

Chapter One

EXPLANATION

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SCIENCE IS A CONVERSATION between rigor and imagination. What one proposes, the other evaluates. Every evaluation leads to new proposals, and so it goes, on and on.

Many people think of social science less as a conversation than as a monologue. For them, it is a long speech that ends with a formal question, to which reality meekly answers yes or no like the plastic heroine of a Victorian novel. Yet no good researcher believes in such monologues. Researchers know all about the continual interchange between intuition and method, just as they know about the endless teasing of reality as it evades them. Social science in practice is less old-style romance than modern soap opera.

The monologue version of social science is of course easier to describe. There are many excellent books about its machinery:

how to propose a question, how to design a study, how to acquire and analyze data, how to draw inferences. Indeed, many books are organized around particular ways of doing these things, the various “methods,” as we call them: ethnography, surveys, secondary data analysis, historical and comparative methods, and so on. All that is fine and good.

But such books forget the other voice, the imaginative voice of whimsy, surprise, and novelty. This discovery side of social science is more systematic than we think. Social scientists use gambits of imagination, mental moves they employ to hasten discovery. Like gambits in chess, these mental moves are formulas for the opening, developing, and realizing of possibilities. Some are general gambits implicit in the nature of argument and description, while others arise in conceptual issues that pervade the disciplines. All of these gambits work within *any* kind of method. They make up the heuristic of social science, the means by which social science discovers new ideas.

We need heuristic because, as I said, social reality often resists the charms of methodology. As social scientists, we aim to say something interesting—perhaps even true—about social life. Yet social reality often makes a stingy reply to even the best of our methodological monologues, returning tiny correlations even though challenged by the best of questionnaires, returning simpleminded truisms even though watched by months of earnest ethnography, returning boring stories even though questioned by years of painstaking archival research. Social reality wants a subtler wooing; it wants rigor *and* imagination.¹

So this is a book about heuristic, a book of aids to the social scientific imagination. Because I am a sociologist, many of the

examples I use in the book come from sociology. But because the social sciences are all mixed up together, not all of the examples will be sociological. The social sciences share subject matters, theories, and a surprising amount of methodology. They are not organized into a clearly defined system but take their orientations from various historical accidents. Loosely speaking, economics is organized by a theoretical concept (the idea of choice under constraint), political science by an aspect of social organization (power), anthropology by a method (ethnography), history by an aspect of temporality (the past), and sociology by a list of subject matters (inequality, the city, the family, and so on). Thus, there is no single criterion for the distinctions among disciplines. As a result, when one or another discipline becomes too much of a bore, the others make fun of it and steal its best ideas to put them to better use elsewhere. All of this flux means that a heuristics book can range widely, as this one will.

THE FIRST TWO CHAPTERS introduce the aims, means, and assumptions of social science research. I begin with explanation because explanation is the purpose of social science. I then introduce some types of methods—some of the various ways in which social scientists have tried to be rigorous. I treat these methods as concrete realizations of “explanatory programs,” programs that carry out the different concepts of explanation introduced earlier in the chapter.

Chapter Two turns to a more customary approach. I characterize methods in terms of a set of conceptual issues—nine of them, in fact. I first introduce these conceptual issues, then give the customary account of methods (I skipped it in Chap-

ter One), which says that methods are best defined in terms of these nine issues. Then I leave the beaten path. I discuss the critiques that each method poses to the others and show that these critiques lead us into an endless cycling through the methods (both in theory and in practice). Moreover, the conceptual issues themselves turn out not to be fixed things; they have an unstable, fractal character. Not only do they differentiate one method from another, they also differentiate internal strands *within* each method—and internal strands within the internal strands. And so on.

Chapters One and Two are the heavy lifting before the fun part begins. While the main aim of the book is to stimulate imagination, it needs to present a clear sense of rigor as well. Otherwise, we won't be able to tell the difference between imagination and foolishness. Recognizing that difference means getting a secure sense of what explanation is, of why we seek explanations, and of what different kinds of explanations and programs of explanation exist in social science. It also means having a solid grasp of more traditional ways of thinking about rigor, which are presented in Chapter Two, with its litany of the classic methodological debates in social science and its endless isms. (Ultimately, I will turn these isms from dead methodological debates into live heuristics.)

Having set forth the basics of rigor in Chapters One and Two, I then turn to imagination. Chapter Three discusses the general concept of heuristic and sets forth the two simplest heuristic strategies: the additive heuristic of normal science and the use of commonplace lists to generate new ideas. Chapter Four considers in detail the general heuristic gambits that search for importable novelty elsewhere and produce it by

transforming our existing arguments. Chapter Five looks at the heuristics of time and space, the heuristics that change ways of describing or envisioning social reality so as to produce new ideas. Chapter Six examines the gambits that arise out of the basic debates and methodological concerns of Chapter Two—making a positivist move within an interpretive tradition, for example. Finally, Chapter Seven discusses the problem of evaluating the ideas produced by heuristics. It asks how we know a good idea when we see one.

I have drawn examples from as far back as the 1920s and as recently as 1999. Old work is not necessarily bad work. Newton himself is a good example. Newton became the greatest name in modern science by *giving up on* the medieval question of the nature and origins of motion. He solved the problem of motion by simply assuming that (a) motion exists and (b) it tends to persist. By means of these assumptions (really a matter of declaring victory, as we would now put it), he was able to develop and systematize a general account of the regularities of motion in the physical world. That is, by giving up on the *why* question, he almost completely answered the *what* question. So following his example, we learn that switching questions is a powerful heuristic move.

The very same move has occurred in social science. One of the great difficulties in the work of Talcott Parsons, the dominant American sociologist of the mid-twentieth century, was in explaining social change. Parsons held that social behavior was governed by norms, which were themselves governed by values, which were themselves governed by yet more general values. In such a system, change could be conceived only as local breakdown, a problem event that had somehow escaped

the supervising norms. Later writers handled the same problem—explaining change—by simply assuming that social change was not unusual at all; rather, it was the normal state of affairs. With this assumption, the various historical sociologists who challenged Parsons were able to develop much more effective accounts of social movements, of revolutions, and, indeed, of the rise of modernity in general. This was exactly the Newtonian move: historical sociologists gave up on explaining change and simply assumed it was happening all the time. Then all they had to do was figure out what is regular about the way it happens. (They should have gone on to explain stability, of course, but they pretty much forgot about that!)

Thus, old work provides useful examples of heuristics just as new work does. This means that as I introduce the reader to the basic tool kit of heuristics in social science, I can simultaneously introduce some of the great heritage that that tool kit has produced. Let's begin, then, at the beginning—with explanation.

I. EXPLANATION

Social science aims to explain social life. There are three things that make a social scientist say that a particular argument is an explanation. First, we say something is an explanation when it allows us to intervene in whatever it is we are explaining. For example, we have explained the economy when we can manage it. We have explained poverty when we know how to eradicate it.

Second, we say an account explains something when we stop looking for further accounts of that something. An explanation

is an account that suffices. It frees us to go on to the next problem by bringing our current problem into a commonsense world where it becomes immediately comprehensible. So sociobiologists say they have explained altruistic behavior when they show it to be merely an accidental result of selfish behavior. They go no further because they think selfish behavior is self-evident; it needs no explanation.

Third, we often say we have an explanation of something when we have made a certain kind of argument about it: an argument that is simple, exclusive, perhaps elegant or even counterintuitive. Thus, we may think Freudian psychology is better than folk psychology because it is better worked out, more complex, and more surprising. In this third sense, an account is an explanation because it takes a certain pleasing form, because it somehow marries simplicity and complexity.

The first of these views—the *pragmatic* view that an explanation is an account that enables us to intervene—is the most familiar. Consider the explanation of germ-based disease. We think discovering a germ is explaining a disease because by discovering the germ, we have discovered something that enables us to stop the disease. Note that this pragmatic approach to explanation works best for phenomena that have somewhere a narrow neck of necessary causality: something absolutely necessary to the phenomenon yet clearly defined and subject to outside action. It is this narrow neck—the necessity of a particular organism—that makes the germ-based diseases easier to fight than diseases “caused” by the interaction of millions of small random events—cancer, heart disease, and arthritis. The move to the microcellular level in studying these diseases aims precisely to find a *new* realm where there *may be* a narrow neck—

the necessary presence of a certain gene or enzyme, for example. In social science, however, relatively few phenomena seem to have this narrow-neck pattern. So, as we shall see, the pragmatic approach to explanation in social science has taken a different path.

In the second view of explanation, where an explanation is an account that enables us to stop looking for further accounts, things are different. This kind of explanation works by transposing the thing we want to explain from a world that is less comprehensible to one that is more comprehensible. The attempt to explain all human activities without any reference to group phenomena is a good example. The utilitarian philosophers tried to show that systematic pursuit of self-interest by everyone (an individual phenomenon repeated many times) would, when aggregated, result in the social world that was best for all. Social reality was just an additive total of individual realities. *Apparent* social phenomena, like the (to them unbelievable) phenomenon of people getting along without obvious coordination, *must* be explained as the result of some ensemble of individual behaviors.

This second view of explanation—in which we think explanation is a move from one conceptual world to another—is not a pragmatic but rather a *semantic* view. It defines explanation as *translating* a phenomenon from one sphere of analysis to another until a final realm is reached with which we are intuitively satisfied. So the utilitarians “explain” prosocial behavior as an outcome of individual selfishness because they feel the latter realm—that of individual selfish activity—is more real, more intuitive, than any other. It doesn’t need to be explained any further. It is a “final realm” for explanation.

Of course, different schools of thought have different final realms for explanation. Utilitarians and their followers, the economists, aren’t happy until they have translated a phenomenon into something recognizable on their familiar turf of individuals with preferences and constraints. But anthropologists are equally unhappy until they have translated those very same preferences into what is for them the familiar realm of culture. This difference makes it awkward to refer to the semantic view of explanation as reduction, which is the usual name for it in the philosophy of science. The word *reduction* seems to imply a hierarchy of explanation, in which “emergent” phenomena are “reduced” to “lower-level” ones. Such a view may make sense for the natural sciences, where it is common to think about reducing chemistry to physical chemistry and ultimately to physics. But it isn’t very helpful in social science, where the final realms of the various disciplines and research traditions are not shared or ordered in any way.

The third view of explanation, as I noted, derives from the characteristics of explanation itself. Often we think an explanation is satisfactory simply because it is logically beautiful and compelling. Indeed, sometimes we find an explanation beautiful and satisfying without believing it at all. This is the reaction most people have to Freud on a first reading. It may or may not work, but how elegant it is! How simple yet comprehensive! Many have the same reaction to Jean Piaget’s early work on the origins of intelligence in children. From such tiny postulates, he managed to produce so many insights! Reflective life creates in us a desire for pretty argument. We may not like its premises, its content, or its results, but we all appreciate its enticing mixture of complexity and clarity.²

Formal writing about explanation has usually taken this third view, that explanation has to do with the properties of an argument—specifically, its logical structure. In the most famous article on explanation in the twentieth century, the philosopher Carl Hempel argued that to explain is to demonstrate that the starting conditions in the case that we want to explain fit the hypothesis conditions of some general “covering law” (1942). For example, we might have the covering law that when a political party has a substantial majority in a parliament, it will be able to have a large effect on the country. Then we demonstrate in a particular case (say, Great Britain in 1997, after the Labour landslide) that one party had such a substantial majority. We can then say we have explained why the Labour Party has had a strong effect on British policies in the years after 1997: the conjunction of our covering law—“whenever a party has a strong majority, it has a big effect”—with our empirical premise—“Labour in 1997 got a strong majority”—logically entails the empirical conclusion that “Labour had a large effect on the country.” By combining the general law with a demonstration that our particular case fits the condition of that law, we can use the conclusion of the law to explain the particular outcome in our particular case.

Hempel’s view of explanation focused on the logical pattern of an account, on the way its parts are put together. His is a *syntactic* view of explanation, for it emphasizes the syntax of an account rather than its ability to help us act (the *pragmatic* view) or its ability to translate a phenomenon into a realm we think we understand intuitively (the *semantic* view).

Now the goal of social science, as I have said, is explanation

of social life in whichever of these three senses we choose. A century or so of experience has taught social scientists some standard ways to go about this.³

II. METHODS

Social scientists have a number of methods, stylized ways of conducting their research that comprise routine and accepted procedures for doing the rigorous side of science. Each method is loosely attached to a community of social scientists for whom it is the right way to do things. But no method is the exclusive property of any one of the social sciences, nor is any social science, with the possible exception of anthropology, principally organized around the use of one particular method.⁴

One might expect that the various social science methods would be versions of a single explanatory enterprise or that they would be logical parts of some general scheme, but in practice they don’t work that way. Far from being parts of a general scheme, they are somewhat separated from one another and often mutually hostile. In fact, many social scientists use methods that take for granted that other methods—used by other social scientists—are useless. But nobody cares much. The various methodological traditions roll along, happily ignoring one another most of the time.

It is therefore not at all obvious how best to classify methods. If we recall the basic questions of method—how to propose a question, how to design a study, how to draw inferences, how to acquire and analyze data—we can see that any one of these questions might be used to categorize methods. If we categorize by type of data gathering, there are four basic social science methods:

1. *ethnography*: gathering data by personal interaction
2. *surveys*: gathering data by submitting questionnaires to respondents or formally interviewing them
3. *record-based analysis*: gathering data from formal organizational records (censuses, accounts, publications, and so on)
4. *history*: using old records, surveys, and even ethnographies

If, by contrast, we begin with how one analyzes data, we might have three methods:

1. *direct interpretation*: analysis by an individual's reflection and synthesis (for example, narration)
2. *quantitative analysis*: analysis using one of the standard methods of statistics to reason about causes
3. *formal modeling*: analysis by creating a formal system mimicking the world and then using it to simulate reality

If we begin with how one poses a question, we might note the important issue of how many cases we consider. This would give us three kinds of methods:

1. *case-study analysis*: studying a unique example in great detail
2. *small-N analysis*: seeking similarities and contrasts in a small number of cases
3. *large-N analysis*: emphasizing generalizability by studying large numbers of cases, usually randomly selected

Any one of these categorizations could be used to classify methods. Moreover, putting these three category systems together gives one $4 \times 3 \times 3 = 36$ possible subtypes. And in fact, the majority of these subtypes have been tried by someone at some point or other.

Because there is no obvious list or categorization of methods, I will simply give five examples of conspicuously successful methodological traditions: ethnography, historical narration, standard causal analysis, small-N comparison, and formalization. Most of these have been hybridized in various ways, but we can look at the hybrids later if we need to. (Actually, small-N comparison will serve as an example of hybrid methods throughout.) Note that these five examples do not make up an exhaustive list. Indeed, they come out of different ways of categorizing methods. Ethnography is a way of gathering data, narration is a way of writing it up, small-N comparison is a choice of data size, standard causal analysis is a general analytic approach, and formalization is a specific analytic approach using purely abstract data. Let me reiterate. There is *no one basic way* to categorize methods, nor is there any simple set of dimensions for arraying them. Methodological traditions are like any other social phenomena. They are made by people working together, criticizing one another, and borrowing from other traditions. They are living social things, not abstract categories in a single system. Each of the five methods that follow is a living mode of inquiry with a long and distinguished lineage.

A. *Ethnography*

Ethnography means living inside the social situation one is studying and becoming to some extent a participant in it.

One's participation can range from mere observation to going native, from occasional afternoons to round-the-clock immersion. One can augment this participation with interviews, guidance from key informants, and review of official records.

An ethnographer's questions are often not very detailed before the field research begins, although the researcher will have a general puzzle or problem. As an ethnographer proceeds, he or she generates a mass of field notes: records of events, interviews, observations, and reflections about personal reactions, as well as endless verbatim records of conversations and interactions. The ethnographer floats into and out of the field situation, trying to keep an outsider's view even while developing an insider's one as well. Continually reading and rereading field notes, the ethnographer thinks up new questions to ask and new avenues to explore. This constant reflection is difficult, and as a result the field experience is disorienting, as is evident in the famous field diaries of the anthropologist Bronislaw Malinowski (1989).

When the fieldwork is done, the ethnographer returns home and contemplates these hundreds of pages of notes. Questions become clearer. Connections and themes begin to surface as the inchoate data are classified and reclassified, thought and rethought. The result is most often a monograph of some sort, with chapters that pose the now clear question, set the ethnographic scene, present extensive data from the field, and in the end provide a theoretical insight.

As an example, consider *Witchcraft, Oracles, and Magic among the Azande* by E. E. Evans-Pritchard. Evans-Pritchard made several extended sojourns among the Azande between 1926 and 1930. Interestingly, he did not go to the field to study what he

eventually wrote about: "I had no interest in witchcraft when I went to Zandeland, but the Azande had; so I had to let myself be guided by them" (1976:242). As a result of that guidance, Evans-Pritchard wrote a monumental book that explores not only witchcraft but all the "metaphysical" ideas of the remarkable Azande. The central question eventually became one of *why* the Azande held the beliefs they held about the supernatural and the nonobservable. Evans-Pritchard gave a functional answer to this question; beliefs in witchcraft, oracles, and magic served mainly to reinforce the social and cultural status quo. But this simplistic summary of the book belies its extraordinary richness. One comes away from it having questioned not only Azande beliefs but also one's own.

B. Historical Narration

Historical narration is another methodological tradition. Much of historical work is descriptive, examining the question of *what really was* the state of affairs in a particular place and time. But historians often pose a specific narrative question: most commonly, why did such and such an event take place? Historians apply many methods to such questions. Much of historical work consists of amassing published or archival materials from the time and place studied, so-called primary materials. Strange as it may seem, historical data are often embarrassingly rich; we often know too much about the details of the past. As a result, historical method often takes the form of trolling these seas of old data for important materials.

The heart of historical method is the reading of documents themselves. An informed historical reading of primary materials presupposes extensive—indeed overwhelming—knowledge of

the time and place that produced them. Often this includes not only knowing the environing historical record but also knowing foreign languages (or old usages in one's own language) and indeed recognizing the historical and regional varieties both of languages and of the many forces behind the survival of the documents read. The historian (or any social scientist employing historical methods) walks a thin line between overinterpreting and underinterpreting sources. No source should be read out of context, but the art of historical discovery often lies in figuring out how previous conceptions of that context were wrong. Thus, reading documents *seems* easy but *is* difficult.

Like the ethnographer, the historian carries out many tasks simultaneously, now seeking documents, now reading them, now looking for more, now assembling preliminary arguments and recasting earlier interpretations. As with ethnography, there is a long and painstaking process by which a researcher assembles a synthetic view of something that is first perceived only through a welter of particular detail. But it has long been a custom of historians to hide their arduous research process under an elegant mantle of prose. Without question, history is the best written of the social sciences, perhaps the only social science that is read widely for pleasure by nonspecialists. As a result, history and in particular historical narrative seem at their best to be simple and effortless. That simplicity, however, is deceptive.

A classic example of historical work is A. J. P. Taylor's celebrated and contentious *Origins of the Second World War*. Taylor set himself the task of showing why the European war of 1939 broke out. One of the revolutionary aspects of Taylor's book was that it asked this question at all; previous writers had seen

Hitler's war as requiring no explanation. Taylor's materials included thousands of documents, memoirs, and published works in all the languages of Europe. As with most first-rate history, the methodological efforts that produced the book—the reading of this enormous mass of material, the interpretations tried and rejected, the sources sought but missed—disappear behind Taylor's smooth, ironic prose. His basic interpretation—that German foreign policy in the interwar period was brilliantly (and successfully) opportunistic and that Hitler's ingenuity deserted him only when he gratuitously invaded the Soviet Union and declared war on the United States—caused a furor for decades after its publication.

C. Standard Causal Analysis

Standard causal analysis (SCA) takes large numbers of cases, measures various aspects of them, and employs statistical models to draw inferences about the relationships among those measurements. It then uses the inferences to consider the causal factors that might have produced the correlational patterns that are observed in the data.

Causal analysis starts by defining a universe of cases in which it is interested. These can be anything: people, organizations, families, nations, cities. The cases are then measured by some common yardsticks. These variables can be unordered categories, like race, gender, graduate degree, occupation, or color of eyes. They can be ordered categories, like the familiar five-point attitude scale from "strongly disagree" to "disagree," "don't care," "agree," and "strongly agree." Or they can be continuous scales, like income, wealth, age, and level of education. Much of the hard work in standard causal analysis takes the

form of finding, measuring, and assessing the distributions of these variables. As in ethnography and historical research, this apparently simple task of data gathering is easy to do badly if one is not careful.

One of the variables is taken, in each particular study, to be the dependent variable. That is, the analyst will seek to know the effects of all the other (independent) variables on this dependent one. Mathematically, the analyst tries to replace the dependent variable with a weighted sum of the independent variables. So if the dependent variable is income, for example, one takes so many parts education and so many parts occupation and so many parts gender, and so on, and sees how well one can predict income. There are many mathematical complexities to this approach, and there are several different ways of estimating the results, but the basic approach is always to vary the weights in order to find the weighted sum of the independent variables that *best predicts* the dependent variable. Note, however, that what is independent in one study can be dependent in another, and vice versa.

Analysts choose their variables by trying to think up causal stories that would imply that some variable has a powerful effect on another. Someone predicting individual racial attitudes will probably use region of birth as a predictor, for example. Note, too, that the mathematics does its best to control the interdependencies of the variables. *Either* education *or* occupation does pretty well predicting income by itself, but when the two are together, they aren't twice as good, because they are highly correlated with each other.

A classic example of this type of study is *The American Occupational Structure* by Peter Blau and Otis Dudley Duncan. In

this great work, Blau and Duncan wanted to understand the forces that determine the kinds of occupations people end up in. They were particularly concerned with the degree to which parents' occupations influenced their children's occupations. Twenty thousand male respondents filled out a questionnaire on many topics, among them their race, their occupation and education, and their parents' occupation, education, and employment. The occupations were not treated as categories (doctor, lawyer, and so on) but were converted to a single continuous prestige scale. Thus, the actual dependent variable was the *prestige* of the occupation held by the respondent at the time of the survey (1962). In their basic model, Blau and Duncan showed that the most important factors in determining a respondent's current job status were his educational level and the status of his first job (since the men were of widely varying ages, some had had many jobs). Nearly all the effects of respondent's *father's* education and job came through these two "intervening" variables. (That is, father's education and father's occupation affected respondent's education and first job, which in turn affected the respondent's job as of 1962.) The Blau and Duncan study, which of course had dozens of other findings, helped inaugurate two decades of research on this process of "occupational status attainment."

D. Small-N Comparison

Partway between the detailed analysis of the historical or current reality of a single case and the statistical analysis of many cases lies a method we can call small-N comparison. Typically, small-N comparison investigates a handful of cases, from three to perhaps a dozen. The cases can be many different kinds of

things: bureaucracies, nations, social service agencies, communities, or any other form of social organization.

The particular form of data gathering employed in small-N analysis can vary. There are ethnographies comparing several different field sites as well as histories comparing several different trajectories of nations or classes. Small-N analysis typically emerges within ethnographic and historical traditions and is usually seen as a way of improving generalizations by invoking more (and different) cases. It occasionally arises from the reverse process, in which a quantitative analyst focuses on a small number of cases to improve his or her "reading" of the variables.⁵

Small-N comparison attempts to combine the advantages of single-case analysis with those of multicase analysis, at the same time trying to avoid the disadvantages of each. On the one hand, it retains much information about each case. On the other, it compares the different cases to test arguments in ways that are impossible with a single case. By making these detailed comparisons, it tries to avoid the standard criticism of single-case analysis—that one can't generalize from a single case—as well as the standard criticism of multicase analysis—that it oversimplifies and changes the meaning of variables by removing them from their context.

Small-N analysis has been characteristic of a number of areas in social science. The field of comparative politics has been built on small-N comparison, as has historical sociology. In both cases, there is heavy reliance on secondary literatures concerning the individual cases. Most anthropologists, by contrast, have gone directly from single-case analysis to abstract generalizations based on categorization of dozens of cases (for example,

in studies of kinship, totemism, or folklore), although anthropological linguists have often used comparisons of relatively small numbers of cases.

A classic example of small-N analysis is Barrington Moore's *Social Origins of Dictatorship and Democracy*. This book compares routes to modernity in England, France, the United States, China, Japan, and India. Germany and Russia are also considered, but not in depth. Moore's sources included hundreds of histories of this or that aspect of each country. After endless reading, comparison, and reflection, Moore theorized three basic routes to modernity, all of them depending on how the traditional agricultural classes—lords and peasants—dealt with the coming of commercial agriculture and the rise of the bourgeoisie. In the first route, that of England, France, and the United States, a powerful commercial middle class overthrew the landed classes or forced them to accept middle-class terms. The result was democracy. In Germany and Japan, the bourgeois revolution failed, and the landed classes determined the shape and dynamics of capitalism as it emerged, leading to fascism. In China and Russia, an enormous peasant class provided the main force behind revolution, thus undercutting the drive to capitalism and leading to a standoff between the revolutionaries in the advanced capitalist sector (the Communists) and the peasants. Moore's book provided the stimulus for much of comparative politics and historical sociology in the 1970s, 1980s, and 1990s.

E. Formalization

There are methods in social science that work without much data at all. Or rather they work with what are called stylized

facts. These methods are not methods in the usual sense but rather modes of reasoning about social reality that require some "quasi-factual" input. They are thus halfway between theories and methods.

A good example of this kind of formalization is analysis of the life table. A life table is a description of what happens to a cohort (traditionally, 100,000 individuals) after n years of life: how many are still living, what number and percentage died that year, what the expectation of life is for those remaining, and so on. By combining life tables with birth-rate information, we can work out age distributions for a population, investigate the structure of generations, predict future family structure, and make many other useful demographic projections. We haven't gathered new information but have simply worked out the details implied by the information we already have.

Formalization has gone furthest, of course, in economics, where it has sometimes lost contact with social reality altogether. But formal thinking is important throughout social science. The great anthropologist Claude Lévi-Strauss attempted a largely formal analysis of myths, breaking myths up into a linear, narrative dimension on the one hand and a timeless, structural dimension on the other (1967). The sociologist Harrison White treated job markets (like those for clergymen and college presidents) as if they were electron-hole systems, in which vacancies rather than moving people had the initiative (1970). Mathematical geographers treat arrangements of political boundaries as if they were the product of universal mathematical relationships (Haggett, Cliff, and Frey 1977).

More than any other methodological tradition, formalization lives by borrowing. By nature, formalization is portable, and many a formal analyst has made a reputation by borrowing. Economists borrowed much of their formalism from thermodynamics. Sociologists have borrowed formalisms from physics and biology.

A good example of formalization is Thomas Schelling's famous model of segregation, originally published in 1971 and republished in his remarkable *Micromotives and Macrobehavior*. The Schelling model presumes two kinds of people, one much more numerous than the other, and a neighborhood that people of both kinds would like to live in. Both groups have a similar "tolerance distribution," which describes how willing they are to live in communities of varying mixes of the two populations. The most tolerant within each group will live in a neighborhood as a one-third minority, while the least tolerant will live only in a totally segregated neighborhood, all of their own kind. Under these conditions, Schelling shows, the only two stable equilibria for the particular neighborhood considered are the fully segregated ones. He goes on to demonstrate that if the two groups were of equal size and if the most tolerant of each group were a little more tolerant, there would be a stable fifty-fifty equilibrium. He also shows that if the larger group included *more intolerant* people, there would be a stable integrated equilibrium (because people from the larger group wouldn't keep moving into the neighborhood, frightening out the less tolerant members of the smaller group).

The Schelling models require no real data, only stylized data. But they tell us something important and counterintuitive. They tell us that even somewhat tolerant populations

have a hard time producing integrated neighborhoods when the populations vastly differ in size and indeed that sometimes more tolerance leads to more segregation.⁶

ETHNOGRAPHY, historical narration, standard causal analysis, small-N analysis, and formalization are thus five examples of reasonably successful methodological traditions. Each has its style and its proponents. Each has been combined with these and other methods in a bewildering variety of ways. I want to reiterate that these methodological traditions are not associated *absolutely* with any discipline, although ethnography and narration are somewhat associated with anthropology and history, respectively. I also want to reiterate that these methods do not follow from a single mode of categorization of methods. As I noted, some are methods of analysis, some are ways of gathering data, and so on. They are, if anything, best thought of as practices, as ways of doing social science. As such, they are produced by communities of researchers who practice them, teach them, and develop them. They are living traditions, not abstract recipes.

III. EXPLANATORY PROGRAMS

You may be wondering when you would use one of these methods as opposed to another. Are there hypotheses or empirical problems particularly well suited to particular methods? The usual answer to this question is yes, and the usual procedure would be to present here a list of what method is good for what kind of problem. But my answer to the question of suitability is no. I don't think there are methods that are particularly good for particular questions. So I have no such list. Rather, I will

show that the different methods are in fact aiming to do different things; they envision different kinds of explanations. That argument takes up the rest of this chapter. Chapter Two then shows how the standard idea of "well-suited methods" rests on false assumptions about the methods, and as a result suitability falls apart as a concept. The good news is that that falling apart creates important openings for heuristics, which are, after all, what we are looking for.

We begin by seeing how different methods are in fact trying to accomplish different things. We do this by putting sections I and II of the chapter together, relating the methods just discussed to the three broad senses of explanation introduced earlier.

Each of the three senses of explanation defines an *explanatory program*, a general style of thinking about questions of explanation. And each explanatory program has some versions that are more concrete and some versions that are more abstract. With three explanatory programs, each having concrete and abstract versions, there are six total possibilities. To give the whole analysis in simple form ahead of time:

1. Ethnography is a *concrete* version of the *semantic* explanatory program.
2. Historical narration is a *concrete* version of the *syntactic* explanatory program.
3. Formalization is an *abstract* version of the *syntactic* explanatory program.
4. SCA is an *abstract* version of the *pragmatic* explanatory program.

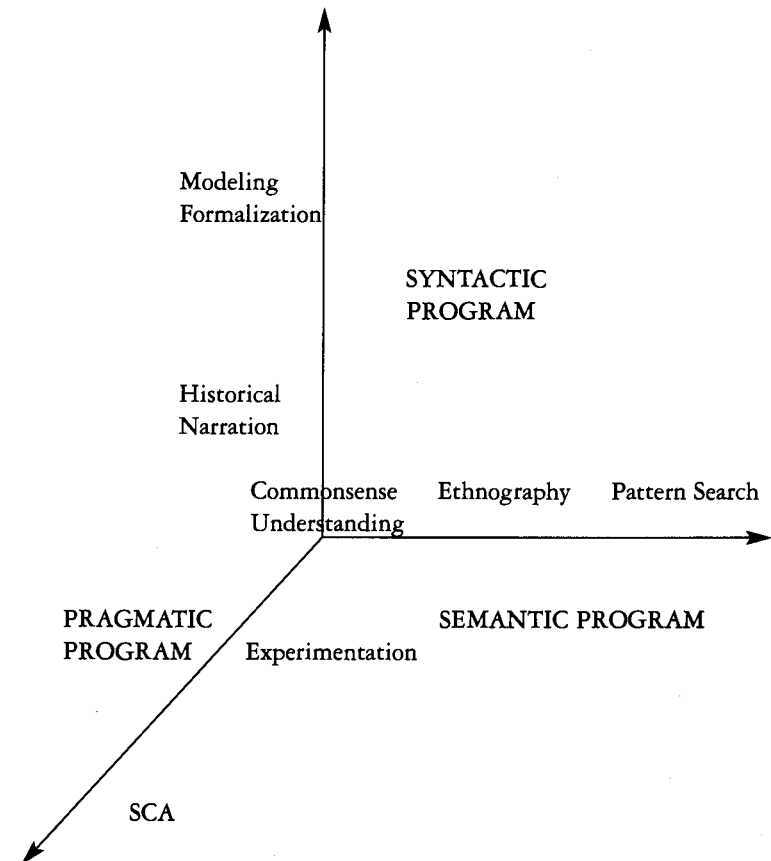
Note that there are two missing possibilities. I shall say very little about one of them: the *concrete* version of the *pragmatic*

program. Think of this as simple experimentation, something we don't do much of in social science unless you think of psychology—which involves a lot of experiments—as a social science. I shall say more about the other missing cell: the *abstract* version of the *semantic* program. Although it has no single name, this is probably the most rapidly evolving area of methods in the social sciences.

This analysis can be seen visually in the figure on page 29. The three dimensions are the three types of explanations. For each of these, the origin stands for explanations focused on everyday particulars, on commonsense events. These are an anchor for each explanatory program, rooting it in the everyday world. From this base, “universalizing” moves reach from the origin toward abstraction along each of the principal axes of explanation. The *syntactic* program explains the social world by more and more abstractly *modeling* its particular action and interrelationships. The *semantic* program explains the world of social particulars by assimilating it to more and more general *patterns*, searching for regularities over time or across social space. Finally, the purely *pragmatic* program tries to separate more and more clearly the effects of different potential interventions or *causes* from one another.

The reader should *not* read this little exercise as a definitive classification of methods but rather as a way to see that the various methods are in many ways trying to do different kinds of things. In particular, I am *not* assuming, as much of empirical social science does, that all explanation involves thinking about causality. We should separate the concept of explanation from that of understanding the causes of something. Our notion of understanding the causes of things has become very narrow in

social science, in contrast to the much more general idea of causality that obtains, for example, in the law.



Let me now show in more detail how this argument works. We start with the programs relating to particulars: concrete, real events rather than abstract ones. Ethnography exemplifies *semantic* explanation of particular events, while historical narration exemplifies *syntactic* explanation of particular events. Both are found near the origin of the figure above, but they lie on

different dimensions. This is not because of their difference in temporality but because of their difference in general explanatory style: translation-semantic type on the one hand, narrative-syntactic type on the other.

A brief aside about temporality. Temporality is a particularly important issue in explanation. Some explanations are focused on processes, on the embedding of social life in moving time. Others devote most of their attention to complex interrelationships in a static "present"; they think social life takes place within a given structure, which they treat as fixed for the time being.⁷ It is important to recognize that *all* explanatory programs have temporal and atemporal versions. For example, there are temporal versions of history (narrative histories like Thucydides' *History of the Peloponnesian War*) and atemporal ones (descriptions of a moment, like Sir Lewis Namier's *Structure of Politics at the Accession of George III*). Temporality is another dimension I could have used to classify methods, but I prefer to leave it for later chapters because of the importance of time in heuristics. What must be emphasized here is that temporality is *not* one of the dimensions that differentiates types of explanations or explanatory programs more broadly. All explanations have to think about time in one way or another.

Returning then to the main argument. In ethnography, the act of explanation is chiefly semantic. When we say that Malinowski, in his great *Argonauts of the Western Pacific*, has explained why the Trobrianders paddle around the islands giving and receiving shells, what we mean is that he has told us enough about their culture and their social life that we can understand why they would do this. We can envision what it is that they see themselves doing, and we can see what they are doing as

reasonable, as something we would do if we were in their place. The field-worker has translated, however imperfectly, their world into one that we find comprehensible. Typically, ethnography accomplishes this by providing detail, by showing ramifications, and by embedding the strange habits of unfamiliar people in the everyday habits of those same people and then connecting their everyday world with our own. The ethnographer may have other professional aims, of course. To return to an earlier example, Evans-Pritchard takes pains, in *Witchcraft, Oracles, and Magic*, to explain to us that the idea of witchcraft serves the epistemological and social function of explaining unfortunate events, an argument by which he sets forth his functional theory of culture. But the *explanation* of witchcraft lies less in the syntax of functionalist explanation than in Evans-Pritchard's ability to translate the activities of the Azande into something thinkable by Western minds. Evans-Pritchard does this semantic translation, for example, in his offhand remark about using the Azande poison oracles to run his everyday life. The Azande make daily decisions by posing a yes-or-no question (for example, should I do ethnography today or not?) while feeding young chickens a small dose of poison. A chicken then makes the decision by living (yes) or dying (no):

I always kept a supply of poison for the use of my household and neighbours and we regulated our affairs in accordance with the oracles' decisions. I may remark that I found this as satisfactory a way of running my home and affairs as any other I know of. (1976:126)

It is not Evans-Pritchard's functional theory that persuades, but this homey detail. *Witchcraft, Oracles, and Magic* is an ex-

planatory success because of its semantic virtues, not its syntactic ones.

Of course, ethnography can have pragmatic and syntactic virtues as well. Ethnography of the drug culture is probably our only effective means to pragmatic intervention in that culture. And Lévi-Strauss's structural anthropology had as its chief virtue an extraordinary syntactic elegance that sometimes amounted to a kind of monomania. But the deep virtue of ethnography as an explanatory program lies in translation. It is principally a semantic program.

By contrast, the great virtue of *narrative* explanation lies in *syntax*. The longstanding literature on the philosophy of history is clear on this point. When Alexis de Tocqueville tells us, in *The Old Régime and the French Revolution*, why that revolution came about, he may here and there employ general laws about social life. But the reason we think his book explains the revolution is that he tells a followable, reasonable story in which a particular sequence of events under those general laws leads in some inevitable way to the revolution. We don't notice his assumptions of general causal laws (for example, "people with large amounts of power don't give it away"). What we notice is the sweeping story that draws us along with France into the maelstrom of revolution.

This syntactic strength is, of course, by no means an abstract one. Narration seems persuasive precisely because telling stories is how we explain most things in daily life. To be sure, there are some quite abstract narrative concepts: evolution (in Herbert Spencer's sociology), habituation (in Max Weber's sociology and throughout psychology), dialectical conflict (in Marxian social analysis), and the like. But these are for scholars.

The real reason we feel that historical narration explains is that narration is the syntax of commonsense explanation, the one we use all the time ourselves. So there is no need to justify it. Indeed, the analytical philosophers of history never could really demonstrate *how* narration explains; they just said over and over that it does.

Like ethnography, narration has other explanatory virtues. Narration often moves us toward a simpler semantic plane. The narrative ideals of followability (Gallie 1968) and reenactment (Collingwood 1946) follow the same semantic principles as ethnography. They measure a narrative's ability to locate us as reasonable persons within itself, as people who *would* have done what *was* done had we been the actors of whom we read. And narration can also have pragmatic virtues. Often, the first step to undertaking action in any particular situation is developing a narrative of how it got to be the way that it is. But again, neither of these is a basic virtue. Serious narration explains things for us because we use unserious narration all day every day. Narration is the syntax of everyday understanding.

The explanatory programs illustrated by both ethnography and narration thus appeal to the commonsense world; the first appeals to the commonsense *content* of everyday experience, the second to the basic explanatory *syntax* of everyday life. Two major streams of explanatory practice in social science grow out of moves to make these two programs more abstract and formal. (This means moving away from the origin in the figure on page 29.) On the one hand, we have the attempt to formalize explanatory *syntax* in modeling and simulation, which embody what I will here call the syntactic explanatory program. This is the explanatory practice that is the *abstract* version of

what *narration* is at the concrete level. On the other hand, we have the equivalent effort to formalize *semantics*, embodied in the family of techniques loosely known as data reduction and pattern search. This strand is the *abstract* version of what *ethnography* is at the concrete level; I shall call it the semantic explanatory program. (It is the important omitted cell mentioned a few pages back, pattern search in its most general version.)

Formal modeling and simulation embody the attempt (atemporal in formal modeling and temporal in simulation) to improve *syntactic* explanation by making it more abstract. The crucial quality sought in the syntactic explanatory program is elegance. In it, a set of statements "explain" some phenomenon if they offer a rigorous, complex, yet simple formal representation of it. On the atemporal side, there are many embodiments of this program: game theory, classical microeconomics, the Markovian tradition in social mobility analysis, the group theoretic version of network theory. The temporal side—expressed most clearly in simulation—has had fewer adherents in social science, although Jay Forrester gave it a very public demonstration in his studies of industrial, urban, and world dynamics in the 1960s, and it has returned in the guise of simulation games. These various methods are astonishingly elegant, some in their mathematics, some in their simplicity, some in their ability to produce unexpected results, some in their extraordinary coherence. All are clear, parsimonious, and in a deep way intellectually pleasing to the abstract mind.

At the same time, these methods share a breathtaking disattention to semantics, to the reference from model to reality. This is well shown by the diversity of some models' applica-

tions. Microeconomics was systematized by Irving Fisher (in the early twentieth century) by borrowing whole cloth the methods of statistical thermodynamics, as if gases and people behaved in the same way. Group theory (a particular branch of modern algebra) saw major application in crystallography and in pure mathematics as well as in sociology's network theory and even anthropology's kinship analysis. Game theory has journeyed from psychological experiments to explaining the stock market and modeling family-planning decisions. Of course, proponents of the syntactic program argue that semantics in fact doesn't matter. These empirical realities all have the same general semantic form, they say, and so one can write abstract syntax for them.

But most readers find the *semantic* assumptions of the syntactic program quite worrisome. What is the point of game-theory models if we can write ten different models for any given social situation? We must choose between those models on semantic grounds, and about those semantic grounds the syntactic program tells us nothing. What is the point of admiring the elegance of microeconomics if microeconomics frankly admits that preferences cannot be generated from inside the system without undercutting the assumptions of the whole edifice? Essentially, microeconomics is telling us that if we can explain what people want to do, *it* can then explain that they do it. So what?

In summary, the syntactic program buys elegance and breadth at the price of semantic indeterminacy and limitation. By contrast with this syntactic explanation via elegant and highly general arguments, the *semantic* program seeks to explain social reality by a different kind of abstraction. It directly

simplifies the complexity of the social world, turning it into a reduced description that a reasonable reader can grasp with the *syntax* of everyday explanation. Thus, techniques like cluster analysis and multidimensional scaling take data of enormous detail and turn it into simple categories and pictures. Pierre Bourdieu, for example, “explained” consumption patterns in France (in his book *Distinction*) by showing that those patterns constitute a language of class distinctions. From the reader’s point of view, the explanation is a matter of common sense once Bourdieu has visually presented the “geometry” of the consumption patterns by using a scaling technique that turns raw data on people’s preferences for cultural materials into a picture locating types of goods and types of people on the same map.

The semantic program has been strong in psychology and particularly strong in market research; marketers routinely use cluster analysis to reduce the American consumer market to one hundred or so basic types of consumers. In that sense, the semantic program has shown considerable pragmatic strength as well. (These are the techniques that are used to figure out your consumption preferences from your Internet use, for example.) On the syntactic side, however, the semantic program has been weak. Its overwhelming focus on one-time analysis makes it static. It can abstractly describe a state of affairs but cannot account for how it changes. Network analysis is one of the glories of abstract semantic explanation, but there is still no real conceptualization for the temporal development of networks. Only when some researchers recently began to think about applying pattern search techniques to over-time data did any kind of syntactic development arrive in the semantic pro-

gram. In short, as with the syntactic program, power of one type was bought at the price of indeterminacy of the other.

I have so far described concrete and abstract versions of the syntactic program (history and formal modeling, respectively) and concrete and abstract versions of the semantic program (ethnography and pattern search, respectively). There is a third abstracting move in social scientific explanation, the one that moves out from the origin along the *pragmatic* dimension of the figure on page 29. Oddly enough, this program has become so successful that social scientists have forgotten that pragmatics is its origin. This is the program carried out by the standard forms of causal analysis in social science, both analysis of the cross-sectional type (as in structural equations models or path analysis) and of the temporal type (as in durational models). Because the SCA program is so dominant in empirical social science, we need to look at it in some detail.

The SCA paradigm arose out of a rationalization of the methods it uses, methods that were originally used to interpret practical experiments. As we saw earlier, these methods work by taking apart the complex particulars in the data (the cases) and treating them as intersections of abstract, universal properties (the variables). Analysis then isolates one of those variables—an arbitrarily chosen dependent variable—and searches out the effects of the other, so-called independent variables on it. Interaction effects—that is, effects arising from two or more variables “working together”—are treated as secondary.

The great explanatory virtue of this method, as originally conceived, was pragmatic. Sir Ronald Fisher and his followers devised these statistical techniques in the 1920s and 1930s to test the effects of experimental manipulations. Should one add

fertilizer or not? Was soil A better than soil B? They put the fertilizer on some fields but not others, measured the effects, and figured out a probability theory for the resulting numbers. They had no particular concern for causes, for why or how growth happened. The point was to decide whether to take some action, not to understand mechanisms. Since the original applications were experimental, these statistical techniques were in fact explanatorily quite persuasive for the pragmatic purpose they served. Used in an experimental context—as they still often are in psychology—they remain so.

Later in the century, however, this approach was applied to nonexperimental data and combined with new ideas about causality. This led to the hybrid explanatory program that is now general throughout the empirical social sciences, the standard causal analysis program. The SCA program still has some pragmatic relevance; the methods are still used in evaluation research, for example. But its main uses are not now pragmatic. Rather, they pretend to be syntactic. So we say (using the weighted-sums approach mentioned earlier) that differences in wages in civil service systems are “caused by” gender, bureaucracy, unionization, and so on. *Semantically*, of course, this whole language of variables is a mirage. The words *gender* and *bureaucracy* do not refer to real entities. Gender and bureaucracy do not exist as independent things; they exist only as *properties* of real things (in this case, of civil service systems). So this “properties” syntax has to be justified by further *semantic* reference. We have to have some way to give empirical meaning to statements about relationships between abstract things like gender and bureaucracy. In economics, this semantic reference is made to formal and simplified models of action. So typical

economics articles in the SCA tradition justify their SCA with a mass of formalizing and calculus that typically begins each article. In sociology and political science, this external reference is made to a set of simplified narratives. So sociology and political science articles of the SCA type begin not with the calculus of the economists but with commonsense historical narratives of the form “such and such people are likely to do such and such things under such and such conditions.” These stories try to justify the “variables-level syntax” by reaching toward the semantic world of everyday reasonable understanding. Thus, in order to be explanatory, the SCA program has to combine its variables-level causal *syntax* with unrelated *semantic* references to other, more credible syntactic approaches to reality: stylized action in the economics case, followable narratives in the sociology one.

All of this complexity happens because in reality the SCA program has no causal foundation at all; it was originally designed to help us make decisions, to be pragmatic. Dressed up as a syntactic program, it is ungracious and silly. (It is also surprisingly difficult to learn, since its rationale—as this long discussion shows—is quite tortured.) Its strongest point remains its ability to tell us about the comparative size of variables’ pragmatic effects on other variables, given the implicit assumption that we have a quasi-experimental situation (which we almost never do). But it can’t even tell us in which direction the causal forces work nor how causes work together. All of those judgments must be imported from elsewhere.⁸

In summary, there is no free lunch. Strongly developing any one aspect of explanation ends up losing much of the rest. In particular, the present moment in social science is probably one

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

I. BASIC DEBATES

- A. POSITIVISM AND INTERPRETIVISM
- B. ANALYSIS AND NARRATION
- C. BEHAVIORISM AND CULTURALISM
- D. INDIVIDUALISM AND EMERGENTISM
- E. REALISM AND CONSTRUCTIONISM
- F. CONTEXTUALISM AND NONCONTEXTUALISM
- G. CHOICE AND CONSTRAINT
- H. CONFLICT AND CONSENSUS
- I. TRANSCENDENT AND SITUATED KNOWLEDGE

II. METHODS AND DEBATES

- A. ETHNOGRAPHY
- B. HISTORICAL NARRATION
- C. STANDARD CAUSAL ANALYSIS
- D. SMALL-N COMPARISON
- E. FORMALIZATION

III. CYCLES OF CRITIQUE

- A. ETHNOGRAPHY
- B. HISTORICAL NARRATION
- C. STANDARD CAUSAL ANALYSIS
- D. FORMALIZATION
- E. SMALL-N ANALYSIS

IV. FROM CRITIQUE TO HEURISTIC