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Chapter Four GENERAL HEURISTICS Search and Argument

I. SEARCH HEURISTICS

- A. MAKING AN ANALOGY
- B. BORROWING A METHOD

II. ARGUMENT HEURISTICS

- A. PROBLEMATIZING THE OBVIOUS
- B. MAKING A REVERSAL
- C. MAKING AN ASSUMPTION
- D. RECONCEPTUALIZING

economics until the 1970s. (Coase won the Nobel Prize in 1991.) The anthropologist Fredrik Barth's *Models of Social Organization* was published in 1966 but didn't become a classic citation until much later. Ludwik Fleck's pioneering book on scientific thought styles lay fallow from its publication in 1935 until it was repopularized by Kuhn in the 1960s and finally translated into English in 1979.

That people took so long to recognize the creativity of these works perhaps tells us something important about the nature of creativity. Much of it has to do with how one's ideas fit with others' current beliefs. Creativity is *relational*. Coase's work went unappreciated until the rest of the economics community came around to the broad conception of economic thinking that Coase took for granted. Fleck's book was completely ignored until Kuhn's *Structure of Scientific Revolutions* prepared people for it. Often a mainstream cannot see new ideas as creative. Often it cannot see them at all.

CHAPTERS FOUR AND FIVE DESCRIBE general rules for coming up with new ideas. I shall illustrate these heuristics with a variety of examples drawn from several disciplines. The examples are illustrative, not definitive. The reader should not get the idea that a particular example illustrates one and only one heuristic. In fact, I end up reusing some examples. Just as there are several ways to think about any given method, there may be several ways to interpret the intellectual moves of any given article or book.

I shall also use some examples that were perceived as clever only a long time after they were written. Such papers are curiously common in the social sciences. The economist Ronald Coase's celebrated paper on the nature of the firm was published in the 1930s but did not become a touchstone of modern

This tells us about an important limitation on the practice of heuristic. You can easily be too radical for an audience. If you aim to have an impact, you have to adjust your heuristic gambits to your audience—whether it is a bunch of college friends, a seminar, or a subdiscipline. Note also that the cyclical relations among methods and the fractal character of social scientific debates mean that it is quite possible to be too radical for one group while being insufficiently radical for another. Practitioners of SCA might find Fleck's view of the conditionality of facts so radical as to be irrelevant, while contemporary sociologists of science would find him tame.

This rule—be different but not *too* different—takes us back to some earlier themes. As I said in Chapter One, the aim of

social science is to say something interesting—perhaps even true—about social reality. We have some conventional ways to do that, which we call methods. The rule to be different but not too different reminds us that each methodological community has its own sense of how far is too far. It changes from time to time, of course. Many sociologists my age remember well the kid gloves with which we handled multiple regression in the 1960s, before it could be done in nanoseconds by eleven-year-olds. We always tested for interaction; we always reported variances. No such care exists today. There are, however, *never* rules about what's OK.

The heuristics in this book will sometimes take you clean out of whatever standard world you're currently in. That's the fun of it, as far as I'm concerned. But you should be advised that once you're outside the usual methodological communities, there are a lot of things that make strange noises in the social scientific night. That's why methodological communities and the addition heuristic exist—so you won't have to deal with those things on a regular basis if you don't want to.

In this chapter and the next, I discuss general heuristics. Unlike those in Chapter Six, these do not derive directly from the fractal debates of Chapter Two. They are tested ways of broadening what you are doing, ways to come up with new ideas, new methods, or new data, ways to get unstuck. Remember that these are not specifically aimed at any particular phase or aspect of the research process. They are useful at various times and in various ways.

I will discuss two kinds of general heuristic gambits in this chapter. The first are search heuristics, the simplest form of

general heuristic. They involve seeking out new data, methods, and ideas. They are the first step beyond the additive heuristics of the preceding chapter. The second are argument heuristics. These are ways to play with or pose arguments in order to create openings for ideas. Like search heuristics, argument heuristics are general strategies for producing new ideas. But rather than helping you look outside your problem or data or way of thinking, argument heuristics help you look within, bending what you have into new shapes and new uses.

I. SEARCH HEURISTICS

Search heuristics are ways of getting new ideas from elsewhere. When you use search gambits as heuristics, you are betting that someone else has already thought seriously about your problem or something like it and that you can borrow that thinking. The central search heuristic is analogy. It could be an analogy about data: "the marriages I am studying are really like negotiations in business." Or it could be an analogy about a problem: "the problem of trying to explain why unions fail is just like the problem of trying to explain why X-ray machines fail." Note that in the second case, we aren't saying that unions are like X-ray machines, only that the process of failure has a certain logic to it in any circumstance.

A specialized but important search heuristic is the borrowing of methods. Borrowing usually involves analogy but goes beyond it to invoke not only some ideas but also a whole apparatus of analysis. It can be quite general or narrowly specific. Let us now look at these two search heuristics in detail, with some famous examples.

A. Making an Analogy

The first and in many ways most important of the general heuristics is making an analogy: saying that an X is really a G. (See? I surprised you—you were expecting Y. That would have come next if I were using the *additive* heuristic.) Examples of analogy are common in creative social science. Applying rational-choice models to explain state formation in feudal times means making an analogy between feudal kings and modern rational actors. Applying ecological models to humans—Park and Burgess applied them to cities in 1925, and Hannan and Freeman applied them to organizations in 1977—means making an analogy between human societies and biological systems. Applying economic models to family planning means making an analogy between people having children and people buying hamburgers.

These may seem like far-fetched analogies, but they were very productive. Consider the “economic” analogy. Gary Becker, the greatest apostle of this analogy, began his career with what was at the time a truly astonishing book, *The Economics of Discrimination*. Suppose, Becker said, we think about racial discrimination as basically an economic phenomenon. We can estimate a “price” of discrimination by the following method: We compare the hourly wages paid in southern textile mills that employ all-white labor forces with wages paid in mills employing mixed or all-black labor forces. The difference will be the price the factory owner is willing to pay for his discrimination, as if he were buying it like a suit of clothes. We can then bring all the apparatus of microeconomics to bear on that price, analyzing how it fluctuates with labor demand and supply, studying the trade-off between spending one’s money on dis-

crimination versus spending it on other things (new capital for the plant, for example), and so on. Becker’s analogy must have seemed shattering at the time. Indeed, nobody outside the economics profession paid a lot of attention to *The Economics of Discrimination*. But the analogy was powerful, and when Becker began to analyze more mainstream topics, like family-planning decisions, his work began to be regarded as truly revolutionary.

Analogy is fundamentally different from addition. It means truly changing the terms of analysis, not simply adding something to them. It has a risk to it: there will be naysayers. At the same time, it can be very productive.

Many analogies take the form of Becker’s, which begins with the theory and method and moves toward the data. The Becker claim was really “You may think that phenomenon X cannot be analyzed with my theory/method T, but in fact you’re wrong: it *can* be.” It is equally common for people to start from the data and use analogy to find new theories and methods. That was the source of the ecology analogies mentioned above. Park and Burgess looked at the raw complexity of the city of Chicago and asked whether the city looked like something that someone else had already come to understand. The answer was that it looked like the thing biologists call an ecology. So one way to understand it was simply to borrow the language and some of the analytic machinery thought up by biologists to analyze complex natural systems: the city is an ecology. Ditto for Hannan and Freeman, with their ecological approach to organizations. Organizational fields, too, can be seen as ecologies.

Looking for analogies from the data end is the more common experience for students. Suppose you are interested in the

way cities are governed. The usual line of analysis treats this problem quite traditionally, as a question of understanding politics: voting, councils, bureaucracies. But it is perfectly possible to treat city government completely as a problem of economics: economies of favors, economies of patronage and politics, economies of location. In this analogy, city politics becomes simply an economy, and you can apply to favors, patronage, and decision-making all the machinery of economics: supply and demand, trade-offs, budget constraints, elasticity, and so on. You may not end up writing the final paper using the economic language, but under whatever surface rhetoric you *do* use, you can employ the borrowed arguments and ideas to understand things that may seem puzzling when you think of them purely in traditional terms as problems of power, authority, and influence. As this example makes clear, one of the useful aspects of analogy is that most often the ideas you borrow will be quite well worked out. When you forage in other disciplines and subdisciplines, you will find the intellectual supplies plentiful and well kept, ripe for the taking.

Analogies don't always work, not even the ones that make it into print. In two essays, the famous sociologist Talcott Parsons once gave an analysis of power and force in economic terms (1967a,b). He treated power as a medium of exchange, exactly like money. He treated force as the "gold" backing up the power ("money") system. He reflected on the uses of embodied power ("capital") to produce political growth (exactly analogous to economic growth). All of this hinged on a simple, direct analogy between power and money.

The two papers carrying out that analogy are brilliant but somewhat bizarre. They are brilliant because they make us

think about power in a completely new way. They are bizarre because Parsons never used the analogy to question the distribution of power to individuals. Yet this is the basic topic of politics—who gets what where, how, and why?—though not that of economics (other than Marxian economics). This example teaches another useful lesson: in analogy, something centrally important can be lost—in addition to the something gained—unless we are very careful.

Note that analogy is not simply a matter of going to other disciplines and other bodies of knowledge. It is first and foremost having the ability to break out of the standard frames we put around phenomena. Having this ability means seeing, for example, that there is a close similarity between schools, prisons, and mental hospitals (David Rothman, *The Discovery of the Asylum*); that bodily fluids like mucus and semen cross boundaries in the same way unclassifiable objects do (Mary Douglas, *Purity and Danger*); that everyday interaction can be treated as drama (Erving Goffman, *The Presentation of Self in Everyday Life*). Obviously, it is crucial to know when and how an analogy works; after all, many people besides Goffman have seen life as drama, not least among them William Shakespeare. Often the key to an analogy is not having it but being willing to work out the details, which is exactly what Goffman did.

To cultivate analogy, you must do two things. First, you have to be willing to make rash connections. This willingness is itself a character trait, and you will need to get a sense of whether you are more or less analogical than others. If you have too little analogical power, you need to cultivate it; if too much, you may need to restrain it. But to use analogy effectively, you must have not only the character but also the means.

You must read broadly in social science and beyond. The more you have to draw on, the better. That is why many great social scientists are part-time dilettantes, always reading outside their fields, always dredging things up from some old high school or college course and putting them to new uses. (It's also one of the reasons why many great social scientists began life as historians, physicists, chemical engineers, literary critics, and even generals or lawyers.)

Of course, as I noted, the origins of analogies are generally well concealed by those who use them. And analogy often provides only the starting point for an argument, which must then be carefully elaborated and critically worked out on its own. But the overall fact is that many an influential paper has its roots in a fairly simple analogy that is carefully worked out. The pervasiveness of analogy is quite evident in famous titles and catch phrases like "economy of favors," "vocabularies of motive," "politics of knowledge," and so on, each one of which flaunts the analogy involved. Analogy is the queen of heuristics.

B. Borrowing a Method

Often there is a subterranean force driving analogy. That force is the desire to borrow (use, steal) a method. Students generally avoid borrowing. They feel that they are learning the methods of this or that field and that their faculty supervisors will expect them to use the local methods. Certainly in methods courses, that's true enough. But for the more general course paper and certainly for research papers and professional work, borrowing is often a smart thing to do.

Typically the borrowing relation can be put simply: "if only I could make an analogy between X and G, I could use all

those methods people have invented for analyzing G." Sometimes these are quite general borrowings. Most of the statistical tools in SCA were borrowed in toto from biology and (later) econometrics (which got most of them from biology in any case). Correlational analysis, multiple regression, experimental and quasi-experimental design, hypothesis testing—nearly all were developed to analyze crops and fields and fertilizers and genetics. Other techniques came from elsewhere. The durational methods used by social scientists to analyze how long things take to happen (how long until a certain kind of law gets passed, how long until a given company folds) were developed to investigate the failure of industrial devices and the survival of sick patients. At the other end of the social sciences, much of anthropology, particularly since Clifford Geertz's famous methodological essay "Thick Description," has borrowed heavily from the textual-analysis methods developed by generations of literary critics.

Often, however, the borrowings are more specific and rest on contested analogies. I am responsible for one such borrowing myself. In the early 1980s, I realized that one could think of occupational careers—one of the most basic things to be explained in all of sociology—as simple sequences of events. I reasoned that if they were simple sequences, one could apply "sequencing" methods to them, and I had heard about the new computer algorithms just then being developed by computer scientists, cryptographers, and biologists to compare files, ransom code systems, and comb protein databases. Why not apply these to social data?

This idea proved quite powerful and spawned a mini-industry. But I had lost something important in the analogy.

The sequences in biology and computer science were not generated in a particular direction, as careers are generated in time. Surely the early stages of a career are more important in some sense than the later ones (because they can dominate where one ends up). The methods I borrowed did nothing with that importance. So the analogy had its weak side as well as its strong one, and the borrowing was consequently not a complete success.¹

Like analogy, borrowing rests above all on a wide command of methods in one's own and other disciplines. It is by freeing oneself from the conventional association of certain objects of analysis with certain kinds of methods that one opens oneself to the rich possibilities of borrowing. But freeing oneself means nothing unless one has the knowledge, close or distant, accidental or carefully sought, of other methods and means of analysis. Analogizers and borrowers must always be reading and learning.

II. ARGUMENT HEURISTICS

Argument heuristics are ways of turning old and familiar arguments into new and creative ones. Search heuristics look elsewhere for ideas. Argument heuristics work with the ideas one already has, trying to make them look unfamiliar and strange.

The first argument heuristic is to problematize the obvious. For example, problematize the obvious notion that college is about learning things. Suppose the purpose of college isn't education at all. What else might it be? Indeed, is there any reason why college might be expected to have any purpose? Think of all the alternative reasons (other than education) for the existence of colleges, and make a decent case for each: saving par-

ents' marriages by getting cranky adolescents out of the house, lowering unemployment by keeping millions of young people out of the labor market, providing a maximally supportive environment in which young people can experiment with erotic and emotional relationships, and so on. You will suddenly find that you know a lot more about the educational purposes of college as a result of this reflection. More important, now you can see the crucial questions about the *educational* purposes of college in a way that you couldn't before you thought about all the noneducational aspects of college. You have problematized the obvious.

A second argument heuristic is to make a reversal. Since everyone assumes universities educate students, assume they prevent education. List all the ways college life suppresses education: scheduling boring classes, providing differing individuals with uniform, uncustomized learning. There are dozens of ways—the nucleus of a good, contentious paper. Reversals are not necessarily reversals of truisms, however, although that is always a useful place to start. You can also just reverse phrases and ideas. I look at my bookshelf and see a copy of Edward Laumann and David Knoke's book *The Organizational State*. As I know well, the book tells how state actors (bureaucracies, boards, legislatures) are embedded in and surrounded by networks of organizations that seek to influence policies in various ways. But suppose I turned the title around and made *state* the adjective and *organization* the noun: *Statist Organization(s)*. What would such a book be about? Perhaps the ways in which organizations take on the properties of states—monopoly of force? Well, not real force, but perhaps economic force? bureaucracy? taxation? How can an organization be said to have

citizens like a state? Now when I've gotten there—to citizens—I see that I have a topic. The waves of recent layoffs and the anguish of those laid off make it clear that for many people their work relationship does entail citizenship of a kind, with not only responsibilities to some organization but also rights in that organization. What kinds of organizations have citizens rather than employees? When in history have there been such organizations? How does the idea of employees' rights grow up? All of a sudden, I have the nucleus of a puzzle. Note, too, that I have drifted from reversal to analogy: the new title forced me to move the idea of citizenship to the world of organizations. But the starting point was a simple grammatical reversal: that's where I found the nucleus of the idea.

A third argument heuristic is to make an assumption—usually a rash one—and see what it gets you. The most familiar of these rash assumptions in social science is to assume that some actor or actors are “rational”; that assumption buys you all the methods of microeconomics and game theory. (It also has a contrary version: Herbert Simon's celebrated assumption that all rationality must be “bounded” in some way.) But you can assume plenty of other things. You can assume, for example, that because most human activities are conducted through language, language holds the key to all social explanation. One must therefore analyze it in any situation. This assumption led to exciting advances in the sociology of science, among other fields. As you can see, making an assumption is often a prelude to borrowing. You usually make an assumption in order to simplify or to translate.

A final important argument heuristic is reconceptualization, saying that what you thought was D is really E or even F. Sup-

pose we reconceptualize college dating. Perhaps dating in college is not really about sexuality at all but about bragging rights. People date not because they are interested in intimacy but in order to prove something to people other than those they are dating. Therefore, dating should be categorized with other forms of bragging. Who knows if such an argument is true, but it suggests an interesting way of rethinking a familiar phenomenon.

Let us now consider these argument heuristics in more detail, using examples.

A. *Problematising the Obvious*

Is there something everyone thinks is obviously true? A useful heuristic is to attack it systematically. Much of the time this gets nowhere; people are often right. But a substantial amount of the time, well-accepted and carefully tested ideas are profoundly wrong. They turn out to have been not carefully tested at all.

Perhaps the most famous recent example of this heuristic is *Time on the Cross* by Robert Fogel and Stanley Engerman. Fogel and Engerman attacked several widely accepted “facts”: (1) southern slavery was dying as an economic system immediately before the Civil War, (2) slave agriculture was economically inefficient (and, consequently, defense of it was economically irrational), and (3) the southern economy as a whole was actually retarded by the existence of slavery. Fogel and Engerman rejected all of those propositions, which had been mainstays of the scholarly literature for many years when they wrote their book. In the process of that rejection, they demonstrated dozens of counterintuitive results: the money

income of slaves in gang labor was higher than what it would have been had they been free sharecroppers (1974:1:239, 2:160); many large plantations had black management (1:212, 2:151); and so on. Fogel and Engerman's two-volume work caused a furor upon publication and for many years thereafter.

Fogel and Engerman were quite clear about problematizing the obvious. In fact, they devote many pages to explaining how a view of the economics of slavery that was so erroneous became standard. They also reveal (2: appendix A) that they were not the first problematizers of these "obvious" facts and point to the extraordinary difficulty such a heuristic sometimes faces.

Another fine example is Claude Fischer's *To Dwell among Friends*. Among the many truisms deflated by this book is the notion that people who live in cities are more isolated—have fewer friends and acquaintances—than people in small towns or rural settings. This belief is a staple of pop psychology and even of much serious scholarly work. Fischer went out and simply asked the question. It turns out that the truism was wrong, although, like many truisms, it contained a grain of truth in that the *kinds* of people urbanites know are somewhat different from those rural people know. They are more likely to be non-kin. But this turns out to be because urbanites are more likely to be young people, people looking for new opportunities and jobs, and so on. That is, people who are more likely to have networks full of non-kin are likely to live in cities *for other reasons*. Again, problematizing the obvious led to an exciting investigation, one that challenged old truisms and raised new questions.

A student doesn't need to take on so monumental a project as attacking truisms about slavery or the city. The world is lit-

tered with obvious facts that are wrong. Newspapers and magazines, with their strong interest in astonishing their readers, are fine sources of unsupported pieces of common sense: consider the beliefs that members of generation X hold certain attitudes or that the 1950s were particularly staid or that Americans are losing their belief in God or that the family is falling apart as a social institution. None of these has much truth in it, but all are standard fare in public discourse.

Social science is full of such hollow truisms, too. Take the common belief that social change is happening faster than ever before. It is not even clear what this means, much less that it is in any way true, yet it is a devout assumption of dozens of articles and papers. Or to consider something more controversial, take the idea that departures from equality in human systems need to be explained. This is a universal assumption of nearly all social scientific writing on inequality. We make this assumption every time we write articles on the causes of inequality across genders, races, classes, and so on. If inequality in these areas doesn't need to be explained, we don't need to write articles about it. Now, we might want to get rid of inequality for moral or political reasons, but why should we think it needs some special explanation? That is, why should we think it is unusual? We normally explain things that are unusual states of affairs, as I noted in Chapter One. Yet inequality, far from being unusual, seems to be nearly universal in human systems. If something is universal, we have to think very differently about its causes than we would if it were some special state of affairs.

Or you can simply take something as a problem that no one else has treated as such. When Bruno Latour and Steve Woolgar did an ethnography of life in a scientific laboratory (1979),

all of a sudden people realized that we had taken life in the lab to be obvious and unproblematic. Turning the weapons of ethnography on it made it suddenly new and strange.

Problematizing the obvious grows out of the habit of always questioning things that are said or taken for granted. It's like a program running in the background on your computer. Every argument, every generalization, every background assumption that you run into, should be scanned with this simple check: Is that really true? Could I get somewhere by regarding this as a problem rather than as something taken for granted? The most extreme version of this scan is simply turning such arguments on their heads. That is the heuristic of reversal, to which I now turn.

B. Making a Reversal

Another of the central argument heuristics is to make a reversal. Sometimes this is simply a grammatical reversal. I was once asked to write a paper for a special journal issue on the subject of boundaries. Boundaries and boundary crossing had become very fashionable, so I was bored with the idea. "Boundaries, boundaries of things, of boundaries of things, of boundaries of things," I sang to myself in the shower one day. Suddenly, the commas moved, and I had the phrase "things of boundaries." What could that mean? I puzzled over it (after I got out of the shower) and tried to give it a real sense. Maybe social things like professions (groups I've spent much of my life studying) are "created" out of boundaries. The edges come first, then the thing, as if we created nations by having a border with place A and another discontinuous border with place B, and yet another with C, and so on, and then we hooked them up to make

something continuous, and all of a sudden there was an inside and an outside, and we called the inside a nation.

The resulting paper—titled "Things of Boundaries," of course—grew out of that simple reversal. I made up the phrase, then tried to think of phenomena that fit it. Often reversal is not such a simple grammatical move but rather a reversal of some standard theory. Among the most famous examples of this is Howard Becker's paper "Becoming a Marijuana User," based on ethnography among marijuana users at a time when marijuana use was much less common than it is today. Becker started from the standard view of "deviant behavior": that certain people have propensities to do deviant things. In such a view, people take up pot smoking because of something characterological, a motivation to be deviant. Becker turned that idea on its head: "[I]nstead of deviant motives leading to deviant behavior, it is the other way around; the deviant behavior in time produces the deviant motivation" (1962:42). Becker's argument was that people had to learn to think of the loss of control and other physiological symptoms of getting high as *pleasant* experiences, rather than confusing or frightening ones. Hence, behavior came first and motivation—sometimes—afterward. This is precisely the reverse of our standard assumption about human behavior. That reversal opened up zones of investigation and possibilities of interpretation to Becker that had been closed to others.

Note that it is not necessarily clear, without talking to the authors who use this trick, whether the data forced it on them or it came to them in a flash, like my "things of boundaries" idea. But the best reversal papers combine data and interpretation in a way that seems magical. Mark Granovetter's

"Strength of Weak Ties" tells its reversal right in the title. Granovetter was interested in what makes interpersonal connections consequential. For years, scholars had drawn sociograms, diagrams with people as points and with lines between the points representing connections between people—connections by friendship, communication, exchange of money, or whatever. It was always loosely assumed that dense sociograms—sociograms in which most of someone's connections are also his or her connections' connections—are the strong type of network. What Granovetter noticed was that if we think about the *overall* degree of connection in a group that has several of these strong "cliques" as well as some links across the gaps between them, the nonclique ties (so-called weak ties) actually do most of the connecting. Because they were bridges between cliques, *overall* connection fell rapidly if they were taken away. By contrast, if any one tie within a clique disappeared, it didn't much matter, because the two individuals involved were probably connected through several other people as well.

Granovetter's empirical data involved finding employment. It turned out that the people Granovetter studied usually found jobs through some secondhand connection—a weak tie—rather than through an immediate friend. The key to employment was your distant friend's uncle's sister, not your best friend. Many people have had the experience of this kind of "accidental" job contact. And we all think of it as unusual. In fact, as Granovetter's theoretical argument shows, it's the common experience. Within our clique, all the people we know have the same job information we have because they are tied to the same people we are. It is through their friends *outside* the clique that new information comes in.

Another example is Paul DiMaggio and Walter Powell's famous paper "The Iron Cage Revisited," which was built on a direct challenge to the Hannan and Freeman paper I mentioned earlier (the one that borrowed ecology to study organizations). The central question of the Hannan and Freeman paper was why are there so many types of organizations? Their answer was that ecological forces produced differences. DiMaggio and Powell simply turned that question on its head. They asked, why do all organizations look alike? Obviously, on the empirical side, the two pairs of authors were looking to some extent at different aspects of organizations. But the fact remains that they used their different questions to make very different things out of what they *did* see in common. DiMaggio and Powell argued that only at the beginning of their lives were organizations subject to the ecological pressures for differentiation that Hannan and Freeman had seen. Afterward, they were pushed toward each other by forces of "isomorphism."

My interest here is not with the content of the DiMaggio and Powell paper but with the now familiar nature of its heuristic gambit. The paper turns the argument of another paper on its head, seeks a way to allow both to be right (by saying that ecological differentiation comes early in the lives of organization and isomorphism comes late), and then lays out a general theoretical argument about isomorphism and illustrates it with examples. Reduced to its barest form, it's just like Becker and Granovetter: "They've told you that X is true, but under certain conditions X is false. Let me tell you about those conditions." This is the simple reversal heuristic, and it produced—in the Becker, Granovetter, and DiMaggio and Powell papers—three of the most widely cited works in modern sociology.

My final example involves making a reversal in the data itself. Harrison White, a physicist turned sociologist, noticed that there are some mobility systems in which holes, rather than people, have the initiative (1970). No one can become president of Harvard until the current president resigns. Then somebody moves to Harvard to become president. This merely makes the hole—the vacancy—move to some other place. Then someone moves to fill *that* place, leaving a hole somewhere else. Eventually this “vacancy chain” gets to the edge of the system, and somebody enters academic administration from outside to fill the last slot. (Or perhaps the slot itself is abolished, ending the chain another way.) In such a system, *holes* have initiative. Nobody can move until a hole opens, and nobody can move exactly where he or she chooses; the possibilities are dictated by the holes that exist when an individual wants to move.

White saw that there was a whole class of occupations like this (football coaches, college presidents, Protestant clergy, company CEOs) and that there was a much larger class of mobility systems in which it was loosely true (university departments, law firms, hospital medical staffs). This insight turned our whole view of mobility on its head; it said that constraints were more important in mobility than either the choices or the character of those trying to move.

This reversal, like so many things, had its roots in analogy. In crystalline solids like semiconductors, there are electron holes, which are more or less negative electrons, absences that behave in most ways like electrons with positive rather than negative charge. So White the physicist already knew about a system in which holes played an important role. Perhaps the

suggestion to make a reversal in the thinking about *people's* mobility simply worked its way out through his subconscious.

Like so many of my examples, the idea of vacancy chains is an example of several kinds of heuristics coming together. One of these is reversal—making holes more important than people. Another is analogy—between mobility systems and crystalline solids. The third is borrowing methods, for White turned his insight into empirical analysis by invoking a general class of probability methods (Markov models) well known (as of 1970) by physicists but unfamiliar to most sociologists.

C. Making an Assumption

Making an assumption—usually a simplifying assumption—can be a powerful heuristic. As I noted above, a simplifying assumption is often a step toward borrowing, usually from a discipline that analyzes simpler or more tractable systems. Thus, by assuming that “value” was a conservable substance like energy, economists were able to borrow the mathematical tools of statistical thermodynamics whole cloth (Mirowski 1989).

There are other reasons for making an assumption, besides adapting someone else's methods. Assumptions make for tractability; they make systems easier to think through. In formal demography, for example, it turns out to be useful to disregard men. As far as formal demographers are concerned, all men do is impregnate women; there are always plenty of men around to do that. It is the women who have the initiative; their age-specific fertility behavior determines the size and shape of a population. So demographers generally start from investiga-

tions of populations of one sex, assuming that women can determine their own fertility, getting pregnant if, and only if, they please.

It is important to distinguish between such tractability assumptions, which are deliberately chosen, and background assumptions, which are merely implicit. All forms of analysis have implicit assumptions. It is always a useful exercise to reflect on and question those assumptions. But I am here concerned with more conscious assumptions, which are designed to open up a situation to analysis.

An excellent example of such an assumption comes in Blau and Duncan's *American Occupational Structure*, already mentioned in Chapter One as a classic example of SCA work. Recall that the book analyzes the dependent variable of the respondent's current job status by studying the way it is affected by independent variables like father's job status, respondent's education, and respondent's first job. When we write an equation to estimate these effects, one thing we assume is that the causal pattern—the arrows describing what affects what in the model—is the same for every case. This translates into the assumption that every case follows the same story.

Obviously this is a radical assumption. Otis Dudley Duncan, the methodological master who did the study, knew this perfectly well. The idealized model order was father's job status and father's education taken together lead to respondent's education, which leads to respondent's first-job status, which leads to respondent's current-job status. Obviously, many cases will reverse some of these steps. Men go back to school after starting work; men's fathers may make deliberate status sacrifices to

guarantee their sons' educations; and so on. But by making the radical assumption that the sequence was everywhere the same, Duncan was able to apply path-analytic regression and make some powerful guesses about the relative importance of all of these forces in shaping men's lives. The actual relationships were of course weaker than they seemed because they were conditional on an assumption known to be erroneous to some degree. But the power of the assumption was great, and the results, even though conditional, were worth the price.

Any strong assumption—like the Duncan assumption—creates the possibility of reversal. Although Duncan was well aware of his radical assumption, many of his followers lost sight of it. Obviously, a useful heuristic gambit is to challenge such a foundational but forgotten assumption. Peter Abell (1987) and I did exactly that with the Duncan assumption, insisting that we investigate the order of events in careers. The result: a variety of new concepts of career as well as new methods for analyzing narrative models for social life.

Another body of inquiry that was built on questioning a standard assumption is the bounded-rationality literature noted above. Starting in the early 1950s, the economist Herbert Simon challenged the idea that all economic actors are rational. In his book *Models of Man*, Simon argued that rationality was bounded—because there are costs to the information one needs to be rational, because the problems involved may be too difficult to solve, and so on. He proposed that people “satisficed” (from *satisfy* plus *suffice*); they make decisions by setting minimal thresholds for success and then search for actions only until they find one that beats the threshold. Later researchers have elaborated on this idea in dozens of ways.

Making and denying major assumptions thus constitutes another basic heuristic in the social sciences. Both moves produce challenging and surprising results.

D. Reconceptualizing

A final argument heuristic is reconceptualization. By this, I mean taking a familiar or taken-for-granted phenomenon and treating it as if it were an example of something quite different. Treat it not as a case of X but of Y or, even better, Z.

I gave in the preceding chapter the famous example of Joseph Gusfield's reconceptualization of drunk-driving accidents as a "setting" or location problem (too many people have to drive in order to drink in social places) rather than an actor problem (too many people are unable to control their cars because of alcohol intake—the concept implicit in the phrase "drunk-driving"). But automobile accidents had already provided a famous example of reconceptualization by a non-social scientist. Prior to the writings of Ralph Nader, it was thought that high speed "caused" accidents. Nader's book *Unsafe at Any Speed* reconceptualized injuries from automobile accidents; they were not a driver (agent) problem but a car (material) problem. Gusfield then later reconceptualized accidents involving alcohol as not a driver (agent) problem but as a location (place) problem. (Thus, both of these are based on moves in the Burke five-keys list of Chapter Three.)

Sometimes reconceptualization is almost forced on one by data. In the 1980s, some criminologists noticed that rates of motorcycle theft fell radically in states with compulsory helmet laws (Mayhew, Clarke, and Eliot 1989). They saw a possible explanation for this if they reconceptualized motorcy-

cle theft (and, later, most minor crime) as driven by opportunity, it was an opportunistic rather than a planned action. In a compulsory-helmet state, if you haven't got a helmet and you suddenly decide to steal a motorcycle, the police will stop you at once for the helmet violation and then figure out that you are a thief. The fact that motorcycle theft falls with compulsory-helmet laws makes immediate sense when you stop thinking of the crime as planned and start thinking of it as opportunistic. But the notion of opportunistic crime challenged long-standing "criminal personality" views of crime. Hence, the reconceptualization was a radical one.

Reconceptualization is always easier when one is working with the lists of topics or commonplaces I mentioned in the preceding chapter. A seasoned social scientist always keeps these kinds of lists in mind. He or she is always rethinking things of interest. Is my case really X or really Y? Can I say something new by recasting the whole framework within which I view my problem?

SEARCH AND ARGUMENT HEURISTICS are the simplest of the general heuristics. Analogy and borrowing, the major search heuristics, open to our use distant areas of investigation and thinking that aren't normally part of our repertoire. But as I noted, one can take advantage of these other areas only if one is aware of them in the first place. That's what makes insatiable reading and broad taste crucial to a good social scientist. They provide the basis on which search heuristics work. Argument heuristics, by contrast, make changes in what we already have at hand. Problematising the obvious, making reversals, making assumptions, and reconceptualizing—these are all ways of

taking what we already have and making it into something new and strange. Unlike analogy and borrowing, they aren't dependent on reading or breadth of knowledge. But they aren't dependent on depth of knowledge either. They are simply a matter of practice, of having the habit of doing them.

Note, too, that making assumptions differs from the other three argument heuristics. The other three are guaranteed to cause public notice. They explicitly change or challenge something. By contrast, making a big assumption is often something an author is conscious of but his or her followers are not. That certainly was the case with Duncan's assumption about uniform career sequences, although it was certainly *not* the case with Becker's assumptions about family-planning "rationality," which stayed controversial for a long time. It is probably the case that a good heuristic assumption is a radical one—one that gets noticed. Beware of assumptions that are mere conveniences.

Chapter Five

GENERAL HEURISTICS

DESCRIPTION AND NARRATION

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- I. DESCRIPTIVE HEURISTICS
 - A. CHANGING CONTEXT
 - B. CHANGING LEVELS
 - C. SETTING CONDITIONS: LUMPING AND SPLITTING
 - II. NARRATIVE HEURISTICS
 - A. STOPPING AND PUTTING IN MOTION
 - B. TAKING AND LEAVING CONTINGENCY
 - C. ANALYZING LATENT FUNCTIONS
 - D. ANALYZING COUNTERFACTUALS
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THE GENERAL HEURISTICS of the last chapter were largely concerned with the methods we use and our general conceptions of the objects of study. In this chapter, I will focus on how we actually imagine our object of study as something in the world, both at a moment and over time. Indeed, one could think of these as the heuristics of space and time. In the discussion of topics lists in Chapter Three, I mentioned the importance (in both Kant's and Aristotle's category lists) of space and time. This chapter recognizes that importance, suggesting some particular heuristic moves that have proved useful in recasting our conceptions of reality's layout in social space and of its flow through social time.

I. DESCRIPTIVE HEURISTICS

Descriptive heuristics have to do with how we imagine social reality itself. Description is not an innocent process. Every description has assumptions built into it, and challenging those assumptions is an easy way to produce new ideas.

First, a description always has a foreground and a background, a focal area and a context. So when we study industrial firms, for example, we take the economic conditions they face as context. We also think of the workers who work in them as part of their context, and we consider the local politics and schools in the towns where they are located as part of their context. When we study family dynamics, however, we take the industrial firms in which the family members work as context, as we do the schools and neighborhood in which family members study and live. There's no particular reason to make something part of the context rather than part of the focal area. The social process itself is completely continuous. But in order to cut down on the complexity of what we study, we make some things foreground and others background. Challenging these decisions is always an effective move.

Second, any description also has a "level," in the sense that there are things we imagine that are bigger than our object of study, things of which it is a part (and that possibly determine it), and things that are smaller than it, things it contains and in turn determines. An important heuristic move is to change this level of analysis, to decide that maybe the determining action takes place at a different level than we thought it did. Consider the subject of success in school. There is a long history of researchers' trying to decide whether the determining action

takes place within individuals (differences in talent), within families (differences in family resources and values), or within school systems (differences in school resources for teaching). In this literature, the explicit question is the determining level of causality.

Finally, a description doesn't necessarily apply everywhere. Perhaps we want to limit the range of a description, to say it applies in some places but not others. Changing this range of application is another important heuristic, one that raises important and novel questions for analysis. Suppose, for example, we argue—as is commonly stated in various public media—that illegitimacy rates among African Americans are frighteningly high. An obvious heuristic for opening up this question to analysis is to ask where else that description might apply: among whites? Hispanics? the highly educated? and so on. (It turns out that illegitimacy rates are rising throughout the population as a whole.)

More generally, condition-setting concerns the question of "how big" the phenomenon of interest is. We might be studying the rise of professions in modern history, for example. But perhaps the rise and structuring of expert occupations are not really phenomena that happen in isolation but are part of a much larger movement regularizing and formalizing all sorts of behaviors: investment (formalized in accounting), law (in codification of laws), and even music (in the creation of the even-tempered scale). In that case, we *really* should be studying a broader phenomenon, called rationalization. (This was Max Weber's argument.)

A. *Changing Context*

Changing context is a powerful heuristic because it brings together things we have carefully set apart or it rearranges the way we connect social things. I am not thinking here so much of the idea that the context determines what happens. (I'll consider that next.) I am more concerned with simply rearranging things on a given level, rearranging what is in our focus of attention and what is outside.

For example, suppose you are studying why students choose to go to particular colleges. You gather material from college view books, promotional materials, Web sites, and so on. You study students' interests and search patterns. But you don't find much. Students seem to apply to a strange variety of schools: a mix of four-year colleges and universities, urban and rural, famous and not so famous. Moreover, students seem to respond to extremely minor differences between schools. How about changing the context? Could it be that applying to and considering colleges are really, at first, about staking out a position as a kind of person at home or in high school or among a friendship group? That is, the context of the decision is not simply the student in the abstract but the student as someone who is trying to tell his parents that he is sensible or her friends that she is dating or his school that he can run with the best, and so on? This context narrows and changes as decision time draws near and family economic and practical realities loom. But the crucial issue is one of context. We change our thinking about college applications by asking whether we have the right context for the problem.

A splendid example of context changing is Arlie Hochschild's *The Managed Heart*. Hochschild's book puts together

two realms of investigation normally considered separate: emotions and work. Traditionally, studies of work treated emotional life as a *context* for understanding what goes on in the workplace. There had been a substantial literature on the "informal organization of the workplace" and whether it helped or hindered the organization. This literature saw friendship, personal rivalry, and so on as a part of the context of the formal structure of the organization, but no one had thought about emotions as part of the foreground, as part of work itself. Hochschild's decision to make emotions the foreground led her to the concept of "emotion work," work that involves changing one's own feelings in order to produce a "proper state of mind in others" (1983:7). It also led her to a remarkable study of the lives and experiences of people who do such emotion work (flight attendants, bill collectors, and others), which remains one of the most interesting pieces of sociology of the last quarter century. Bringing emotion to the foreground was a brilliant idea.

Changing context is a particularly powerful heuristic tool because contexts are usually established by largely conventional rules within disciplines and disciplinary subcommunities. In many ways, undergraduates are better positioned to change the contexts of their problems than faculty members are, because they don't know the conventional contexts assigned in the literature. It is always worthwhile to think about changing the context. Are there parts of your phenomenon that you are treating as background that could become foreground, or vice versa?

B. *Changing Levels*

When we think about some social phenomenon—work, say, or cities—we have a level at which we start thinking about it. Take the example of cities. When we ask what cities look like—how they are shaped, what kinds of people live where in them, and so on—our first inclination is to think at the level of the individual city. So we look at who doesn't like whom and who doesn't want to live next to whom and who moves where, when, and why. We look at transportation structures, land values, industries.

But it might well be that the structure of cities is mainly determined by some larger phenomenon, the national or global economic pattern, for example. This is the theme of Saskia Sassen's *The Global City*, which holds that the structure of certain "primate cities"—New York, London, Tokyo—is determined by their nature as centers for producer services (law, accounting, banking, insurance, and other services businesses need) in the global economy. This centrality generates a demand for certain kinds of employees, who in turn have certain kinds of incomes and tastes, which in turn generate a lot of follow-on markets and kinds of employment. That is, the producer-services industry thrives on concentration, which in turn dictates where producer-services employees live and what kinds of retail operations and services must be locally available. By implication, Sassen's argument (the dominance of the international division of labor) could be extended to other types of cities in a global economy.

In this argument, explaining a "lower-level" phenomenon can be a complete mistake. The real phenomenon of interest may be much larger and the lower-level one driven by the part

it plays in the larger one. The same argument can of course be made in reverse. In a famous article on "The Cumulative Texture of Local Urban Culture," Gerald Surtles argued precisely the opposite of Sassen. Any city, he said, acquires over time certain political habits and rigidities. These will be in many ways unique, and they will overdetermine the fate of all sorts of urban change: political, cultural, even industrial. Chicago, with its relatively cohesive elite closely tied to an aging political machine, is quite different from multi-elite New York and more open and freewheeling Los Angeles. To see a single pattern in city politics is to look at too general a level. Not only should one not see particular cities as determined by global structure, one should also not believe in general patterns of city politics but only in a general process (aging) that produces unique patterns in each city.

What matters is not that one or the other of these arguments is right or wrong but rather that both of these works have become celebrated and fruitful foundations for further studies of urban life. Both led to extensive bodies of research because both invoke the important heuristic of changing levels.

Perhaps the most extraordinary example of such context changing in recent social science—the grandfather of all "globalization" arguments—is Fernand Braudel's monumental study *The Mediterranean*. Braudel argued that the "events" of the Mediterranean in the sixteenth century were just so much flotsam and jetsam on the surface of the sea. The nature of events was dictated by what he called conjuncture, a middle level of historical reality that included fluctuations in prices, changes in trade patterns, and developments in naval practice and power, in types of governments, and in forms of war. But

beneath everything, like a steady foundation, was "structure," the unchanging and determining basis of Mediterranean life. Structure began with the environment—geography, seas, islands, boundaries, climate—but also included foundational human practices: the nature of towns, nomadism and "transhumance" (regular long-distance migration and return), types of ships, and other such things. For Braudel, structure was the most important (about four hundred pages worth), conjuncture came second (about five hundred pages), but then conjunctural things changed, so they took more space). Events—the stuff of most histories—take only the last three hundred pages of Braudel's two volumes. The structural and conjunctural contexts determined them.

Braudel's book abounds in interesting heuristics. His upside-down map of Africa (showing "how the great Sahara desert dominates the sea" [1972:1:169]) is a spectacular example of reversal. But his most extraordinary effect was to give rise to several generations of level-raising arguments, from the world-systems theory of the 1970s to the globalization theory of the 1990s. All of these result from Braudel's radical changing of level, his insistence that grand conjuncture above all drives the little events below.

C. *Setting Conditions: Lumping and Splitting*

Setting conditions is a matter of deciding where a particular description applies. Put another way, it is a matter of deciding whether to split some social phenomena apart or lump them together.

Thus, another way to think of what Sassen did in *The Global City* is to say that the book draws a distinction between the

great producer-services cities—New York, Tokyo, and London—and all other urban places. Of course, the distinction was overdrawn. Many other cities partook of this or that characteristic of the global triumvirate. But precisely what made the book powerful and attractive heuristically was that drawing such a tight line around the phenomenon allowed Sassen to write about an extreme version of it. This in turn allowed her to explore the phenomenon of globalization at a depth that might not have been allowed had she analyzed a larger class of cities. Making a strong distinction allowed her to push an argument to the limit.

One could, by contrast, choose not to make a distinction but to lump things together as instances of a single phenomenon. Among the most celebrated examples of this in social science is Norbert Elias's *The Civilizing Process*. Elias took dozens of subjects that used to be separate—table manners, nose blowing, spitting, bedroom behavior, and so on—and assembled them into an image of private "civilization," which he then even more audaciously connected to the formation of modern states. All of these things together, he argued, constituted a grand "civilizing process." Like Braudel's "structure," Elias's civilizing process was a huge conception. But here the idea was not Braudel's of changing our idea of the determining level of a system but rather an argument that things we had thought utterly separate—the history of nose blowing and the history of the absolute state—were in fact part of one large process.

Again, there is no need for the student to be so audacious or grandiose. But it is often a useful heuristic to lump together things that others have left separate. Merely to propose such a

lumping together is to raise a hundred interesting questions and issues for investigation.

So, too, one can split things apart. This is not the same as saying that the lower level is the determining one. Rather, it asserts that some regularity or description applies over a narrower range than we had thought. This has been the overwhelming strategy of writing about women for the last twenty years. For example, Cynthia Epstein's *Women in Law* makes the case that while there are already many books about lawyers, most of the generalizations in them don't apply to lawyers who are women. Splitting has been the order of the day in many fields: ethnic and racial studies, gender studies, and so on. Note, however, that it is a quite general heuristic move and has nothing inherent to do with activist research. Jerome Carlin's *Lawyers on Their Own* made precisely the same claim about lawyers in small, solo practices—that they were quite different from other lawyers—that Epstein made about women lawyers.

II. NARRATIVE HEURISTICS

Descriptive heuristics propose changes in the way reality is described. Narrative heuristics involve changing the way we use events and stories to think about social life. In this sense, Elias's "civilizing process" is as much a narrative move as it is a descriptive one. It weaves a number of separate narratives into one grand story. (This underscores an important point: it doesn't matter what we *call* the ways in which we generate new ideas just so long as we *have* new ideas.) There are four important narrative heuristics to discuss. The first two involve the degree to which narration enters our thinking about a problem:

whether we view processes dynamically or not and whether we focus on contingency.

An obvious first move is to take something that has been viewed statically and put it into motion or, conversely, to take something that has been seen narratively and make it static. As usual, there is no great issue of faith here. To those who are fascinated by the processual nature of social life (I'm one), it may seem crazy to treat freeze framing as a legitimate heuristic. But sometimes that's the best way to understand social life. Indeed, much of history works this way. Grand-narrative characterizations come apart on close inspection. For example, most histories of America speak of the 1920s as the Jazz Age, but on closer inspection—looked at in isolation—the 1920s seem extremely diverse. Conversely, many static interpretations become quite different when seen dynamically. Consider conditions in high-tech industries today. The senior managers of these companies view the situation more or less statically, within the narrow time frame of quarterly returns and stock market value. But the workers themselves experience their work within the longer, dynamic time frame of their careers. Depending on our research interest, we are going to want time to freeze or flow.

A second narrative heuristic involves contingency. Many social science models disregard contingencies. They are based on the belief that the same kinds of results can come about in several ways and that if we aren't specifically interested in the details of the pathways, we might as well disregard the contingencies that determine them. A rather interesting example of this comes from the literature on people's lives. A long-

standing belief held that negative life events—sickness, bereavement, unemployment, and so on—could lead to various forms of distress. The surprise came when several writers proposed that positive life events—promotion, marriage, and so on—would have the same effect, something that turns out to be more or less true. Thus the contingency—distress came only if the life events were negative—turned out to be irrelevant. Sometimes contingency matters *less* than we think.

On the other hand, sometimes contingency is centrally important. Harrison White's vacancy-chain model, mentioned earlier, is an example of a completely contingent model, at least with respect to individuals' careers. The presence of such overwhelming contingency effects often means that we are working at the wrong level of analysis. White's model is ultimately a structural one, in which the larger system has dominance over local initiative.

A third narrative heuristic involves latent functions. Latent functions are unplanned or largely unnoticed results of social institutions or actions, which, however, turn out to be important. Indeed, it may be the case that these latent functions become more important than acknowledged functions. When I discussed problematizing the obvious, I used an implicit example of latent functions: the alternative purposes of college. Maybe college is not for education but for reducing unemployment by keeping many young people out of the full-time labor force. In that case, education is the ostensible function, unemployment the latent function. This particular example of problematizing the obvious worked by problematizing the ostensible function of an institution and looking for latent functions. There are many other things to problematize about

college, of course: common beliefs about who goes there, what people actually do there all day (faculty do *not* teach all day, for example), and so on. But looking for latent functions is always a useful heuristic.

My final narrative heuristic is the counterfactual: what would have happened if . . . Some disciplines are particularly well set up for counterfactual analysis. Economics has a particular advantage here, because of its ability to "impute" prices to unpriced things by estimating the costs of the other things people forego to have the unpriced ones. But counterfactuals are also widely used in history. For example, the implicit counterfactual in A. J. P. Taylor's *Origins of the Second World War* (discussed in Chapter One) is that if Hitler had not invaded the Soviet Union and gratuitously declared war on the United States, he might have gotten away with most of his gains up to that point. The counterargument to *that*, however, is that he got the earlier gains by making precisely those kinds of audacious moves, but on a smaller scale. Someone who knew when to stop could never have made the earlier bold moves that got him to the point where he made his "mistake." Thus we see that the core of the argument about Taylor's thesis lies in the nature of Hitler's personality and the political system that allowed his personality such comprehensive sway in German policy. By thinking counterfactually, we see where the argument's hinges are.

Let us now examine these narrative gambits with more detailed examples.

A. *Stopping and Putting in Motion*

The first narrative heuristic involves history itself. If your present analytic strategy is static, how about making it dynamic? If

it's dynamic, how about making it static? As with so many heuristics, the question isn't whether the social world is in fact historical or not. You don't have to be Max Weber to know the answer to that question. But sometimes it's useful to *attend* to that history, and sometimes it isn't.

The more familiar move is from static to dynamic. Whenever we move to a new town or school, it seems fixed, a slice of time. Only after staying for a few years do we know which parts are changing and which stable. Theories we adopted at first seem silly once we understand that this or that part of the slice was in fact in rapid motion. Thus you might wonder why some favorite store leaves a mall to set up in a new location. You might develop a story about problems between the store and the mall, problems with competitors, and so on. But then if a new development springs up around the store's new location, you may suddenly realize that in fact your favorite store had been located in the original mall only temporarily, while its new quarters were being built. What seemed to be static turns out to have been in motion, but because you first got to know the store in its temporary location, you didn't see that.

One of the central difficulties of assessing any social situation at a single moment is precisely our inability to see the snapshot merely as part of a movie reel. This point is made with unerring accuracy in one of the most influential works of modern anthropology: *Political Systems of Highland Burma* by Edmund Leach. Leach set out to do "a functionalist study of a single community," the classic ethnographic slice of life. He was only a few months into the work when the Second World War turned Burma into a war zone. Shortly afterward, Leach entered the army and spent the next five years drifting in and

out of northern Burma, visiting nearly every society in the area. Most of his field notes were destroyed in enemy action, and he wrote his great book from memory, his few surviving notes, and what published materials he could find.

Leach's central point was that the stability implied in the classic community studies was a mirage. In his characteristically blunt prose, he wrote:

The generation of British anthropologists of which I am one has proudly proclaimed its belief in the irrelevance of history for the understanding of social organization. What is really meant by these arguments is not that history is irrelevant but that it is too difficult to put on paper. . . . Thus Professor Evans-Pritchard, who is one of the most staunch upholders of equilibrium analysis in British social anthropology, is also an advocate of the use of history in anthropological analysis, but he has not yet explained how the inconsistencies between the two positions can be resolved. (1964:282-83)

Leach was right about "history," of course. Often the best move possible is to put one's data in motion, to see long-run change rather than simple equilibrium. But having made that move, Leach himself made a quite peculiar reverse. He preserved equilibrium by saying that the ritual and symbolic systems of the Kachin act *as if* there were equilibrium societies in Burma. Their cultural system draws on a language of stability but uses that language to do "historical," changing things. The anthropologists' mistake, then, lies in taking the tribes' symbols for the reality. Curiously enough, Leach made the move into history and then took it back again. (This wading into the water and then hurrying back to shore seems to be common among

anthropologists. Marshall Sahlins's influential *Islands of History* makes much the same move.)

From our point of view, what matters is the heuristic. Leach and Sahlins to some extent got caught up with the issue of whether the flow of events was really there, whether it did or didn't matter because there is an equilibrium. In some ways, Evans-Pritchard may have been better off. By trying to keep both sides—whatever the inconsistencies—he was testifying to the heuristic utility of invoking as well as ignoring the passage of time.

So, by contrast, sometimes we need not put our problem into motion but must stop the motion that is already there. Typically we want to do this when our interpretation of some particular moment is being driven more by the narrative in which we have embedded it than by things we actually know.

An excellent example of this is the magnificent historical ethnography *Montaillou: The Promised Land of Error* by Emmanuel Le Roy Ladurie. Throughout the course of modern scholarship, the heretic peasants of southern France had been perceived chiefly in their role as the last representatives of the Catharist (Albigensian) heresy. They are noticed in history mainly for the strangeness of their beliefs (the highest Catharist virtuosos, the perfecti, fasted completely—until death resulted—after their ceremony of “heretication”) and for the brutal crusades that suppressed them. By provoking these crusades, these peasants played a central role in establishing the (northern) king of France's power in Languedoc, the southernmost part of what is now France. That is the usual story of the heretic peasants of southern France.

But the inquisition that rooted out the heresy kept detailed notes. And Le Roy Ladurie realized that one could read the inquisitorial records not so much as evidence about Catharism *per se* but as evidence about the community as a whole, about economy and residence, about family and marriage, about sheep and migration. Suddenly, Catharism becomes not something strange and perplexing but something deeply comprehensible in the context of the culture at the time. History thus becomes ethnography in this book; long-dead historical records give rise to a living, daily culture.

This practice of stopping the clock is an important one, and it is important not only in areas of historical inquiry. The move of stopping the clock is central to all forms of equilibrium analysis. Thus in many branches of economics, a market may be far from stable at any moment, but by analyzing its behavior in equilibrium, one can specify the direction of the forces playing upon it. So, too, in certain forms of game theory. Even extended games—games that take place as repeated plays over time—can sometimes be reduced to a strategic form, in which the answer is given at once, no matter how the repeated plays might get there in practice.

Stopping the clock essentially enables you to attend to more things in the present. It allows you to broaden the context, possibly to change levels. That is, it can be a gateway—like so many of these moves—to other heuristic moves. We often think reality is fundamentally historical. But it is still useful to imagine it, from time to time, as frozen for a moment. These can be big moments to be sure. When Braudel is justifying his concept of structure in *The Mediterranean*, he writes at one

point: "[T]hat these two hundred years, 1450–1650, should form a coherent unit, at least in some respects, clearly demands some explanation" (1972:2:895). How can two centuries be a "moment"? Well, they can't, but by pretending that they are, we can open ourselves to some important insights.

B. *Taking and Leaving Contingency*

Contingency also produces an important heuristic. One can generate many new views of a theory or a regularity by arguing that it is contingent on something. Conversely, one can sometimes produce extraordinary results by disregarding contingency. The latter is, indeed, one of the standard moves made in formal and quantitative work.

As an example of taking contingency seriously, consider Michael Piore and Charles Sabel's argument in *The Second Industrial Divide* that there was nothing foreordained about mass production. According to their argument, modern economic growth might have been sustained by small, flexible production units. There was no absolute need for assembly lines and interchangeable parts. Piore and Sabel's controversial argument has spurred an enormous mass of research on those areas of the world (southwestern Germany and northern Italy, for example) where complex webs of flexible producers have indeed survived. A number of interesting consequences followed from the book. First, the book suggested investigating the web-like subcontractor structures, educational systems, and credit arrangements that *support* these industrial areas of "flexible specialization." That is, the book had direct consequences for industrial policy. Second, it suggested rethinking the old narrative of industrialization itself: Was the role of artisanal

labor as tangential as it had been made to appear? What were the consequences of the "industrial divide" for the labor movement? On the one hand, "massification" created more powerful employers. On the other, it created conditions that made union recruitment easier. Suddenly, the history of modern work looked different.

By contrast, there are also arguments implying that perhaps contingency isn't as important as we think. Making an even stronger argument in his book *Normal Accidents*, Charles Perrow suggested that one could work out a relatively systematic theory for rare and contingent events, such as nuclear-plant accidents, ship collisions, and the like. The book opens with a thrilling, utterly contingent account of the Three Mile Island accident in 1979. Perrow then asks what kinds of factors allow contingency—in the sense of random probability—to dominate systems. He comes up with two. The first factor is the complexity of a system; complex systems have lots of feedback loops and lots of parts serving multiple functions, possibly in ambiguous or unmeasurable ways. The second factor is the coupling of the system; tightly coupled systems are strongly time dependent, with many invariant sequences of action in them and, typically, only one way of successfully operating. Perrow's basic theory is that normal accidents—that is, "systematically produced" contingent events—are most common in complex systems that are also tightly coupled systems. He thereby achieves something of a theory of contingency.

Considering the role of contingency is always important in thinking about social life. The heuristic moves of either invoking more contingency or ruling out contingency can often burst open an intractable problem. Suppose you are writing a

term paper on medical paraprofessions like pharmacy, radiography, and nursing. You read books on each one, and it looks as though they are all getting more and more professional, taking over more and more functions. At the same time, they seem to be involved in lots of little fights with other paraprofessions or with medicine itself. That seems a rather flaccid, dull design for a paper: "professionalism is on the rise, but there is lots of squabbling." Is there a way to regard all of these little fights not as contingent but as systematic? By viewing the competitors all at once in a competitive field, you can see them as contesting a limited set of resources. As in White's vacancy chains, you may tame contingency by seeing it as the outcome of a structured system of competition. You have moved to a new level and can theorize an arena of competition within which these fights can be understood systematically.

C. *Analyzing Latent Functions*

Functional analysis has come into and gone out of fashion several times in the last half century. Functional arguments are elusive. Sometimes they seem to be simply elaborated versions of rational-choice arguments: function equals purpose equals something we choose to do. Sometimes they are purely logical in nature, as in the classic "imperative function" argument that since there are certain things that must occur for a society to work, we must (and can) always identify the social structures that make those things happen. (Sometimes such arguments are simply rationalizations for moral arguments about how society *ought* to be organized.)

The debate over functionalism is deep and complex, but I am interested in functional arguments merely as heuristics. Of-

ten we look at a social institution or structure and develop a theory of it based on what seem to us the obvious purposes or functions that it serves. But it may well be that there are *hidden* forces keeping it in place, either purposively or otherwise. Reflecting on latent functions can take us to these forces, which we can then analyze as we see fit.

An example of latent functional analysis is Richard Edwards's book *Contested Terrain*. Divested of its fairly strong political overlay (Edwards was a radical with a distinct point of view), the book's basic argument is that the usual history of employment relations in the United States got it all wrong. The traditional argument was that an "efficiency" movement dominated American labor relations for the early years of the twentieth century. This was "scientific management," with its stop watches, piece rates, stringent work rules, and so on: the world of work skewered by Charlie Chaplin in the film *Modern Times*. On the traditional interpretation, scientific management was driven by the engineering profession; the attempt to rationalize labor on the shop floor had grown out of rationalized production itself. In this story, scientific management was then replaced in the 1930s and after the war by the "human relations" school of management, with its much broader focus on workers' lives and happiness, welfare capitalism, and similar policies.

What Edwards pointed out was that although the human-relations school looked like a kinder, gentler form of management, in fact it concealed an enormous expansion of bureaucratic rules and regulations that vastly extended firms' control over workers' lives. He argued—quite persuasively—that the real purpose of both schools of management was to

discipline the labor force. The surface, ostensible functions may have been hyperefficiency and "taking care of workers," respectively. But the latent function was the same in both cases: control of the workers.

It doesn't matter, for our purposes, whether that control was planned or accidental. The point is that the search for latent functions often turns up important social forces. I have several times mentioned the latent function of college in safeguarding employment opportunities for the larger labor force. This may seem an odd way to think, but in the years when American labor was dominated by industrial workers, organized labor strongly opposed any attempt to create the kind of combined apprenticeship-schooling system that trains much of the labor force in Germany. Such a system would, in fact, have threatened too many jobs. College's function of controlling unemployment may be more important than you think.

So latent functional analysis is always a useful strategy. You may turn up nothing at all. But you may turn up important things indeed.

D. Analyzing Counterfactuals

Finally, I wish to consider counterfactuals. One of the most useful narrative heuristics is *what if?* We are used to practicing this in our own lives, as in "If I hadn't gone hiking in Europe that summer, we wouldn't have met and gotten married." From the social science point of view, of course, there are hundreds, thousands, maybe even tens of thousands, of people to whom one could have been successfully married. Our lives might have differed in various ways, but most of them wouldn't have been very consequential—in terms, let's say, of our ultimate type of

employment, our financial situation, or the socioeconomic status of our children. There is thus little general interest in investigating counterfactual might have been, although there may be considerable personal interest in them.

Often, however, counterfactuals are vitally important. Would there have been something like a fascist Germany without Adolf Hitler? Would Chicago have become the major city of the Midwest if St. Louis had become a major rail hub? Would American history look fundamentally different if a watchman in the Watergate complex hadn't noticed that the latch was taped on a basement door in 1972?

Posing counterfactuals can be very productive. We often do it merely for the purpose of improving our case against them—that is, to improve the argument for what *did* happen. But sometimes—particularly in the 1960s and 1970s—counterfactual analysis has been an elaborately developed mode of analysis.

One of the most brilliant uses of counterfactual heuristic was Robert Fogel's *Railroads and American Economic Growth*. Fogel problematized the obvious "fact" that railroads were central to American economic growth. What would have happened, he wondered, if there had been no railroad? Obviously, there would have been a *lot* of canals. But what of the actual economic consequences? As Fogel pointed out, to a considerable extent the railroads were *given* their role flat-out; 30 percent of their total capitalization came from federal and state governments as gifts. Indeed, Fogel's introductory chapter is filled with such information, a brilliant use of familiar facts and simple economic theory to demolish what most of us accept as a truism.

Two hundred pages later, Fogel concludes that “no single innovation was vital for economic growth during the nineteenth century. . . . The railroad did not make an overwhelming contribution to the production potential of the economy” (1964:234–35). In between, those two hundred pages contain calculations of wagon-haulage distances in primary markets, studies of the impact of demand for rails on the American iron market, maps of canals that could have been built, and so on. The book is a tour de force, teaching us not only something important about a period but, much more important, teaching us something about the vulnerability of received and accepted ideas.

Few students will have the temerity and energy Fogel has displayed (and continues to display) over his career. But counterfactual thinking is always useful. Return to the example of marriage. If you could have been happy with any one of hundreds of spouses, then the key to understanding marriage and divorce lies not in the detailed dynamics of dating and household life but rather in the larger barriers that shape “pools” of partners likely to come together—barriers like college attendance, for example. Moreover, we can put the issue of partner choice in motion, for as everyone knows, the number of plausible candidates available in the market declines rapidly at certain stages in the life course—for example, at the end of one’s college or graduate school years. This makes us think about another counterfactual: how would people go about finding and sampling partners if there were no such thing as college? (That is, do colleges have the latent function of facilitating the marriage market?) Of course, there is a natural experiment of sorts for that proposition, since much of the population doesn’t go to

college. (And note that that portion of the population tends to marry earlier!)

Thus, starting with a simple counterfactual spins us out, via a number of other heuristic leaps, into a wide variety of interesting hypotheses about marriage and marriage patterns. This is the utility of counterfactual analysis. It drives us to problematize the obvious and suggests dozens of new ways in which to think.

COUNTERFACTUALS BRING US to the end of this survey of general heuristic gambits. This chapter and the preceding one have discussed a wide variety of ways of producing new ideas. I should underscore—as I have before and will again—that these heuristics should not be reified. They are not about the true and the untrue but about finding new ideas. They should be taken as aids to reflection, not as fixed things.

They are also very powerful. Although many of these examples have involved more than one heuristic move, you should use them one at a time and carefully work out the results of each one. Otherwise, they can get you into deep water very quickly.

These general heuristics are not the final or even the most powerful set of heuristic tools for social science. That honor goes to the fractal heuristics founded on the basic debates of Chapter Two. I now turn to them.