1 Introduction

The complexity of sociology

1.0 Telling the Story of Sociology's Complexity

Deciding how to tell the story of western sociology and its complexity is not easy (Baehr 2002; Collins 1994; Coser 1977; Lepenies 1988; Merton 1968, 1996; Ritzer and Goodman 2004). One problem concerns the "nostalgia trap" of sociology—the tendency to conflate Merton's distinction between the *history* and *systematics* of sociology (1968). History has to do with hermeneutics: "recovering" the meaning of the historical texts of sociology by reading them as they were intended, including the audience for which they were created and the social and material contexts in which they were situated (Merton 1968). Equally important, history concerns historiography: the method of getting the "history" of sociological texts correct, including the exact influence they had upon whom and why and to what extent (Jones 1983). In contrast, systematics has to do with exegesis: making use of historical texts by applying them to the present; that is, creatively reading and interpreting "texts" from the past in terms of the concerns and intentions of today. Systematics involves "creating" new links between the present and the past (Jones 1983, p. 447). Said another way, the "history" of sociology has to do with reading the past for its own sake, while systematics has to do with constructing a "history of the present" (See Foucault 1991, Chap. 1).

The nostalgic trap is the process of conflating exegesis with hermeneutics and historiography. In so doing, history falsely becomes the confused with creative links contemporary sociologists make with the past; not history as it actually happened.

Moving forward from Merton, the new historians of sociology (circa 1980s) refer to the nostalgic trap as *presentist* history, in contrast to their own approach, which they call *historicist* history. For the new historians, while historicists keep history and systematics separate, presentists fall into the nostalgic trap, treating exegesis as history (Jones 1983; Seidman 1985).

For the new historians, the nostalgic trap is a problem, in part, because it ignores the political, economic, cultural, disciplinary and academic (i.e., historical) realities in which the discipline of sociology emerged and developed; and because it gives a false impression of the role different scholars and scholarly traditions have played in the progress of the discipline (See Connell 1997 and Jones 1983 for a review of this debate). For example, while Karl Marx is not a sociologist, his tremendous and continued influence on many sociologists renders his work, from a systematics perspective, "classic" and therefore part of the "cannon" of the discipline. From a historical perspective, however, Marx was not involved in the creation or development of sociology. Furthermore, most scholars writing under the disciplinary auspices or academic letters of "sociology" during the late 1800s and early 1900s did not treat Marx as a sociologist or his work a "classic." Neither did many of them—particularly in the United States, where the discipline of sociology would primarily take shape—treat Weber or Durkheim with much admiration or awe (Jones 1983). In fact, as Connell explains:

> Turn-of-the-century sociologists had no list of classics in the modern sense. Writers expounding the new science would commonly refer to Comte as the inventor of the term, to Charles Darwin as the key figure in the theory of evolution, and then to any of a wide range of figures in the intellectual landscape of evolutionary speculation (1997, p. 1513).

The other reason the nostalgic trap is a problem for historicists (and for Merton) is because it is so pervasive. As Jones (1983) and Connell (1997) explain, from Durkheim to Parsons to Giddens, the name of the historical game seems to be exegesis-as-history; or, as Merton states, "retrieving" past sociological texts for their use in the present (1968). Given the fame of the numerous presentists in sociology, their view has become—particularly since the 1920s—the standard account of the discipline. For example, as Connell points out, the majority of contemporary undergraduate and graduate textbooks in "English speaking" sociology consistently treat systematics as history (1997, pp. 1512–1515).

Because the nostalgic trap is an important issue in the historiography of sociology, we will keep the following five points in mind while telling our story of sociology's complexity.

- First, we will remember that the term "sociology" refers to a somewhat heterogeneous and often times conflicting and discontinuous network of scholars, theories, concepts, methods, intellectual traditions, schools of thought and substantive topics generally associated with the study of society.

- Second, we will remember that different scholars gather, organize, center, marginalize and ignore aspects of this "sociology" in distinctive ways, each telling a somewhat unique "story" about the discipline based on the particular "history of the present" they seek to construct—think Michel Foucault (1977, 1980, 1987).
- Third, we will remember that the storyline of sociology is not necessarily linear, seamless, progressive, or continuous. In fact, in many ways it is filled with intellectual cul-de-sacs, "dead-ends," breaks, retrogressions, tangents and, in some cases, unrecognized work. One example would be the continued marginalization of the works of W.E.B. Dubois and Jane Adams (Ritzer and Goodman 2004).
- Fourth, we will remember that there is no single sociology; instead, there are many. As Collins, for example, has made clear, the story of sociology in France is not the story of sociology in England; and the story of European conflict sociology is not the story of pragmatic sociology in the United States (Collins 1994).
- Finally, we will remember that, despite the nonlinear trajectory of sociology, and despite the different ways its stories can be told, there is a natural history to sociology and its various traditions, lineages, and so forth.

Reminding ourselves of these five points, however, will not keep us from exegesis. As Collins explains, while the new historians of sociology are correct to remedy the conflation of history and systematics, their remedy does not force one to avoid exegesis or its *integration* with hermeneutics. Even Merton makes this point. The history of sociology does not do away with exegesis. It makes exegesis better (1968, p. 33). In fact, despite the importance of hermeneutics and historiography (i.e., getting the past "right"), exegesis (i.e., reacquainting one's self with the classics, See Merton 1968, p. 33) moves ideas forward. One looks to the past (even if it is the immediate past) to create a new storyline of the present—think Foucault (1977) and Randall Collins (1994).

Given these important points, we will use the genealogical methods of Foucault and Collins to tell our story of sociology's complexity. While different in focus, both scholars combine hermeneutics, historiography and exegesis. First, Foucault, by placing great emphasis on the historical conditions of classic texts—that is, the relevant social practices in which they are situated, from the cultural to the institutional to the scientific—seeks to understand the past in terms of the concerns of the present (1977, 1987). Foucault is not interested in history for its own sake. Instead, he seeks to illuminate our current condition by searching out its breaks with and discontinuities from, as well as its connections to and links with the past

(Foucault 1991). Foucault's genealogies connect the present to the past by going back to the future. The genealogies of Randall Collins are somewhat opposite: they connect the past to the present. Through a firm footing in the historical conditions of the ideas he explores, Collins searches out and articulates, with great facility, the continuities of sociology; what one might call a sort of ongoing "historical exegesis" that focuses on the disciplines' major traditions, family resemblances, common challenges and comparable mistakes (1981, 1994, 1998). The value of both methods is their success at integrating the history and systematics of sociology.

By relying on these twin genealogical approaches, our story about sociology's complexity will move in dual directions, from the present to the past and the past to the present. Our story seeks out breaks and continuities, differences and similarities and it immerses itself in the history of sociology while taking at face value previous exegesis. With all of these points in mind, we turn to our story of sociology's complexity.

1.1 The Story of Sociology's Complexity

Our basic thesis—that is, the genealogy we wish to construct—is that western sociology (including its various smaller, national sociologies) has been and continues to be a profession of complexity, although not always of the same type. Industrialism, for example, is not postindustrialism, and European modernity is not American modernity. Nevertheless, since its formal emergence in the middle 1800s and, more specifically, since its establishment within the modern universities of Europe and North America at the turn of the previous century, the major challenge of sociology has been complexity (Baehr 2002; Collins 1994; Coser 1977; Heilbron 1995; Lepenies 1988; Merton 1968, 1996).

The primary basis for this challenge is western society. To study society is, by definition, to study complexity (Buckley 1998; Luhmann 1995; Urry 2003, 2005b). Starting with the industrial and "industrious" revolutions of the middle 1700s to early 1900s (Ashton 1964), western society transitioned—teleology not implied—into a type of complexity that, in many ways, did not previously exist (Toynbee 1884/2004). Urban centers and cities emerged, massive waves of emigration and immigration took place throughout Europe and North America; multiple ethnicities were forced to interact with one another; major innovations in technology, science and philosophy took place; democratic governments of various forms emerged, as did new forms of economic, political and cultural inequality, domination, oppression, conflict, and struggle—not to mention the impact all of this had on traditional ideas of family, marriage, gender, religion, the meaning of life, and one's private sense of self (Hunt, Martin, Rosenwein, et al. 2004; McKay, Hill and Buckler 2003; Wiesner, Ruff and Wheeler 2003).

Furthermore, as industrialization evolved into its later stages (i.e., Taylorism, Fordism, post-Fordism, etc), the complexity of western society evolved as well (Gilbert 1997; Howard and Louis 2006). This advance in complexity was further facilitated by the increasing division of labor, growth of the middle-class, expansion of the professions, civil rights, continued developments in technology and medicine, the rise of counter-culture, increases in the lifespan of the general population and, finally, continued reform in the welfare state and social welfare (Diner 1998; Hofstadter 1955). It is within this material and ideological milieu of profound and rapid societal change that the first scholars of sociology made their mark (Baehr 2002; Collins 1981, 1994; Lepenies 1988; Ritzer and Goodman 2004).

1.2 Sociology's Complexity: The Early Years

Given our discussion about historiography we will assume that there is no definitive list of "classical" western sociologists. Instead, there are various "lists." We will therefore be specific about the canonical scholars in which we are interested. Of the numerous scholars writing during the middle 1800s to early 1900s, we focus on the following: Auguste Comte, Herbert Spencer, Karl Marx, Max Weber, Emile Durkheim and Vilfredo Pareto. We did not choose these scholars because of their status as "classical" thinkers. We chose them because they all participated in the formation of what, by the 1920s, would become known as the systems tradition in sociology. We call these scholars *systems thinkers* for three reasons:

1.2.1 Embracing the Complexity of Western Society

First, while working under different academic letters, governments, professional titles, political positions, cultural contexts and institutional arrangements (or the lack thereof), and while working at varying distances from the creation and development of the profession of sociology, these scholars conceptualized their work as a direct response to the increasing complexity of western society.

The idea that much of sociology or sociological thinking was created to address the major changes taking place in western society is a familiar and, in many ways, accurate story that students learn in undergraduate and graduate school (Baehr 2002; Collins 1981, 1994; Heilbron 1995; Lepenies 1988; Ritzer and Goodman 2004). Class conflict, industrialism, the rural/urban shift, social alienation, the challenges of laissez-faire government, cultural diversity, ethnic conflict, political revolution, the growth of the welfare state, religious fervor and collapse, church/state conflict, the encroachment of bureaucracy, capitalism, inequality, imperialism—this provided the core fodder for our canonical sociologists. All of this "fodder," including its associated social problems, was a direct outcome of society's growing complexity.

The concept that most aptly captures this focus during the classical era of thinking is *evolutionism*. Whether Darwinian or Hegelian, our short list of scholars adopted some form of evolutionism. In fact, the adoption of an evolutionary perspective goes well beyond our short list to include many early sociologists and sociologically minded thinkers now forgotten or marginalized through the annals of time (Baehr 2002; Collins 1981, 1994; Heilbron 1995; Lepenies 1988).

Evolutionism is the view that societies develop along a timeline moving from simpler to more complex forms of existence—think, for example, of Toennies' Gemeinschaft and Gessellschaft or Durkheim's mechanical solidarity and organic solidarity. This evolution can be conceptualized in organic terms (as in the case of Durkheim and Spencer), or in stages (as in the case of Comte and Marx). It can rely on Darwinian evolutionism (as in the case of Spencer) or Hegelian idealism (as in the case of Marx). Its can be observed through a single lens (as in the case of Marx) or multiple lenses (as in the case of Durkheim). Furthermore, it can be optimistically conceptualized in terms of progress, development, advancement and growth (as in the case of Spencer), or it can be somewhat pessimistically conceptualized in terms of exploitation, imperialism, regression, and decline (as in the case of Marx and Weber). It can even be viewed as some combination of both progression and regression (as in the case of Durkheim). Whatever the view—and whatever the political, economic, cultural or moral agenda of the scholar writing it—the common theme in all these approaches is that, starting in the 1700s and culminating in the 1900s, western society went through a period of increasing complexity. The goal of sociological inquiry for these scholars was to understand this qualitative change in complexity.

1.2.2 Embracing a Systems Perspective

The second reason we call these scholars systems thinkers is because they conceptualized the changes taking place in western society in systems terms; that is, they treated western society (and its various substantive issues) as a system. A system is a general concept that refers to a set of things and the relationships amongst them (Klir 2001). Systems come in all shapes and sizes: from airplanes and library catalogues to chemicals and biological cells to biospheres and the universe. They also vary in their degree of complexity. A system for counting numbers, for example, is rather straightforward, while a tropical rainstorm is rather complex. Given the wide range of possible systems, researchers catalogue them according to type (Klir 2001). One particular type is the set of all human social systems. Human social systems are distinguished in two important ways: the "things" of which they are comprised, which is some set of human social agents (individuals, groups, formal organizations, etc.), and the relationships amongst these social agents, which constitutes some form of social interaction (Byrne 1998; Holland 1995, 1998; Klir 2001; Luhmann 1995).

Conceptualizing western society as a system was very appealing to many sociologically minded scholars writing during the middle to late 1800s (Collins 1988, 1994). This is understandable given the wide-angle view they were trying to achieve. They were struggling to make sense of the incredible shifts taking place in western society and they needed immediate, concrete ways to treat society on its own terms—something that could be studied without "reducing it away" to the micro-level behavior of individual agents. Even Weber, for all his musings on interpretive method, nevertheless focused on primarily aggregate-level social behavior. The concept of "system" gave these scholars the conceptual weight and rigor they needed. With its strong macro-level or biological overtones, the concept of social system carried with it the semiotic sense of being a solid, tangible object for scientific study.

Examples: Marx studied the economic systems of Europe and, later with Engels, the class structure these economic systems produced. Durkheim focused on cultural systems and the functional role that solidarity and ritual played in holding together modern society. Comte examined the evolutionary stages through which society, as a social system, passed. Weber compared the evolving economic and cultural systems of western and eastern societies, along with the role bureaucracy plays in organizing an increasingly complex western society. Spencer studied the role that struggle and competition and, alternatively, negotiation and cooperation play in the formation, evolution and maintenance of society. And, Pareto studied how similar modern societies (as social systems) tend to reproduce similar structures of inequality. In fact, the idea of society as a system appealed so directly to the times that most of the scholars on our short list reified it, treating society and its various subsystems as real objects.

The tendency to treat a social system as a real, tangible object is known as *organicism.* For scholars such as Spencer, Pareto, and Durkheim, society is not just a system. It is every bit as real as a human being. Like the human body, western society is emergent, self-constituted, bounded, environmentally responsive and functionally differentiated. It is comprised of its own internal network of communication, which allows its various subsystems (economic, political, cultural, legal, etc) to coordinate with one another. Furthermore, it is constantly evolving, growing, changing and developing, all the while seeking balance, order, homeostasis and cohesion. Most important, following Haeckel's recapitulation theory of 1866, social systems like western society follow a phylogenic order: as they evolve, they move from simpler to more complex forms of existence.

The complexity of a human social system can be understood in two basic ways. It can be understood as a particular phase-state that emerges and develops over time, which we just mentioned, or it can be understood as an inherent characteristic.

Without being overly simplistic, the later view represents the celebrated insight of complexity science: all social systems, by definition, are complex (Klir 2001; Luhmann 1995).

The phase-state view, in contrast, represents that of many early sociological thinkers such as Spencer, Pareto and Durkheim. The concept of complexity as a phase-shift in the life of a social system like society (or any of its subsystems) is linked to our earlier concept of evolutionism. As societies increase in their internal differentiation (i.e., growth in industry or urban centers), they grow in complexity (i.e., growth in the division of labor or bureaucratic institutions).

Again, not all early sociologists based their systems perspective entirely on evolutionism. Nevertheless, despite their variances in approach, our short list of scholars treated the complexity of western society from an evolving systems perspective.

1.2.3 Learning from the Past

A third reason for identifying our short list of canonical scholars as systems thinkers is that their successes and failures can teach current sociologists and like-minded scholars how best to think about the complexity of western society from a systems perspective.

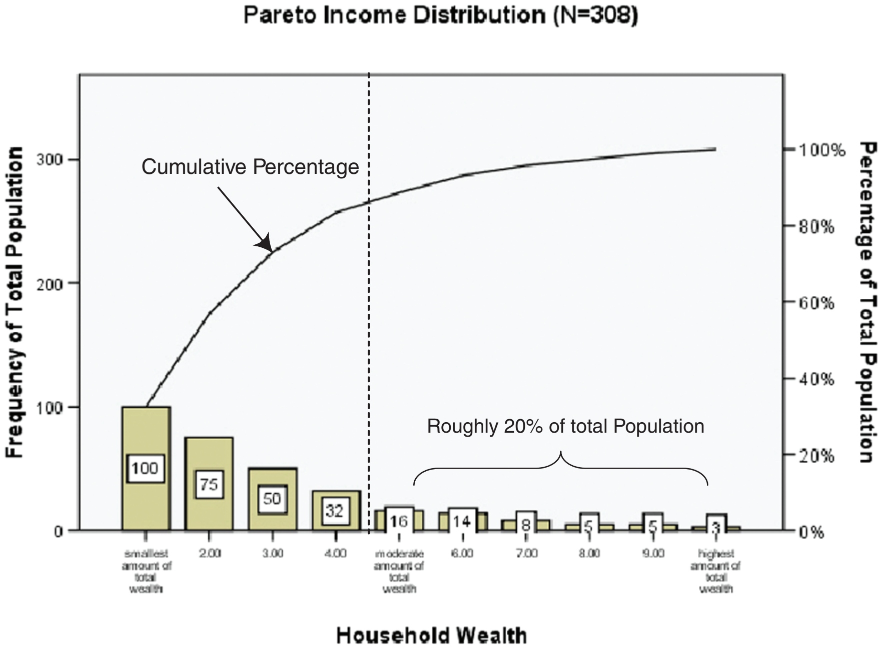
In terms of their failures, for example, these systems thinkers have demonstrated the limited utility and, often times, futility of treating social systems in strictly natural science terms. Evolutionism, equilibrium, homeostasis, organicism and Darwinian functionalism are concepts either to avoid or, at the very least, critically redefine.

In terms of their successes, many of the ideas our short list of scholars formulated actually predate or have been developed into key ideas in complexity science. An excellent example is the 80/20 Rule of Pareto.

1.2.3.1 Pareto is a Complexity Scientist?

Pareto (1848–1923) was an Italian economist, sociologist, engineer and political activist who wrote widely on the topic of western society during the industrial revolution. Strongly influenced by the systems thinking of both Marx and Spencer, including their views on inequality, Pareto set out to study the distribution of wealth in various European countries (Coser 1977). During his studies of the Italian economy, in particular, Pareto discovered that 80% of the land was owned by 20% of the population. His studies of other national economies revealed a similar pattern. Roughly 80% of the wealth of these countries ended up in the bank accounts of less than 20% of their citizens (Barabási 2003).

To demonstrate this fact, Pareto graphed his results, which he plotted as a basic mathematical curve—See Graph 1 as an example. (As a side note, remember that all of the maps, graphs, and figures for this book also are found in Chap. 10.) On Graph 1, the X-axis represents the relative wealth of a population (defined as percentage of total wealth owned) and the Y-axis represents the population in both frequency and proportion (expressed as a percentage of total population).



Using a graph similar to Graph 1, Pareto was able to determine how many people, at any particular point along the curve, had an income greater than a given x value. What he found was that as x increased at a particular rate—the exponential constant in the formula—the proportion of people with an income greater than x (shown on the Y-axis) decreased rapidly, always ending somewhere in the range of 20% of the population owning 80% of the total wealth (Adamic 2007; Buchanan 2002).

While numerous subsequent studies have found this distribution to be generally consistent, some have found the distribution to vary as much as 90–10, 95–20, or even 90–20 (e.g., Adamic 2007; Bak 1999). In terms of mathematical modeling, these types of variations are, however, expected and therefore not important. What is important is that the rule works! In fact, it works so well that it has been applied to numerous fields of inquiry, most notably mathematics, economics, physics and biology (Adamic 2007; Anderson 2006; Bak 1999; Barabási 2003; Buchanan 2002; West, Brown and Enquist 1997). Furthermore, the past hundred years of applying Pareto's insight have turned it into a scientific principle, which scholars now refer to as the 80–20 rule or, more generally, a power law (Adamic 2007; Bak 1999; West, Brown and Enquist 1997).

A power law is a polynomial relationship of the following type ($Y = X^{\alpha}$) where some quantity Y is a function of the exponential increase (α) in another quantity X. In this formula, Y is typically the dependent variable, which is related to changes in X, the independent variable, and its exponent α (Adamic 2007; Gunduz 2000, 2002). Readers may recognize the similarity between this equation and the binomial regression equation ($Y = bX + a$), where "a" is the intercept and "b" is the constant for changes in Y as a function of changes in X. In fact, when transformed into a log-log plot, the power law becomes linear, allowing for it to be treated as a basic binomial correlation, including the test for significance (Adamic 2007; West, Brown and Enquist 1997).

Three characteristics distinguish a power law such as the 80–20 rule from other binomial relationships. First, as shown in Graph 1, the relationship is not bell shaped. While a significant number of phenomena in mathematics and the social and natural sciences tend to take a bell shaped (Gaussian) distribution, the phenomena explained by power laws do not. They are curvilinear. Second, they are curvilinear because, while smaller events take place at a much higher frequency than the larger events on the graph, the larger events dominate. As Barabási states, "Power laws formulate in mathematical terms the notion that a few large events carry most of the action" (2003, p. 72). In the case of the 80–20 rule, for example, there are more poor people than rich people, but rich people, "collectively speaking," have most of the money. Third, some degree of scale invariance exists (West, Brown and Enquist 1997). At smaller or larger levels of scale (up to a point) the same basic relationship between Y and X is found. In the case of the 80–20 rule, for example, as one moves from the national to the state to the community level, one typically finds that the distribution of a population's wealth remains roughly similar, with most of the wealth at each level of analysis being in the hands of a few (Adamic 2007; Bak 1999; Mandelbrot 1997; West, Brown and Enquist 1997).

It is the muscle of the power law that eventually led to its usage in complexity science. From biological cells to social groups to ecosystems, the power law is reflected in the structure and dynamics of many complex systems (Bak 1999; Gunduz 2000, 2002; Mandelbrot 1997; West, Brown and Enquist 1997). To demonstrate this amazing insight, we will take a quick look at the research on structure.

In the new science of networks, a major area of study is the structure of very large, and highly complex systems (Barabási 2003; Buchanan 2002; Watts 2004). The small-world hypothesis, six-degrees of separation and scale-free networks are some of the more profound insights that sociologists, physicists, and mathematicians have made in the study of large, complex systems. These insights have to do with the fact that, while stochastic, complex systems are not entirely random or chaotic. In fact, while most complex systems cannot be fully determined, they do possess a tremendous level of order. Specifically, they tend to self-organize. Phrased in network terms, only a few nodes (called hubs) tend to be densely connected. On the internet, for example, less than 20% of all websites generally receive over 80% of all traffic; in the business world, 80% of a company's profits come from 20% of its products; and in the world of management, 80% of an organization's work is usually done by less than 20% of its employees (Buchanan 2002; Watts 2004). The same general phenomenon has been found in the study of cities, large friendship networks, international business networks, ecosystems and epidemiology (Buchanan 2002; Gunduz 2000, 2002; Mandelbrot and Hudson 2004; Newman, Barabási and Watts 2006). Most important (at least with respect to the focus of this chapter), this amazing phenomenon traces its intellectual lineage, in part, to the work of a sociologist, Vilfredo Pareto. Oddly enough, the 80–20 rule is not part of the sociological cannon. The reason why takes us to Parsons. But, first a concluding point.

1.2.4 Intellectual Dead End

Despite its initial widespread popularity, the systems tradition started by our short list of canonical scholars was, in large measure, dead by the first half of the 20th century. This is not to say, obviously, that these thinkers no longer had an impact on sociology. Certainly the work of Marx, Weber and Durkheim, for example, went on to have a profound impact on their separate national traditions in sociology—they are, after all, considered the classics—with influence in the formation and development of other disciplines such as economics, political science and anthropology. Still, despite the profound influence our canonical scholars had on the discipline of sociology, it was for the most part an impact devoid of any "systems" perspective. In fact, scholars such as Spencer, in whom the separation of sociology and systems thinking was impossible, were simply discarded. So was the terminology of systems thinking. Evolutionism, organicism, system differentiation—they were all thrown into the intellectual garbage bin of useless sociological ideas; that is, until the arrival of Talcott Parsons.

1.2.5 The Case of Parsons

Our story about sociology's complexity makes an exegetical break with the 1800s and Europe, crossing the Atlantic in search of the emerging discipline of sociology in the United States. Our destination is not, however, the University of Chicago and the work of Robert Park, Jane Addams, W. I. Thomas, or George Herbert Mead. Nor is our destination the work of W.E.B. Du Bois. We also do not visit Charles Horton Cooley at the University of Michigan. We even ignore the nomadic Thorstein Veblen. While all of these scholars are profoundly important to the development of western sociology, particularly in the United States, they contribute to sociological traditions other than the one in which we are interested.

As we stated at the beginning of this chapter, we are constructing a genealogy of the systems tradition. As such, we purposely ignore the above list of scholars and their work, turning instead to the Department of Social Relations at Harvard University, circa 1950s. Our destination is Talcott Parsons. Our reason is straightforward enough. Of the various attempts within sociology to address the growing complexity of western society from a systems perspective, including our short list of canonical scholars, the work of Talcott Parsons is the most important.

Most know the story of Talcott Parsons: his creation of structural functionalism, his triumphant rise to academic power and his eventual dominance of American sociology. Most also know about his presidency of the American Sociological Association in 1949, his work to develop the Department of Social Relations at Harvard University, and his significant influence on several generations of graduate students (Gerhardt 2002). Most also know about the crushing criticisms that were leveled at Parson's theoretical diamond, structural functionalism, during the 1960s and 1970s. These criticisms included such important issues as: (1) structural functionalism is not a theory; (2) it lacks explanatory power; (3) it explains away conflict and social change in the name of solidarity and order; (4) it is highly conservative and overly normative; (5) it misinterprets the ideas of many European sociologists; (6) it blatantly ignores the work of Karl Marx; (7) it is exceedingly abstract with almost no empirical grounding or application; (8) it makes the same evolutionist errors as Spencer and Durkheim; and (9) it falls into the trap of treating society as a biological organism (Collins 1988, 1994; Gerhardt 2002; Ritzer and Goodman 2004; Trevino 2001).

Given such criticisms, this usually is where the story of structural functionalism (at least as told in most textbooks), comes to an end (Collins 1988, 1994; Ritzer and Goodman 2004). While certain key concepts such as the *sick role* remain important, structural functionalism is another "dead-end" in the genealogy of the systems tradition in sociology. Or, at least, that is what most sociologists think. Unfortunately, this is not a "historically" correct story.

At present, the Parsons story is shaping up to be one of profound irony. It turns out that at the very moment that Parsons and "all things systems" were being discarded by most sociologists, several key advances in the newly emerging field of complexity science were taking place. And here lies the irony: these advances came from the same toolbox and interdisciplinary attitude Parsons had attempted, but failed, to foster in sociology (Capra 1996, 2002). The collapse of structural functionalism therefore does not appear to be the victory everyone had anticipated. Instead, our collective need to "do away" with Parsons and all things systems looks grossly shortsighted, particularly in light of the current challenges of complexity facing sociology today—which we will discuss in moment. For now, let us defend our provocation.

1.2.5.1 The Other Leg of Parsons

While Parsons' grand theory was grounded in the systems tradition of European sociology, his foundation had another leg, grounded in the emerging fields of cybernetics and systems science (Collins 1988, 1994; Gerhardt 2002; Ritzer and Goodman 2004; Trevino 2001).

As we explain in detail in Chap. 5, cybernetics and systems science are the first two "sciences" explicitly and specifically devoted to the study of systems; and, at the time of Parsons, represented the cutting-edge of science (Capra 1996; Hammond 2003). In fact, these twin sciences turned out to be two of the most important areas of scientific inquiry in the 20th century, leading to the development of artificial intelligence, game theory, communications, the computer, the internet, informatics, systems biology, computational modeling, machine intelligence, and a significant number of advances in modern mathematics, such as computational/discrete mathematics.

More important for Parsons and our study, they also turned out to be the intellectual forbearers of complexity science. In fact, just about every major accomplishment in complexity science can be linked to work that was done in these two sciences between the 1940s to the 1970s (Capra 1996, 2002; Casti 1994; Lewin 1992; Waldrop 1992). As shown in Map 1, in terms of complexity science method, there is the lineage that runs from cybernetics to distributed artificial intelligence to agent-based modeling and its key methods such as neural networking, genetic algorithms, artificial life and multi-agent modeling. This lineage also includes the historical links that cybernetics and systems science have with cellular automata and the development of fractal geometry and chaos theory. In terms of theory, this lineage includes the links that runs from systems science and second-order cybernetics to the creation and development of such key topics in complexity science as emergence, self-organization, autopoiesis, system dynamics and networks—all shown on Map 1.

But that is not the end of it. Not only did Parsons partially ground his theoretical dreams in cybernetics and systems science, he grounded his organizational and cultural hopes in them as well. From their historical beginnings, cybernetics and systems science have been resolutely interdisciplinary, seeking out empirical and theoretical insights relevant to the conduct of science in general (Bailey 1994; Hammond 2003). This interdisciplinary mentality is well demonstrated in their organizational arrangements. One example of such an interdisciplinary infusion is the Massachusetts Institute for Technology (MIT), home to most of the leading scholars in cybernetics and systems science.

Parsons took this mentality to heart by creating the *Department of Social Relations*, a few short miles from MIT up Massachusetts Avenue (Gerhardt 2002; Trevino 2001). For Parsons, the purpose of the Department of Social Relations at Harvard was to promote the interdisciplinary culture of the Arts & Sciences by creating a place where scholars could come together to work. The list of thinkers involved in Parsons' department, as either student or staff, is beyond impressive, including such important scholars as George Homans, Richard Solomon, Gordon Allport, Jerome Bruner, Harold Garfinkel, Robert Merton, Neil Smelser, Harrison White, Mark Granovetter, Barry Wellman and Stanley Milgram, to name a few (Collins 1988, 1994; Gerhardt 2002; Ritzer and Goodman 2004; Trevino 2001). For those fluent in complexity science, Harrison White is a pivotal figure in both computational sociology and social network analysis (Gilbert and Troitzsch 2005); Granovetter and Milgram's work is central to the small-world phenomenon and the study of complex networks (Watts 2004); and Homan's exchange theory is pivotal to Robert Axelrod's iterative game theory and the complexity of cooperation (Axelrod 1984, 1997). Thus, while the theory of Parsons may have failed, and while his department eventually was discontinued, his impact on the genealogy of systems thinking remains alive in complexity science—just not in sociology.

1.2.5.2 Sociology's Infinite Regress

In many respects, Parsons should be applauded for his incredibly prescient efforts to ground his theoretical and organizational efforts in cybernetics and systems science. This applause, however, does not lessen the crushing and altogether correct criticisms made of his work. In fact, cybernetics and systems science were criticized for many of the same issues as Parsons (Capra 1996; Klir 2001).

There is, however, a difference in the way sociologists (versus natural scientists) employed these criticisms. While sociologists used their criticisms of Parsons to "do away" with all things systems, including the rejection of cybernetics and systems science, scholars working in mathematics and the natural and computational sciences did not engage in such a whole-sale rejection (Capra 1996; Klir 2001). While these scholars were equally concerned with the ways in which cybernetics and systems science were "incorrectly applied" within their disciplines, and while they stuck to their critiques, they nevertheless sought to overcome the problems of these fields by "staying with them" so to speak. They developed the ideas; fixed them; moved them into the future and made new exegetical histories of the present. As a result, the natural sciences are at the forefront of complexity science (Casti 1999; Cilliers 1998). Sociology is not.

One of the well-documented problems of sociology is its tendency to use a theory's errors as an excuse to replace it with something else (Abbott 2000, 2001). For example, because Parsons and Spencer made the mistake of functionalism or evolutionism, respectively, the practice in sociology is to discard everything they said, condemn their work, and redeposit intellectual favor somewhere else. In the case of social systems, this means to refrain from talking about systems or progress or anything of that sort, even if these concepts have some value. In short, sociologists have a bad habit of "throwing out the baby with the bathwater." A related problem, given our apparent eagerness to discount things, is that we have the easy ability to work alongside other theoretical perspectives, all the while completely ignoring their critical utility to our work. As Abbot explains (2001), this ability has a lot to do with the boundary permeability of our discipline, which makes it is rather easy for sociologists to dismiss an idea, relevant or not.

While our discipline's characteristic approach to "dismissing knowledge" often has the advantage of immediate gain (we get away from bad ideas fast), it comes at a price. In terms of the increasing complexity of sociological work, the cost is being in the conceptual backwaters rather than at the forefront of the systems tradition and its latest manifestation, complexity science.

To illustrate this point, we will create a short laundry list of the things most sociologists lack or cannot do. Most sociologists:

- Have little to no training in agent-based modeling.
- Are not able to engage in or converse about neural networking.
- Lack the skills necessary to employ the tools of data mining.

- Cannot converse in computational or discrete mathematics, let alone make use of such techniques as cellular automata.
- Do not know how to employ the tools of dynamical systems theory, either in the form of fractal geometry or chaos theory.
- Cannot converse in the rich vocabulary and language of complexity science.
- Do not know how to use or critique the new science of networks.
- Are on the margins of the major journals, conference and funding streams devoted to the study of formal organizations as complex systems.
- And, finally, do not have the necessary techniques for studying the large, multidimensional, electronic databases that are now readily available on the internet for study.

Given these limitations, sociology now must "look in" on complexity science as something strange and wonderful that might benefit its work, rather than looking at it from "within" as something sociology helped to create. So, in many ways, sociology ends up back where it started prior to Parsons or, even worse, prior to our short list of canonical scholars: trying to understand the changing complexity of western society, but still basically unprepared to do so. Another genealogical dead-end.

1.2.6 The Rise of Complexity Science

Given the second collapse of systems thinking in sociology (first, the canonical scholars, second Parsons), our story makes another exegetical break. This time we leave sociology altogether. Our destination is mathematics and the natural sciences, specifically physics and biology during the 1970s and 1980s. Our focus is a small but growing network of scholars, including some of the most important scientists of the 20th century: Cowan, Gell-Mann, Prigogine, E. O. Wilson. There are two reasons for turning to this network. First, this network immersed itself within and helped to critically develop the systems tradition at about the same time that most sociologists were discarding this tradition. Second, this network stumbled onto an accomplishment that could have been, at least partially, sociology's. By drawing upon the latest developments in mathematics and computational modeling, this network advanced the tools of cybernetics and systems science to study, among other things, western society as a complex system. The name of this network of scholars, as we now know it, is *complexity science* (Capra 1996; Lewin 1992; Waldrop 1992).

As we discussed in the preface to this book, complexity science has captured the public imagination with discussions of emergence, swarming behavior, self-organization, computer simulations, and so forth. There is, however, a downside to this ubiquity. Complexity science has been confused with or mistaken for a variety of things. This is a real issue and something we hope we can "clear up."

First of all, complexity science is not a quasi-spiritual embrace of the great web of life—the idea that everything is interconnected with everything else, forming a seamless tapestry of existence (Capra 1996). While some scholars, such as James Lovelock and Lynn Margulis have put forth once controversial theories such as the Gaia hypothesis (which explains how the outer layer of the earth functions as a living, complex system), and while such ideas have inspired awe, they nevertheless are empirical propositions, meant to be "fought out" scientifically. To dismiss or conflate these ideas as metaphysics is to miss the point. The same is true of the brilliant work of Francisco Varela and colleagues, who spent decades examining the intersection of cognitive science and Zen Buddhism (Varela, Thompson and Rosch 1991). While their work creates a dialogue between complexity science and Buddhist philosophy, it does not treat the two as equivalent.

Second, complexity science is not beholden to any particular moral or political agenda (Hammond 2003; Klir 2001). Over the last half century, complexity science has served a variety of purposes, from the development of missile guidance systems and global corporations to smart-shopper cards and self-regulating washing machines to biotechnology and ecosystems research (Capra 1996). Given its numerous usages, complexity science will no more "save" or "destroy" the world than any other scientific discourse (Capra 1996; Hammond 2003). Complexity science is a very specific approach to empirical inquiry that has very specific features. While loosely associated with a variety of moral and political perspectives, these wobbly associations do not define the science. It is a science that can serve a variety of purposes.

Third, complexity science is not chaos theory or fractal geometry. In the popular science literature it is typical for authors to discuss bifurcation points (chaos theory), the nonlinear pattern of fjords (fractal geometry) and the network structure of global diseases (complexity science) as if they were the same science (Cilliers 1998). They are not. Most fractal structures are not complex systems. Total chaos is not a general trait of the universe and many complex systems are only marginally chaotic or fractal.

Furthermore, while complexity science is a general field of inquiry, chaos theory and fractal geometry are mathematical branches of dynamical systems theory; which, in turn, traces its roots back to Newton and the Calculus (Capra 1996). Despite these differences, chaos theory and fractal geometry do inform complexity science. In fact, they have proven amazingly powerful in the study of complex systems, helping researchers understand such various phenomena as stock markets, weather patterns, earthquakes and collective behavior (Bak 1999; Mandelbrot and Hudson 2004). Still, while these two areas of study are part of the mathematics of complexity, they are not complexity science.

Fourth, complexity science it is not postmodernism (Eve, Horsfall and Lee 1997). In the postmodern literature much has been made about the limits of modern science; how science is only one type of knowledge amongst many; how science has no authority over other forms of knowledge, and how empirical inquiry is basically writing (Best and Kellner 1991). To make their case, postmodernists have turned to a variety of sources. The main ones have been chaos theory and, to a lesser extent, fractal geometry, along with catastrophe theory and, most important for our discussion, the early work of complexity science.

In the thirteenth section of his now famous report, *The Postmodern Condition* (1984), Jean-François Lyotard makes the argument that chaos theory and its ilk (catastrophe theory, complexity science, etc) are creating a paradigm shift in academia, one that goes from the modern to the postmodern. His argument, which revolves around the concept of system, is as follows: (1) new research in the fields of chaos theory, fractal geometry, catastrophe theory and other related areas demonstrates that complex systems are not stable, controllable or knowable; (2) instead, they are unstable, uncontrollable and largely unknowable; (3) this realization about the nature of complex systems has forced these researchers to break with the reductionistic, quantitative and mechanistic methods of modern science; (4) this realization has required researchers to let go of the Newtonian, Enlightenment paradigm and its belief in a directly observable and knowable universe; (5) in place of modern science, these researchers have constructed a new, post-modern science, one that is based on the search for instabilities, irregularities, differences, dynamics, local knowledge, fractals, chaos, and so forth.

Two things are amazing about Lyotard's report, along with the postmodern literature that followed. First, Lyotard and his colleagues pretty much get the "science of complexity" wrong. Often not even close. This would be permissible if the purpose of their work was strictly playful or metaphorical, in the way that Derrida, for example, engages scientific ideas. Unfortunately, many of these writers are rather serious. They truly believe that modern science is nothing more than writing, politics and power, and that the latest advances in complexity science support this point. To demonstrate the lack of rigor and reason in such mischaracterizations of science, the physicist John Sokal decided to perpetrate his famous hoax (See Sokal and Bricmont 1999). In 1996 he published a paper in the journal *Social Text* that was, in his own words, a pastiche of fashionable nonsense—impossible mathematical statements and pseudo-scientific claims made by postmodernists, which he assembled together to create the simulacra of an argument. Remarkably, it was published. Sokal perpetrated his now-famous hoax to make an important point. When it comes to the usage of science on behalf of postmodern critique, standards of rigor and reason are required. Science is more than discourse and more than "just" politics and power. Science is real.

Still, for all the hoopla, Sokal never argued that all of postmodernism lacks rigor or reason; and here is where we return to the second amazing thing about Lyotard's report. Despite Lyotard's gross misrepresentation of the technical details of ideas such as dynamical systems theory, he correctly realized that the study of complex systems constitutes a new way of doing science. Even those critical of postmodernism, such as Sokal (Sokal and Bricmont 1999) and others (e.g., Cilliers 1998) concede this point. Lyotard also is correct that many of the major criticisms that postmodernists make of modern, western science are likewise made by complexity scientists. For example, postmodernism and complexity science share a similar concern with the limitations of modern science, particularly its reductionistic, linear, hierarchical and mechanistic thinking. It also is true that both take a subjectivist approach to knowledge and share a common interest in complexity, local knowledge and difference. Furthermore, both recognize the limits of quantitative science and take a more qualitative approach to their work.

However, it is not true that complexity theory has any intention whatsoever, as Lyotard states, of "producing the unknown," or in "theorizing its own evolution" as "discontinuous, catastrophic, non-rectifiable, and paradoxical" (1984, p. 60). It also is not the case that complexity theory is necessarily "anti-modern." Here is where complexity science and the postmodernists part ways, and here is why we make the point that complexity science is not postmodernism.

Complexity science can be called postmodern only insomuch as it is "beyond" modernism (Cilliers 1998). In other words, in an effort to understand the nonlinear, dynamic, evolving, emergent, negotiated, conflicted, highly interdependent, distributed, far-from-equilibrium, self-organizing properties of complex systems, complexity science has had to develop new ways of doing science, including new epistemologies, methods, concepts and theories. This change in the ways of "doing science" also has required complexity scientists to ask questions likewise posed by the postmodernists (Cilliers, 1998; Klir 2001). However, in contrast to the postmodernists, complexity scientists still believe in mathematics, albeit in a new computational, qualitative, highly nonlinear, form (Capra 1996). They still believe in science, albeit in a non-reductionistic, non-mechanistic, dynamic form. They still believe in rigorous empirical study—although they know that complete description of anything is impossible, both because of the limits of the knower and the methods used. They still believe in theory, albeit in a more meta-theoretical manner. They still believe that science solves problems by providing workable solutions, even if only temporary and partial. And, finally, they still believe in something that postmodernists cannot: synthesis. Contra postmodernists, complexity scientists believe that difference, local knowledge, and complexity are systems phenomena.

Now that we have clarified some of what complexity science is not, we can turn to what complexity science is. The basic viewpoint of complexity scientists can be articulated through a series of tenets, which begin from two different starting points. The first is similar to the theme we have put forth in this chapter: (1) in the last two decades, the complexity of western society has reached a tipping point, (2) this tipping point has resulted in a major phase shift in the organization of western society, including its involvement in the larger global society of which it is an active part; (3) this phase shift is, in large measure, a function of the computer revolution, post-industrialization and globalization; (4) the consequences of this phase shift (e.g., environmental collapse, global economics, cultural and political conflict, etc) cannot be adequately addressed by the normal tools of science; (5) new tools are needed, grounded in a systems perspective and the latest advances in computational modeling and mathematics; (6) complexity science is therefore the future of science (Byrne 1998; Luhmann 1995; Urry 2003).

The second argument emerges from the "cul-de-sac" view of modern science, the idea that scientific inquiry has reached a dead-end (Casti 1994; Capra 1996; Kauffman 1993, 1995; Klir 2001). The basic argument is that: (1) despite its tremendous successes, reductionistic science has "run its course;" (2) likewise, the quantitative program, specifically statistics and traditional mathematical modeling, has reached its limits; (3) new ways of doing science are necessary to move inquiry forward; (4) the best way to do this is by adopting a "complex systems" view of the world; (5) this view is characterized by the idea that life is holistic, self-organizing, emergent, highly relational, dynamic, interconnected, nonlinear, and evolving; and finally, (6) the latest advances in mathematics, networks, informatics and computational modeling give scholars the tools to employ a "complex systems" approach to scientific inquiry (Kauffman 1995, 2000; Wilson 2003; Wolfram 2002).

Despite starting from different points of departure, these two viewpoints arrive at the same place. First, while normal science (characterized as reductionistic and statistical) has achieved a great deal, it is insufficient for addressing the current challenges most researchers face. Second, to address these new challenges, two things are needed: a vocabulary grounded in the study of complex systems and a methodological toolkit created from the latest advances in computational modeling, data mining, qualitative method and mathematics. This is, for the most part, the purpose of complexity science. Everything it does methodologically, theoretically, substantively and organizationally follows.

So, why should sociologists care about complexity science? One good reason is that complexity science is making better usage of the systems tradition than we are, despite the fact that we helped to create this tradition. More important, complexity science offers us ways to effectively address the growing complexity of sociological work.

1.2.7 The New Challenge of Complexity

As in its early days, sociology is once again faced with the challenge to make sense of its complexity. Overnight, it seems, western society has gone through a major transformation in technology, economics, politics, and culture (Castells 2000a, 2000b; Urry 2003). As the 1970s progressed into the 1990s, many western societies began to transform from an industrial-based technology (and economy) to a post-industrial technology (Bell 1974/1999). The computer and related technologies underscoring the informatics revolution changed everything; global capitalism, politics, and culture merged at a new level (Castells and Cardoso 2006). Everything, including science, became more complex and faster (Gleick 2000).

In a manner similar to its formal emergence a century ago, sociology once again has the opportunity to recognize itself as a discipline of complexity. Once again, the growing complexity of western (and now global) society challenges organized sociology: (1) in the epistemological assumptions sociologist hold (Luhmann 1995); (2) the topics they study (Watts 2004); (3) the vocabularies they speak (Geyer and Zouwen 2001); (4) the data they collect (Castellani and Castellani 2003) and the methods they use (Gilbert and Troitzsch 2005); and, adding something new to the list, (5) the changing forms of institutional organization in which they are situated (Abbott 2000, 2001). Let us review these five challenges in greater detail.

First an extended caveat. At this point, the reader may get the impression that our storyline of sociology's complexity makes the same evolutionist error of previous systems thinking, primarily because we assume that the introduction of industrialized modernity and, later, postmodernity (post-industrialism, network society, globalization, etc.) constitute increased levels of complexity in western society. This impression is incorrect. As we explained earlier, evolutionism sees societies on a timeline moving from primitive to sophisticated, simplistic to complex, with some sort of teleology implied. That is, as things progress they get better, hence the affiliated terms: evolutionism and progressivism (Hofstadter 1955). We make no such progressive assumptions. We do, however, consider the data in our favor when we argue that pre-industrial, agrarian, western society is not as complex as modern and, later, postmodern society. By complex, we mean western society and its various parts have become more interdependent and inter-reliant, much faster and chaotic, more interconnected and informed (think information technology), much more quickly impacted globally by localized change, and, most important, more difficult to manage as a system—think international economics, global warming, etc. There also is no particular direction (trajectory) toward which all of this is going. No progress is implied. By "more complex" we also mean that, in moving from industrialized modernity to post-industrial post-modernity, the scientific study of western society has become more challenging, primarily in terms of the amount of information available, the speed at which this information develops and changes, the interdependence of different academic and disciplinary domains of investigation involved in managing and studying this information, and the increasing complexity of the methods needed to study it.

The growing complexity of scientific work brings us back to the five challenges of complexity facing sociology today, which we will review now. First, in terms of the epistemological perspectives of most sociologists, the challenge of complexity comes in the way that the philosophical assumptions of modern scientific practice have been shown to be limited in their ability to conceptualize and model it. Here we are thinking of the past fifty years of critiques made by social constructionism, the sociology of knowledge, the philosophy of science, feminism, postmodernism, neo-pragmatism, and post-structuralism (Best and Kellner 1991; Foucault

1980; Fuchs 1992; Haraway 1990; Rorty 1991a, 1991b; 1998). We are also thinking of the more recent critiques, as we discussed above, made by complexity science (Axelrod 1997; Capra 1996; Castellani, Castellani and Spray 2003; Casti 1999; Cilliers 1998; Gilbert and Troitzsch 2005; Holland 1995; Klir 2001; Klüver 2000; Macy and Willer 2002; Ragin 2000). For most sociologists, the limitations highlighted by these critiques come through a series of well-worn issues, each having to do with the difficulties of studying the dynamics of aggregate social behavior (Bonacich 2004a, 2004b; Ragin 2000; Watts 2004). These issues include the limitations of reductionism and nomothetic explanation, the troubles of deductive reasoning, the restrictions of the linear model of statistics, the difficulties of conceptualizing social reality in terms of variables and independent observations, and the problems of self-reference and representation (Byrne 2001, 2002; Eve, Horsfall and Lee 1997; Geyer and Zouwen 2001; Gilbert 1999, 2000; Luhmann 1989, 1995).

Second, in terms of what sociologists study, one of the best examples of the challenge of complexity is the confounding factor of globalization—which has made just about everything more complex—from managing and controlling businesses as complex organizations (Capra 2002; Richardson and Cilliers 2001) to analyzing and correcting stock markets and economic trends (Mandelbrot 1983, 1997; Mandelbrot and Hudson 2004; Urry 2003) to improving various ecosystems and environmental issues via their relationships with human socio-ecological systems (Capra 2002; Wilson 2003) to addressing the epidemiological dynamics of global information and health networks (Barabási 2003; Watts 2003, 2004). Polity, economics, culture, health care, inequality, work, family, identity; all have become more complex due to the rapid advance of globalization (Capra 2002; Castells 2000a, 2000b; Luhmann 1995; Urry 2003). Globalization also has also led to an increasing interdependence amongst many of the above issues. The way that ecological systems and their problems overlap with economic and political systems is one example. Another is the way in which cultural systems overlap with technological systems (Capra 2002; Castells 2000a, 2000b; Urry 2003).

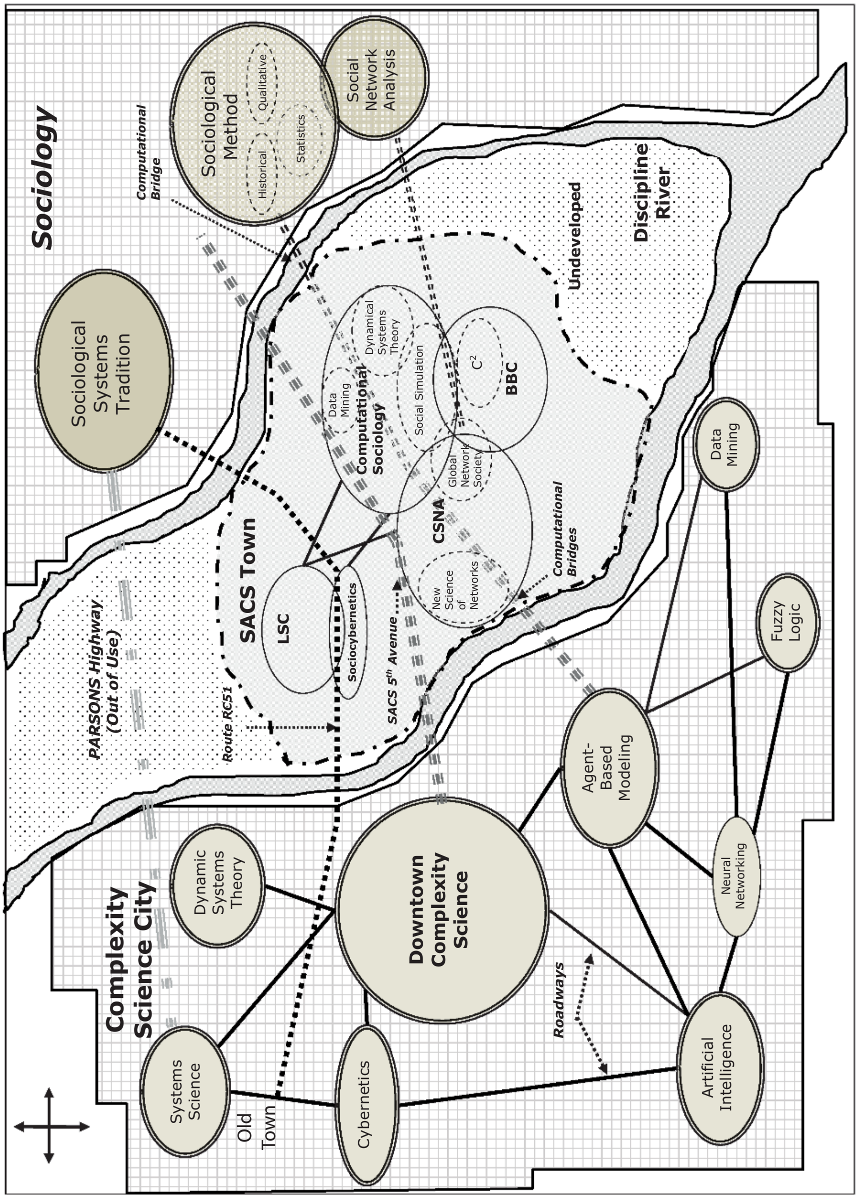
Third, in terms of the language and vocabulary (e.g., concepts and theories) spoken by sociologists, there are numerous examples of complexity's new challenge. Four quick examples illustrate this point. There is the failure of sociology—in the aftermath of Parsons—to arrive at any sort of formal or explicit theory of social complexity or social systems (See Castells 2000a, 2000b). Then there is the inability of sociologists to capitalize on early systems thinkers like Pareto and Spencer. Sociology also finds itself outside the complexity science loop in utilizing concepts such as autopoiesis, emergence, self-organization, along with the grants and funding associated with the analysis of these concepts. Finally, sociology has yet to recognize the value of treating such concepts as social class, formal organizations, inequality, social movements, or collective behavior in systems terms.

Fourth, similar challenges exist in terms of the increasing complexity of method and data. One example (which we have mentioned several times) is the increasingly complex and high-dimensional databases that are emerging through the informatics revolution. While powerful, statistics and qualitative method alone cannot handle these databases; sociologists need other methods. Another example is policy and evaluation research. At present, sociologists lack effective tools for modeling the consequences of small and large-scale policies or programs before they are implemented. Without such techniques as computer simulation, there is no good way to determine, ahead of time, what types of social change various policies or programs might produce. This is particularly problematic given the speed and range of impact that policies and programs have in the smaller, more global worlds in which we now live—think global disease transmission, international economy, bioterrorism, etc.

Fifth, there is the issue of the organization of sociology. As suggested above, the increasing extent to which the topics of science, writ large, no longer can be separated is challenging the disciplinary boundaries of sociology and the tendency for sociologists to become immured within smaller and smaller intellectual cul-de-sacs (e.g., Abbott 2000; Cole 2001). These challenges have led some sociologists to call for some type of trans-disciplinary or post-disciplinary sociology or for new fields of study such as a *social physics* (e.g., Urry 2004).

Given the above five challenges, one can argue, in the spirit of C. Wright Mills that complexity has become the theme that unites the intellectual struggles and sociological imagination of many sociologists.

This is not, however, where our story about the systems tradition in sociology ends, with complexity scientists doing a better job addressing this theme than us. We have one last break to make. This time, however, it is a break filled with hope and promise—the hope and promise of new ways to effectively address the growing complexity of sociological work. We go to the late 1990s. Our destination is a small but growing intellectual community being built on the disciplinary edge of western sociology, a place we call *Sociology and Complexity Science*; or *SACS* for short (See Map 2).



Map 2: SACS Town

1.3 The Emergence of SACS

Sociology and Complexity Science (SACS) is an interstitial community taking root on the "outer banks" of sociology. As shown in Map 2 (See also Chap. 10), SACS resides at a fork in the intellectual river separating sociology from the natural sciences.

This fork (which goes around the eastern, right-hand side of SACS) did not always exist. In fact, it was intentionally created by main street sociologists during the 1970s, in an explicit and concerted effort to literally *wall off* systems thinking and its naturalistic views (think cybernetics, systems science, evolutionism, organcism, etc.) from mainland sociology. The consequence of this effort was that systems thinking (at least in sociology) became an island of intellectual inquiry. In time, the highways and bridges connecting sociology to the natural sciences, and, more specifically, the traditions of cybernetics and systems science (such as the Old Parsons Highway), fell into disarray.

It is on this island that the scholars of SACS took up residence. Why? Just on the other side of this island, as one crosses the intellectual river separating sociology from the natural sciences, is the new city of *Complexity Science*.

The community of SACS is part of what John Urry (2005b) calls the *complexity turn* in the social sciences. As Urry explains, most of the work being done within the SACS community got its start in the late 1990s, around the same time that complexity science was finally gaining international recognition; thanks, in large measure, to the growing prestige of the Santa Fe Institute (Santa Fe, New Mexico, USA), the birthplace of complexity science (Lewin 1992; Waldrop 1992). The scholars of SACS, however, lacked both a name and (as we have already pointed out) a mutual, collective awareness. In fact, they were spread out across Western Europe and North America, working (for the most part) in intellectual and geographical isolation from one another, pursuing diverse areas of study that, at the time, seemed hardly related.

In the late 1990s, these areas included: (1) *computational sociology* (e.g., Gilbert and Troitzsch 2005); (2) *complex social network analysis* (CSNA) (e.g., Watts 2004); (3) *sociocybernetics* (e.g., Geyer and Zouwen 2001); (4) the *Luhmann School of Complexity* (LSC) (e.g., Luhmann 1995); and, (5) the reconstruction of a post-disciplinary sociology grounded in complexity science, which we call the *British-based School of Complexity* (BBC) (e.g., Byrne 1998; Urry 2003).

Interestingly enough, despite this diversity, the agenda of these "sociologically minded" scholars was remarkably similar. The agenda basically was an exegetical restoration of the twin goals of Talcott Parsons. While it is historically accurate to say that Parsons was dead to the majority of western sociologists during the late 1990s, he was historically and exegetically alive within the growing network of SACS scholars. As shown in Map 2, this is why, in terms of its intellectual longitude and latitude, SACS lies just south of old Parsons Highway, which constitutes the first major intellectual thoroughfare connecting sociology to systems science and cybernetics, the original intellectual "downtown" of Complexity Science City, some fifty years ago.

1.3.1 Reinventing Parsons

By the late 1990s, the scholars of SACS were similar to Parsons in two important ways.

First, the scholars of SACS have sought to somehow integrate sociology with the latest advances in cybernetics and systems science. The reader may be wondering why we are talking about cybernetics and systems science instead of complexity science. There are three reasons. First, as mentioned earlier, complexity science is the intellectual outgrowth and, in many ways, the continuation of cybernetics and systems science. In fact, many of the leading scholars in complexity science, such as Stuart Kauffman, John Holland and W. Ross Ashby, are stars in cybernetics and systems thinking. Second, while complexity science gained international fame in the late 1990s, it still lacked an agreed upon name, going by such various titles as the study of complexity, complex systems research and systems thinking. In fact, the most widely acclaimed and definitive review of complexity science published at the time, *The Web of Life* (1996) written by Fritjof Capra, generally refers to this "new" science as systems thinking. Even Lewin (1992) and Waldrop (1992), both of whom wrote the first biographies of the Santa Fe Institute, concede this point, referring to it as the science of complexity, which they define as a cross-disciplinary grab-bag of scholars and viewpoints, all attempting to coalesce into some agreed-upon approach to addressing the growing complexity of scientific work. Third, and most important, many of the early scholars in SACS began their work as far back as the early 1980s, when complexity science was still cybernetics and systems thinking.

For example, during the late 1980s and early 1990s, the noted German sociologist, Niklas Luhmann (1995), was busy at work, integrating sociology with the research of cognitive scientists and leading Cyberneticians, Humberto Maturana and Francisco Varela (1998). Both Maturana and Varela were becoming leading thinkers in complexity science (Capra 1996), particularly in the areas of autopoiesis (a term they coined), emergence, and self-organization (See Map 1). Luhmann's work would lead to the development of new social systems theory, which led to the *Luhmann School of Complexity* (LSC).

Working in close intellectual proximity to Luhmann, various other systems thinkers from the same "post-Parsonian" generation, such as Walter Buckley and Felix Geyer, were busy keeping the sociological systems tradition going, albeit in a very different way. They sought to integrate sociology with second-order cybernetics. Their work resulted in the creation of *sociocybernetics* (e.g., Geyer and Zouwen 2001).

Still other scholars, working primarily in the area of method, would turn to the recent advances in complexity science. For example, during the 1990s, the British sociologist, Nigel Gilbert, worked to integrate mathematical sociology and computer-based simulation with the new area known as agent-based modeling—which is the intellectual outgrowth of cybernetics, second-order cybernetics, distributed artificial intelligence and systems science (See Map 1). The result was the new field called *computational sociology* (See Maps 1, 2, 3 and 8).

In an entirely different field of study (physics) and working with an entirely different set of tools (agent-based modeling, social network analysis and modern mathematics), physicists Duncan Watts and Steven Strogatz sought to solve a major problem in sociology: how people are connected to one another across very large networks (Watts 2003). Their research, known as the small-world hypothesis, led to the emergence of an entirely new field of study, called *the new science of networks*. By the early 2000s, this field would eventually become more sociological, turning into a broader area of inquiry, which we refer to as *complex social network analysis* (CSNA).

And finally, there was a small group of British sociologists, in particular John Urry (2003) and David Byrne (1998), who sought to integrate sociology with complexity science, but in a very different manner. Like all of the above examples, they drew on the same set of theories and methods. Unlike the above examples, however, this new *British-based school of complexity* (BBC) took an entirely different approach to their practice of sociology. Working with the theories of Michel Foucault, Anthony Giddens and Manuel Castells, these scholars integrated complexity science with a post-society, post-disciplinary, mobile-society sociology, and in the process created a very powerful model for doing global sociology from a systems perspective (Urry 2000a).

The second way that the early scholars of SACS are like Parsons is that despite differences, they all thought about sociology's complexity and its five major challenges in systems terms. Luhmann, for example, called his work *new systems theory*. Sociocybernetics is almost entirely comprised of European systems thinkers. Computational sociology was trying to simulate complexity as an agent-based system. CSNA was conceptualizing the structure of complexity in network/systems terms. The BBC was attempting to employ the network/systems perspectives of John Urry and Manuel Castells in order to treat various global topics (e.g., societal mobility, tourism, urban sprawl, etc) as complex social systems (Byrne 1998; Urry 2000a).

Nonetheless, this did not mean that these scholars embraced Parsons or much of the canonical work in sociological systems thinking. For example, while Luhmann not only drew upon the work of Weber, Marx and Parsons, including a brief time spent studying with the latter (Luhmann 1995), others, such as Duncan Watts (trained as a physicist) or Phillip Bonacich (trained as a social network researcher and mathematical sociologist) had no interest in Parsons or any of the canonical scholars whatsoever. Being mathematically trained, both Watts and Bonacich, however, would eventually express an interest in Pareto and the power law, as would Nigel Gilbert and other leading sociologists in SACS, such as Jürgen Klüver (2000).

But we are getting ahead of ourselves. Before we can continue our review of SACS, we need to get our study in order. First, we need to outline the questions upon which our review of SACS is based. Second, we need to discuss the method we used to conduct our review.

1.3.2 Questions for Our Study

During the course of our investigations, we were guided by three sets of questions, which we outline here.

- The first had to do with the composition of SACS. Basically, we wanted to know what SACS looks like today, circa 2008. For example, what do we know about its major areas of research? Which areas are most important? Who are the field's key scholars? Are there any identifiable subfields of study? How are the areas of research positioned in relation to one another? Do any of the areas overlap? What are the dominant trajectories within the field? Is the field stable? Will it differentiate into even newer areas of research? Finally, what environmental forces have had the biggest impact on the field and its major areas of research?
- The second set of questions had to do with the legitimacy of SACS. We basically wanted to know if it is an area of research, field of inquiry, subdiscipline, etc? Also, if it is a legitimate area of inquiry, is there a starting date for its formal emergence? And, if so, what did SACS look

like before and then after this date? Also, in terms of the current composition of SACS, does it meet the structural and intellectual criteria usually associated with an area of sociological study? For example, are its major areas of research well organized? Does it have a publication record comparable to other fields at similar stages of development? Do the scholars and areas of research in SACS have a common identity and a shared knowledge base? Are the scholars and areas of research in SACS connected to one another to the extent one would expect for a field in its first ten years of existence? Does the field have a sense of its boundaries and the environmental forces impinging upon it?

- The third set of questions had to with SACS' position within and impact upon organized sociology. For example, is SACS an extension of the systems tradition in sociology, or is it something else, perhaps something new? Also, what is SACS's role in the development of sociology? Are sociologists finding the work of SACS useful?

1.3.3 Method for Our Study

Our review of SACS is an *historical inquiry* into the emergence of a new field of study. It follows standard methods of demonstration, including "proof by means of historical documentation, quoting other texts, referral to authoritative comments, the relationship between ideas and facts, the proposal of explanatory patterns, etc" (Foucault 1991, p. 33). In this way, our overview of SACS, including our identification of the leading scholars, major areas of research, and the historical unfolding of this field, can be "verified or refuted as in any other history book" (Foucault 1991, p. 33). The data for our study is also standard: published reports, articles, books, websites, biographies, autobiographies, etc.

The quantitative component of our study is equally verifiable and normative. While most of our study draws upon historical archives, our study has a strong quantitative component. Chaps. 7 and 8, which examine SACS as a system, including its evolution over the last decade, draw extensively from a *Web of Science Citation Index* database we built of the Top 25 Scholars in SACS. With this database, we built a citation network, which we analyzed with *Pajek*, a freeware program for analyzing complex social networks (Nooy, Mrvar and Batagelj 2005).

However, while the data and basic techniques we use are normative, our approach to modeling SACS as a social system is new. During the course of our investigations, it became clear that the new community of SACS was best conceptualized as a social system—a formal intellectual system with identifiable boundaries, weak-ties drawing the various (clustered) areas of research together, pioneering scholars, leading institutions, periodicals, and so forth. Given this realization, we decided to employ the SACS Toolkit for our study, given it was created to model social systems. In other words, our book uses the tools of complexity science and the SACS community to study this community as a social system.

We believe that our usage of the SACS Toolkit is advantageous to the reader in two important ways. First, it allows the reader to gain a better understanding of the tools of complexity science and (more specifically) SACS by seeing them in action. Second, it allows our book to function, in part, as a handbook. Upon completion of this book, and along with the information and case studies at our website, readers should be able to use the SACS Toolkit in their own work. We therefore turn to our review of the SACS Toolkit—Chaps. 2 and 3.