

in which the syntactic and semantic programs are about to turn the tables on the pragmatic one, which has dominated social science for about sixty years. The latter remains the best program when we think about social policy. But if we are trying to understand why and how things happen, it has little to recommend it.

Chapter Two

BASIC DEBATES AND METHODOLOGICAL PRACTICES

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THE PRECEDING CHAPTER located standard methods in larger explanatory programs directed at understanding social life. In this chapter, I turn to the more traditional understanding of these methods, according to which they embody certain assumptions about science and social life. The chapter first discusses the principal debates about these assumptions. It then locates the methods of Chapter One with respect to these major debates.

It is here that the argument leaves the standard path. The customary text would at this point go on to a chapter-length analysis of the details of each method. Many excellent texts do so. Instead, I will show that on closer inspection, the usual, simple picture of the methods comes apart in our hands. In the first place, *each* method offers a profound critique of *each* of the others, critiques that are aligned along quite different dimensions. As a result, the various methodological critiques can be arranged in tail-chasing circles. They do not offer the single choice that they are usually said to embody (quantitative versus qualitative, science versus interpretation, or something like that). This circular quality guarantees an openness, a heuristic richness, to mutual methodological critiques. And in the second place, the great debates themselves prove to have a fractal character; they repeat themselves again and again at finer and finer levels within the methods. As a result, they too function less as fixed positions than as methodological resources, as gambits of invention and discovery. Later in the book (Chapter Six), I will show that these debates are in fact our richest resources for new ideas.

I. BASIC DEBATES

Chapter One showed how methods can be loosely identified with different programs of explanation. But it is more common to look at methods in terms of their positions on certain basic social science debates. I shall list nine such debates.

A. *Positivism and Interpretivism*

The first two debates concern methodology proper. One strand of social science argues that social life can be measured. These measures are independent of context, replicable by different people, and comparable for accuracy and validity. By contrast, another strand of social science holds that measurement of social life is not possible or—what is the same thing—that the things that can be measured are unimportant or meaningless. Events that seem to be measurable in fact acquire meaning only when it is assigned to them in interaction. Hence, there can be no decontextualized, universal measure.

This opposition is quite drastic. For the first group, social research takes the form of measurement and counting. For the second, it takes the form of interaction and interpretation. These two positions are called *positivism* and *interpretivism*.

B. *Analysis and Narration*

A second deep debate in social science—one already apparent in the preceding chapter—concerns types of analysis. Many social scientists think that telling a story is a sufficient account of something. For them, narration can explain. By contrast, many others believe that only some more abstract analysis can explain something. Usually the latter position emphasizes causality. To

tell why something happens, in this view, is not to tell a story about it but rather to list the various effects *individual* forces have on it “net of other things”: what is the effect of race on income? of education on occupation? and so on. This second debate pits *narration* against *analysis*.

These two debates—positivism/interpretivism and narration/analysis—are easily stated. But it would be hard to overestimate their importance. They are utterly pervasive in the social sciences. Probably the majority of methodological reflection addresses them in one way or another.

These first two debates concern issues of method proper. But debates about the nature of social reality itself—debates about social ontology—also have important implications for methods, and so we shall consider them as well.

C. Behaviorism and Culturalism

A first ontological debate concerns analytic realms. Many social scientists draw a distinction between social structure and culture. Loosely speaking, *social structure* refers to regular, routine patterns of behavior. Demographic phenomena are perhaps the best example. The processes of birth, death, marriage, and migration seem to have a regularity all their own. One can discuss the demographic life and future of a population without much reference to phenomena outside demography or even to the “meaning” of demographic events themselves. By contrast, one would hardly think about the development of language or of religion in such behavioral terms. Language and religion are *cultural* systems, systems of symbols by which people understand and direct their lives; one cannot ignore their meanings.

The analytic distinction between social structure and cul-

ture has an obvious methodological avatar. The methodological position of *behaviorism* rejects any concern with culture and meaning. One can consider only social structure and behavior, not meaning. There is no standard name for the opposite position, which I shall call *culturalism*. On this position, social life is incomprehensible without investigation of the symbolic systems that index and encode it. The behaviorism/culturalism debate is obviously close to the positivism/interpretivism one. But as with all of these distinctions, it is useful to cross the two and see what comes out. Suppose one were a positivist and a culturalist. That would mean that one was committed to the study of cultural phenomena but with positivist methods. Indeed, such scholars exist: anthropologists who measure and count the various meanings of category systems among primitive peoples, for example.

D. Individualism and Emergentism

A second debate about the nature of the social world—another that we have already encountered—is the debate over individuals and emergents. Certain social scientists believe as a matter of principle that the only real entities in the social world are human individuals. All activity is done by human individuals, and anything that appears to be “emergent” (social) behavior must be the merely accidental result of individual processes. This program of *methodological individualism* goes back historically to the notion that the interaction of individual self-interests produces the social world we observe, an idea that first emerged full-blown in the early eighteenth century with Bernard Mandeville’s *Fable of the Bees*. As a general scientific program, methodological individualism is even older, looking

back to the long scientific heritage of atomism, with its concept of a universe built by combining little units.

Emergentists disagree. For them, the social is real. In more recent social thought, it was Émile Durkheim who argued most strongly for the explicit reality of social level. His famous book *Suicide* used the astonishing stability of suicide rates over time in particular countries and particular populations to demonstrate the existence of social forces irreducible to combinations of individual events. In practice, emergentist assumptions are quite common in social science methods. There may be many social scientists who deny the existence of Marxian-type classes, but there are few who deny the existence of occupations as social groups or the reality of commercial firms as social actors.

E. Realism and Constructionism

A third ontological debate concerns the question of whether the things and qualities we encounter in social reality are enduring phenomena or simply produced (or reproduced) in social interaction as need be. If we ask survey respondents to tell us about their ethnicity, for example, we may simply be encouraging them to invent an answer. In their everyday life, they may not think of themselves as ethnic. Or consider homosexuality. We know from national data that far more men and women have had sexual experiences with members of their own sex than think they are homosexual. If we ask about experience, we get one figure; if we ask about identity, we get one much smaller. That being true, can we in fact determine sexual identity with a questionnaire, or is it revealed only in interaction?

Here again we have two positions, in this case *realism* and *constructionism*. According to the first, the social process is made

up of well-defined people and groups doing well-understood things in specifiable environments. According to the second, the social process is made up of people who construct their identities and selves in the process of interaction with one another; they and their activities have no meaning outside the flow of interaction itself. In this second view, people become ethnic (sometimes) when they are in interactions that call on them to be so: when challenged by others with strong ethnic identities, when ethnic identity might be materially rewarded, and so on. Otherwise, many of them may not be ethnic in any sense. The same argument might apply to homosexuality.

F. Contextualism and Noncontextualism

The distinction between realism and constructionism (or as it is sometimes called, objective and subjective views of social reality) overlaps another one, between thinking *contextually* and thinking *noncontextually*. In the contextual mode of approaching social life, a social statement or action has no meaning unless we know the context in which it appeared. If I say I am a political liberal, my statement has no real content until you know with whom I am comparing myself. I could be a middle-of-the-road Republican speaking to a member of the new Christian right, or I could be a left-wing Democrat comparing myself with all Republicans. Or again, if I say a community is disorganized, I could mean not that it is disorganized in some abstract sense but that it is disorganized relative to other communities around it. Note that the latter statement is not only a statement about the state of a community but also potentially a predictive statement about causal affairs. A community may attract certain kinds of people *because* it is disorganized relative to

its surrounding communities, whereas it might be *losing* precisely those kinds of people if it were surrounded by a different set of communities. From this point of view, there is no absolute scale of disorganization, only disorganization relative to a context. In the noncontextual mode, by contrast, the meaning of disorganization or liberalism is the same no matter what. Obviously, the assumption of such noncontextuality is central to survey methods. When we send out questionnaires, we are assuming that everyone who answers has the same frame of reference in mind.¹

THERE ARE THUS several important debates about the nature of social reality that have methodological implications. The first involves the analytic distinction between social and cultural realms, with its associated methodological schemes of behaviorism and culturalism. A second, long-standing debate is between individualism and emergentism, with its associated schemes of methodological individualism and methodological emergentism. Third is the pairing of realism and constructionism, and fourth is its closely related cousin pairing of contextualism and noncontextualism. Each of these debates has important implications for methodological positions.

G. Choice and Constraint

Not all of the basic social scientific debates concern methods or ontology, however. Some of them concern the kinds of things that are to be explained, what is taken to be problematic in social life. A first issue is whether to focus on *choice* or *constraint*. In many ways, this is another version of the individualism/emergentism debate. For economists in particular, the key to

understanding society lies in understanding how people make choices or rather in figuring out the consequences of their making choices in groups. (Economists feel they already know how people make choices—by maximizing utility subject to a budget constraint. The question lies in figuring out how they make those choices and what the social consequences are when groups of people make such decisions in parallel.)

For many other social scientists, however, the key to understanding society is in figuring out—as the economist James Duesenberry once famously put it—“why people have no choices to make” (1960:233). On this view, social structure constrains and directs individuals. They are not free to make their way unconstrained, except in specifically designed institutional structures like economic markets. Rather, they are shaped by social forces, arrangements and connections that prevent free choice from exercising anything like a determinant role.

H. Conflict and Consensus

Another long-standing debate concerns *conflict* and *consensus*. The consensus position is that while people are inherently disorderly and social order is therefore precarious, social organization and institutions keep people from destroying themselves. (The reader may recognize this position as descending from the English philosopher Thomas Hobbes.) For this position, the standard question is why conflict does not pervade the social system. The answer is usually sought in norms, rules, and values—all the apparatus of social institutions, as this position calls them. Much of consensus research takes the form of teasing out hidden norms and rules that maintain stability in social situations, from the grand social values seen by writers like

Talcott Parsons to the petty regulations of interaction rituals seen by writers like Erving Goffman.

The conflict position, with a genealogy reaching back through Marx to Rousseau, is precisely the reverse. Why, conflict theorists ask, is there so *much* conflict? The answer is that while people are inherently good, their lives are clouded by oppressive institutions that make them act in socially destructive ways. Conflict theorists also seek hidden norms and rules, but for them these are the concealed sources of conflict, not the visible bulwarks against it. Conflict thinkers always begin with social conflict and look backward for its causes, since they believe these do not lie in human nature. Consensus theorists think from conflict forward, to its consequences, believing as they do that conflict does arise in human nature.

In the area of problematics, then, we have two important debates: choice/constraint and conflict/consensus. It should be obvious that the conflict and consensus positions have distinct political sympathies, conflict with left-liberal thinking and consensus with conservative thinking. (Constraint and choice often follow the same divide.) These political positions themselves are often linked to a further debate, one on the nature of knowledge.

I. Transcendent and Situated Knowledge

Much of social science strains toward knowledge that applies at all times and in all places. This is the traditional "scientific" position in favor of *transcendent*, or *universal*, knowledge. An equally strong strain holds that such knowledge is not possible. Knowledge is always *situated*. The latter argument often rests on the constructionist position that social life is built in action

and hence that only the participants can correctly define what is happening in their own place and time. They have privileged access to their own reality. (This is certainly a position that even quite a few survey analysts would accept.)

The political sympathies of these positions are by no means consistent. The universalist, or transcendent, position is usually portrayed as politically conservative, while the left is identified with situated knowledge that accepts the limits of place and time. At the same time, much of left-liberal social science consists of applying universal moral positions (for example, "oppression is bad") to places and times that would by no means have accepted them. The connection is thus not consistent.

THE TRANSCENDENT/SITUATED KNOWLEDGE DEBATE is a useful place to complete this short survey of profound debates in social science. As we have seen, these begin with purely methodological debates: positivism/interpretivism and analysis/narration. They continue through the debates rooted in ontology: behaviorism/culturalism, individualism/emergentism, realism/constructionism, and contextualism/noncontextualism. To these are added the great debates over problematics: choice/constraint and conflict/consensus. Finally, as we have just noted, the characterization of the social sciences as transcendent or situated captures a host of differences about the sources and status of social scientific knowledge. I have listed all of these debates schematically in Table 2.1.

Table 2.1
THE BASIC DEBATES

Methodological Debates

- *Positivism*: reality is measurable.
- *Interpretivism*: there is no meaning without interaction and hence no measurement in the abstract.
- *Analysis*: there is no explanation without causality.
- *Narration*: stories can explain.

Debates about Social Ontology

- *Behaviorism*: social structure (i.e., routine behavior) is the proper foundation of analysis.
- *Culturalism*: culture (i.e., symbolic systems) is the proper foundation for analysis.
- *Individualism*: Human individuals and their acts are the only real objects of social scientific analysis.
- *Emergentism*: social emergents exist, are irreducible to individuals, and can be real objects of social scientific analysis.
- *Realism*: social phenomena have endurance and stability; analysis should focus on the enduring, stable qualities of social phenomena.
- *Constructionism*: social phenomena are continually reproduced in interaction; analysis should focus on that reproduction.
- *Contextualism*: social phenomena are inevitably contextual and cannot be analyzed without taking account of context.
- *Noncontextualism*: social phenomena have meaning (and can be analyzed) independent of their contexts.

Debates about Problematics

- *Choice*: analysis should focus on why and how actors make choices and on the consequences of those choices.
- *Constraint*: analysis should focus on the structural constraints that govern action.

- *Conflict*: we need to explain why there is so much social conflict.
- *Consensus*: we need to explain why there is not more social conflict.

Debate about Types of Knowledge

- *Transcendent knowledge*: our knowledge should apply at all places and times. It should be "universal."
- *Situated knowledge*: our knowledge must be limited in its application. It is always local or particular.

II. METHODS AND DEBATES

The most common way of characterizing the methods introduced in Chapter One is by defining them not as flexible explanatory programs (as I did in that chapter), but in terms of these basic debates. For each method, I have summarized the traditional view of its positions in Table 2.2.

A. Ethnography

Ethnography is usually seen as quite well defined in terms of these debates. Methodologically, it is strongly interpretive, attending extensively to multiple subtleties of meaning. It is often narrative, although ethnographies of the interwar and immediate postwar period were often filled with explicit analysis of societies in terms of social functions and formal social structures, such as kinship systems.

Ontologically, too, ethnography has drifted; its earlier incarnations emphasized behavior and social structure more than culture, but the latter has come to dominate it in the last quarter century. Ethnography is almost never conducted in a methodologically individualist vein nor in a strongly realist one. It is also always highly contextualized, although the type of context has differed. Ethnographies of the classical era

Table 2.2
METHODS AND THEIR POSITIONS

Debate	Ethnography	Narration	SCA	Small-N Analysis	Formalization
Debates about Methodology					
Positivism/Interpretivism	interpretivism	interpretivism	positivism	D	positivism
Analysis/Narration	narration?	narration	analysis	D	analysis
Debates about Ontology					
Behaviorism/Culturalism (Social Structure/Culture)	behaviorism → culturalism	?	behaviorism	D	behaviorism
Individualism/Emergentism	emergentism	?	individualism	D	individualism
Realism/Constructionism	constructionism	?	realism	D	realism
Noncontextualism/ Contextualism	contextualism	contextualism	noncontextualism	contextualism	noncontextualism
Debates about Problematics					
Choice/Constraint	?	D	choice?	?	choice
Consensus/Conflict	?	?	?	?	?
Debates about Knowledge					
Transcendent/Situated	situated	situated	transcendent	D	transcendent

Each cell contains the name of one of the positions, if that is what the method involved generally believes. A question mark signifies that a position is not strongly held. D means "denies" the debate is real. A tilde (~) means "indifferent."

tended to isolate societies from larger systems but always treated the local scene in a comprehensively contextual fashion. By contrast, the main focus of contemporary ethnography is precisely the clash of global and local contexts, with much less study of the details of local context. As for problematics, neither choice/constraint nor conflict/consensus has been a strong debate in ethnographic study, although (as in all social sciences) one could see a drift from consensual to conflict positions from 1960 to 1990. Certainly ethnographies have not commonly been done under anything like strong choice assumptions. Finally, ethnography virtually by definition emphasizes situated knowledge. The generation of universal knowledge from ethnography has been very difficult. In the early years, the emphasis on functions and social structures like kinship led to considerable generalizing, but the flood of "cultural analysis" has washed most universalizing out of ethnographic studies. The only universal statements in ethnography today concern the universally creative and interpretive flux of culture and meaning.

B. Historical Narration

Like ethnography, historical narration is strongly interpretive. Multiple meanings and ambiguities are its everyday fare. And it is of course narrative, both as a rhetoric and as a mode of questioning and understanding. Narration as a rhetoric has come under attack in the last thirty years, both in the focus on social science history (standard causal analysis as applied to historical problems) and in the newer focus on letting multiple voices speak, which has impugned the grand narratives of nineteenth- and early-twentieth-century historiography. But

problems in history are still usually posed narratively—why did A happen and not B?—and social reality is still understood largely as a woven web of stories, not as a systematic social or cultural structure.

Among the ontological debates, historical narration has taken a strong position only on the issue of contextualism, always insisting on the embedding of any historical inquiry in a general knowledge of its time and place. Again, there has been some relaxation, but historical narration remains far more contextualized than nearly any other social scientific method. On the issue of behavior/structure and culture, historical narration has varied, emphasizing now one, now the other. This has been the case with individuals and emergents as well, although the de-emphasis on political history over the last quarter century has generally meant a greater emphasis on emergent groups and their histories. It is the same with realism and constructionism. The inevitably processual character of historical narration inclines it toward a constructionist position, but the mass of detail that must be told in a narrative makes realism an important defense against sheer informational chaos.

In problematics, historical narration has always emphasized a dialogue between choice and constraint. Indeed, one might see this insistent denial of the entire choice/constraint debate as one of the basic marks of historical writing. Both conflict and consensus, on the other hand, have been motivating schemes for historical narration, often being combined in narratives of the exacerbation and reconciliation of conflicts (as in much writing about social movements).

Finally, historical narration, like ethnography, always emphasizes situated knowledge. The last time historians seriously

envisioned universal processes was in the mid-nineteenth century—Spenser's social Darwinism and Marx's dialectical materialism are examples—although globalization may be a candidate in the near future. Indeed, world history is enjoying a new vogue, so we may be headed for a new type of universalism in history.

C. Standard Causal Analysis

Standard causal analysis reverses many of the positions of ethnography and narration. It is positivistic, believing that social measurement is possible and indeed necessary, although sometimes difficult in practice. It is unrelentingly analytic, invoking narration only to imagine relations among variables or causal forces.

Ontologically, it has usually emphasized the individual, since it always works with individual units of analysis that are characterized by properties. (One can imagine an emergentist SCA mathematically based on emergent continuities—an SCA based on mathematical topology, for example—but it hasn't "emerged.") SCA has also emphasized behavior/structure more than culture. For the most part, SCA denies context, because contextualism is a major inconvenience to the statistical methods it uses. The whole idea of variables is to remove particular attributes of particular cases from the contexts provided by other attributes of those cases. Realism is likewise a strong assumption of SCA, since it presumes fixed and given meanings.

On problematics, the standard causal position is more open. The sociological version of it is not very welcoming to constraints, since one of the assumptions of its methods is that independent variables are free to determine the dependent

variable. In a model of occupational achievement, for example, SCA would not recognize the fact that the overall size of most occupations is determined by forces other than the qualities of the people who go into them. (Occupational size is largely determined by the mode of production in the economy.) There has, however, emerged a small school of sociologist "network analysts" who work under SCA assumptions but study constraint directly. On the conflict/consensus issue, by contrast, standard methods are agnostic. Finally, the standard causal position is overwhelmingly universalist. Indeed, this is one of the foundations of its appeal. Its whole aim is to achieve knowledge transcending locality.

D. Small-N Comparison

As I noted, small-N comparison is a hybrid. It aims to keep the interpretive and narrative subtlety of ethnography and narration but to add to these an analytic strength that echoes standard causal analysis. Ontologically also, small-N comparison has retained the openness of ethnography and narration. It emphasizes neither the individual nor the group, neither behavior/structure nor culture, and has operated on both realist and constructionist assumptions, although like ethnography and narration it leans toward the latter. Like them, too, it is highly contextualized. Indeed, the central point of small-N analysis, when compared with standard causal analysis, is precisely to retain the contextual information that standard causal analysis strips from its multitudes of cases.

By doing this, small-N analysis hopes to produce knowledge that is both situated and universal. On the one hand, the retention of detail in the case studies produces situated, contex-

tualized knowledge; on the other hand, the use of different cases allows the analyst to separate the particular aspects of particular cases from more general processes. As for what it takes to be problematic in social life, small-N analysis has no strong identity, emphasizing neither choice nor constraint, neither conflict nor consensus. By contrast, small-N comparison is uniquely identified by its stand on the aims of knowledge. Its basic aim is to square the methodological circle by combining situated and transcendent knowledge.

E. Formalization

As in many other ways, formalization is the most extreme of the methods discussed here. It is almost absolutely positivistic, although curiously so in that it involves no real measurement. The practice of measurement is unnecessary to it, and indeed in economics, the stronghold of formal analysis, concern with measurement of social facts is probably lower than anywhere else in the social sciences. At the same time, the presumption that accurate and valid measurement is *possible* is an absolute for formalization.

It might seem to go without saying that formalization is analytic rather than narrative, but game theory—which is certainly formalistic—contains at least the beginnings of an abstract approach to narration. Narrative formalization was also characteristic of the literary structuralism of the 1950s, 1960s, and 1970s and entered the social sciences through Lévi-Strauss. But it has not endured as a standard method.

Ontologically, formalization has generally been both individualistic and realist. It has been overwhelmingly concerned with behavior/structure rather than culture and has been

acontextual, although formal models of context, like the Schelling segregation models and other contagion models, are not uncommon. But context is, in these models, highly formalized.

As for what it takes to be problematic, formalization has typically attended more to choice than to constraint. It has been agnostic on the conflict/consensus issue but has been absolute in its allegiance to transcendent knowledge.

III. CYCLES OF CRITIQUE

It is thus easy to sketch the basic philosophical stances of the standard methods already introduced. And indeed sketching those stances helps make the methods more clear and comprehensible and emphasizes the ways in which they disagree with one another. Looking at these disagreements, we might conclude that our methods lie on a grand sweep from ethnography and history to small-N analysis, then SCA, then formalization—a grand move from concrete to abstract. Indeed, it is common to run most of the debates discussed in the first part of the chapter into one huge thing, an apparent gradient from interpretive—narrative—emergentist—contextualized—situated knowledge to positive—analytic—individualist—noncontextualized—universal knowledge.

This conflation is a mistake, for a number of reasons. First, there are obvious counterexamples. Ethnography and formalization came together in Lévi-Strauss's attempt to find a formal model for the structure of myths. Well, one might say, that wasn't real formalization. No calculus, no numerical matrices, only a couple of charts and some coding—that's not much formalization. But the deeper point is that Lévi-Strauss did turn

toward formalization. He wished to make a syntactic move, in the terms given in Chapter One. That he didn't happen to use the usual machinery of the best-developed formalizations around—microeconomics, game theory, and such—doesn't help us to understand what he was trying to do. What does help us is to see his new method for myth as part of the explanatory program he was trying to create—a syntactic one (with an emphasis on elegant arguments within it), rather than the semantic one that had dominated the study of myth up to that point (which had emphasized the reference between myths and daily life or between myths and social structure).

It was for this reason that I stressed in Chapter One that the three explanatory programs I was discussing were directions rather than specific contents or methods. Abstraction is a magnitude—a distance away from concrete reality. But one can become abstract in several different ways and one can take a new direction any time, anywhere. That is what the idea of explanatory programs emphasizes. It so happens that we have a number of living methodological traditions, and they happen to have embodied explanatory programs in various ways, just as they have taken various stances on the great debates just listed. But they are living and changing traditions, and it is possible for them to turn in pretty much any explanatory direction any time they like.

The conflating of all the different debates into one big opposition or gradient is wrong for another reason, too. A short reflection on our methods shows that far from lying on a gradient, they are in fact organized more in a circle. We are all familiar with cyclic order from the children's game Rock-Paper-Scissors; our methods set up a methodological

Rock-Paper-Scissors game. Put any two studies using slightly different methods together, and one will seem to have a more effective method. We will then find that this method can be improved further by moving toward yet a third method. And that third method may in turn be improved by moving toward the first!

For example, suppose we want to pursue Lévi-Strauss's topic of myth. We do an ethnography, gathering all the myths of the Bella Coola, a people of western Canada. Reflection on our notes makes us see a close connection between the mythic structure and the clan structure, so we decide the myth system is in fact a loose cultural picture of the clans. The clans use the myth system to talk about, modify, undercut, and otherwise manipulate the strong social structure that is the everyday reality of clan life. Naturally, we would want to discuss this data with other students of myth, comparing our theories with theirs.

Systematic data on the Bella Coola, like data on hundreds of other societies, has been collected in something called the Human Relations Area Files. Using this enormous database, someone might develop a classification and coding scheme for the myth systems of *dozens* of primitive societies, as well as for other aspects of cultural and social structure. With those codes, he or she could then do an excellent SCA, showing that type of myth system could be predicted by knowing, say, the type of lineage system (patrilineal, matrilineal, bilateral), certain aspects of the gender division of labor, and type of contact with the Western world. This knowledge would reduce our Bella Coola study to one example of a phenomenon we now "understand" because of the "more general analysis."

One could imagine a series of such SCA studies of myth and other aspects of primitive societies, a literature developing its own internal debates and questions by changing the variables observed, the types of analysis, and so on. But one can also imagine a historian studying the process through which cultural artifacts and myths were collected in a number of tribes. It might well turn out that the myths and physical artifacts were produced for, and therefore determined by, the demands of anthropologists, museum workers, and other collectors of "primitive material." As is true of many of the Northwest totem poles, these myths may have been produced "for the anthropology trade" as much as for the primitive societies themselves (see Cole 1985). In fact, the social structures of these tribes may have been reconstructed in various ways by contact with modern societies; we now know, for example, that the famous potlatch ceremony of the Bella Coola and the Kwakiutl as it was studied by the early anthropological collectors was in large part a *creation* of that contact (Cole 1985; Cole and Chaikin 1990). On such an argument, the SCA tradition goes up in smoke. It is talking about a causal situation that wasn't in any sense real. So we give up on our SCA tradition just as we gave up on the ethnographic tradition, and we begin a literature of historical inquiry into the nature of contact between primitive societies and the West. (Indeed, such a literature has emerged, although not out of critique of an SCA literature but rather out of critique of ethnography per se.)

We can, however, imagine an ethnographer going to the field deliberately to study culture contact. And we can imagine that ethnographer telling some historians of contact with the West that they have missed the extraordinary creativity with

which primitive societies reshape the cultural and social materials that come to them through contact. So here we are back at ethnography again, right where we started before our little detour through SCA and historical analysis. Moreover, perhaps that ethnographer has just read some game theory (which is, after all, a type of formalization) and thinks that we should perhaps recast the process of culture contact as a repeated-play Chicken game, in which every time contact recurs, both sides attempt to enforce their interpretations of the situation until at the last moment one or the other transforms its interpretation through a complete redefinition. But this redefinition lasts only until the next play, and so on.

This is exactly a Rock-Paper-Scissors situation. SCA trumps ethnography by generalizing. History trumps SCA by historicizing its categories. Ethnography trumps history by undercutting the very idea of historical continuity, invoking formalization into the bargain. Note that each of these trumpings involves a move to a new dimension of difference between methods, and thus each methodological replacement is really an assertion that the dimension emphasized by the *replacing* method is more important than the one *replaced*. SCA trumps ethnography by asserting that generalization is more important than detail. History trumps SCA by asserting that historical verisimilitude is more important than simple generality. Ethnography trumps history by asserting that the power of cultural reinterpretation can undercut our belief in any historical continuities.

It seems likely, then, that each method can trump all the others, although in different ways. There are thus many different methodological "cycles" like the one above. Moreover,

nearly all of these trumpings have been tried and have led each methodological community to forms of revisionism that try to deal with the shortcomings other communities have pointed out. These, too, complicate the methodological landscape.

Even worse, each method offers a metacritique of the others. That is, each method can be used to analyze the *practitioners* of the others; one can do an ethnography of historians or an SCA of formalists, for example.

It is useful to run through all of these critiques and trumpings and revisions, just to put them all down in one place. In part, I do this so that the reader will not take them too seriously. When we see them all together, it is hard to believe that these little round-robins amount to much. But I also provide this list to emphasize again that *there is no inherent gradient or order to methods*. Each method privileges some aspects of analysis over others, and as a consequence each is more or less important as we attend to this or that criterion for our analyses. I have gathered all of these comments in Table 2.3, showing both the metacritiques and the directed critiques. I also show examples of responses (implicit or explicit) to the directed critiques.

A. Ethnography

Ethnography argues that historical narration overlooks the extraordinary variety of human life in its attempt to find the trends and general principles of an age. Responding to this critique, historians throughout the 1960s, 1970s, and 1980s moved toward history "from the bottom up," studying the "people without history," often employing an oral history that looks no different from ethnography. Although all of these studies were in part inspired by a political impulse to study the

Table 2.3
METACRITIQUES, CRITIQUES, AND RESPONSES

Method	Metacritique	Critique	Response
Ethnography	Others lack ethnography of selves.		
Historical Narration		misses extraordinary variety of the social world	history from the ground up; oral history
Small-N Comparison		compares sites despite major differences; doesn't necessarily have same researchers at all sites	
SCA		uses worthless or meaningless data; assigns meanings arbitrarily	focus groups
Formalization			
Historical Narration	Others lack sense of their own history.		
Ethnography		is static; misses change of meaning; lacks history of its own terms, of its types of analysis, of itself	rise of work combining history and ethnography—for example, Sidney Mintz, Eric Wolf
Small-N Comparison		lacks primary data; misses context	primary-data-based comparative historical sociology
SCA		ignores contingency; lacks account of action; cannot represent "history" of its variables	social science history; conditional models; periodized time series analysis
Formalization		assumes that underlying model does not change	evolutionary algorithms

SCA	Others' methodological allegiances can be explained by various causal forces. (implicit only)		
Ethnography		lacks generalization; lacks causal analysis; is unfalsifiable; uses unreliable measurement; is not scientific	group ethnographies combining multiple sites
Historical Narration		lacks generalization; lacks causal analysis; is unfalsifiable	comparative historical sociology
Small-N Comparison		uses case numbers too small for generalizing; retains meaningless detail, keeps worst of both worlds	qualitative comparative analysis (QCA)—Charles Ragin
Formalization		lacks content; accepts bad data	
Formalization			
Ethnography		lacks theory	Claude Lévi-Strauss on mythological analysis; Harrison White on kinship
Historical Narration		lacks theory	rational choice history—Hilton Root, Margaret Weir
Small-N Comparison		lacks theory	
SCA		lacks theory	testing of game theoretic hypotheses

The table is not saturated in the sense that every possible cell is filled in. I have left blanks where I am not aware of a major critical literature or response. In addition, the "responses" here do not necessarily come from the criticized community. Comparative historical sociology came from sociology, not history, although it responds to the SCA critique of "uncausal" historical work.

forgotten and downtrodden, they were also rooted both directly and indirectly in an ethnographic impulse to get closer to the data underneath the “grand syntheses” that ignored so much.

Ethnography argues that in small-N analysis there are fundamental problems of comparability between cases, even if the analysis involved is itself ethnographic. Small-N analysis contextualizes, but not enough. Against SCA, the ethnographic case is much clearer. Ethnography thinks that social facts derive their meaning from other facts around them. To treat social facts as “variables” on universal scales (where a given fact has a given meaning irrespective of the other facts in its context) destroys that meaning. Ethnography therefore regards coding and quantification with profound suspicion and believes that the data on which SCA bases itself are quite literally meaningless. While there has not been a direct infusion of ethnography into SCA because of this critique, there *has* been an enormous increase in the use of focus groups and other quasi-ethnographic devices to make sure that questionnaires make sense with respect to the people being surveyed, rather than simply coming from the minds of surveyors, as they often did in the early days.

Oddly enough, ethnography and formalization have had a long-standing flirtation. They share a certain love of complexity. For ethnography, this is a complexity of facts and events. For formalization, it is a complexity of formal details and inferences, very much evident in the dozens of different games (Chicken, Tit for Tat, Prisoners’ Dilemma, and so on) invented by the game theorists. Lévi-Straussian anthropology was highly formal, as was cognitive anthropology in the 1960s and as is much of anthropological linguistics today. For their part, the

formalists had a fine time trying to mathematize the kinship systems of the world. This odd flirtation between what are apparently the ends of a concrete-abstract scale underscores the cyclic nature of methods. The ethnographic discipline of anthropology has been far more hospitable to formalization than to any version of SCA.

The ethnographic metacritique of other methods is carried out in the now widespread ethnographic analysis of groups of natural and social scientists. The content of the critique is simple enough. Without a serious ethnographic analysis of their practices and beliefs, social scientists cannot understand what they themselves are doing. Their surface discourse—of methods and theories and findings—in fact covers a much more complex set of cultural structures. What is going on may then not be “social science” but rather making sense of local anomalies in the data, controlling the way in which surveys simplify reality for large or small political reasons, and so on. In this way, ethnography can claim that methodological discussion is in practice a cover for other agendas: personal, institutional, societal, political.

B. Historical Narration

The historians have a different metacritique. For them, the great problem of social science is that it does not historicize itself. That is, methodological communities lack a sense of their history and hence a sense of the transitory nature of the very terminologies with which they debate central methodological and theoretical issues. Until social scientists understand themselves as working in cultural communities that interact in highly structured and even ritualized ways, they will be forced

by their own rhetorics and symbols to walk on a treadmill, imagining that they are advancing, but in fact going nowhere. Indeed, it may well not be *possible* to go in any direction. We may simply be wandering around aimlessly. Historical analysis emphasizes the role of contingency and accident in all methodological development.

If we turn to the specific critiques that historical analysis levels at other methods, we find an interesting variety. Historical analysis criticizes ethnography for being static. By going to a single place at a single time, an ethnographer loses the ability to distinguish things that are changing from things that are not. Everything that endures as long as the ethnographic encounter looks permanent. Indeed, from 1970 onward, writers have criticized the classic ethnographies of the interwar period for treating the fleeting moments of the last stages of colonialism as if they were stable moments of “traditional societies.”

Against small-N analysis—usually, comparative historical work—history’s claim has been quite simple. Small-N analysts typically do not use large amounts of primary documents and typically know far less than do specialists on one case. Historians think small-N analysts simply don’t know their cases. By contrast, the historical case against SCA is much more vague. In fact, there has been a substantial move to marry SCA methods to historical questions, in the large and amorphous movement called social science history. (Not all of the participants in this have been historians; there have been many historical demographers, economists, and sociologists involved as well.) The deeper “historical” case against SCA is that reality happens not in isolated events and properties, as the SCA practice of variables analysis assumes, but rather in cascades of action and

reaction, choice and constraint. SCA really has no account of action and reaction whatsoever; its only standard method for analyzing action is to estimate the effects of different variables on the waiting time till some dependent event occurs—that’s hardly history. Finally, historical narration argues that SCA’s variables have histories, which are always ignored. One cannot really do over-time models of changes in the relationship between occupation and education because the very categories—the names and contents of occupations and the names and contents of types of education—change over any time period worth analyzing.

Against formalization, the chief argument of historical analysis is that it always presupposes a formal model that doesn’t change, whether that model is game theoretic or micro-economic or structuralist. But it is the cardinal presupposition of historical analysis that anything, even the very rules of the game, can change. To the extent that there are universal rules, they are contentless, definitional truisms—“people do what they want to do” and that sort of thing. Interestingly, there have been occasional outbreaks of formalist history, generally coming from outside history as a discipline. Nicolas Rashevsky once wrote an amusing book called *Looking at History through Mathematics*, and more recently there have been various rational-choice models applied to historical events. But no one has ever seriously attempted the central task of making formal models themselves fully historical (by making the rules of the games completely internal, a part of the game). This question belongs to the computer science field of recursive theory and will no doubt be addressed soon enough.

C. Standard Causal Analysis

SCA's critiques of other forms of method are familiar. SCA condemns ethnography for not allowing general conclusions, for being unfalsifiable, for using unreliable and unreplicable subjective "measurement"—in short, for not being scientific. SCA condemns historical analysis for many of the same reasons, although particularly emphasizing the fact that historical analysis is not "causal analysis." By this criticism, SCA means two things, one more limited than the other. The limited critique is that historical analysis doesn't produce coefficients telling us how much of each independent factor is involved in the dependent result. Historical narration is more likely to combine the factors in a story, to envision multiple contingencies and interdependencies. This limited critique is largely definitional; SCA is saying that history isn't SCA, which does produce such coefficients and, more important, claims that story telling is not a legitimate form of explanation.

The broader critique is more profound. SCA legitimately argues that historical analysis rarely if ever investigates *common* forms of "stories" across cases; it never attempts even "historical," much less causal, generalization. This critique gave rise to comparative historical sociology, a form of small-N analysis designed to deliberately evaluate different causal patterns in small numbers of cases. It also led to various forms of narrative positivism, which attempt to directly measure and analyze large numbers of historical "story" patterns like careers or revolutions. SCA then criticized these revisions themselves. It criticized small-N analysis (in the guise of comparative historical sociology) for *still* having too few cases for effective generaliza-

tion, while it criticized narrative positivism for not having enough causal analysis.²

Against formalization, SCA argues that it is too vague and contentless. There is no necessary connection between a formal model and any particular set of data, as we have seen before. This is both a theoretical and a practical objection. On the one hand is the theoretical problem that any given social situation can be represented by dozens of formal models with varying assumptions and implications. On the other is the practical problem that formalists have often been extremely cavalier about data.

As a metacritique, SCA is less direct than are ethnography and history, whose metacritiques are almost *ad hominem*. They can point to particular misunderstandings, particular anachronisms. They can be and are used as weapons in intellectual debate. The SCA metacritique is more implicit. It implies that one could model the output of the various disciplines and show that various causal factors—the talent of practitioners, the levels of funding, the structure of interlocking elites—might explain that output. It is interesting that hardly anyone today bothers to do such models either as critique or even as simple sociology of science, although there is certainly a persistent folk belief among SCA practitioners that the form and content of ethnography, narration, and small-N analysis are determined by the (supposed) lack of mathematical skill among those who use them.

D. Formalization

The formalists, too, spend little of their time in metacritique. They don't bother to write models for others' scholarship, al-

though I suppose they could easily enough. Rather, they have a single common critique that they apply to nearly all other forms of method. That critique is simply that all other methods use causal and explanatory arguments whose implications have not been well worked out. So the first few pages of an SCA analysis of why people stay at jobs might contain two or three "hypotheses," which would basically be stories about plausible behaviors of certain kinds of workers under certain kinds of conditions. An economist could easily write twenty pages of calculus to justify (or reject) just one of those stories. The same applies—only more so—for ethnography, historical analysis, and small-N arguments. For the formalist, these methods are simply not thought out. Not only are the arguments in each study undeveloped in formal terms, but there is also no broader, purely theoretical argument that holds them in a firm common framework. As far as formalists are concerned, this is just as true of SCA, with its somewhat ad hoc, just-so "theorizing," as it is of ethnography and historical analysis, with their attempts to explain particular cases. All the same, there are formalist connections to nearly all of the other methods, sometimes originating on the formalist side, sometimes on the other.

E. Small-N Analysis

Small-N analysis is in many ways a compromise method designed to deal with all of these criticisms. Small-N ethnography tries to avoid the no-generalization critique SCA makes of ethnography, just as small-N historical analysis tries to avoid the no-causal-analysis critique SCA makes of historical analysis. At the same time, small-N comparison tries to avoid the meaningless-variables and no-events critiques that go the other

way. Like most compromise strategies, small-N analysis often ends up falling between two stools. As is also implicit in the idea of compromise, small-N analysis does not have any general metacritique of the other methods.

IT IS THUS CLEAR that each method considered here has solid and profound objections to all the others. The result, as I noted at the outset, is that methods have a cyclical relationship. Each one is capable of correcting the others. Indeed, as we have seen in this discussion, many of these corrections have taken form in substantial bodies of literature. But when all of these various corrections are laid out together, we find ourselves in a labyrinth where any method can be found both superior *and* inferior to any other.

IV. FROM CRITIQUE TO HEURISTIC

It is useful to summarize the argument of the chapter so far. In the first section, I discussed some basic debates in the social sciences. In the second, I pointed out how the methods of the preceding chapter are defined in terms of these basic debates. At this point, it was noted, a standard methodology text would launch into the details of each basic method, leaving the profound differences of assumptions as simply something to take notice of and then move past. There would be a single chapter on each method, elaborating the positions inherent in these debates and showing how the methods go about proposing questions, designing studies, acquiring data, and drawing inferences.

Instead, I showed that the usual way of relating these methods to one another is wrong. The apparent gradient from one

methodological type to another is indeed merely apparent; methodological critiques actually go around in circles. With all of these critiques laid out in one place, one can see that as a system they do not form a logical structure. (As a result, most writing that attempts self-conscious methodological critique is nonsense or pure polemic.)

The more important reason for setting out these arguments in one place is to begin to show how, in the hands of some scholars, problems and critiques become creative. It is by making these critiques that we have in many cases figured out new things to say in our research. Not that the new things are necessarily better in any global sense. They may be better locally, but overall the cyclical character of methodological critique guarantees, as I have noted, that there is no real “better” in a global sense. What *is* better in the global sense is to know more or to know reality in more detailed ways or in more different and mutually challenging ways—or something like that. It is as if we were interested not in separating the true from the false but simply in trying to say all of the things we could possibly say about social life, given an ideal that we somehow be rigorous in our ways of saying them. (Put another way, we have to define truth in a much more flexible way if we are going to understand what we do as social scientists.)

So mutual methodological critique is important not because it makes us more right but because it gives us more—and particularly more complicated—things to say. That is, mutual methodological critique is useful heuristically. It generates new ideas. Seeing SCA from the viewpoint of ethnography leads SCA to produce more interesting and more complex results. Seeing historical narration from the viewpoint of formalization

produces surprising insights. Sometimes such critiques lead to whole new methodological communities, hybridizing older methods. Social science history emerged out of the SCA critique of historical narration, while history “from the bottom up” emerged out of an ethnographic critique of historical narration. Both were exciting and intellectually decisive movements.

We have, then, already seen our first heuristic move. It is the move you make when you ask yourself how someone from another methodological approach sees what you are doing. Mutual methodological critique is thus the first of the general heuristics I discuss. The next three chapters discuss other kinds of heuristics. In Chapter Three, I discuss the idea of heuristic generally, examining what we mean by a trick or rule for coming up with new ideas. I also discuss the two simplest means for producing such ideas. The first is the additive heuristic of normal science, making a new idea by making a minor change in an old idea and repeating the analysis. The second is the heuristic of topics, using lists of standard ideas to avoid getting stuck in one way of thinking.

In Chapters Four and Five, I turn from such global heuristic strategies to more particular rules for producing new ideas. Some of these are ways of searching elsewhere for ideas; others are content-free rules for changing arguments. Some are ways of changing the description of the events we are trying to theorize about; some are ways of changing the way we tell stories about those events. All are potential tools for transforming existing arguments into new ones.

Chapter Six returns to the heuristics implicit in the mutual methodological critiques just discussed. The heuristic fertility

of mutual methodological critique can be extended by a further analysis of the basic debates with which I began this chapter. Much of the power of mutual critique comes from a peculiar quality of those debates. It turns out that they are fractals. That is, they are not simple linear scales from positivism to interpretation, say, or from narration to analysis. Rather, they are continuously subdividing structures. The positivists fight with the interpretivists, but then each group divides within itself into positivists and interpretivists, and so on and on.

To take an example, positivist sociologists like to do surveys, and interpretivist sociologists like to do ethnography. But among those who do surveys, some are very worried about exactly how respondents understand a question, while others trust random error to take care of interpretive problems. Once again, we have interpretivists and positivists—only *within* what we thought was a group of positivists. This happens on the interpretive side as well. There we will have, on the one hand, the indexer-coder types, who carefully index their field notes and develop “hypotheses” based on the patterns of codes they see, and, on the other hand, the deep interpretivists, who want to consider the way particular words were used in particular sentences. Oddly enough the random-error surveyors (positivist positivists) in some ways have more in common with the indexer-coder ethnographers (positivist interpretivists) than with the respondent-bias surveyors (interpretivist positivists)—not in all ways, but in some.

I could multiply examples, but the point is made. These basic debates are *not* grand, fixed positions taken once and for all in one’s choice of method. They arise as choices day in, day out. They pervade the process of research. And hardly anyone makes

them the same way in all contexts and at all moments. Chapter Six shows how this complex and fractal character of the basic debates makes them into a crucial heuristic resource for social science. Just as the trumping critiques of the last section provide bases for whole new literatures, so too do the fractal debates at the heart of social science provide endless ways to come up with new ideas and even new ways to imagine our questions. That is exactly what we mean by heuristic.

Chapter Three

INTRODUCTION TO HEURISTICS

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- I. THE IDEA OF HEURISTIC
 - II. THE ROUTINE HEURISTICS OF NORMAL SCIENCE
 - III. TOPICS AND COMMONPLACES
 - A. ARISTOTLE'S FOUR CAUSES
 - B. KANT'S LIST OF CATEGORIES
 - C. BURKE'S FIVE KEYS OF DRAMATISM
 - D. MORRIS'S THREE MODES OF LANGUAGE
-

I. THE IDEA OF HEURISTIC

The classic story about heuristics tells how Archimedes jumped out of the bathtub and ran naked through the streets of Syracuse, shouting "I've found it." As he had watched water slosh out of the tub, he had suddenly realized that something that weighed the same as his body but was more dense would make less water slosh out of the tub. Hence, if the supposedly golden crown of his friend King Heiron was actually made of a cheaper silver alloy, it would displace more water than an all-gold crown, because silver is less dense than gold. So he could tell whether the crown was made entirely of gold without melting it.

What Archimedes actually shouted, of course, was not "I've found it," but "Eureka," the first-person singular perfect of the Greek verb *heuriskein*, meaning "to find."¹ From this word

comes the English word *heuristic*, which denotes the study of how to find things out—the discipline, as it were, of discovery. The Archimedes story is a good place to start thinking about heuristic. Archimedes had a problem. Bobbing in the bathtub gave him the solution. And so heuristic is the science of finding new ways to solve problems, the science, as it were, of bathtubs. Thus, in computer science, *heuristic programming* refers to programming that takes an experimental approach to problem solution rather than an analytically exact one.²

Most modern writing about heuristic comes from mathematics. Mathematicians often have particular problems to solve: how to solve the normal distribution integral (hint: you can't do it analytically), how to create a perfect pentagon, how to categorize all the possible types of disconnection in six-space, and so on. Mathematicians often know or suspect the answer they seek but need to be sure of how one gets there. Even when they don't know the answer, they usually have a clear idea of what an answer looks like. In such a context, heuristic means thinking creatively about how to get from problem to solution. Often one builds out from the problem on the one hand and from the solution on the other until the two halves meet in the middle like a bridge built from two banks.

The greatest modern writer on heuristic, the probabilist George Pólya, wrote his brilliant *How to Solve It* precisely about such mathematical problems. Pólya presented a large number of tricks and schemes for making difficult problems solvable. He thought there were four crucial steps to problem solution: understanding the problem, developing a plan to solve it, carrying that plan out, and looking back from the solution. Each of these steps involved a number of questions and tasks:

1. Understand the Problem:

What is the unknown? What are the data? What are the "conditions"?

Draw a figure. Introduce suitable notation.

Separate the parts of the conditions.

2. Devise a Plan:

Have you seen this problem before or something like it?

Do you know another problem with the same unknown?

If you have a related problem and its solution, how can you use that here?

Can you restate the problem? Solve a part of it? Solve an analogous problem? Solve a bigger problem of which it is a part?

3. Carry Out the Plan:

Check each step. Are they really correct? Can you prove it?

4. Look Back:

Can you check the result? Can you derive the result differently?

Can you use the result to solve another problem?

(1957:xvi–xvii)

Most of Pólya's book is a "dictionary of heuristic"—really a set of meditations on various topics relevant to discovery. Some of these topics are strategies for problem solving: auxiliary problems, decomposing and recombining, mathematical induction, variation of the problem, working backward. Others are extended essays on the questions listed under items 1–4 above.

But in the social sciences we often have a different situation. We often don't see ahead of time exactly what the problem is,

much less do we have an idea of the solution. We often come at an issue with only a gut feeling that there is something interesting about it. We often don't know even what an answer ought to look like. Indeed, figuring out what the puzzle really is and what the answer ought to look like often happen in parallel with finding the answer itself. This is why many if not most writers of social science dissertations and books write the introductions to their dissertations and books *last*, after all the substantive chapters have been written. Their original research proposals usually turn out to have just been hunting licenses, most often licenses to hunt animals very different from the ones that have ended up in the undergraduate thesis or the doctoral dissertation.

This difference between mathematics and the social sciences means that I do *not* necessarily assume here that the reader is someone at the beginning of a research project, looking for new ideas. Most teaching on methods assumes that the student will start a research project with a general question, then narrow that to a focused question, which will dictate the kind of data needed, which will in turn support an analysis designed to answer the focused question. Nothing could be further from reality. Most research projects—from first-year undergraduate papers to midcareer multiyear, multi-investigator projects—start out as general interests in an area tied up with hazy notions about some possible data, a preference for this or that kind of method, and as often as not a preference for certain kinds of results. Most research projects advance on all of these fronts at once, the data getting better as the question gets more focused, the methods more firmly decided, and the results more precise. At some point—the dissertation-proposal hearing for

graduate students, the grant-proposal stage for faculty, the office hour with the supervising faculty member for any serious undergraduate paper—an attempt is made to develop a soup-to-nuts account of the research in the traditional order. Now emerges the familiar format of puzzle leading to literature review leading to formal question, data, and methods. Even then, the soup-to-nuts menu is likely to be for a different meal than the one that ends up in the final paper.

As any senior researcher can tell you, the typical grant-funded project has some of its final results in hand by this midpoint in the research process. Put another way, you can't tell a granting agency what you are going to do until you've very nearly finished doing it. And indeed, many faculty use grant funds from one project to do their *next* project, which they apply for—when *it* is nearly done—to get funds to do the project after that. (That is, expecting you to know exactly what you are going to do ahead of time is completely unrealistic in the social sciences.) So the first version of a traditional proposal is pretty tentative. The real reason for forcing research into that format is that the format makes it easier to see what remains to be done and what hasn't worked so far.

All of which means that I am *not* assuming that the reader is reading this book in hopes of getting an idea, which will then lead to focused questions, and data, and so on. The gambits I discuss can be useful at any time in a project, because data, methods, and theory will all be recast again and again throughout the course of any research project.

This talk about senior researchers may seem to suggest that my argument is losing its original focus on the beginning student. So a word is useful here about the stages of an intel-

lectual life. It turns out that heuristics do different things for us at different ages.

I noted in my remarks To the Reader that a common problem among students is a feeling that one has nothing to say. And the principal theme of this book is resolving that problem by finding bases for new ideas. The problem of having nothing (new) to say is for the most part a problem that arises because you, the student, are doing social science for the first time. So you find the huge variety of things that *could* be said almost as overwhelming as the huge diversity of things that *have been* said.

In this common situation, heuristic helps you deal with both problems. On the one hand, it gives you tools to question what has been said, transforming it into new ideas and new views. On the other hand, steady practice of heuristic will teach you rules for separating good things that could be said from bad ones, as we shall see in Chapter Seven.

Having a hard time deciding what to say is to some extent a problem of people who don't have a ready-made stance toward social life. We all know many people who *do* have such a ready-made stance, for that is the position of people who have a strong political interest of some kind. Whatever the issue raised, people with such political interests have a stance on it, a way of thinking about it. Often they even have stock questions and puzzles about it (as in the feminist's questions "what about *women* and social networks?" "what about a *gendered* concept of narrative?" and so on). These flow from their relatively one-sided view of social life, which is somewhat easier and in some ways less intellectually self-defeating than a position that tries to see a problem from all sides. The proverbial view from

nowhere is willy-nilly characteristic of people just starting out in social science or of people who don't yet have particular commitments, and it is much harder to work with than the more comfortable view from a point.

This comfortable one-sidedness, which only strongly political people have from the start, is a quality we all achieve after our early outings as social scientists. It is a kind of second stage of our development. You don't necessarily become dominated by this or that political concern, but you decide you're a Marxist or a Weberian or Foucauldian, and voilà—for any given problem you have a viewpoint and even some standard questions. At that point, you need heuristics not so much to get started as to free yourself from the restrictions of your point of view. Otherwise, you are always writing papers in the form of "a neo-institutionalist view of church organization" or "Bourdieu's habitus as an educational concept" or "Marxian theories of education" and wondering why no one outside your camp gets excited.

The reason you want to free yourself from those restrictions is of course that there are always lots of other people around who aren't Marxists or Weberians or whatever you are. Those people always seem to have their own well-worked-out views of issues and problems and data. If you can't learn to think in their modalities, you can't talk to them. So now you begin to use heuristics not just to loosen up your own views. You try to master the basic viewpoints and even the heuristic repertoires of *other* stances toward the social world. This is the third stage of a social scientist's intellectual development. We look for this in good students when we say, "OK, now what's the

game-theory approach to that question?" and then follow with "Would a Weberian be comfortable with that?"

You have come of age as a social scientist when you know all of the diverse second-level repertoires of concepts and questions so well that you use heuristic strategies to set various points of view against one another. This is the fourth and final level of social science work. You start using the different standard stances to question one another; each becomes the others' heuristic. This is to some extent what I meant by the discussions of mutual criticism between methods in the preceding chapter. Each stance begins to challenge all the others.

More important, you can do something at this advanced stage that many never manage. You can combine stances into far more complex forms of questioning than any one of them can produce alone. An example from the arts will show what I mean. In the early 1780s, Mozart found some Bach manuscripts and was amazed by them. He decided to learn to write Baroque-style music, and his C Minor Mass shows that he could indeed write such music as easily as he could write the classical style for which he is more famous. So in the opera *Don Giovanni*, he defined different characters by writing music for them in different styles. The arias for Donna Elvira—the most traditional of the five women Don Giovanni hustles in the opera—are written in a rigid Baroque style that would have struck any listener at the time as completely old-fashioned, just right for the old-fashioned woman Donna Elvira is meant to be. Don Giovanni's music is much more current, befitting his energetic but sleazy self, while the music of his servant-fix-it man, the scamp Leporello, is written in the rhythms of the

peasant dances of the time. For Mozart, different styles are not a problem but a resource (see Allenbrook 1983). Only a master of many styles can make them talk to each other in this way. At the highest level of social science, this is what serious heuristic can accomplish.

In short, heuristic is useful to all of us, each at our own levels in the social sciences. But while the basic repertoire of heuristics can be deployed in a number of ways and at a number of levels, it is still a unified repertoire. I begin, then, by discussing in the rest of this chapter the two simplest means for producing new ideas: the additive heuristic that we call normal science and the use of heuristic “topics,” or commonplaces.

II. THE ROUTINE HEURISTICS OF NORMAL SCIENCE

George Pólya argued that “[t]he aim of heuristic is to study the methods and rules of discovery and invention” (1957:112). That might make us think that discovery can be made utterly routine; we learn some rules, turn a crank, and voilà—discoveries! But Pólya clearly meant something more as well. Heuristic *does* go beyond the routine ways we have for producing discoveries. Yet before seeking those, we need to think for a moment about the routine roads.

Thomas Kuhn has provided what for many people is the standard account of discovery, both routine and nonroutine. When Kuhn wrote *The Structure of Scientific Revolutions*, he aimed to replace what we might call the big-edifice model of science. On this model, science at any given time is a big structure of accepted facts, theories, and methods. Scientists are perpetually making new conjectures, testing them on reality with various methods, and then finding them rejected or accepted. If

accepted, they become part of the edifice; if not, they don't. The model is gradualist and incremental. Science grows bit by bit, like a big brick building being put up on a firm foundation. We might occasionally replace sizable walls, but we spend most of our time tuck-pointing or building small additions.

To Kuhn as to many others, this vision of science seemed inaccurate. Most major scientific theories seemed to burst on the world like the revolutions of Copernicus, Newton, Darwin, and so on. They were hardly gradualist. Kuhn resolved this dilemma by separating normal science from paradigm-changing science. He argued that science is organized in paradigms, within which research happens incrementally. Little results pile up. New parts of the building are built. Decayed bricks are replaced. But as this normal science goes on, some stubborn realities refuse to fit. These anomalies pile up to the side. They are attributed to mistaken observation, errors in analysis, and so on. Once the pile of anomalies becomes very large, someone sees that by looking at everything differently—different method, different theory, different interpretation of findings—one can account for everything the old paradigm covered as well as for all the anomalies. Kuhn called this transformation a paradigm shift. It embraces new methods, new theories, even new definitions of the facts of the real world. It means tearing the old building down and building a new one with the leftovers, the anomalies, and some new materials.

As this description implies, the central heuristic rule of normal science—science *within* paradigms—is simple addition. If one is an ethnographer, one studies a new tribe or a new situation. If one is a historian, one chronicles a new nation or a new profession or a new war. If one is an SCA analyst, one uses a

new independent variable or sometimes even a new dependent variable; one gets a new data set with which to study an old problem or asks an old question in a new way; one tries a new model. If one is a formalist, one changes the rules a bit and recomputes the equilibriums or the parameters of the consequent structure or whatever. If one is a small-N analyst, one adds a few more cases or goes into more detail with the cases one has or perhaps adds a new dimension of analysis.

There are several versions of this more-of-the-same heuristic. The simplest is more data: we take the same ideas to a new place. To be sure, the ethnographer with a new case and the SCA scholar with a new data set are usually not just adding another example. Usually there are minor differences that enable the new data to improve old ideas rather than simply repeat them. But for the beginning social scientist, the normal-science heuristic of "it works here, but will it work there?" is a perfectly fine opening for a research project.

The second version of addition is the addition of some new dimension of analysis. Usually this is a minor dimension. Major recastings are the objects of the stronger heuristics I discuss below. But under this heading we have, for example, the huge number of SCA studies of the form "I know that x leads to y ; suppose now I introduce controls for s , t , and u ." For example, women are less likely to end up in the natural sciences and mathematics. Will this be true if we control for native ability? for college major? for parental encouragement? for choice of high school classes? and so on. Or consider the long-standing historical finding that the revolutionary political parties of the nineteenth century usually had their origins among artisans rather than among unskilled town laborers or agricultural

laborers. Was this also true in areas where artisans were few? Was it true in Catholic as well as Protestant regions? east of the Elbe? and so on.

Finally, addition sometimes takes the form of adding a new model or methodological wrinkle or theoretical twist. For an ethnographer of science, this might be taking a more careful look at the exact language that was used in interviews, to see whether the order in which scientists said certain things revealed new aspects of their assumptions. For a rational-choice modeler, this might be trying four or five different forms of "game," rather than just one or two, to understand a particular bargaining structure. For an SCA analyst, it might be putting exponential terms into the equation, to see whether certain independent variables had not only linear but also nonlinear effects.

All of these—from simply adding data to adding a new dimension for analysis to adding a new methodological or theoretical wrinkle—are basically minor, incremental additions. They are the tuck-pointing and reshingling and addition-building of normal science. They are the conservative strategy for social scientists, and it should come as no surprise that graduate students—the most conservative of all social scientists (because they have the most at risk)—should be assiduous practitioners of the additive heuristic. Libraries are filled with unpublished doctoral dissertations that carry out such additive projects. Scholarly journals receive dozens of submissions based on them.

Such studies are profoundly useful. One brilliant contribution does not fully establish a new argument. Adding new cases or variables or rules is always a useful first step in the full

evaluation of ideas. And so it is right and fitting that most of us begin our careers with the additive heuristic, and it is not at all surprising that many of us never leave it.

But the ultimate aim of heuristic is to improve on such normal science. Remember Pólya's definition: "The aim of heuristics is to study the methods and rules of discovery and invention." Invention is what we seek, not just addition. How exactly does one go about creating rules for invention?

III. TOPICS AND COMMONPLACES

There is, it turns out, something of a tradition about invention. It is not found in the sciences but rather in the field of rhetoric. We often use "rhetoric" as a negative word, to label tricks of language or argument. We think of rhetoric as false or at least deceptive. But the ancient writers on rhetoric—people like Isocrates, Aristotle, Cicero, and Quintilian—were mainly concerned with training people as knowledgeable speakers in public settings or as articulate experts in legal settings. And so for them, rhetoric was a good thing, both positive and creative.

The ability to come up with dozens of arguments was central to the classical writers' vision of rhetoric. (Ideally one could do this on one's feet, talking, but in practice speeches were written ahead of time and rehearsed extensively.) Rhetoric textbooks customarily began with a section entitled *inventio*. (*Inventio* is the Latin word; the Greek for this was *heuresis*, from the same root as *heuristic*. See Clarke 1953:7.) This section covered the many ways to think up or invent arguments. The most general ways to do so were called topics and included extremely abstract things like "sameness," "difference," and "genus and

species." More concrete sources for arguments were called commonplaces, which were familiar notions, like the idea that criminals did or did not keep committing the same crime—common beliefs that often came in pairs, one on each side of an argument.

Apprentice speakers learned huge lists of topics and commonplaces and their subdivisions. Mastery of such lists was considered the foundation for effective argument. It is hardly surprising that in time there were complaints that oratory had become boring. What had been meant as a guide to inventing new ideas had become a machine producing endlessly familiar ones.

We social scientists have such rhetorical forms, topics, and commonplaces ourselves. The most famous—as familiar to high school students in America as the six parts of a classical speech were to similar students two millennia ago—is "compare and contrast." (It was on Aristotle's and Cicero's lists, too.) "Pros and cons" is another enduring rhetorical form, also on most ancient lists, as it is in the repertoire of most scholars today. Each of these rhetorical forms can be invoked in the heat of argument to provide a prefabricated layout for a discussion. And each can sometimes become very mechanical.

But the use of rhetorical forms and topics as means to invention suggests that there might be similar forms and topics for social science invention. These would be lists of topics that could be applied to any argument at any point to generate new things to say. The idea is simple. You have a tried-and-true list of abstract categories or concepts, and when you find yourself running out of ideas about some aspect of social life, you go to

the list and see what it suggests to you. The problem is that you must first get some good lists of categories or concepts to use as topics.

Bearing in mind the fate of these lists in ancient times (that is, people took them too seriously, and the lists got very boring), we are not going to be particularly worried about whether our lists are the right lists or the true lists. It doesn't matter whether they are justified ontologically or epistemologically or whatever. (I wasted at least two years of graduate school trying to decide on the "right" abstract concepts and came to no conclusion at all. What I *should* have thought about was which lists seemed more fruitful, not which were "right.")

Here I will mention four such topical lists—two classical and two modern—that I myself have often found useful: Aristotle's four causes, Kant's list of categories, Kenneth Burke's five keys of dramatism, and Charles Morris's three modes of language. There's no particular reason these should be your topics lists. Indeed, I've used other lists from time to time. But these happen to be the ones that have most often proved useful to me. They are also lists that have recurred in the works of many writers under many different labels. But let me reiterate that this is not necessarily because they are "right" (although it would be hard to come up with a concept of cause that didn't fit Aristotle's analysis one way or another.) Rather, it's because they are useful. They help us make quick switches in our intellectual attacks on problems. You have already been introduced to one of these lists, by the way; I used Morris's modes of language to organize the first chapter of this book.

A. Aristotle's Four Causes

I start with Aristotle's four causes. It's a simple list:

- material cause
- formal, or structural, cause
- effective cause
- final cause

When we say, "The Republicans lost the election because they lost the women's vote," we invoke material cause. In this case, something happens because of the social materials that went into making or unmaking it. Demography is par excellence the social science of material cause. It concerns numbers of people of varying types and the ways in which those differing numbers shape social life.

By contrast, we might say with Georg Simmel (1950) that all social groups with three members are inherently unbalanced, because two of the three always ally against the third (something those of us who were only children in two-parent homes know very well). Here we are saying something not about social material but about social structure. It is the shape of the triad that gives it its peculiar properties. This is *structural* cause.

Aristotle's *effective* cause is the most familiar of his four. The effective cause of something is what brings it about, what forces it to happen. So we say that a strike caused employer retaliation or that a newspaper caused a war. These are statements about a direct kind of forcing.

By contrast, *final* cause refers to the aims of events. When we say the cause of universities is the need for education, we are

attributing the existence of universities to their final cause (which today we often call function, although that's not exactly what Aristotle meant). When we say the reason for pollution laws is the need for clean air, we speak of final cause. Note that a lobbying group is likely to be the *effective* cause of those laws, even as a configuration of larger political interests and oppositions is likely to be their *structural* cause. And the numbers and distribution of those interests are the laws' *material* cause. Every event has causes of all four kinds.

Another example can show how using the four-cause list helps us think up new questions to ask. Consider unemployment. One can think of unemployment in terms of its *material*. The unemployed: Who are they? What are they like? What kinds of qualities do they share? Does unemployment concern a kind of person or a transitory state for many different kinds of people? This is to think of unemployment demographically. Or one can think of unemployment in terms of its proximate, *effective causes*: How do layoffs work? Who decides who gets fired or laid off? What are the incentives for choosing unemployment? What are the economic forces driving lowered employment? Or one can view unemployment in terms of its *formal, structural* properties: Could it be the case that unemployment is a general structural quality of a certain production system and that merely random forces decide who in particular is unemployed and why? Or one can view unemployment *functionally*, asking whether it does something useful for somebody (for example, does it help employers by lowering wages for those remaining in jobs, because they can be threatened with unemployment if

they complain?) and whether that somebody, directly or indirectly, maintains it because of this utility.

As you can see, the Aristotelian list is very useful. Time and again, you can come up with something new by switching to a new type of cause from the one that you are implicitly using. It's also true that you can often come up with something new by switching from one to another *logical* concept of cause, from *sufficient* cause (something sufficient to bring another thing about) to *necessary* cause (something without which another thing cannot occur) and vice versa. But the Aristotelian list is probably more useful, which perhaps explains why it reappears with so many different names and guises; it can always be used in a tight spot to come up with a new attack on a problem.

B. Kant's List of Categories

The Kantian categories, although much more abstract than Aristotle's four causes, are also a useful list of topics. Kant thought there were some basic frameworks through which all experience was filtered. There are twelve of these categories, and they make another useful list of aspects of a problem to think about. Kant organized them under four basic headings: quantity, quality, relation, and modality. In what follows, I give the categories commonsense meanings, not the formal philosophical ones Kant gave. Our aim is not to get Kant right but to make him useful for us.

Quantity

- unity
- plurality
- totality

Quality

reality
negation
limitation

Relation.

substance/accidents
causality/dependence
reciprocity

Modality

possibility/impossibility
existence/nonexistence
necessity/contingency

The Kantian quantity categories are unity, plurality, and totality. These suggest a number of essential ways to rethink a research question. Unity raises the issue of the *units* of our analysis: What are they? Why? How are they unified? What, for example, is an occupation? It's obvious what holds doctors together as a unit, but what about physicians' assistants? what about janitors? waiters and waitresses? Are these really units?

Plurality raises all the concerns of *number*. Are there few or many units? Does it matter how many there are? Could different people count them differently? So, for example, how many occupations are there? Does it make a difference whether we lump wait staff and cooks together? What about baby-sitters and elder-care workers? Or social classes: how many of them are there?

Totality raises the problems of the *overall nature* of a subject. Is it a unified whole? How would we know? In what ways is it

divisible or indivisible? Social class is a famous example here. Is there a power elite, as C. Wright Mills thought? How unified are elites and ruling classes? Are social classes unified wholes or loose units that fade continuously into one another?

The Kantian quality categories are reality, negation, and limitation. These, too, suggest important ways to change our first conceptions of a research problem. The reality category raises the subtle but important question of *reification*, of mistaking an abstraction for a reality or—what is very common in bad social science thinking—imagining that because we have a name for something, it is therefore real. Take the famous concept of socialization, which is supposed to refer to all the training by which an infant and, later, a child becomes an adult. It is by no means apparent that this word refers to anything other than the sum total of experiences a young human has. Put another way, it isn't clear what experience a young person has that could not be said to be socializing that person for something or other. Nor is it apparent when socialization stops and life begins. There is in fact absolutely nothing that is denoted specifically by this concept; it is simply a reification following from the (fallacious) functional argument that because people acquire skills, there must be some special process—different from the rest of life—that “trains” them. Thus, the reality category invokes for us a crucial heuristic discipline, forcing us to ask whether the nouns we use in social science refer to real things.

Negation, too, is a centrally important topic. I shall later discuss several heuristics based on negation: problematizing the obvious, reversal, and the like. I shall also discuss the central heuristic importance of making sure that your idea is capa-

ble of being wrong. We should never forget to think about negation.

Finally, limitation is a crucial heuristic tool. Much of normal science actually takes the form of *setting limits* to generalizations, exploring what sociological positivists like to call scope conditions. Under what conditions is some argument true? At what times do certain forces take effect? These and a hundred other questions all arise from thinking about limitation. So, for example, we might find that many things that we think are long-standing traditions are in fact invented at particular moments. Under what conditions do people invent traditions: When their nationhood is threatened? When a nation is newly formed? Are there particular kinds of people who are more likely than others to invent traditions? Are they leaders of social movements? fallen aristocrats? Are there ways to differentiate invented and "real" traditions? All of these questions arise when we try to set limits on the concept of invented tradition.

The Kantian relational categories are even more important, and all have famous lineages in philosophy. The first of them is substance/accidents—the division of the world into given things (substance) and the properties of those things (accidents). In some parts of social science, the substance/accidents category provides no useful basis for heuristics. When we say that a person is a certain age, for example, we know very well that the person is the substance and the age is the property. But if I ask myself what, say, sociology is, it is not at all clear (unless I fall into reification) what the substance is and what the accidents. Is sociology a name for everybody with certain kinds of degrees and training? Then education defines the sub-

stance of sociology, and other things—people's political values, types of employment, sociological ideas and concepts—become accidents. But I could just as easily define sociology as people who hold certain kinds of jobs, in which case the jobs define substance, and political values, sociological ideas and concepts, and education itself become accidents. Note that this kind of analysis begins to suggest that the whole distinction of substance and accidents is probably a mistake (as, indeed, a large body of social theory believes). At the very least, reflecting on substance and accidents can help you change your way of seeing something.

The second of the relational categories is causality/dependence. Causal questions are obviously central to any heuristic, as we have seen in Aristotle's celebrated list of causes. I won't consider causality further here but simply refer the reader back to that discussion.

The third relational category is reciprocity. This, too, provides a helpful way to rethink social scientific questions. Often we find ourselves in a cul-de-sac, trying to decide which of two things causes the other. We know that higher levels of education are associated with higher income, but which causes which? Higher levels of education lead to higher income over the course of life, but availability of higher income allows the transmission of educational advantage across generations. There is a kind of reciprocity here between income and education that forces us to be much more specific about whose income, whose education, and what temporal orders are involved. The category of reciprocity reminds us to consider such chicken-and-egg models. Many, many systems in social life take this circular format of reciprocal causality. They can be

self-reinforcing systems that stabilize themselves, or they can be runaway systems that blow up. (Loosely speaking, one arises from positive feedback, the other from negative.) The reciprocity category reminds us to think deeply about such systems.

Finally, the Kantian categories of modality are possibility/impossibility, existence/nonexistence, and necessity/contingency. Possibility reminds us that it is easy to come up with social science arguments that are impossible and that, therefore, we need to check our ideas constantly for possibility. This is particularly true because much social science is motivated by a desire to improve society. But certain kinds of improvements are logically impossible. It is impossible, for example, for everyone to be successful if being successful entails some form of superiority to others. At least it is impossible unless we define all forms of success as being absolutely idiosyncratic. Yet social science is filled with arguments that implicitly believe everyone can be successful. So we must always reflect on the range of possibility in constructing our arguments.

The category of existence raises questions much like those of the category of reality. There are many types of social actors: doctors, left-handed people, the insane, and so on. Which of these types actually have existence as groups rather than as simple types? Indeed, what does it mean to say "have existence as groups"? There are many famous examples of this set of heuristic problems. It is easy, for example, to talk about class. But do classes exist? And what does it mean to say that classes exist? Are we talking about self-consciousness of class? about coordinated action? about simple common experience? Or take occupations. Are they simple categories of people? bodies of work? organized associations of workers? What does it mean to say

that an occupation exists? Clearly the most famous examples of contemporary social science involve gender and race. Are women a group? In what sense? The heuristic questions raised by the category of existence are thus like those of the reality category. They lie in questioning nouns we commonly use to denote social groups and asking what kinds of things those nouns actually label.

Finally, the category of necessity/contingency raises obvious heuristic questions about how events relate to one another. In one sense, these are like the questions of the limitation heuristic: are certain relationships necessary, or are they contingent on other things (that is, limited)? But contingency is a much more complex phenomenon than mere limitation. It invites us to ask about the multiple dependencies among social processes, about the many paths that social processes can take. And necessity invites us to focus on necessary causality and its implications. When half the young men of England, France, and Germany disappeared in the trenches of World War I, a generation of young women couldn't marry—because there was no one alive for them to marry. The resultant family structure and indeed the resultant larger social structures of employment and opportunity shaped European society for generations. Like contingency, necessity pervades the social process. A good list of heuristics will never omit it.

The Kantian categories thus provide another useful list of heuristics. As with Aristotle's four causes, we can let the philosophers worry about the philosophical validity of this list. For us it is a useful checklist of things to think about. As it happens, Aristotle had a category list, too, which cut up the world a little differently. Aristotle included two things that

Kant made separate: space and time. Both of these are themselves useful heuristic reminders. Always ask yourself what the spatial and temporal settings of your problem are. How can they be changed? Which aspects of them are necessary or sufficient to determine which parts of the problem? Are there regularities to your question in space (either social or geographical) or time?

C. Burke's Five Keys of Dramatism

Moving to the modern setting brings us to the five keys of dramatism set forth by the famous literary critic Kenneth Burke in his book *A Grammar of Motives*: action, actor, agent, setting, purpose. We can use this list, too, as a heuristic aid to rethinking any particular problem.

Since this is a modern list, I can give a famous example. In his splendid book *The Culture of Public Problems*, Joseph Gusfield reconceptualized drunk driving. He said (among many other things) that accidents caused by drunk drivers are really a transportation problem, a problem of the *setting*, the locations where people drink. The San Diego police had consulted Gusfield about a sudden rise in accidents involving alcohol. He pointed out that if you built four major hotels on vacant land near interstate highways, all of them filled with bars and all of them inaccessible by foot, it was pretty likely that you were going to see more automobile accidents involving alcohol. If people get drunk where they can walk home (as in the pub in England), they are much less likely to drive drunk.

Behind this intellectual trick lay an analysis of alcohol-based accidents in terms of Burke's five keys of dramatism: Are fatal accidents best understood as a matter of

action—driving a certain way, doing (or not doing) certain things (like fastening seat belts)

agents—certain kinds of actors (It turned out plenty of older drivers were drunk on the road, but they were less likely to get into accidents, possibly because they had more experience driving drunk and so were more skilled at it.)

scene—where people drink, how they get there, and how they leave (This was Gusfield's way of attacking the question.)

agency—vehicles and roads (If cars wouldn't move unless seat belts were fastened around passengers, fatalities would be reduced.)

purpose—why people decide to drive when, where, and how they do (Some people drive to get somewhere; others— young men, for example—drive to show off . . .)

Another excellent example of Burkean thinking is the famous paper of Lawrence Cohen and Marcus Felson that introduced the so-called routine-activities theory of crime (1979). Prior theorists of crime had emphasized criminals (that is, positive actors) as the key to crime. Cohen and Felson noted that crime takes three things: an actor (this had been the focus of prior research), a target, and an absence of guardians. We can think of an unguarded target as a certain kind of scene in Burkean terms. The central thrust of Cohen and Felson's argument is that changes in scene caused the crime increase after 1960. More consumer goods were in the home, they were lighter in proportion to their value (and hence more portable), and the entry of women into the labor force meant fewer people

were at home to watch over property. The authors actually compared the weight of dozens of goods in Sears, Roebuck catalogs over the years, as well as the percentages of homes with no one home the first day the census taker called in 1960 and 1971. These and many other equally curious factors paralleled the huge increase in property crime from 1950 to 1975. Once again, a Burkean move raised a whole new theory, in this case of the sources and causes of criminality.

Burke's list is really just another version of the famous old reporters' list of topics: Who? What? Where? When? How? Why? And one can also see in it a fairly strong echo of Aristotle's four causes. Remember that the utility of all of these lists lies less in their novelty than in their heuristic power. Reporters use the who-what-when list to remind themselves to touch all the bases. We are more interested in using lists to remind us that our theories often focus excessively on one or another aspect of what we study. When we need to think anew, it's usually a question of figuring out what aspect of our analysis could be changed to produce a whole new view.

D. Morris's Three Modes of Language

A final topics list is Charles Morris's three aspects of symbolic systems: syntactic, semantic, and pragmatic. This list was of course used in Chapter One. Syntactic relations are relations between elements of the system. Semantic relations are relations between system elements and things to which they refer. Pragmatic relations are relations between symbolic statements and the context of action in which they are made. What is radical about my argument in Chapter One is its noting that many of my colleagues believe that pragmatic approaches to explana-

tion are the only "real" ones. I used the Morris triad to start us thinking about explanation more broadly than is customary. That is, I used the Morris argument heuristically.

It can of course be used in other contexts. There is no necessary reason, for example, to think that it applies only to symbolic systems. You could think about the syntax of markets (internal market relationships) over against the semantics of the connections between groups in the market and their existence outside it. And you could go on to think about what actors in markets are doing (saying) and what the actions (the pragmatic context) of those market assertions are. One way of stating Marx's analysis of work is to say that there was a fundamental error in the belief of liberal economic theory in the separability of the syntax of markets (that is, the wage relationship) and the semantics of the social groups in those markets (workers and capitalists as they were outside the market). Liberal theory said these things could be separated; Marx showed, in endless empirical detail, that they could not. Maybe this is far-fetched, but seeing market relations as related to social relations outside production in the same way linguistic syntax is related to meaning and reference makes the traditional analysis of work suddenly look alive. We can think of new questions to ask.

WITH THE MORRIS LIST, I come to the end of my own current set of topical lists. Social scientists use many such lists through their careers. I have often used knowledge, feeling, action (from Plato, Aristotle, Kant, and any number of others) as a useful commonplace list. Many of us have used various lists of social functions—Talcott Parsons's adaptation, goal attainment,

integration, and pattern maintenance, for example. Most of us also use the disciplines from time to time as a commonplace list: What will the economists think? What would an anthropologist say? Sometimes there's no faster way to come up with a new idea than to wonder how somebody from a different discipline would think about your issue. This is particularly so because, as I noted in the preceding chapter, academic disciplines are organized around different dimensions of difference.

The reader will want to use these and many other lists. But in closing my discussion of topics and commonplace lists, I want to underscore two cautions. First, do not reify these lists. Despite the philosophical fame attached to some of them, we don't need to assume their correctness or truth. They are simply useful lists of reminders of things to think about, reminders to use when you get stuck. Don't worry about their reality or truth.

Second, don't overuse them. Classical rhetoric died because students began to treat it as a meat grinder. So everything from tenderloins to rib eyes to pure gristle was turned into ground beef. Don't use these lists as some kind of comprehensive system that you put each of your research questions through. Just use them when you get stuck. Use them to stimulate your thinking. When you find that stimulation, turn to working out the details of the new argument. Don't run through every last heuristic list for every last idea and then try to put everything together. You'll never get anywhere.

Put another way, a little heuristic goes a long way. You are far better off making one major leap and then working out all the details and subparts of that leap than you are trying to work out the myriad minor leaps and subleaps that could be

taken. Take the time to work out the details of a major heuristic move. As we shall see in the next chapter, most brilliant articles and books are built on *one particular move*. The author made a big move, then spent a lot of time working out the details.