Chapter Six
FRAC TAL HEURISTICS
(first part)

(This chapter has been divided into two parts. The first part includes sections I-IV, the second, sections V-IX)

I. POSITIVISM AND INTERPRETIVISM
II. ANALYSIS AND NARRATION
III. BEHAVIORISM AND CULTURALISM
IV. INDIVIDUALISM AND EMERGENTISM
V. REALISM AND CONSTRUCTIONISM
VI. CONTEXTUALISM AND NONCONTEXTUALISM
VII. CHOICE AND CONSTRAINT
VIII. CONFLICT AND CONSENSUS
IX. TRANSCIDENT AND SITUATED KNOWLEDGE

WE HAVE SO FAR SEEN three general types of heuristics. The simplest are additive rules for creating minor variations in ideas. The second are lists of generic topics and common notions that we can use as stimuli to point us in new directions. The third—the general heuristics of Chapters Four and Five—are more self-conscious devices for producing new ideas by manipulating arguments, descriptions, and narratives in particular ways.

In this chapter, I take up a fourth type of heuristic, one that arises in the “great debates” of the social sciences that I discussed in Chapter Two. It makes use of a particular quality of these debates, one that I noted briefly at the end of that chapter: their fractal nature. A fractal is simply something that looks the same no matter how close we get to it. A famous fractal is the woodland fern, each of whose fronds is a little fern made of leaves that are actually little ferns made up of tinier ferns, and so on.

The great debates I discussed in Chapter Two are fractals in the sense that they seem to be important debates no matter what the level of investigation at which we take them up. Take the famous opposition of realists and constructionists. Realists think social reality is a real thing, fixed and repeatable. Constructionists don’t. Constructionists think the actors and meanings of social life are made up as we go along, by playing with past repertoires. Realists don’t.

Now most sociologists have a pretty clear idea of who the realists are and who the constructionists are. Survey analysts are usually thought to be realists and historical sociologists to be constructionists. Stratification scholars are usually realists; sociologists of science are constructionists—and so on. But suppose we take some sociologists of science and isolate them somehow. Sure enough, they will start to argue internally over precisely this issue of realism and constructionism. Some will argue that science is a given type of knowledge produced by a certain kind of social structure; the big issue is how that knowledge is shaped by larger social structures. Others will argue that you cannot understand what science itself is until you understand the actual flow of the daily language that scientists use to build the scientific knowledge that gets rationalized in textbooks. That is, the two groups will fall into violent debates
over precisely the issue of realism versus constructionism even though the rest of the discipline regards them all as strong constructionists. (This is, in fact, exactly what happened in the sociology of science in the 1980s, when the field had a kind of “I think more things are socially constructed than you do” contest that ended up with the whole field pretending, somewhat nervously, that it didn’t believe in the reality of anything at all.)

To take an example on the other side of the discipline, the sociology of crime was for a long time one of the strong realist fields in sociology. Crime statistics have a long history in American public life, and few events seemed more obviously real than an arrest. But in the 1950s, there emerged within this highly realist literature a constructionist critique. This “labeling theory” argued that there was something more to becoming a criminal than simply doing the act; you had to get caught, detained, held, charged, convicted, and sentenced. Many people slipped away at each step along the way, yet only at the end did you really become a labeled “criminal.” The labelers insisted that the long-observed inverse correlation between social status and criminality happened because lower-class offenders were more likely to make it through the long process that leads from act to conviction. Criminality was not a simple, real fact but a complex, constructed one.

Meanwhile, there was also a similar but smaller debate within the purely realist group of criminologists. These realists were in an uproar because of the unreliability of arrest statistics. Chicago’s crime rate rose 83 percent in one year (1962), and everyone knew that reality had not changed but reporting procedures had. So a vociferous group argued that arrest statistics were arbitrarily constructed and crime should be measured by surveys of victims, not by counts of offenders. And in setting up the victimization surveys, dozens of realist/constructionist questions were asked: Is a series of harassing acts one event or many? Do closed-form survey questions necessarily coerce respondents to follow a certain pattern? When is a question to be considered “suggestive”? These were all the same debates that the sociologists of science were to have in the 1980s, but they were located in a community that the discipline widely regarded as realist.

As this example shows, the central social scientific debates of Chapter Two are fractal in nature. No matter how large or how small the community of social scientists we consider, most of these issues will be debated within it, even if we think that the community already represents one extreme or the other on the issue. By itself, that is just a curious fact. But this curious fact means that we can use the basic debates as heuristic tools. Wherever we find ourselves with respect to the complex arrangement of forms of knowledge that is social science, we can always use these fractal heuristics to produce new questions and new problems.

A simple example of this comes from the literature on anxiety and stress. How are we to explain stress? Who suffers most? What can stop it or mediate it? The literature investigating these questions from the 1960s through the 1980s was strongly positivist. But what is most noticeable to an outsider reading the
stress literature is that whenever the positivist researchers came up against a blank wall, they would develop narratives and reinterpretations of data that would open new research vistas for them. Thus, the original literature looked only at the correlation between stressors (unusual events) and distress (unhappiness). When those correlations proved to be weak, researchers started to think about “coping,” defined as a mediating phenomenon on the path (that is, in the narrative) from stressors to distress. Differences in coping skills and resources would account for the weak simple correlation between stressors and distress: better copers would suffer less from a given number of stressors than weaker copers would. When these coping variables proved weak in their turn, analysts started asking even more subtle interpretive questions, such as “[at what point does heavy drinking change from a coping strategy into a symptom?” (Kessler, Price, and Wortman 1985:552). Now the answer to this last question has been the subject of numerous famous novels (for example, Fitzgerald’s The Beautiful and Damned) and films (for example, The Days of Wine and Roses), which show well that there is nothing like an objective answer to it. But just thinking about it gave the stress positivists something new to do. They weren’t stuck any longer in their cul-de-sac with the lousy correlations. They had new questions to investigate. That is what I mean when I say that the main importance of the fractal debates may not be as organizing principles of the disciplines, but rather as heuristics for the disciplines. Indeed, I might even propose that the great debates had their first existence as heuristics and became general, organizing principles for how we view whole disciplines and methods only because so many kinds of people, believing so many substantive things, used them as heuristics. On this argument, it is their widespread use as heuristics that leads theorists of various disciplines to assemble all the locally different uses into what appear to be grand organizing debates. Here I’m just pulling one of my own heuristic tricks: reverse the direction of causation, and see if your argument is still credible! I’m not sure whether this argument holds—and this is not the place to evaluate it—but it’s an interesting possibility in the historical sociology of social science.

In summary, the great central debates of social science are themselves widely (if implicitly) used in a heuristic mode to open up new questions and possibilities. In this guise, they are as common a heuristic as any of the others I have examined. Like other heuristics, they can be greatly overused. And like other heuristics, they should not be taken to be the one, true nature of things. (That has been the problem with treating them as great debates.) But treated well, they will be a useful part of your heuristic armamentarium, good anywhere anytime.

I shall organize this chapter according to the nine basic debates discussed in Chapter Two. For each debate, I shall give a few examples, trying to show how each one can be used no matter what the method, no matter what the current definitions of the research. I do not give examples for both choices for each of my five methodological traditions. That would be 90 examples (2 choices × 5 methods × 9 debates), and you don’t want to read them all any more than I
want to go looking for them all. But I shall try to offer enough possibilities to give you a sense of the richness of fractal heuristics. And I shall emphasize moves that went against the grain: deep interpretivists who turn positivist, emergentists who try out individualism and so on. As before, I have tried to select papers that have had a strong influence on subsequent social science, although in some cases, I've been seduced by favorite recent work. I apologize in advance for the almost bewildering diversity of the examples, but that is part of the point; fractal heuristics are used throughout the social sciences in a bewildering variety of ways.

I. POSITIVISM AND INTERPRETIVISM

The first of the fractal debates is between positivism and interpretivism, between thinking you can and should measure social reality formally and thinking you can’t and shouldn’t. In fact, it is easy to find examples of positivistic and interpretive moves in nearly any methodological tradition. The two are engaged in an incessant dialogue. So in ethnography, sometimes our impulse is to count things (as William F. Whyte counted bowling scores in *Street Corner Society*), and sometimes our impulse is to delve into even more interpretive detail (as Mitchell Duneier does in discussing police busts in *Sidewalk*). In SCA-type analysis, the moves toward positivism are too numerous to count, but there are equally as many moves the other way, as I just noted in my discussion of the stress literature.

A particularly elegant example of an interpretivist move in positivist work is Richard Berk and Sarah Fenstermaker Berk’s influential article on models for the household division of labor (1978). Berk and Berk are attempting to evaluate the “new” home economics, with its theory of the household as a production system. They employ an extremely elaborate positivist design: a two-stage least squares operationalization of a structural equation model for a data set on the allocation of household tasks. But the article ends up in an interpretive discussion about the definition of “sharing” and “substitution” in household tasks. Noting the complex differences between husbands’ and wives’ effects on the household division of labor (changes in which tasks the wife does affect which tasks the husband does but not vice versa), the authors point out that husbands tend to participate jointly with other family members in production, and they provide quotations from respondents illustrating three different models for this “sharing”: “moral support,” “assistance,” and “supervised help.” These definitions of sharing have different implications for the substitution of the husbands’ effort for the wives’ effort and hence for the project of analyzing the family as a production system. Berk and Berk leave the reader wondering about the question of the exact trade-off between husbands’ and wives’ housework. In short, after all the rules are followed and all the regressions are run, the way out of a quantitative dilemma takes the form of reinterpreting a variable by anchoring it in a more complicated story with more ambiguous meanings. Thus, a positivist blind alley is escaped via an interpretive move.

It is equally important to note moves toward positivism in a place like historical analysis, where we least expect it. One example is the paper by V. O. Key on
critical elections (1955), one of the single most influential papers in political science in the twentieth century. Key’s paper removes elections from the one-by-one tell-a-story approach that had been common before his time. By analyzing detailed counts of votes in particular constituencies over many national elections in a row, he showed that in certain elections there were sudden realignments that then persisted for three or four elections thereafter. Key’s move might be seen as a form of temporal lumping; his argument was that the “event” of realignment was often bigger and more enduring than it seemed. But it is important to see that Key made his claim stand by taking a distinctly positivist turn in a literature that was until then given mostly to historical, discursive analysis. It was by getting analytic that Key made his mark.

A similar, much more recent move occurs in John Mohr’s brilliant analysis of images of poverty in nineteenth-century New York (Mohr and Duquenne 1997). Mohr sought to uncover New Yorkers’ images and concepts of poor and needy people. Rather than follow the normal strategy of critical analysis of texts about poverty, he fed the official descriptions of clienteles from dozens of New York social service agencies into a computer. He then analyzed these official descriptions by saying that two types of needy people were “close,” in the eyes of New Yorkers, if they appeared in the same descriptions together. Once he had calculated the “closeness” of all possible pairs of types, he could use clustering and scaling methods that turn such “distance” data into clusters and pictures. As a result, he produced an astoundingly comprehensive picture of poverty as it was envisioned by the very agencies that dealt with it. The move was a radically positivist one, but it revealed aspects of nineteenth-century theories of poverty that had never yet been conceived.

In short, we find that the positivist/interpretivist choice can be made by any kind of analyst at any point. Often, as in the cases mentioned here, the moves have their most decisive effect when they go against the expected direction. We don’t expect an interpretive move in SCA, and we don’t expect positivist moves in historical and cultural analysis. Therefore, the results of such moves seem all the more dazzling. In fact, either move is possible in any method at any point.

And at any level! One can easily envision moving on to the next level of detail and making either move with respect to any of these examples. Take critical elections. It is clear that one could get far more positivist than Key in evaluating the question of whether critical elections really do exist, and indeed there is a large literature since Key that has done just that. One can also imagine making the move to counting election results and to examining longer runs of elections, as Key did, but then insisting on a more interpretive form of analysis of the results. Key’s own rendition of the phenomenon, in the original paper, is completely demographic. He simply identifies the phenomenon in the voting patterns but makes no attempt to interpret it. Was it a result of new party ideologies? of new party organization? of legal changes in registration? Was it a downstream result of the changed immigration laws of 1924? of a new voting coalition in subgroups? of a change of heart by some major subgroup? The possibilities are many and immediately encourage a large interpretive, historical
literature, which did in fact emerge to try to explain the phenomenon Key had uncovered.

In short, not only is the pairing of positivist with interpretivist a heuristic pairing useful across all methods, but it also applies at any level in those methods. This pairing is truly a fractal heuristic. If your current thinking is blocked, one way to move ahead is to use it to sidestep the blockage and open up new research problems and opportunities.

II. ANALYSIS AND NARRATION

Like positivism and interpretivism, the pairing of analysis and narration is used throughout social science as a fractal heuristic. Sometimes we need to follow a story through as a story, sometimes we need to break it into bits and compare the bits, but no matter what the method or the level, the switch between narration and analysis is always available and often used.

In the first instance, this switch can be seen as simply the narrative heuristic of freezing or setting in motion, discussed in the preceding chapter. For example, in one of the most influential theoretical papers of gender literature, Candace West and Don Zimmerman argued that “gender is not a set of traits, nor a variable, nor a role, but the product of social doings of some sort” (1987:129). That is, gender is a performance, a process of making certain gestures and invoking certain symbols in certain contexts with the intent of pointing to oneself as gendered. The insistence that gender is not a fixed thing but an ongoing performance challenges gender research whatever the method it employs. In ethnography, it means forgetting about preexisting gender roles and watching how people mark gender distinctions over time. In SCA analysis like the Berk and Berk paper just mentioned, it means investigating trade-offs in housework over time rather than assuming that there are stable contributions of men and women. And so on.

But the analysis/narration heuristic move is often not just a matter of setting in motion or stopping but can be a specific move with respect to a particular current method. In ethnography, for example, the strong drift of the last twenty years has been toward much more narrative, temporal approaches. The new ethnography embeds its local events in larger narratives of culture contact (as in the work of Marshall Sahlins on Captain Cook in Hawaii [1985]), developing capitalism (as in the work of Michael Burawoy on de-skilling in American factories [1979]), globalization (as in Janet Salaff’s work on young girls in Hong Kong factories [1981]), or some other large-scale historical process. Even in anthropological linguistics, which relies far more on technical and analytic machinery than the rest of anthropology, the move toward more narrative methods has been marked. Indeed, the drift to narration is so strong that ethnography is ripe for an antihistorical move—perhaps based on an ahistorical theory like rational choice, perhaps based on a renewed insistence on local ethnographic validity.

The constant tug of war between narrative and analytic moves is even more evident if we consider not a particular methodological tradition but a general field of research. Studies of social class in modern societies are a good example. The great classic of mid-twentieth-century social-class studies was W. Lloyd
Warner’s immense “Yankee City” study of Newbury-port, Massachusetts, a kind of industrial-strength ethnography done by dozens of workers who talked to hundreds of people and evaluated their class status based on their language, furniture, place of residence, and many other things (Warner et al. 1963). The social-class concept Warner used in this work was highly analytic and static (Warner, Meeker, and Fells 1949). Not surprisingly, it was widely attacked by historians. While some of the attacks were, predictably, based on the simple “putting into motion” heuristic (that is, “Warner got it wrong because he took a snapshot when he should have watched the movie”), the most damaging was Stephan Thernstrom’s highly analytic study Poverty and Progress. Thernstrom traced individuals through manuscript census records, counted noses, and showed that there was far more class mobility than Warner had suspected.

From our point of view, Thernstrom made a narrative move in that he looked at the life histories of individuals rather than simply talking to all of the residents of Newburyport at one point. He made it in a very analytic way, however, in that he did not view people or seek detailed histories of individuals, but rather reduced their lives to coded sequences of the class statuses they successively held over time. This narrative move with an analytic accent contrasts strongly with the contemporary move by Blau and Duncan’s already discussed American Occupational Structure. These students of mobility—and indeed the whole tradition they stood in—conceived of the “narrative” of mobility as a jump from the static class status of a father to the static class status of a son. The move was analytic at nearly all levels, assuming away most of the lifetime change in the father’s class standing, most of the change in the prestige structure of occupations, and (as we have seen in an earlier chapter) all of the cross-individual variation in the “narrative” pattern of causes. All of this in order to make dramatic analytic comparisons.

To make so many analytic moves—moves away from narration—sounds worrisome, of course, but it is important to realize that a literature has to make such choices in order to move ahead. The sociological-mobility literature deliberately assumed away certain parts of the history in order to get at others. For example, the enormously influential paper of Robert Hodge, Paul Siegel, and Peter Rossi on the “history” of occupational prestige in the United States establishes that the occupational prestige ratings are stable over time (1966), a crucial element in the structural view taken by the Blau and Duncan book and most later sociological study of mobility. But the Hodge, Siegel, and Rossi paper accomplished this by assuming that there were no changes in the nature of occupations themselves between 1925 and 1963. That assumption was necessary, of course, if we were to think that people were rating the prestige of the same things throughout the period. But in fact, the identities of occupations like secretary and bookkeeper changed almost completely in that period. Ignoring that change—at least for a while—was the price that had to be paid. Only by assuming away some parts of a narrative can you open other parts to analysis.

Before leaving the fractal heuristic of analysis/narration, we should consider some examples of studies that move with the grain rather than against it, stud-
ies that are already highly analytic but make a decisive move to become even more so or studies that are already narrative but make further narrative moves. The reader should not think that against the grain is the only possibility.

An example of narrative analysis that deepens itself by moving to an even more complex narrative level is Goran Therborn’s influential paper on “The Rule of Capital and the Rise of Democracy.” Therborn’s paper considers one of the classic narrative problems—the rise of democracy—by comparing (in capsule form) the histories of two dozen modern democracies. His argument starts where Barrington Moore’s *Social Origins of Dictatorship and Democracy* leaves off, with the notion that the rise of democracy is a complex and contingent process, not the result of a single variable or constellation of variables, as it appeared to be in the much more analytic work of Seymour Martin Lipset and others. But Therborn insists that prior narrative analyses have left out another narrative essentially related to that of democratization: participation in or threat of foreign war. He makes a strong case that war or its threat was central in forcing bourgeois states to spread access to power and authority more broadly throughout their populations. He thus took what was already a comparison of complex historical narratives and made them even more complex. (Note that his move was not simply to introduce a single variable of war but rather to look at the different roles different wars played in each of the historical trajectories he examined.)

An even more striking example (but in the other direction) is John Muth’s “Rational Expectations and the Theory of Price Movements, a paper that lay unnoticed for a decade, until Robert Lucas and others fashioned from its kernel a theory that transformed our view of government intervention in the economy. Muth, an economist, makes a strongly analytic move in a tradition of research that is already highly analytic; not only are economic actors “rational maximizers” at the first level, he says, but they in fact act the way economists would. The paper starts with a purely formal analysis of an economy in which producers are predicting the prices they will be able to get for their goods in future time intervals. It specifically attacks Herbert Simon’s hypothesis of “bounded rationality” (discussed in Chapter Four; see Simon 1982):

> It is sometimes argued that the assumption of rationality in economics leads to theories inconsistent with, or inadequate to explain, observed phenomena, especially changes over time….Our hypothesis is based on exactly the opposite point of view: that dynamic economic models do not assume enough rationality. (Muth 1961:3 16; emphasis added)

Muth’s argument is essentially that if there were a substantial and predictable difference between firms’ expectations and the behavior of the market, someone would have been able to create a firm or a speculation taking advantage of it. On the general economic assumption that people are rational, someone would therefore have done that (if it were possible), and therefore we are safe in assuming that prices as they currently exist reveal all such predictions about the future, including secret speculative ones. For if secret speculative reward exists, then someone has taken advantage of it and hence removed the
possibility from the market. Muth’s argument was later used to attack Keynesian management of the economy. Since government fiscal policy was a matter of public record, it was argued, speculators would take advantage of any difference between government-supported prices and “real market” prices, in the process canceling the effects of government intervention.

Our interest here is less in the policy implications of Muth’s celebrated article than in its seemingly extremist insistence that an already absolutely analytic literature become even more analytic. Effectively, the Muth paper assumed that at least at the level of expectations, firms (as a group) were as good at predicting the future as were economists. As Muth himself pointed out, this was quite close to “stating that the marginal revenue product of economics is zero” (1961:316). Not only were economists analytic, but they also might as well assume that the firms they studied were as analytic as they. This extraordinary assumption produced two or three decades of exciting research before the rational-expectations hypothesis was finally deserted for newer, more exciting ideas.

Thus the analysis/narration debate also functions as a fractal distinction. We should note, however, that the order in which one takes narrative or analytic turns makes a big difference. Taking a narrative turn after an analytic one does not get you to the same place as taking an analytic turn after a narrative one.

A good illustration of this comes from the story of my own borrowing of optimal-matching methods from biology, mentioned in Chapter Four. I did this borrowing because I had decided that it was important to think about the full sequences of people’s careers rather than just each separate instance of employment and occupation. That is, I made a narrative turn first, toward treating the full sequence of someone’s work life as important. My next turn was analytic; I realized that I could compare careers by employing the sequencing-comparison algorithms that were used to compare strands of DNA. The algorithms would create “distances” between careers, and I could then classify them, using the usual array of pattern search methods.

By comparison, if when we study workers, we make the analytic turn first, we inevitably think of individual episodes of particular workers’ being employed to do particular things at a given moment. This in turn leads to thinking in terms of labor markets, where these worker-job units are transacted. If we then make a narrative move and start to ask about the changing nature of some particular labor market, we are seeing a different set of things than are visible using the methods I developed. We don’t have a continuous set of people but rather a continuous set of transactions. The questions of interest aren’t patterns in people’s careers but rather the historical developments of a general labor market: changes in likelihood of hiring, changes in hiring firms, changes in types of individuals hired, and so on.

Note that both sets of questions are interesting. It is not that one set is the right set and one the wrong. Rather, they’re both interesting and important questions, but for different reasons to different people with respect to different
theories. The example shows that the order in which you invoke fractal heuristics has a big impact on where you end up.

III. BEHAVIORISM AND CULTURALISM

With the heuristic involving behaviorism and culturalism, we move away from debates about forms of analysis to the heuristics drawing on differences in how we think about the ontology of social life—the elements and processes that we imagine make up the world. In this first case, the issue is whether we focus on social structure or on culture, on observable behavior or on meaning.

One of the best examples of this heuristic I have already given: Howard Becker’s magnificent paper on marijuana use. I used this as an example of making a reversal in Chapter Four. The reversal Becker made involved just this heuristic. Rather than assuming that attitudes precede behavior, as is more or less standard, Becker argued that behavior produces attitudes. He was playing with our sense of the relation between behavior and meaning.

A useful way to see the fractal character of this contrast is to look at two influential papers, both in a single methodological tradition (SCA), one of which takes a behaviorist turn and one a cultural turn. We normally think of the SCA tradition of methods as largely behaviorist, unconcerned with the meanings of things, but even within that framework it is possible to move in either direction. As it happens, both of these papers consider the application of economic ideas to family life. In one that application is part of the hypothesis, while in the other it is something to be explained.

First, a move toward behavior. George Farkas’s “Education, Wage Rates, and the Division of Labor between Husband and Wife” was one of the first papers to look directly at the family-division-of-labor question with strong modern data. Not surprisingly, it has been very influential. It is a model of social science, with excellent data and effective analysis and, perhaps more important, with clear alternative hypotheses to which the author gives equal attention. Farkas aims to test three basic theories about the household division of labor: the economist’s wage rate view that couples seek to maximize total household utility and hence adjust their division of labor to the relative ability of husband and wife to make money outside the household; the “subcultural” theory that middle- and upper-class husbands and wives are more likely to accept women’s work outside the home; and the “relative resources argument that relative differences in education (not available wages outside the home) drive the division of labor.

What is behaviorist about the paper is its insistence that we examine not attitudes about the household division of labor but actual performance. Hence, the dependent variables are the wife’s annual work outside the home and the husband’s reported hours of housework. Most earlier work on households was based on ethnographic or interview-based research that gave less attention to behavior than to attitudes. Indeed, it was clear from the earlier research that those attitudes took the form implied in the relative resources and subcultural hypotheses. What was not known was whether behavior did as well. Did upper- and middle-class households just talk a good line, or did they live it? It was easy to suspect that couples might talk a more egalitarian line than they actually
lived. As it happened, Farkas found that the relative-resources (educational differences) theory did badly, subculture (class differences) did best, but the wage-rate (ecological) theory could not be ruled out. As often happens, the big results were surprises; that the presence of children played a central role in determining the division of labor, and that division of labor changed radically over a family’s life cycle.

For us, the important matter here is that by insisting on predicting behavior, not attitudes, Farkas made a distinctly behaviorist move within a tradition generally regarded as already quite behaviorist. It was a matter of doing what we already do, but doing it better. We can see the contrasting move—which is more surprising—in Ron Lesthaeghe’s widely cited “Century of Demographic and Cultural Change in Western Europe.” Lesthaeghe’s paper advances our understanding of changes in demographic behavior, but it does so by moving toward culture.

There are two heuristic moves involved in the paper. The first is locating demographic change within something larger. This move of lumping things together is one of the descriptive heuristics of Chapter Five. An important consequence of Lesthaeghe’s choice of the lumping heuristic is that he employs a quantitative technique aimed specifically at lumping: factor analysis. As opposed to SCA’s much more common regression techniques, which are designed to separate the effects of different variables, factor analysis specifically asks whether certain variables cannot be lumped together as part of larger phenomena. (It is important to realize that once one starts looking, there are formal, mathematical methods for many heuristic moves. Statistical and mathematical techniques reach far more broadly than a glance at the journals—or a course on sociological statistics—might make you think.)

For our purposes, Lesthaeghe’s paper is less interesting for its lumping than for its move toward culture. This is clear from the opening sentences:

A fertility decline is in essence part of a broader emancipation process. More specifically, the demographic regulatory mechanisms, upheld by the accompanying communal or family authority and exchange patterns, give way to the principle of freedom of choice, thereby allowing an extension of the domain of economic rationality to the phenomenon of reproduction. . . . The purpose of this exercise is to explore the extent to which current changes in fertility and nuptiality can be viewed as manifestations of a cultural dimension that had already emerged at the time of the demographic transition in Europe. (Lesthaeghe 1983:411)

In making this move, Lesthaeghe moved very much against the grain of demography as a social science. Demography is in many ways one of the most behaviorist of the social sciences. Its central variables are rates of four unmistakably explicit behaviors: birth, marriage, death, and migration. The apparatus of life-table analysis, through which rates of these four behaviors can produce estimates of populations’ age and marriage structures, is one of the glories of formal social science. Yet Lesthaeghe’s whole enterprise in this influential article is
to make us see demographic change as a part of a cultural shift, not a behavioral one. And he manages to use quantitative techniques to do it!

We see, then, that within a particular tradition of methods that is widely understood as strongly behaviorist, it is still possible to move in either direction. Farkas’s move is strongly toward behavior that can be measured. Lesthaeghe’s is toward a cultural construct (the rise of individualism) that can be “measured” only as an implicit commonality among existing sets of measured variables. Once again, we see that a commitment at one level to one or the other side of a fractal heuristic does not translate into a commitment at the next level. All roads are always open.

IV. INDIVIDUALISM AND EMERGENTISM

The debate over individuals and emergents has been one of the most enduring in social science. Methodological individualists are forever insisting that only individuals are real. Yet most of us are closet emergentists with working beliefs in social groups and forces. Philosophically, emergentism has found itself the embattled position. Every reader of Durkheim’s *Suicide* knows that the author spends many (probably too many) pages defending his emergentist views and attacking individualism.

Yet this pairing, too, can be a fractal heuristic. Emergentist literatures invoke individualist theories and vice versa. One can see this in any methods tradition. In ethnography, for example, the dominant tradition is ethnography of groups, from Malinowski onward. Yet there is an equally old tradition of individual study or life history, beginning with W. I. Thomas and Florian Znaniecki’s five-volume series on *The Polish Peasant in Europe and America*, which was largely built on life histories and life-history documents. The historical turn of anthropology has brought a renewal of such a focus on individuals, as in Sahlins’s work on Captain Cook in the Hawaiian Islands. Historical analysis has of course seesawed for many decades between great man biographical history and corporate history. Within particular historical works, the two levels of analysis are often completely intertwined.

Again, a good way to see this fractal duality in action is to discuss contrasting papers within one major method tradition, in this case, formalism. Among the most famous books in social science over the last half century is *The Logic of Collective Action* by Mancur Olson, Jr. Olson’s basic aim is to show why people join groups and participate in group activities; he starts from a resolutely individualist premise: he wants to question the notion that people join groups because of the benefits they get from them. He notes that groups often provide benefits for all their members, whether the members contribute or not. When it comes to these collective goods, as they are called, those who can get away with it have every incentive to take them without contributing anything. (Those who do so are the “free riders.” Olson’s was the analysis that popularized the concept—but not the term—of “free riding.”) But if this is the case, how can we explain why groups that provide collective goods ever exist? Olson’s answer to this question was ingenious, invoking what he called selective incentives—various ways the group has of targeting those who contribute (giving them
positive rewards) and those who don’t (giving them punishments). Of course, there were further problems (who was to pay for the system of selective incentives? and so on), but the book ignited a debate on the nature of collective action that continues to this day. All of this was argued in the classic formal style of economics, using fairly simple representations of supply, demand, contribution, and so on. And all of it started in the traditional manner, with isolated individuals.

At the same time Olson was writing, the sociologist Harrison White was moving in precisely the other direction. White employed similarly formal methods to ask nearly the reverse question: not how is it that individuals with similar interests get together in groups but rather should we define individuals as similar when they are located in similar positions in all of their social groups? For Olson, similarity of individual interests came first, and location in groups (with the aim of collaborating on producing collective goods) came second. For White, it was exactly the other way around. Location in groups came first, and we could understand people as being similar (in interests or in anything else) if their patterns of social location were similar.

François Lorrain and Harrison White’s “Structural Equivalence of Individuals in Social Networks” starts not from the notion that there are individuals and groups but, rather, from the notion that there are individuals and types of relations between them. As is often the case with such original papers, many levels of complexity were included in this exposition that have since been forgotten. But hidden in the complexity and couched in the impenetrable mathematics of category theory was a concept that would revolutionize the study of networks: the concept of structural equivalence. Loosely speaking, structurally equivalent actors are defined as those actors all of whose network ties are the same:

In other words, a is structurally equivalent to b if a relates to every object x of C in exactly the same ways as b does. From the point of view of the logic of the structure, then, a and b are absolutely equivalent, they are substitutable. Indeed, in such a case there is no reason not to identify a and b. (Lorrain and White 1971:63)

White and his collaborators and followers would elaborate the concept of structural equivalence, making it into a comprehensive model for understanding roles and social structures. Similarity became network similarity. Relations come first; individuals second.

Once again, then, we see that moves toward individual conceptions or emergent ones are possible despite the usual association of formalization with methodological individualism. The history of network analysis is extremely instructive in this regard. The “individualist” network analysts (those opposed to White—James Coleman, for example) conceived of networks largely in terms of cliques and measured “centrality” in networks, whereas the emergentists like White (usually called structuralists in this literature) focused on structural equivalence. The structuralist Ronald Burt wrote a widely cited paper in which he tested the two against each other (1983). Not surprisingly given Burt’s allegiance, structural equivalence won. But the individualists went merrily on and
eventually developed the notion that having a lot of network ties was a kind of resource for individuals. Baptised by Pierre Bourdieu and James Coleman with the name social capital, this notion has become one of the great growth concepts of the 1990s, now virtually a standard variable in traditional SCA-type analyses of field after field. Meanwhile, the structuralists have pared down the elaborate logic of multiple types of relations that drove White’s original work and are developing “network” concepts of markets that invoke many of the classical incentive theories of traditional microeconomics. Peter Abell wrote about “games in networks” (1990), bringing together the structural concept of networks and the relatively individualistic concepts of game theory.

So this fractal heuristic, too, is steadily taking new turns within the old turns, and so on. Just as it drives the research frontier, so also is it available for us in more routine social science. Making a move toward individualism or emergentism is always available as a means of rethinking a problem or finding a new line of investigation.

(Part 2 of this chapter will be assigned next week)

Note:
1. This argument led eventually to a joke about a Chicago economist and his student walking down the road. The student tells his mentor he sees a one-hundred-dollar bill on the ground. The economist says, “You should have your eyes examined. If a bill were there, someone would have picked it up.”