



I

The Chaos of Disciplines



HOW DOES social science change? In its complex history some see immanent trajectories, others see local practices. Some see political determination, others see internal competition. I shall here set forth yet another theoretical account of changes both in social science in general and in sociology in particular. The mechanism proposed is a very general one, applying equally well to other kinds of interacting cultural systems—the plastic arts, music, perhaps even language. But it is best seen in a particular case, and so I shall analyze this very familiar example.

I write about sociology partly because it is my own discipline. But it is also the most general of the social sciences, or, to put it less politely, the least defined. So it provides within a single disciplinary compass examples of many of the processes I am discussing at the level of social science in general. As the reader will see, the idea that a subset of a larger unit can contain scaled-down versions of structures and processes in that larger unit—the idea of microcosm—is central to my argument. The immediate result here is that in this chapter I shall switch back and forth (or perhaps better, up

For those who happen to have read my book *Department and Discipline* (Abbott 1999a), let me point out that the first five chapters of the present book respond to the promise in the final sentence of that earlier one—they discuss how social science must work if we take the view that its cultural structure is comprehensively indexical. (Of course this book was in fact all but complete when the “earlier” one was finished.) As such, this book continues the program, hinted at in *Department and Discipline*, of creating a social theory of contexts (temporal and structural), a project that in turn derives (logically but not biographically) from yet another half-finished book entitled *Time and Social Structure*. From the reader’s point of view then, chapter 1 of this book follows immediately from chapter 7 of *Department and Discipline*. I dedicate this chapter to Colin Lucas, Vice Chancellor of the University of Oxford.

and down) between talking about sociology and talking about social science.¹

The mechanism I propose is in the first instance purely cultural; my account is, in that sense, internalist. By contrast, most current views of intellectual succession are externalist; knowledge is somehow wed to power and power propels change.² But I shall treat both sociology and social science as more or less autonomous bodies of thought under their own rules. I do not challenge the foundational uncertainties of modern epistemology; there is indeed not one sociology but many. But the way those many sociologies interact betrays a common pattern, a universal knowledge upon whose terrain the local knowledges wander. No one can deny the importance of local knowledges and practices—what people call sectarian subdisciplines or alternative epistemologies depending on their academic politics. But I am here more interested in the larger but implicit framework such local knowledges end up making together.

1. A simple form of this notion of self-similarity—the idea that man is a microcosm of the macrocosmic universe—is a long-standing staple of philosophy, and in particular was a commonplace of much of ancient Greek thought. I am unaware of a version that saw microcosms as multiply nested in the manner of fractals, although one could imagine such a view as implied by various versions of the “great chain of being” [Lovejoy 1960].

2. As many people have remarked, there is an obvious “externalist” account for the recent French revival of the idea (originally in Marx) of an intimate connection between knowledge and power, in French, the two words rhyme.

This is as good a place as any to warn the reader not to expect a review of prior writing in the sociology or philosophy of science. One might ask [both reviewers of the manuscript did] how this book relates to Whitley 1984 or Fuchs 1992 or even Collins 1998. The answer is easy; other than sharing some subject matter, it doesn't relate to them much at all. Whitley referred complex contingencies in disciplinary development to the outcome of a series of variables. Fuchs follows in the same tradition, attributing variety in scientific production to various antecedent organizational variables. Both follow precisely the kind of approach that I have rejected in my work on professions (1988a) and my various attacks on the variables paradigm (1988b, 1998; 1999a, chap. 7). Moreover, neither book has much of an account of the origin of ideas (only of styles of ideas), whereas the present book does. Collins's book is a mass of facts undigested by the sketchy theory he applies to them, whereas the present book is mainly a book of theory, illustrated by some detailed and some not-so-detailed examples.

More broadly, this is not a book about the sociology of academic disciplines, but a book using the example of the sociology of academic disciplines to set forth an argument that is much more general. The true intellectual sources of my views on symbolic systems lie in the theory of culture as it was before it was overrun by the textual glitterati. I grew up on the Cassirer-Langer-Mead philosophy of knowledge, the Kuhnian sociology of science, the Marxist theory of ideology and the classical tradition of social and cultural anthropology from Malinowski to the early Geertz. But as the book before you will argue, these are just my personal footnotes for what are in fact generally available ideas. Part of the message of the book lies in its attempt to step out of the metaphor of cumulation. So again, there will be no review of the literature, whether in sociology of science or beyond it.

My interest in that larger implicit framework is both theoretical and practical. On the one hand, I feel that an understanding of it will clarify the relations between various subsets of social science and sociology. Knowing the framework simplifies—perhaps even explains—those relations. But on the other hand, I also feel that a focus on the larger framework is not merely intellectually useful as an idea, but also normatively proper as a commitment. That is, we should become explicit about what is implicit in our practices. For our debates within the social sciences about “universalism” and “local knowledge” have obscured the fact that the vast majority of social scientists share the moral project of knowing society in a way that everyone else in society thinks of as universalist. We can try to add “the voice of the unheard” to our work, but the unheard know very well that social science is something other than their world, that it is addressed to someone other than them. The project of social science as a definable enterprise is, in reality, the production of sharable, “universal” knowledge of society. We ought to stop kidding ourselves that it is not.³

But the larger, universal framework for social science is by no means the standard, often-parodied axiomatic structure. Rather it resembles what the Romans called the law of peoples (*ius gentium*), a law that they applied to diverse groups at the edges of the empire and that they distinguished from the formalized civil law (*ius civile*) that applied specifically to Roman citizens. There is no universal social scientific knowledge of the latter kind—systematic, axiomatic, universal in a contentless sense. There is only universal knowledge of the former kind, a universal knowledge emerging from accommodation and conflict rather than from axioms, a universal knowledge that provides tentative bridges between local knowledges rather than systematic maps that deny them, a universal knowledge that aims, like the *ius gentium*, at allowing interchange among people who differ fundamentally.⁴

1. The Interstitial Character of Sociology

I begin by noting a defining characteristic of sociology—the fact that the discipline is not very good at excluding things from itself.

3. As the argument will make clear and as I shall show in detail in chapter 7, the debate just summarized provides yet another example of a self-similar social structure. It re-creates within the academic community the larger opposition between various partisan groups in the “real world” and the universalists of academia.

4. On *ius gentium* and *ius civile*, see Paton 1964. An obvious implication of this view of “universal” knowledge, one that I shall sketch in chapter 7 and explore in a book tentatively entitled *Methods of Discovery*, is that social science methodology

Not that particular topic areas haven't been excluded; the study of women's lives, for example. But once such an area makes a claim for sociological attention, the discipline doesn't have any *intellectually* effective way of denying that claim. So sociology has become a discipline of many topics—always acquiring them, seldom losing them.

Styles of sociological thought have this character as much as do subject areas. The many sociologists who deny that sociology is a science have not persuaded their scientific colleagues that sociology is humanistic. Conversely, for every sociologist who thinks causal analysis important there is another who pursues narrative explanations. For every sociologist who believes in objective knowledge, another denies it. For every reflective interpretivist there is a rigorous positivist.⁵

Sociology, in short, is irremediably interstitial. In fact, this interstitiality is what undergirds sociology's claims as a general social science, claims not necessarily justified by its contributions in theory, method, or substance. Rather, sociology's claim as the most general social science rests on its implicit and fuddled claim that "no form of knowledge [about society] is alien to it."⁶ The discipline is rather like a caravansary on the Silk Road, filled with all sorts and types of people and beset by bandit gangs of positivists, feminists, interactionists, and Marxists, and even by some larger, far-off states like Economics and the Humanities, all of whom are bent on reducing the place to vassalage. The inhabitants put up with occasional rule by these gangs and pay them tribute when necessary, but when somebody more interesting comes along, they throw off the current overlords with little regret.⁷

This interstitial quality of sociology recapitulates locally the relationship ought to consist of a flexible series of "ways of changing what we are currently doing" rather than a fixed list of possible modes of analysis.

5. As a result, general sociological discourse consists of dire prophecies of disciplinary demise from one embattled oracle or another, even while the discipline muddles along ignoring the prophets' advice.

6. After all, the "classics" of sociology—Marx, Weber, Durkheim, et al.—were in fact general social scientists, not sociologists in the modern sense. The "no form . . . to it" is a paraphrase of Terence, *Heauton Timorumenos*, 1.77.

7. This caravansary quality can be illustrated by sociology's sources of faculty, in my own department, less than half of the faculty were undergraduate majors in sociology. Most are people who stopped by sociology along the road and just settled down there.

The reader may well ask whether such generalism will not wither in competition with other, specialist disciplines, much as the towns of the Silk Road eventually died from the single-minded force of empires. I leave until later in the volume my full answer. For the moment let us recall that generalism may be an excellent strategy in an age of interdisciplinary study and university reorganization and shrinkage, an age when the caravansary seems like a better model for real-world organizations like firms, markets, and states than the model of formal organization so recently popular. An interesting set of review essays on sociology is Halliday and Janowitz 1992.

tion of the social sciences in general to the natural sciences and humanities. The social sciences stand uneasily between these other modes of knowledge, the mode of facts and the mode of values. In its modern version, this placing of social knowledge in the gap between facts and values derives from Kant. It is revealing to study the exact way in which Kant did this.

As is well known, Kant began his analysis of knowledge by splitting knowledge into pure and practical reason: the knowledge on the one hand of the natural and on the other of the moral world. The first of these was mediated by objective cognition, the second by the intuition of freedom. What Kant put asunder few indeed have reunited.⁸

Yet Kant himself repented. In his third *Critique*, he introduced the concept of "judgment" to cover the gap between facts and moralities, and it is in the opening pages of that book that we can see him deciding where in his system to place knowledge of social life. He tells us that when the will behaves in a routine way attributable to some "natural concept," rules describing the will's behavior are "technically practical principles" that lie under the pure (that is, the cognitive or scientific) reason.⁹ Among the examples of such "precepts" is statesmanship (along with housekeeping, farming, the art of conversation, and the prescribing of diet!).¹⁰ Since statesmanship here refers to rule-based knowledge of how people are likely to behave, it is plain that Kant considers some large portion of social science to consist of such "technically practical principles" under the pure reason.

But this means that Kant's dualism begins to collapse. The first two critiques make a chasm between the world of knowledge (pure reason) and that of action (practical reason). Yet here in the third critique certain kinds of rules of *action* are placed under the *pure* reason because they refer to "lawlike" regularities of action. Thus, Kant first makes an overall distinction between pure and practical reason, but then here within the pure reason, he again distinguishes a pure and a practical reason.

Kant made the same move within the practical reason, although not in the *Critique of Judgment*. In various works on politics, he defines the laws of human commonwealths as quasi-natural constraints on the moral individual's transcendently free activity.¹¹

8. Throughout this particular discussion, "practical" means "having to do with action" (*praxis*, in Greek), not "useful in an everyday way."

9. Kant 1968:8.

10. Kant 1968:9.

11. My general sources here are "Perpetual Peace," "Universal History" (both in Kant 1963), and "On the Old Saw: That May Be Right in Theory but It Won't Work in Practice" (Kant 1974).

of hierarchical standing, although there may be a division of labor differentiating them.

But Kant has made a *relational* judgment at one level and then repeated it at the next. We might write a normal hierarchy as follows:

- a is over b and c
- b is over d and e
- c is over f, g, and h

But under Kant's relational judgment here, we have:

- a is over b and c
- b is over d and e, and also d is to e as b is to c
- c is over f, g, and h, and also f is to g as b is to c
- and also g is to h as b is to c

That is, the relation of the general terms is recapitulated in the specific ones, as we see in figure 1.1. This is not a simple hierarchy.

Such a relationship takes a simple form only in one case—if the proliferating relations embody an ordered scale. Then the relational pattern simply picks up points on that ordered scale. So, we might divide 100 percent into the top 50 percent and the bottom 50 percent, then divide each half in half and so on. The result is an elaborate way of using dichotomies to represent linear order.

But Kant obviously does not think there is an infinite gradation from absolute pure reason through some proportionately mixed varieties of reason to absolute practical reason. He has done something else. He has created what I shall call a "fractal distinction." The name captures the fact that such a distinction repeats a pattern within itself, as geometric fractals do.¹³

13. There are, of course, dozens of general sources on fractals. For useful expositions, see Barnsley 1988 (somewhat mathematical), Lauwerier 1991 (general and straightforward), and Feigen, Jürgens, and Saupe 1992 (monumental). Throughout the following exposition, and indeed throughout the book, I have tended to focus on fractals that are nested dichotomies. There is no necessary restriction to this case; it is simply the most familiar and hence makes for the easiest exposition. In chapter 6, I shall discuss some functional fractals that are obviously not dichotomous, and in chapter 7 I will return to the topic of changing the "shapes" of fractal with which we think. For the present, let the warning stand that my argument concerns something much broader than mere nested dichotomy.

I should note, for those interested in sources, that the Barnsley book was my first serious exposure to the concept of fractals. However, I had some inkling of them before reading it; my first paper mentioning fractal distinctions dates from 1986. A crucial paper in making me wonder about scale phenomena in social life was physicist Kenneth Wilson's "Problems in Physics with Many Scales of Length" (1979), which introduced me to the concept of renormalization.

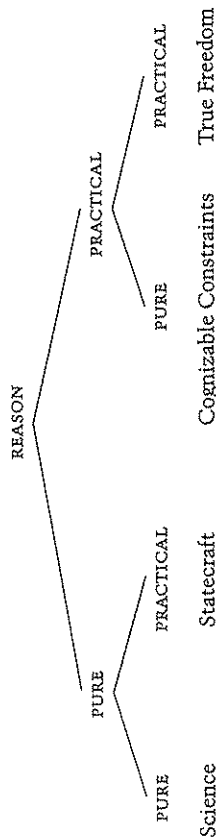


Figure 1.1

Thus, in the essay "On the Old Saw: That May Be Right in Theory but It Won't Work in Practice," he says, "Law is the limitation of each man's freedom to the condition of its consistency with everyone's freedom to the extent possible in accordance with a universal law."¹² Kant's aim in "The Old Saw" is to set aside worldly wisdom (the craftlike statesmanship that he identified as the *practical* part of pure reason in the third critique) in favor of these "duty-creating" constraints that keep transcendently free individuals out of each other's way. Such constraints construct a quasi-natural, cognizable social world within the transcendental realm of free individuals, thus separating the world of *practical* reason into this pure (cognitive) part and the other, practical (transcendentally free) part.

But this last is precisely the same distinction that Kant made under the pure reason, a distinction that itself recapitulated the overall distinction of pure and practical reason. In summary, Kant has first split pure and practical reason and then, under *each* of those headings, has split pure and practical reason once again (see fig. 1.1).

I want to draw two morals from this philosophical excursus. First, the subject matter of social science has by this procedure been made sometimes subject to pure and sometimes to practical reason. It lies between knowledge and action, between facts and values. I shall return to this topic in chapter 7.

Second, Kant has produced this interstitiality via the tricky logical device of making a distinction and then repeating it within itself. This procedure is familiar, but not usually as a mode of reasoning. We usually use it in the concept of nested hierarchies. So we say, for example, that captains stand over lieutenants, lieutenants over squad leaders, and squad leaders over individual troops. Note, however, that such a hierarchy specifies no relation between people or units at a given level; one platoon is the same as another in terms

12. Kant 1974:58.

2. Fractal Distinctions

The concept of fractal distinctions not only proves useful in understanding the external location of the social sciences generally. It also provides an essential tool for understanding relations within them. Indeed, as I shall show, both the external and the internal structures are produced by the same mechanism.

We typically distinguish the various social sciences and various positions within them using a set of dichotomies. Every graduate student learns them. Some of these dichotomies concern the object of study: a focus on social structure or on culture, emphasis on the emergent versus the individual levels, beliefs about the constructed versus the real nature of social phenomena. Other dichotomies involve the aspects of social phenomena taken as problematic—choice versus constraint, conflict versus consensus. Still others differentiate methodological styles—narrative versus analysis, positivism versus interpretation. And some concern the nature of the knowledge obtained: pure versus applied knowledge, situated versus transcendent knowledge.

All of these dichotomies are, like Kant's pure versus practical reason, fractal distinctions. Synchronically, this means that if we use any one of them to distinguish groups of social scientists, we will then find these groups internally divided by the same distinction. Diachronically, as I shall argue below, fractal distinctions cause a perpetual slippage of the concepts and language of social science.

We begin with synchronics. Consider methodological approaches. For about sixty years, sociology has been divided into two broad methodological strands, usually called quantitative and qualitative. Put starkly, the quantitative position recognizes only those social phenomena measurable on univocal scales. The qualitative side attributes multivocality to all social phenomena and therefore denies strong measurability. This sounds like a simple opposition. But *within* each one of these strands can be distinguished "quantitative" and "qualitative" positions. On the quantitative side, for example, the admired "causal" methods like regression contrast with the denigrated "descriptive" methods like scaling and clustering. On the qualitative side, there are relatively formalized measurement procedures that are used by some sociologists of culture and by most practitioners of conversational analysis, while strongly interpretive strategies characterize much of the new sociology of science (see fig. 1.2).

Reflection reveals that all the dichotomies just mentioned behave in this fractal manner. Consider "pure" versus "applied" sociology.

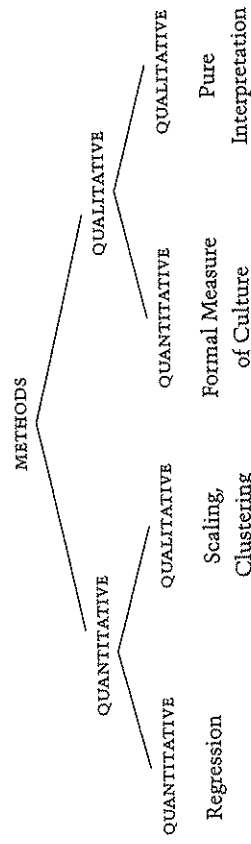


Figure 1.2

Within "pure sociology" we have general theory on the one hand and empirical research in areas like stratification or demography on the other. But within "applied sociology" there too is a theoretical strand, focusing on how application works, which stands over against the actual applications themselves. Or again, a longstanding wisecrack (usually attributed to James Duesenberry) holds that economics is about how people make choices and sociology is about how people have no choices to make. Yet within economics, constraints (e.g., budget constraints) play a major role, while much sociological theorizing in research areas like stratification relies heavily on theories about individuals' choices. The reader can no doubt supply the examples that fit the other dichotomies mentioned.

Such fractal distinctions are analogous to segmental kinship systems. A lineage starts, then splits, then splits again. Such systems have a number of important characteristics. For one thing, people know only their near kin well. I may be quite clear that my collaborator is more positivistic than I and that our research group as a whole takes a more complex, interpretive approach than do other groups working in the area. But I am likely to be hazy about matters further away. To a sociological theorist, OLS and LISREL amount to the same thing, just as ethnomethodology and symbolic interactionism are indistinguishable to a sociological empiricist.¹⁴

Second, like tribesmen sociologists get to know one another through long discussions about kinship that serve to establish their common ancestors. That is, two sociologists newly together will argue about the relative merits of positivism and interpretation until they know roughly where they stand relative to one another and relative to the principal methodological communities in the disci-

14. The classic work on such systems is Evans-Pritchard's *The Nuer* (1940). See chapter 5.

pline. The single dichotomy thus encapsulates a whole proliferating system of relations without requiring new labels for each "generation" and each position. In this lies the great power of these simple contrasts; they are, in Lévi-Strauss's celebrated phrase, good to think with.¹⁵ Cheap and portable, yet immensely fruitful.

But simplicity has the price of indexicality. A simple contrast summarizes an entire structure only by becoming rootless. If I tell you I am a positivist, you in fact know only that in my usual domain of interaction most people I deal with are more interpretive than I. Unless you can already identify that usual domain of interaction, you don't really know anything more than you knew before I spoke. Relative to you, I might be strongly interpretive. Where people do not already know each other's positions, therefore, the indexical character of our most important terms guarantees cacophony. (Hence the futility of most seminars.)

This indexicality can of course be used instrumentally. In the first instance, it provides a general means for rejecting any assertion about a fractal dichotomy by changing the frame of reference. For example, it enables one to reduce an opponent's position to a version of one's own simply by changing that frame of reference. For example, in *Culture and Practical Reason* (1976), Marshall Sahlins is at pains to say that Marx ultimately agrees with him on certain fundamental matters. First, he shows that his and Marx's apparently opposed positions on culture and social structure can be found to have a common logical ancestor that is "cultural" by contrast with some "social structural" concept of equivalent generality. (In kinship terms, he and Marx have a common ancestor who had a sibling producing an entirely separate lineage.) But he also shows that Marx at times assumes a *cultural* position within his (Marx's) own local arena. That is, Marx has logical siblings relative to whom *he* seems cultural. Now these two demonstrations do not add up to a demonstration that Marx and Sahlins take fundamentally similar positions, as Sahlins implies. They simply show that indexicality allows one to find contexts that make the two positions *look* alike [see fig. 1.3].

This strategy fails only in the case of extremists who take the same side of a dichotomy at every level, a fact that explains why extremist positions prove so paradoxically defensible in intellectual life. An example is the work of certain economists, who at any level

15. Actually, the original phrase is "good to think" (*bonne à penser*). Like "play it, Sam," this locution has been improved by the oral tradition into "good to think with." The original is in Lévi-Strauss 1963b:89.

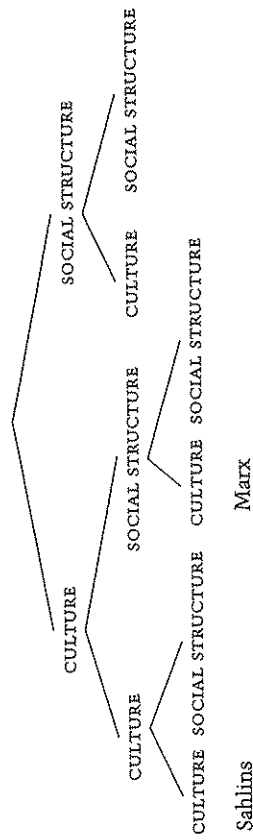


Figure 1.3

of argument unflinchingly choose to think in terms of choice rather than constraint. Any constraint they deconstruct as an aggregated outcome of prior choices. The immense internal consistency of such a position renders it unassailable by context-changing fractal arguments.¹⁶

At any given time, then, a fractal distinction profoundly shapes our understanding of our own and others' social science. On the one hand, it measures our similarities and differences no matter how great or small those may be. On the other it generates endless misunderstanding and provides a disturbingly powerful tool for nonsubstantive argument. All of these characteristics arise from the relational character of fractal distinctions, which makes them generate a clear local structure that replicates a hazy larger one.

Note that none of this presupposes any external agendas. It is true, of course, that most remarks about fundamental principles in the social sciences have more to do with establishing the lineage and power of the speaker than with clarifying those principles. It is also true that general statements about the position of particular disciplines within this mesh of dichotomies are usually concerned with disciplinary hegemony and resources rather than with knowledge of the social world. But most of the actual effects of these di-

16. There are in fact only two avenues for attacking such fractal consistency. The first emphasizes its cumbersome character. So, for example, if we want to explain how people achieve Nobel Prizes in economics, constraint just offers a much simpler approach than choice. The most useful fact in predicting who gets the Nobel Prize is not knowing who made which career choices, but rather knowing that there is only one prize for perhaps every five thousand economists. The second attacking argument involves *reductio ad extremum*. Thus, one challenges a consistent choice position; for example, by asking whether all suffering necessarily reflects the choices of those who suffer. Or, one challenges an absolute denier of objective knowledge by asking whether there is no criterion for calling Nazi eugenics false science. Or one challenges a consistent narrativist position by asking the narrativist whether he believes in the absolute uniqueness of all events, and, if so, how he believes any form of general interpretation of society to be possible.

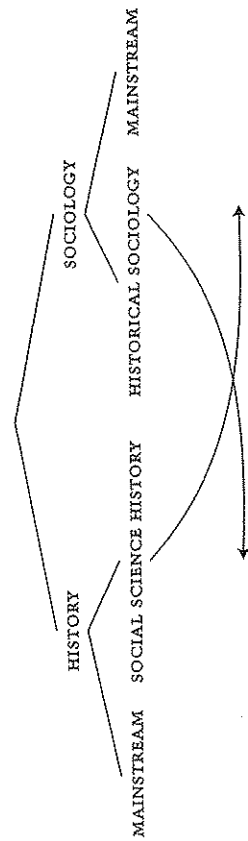


Figure 1.4

chotomies—both the complications created and the clarifications enabled—arise in the fractal character of their syntax rather than in the pragmatic uses to which they are put. External agendas invoke fractal distinctions for many purposes, but the distinctions' real power resides in their fractal, indexical nature, not in something outside.

This relational character makes fractal distinctions more general than linear scales, which seem superficially similar. For example, most of us would say that the distinction of history from sociology reflects the distinction of narrative from causal analysis. But within each discipline the fractal distinction is repeated, producing on the one hand mainstream history versus social science history and on the other historical sociology versus mainstream sociology. But social science history is closer to the mainstream of sociology than to that of history, and historical sociology to the mainstream of history than to that of sociology. That is, we cannot assume that the dichotomy of narrativism versus causalism simply produces a linear scale from pure narrativism to pure causalism, because the second-level distinctions produce in this case groups that have moved past each other on the scale (see fig. 1.4).

It is, however, wrong to emplot this particular example as a simple fractal distinction. For the two levels of the split reflect different kinds of structures. The first split—the disciplinary distinction of history and sociology—concerns how problems are posed. The subsequent distinction of social science history from other history, like that of historical sociology from other sociology, more concerns method.¹⁷ Moreover, this structural pattern arose because fractal distinctions in fact work themselves out in time, not simply at a

17. The best definition of social science history is thus as a field where narratively posed problems are addressed with causalist methods, while historical sociology is a field where causal problems (that is, general questions like when does revolution occur) are answered by (somewhat) narrative analyses of cases.

single point. The general distinction between history and sociology originated in the structuring of academic and professional associations in the late nineteenth century, while the particular realization of social science history came in the 1960s and that of historical sociology in the 1970s. We must therefore turn to the temporal structure of fractal distinctions.

3. Fractal Distinctions in Time

Once again, we begin with the interstitiality of social science in general and of sociology in particular. How might ideas change in an interstitial social science, a social science unwilling to let go of pure or practical reason, of objectivity or subjectivity, of analysis or narrative, positivism or interpretation?

Under a Hegelian model, any unified version of social science would call its opposite into existence, and the two would then be synthesized into a new social science transcending the earlier versions. This new version would then itself call forth a new opposition, another synthetic transcendence and so on. Such a progressive, dialectical model was at the heart of Gouldner's (1970) concept of a crisis in sociology. Parsons was the thesis, Marx the antithesis, and the welfare-state apologetics of "Academic Sociology" the synthesis. To this, "Reflexive Sociology" would be the new antithesis.

In retrospect, Gouldner seems very much ahead of his time. He defined Reflexive Sociology as the recognition that "the search for knowledge about social worlds is also contingent upon the knower's self-awareness." This statement could be taken straight from any contemporary postmodernist text. The problem is that it could also be taken from the homespun prose of Evans-Pritchard writing more than thirty years *before* Gouldner:

I wonder whether anthropologists always realize that they can be, and sometimes are, transformed by the people they are making a study of ... I learnt from African "primitives" much more than they learnt from me, much that I had never been taught at school.

Such continuous revival sounds less like Hegelian dialectic than rediscovers or renaming. Every now and then, we might think, social scientists recall that their ideas are as contingent on themselves as on their objects of study.¹⁸

18. Gouldner 1970:493; Evans-Pritchard 1976:245. As it was put by K. Burrige, a student of Nadel and Evans-Pritchard, in his retirement address:

The history of anthropology as I came to realize later, is not only a series of cycles, readdressing much the same problems under different names and idi-

The centrality of rediscovery is also evident in the litany of articles entitled "Bringing the Something-or-other Back In." Some ninety-one articles and books have brought something back in since George Homans first used the phrase in the title of his 1964 ASA presidential address, a virulent attack on Parsons for ignoring purposive action. And the things brought back in have included both sides of most of the important social scientific dichotomies. Some writers have brought people back in, others behavior. Some have brought social structure back in, others culture. Some have brought ourselves, others the context. Some circulation, others unions.¹⁹ Some capitalists, others workers. Some firms, others unions.¹⁹

oms . . . there was no question at the time of reinventing the wheel. It was a restatement in current idioms of discourse on what had, over time, become ritualized, opaque. Burrige 1989:92.

And lest we think that rediscovery itself has not been rediscovered many times, consider the celebrated lines of T. S. Eliot:

And what there is to conquer
By strength and submission, has already been discovered
Once or twice, or several times, by men whom one cannot hope
To emulate—but there is no competition—
There is only the fight to recover what has been lost
And found and lost again and again; and now, under conditions
That seem unpropitious.

"East Coker," 182–88 [from Eliot 1943:31].

Gans 1992 discusses a number of works on rediscovery, especially Sorokin 1956.

19. It should not be thought that forgetting and reinvention happen only with respect to subject matters. Several colleagues have pointed out the reinvention of methodological techniques. Path analysis is the most famous of these (really more a rediscovery, of Wright's work by Wald and Simon), but one could also include the reinvention of AID (the automatic interaction detector) as CART (classification and regression trees). Game theory the current vogue in economics and political science, had an earlier vogue (in the 1950s and 1960s) in social psychology.

It should also be noted that fractal divisions occur even deep within purely methodological communities. Much of the Bayesian versus frequentist debate in statistics actually encapsulates, within that highly technical field, much broader conflicts between subjective and objective approaches to social reality. When the frequentist Sir David Cox, in speaking of the work of Bayesian Adrian Raftery, concedes, "Integrating is more appealing than maximizing . . . that's the Bayesian point of view" or later "Bias is a relative term, is it not?" we are clearly replicating a debate in which most outsiders would see all statisticians as located absolutely on the positivist pole. [Both quotes from spoken remarks at a conference on Bayesian analysis at Nuffield College, Oxford University, 22 June 1999.] Also perpetually rediscoverable are theoretical paradigms. Think how many times "institutionalism" has been discovered, reinvented, or otherwise brought back in during this century, from Commons to Williamson in economics, from Selznick to Meyer in sociology, from Ely to Skowronek in political science, and so on.

A similar pattern of fractal cultural structure has been discussed by Gal and Irvine (1995). They are more concerned to see the fractal system as used pragmatically, with political intent, hence their process of "erasure." Although I mention the pragmatic use of fractals here (in the Sablins and Marx example, above), I return to the topic at more length in chapter 7.

A glance at these articles makes one think that sociology, and indeed social science more generally, consists mainly of rediscovering the wheel. A generation triumphs over its elders, then calmly resurrects their ideas, pretending all the while to advance the cause of knowledge. Revolutionaries defeat reactionaries; each generation plays first the one role, then the other.

A similar rediscovery seems to underlie the history of "social construction." The insight that social reality is produced by practice rather than given *ex ante* has made at least four separate appearances in this century's social science: first in the pragmatism of Dewey and Mead, then in the relational Marxist epistemology of Mannheim, then in the strong constructionism of existentialism and phenomenology, and finally in recent theoretical work from France. There are different wrinkles to these appearances, and of course there is a new terminology in each case. But there is no real progress, no fundamentally new concept. We simply keep recalling a good idea.

But to see here the simple harmonic motion of a pendulum is to miss the importance of the history that does occur. For example, a quite different process appears in sociology's great dispute between "conflict" and "consensus." The "conflict theorists" of the 1960s and 1970s asked why there was so much social conflict. They took individuals as inherently orderly and attributed conflict to oppressive social institutions. They labeled their opponents of the older generation as "consensus theorists." The people so labeled did not see *themselves* as arguing a particular viewpoint, but rather thought themselves an eclectic mainstream. Yet most of their arguments did seem particular when viewed in the new context provided by conflict theory. For the mainstream had asked why there was not *more* social conflict. For them social life was a Hobbesian free-for-all precariously ordered by normative institutions of social control. This was indeed the reverse of the conflict position.

There is little question that conflict theory won the day, not only by sheer youth, but also in direct combat. So as the 1970s passed, the dichotomy really wasn't a dichotomy any more. Only the conflict lineage survived. Yet by the mid-1980s, the younger generation was rediscovering the centrifugal tendencies of individuals and the problem of order amid diversity, the same things that had obsessed the now-defunct consensus theorists. With this rediscovery came a renewed defense of normativism. But now the politics were reversed. Group norms (as a concept) were now being defended by the "conflict" school (now under the leadership of the baby boom Marxists) against a new opponent. This was rational choice theory—

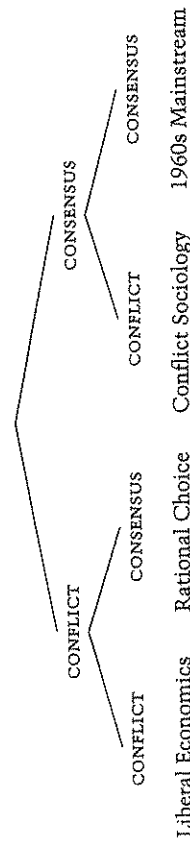


Figure 1.5

again a view that individuals make a good social world unless disorderly institutions somehow mislead them and thus itself a "conflict theory" (see fig. 1.5).

This is a particularly nice example. The death of consensus theory in sociology left 1970s conflict sociology (broadly defined) as the sole representative of a much larger lineage that was *consensualist* by contrast with the conflict theory presumed by economics. More generally, the triumph of a position in intellectual life usually guarantees that position's downfall by placing it in a new context of fractal comparison. Indexicality works through time.

This perpetual recontextualization forces each newly triumphant position to recognize that it has omitted central matters of concern or that, like sociological conflict theory, it is itself now representing what it thought it had defeated. Another consequence of rediscovery in real time is that there is an extraordinarily complex history of terminologies. For the old ideas return under new names.

The term "social construction of reality" was created, for example, by the third of the four generations of constructionists. They created the label to underscore what Mannheim had only implied under his theory of relational knowledge. Moreover, the third generation (Berger and Luckmann) used the phrase "symbolic universes" to refer to what would now be called "discourses," thus pointing toward the philosophical literature (e.g., Mead and Cassirer) where the present term points toward literary studies. The different contextualizations make the one concept look quite different.

But finally and perhaps most importantly, any temporarily victorious pole of a dichotomy must comprehend subject matters that had been more comfortably comprehended by its erstwhile opponent. If sociology (or social science, for that matter) is not to relapse into the pure humanities or the pure natural sciences—if it is not to lose its interstitiality—then defeating one's enemies means taking up their burdens.²⁰ Thus, it has now been elegantly shown that

20. In fact the humanities and natural sciences are split like this as well. The problem is simply particularly evident in the social sciences.

occupational categories are socially constructed by census makers, often with implicit political agendas—making women's work invisible, for example.²¹ But since unemployment and job loss are still of burning scholarly and political interest, the victory of a constructionist approach to occupations does not mean that we can stop studying job mobility, but rather that we must now somehow restructure our realist analyses with the appropriate constructionist doubts. This is a difficult task indeed, but we must do it, lest the constructionist insight simply lead us away from any politically effective analysis of unemployment and job loss.

This necessity of ruling an alien turf places both long- and short-run constraints on the extent of victory in the contests that unfold in these fractally proliferating lineages. In the long run, it forbids complete victory by one side or the other. For a truly universal predicate is inherently uninteresting, even meaningless. If "everything is discourse, because everything is mediated through language," then language is neither interesting nor consequential. It cannot explain the differences that interest us in social life, for it does not explain where differences originate, but only the means by which preexisting differences cause later ones.

In the short run, the takeover of alien turf means a reshaping and restructuring of the victorious terminology itself. Calling mathematics rhetoric brings great insights as long as we actually think rhetoric denotes something quite different from our ordinary concept of mathematics. As long as we think rhetoric means artifice dominating substance, persuasion dominating logic, and so on, it is possible to say quite insightful things about the "rhetorical" nature of mathematical arguments—about the hidden persuasions in an apparently objective language, about the nature of the taken-for-granted, and so on. But once we believe mathematics really is rhetoric pure and simple, metaphor becomes denotation and its foundation crumbles.

The remapping of alien turf into one's own terminology can take several forms. The simplest is takeover; mathematics, in the example just given, can become recognized simply as one subdivision of rhetoric rather than as a subdivision of something different called, say, objective argument. But meaning is a two-way relation. Concept denotes content but at the same time contents define concept. So the ingestion on equal terms of a mass of alien material simply destroys an overarching concept that much the faster.²²

21. Sources on occupational classification include Conk 1980, Szreter 1993, and Desrosières and Thévenot 1988.

22. Of course, this inference assumes that the contents under a concept have a certain stickiness (although not an "objective reality") that makes them resist re-

Therefore, far more likely is what I would like to call the "New York" form of ingestion, recalling the well-known "New Yorker's Map of the United States," half of which is taken up with Manhattan, another quarter with New Jersey, and then the remainder with America west of the Delaware. The New York ingestion is the diachronic analogue of what we noted above: the tendency to know one's close relatives well and one's distant relatives not at all. In it, an opponent's turf is absorbed but vastly reduced in relative size. The concept of attitude provides a good example. For W. I. Thomas and his school in the 1910s, "attitude" was a large and complex concept; interpreting the attitudes of Polish peasants took five volumes. But after positivism triumphed in postwar sociology, attitudes—now redefined as simple answers to certain kinds of questions—became merely one among many causal forces determining behavior. They became one more personal attribute alongside sex, race, religion, socioeconomic status, income, and so on. The positivists' Manhattan—the demographic variables—remained much the larger part of the map.

There are undoubtedly other modes of ingestion. All require bringing the conceptual and substantive knowledge of the defeated side of a dichotomy under the victorious one. All therefore tend to undermine the consistency and clarity of the victor; indeed all conduce to reproducing the old dichotomy under the new heading. The fact that sociology cannot effectively rule out forms or areas of social knowledge thus perpetually undermines hegemonic discourses within the discipline. Every triumphant discourse finds itself having to take up the problems, and implicitly the language, of the defeated.

This process continuously creates new terms for old things. As "culture" has become the dominant trope for discussion of social life, what used to be called solidarity (with distinct echoes of social structure) is now called identity (with equally distinct echoes of culture).

The consequences of this sliding for graduate education can easily be imagined. Faculty and students look to fundamentally different sources and assign different meanings to any given technical term. My generation footnotes the constructionist position to Berger and Luckmann. Our elders attribute it to Mannheim or, if students of Blumer, to pragmatists like Mead. But many of our current

definitions, of whatever kind or degree. I am quite willing to assume, on empirical grounds, that ideas about society (and indeed social reality itself) have that sticky quality although they certainly do not have the immobility implied in the phrase "objective reality."

students are convinced that social constructionism was invented by Foucault and the feminists and that all of preexisting sociology from Mead and Thomas to Mannheim to Berger and Luckmann was an exercise in hegemonic objectivist discourse. They talk about construction with different labels, and when they do use words familiar to the middle generation, they use them in different senses.

The word "culture" shows this process with painful clarity. A generation ago the word received a clear and limited definition from Geertz ("culture is a system of symbols by means of which people communicate their knowledge of and attitudes towards life") that presupposed the opposition of culture and social structure or of culture and behavior. Today's students use the word in a much broader form, embracing most of what is called social structure by my generation. But of course Geertz's position itself changed the situation that preceded it. After all, the contentious history of the culture concept in the nineteenth and early twentieth centuries preoccupied Kroeber and Kluckhohn (in 1952) for an entire book.²³

A fractal distinction thus produces both change and stability. Any given group is always splitting up over some fractal distinction. But dominance by one pole of the distinction requires that pole to carry on the analytic work of the other, so the endless subdivision that we label by the word differentiation does not seem possible. There results a continuous bending of terminologies that breaks down the original metaphors that produced dominance.

4. Mechanisms

This pattern of split, conflict, and ingestion needs more detailed analysis. As I just noted, the most familiar comparable temporal process is differentiation. Indeed, under the guise of "branching processes," differentiation is often taken as a general model for action in time.

When increasing size allows, differentiation does indeed sometimes occur in social sciences. For example, market research began within sociology as Lazarfeld and others used their new variable-based methods to predict consumption behavior. But once established as a separate body of work and workers, market research itself then split (in the 1960s) into market research proper—the quasi-sociological analysis of consumption with formal methods—and a looser study of general strategies for distributing and "positioning"

23. See Geertz 1973:89.

consumption products, a study that eventually merged with marketing more generally.

In such a case, a social structural differentiation recapitulates or parallels a cultural one. And this particular form of differentiation, along the lines of "purity" is quite general to knowledge-based occupations. Specialists in knowledge tend to withdraw into pure work because the complexity of the thing known eventually tends to get in the way of the knowledge system itself. So the object of knowledge is gradually disregarded.²⁴ This process is familiar throughout the professions, where applied work ranks below academic work because the complexities of professional practice make practical knowledge messy and "unprofessional." But we see the process in academia itself; much of the very high-status discipline of economics is quite unconcerned with empirical reality. Similarly, it was the gradual withdrawal of sociology and anthropology into inward, professional concerns that left the terrain of general social commentary open to humanists, who have invaded it with vigor and insight, if not always accuracy and intelligibility.

But differentiation survives within a fractal lineage only when increasing size and resources permit it. To see exactly what this means, we need to be more careful about definitions. Figure 1.6 shows three patterns I want to distinguish. The first is traditional differentiation. At each generation, a lineage splits into subordinate parts of increasing specificity. The second pattern is fractal differentiation, which is the simple version of the phenomenon discussed here. In this pattern the fractal distinction repeats itself at each succeeding generation *within all lineages*.

The third pattern is that of "fractal cycles," in which only one line divides per generation, because intense conflict exterminates all but a particular hegemonic view. However, the concerns of the "sterile" line are "remapped" onto a version of the fertile one; this is the "taking up of the concerns of the defeated" discussed above. The fractal cycles pattern is thus a subset of the fractal differentiation one.

We can see the difference between fractal differentiation and fractal cycles by looking at sociology itself. The great growth of sociology during the 1950s and 1960s allowed rapid fractal (and traditional) differentiation until the 1970s and 1980s. But while most academic disciplines expanded greatly in the postwar period, they

24. I first made this argument in Abbott 1981, and have an enduring fondness for it because it was my first major article. Luckily for me, it has recently been confirmed in resolutely technical fashion by Sandefur (2000). I shall return to this mechanism in chapter 5.

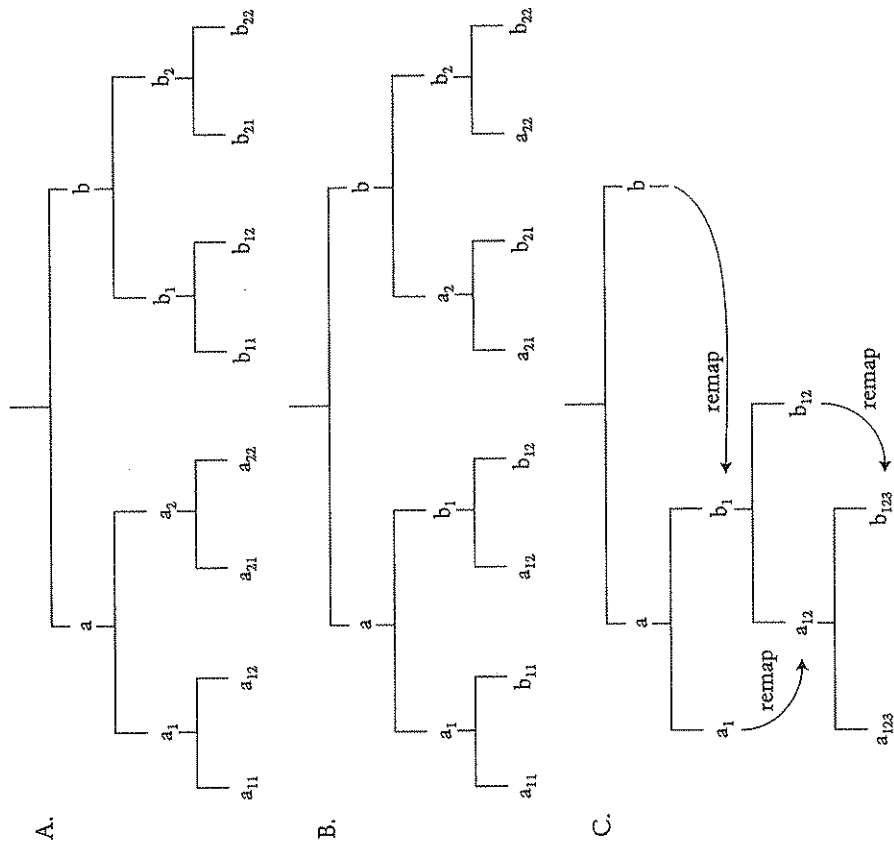


Figure 1.6

grew much more slowly in the periods before and since. Space is most clearly constrained in the elite cores of disciplines, where there is never much increase of crucial resources—of university positions, of space in elite journals, of visible venues at conferences, and so on.

It is therefore particularly at the center of disciplines that we see the ceaseless play of fractal distinctions that I outlined above. Its simplest form is the generational paradigm. By generational paradigm I mean a single step in the fractal cycle: an episode of conflict, defeat of one side, division of the winners, and remapping of the losers' concerns onto the equivalent descendant of the winners. The typical "victory" of a particular side of a fractal distinction seems to

last about twenty to thirty years. Social constructionism, as I noted earlier, reappears at about twenty-five-year intervals: at the turn of the century, in the 1930s, in the 1960s, and again in the 1980s. Academic Marxism first flowered in the early 1960s and was largely moribund—done in less by the end of the Cold War than by the rise of feminism and cultural studies—by the late 1980s. Labeling theory—a radical constructionist paradigm in the sociological study of deviance—was proposed in the late 1950s and dead by 1980.²⁵

There is good reason to expect a cycle of about this length. Twenty years is about the length of time it takes a group of academics to storm the ramparts, take the citadel, and settle down to the fruits of victory. There is a common pattern. First come insightful theoretical treatises and quirky but creative empirical work. Often, as with labeling theory, it is the quirky empirical work—in that case Erving Goffman's work on mental patients—that catches the disciplinary eye and imagination. And, as labeling theory also shows, there is no need for personal association between members of the new "school." It was really Howard Becker, writing after the fact, who named and thereby created the labeling "school."

The five years or so of "exciting new work" are followed by systematic treatises setting out the new point of view. Theoretical terms are temporarily stabilized in books with titles like *Keywords* or *The New Sociology of X*. The stream of empirical work swells. Now well into their assistant professorships, the new generation holds counter-plenary sessions at meetings, drawing the multitude from the elders' celebrations of old pieties. By the end of the first decade the new [sic] view has invaded survey articles and the more current of textbooks. Consolidation follows. By this point, the major theoretical work is done. So students are directed to produce more and more detailed empirical studies (complex comparisons, analyses of subproblems), which help anchor the paradigm. Results are often disappointing, however; somehow the excitement of *Asylums* or *The Making of the English Working Class* is never recaptured.

To be sure, there are always incentives to oppose the new argument. Because the orthodox hold much patronage, some young people defend orthodoxy. But the emphasis of the new school on one side of the fractal distinction inevitably forces orthodoxy's paladins into the other. Orthodoxy itself begins to seem the muddled eclecti-

25. Examples are endless and of course not limited to sociology. "Culture and personality" and "neo-Freudianism" [McLaughlin 1998a,b] are good examples from anthropology and psychology respectively.

cism of the middle-aged when compared with the clear logic of battle among the young. Thus the fractal reappears.

At this point, two directions are possible. The fractal debate can relapse into eclecticism as the combatants rise to disciplinary and university responsibility and slow their scholarly output. Alternatively, one or the other side of the fractal distinction can win. In that case, eclecticism arrives through the remapping mechanism noted above. Victory simply forces the victors to take over the turf of the losers, a seizure that in turn starts the victorious terminology sliding away from its one-sided purity.

My picture is of course oversimplified. The Young Turks often seek and often find allies among the elder generation. Yet even so the generational lines seem strong. In the 1970s and 1980s, Charles Tilly refused to become the sectarian historical sociologist the younger generation wanted him to be, taking a strong eclectic position that since all sociology was (or ought to be) historical, there was no point in developing a specific "historical sociology." This decision cost him the leadership of the new field, which went instead to the younger generation's Theda Skocpol. The core of most "new schools" usually does consist of a relatively narrow band of ages.

The fractal cycle indelibly marks careers that begin in its different phases. Students whose dissertations constitute the detailed empirical work of the later phases can find themselves advocating strong opposition when the cycle has already moved toward reconciliation and eclecticism. Leaders who articulated a fractal split as brilliant younger scholars are becalmed as the cycle's winds blow themselves out. The slide of meaning leaves little ground for the doctrinaire—or sometimes even for the consistent—although, to be sure, one way to survive is to put one's energy into staying continually abreast of the changing languages for the constant problems.

Generational paradigms are the simplest form of fractal cycle. Obviously, fractal cycles are sometimes longer than one generation. One mechanism lengthening them is what we might call fractionation. Marxism provides a useful example. Because the early voices of academic Marxism were utterly rejected by the academic mainstream, Marxists tended to speak more to themselves than to outsiders, with the inevitable consequence that fractal cycles began within the group rather than between it and the mainstream. The first American generation of academic Marxists—the corporate liberals of the early 1960s—was materialist, sometimes to the point of economic reductionism. But its successors were not. Some of them drew on the mixed materialism of Althusser and Poulantzas, where

historical changes in "determination in the last instance" allowed one to occasionally escape economic reductionism for other forms of material determination. Others turned to the more explicitly cultural Marxism of the English school, which at times escaped material determination altogether. Fractal squabbles among themselves in an explicitly Marxist arena thus stalled the larger fractal dynamic between the Marxists and the mainstream until academization brought Marxism into that mainstream. Once the serious academization of Marxism began, the larger dynamic took over again and Marxists joined, indeed often led, the large-scale social scientific swing toward the cultural and the immaterial that we observe today. But during the earlier period, fractionation reigned triumphant and the Marxists spent most of their time attacking themselves. Note that intense issues within fractionalized battles can seem incomprehensible to outsiders; most non-Marxists never took the position of extreme economic reductionism and hence had no particular need for Althusser's great insight, which took the form of "bringing the non-economic back in." The "discovery" of the problem of structure and agency is the same. Those who never believed in absolute structural determination wonder what all the fuss is about.

Fractionation thus produces fractal cycles of several generations. Other mechanisms lengthening the cycle usually involve interaction between multiple fractal distinctions, a topic I shall take up shortly.²⁶

The fractal cycle is at heart a profoundly traditional mechanism. Like any good ritual, it unites opposites. On the one hand, it generates perpetual change. Old ideas are always being thrown out. Intellectual autocracy is perpetually overthrown. On the other, it produces perpetual stability. The new ideas are always the old ideas under new labels. The new people are the old people in new roles. This last is of course the price of having our cake and eating it too. The wine of youthful revolution is usually followed by the hangover of rejection or stagnation in middle-age. But on the whole, the ritual is profoundly useful. We get to keep our best concepts forever and

26. An interesting example of a fractionation that failed is the neo-Freudianism of Sullivan, Horney, and Fromm, discussed by McLaughlin (1998b). The case of neo-Freudianism makes clear (by its counterexample) that one of the crucial foundations of the kind of fractal system I describe here is the underlying substrate of tenured positions, which ensure that no broad type of thought is ever completely lost. Fromm fell from grace not only because his ideas became more and more heretical, but because he lacked an institutional base of any kind. Most academic groups can always preserve some kind of minimal institutional base. For an interesting example of ongoing fractionation on a methodological basis, consider Leontief's (1982) remarks about mathematicization in economics.

yet can retain our belief in perpetual intellectual progress. We get the best of traditionalism with the best of modernity. Because our basic concepts are perpetually burnished by the complexities of re-mapping, the fractal cycle undermines the familiarity that breeds contempt or, worse yet, indifference.²⁷

Before going on to the question of interaction between fractal distinctions, I would like to underline the general power of the concept of fractal distinctions. The concept makes sense of many incomprehensible things about social science and its development. First of all, it explains the persistence of terms that appear to be undefinable despite their central importance to our disciplines. They survive because they are indexical terms that facilitate our discourse by their very indexicality. They give us a common if slippery language to establish relations between one another. They provide an extraordinarily powerful element for both offense and defense in academic discourse.

Moreover, the motion of fractal distinctions in time accounts well for the perpetual sliding of conceptual definitions in social science, as it does for the steady waxing and waning of new paradigms. It gives us a model for the perpetual rediscovery that seems so central to social science and shows us elegantly how groups can, through changing contexts of fractal comparison, find themselves defined publicly in ways precisely opposed to their self-images. Above all, it accounts for the ways in which the surface revolutions of social science articulate with an extraordinary constancy in basic concerns and shows us indeed how those emergent-level phenomena articulate, in ways not always pleasant, with individual lives.

27. There are, of course, other important mechanisms involved in the replication of academic knowledge over time. I defer a more general discussion until chapter 5. For example, a centrally important mechanism in creating drift in academic knowledge is the fact that academics generally do not try to teach what they learned, but what they wish they had learned. They most often forget to teach what they take for granted. Graduate students are left to learn that taken-for-granted from pure example. I should also note, more broadly, that there are many types of cultural change not considered here. Probably the most important, given my emphasis on the change of meaning in terminologies, is the situation where we don't think a terminology has changed in meaning when in fact it has done so radically. In my own work, the most familiar example of this is the word "profession," which meant something almost completely different in the nineteenth century than it does today. Again, Eliot puts it best:

For last year's words belong to last year's language
And next year's words await another voice.
"Little Gidding," 118-19 (from Eliot 1943:54).

The reader will have noticed that the underlying mechanism I propose here has much to do with academic careers and their structure. I will return to this topic in more detail in chapter 5.

5. Multiple Fractal Distinctions

I have so far pretended that the fractal distinctions that undergird social science exist independently. But of course, they do not. Indeed there are strong elective affinities between them. Thus, work on choice usually focuses on the individual level, while constraint is often seen as emergent. To be sure, these affiliations can be broken. When Gouldner wrote *The Coming Crisis*, Marxism had no positivist version, soon to be provided by Erik Wright, nor yet a choice version, soon to be provided by John Roemer. But in general affiliations among various poles of my fractal distinctions are strong and enduring.

Perhaps the strongest of these affinities is what we might call the methodological manifold: an affiliation of four or five separate distinctions generally labeled by the distinction of qualitative versus quantitative. At the heart of this manifold is the nearly absolute association of positivism with analysis and of narrative with interpretation. So strongly linked are these two dichotomies that the other way of lining them up—narrative with positivism and analysis with interpretation—is nearly nonsensical to most sociologists.²⁸ Beyond this initial identity, the affiliation of the two sides of the methodological manifold with particular dichotomies remains clear, if not so rigid. Analytic positivism is nearly always realist rather than constructionist in its epistemology, generally concerns social structure rather than culture, and usually has a strongly individual rather than emergent cast. Most of its proponents are strong believers in transcendent social knowledge. By contrast, narrative interpretation usually invokes culture (perhaps social structure as well), is willingly emergent, and nearly always follows a constructionist epistemology. Most members of this school—from Blumer to Foucault—believe social knowledge ultimately to be situated, not transcendent.

Quantitative	versus	Qualitative
POSITIVISM		INTERPRETATION
ANALYSIS		NARRATIVE
REALISM		CONSTRUCTIONISM
SOCIAL STRUCTURE		CULTURE
INDIVIDUAL LEVEL		EMERGENT LEVEL
TRANSCENDENT KNOWLEDGE		SITUATED KNOWLEDGE

28. That the associations of positivism with analysis and interpretation with narrative are nearly absolute was made very clear by the reaction to my own 1992 paper "From Causes to Events," subtitled "Notes on Narrative Positivism." Within a short

Breaking these affinities is the most powerful mechanism for knowledge change in social science. Interesting new social science can always be produced by trying a combination hitherto unknown. Hence the appeal of positivist and choice-based Marxist theories, and indeed of the quasi-positivist studies of culture now emerging (e.g., the work of John Mohr). But these, however interesting in themselves, are merely static variety—filling in the holes of a cross-tabulation. Changing these affinities provides a far more powerful mechanism of intellectual change when taken in the context of the fractal cycle. To argue this and to move toward a more general formulation of the argument, let me develop an extended metaphor.

Imagine that we are tourists in a rectangular-grid city. The bus leaves us at a big park in the middle of town. Imagine that one fractal distinction is east versus west. Suppose we choose one of these directions, walk a mile, then stop and choose again, walk half a mile, then stop and choose again, walk a quarter of a mile, and so on. At each choice, half of us go one way and half the other. The first four moves in the procedure are shown in figure 1.7. Eventually, we will end up uniformly spread out on an east-west axis four miles long. By this procedure, that is, our group can discover what is happening anywhere on this axis. The fractal distinction fills the (one-dimensional) space well. Note too the important fact that this rule of motion enables us to achieve that coverage without any initial specification of who goes where. That is, the rule works *precisely because* it is an indexical rule embedded in time.

But now suppose that north versus south is a fractal distinction as well. Now people must walk north or south after walking east or west. Suppose we say that whoever walks east must next walk south, and whoever walks west must next walk north. (This constitutes "affiliating the dichotomies," as in the methodological manifolds discussed above.) We start out choosing east or west, walk a mile, then walk a mile south if we came east, and north if we came west; then we stop and choose east or west, walk half a mile, then walk half a mile south if we came east, and north if we came west, and so on. Again, at each choice point half go one way and half the other. As figure 1.8 shows, this rule spreads us out along the line of the southeast-northwest diagonal. If people started north or south with the same east-west affiliates, they would end up on the same diagonal by a different route. By affiliating the dichotomies, that is, we still know only a one-dimensional fragment of a two-

time of its publication I was being shelved accurately by both positivists and narrativists. See Hanagan and Tilly 1996 and Abbott 1996.

filled) can be achieved by a multitude of quite different sets of rules, nor is there any guarantee that equal amounts of knowledge will be knowledge of the same things. That is, there are many different ways to fill 50 percent of the space. We might fill all the right-hand half, or the right half of every quadrant, etc. Note that these different ways would differ *vastly* in the degree to which all parts of the space were "nearly known," that is, within a certain distance of a known area.²⁹

This model of social scientific knowledge is thus not progressive in the usual sense, although it does admit the loose criterion that "better" knowledge is knowledge that fills the space more completely, knowledge of higher fractal dimension. But at the same time, in the concept of "changing the rules for compounding fractal distinctions" we have a clear model for "scientific revolutions," which often have the properties just noted. They make us know the same things in different ways, and their new knowledge seems to be in some way incommensurable with the old, precisely because it is achieved by a different route. Thus I argue that the major changes in sociology, as in other social sciences and among the social sciences in general, arise through the reshuffling of these affiliations between the fractal distinctions, within the context of individual fractal cycles. And I note that this is not only a descriptive statement; this is not only how social science works in practice. It is also a prescriptive one urging us to recast the compounding rules and giving us at least a conceptual criterion for how to judge the results of that recasting.

It should now be clearer what I meant at the outset by a form of universal knowledge that emerges from accommodation and conflict rather than from axioms. When there are many different epistemological routes to one place, people who have taken them will "see" a different thing when they arrive. What is universal about social science knowledge is the project of getting there and of mutually decoding our routes. This project is all the more complicated once we recognize that, as the concept of fractal cycles implies, most of us have been wandering in the city for long enough to have

29. Readers with knowledge of fractals will recognize that this question of "rules for compounding distinctions" is the question of changing the shape of the fractal with which we try to fill a space. A fractal is always of some lower dimensionality than the space it is trying to fill. But different fractals fill different proportions of the full dimensional space. There are various criteria for thinking about this "filling." In the current example, we might want to think a set of rules for compounding fractal distinctions better if it enabled us to guarantee that the maximum distance from any point in the "city" to an existing form of social scientific knowledge was minimized. I return to this problem in the closing pages of the book.

lost any sense of where we started. Axiomatic retracing of our routes from the central square is impossible; none of us knows where it is. We run into each other on some street corner, our disciplines and subdisciplines having brought us there by varying routes, and we try to locate one another's past and present by discussing our rules for traversing the labyrinth of our fundamental conceptual structure. All the worse, the endless fractal cycles are perpetually changing the meanings of our very languages. But once in a while something remarkable occurs. Somebody learns enough from somebody else to wander into a whole new area of the city.

The next three essays elaborate this argument through a detailed analysis of historical examples of fractal patterns in sociology and in related social sciences.