it down to other things: educational advantage, prior study, desire to help others, and so on. By contrast, students who lack self-confidence are usually quite aware of their timidity, but they often do not see it as their problem so much as that of other students, who (they think) domineer.

In an odd way, people who have too much self-confidence have much the same problem as people who have too little.

Neither one gets the feedback necessary to learn from others. People with too much self-confidence don't pay attention to what others have to say, even if they give them time to say it. They therefore lose most of what other people have to tell them. This makes their own intellectual development harder. They are only as good as their own ability to judge and improve their ideas. They don't find out about facts that others happen to have noticed. They don't hear that others have tried out certain intellectual paths and found them useless. It's as if Mark Granovetter's job seekers (in Chapter Four) were trying to find jobs on their own, without all the weak ties—you can do it, but it takes a long time. The short-run reward for such people is always being right. But the long-run costs are great. They deny themselves the help others can give. Only truly outstanding talent can make much headway with such a handicap, and even then only at the price of incredible labor.

People who lack self-confidence also lose what others have to tell them, but not because they don't listen. Rather, they listen too much, never risking their own ideas independently. As a result, they often end up following the lead of something outside themselves—a book, a friend, a teacher—and never really learn to think for themselves. They can do well under certain academic conditions—particularly if they are students of an overconfident teacher, but they cannot learn to think on their own because they do not risk their own ideas.

Finally, a few words about the emotions of ideas. Having good ideas can be an emotional business. You need to recognize when those emotions take over. For those of us who analogize (as I do, for example), there are moments when we get into an analogizing mood and everything in the world looks like mar-

kets or networks or nested dichotomies or whatever our fascination is for the moment. It's like falling in love. Everything you read seems to fit the analogy perfectly, just as everything about the person you fall in love with seems to fit perfectly with your interests and desires. Feelings can be just as strong for other styles of intellectual personality. The pigeonholer can ponder, with sweet indecision, which might be the best of four or five ways of viewing patrimonial bureaucracies, all the while speculating on the many details one might use to place them better as a type of administration or, perhaps better still, to break them down into patrimonial bureaucracies set up as such and patrimonial bureaucracies deriving from the gradual breakdown of rules in meritocratic administrative systems. Every intellectual personality has its moods of excitement, when hard work becomes pleasure and Edison's 99 percent perspiration suddenly disappears into the 1 percent genius.

As in love, so here, too, it is worth surrendering yourself to the excitement for a while, maybe for a good, long while. Indulge yourself. Wallow in your ideas. But remember that ultimately ideas are for communicating to others, so you have to stand back and judge them, just as you have to stand back and decide whether to move in with or marry someone you love. An idea you become serious about is just like somebody you live with. You get familiar with it. You use it daily. You see it wearing a bathrobe and slippers, without its makeup or aftershave. But you should feel you can never come to the end of it, that it retains the sudden enticement and novelty that grabbed you to begin with, that it continues to challenge and provoke. You shouldn't move in with an idea that doesn't have that kind of endless power and excitement.

The love metaphor suggests something else important. Remember that you and your idea need to spend time alone, without distraction. That means no music, no TV, no talking roommates. Do what you must to create a private world in which you can get to know your idea in depth. For me, it means (I confess it) walking around and talking aloud to an invisible companion about my idea. (My invisible companion doesn't mind when I say things twice or resay them or get boring or whatever, which is very useful.) Somehow, talking my ideas through to someone imaginary makes me more conscious of how others will hear them. (Of course, it's also great fun; an imaginary listener always knows just how far to push you and when to shut up.)

You will do something different, no doubt: perhaps sit in a certain place and look at certain scenery, perhaps clear your mind with certain music before sitting down to think, perhaps take a long walk. The point is that ideas—like the social reality I discussed in the opening pages of this book—have to be wooed to be won. They don't just show up fully dressed and ready to step out for a lovely evening on the town. And they want your full attention, not part of it.

## VI. PUZZLES

All of this brings us to my final topic: the question of puzzles. In the very beginning, I suggested that one of the odd qualities of social science is that we often start a project with only a relatively general interest in an area. Finding the real puzzle and finding its solution occur together as we go forward. I now need to clarify that idea.

What does it mean to say that we start out with a general interest and aren't clear at first what our puzzle is? Consider the rare reverse case: once in a while, a research project starts with a striking, clear, puzzling fact. I once noticed that status rankings of professionals within professions were different from status rankings of professionals by those outside. Professionals themselves give highest respect to colleagues who have little to do with clients: consulting physicians, lawyer team leaders, elite researchers. The public, by contrast, gives highest respect to front-line, hardworking professionals in the thick of client problems: primary-care physicians, courtroom attorneys, classroom teachers. Why should this be? I was working on the psychiatric profession at the time, and this empirical puzzle simply occurred to me one morning while I was thinking about the fact that high-status psychiatrists talked to upper-middle-class clients with minimal difficulties while low-status psychiatrists worked in mental hospitals with mostly lower-class clients with huge difficulties, as I and most people then imagined most psychiatrists did. It was one of those rare occasions when there is an obvious empirical puzzle and a straight march of the research from puzzle to solution.

Most of the time, however, clear puzzles don't appear in data. We are more likely to start out by playing at normal science with our data, trying out all the old additive tricks: What is the effect of another variable? Does such and such a finding hold up in another setting? At the same time, we are generally being urged on by the general (and insoluble) problems that probably got us into social science in the first place: Why does society have the statuses that it has? How does real social

change occur? What drives the division of labor? How are prices and values established? Interesting as these problems are, they are nearly devoid of real content. We can't directly reason about them because the very words in them have infinitely contestable meanings. Status, social change, division of labor, price, value—none of these has a fixed, context-free meaning.

So most often, we find ourselves with a general concern of this type, a mass of data that we can see as relevant to that general interest, and a hunch that bringing the concern and the data together will lead us to a more specific puzzle and a solution. The real issue is how we recognize a puzzle in this amorphous confrontation between interest and data.

Like coming up with ideas, finding things puzzling is very much a matter of taste and knowledge. The knowledge part is obvious. You can't tell whether something is puzzling unless you expect it to be different from what it is. That expectation rests on what you already know. So the basis for finding things surprising is knowing about things that aren't surprising. This is why undergraduate majors require survey courses and why graduate programs (ought to) have general examinations. You have to know the background before you can see that something doesn't fit into it. Note that this explains why people who write pure social theory never come up with much. If you don't know anything about the world, it's hard to see what parts of the world call out for explanation. You end up writing theories of theories.

But there is an issue of taste involved as well. Seeing things as puzzles means being willing to live with ambiguity. If your first instinct with any unusual fact is to jam it into a category or to rationalize it in terms of your favorite idea, you are going

to have trouble seeing puzzles. Our minds are powerful rationalizers, and seeing puzzles means, in part, shutting down that powerful pattern-making machine or, more precisely, letting it drift a bit. Note that this is another place where excessive self-confidence gets in the way. Self-confident people, particularly of the arrogant variety, aren't happy running the engine on idle for a bit. But that idling often helps in seeing puzzles; *not* having the instant answer is what leads to success.

Some of us rely on external puzzle generators. Thus, for many social scientists, puzzle recognition originates in political or moral commitments. The 1960s was a time of strong political and moral commitments—of many different kinds—and those who entered social science in that period usually had a sense that inequality, war, social change, and so on, were burning concerns. No matter what the particular direction of their commitments, these people came to social science already thinking that these phenomena were deeply interesting. They might have thought inequality was wrong, or they might have been angry with people who thought inequality was wrong, but they all thought inequality was extremely important and in many ways puzzling.

The danger of the moral-political source for puzzles is that one always sees the same puzzle. The result is what one of my female colleagues dismisses as "research of the form 'add women and stir.'" Such research is not terribly interesting because it soon becomes relentless normal science. The moral-political source for puzzles works only if one allows new puzzles to grow perpetually within one's broader concern. So you can start with the puzzle of explaining why women and men seem so often to behave differently but then go on to

worry about why it is that within women's groups we often see repeated many of the patterns of difference that we see between the sexes. These subpuzzles can often be in tension with the original driving puzzle, however, and so tend to force a choice to either stick with the original puzzle or allow the subpuzzles to take on a logic of their own. Among the best of the politically-morally motivated, it is precisely the tension between these two logics that drives their creativity.

For some people—this is more characteristic of generations after the 1960s—the social world is perplexing because they are perplexed by their own position in it. The most common form of this attitude today manifests itself in what we usually call identity research. This is research motivated by and focused on some particular identity or attribute of the researcher: gender, ethnicity, race, or whatever. Often identity research takes the form of “Is there any sorrow like my sorrow?” in which case we have the strengths and weaknesses of the political-moral puzzles I just mentioned. The strength is strength of commitment and depth of interest. The weakness is the danger of bias and relentlessly unimaginative normal science.

One can also be driven to study divorce or disability or schooling or wealth because of immediate personal experiences that may not be identity related. If you talk with faculty members at any length, you will find a surprising number whose motivations are of this kind. It is sobering that usually these “experience-motivated” faculty members are reacting to unhappy experiences. Tolstoy was right when he said that “all happy families are alike, but an unhappy family is unhappy after its own fashion.” To judge by social science practice, there is something quite uninteresting about positive experiences. Lit-

tle is written about them, although a school of “well-being” research has finally taken root on the frontiers of psychology and economics.

The most important weakness of these personal motivations is not one from which students suffer. It is, rather, a problem for middle-aged faculty. If we figure out our basic puzzle, we don't have a new source for problems. Perhaps it is this that explains the surprising number of social scientists who undertake passionate research as young professionals and then go to sleep intellectually in middle age, as their personal problems loom smaller in a life filled with marriage, children, students, hobbies, professional and institutional eminence, and so on.

There are, then, personal sources for puzzles as well as social ones. All of these various sources can be dangerous because they give us particular desires for particular kinds of results, because they can get mindlessly routine, and because they are good only as long as the personal and social concerns last. But they also can provide an energy and passion that drive our need to understand a puzzling world. These are the driving forces behind most great social scientists.

There are those, finally, who simply find the social world intrinsically interesting and puzzling, just as some of us wanted to know all about snakes or tadpoles as little kids. Lucky people. And to be blunt, very rare people. For every person whose passion for social science comes from truly disinterested curiosity, there are dozens whose passion arose originally from personal and social concerns. Faculty who are deeply puzzled about the social world without having a personal or social agenda are often the hardest to come to know. Their passionately disinterested curiosity seems strange to the majority of

us, who have come to social science from personal and social concerns. But they are always among the most creative.

A rich vein of puzzlement is then something that all good social scientists have, whether they are beginning undergraduates, graduate students, or senior professors. Whatever its source, this puzzlement becomes a compulsion to figure out the nature of social life. When you find faculty who have it, learn from them. They will have their faults, often great ones, but they have much to teach and are themselves willing to learn. These are the people who will help you find your own gifts of sociological imagination.

Bear in mind, however, that there are active and even talented social scientists who *don't* have this creative puzzlement. These are faculty members who do social science not for love but for a living, going through conventional motions often with considerable success, a success they value more highly than inquiry itself. You will recognize them by their behavior: one is smart but condescending and uninterested; another is eminent but conventional and stale. When you go to office hours and meet such people or their cousin the bland, busy professional with all the answers but no ideas, extricate yourself graciously. Such people have nothing to teach you.

Above all, what they lack is imagination. I said at the outset that social science is a conversation between rigor and imagination. Just as rigor can be practiced and mastered, so can imagination be developed and cherished. I hope in this book to have suggested some useful exercises for doing that. But I have only suggested. It is now for you to find the excitement that comes with inventing your own heuristics and reimagining the social world.

## GLOSSARY

- additive heuristic. The heuristic move of doing more of the same: finding more data, making a new dimension of analysis, making use of a new methodological wrinkle.
- argument heuristic. The heuristic move that turns a familiar argument into a completely new one. The main argument heuristics are problematizing the obvious, reversal, making or denying radical assumptions, and reconceptualizing.
- behaviorism. The position that one cannot measure (or study) the meanings that actors assign to action. One can study only behavior: external actions that are measurable in a reliable and replicable manner. *Opposed to* culturalism.
- case study. A study of a single, particular social actor, object, or situation.
- causality. The reasons things occur. Causality was thought by Aristotle to come in four brands (material, formal, proximate, and final) and by Hume to be unknowable (we can know only regular patterns, not their causes). It is a shibboleth of standard causal analysis.
- cluster analysis. A quantitative technique that sorts objects into groups based on information about resemblance or distance between the objects. *See also* data-reduction techniques.
- conflict/consensus. The debate over whether disorder in social life results from disorderly and oppressive institutions (conflict theory) or from insufficient regulation of inherently disorderly individuals (consensus theory).
- constructionism. The position that the things and the qualities of things encountered in social reality are continuously reproduced anew in interaction. *Opposed to* realism.
- contextualism. The belief that social facts make no sense when abstracted from the other social facts that surround them in social time and space.
- correlational analysis. A form of quantitative analysis based on the study of the covariation of variables.
- culturalism. The position that the symbolic systems of culture can and must be studied. *Opposed to* behaviorism.
- culture. The symbolic systems by which social actors understand, experience, and direct their lives.