Chapter 1

The Revival of Economic Sociology

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Economic sociology is staging a comeback after decades of relative obscurity. Many of the issues explored by scholars today mirror the original concerns of the discipline: sociology emerged in the first place as a science geared toward providing an institutionally informed and culturally rich understanding of economic life. Confronted with the profound social transformations of the late nineteenth and early twentieth centuries, the founders of sociological thought—Karl Marx, Emile Durkheim, Max Weber, Georg Simmel—explored the relationship between the economy and the larger society (Swedberg and Granovetter 1992). They examined the production, distribution, and consumption of goods and services through the lenses of domination and power, solidarity and inequality, structure and agency, and ideology and culture. The classics thus planted the seeds for the systematic study of social classes, gender, race, complex organizations, work and occupations, economic development, and culture as part of a unified sociological approach to economic life.

Subsequent theoretical developments led scholars away from this originally unified approach. In the 1930s, Talcott Parsons reinterpreted the classical heritage of economic sociology, clearly distinguishing between economics (focused on the means of economic action, or what he called "the adaptive subsystem") and sociology (focused on the value orientations underpinning economic action). Thus, sociologists were theoretically discouraged from participating in the economics-sociology dialogue—an exchange that, in any case, was not sought by economists. It was only when Parsons's theory was challenged by the reality of the contentious 1960s (specifically, its emphasis on value consensus and system equilibration; see Granovetter 1990, and Zelizer, ch. 5, this volume) that some of its inherent limitations were recognized and other theoretical models were seriously considered. Although in later work Parsons and Neil Smelser (1956) produced a more integrative sociological approach to the economy, many sociologists from the 1960s to the 1980s shifted to narrower research areas, devoting their attention to specific economic phenomena without making an attempt to arrive at a systematic sociological understanding of economic life. Throughout this period, different subfields within sociology—such as organizations, work and occupations, social stratification, professions, development, and culture—contributed important theoretical insights and empirical evidence related to the study of economic processes.

The sociological study of economic phenomena related to (market) production thrived during the 1960s in the subfields of organizations and of work and occupations. A series of key synthetic books developed a distinctively sociological approach to the study of production and administrative processes in organizations (Blau and Scott 1962; Etzioni 1964; Lawrence and Lorsch 1967; Perrow 1970; Thompson 1967). These new organizational sociologists embraced the study of organizations qua organizations (taking what might be called a Weberian approach, given Weber's interest in the emergence of highly rationalized bureaucracies) and relegated to the background the impact of organizations on group norms and solidarity. (This more Durkheimian approach had been adopted by earlier scholars such as Elton Mayo [1977 (1933), 1988 (1945)], Chester Barnard [1938], and F. J. Roethlisberger and William Dickson [1967 (1939)]). The new sociologists asked questions about the beneficiaries of organizations (Blau and Scott 1962), the dominant control system (Etzioni 1964), the impact of the environment on organizations (Lawrence and Lorsch 1967), and the impact of technology (Thompson 1967; Perrow 1970). Still other sociologists pursued the Weberian agenda by studying authority and stratification systems from a cross-national, comparative perspective (Bendix [2001 (1956)]).

Over the years, however, this organizational subfield drifted apart from sociology, in terms of both theoretical concerns and academic organization. While the Weberian scholars of the 1960s and early 1970s had something to say about society in general, the new organizational theorists of the 1970s and 1980s focused most of their research energies on for-profit organizations (on nonprofits, see Powell 1984). Their theories—resource dependence, transaction-cost economics, and population ecology—established few connections with other sociological areas (for surveys, see Perrow 1986; Scott 1998 [1981]). The new organizational sociology became known as organizational theory, and organizational sociologists increasingly moved from sociology departments to business schools (Gans 1990). More recently, however, efforts have been made to reintegrate organizational theory into other areas of sociological inquiry. For example, the new institutionalism in organizational sociology (see Powell and DiMaggio 1991) has built on, and contributed to, the sociology of culture. Also, some organizational sociologists have established a dialogue between organizational and social stratification research (see Baron 1984).

In addition to studying production inside organizations, sociologists have long studied work, labor, and occupations from the point of view of social stratification. This sociological subfield draws insights from Durkheimian, Weberian, functionalist, Marxist, feminist, and critical perspectives, and it has generated a wealth of empirical research on phenomena such as social mobility, occupational hierarchies, status attainment, and income and wage distribution (see Grusky 1994). Although of all the social sciences since the 1960s sociology has contributed the most extensive and systematic body of theory and evidence regarding gender and racial stratification, these are aspects of economic life that the new economic sociology of the 1990s and 2000s has thus far marginalized (see the contributions to part 3 of this volume). More recently, sociologists interested in social stratification have looked at household work, including the work of caring for children, as an area of production heretofore neglected by the social sciences. (Some argue that this neglect is due to the fact that household work is usually done by women and not paid for through formal market processes; see Zelizer, ch. 11, this volume; Milkman and Townsley 1994.) Some scholars have also sought to analyze stratification in the context of complex organization (Baron 1984; Baron et al., this volume).

The sociology of work and occupations developed fairly independently of organizational theory and social stratification, notwithstanding Reinhard Bendix's (2001 [1956]) early call for a more integrated approach. Much of the early research adopted a Durkheimian perspective, while later work was inspired by theories of monopoly capital (see, for example, Braverman 1974; Burawoy 1979; Edwards 1979). More recently, sociologists of work have returned to a comparative study of work and occupations, mostly from a multidimensional Weberian perspective (for example, Cole 1979; Lincoln and Kalleberg 1990). Meanwhile, the sociology of the professions has grown in importance and prominence, with scholars drawing from the functionalist (Wilensky 1964), Marxist (Larson 1977), knowledge-based (Freidson 1986), and ecological perspectives (Abbott 1988), among others.

Besides the sociology of organizations and social stratification, the sociology of development is another specialty that has devoted systematic attention to the study of economic processes, especially those having to do with industrialization and economic growth. Sociological studies of development have sought to understand the social and political bases of economic growth and development. Early students of development echoed the Parsonsian approach in their emphasis on the shift from traditional to modern values and on the gradual transformation of authority structures, and they offered fairly optimistic forecasts of the prospects for development around the world (see, for example, Kerr et al. 1964 [1960]). Others, by contrast, painted a bleaker view of development prospects in using Marxist theory to highlight the dependence of developing countries on the core industrialized and advanced countries (for example, Frank 1967; Cardoso and Faletto 1979 [1973]). These ideas were later elaborated by Immanuel Wallerstein (1974) in his highly influential "world-system" approach, which emphasizes states rather than social classes, and by Peter Evans (1979), whose "triple alliance" theory (of local, state, and foreign capital) combined both the class-based and state-centric approaches. Today's global economy has fueled the theoretical debates among modernization, dependency, and world-system scholars (Guillén 2001a, 2001b).

Culture is the fourth key subfield that has examined economic processes. During the 1960s and 1970s cultural anthropologists made major contributions to the sociological study of economic life. In fact, we find in this early work, particularly in studies that examine economic action in developing countries, concepts that have become central in economic sociology. For example, the term "embeddedness," introduced as a relational concept by Clifford Geertz in *Peddlers and Princes* (1963, 30, 89, 153), was later turned into economic sociology's most celebrated metaphor by Mark Granovetter (1985), who was also inspired by the work of Karl Polanyi. Furthermore, in his vivid description of the Javanese approach to credit, Geertz highlighted the fundamentally important interaction between economic and social relations and activities: he wrote, for example, that the "main function" of the Javanese approach to credit "is not simply to capitalize trade but to stabilize and regularize ties between traders, to give persistence and form to commercial relationships" (Geertz 1963, 39), and he noted that the Javanese peddler engages in "trading coalitions" and "alliances" (Geertz 1963, 40).

Thus far, we have examined the study of economic phenomena related to production and trade. Sociologists have traditionally devoted less systematic attention to consumption and leisure, although studies of consumption have appeared as far back as the early twentieth century—for example, Thorstein Veblen's (1994 [1899]) work on "conspicuous consumption" and Georg Simmel's (1957 [1904]) studies of fashion. During the 1950s Paul Lazarsfeld made major contributions to the study of public opinion, mass communication, and consumer behavior (see Frenzen, Hirsch, and Zerrillo 1994). Although no subfield in sociology specializes in the study of consumption, several have contributed insights to this important social process. For example, social history and Marxist approaches have long produced fascinating studies linking consumption patterns to social processes (see Frenzen et al. 1994). More recently, sociologists interested in culture and social networks have reawakened interest in the study of how consumption and tastes are socially embedded and consequently how they contribute to social reproduction. The work of Pierre Bourdieu (1988 [1984]) and Bourdieu and Jean Claude Passeron (1990) is perhaps among the most influential and novel in this respect (for a survey, see DiMaggio 1994). Sociologists have also enriched the study of consumption by paying attention to leisure as yet another manifestation of commercialized consumption (for a review, see Biggart 1994).

Renewed interest in economic sociology as a research area in its own right did not develop until the mid-1980s, despite attempts to stimulate interest in the field during the 1970s and early 1980s (see, for example, Smelser 1976; Stinchcombe 1983). In part, the revival of economic sociology was galvanized by the publication of several influential pieces, including Harrison White's paper on the sociology of markets (1981), Ronald Burt's *Toward a Structural Theory of Action* (1982; see also Burt 1992), and Mark Granovetter's paper on social embeddedness and economic action (1985). Other contributions followed quickly, such as Paula England and George Farkas's (1986) synthesis of economic and sociological approaches to the family and labor markets; Gary Hamilton and Nicole Biggart's (1988) paper on markets, culture, and authority; Viviana Zelizer's (1989a, 1989b) work on the meanings of money; Peter Evans's *Embedded Autonomy: States and Industrial Transformation* (1995); and Mary Brinton and Victor Nee's (1998) use of rational-choice theory to study institutions. Arguably, these pieces struck different notes, but the seeds were planted for the revival of economic sociology, this time built on theoretically sound and empirically useful foundations. The publication in 1994 of the *Handbook of Economic Sociology,* edited by Neil Smelser and Richard Swedberg and sponsored by the Russell Sage Foundation, consolidated the field of economic sociology.

Formal recognition of this renewed interest has also increased. For example, the Society for the Advancement of Socioeconomics was founded in 1989. (It is more narrowly focused on community-related issues than on economic sociology per se.) The American Sociological Association recognized a section on economic sociology in 2000. And there appears to be increasing acknowledgment of economic sociology within the popular press—for example, *U.S. News & World Report*'s yearly evaluation of graduate programs now includes a ranking of the best programs in economic sociology.

New Directions in Economic Sociology

Economic sociology is the study of the social organization of economic phenomena, including those related to production, trade, leisure, and consumption. These phenomena, which are not necessarily mediated by monetary payments, can be observed at various levels of analysis—namely, the individual, the group, the household, the organization, the network, the market, the industry, the country, and the world system. Although economic sociologists differ in their theoretical emphases and empirical techniques (see Fligstein, this volume), they share an interest in both processes and outcomes, and in inequality as well as efficiency.

In particular, economic sociology seeks to understand economic phenomena in their social and cultural contexts, without falling into the trap of three fallacies common to economic analysis. The first fallacy is that the social is a realm separate from the economic. (As mentioned earlier, this fallacy was perpetuated not only by economists but by Parsons within the field of sociology.) Economic sociologists argue that all economic activity is socially grounded and enabled (Swedberg and Granovetter 1992), and that no economic phenomenon can be assessed without the shared understandings (culture), institutional structures, symbols, and networks of inter-actor relationships that concretize it and give it form. The market is seen as a social and cultural product: market exchange is facilitated by social and cultural processes that provide market participants with shared understandings (in the forms of values, norms, and symbols) that help them to make sense of what goes on and how they should act. Economic sociologists also reject the notion that the social or cultural dimensions of society "interfere" with the smooth functioning of the economy (Zelizer 1989a, 1989b; see, in this volume, Granovetter; Fligstein; DiMaggio; Zelizer, both chapters).

Economists generally believe that individuals make conscious calculations about how to maximize utility, and that the preferences that determine their utility functions (that is, the sources and associated magnitude of their utilities) are exogenous to the models of interest. Economic sociologists consider this view, however, a fallacy. Drawing on a rich variety of anthropological, ethnographic, social-psychological, psychoanalytic, linguistic, and sociological research, economic sociologists see both preferences and actions as fundamentally connected to and affected by cognitive biases, limited powers of reasoning, nonconscious and ambivalent feelings, role expectations, norms, and cultural frames, schemata, classifications, and myths (see, in this volume, Granovetter; DiMaggio; Bielby and Bielby; Reskin). Hence, both utility maximization and the isolation of strictly "economic" variables are unacceptable to economic sociologists, since such reductionism necessarily hinders the understanding of economic phenomena. It is not just that these reductionist assumptions include the determination of preferences as part of what they seek to understand. Economic sociologists also recognize that social forces often affect reasoning in ways that defy a strict rationality assumption, and thus they dismiss economists' belief that knowing an individual's preferences (even if exogenous) and immediate constraints leads to unambiguous sequences of decisionmaking or action.

A final key theoretical difference is that economic sociologists reject the idea that the aggregation of individual-level behavior is straightforward and unproblematic. Drawing on previous research on social classes, social movements, social networks, power dynamics, and cultural blueprints (see, in this volume, DiMaggio; Burt; Portes and Mooney; Eckstein), economic sociologists seek to improve our understanding of economic behavior at levels of analysis higher than that of the individual or the group. To better explain aggregate processes and outcomes, they consider sociological concepts such as ideology, consciousness, collective action, neighborhood effects, trust, unintended consequences, decoupling, latent functions, and interaction rituals. Moreover, a sociological approach to economic phenomena pays attention to how social class, gender, and race mediate in the process of aggregation of individual decisions and actions generating patterns of inequality. The sociological contribution here is that all action—whether driven by interests, power, or trust—results in outcomes that are shaped not only by the individual actor's motives but also by larger social, cultural, and institutional structures (see Granovetter, this volume).

It is important to note that economic sociologists have produced theoretical insights about economic phenomena that represent either complementary or competing explanations to those proposed by the traditional rational-choice, utility-driven economic models. The following is an illustrative list of sociological contributions to key debates about economic action:

- *Culture:* Sociologists emphasize that economic action *cannot proceed* without shared understandings about appropriate behavior in a given social setting. Cultural understandings lie at the core of economic action because they provide stability and meaning; these shared understandings help actors make sense of the situation, develop strategies for action, and adjust their expectations and behavior as they interact with others (Hamilton and Biggart 1988; see also, in this volume, Granovetter; Zelizer, ch. 5). Economic sociologists invoke values and norms, network structures, the state, and ideologies as factors creating shared understandings among participants in the various arenas in which economic activity takes place, including markets, groups, organizations, and households (Evans 1995; Fligstein, this volume). It is argued that the resultant social structure helps participants search for a niche in which they develop an identity and adopt a certain set of roles (White 1981; White, this volume).
- *Networks and social capital:* Economic action can be facilitated or hindered by the actor's position in a network of relationships. Many economic sociologists invoke the concept of social capital to explain why some actors are more successful than others at mobilizing resources or attaining their goals (Burt 1982, 1992; Coleman 1990). Scholars have defined and assessed social capital at various levels of aggregation, including the individual, the group, the organization, and the country (see, in this volume, Burt; Portes and Mooney). Economic sociologists have also explored the interdependent mechanisms of this construct, pointing out that one actor's social capital may be another's social exclusion—an argument that brings power, interest, and discrimination into the analysis of economic action (see, in this volume, Baron et al.; Portes and Mooney).
- *Trust:* Empirical evidence has shown that economic action is often not based on a self-interested assessment of incentives, as argued by economists. Instead, it is often based on trust, which is historically developed and culturally specific, although not exclusive to any one culture (Dore 1983; Geertz 1963). Economic sociologists are keen to point out that trust helps to explain the observed order, stability, and continuity in social life that occurs because of

genuine emotional caring or norms of obligation that bind actors in spite of their incentives (see Granovetter, this volume).

- *Effort and motivation:* Economic sociologists have pointed out several key factors that shape commitment and effort at work apart from the traditionally considered structure of material incentives. For instance, Bendix (2001 [1956]) demonstrated that blind obedience driven by economic incentives is not sufficient for organizations to accomplish their work. Good faith and initiative are required if complex organizations are to respond to unforeseen circumstances and opportunities—subordinates must "comply with general rules as well as specific orders in a manner which strikes some reasonable balance between the extremes of blind obedience and capricious unpredictability" (204)—and these qualities are guided by a culture, ideology, or ethic of work and effort. More recently, economic sociologists have revisited this important topic, and there has been renewed interest in social-psychological processes and group dynamics, as well as a better appreciation of the complexity of human motivations to work hard (see Bielby and Bielby, this volume).

These theoretical contributions have expanded the variables considered and led economic sociologists to recognize the complexity and interdependence of social and economic elements. They have also extended the type of economic activities studied. Scholars in this emerging field have called for more systematic attention to economic activity not done for money or in the market, including household work, gift giving, and volunteer work (see Wilson 2000; McPherson and Rotolo 1996; Biggart 1994; England and Farkas 1986; Zelizer, ch. 5, this volume), as well as collectivist types of organizations (Rothschild-Whitt 1979). This interest is in keeping with the main sociological contribution to the study of economic phenomena: showing that economic action is, after all, a form of social action.

The Plan of the Book

The contributions to this volume seek to define the field of economic sociology, take stock of its accomplishments to date, identify theoretical problems and opportunities, and formulate strategies for empirical research in each of the key topical areas of the field. There are many ways to group research in this field, and the goal is not to divide academic efforts; as we have seen, rigid boundaries are academically counterproductive. We have chosen to group the chapters into four topical parts. The first presents the main debates and conceptual approaches in economic sociology, with contributions by Mark Granovetter, Neil Fligstein, Paul DiMaggio, and Viviana Zelizer. Harrison White's and Ronald Burt's chapters in part 2 look at the study of economic phenomena from a network perspective. Part 3 considers the role of gender in economic sociology, with chapters by William Bielby and Denise Bielby; Barbara Reskin; James Baron, Michael Hannan, Greta Hsu, and Ozgecan Kocak; and Viviana Zelizer. The fourth part presents the study of economic development and change from a sociological perspective, with contributions by Alejandro Portes and Margarita Mooney, and Susan Eckstein. While this volume is not a comprehensive treatment of the field, it illustrates the various ways in which sociology can be used to understand and explain economic phenomena.

Major Debates and Conceptual Approaches in Economic Sociology

The first four chapters seek to define the boundaries of the field and to identify and develop opportunities for theoretical and empirical development. In his chapter, Mark Granovetter asks: What is distinctive about economic sociology? He submits the notion of "instrumental interests" to close analysis, pointing out that utilitarian theory decontextualizes actors and interests from the social relationships in which they typically exist. In ordinary social interaction, people act out of mixed motives, sometimes explicitly seeking gains, but also responding to social pressures, even those without obvious carrot-and-stick features. It is possible, of course, for individuals to make instrumental use of non-instrumental relations—for example, to cultivate social ties in order to exploit them for business purposes. But, as Granovetter notes, even in economic relations, social trust is most effective when it is not strategized and manipulated but instead operates autonomously. This observation undermines the utilitarian interpretation of social capital. At the same time, Granovetter argues, we should avoid going to the opposite extreme from the selfish rational actor and falling back into the oversocialized view of actors as completely dominated by norms.

These considerations show the limits of a micro focus on the problem; the issues of interests and agency vis-à-vis social obligations and ties are better understood if we view these "from the air"—that is, from a structural perspective. Both self-interested action and the character of culture are shaped by the type of network structure in which they operate. Thus, preexisting networks allow or prohibit various kinds of entrepreneurial action; one cannot engage in brokering across holes unless holes happen to exist.

Granovetter schematizes three kinds of networks: highly decoupled networks, in which each unit forms interests that conflict with those of the other units (for example, firms with strong barriers between them, like Boston's Route 128 high-tech firms); weakly coupled networks with enough weak ties to allow entrepreneurs to bridge holes and amass power; and strongly coupled networks (such as the fluidly bounded Silicon Valley firms) where personnel and information flow so rapidly that a cooperative shared culture overrides conflicting interests while allowing little opportunity for brokers to seize an advantage. This way of putting it makes it seem that the network structures are static background determining what can happen. But dynamics can be introduced into the model, especially by engaging in historical studies of the development of industries (such as Granovetter's current study of the electric power industry). Entrepreneurs do not just passively fall into situations where there are holes to be bridged; sometimes they pursue strategies to keep the holes from closing up behind them or to keep rivals from coming in and following in their paths. Such strategies may require that entrepreneurs operate on the level of several networks at once, bringing political and regulative networks to bear or constructing new kinds of financial ties.

Granovetter thus points us toward a new and more complex phase of network analysis: looking not merely at the immediate network and the presence or absence of structural holes within it, but at the relationships between several kinds of networks and at the special place of actors who can coordinate multiple ties. This is a way of translating into network terms the distinction, implicit elsewhere in economic sociology, between the institutions in which a market is embedded and the structure of the ties that constitute the market interaction itself.

Neil Fligstein's chapter ambitiously defines the scope of the field, lists our main accomplishments so far, and challenges us to move onward. Fligstein suggests that economic sociology has become prominent because of the way in which it brings together many sociological subdisciplines. If we define the field as the study not merely of markets but of all aspects of the organization of material production and consumption, then it provides a unifying perspective on households, labor markets, stratification, networks, and culture, as well as on product markets. Nevertheless, in keeping with the central interest of much of the field up to now, Fligstein narrows his focus to the sociology of markets as the most prominent mechanism of allocation. (For a wider perspective, see Zelizer's chapters in this volume.)

Fligstein adopts Harrison White's influential model of markets as a particular type of self-reproducing role structure, organized by firms that monitor one another to decide how much to produce and the quality level of their product, thereby finding profitable niches that enable them to stay in business. It should be added, in keeping with White's emphasis, that markets are variable and sometimes break down and fail to sustain an array of recognized producers; this variability in the reproducibility or sustainability of role structures in markets gives us empirical leverage for comparisons and thus for ferreting out the mechanisms by which markets are socially constructed. Fligstein goes on to note that we should explain action in markets as we do action in other spheres of social life, and hence our general theory of how society operates should apply here. In fact, we have a variety of such general theories; applied to markets, these theories give us five mechanisms: embeddedness in networks that generate trust; shared meanings or cultures in local or national arenas; institutional rules for property transactions; government power and conflicting political interests; and control by economic elites, as seen by class analysis.

Fligstein challenges economic sociologists to stop relying on conventional economic theory as a foil and to develop their own theoretical mechanisms. We have come through the first phase of economic sociology, having shown the kinds of phenomena that are exceptions to a neoclassical, utilitarian view of markets or the kind that are taken for granted as underpinnings if such markets are to operate. As we feel our way into a second phase, however, we are becoming dissatisfied with our existing arguments. Fligstein issues the challenge: If utilitarian economic theory follows the basic principle that markets operate to allocate resources most efficiently, are economic sociologists really saying anything different? In fact, many economic sociologists also hold that the social mechanisms underpinning markets promote efficiency; social relationships, for instance, in promoting trust, reduce the cost of doing business, and various forms of network structure break down boundaries between traditional firms, thereby making it less costly to enter and exit new markets. Far from challenging orthodox economics, many economic sociologists merely turn up new social patterns for economic theory to explain as Pareto-optimal.

Fligstein advocates focusing instead on the distinction between market "efficiency" and "effectiveness" in the sense of how organizations (and entire markets) manage to survive. This focus brings out a point implicit in some sociological analyses of what social relationships do for market transactions: they provide long-term legitimacy, resources, or other bases for social stability, as opposed to short-term profits. To put it another way, the utilitarian view of market efficiency fails to explain when there is selective pressure for long-term sustenance of market structures and when there is pressure for short-term profit. This is the challenge, and opportunity, for economic sociology—to develop just such a theory.

Fligstein further notes a key methodological problem in our research: we typically show the presence of social relationships in markets and then assert our interpretation that these are what generate legitimacy, social capital, and the like; we fail to show, however, what kinds of social relationships have what kinds of consequences. In other words, economic sociology needs to avoid falling into a contemporary version of functionalist comparative statics and instead to get beyond generalized categories of social capital or trust and become more focused on dynamics. Research on capitalist elites, interlocking directorates, and financial ties illustrates the problem: such analysis focuses on a static description of capitalist control, and misses what determines shifts in the way markets are structured. As emphasized classically by Schumpeter, and currently by Harrison White, markets as structures of niches are differentiated, both by type of product (which gives the entire market its social identity) and by quality niches within markets. It is this variety of markets and niches that we need to explain, not just market sustainability in general. And given the pressures of contemporary "postmodern" markets toward product turnover and niche proliferation, it is essential for us to show the mechanisms that create new product markets and niches, sustain them, control or monopolize them, and change them into new forms.

Granovetter and Fligstein recognize that in showing not only how various kinds of network structures shape and limit different kinds of economic action but also how new network patterns emerge from older ones, a key problem is network dynamics. Chapter 4 by Paul DiMaggio is concerned with just such dynamics. DiMaggio adopts Keynes's term "animal spirits" for the shared moods that propel economic swings—the emotions of business confidence, buoyancy, or indeed "irrational exuberance" and, in the other direction, pessimism, depression, or panic. Researchers measure these types of emotional confidence among consumers, executives, and investors; the theoretical problem is to show how these operate as collective phenomena over time.

The general principle is that animal spirits are not antirational per se but a response to situations in which information as to the behavior of others with whom one is interdependent is characterized by genuine uncertainty. Connecting this phenomenon with network configurations, we can say that decoupled networks increase variability in expectations and volatility in outcomes. DiMaggio then draws on several theoretical notions to explore how these collective moods rise and fall. Pluralistic ignorance, such as overestimating peers' propensities to certain kinds of behavior, reflects a particular kind of decoupled network, in conjunction with an official level of communication that defines reality in a misleading way. Somewhat opposite to pluralistic ignorance is the bandwagon effect: the propensity of participants to rush into conforming with whatever direction the group as a whole seems to be heading. Bandwagons occur quite differently depending on whether the distribution of propensities to join the bandwagon is a continuous normal distribution or is discontinuous or bimodal. The latter limits bandwagons to particular social regions. In our view, the source of these distributions of propensities remains a problem for which we must account. The answer might be found further down on the micro level of interpersonal encounters, conceived as interaction ritual chains that generate higher or lower emotional energy as actors interact with greater or lesser focus, equality, and deference.

An integrative view comes from seeing economic waves in markets as social movements. Both have qualities of emergent social constructions as participants come to redefine their identities and expand their roles. Both depend on recruitment through social networks. DiMaggio adds that the emotional aspects, the so-called animal spirits, are an additional feature making participants in commercial transactions feel a common identity, more generalized trust, and hence more willingness to purchase optional and expensive goods and services, which they would ordinarily acquire only through a close personal network.

In chapter 5, Viviana Zelizer makes an impassioned plea that economic sociology include a consideration of culture—that is, topics often seen as more cultural than economic. Revived in the 1980s, this subfield focuses on meanings, symbols, practices, and beliefs. Zelizer is anxious to see intellectual cooperation between economic and cultural sociologists and a sharing of turf. The economic is about all forms of production, distribution, and consumption, but economic sociologists often limit their subject to only what goes on in firms and markets. They exclude from their agenda production and distribution in the household and the economic transfers associated with intimate relationships, ethnic enclaves, and consumption.

This tacit delimitation of subject matter rests on several unexamined assumptions. In part, Zelizer thinks economic sociologists are imitating economists' typical focus on firms and markets. But there is also a gendered subtext to where the tacit boundaries are drawn by both sociologists and economists. Arenas of life seen as "women's sphere" (household, family, relationships, love, consumption, neighborhood) are off the map. It is disproportionately women scholars who have contributed to these "off-map" areas of study. Perhaps this is why, given these boundaries, so few of the leading scholars in economic sociology are women. These supposedly non-economic areas of life have been seen as appropriate topics for cultural sociologists and as part of the marginalizing of cultural sociology. Zelizer wants to break down these boundaries so as to look at the economic aspects of the household, intimate relationships, and consumption and examine the cultural aspects of what goes on in firms and markets. Here she joins feminist scholars across disciplines in arguing that seeing the world as divided into a commodified public realm and a private realm where love governs is a distortion of both realms that ignores their many interpenetrations.

The neoclassical economic paradigm focuses on rational individual choice within constraint. Zelizer itemizes three ways in which economic sociologists differentiate their approach from that of economists. First, they may use the same paradigm, but they extend it to subject areas that economists usually ignore, as in Gary Becker's work on the family and on addiction. Second, they talk about the social context in which individual decisions are made. Here they are adding a social element to constraints that economists are more likely to see as consisting in prices and laws. Third, they search for alternative descriptions and explanations of economic phenomena, challenging the focus on individual decisions within constraint.

Zelizer favors the third approach. She notes that economists can incorporate values into their paradigm by seeing values as the content of individual utility functions—the preferences that economists assume individuals use to decide what goals to pursue. Economic man or woman takes the goals determined by values or tastes, considers the constraints, and rationally calculates an optimal strategy. Zelizer emphatically rejects this limitation of the role of values. For her and other cultural sociologists, symbols, beliefs, and meanings also affect which strategies are taken for granted, which are seen as rational, the boundaries around arenas where people believe it is appropriate to use rational calculation, and the meanings that people give to constraints and strategies.

Zelizer believes that cultural sociology provides one set of tools that allow economic sociologists to challenge economists' paradigm. The major orienting concept she offers is that of differentiated ties.

Social ties are of different types, and the type of social tie involved in a transaction carries a host of meanings and beliefs about appropriate behavior and the kinds of movements of money that are appropriate. The concept of differentiated ties suggests a research agenda in which economic behavior is affected by gender, race, and other similarities between interactants that define the meaning of their ties. It also suggests research that connects with another prominent theme in economic sociology, that of social networks. Networks of informal ties can be classified according to the nature of the tie as it is conceptualized in cultural meanings.

Social Networks and Economic Sociology

The structural approach to economic sociology—and especially its social network variant—has made astonishing theoretical and empirical inroads over the last twenty years. Harrison White and Ronald Burt are among the most important contributors to the structural approach to economic sociology. In chapter 6, White summarizes his theory of markets, which he first set forth in his paper "Where Do Markets Come From?" (1981) and has elaborated in his book *Markets from Networks* (2001). White's theory has already been influential for economic sociology; using a network approach, it focuses not on the institutions and social relationships in which markets are embedded but on the inner network structure of the market itself. The theory is especially important for providing a predictive model of the parameters that determine the variety of different product markets that can exist. For our purposes, it is especially suggestive because it puts the dynamics of markets at the center of analysis; each region in the "state space" that he describes has its distinctive form of competition and distinctive stabilities or instabilities.

White's basic processes include niche-seeking identity construction by joint action and signaling, driven by producers' commitments rather than consumer demand. Seeking distinctive market niches is the key to economic action. Niches make it possible to avoid head-to-head competition and thus to make profit (similar to Schumpeterian entrepreneurs). Producers construct niches on two levels. On one level, they examine their competitors to find out not only what those competitors have been doing successfully (that is, where competitors have been finding customer demand) but where they can do something dissimilar enough to constitute a distinct niche. On yet another level, the entire industry or market molecule constitutes a product line, an identity amid other markets. Thus, producers depend on their peer rivals and must monitor them, and they rely on being monitored by others as part of such an array. The dynamics of markets are driven from the producers' side; producers do not respond to demand but attempt to anticipate it by judging their competitors. (Again, this may be seen as an extension of Schumpeter on entrepreneurs risking new combinations.) Producers commit to volumes of production into the future and thus have more of a stake in a given market than buyers.

The simplest version in White's model is a grid of the flexibility of supply (producers' cost) and demand (buyers' need or willingness to buy) for the two dimensions of volume and quality. Unlike conventional microeconomics, White's model emphasizes the flexibility of the adjustment of producers and buyers as volume or quality goes up. At the core of White's scheme is a fourfold space, that is, four quadrants that depend on two relationships (see figure 6.1): first, whether buyer demand for volume is greater or smaller than supplier cost per volume; and second, whether buyer demand for quality is greater or smaller than supplier cost by quality. In the first symmetrical region of figure 6.1 (lower left), increases in both volume and quality give the upper hand to buyers. In the upper right region (also symmetrical), the upper hand is held by producers for both volume and quality increases. The other two quadrants (lower right and upper left) are skewed or asymmetrical and present peculiar problems in sustaining markets. Each quadrant has its distinctive dynamics. Near the pure competition line, buyers pay no attention to differences in quality; they are still arrayed by volume cost, but all receive the same price, thereby eroding producer commitment. Thus, pure competition markets cannot sustain themselves and tend to shift to nonmarket forms, such as putting-out arrangements or hierarchy. This represents an alternative argument to Williamson's transaction-cost theory of the interplay between markets and hierarchies.

White's model also refers to upstream-downstream orientation. The state space is easiest to explain in a simple version that concentrates only on downstream market orientation. (Such an orientation was taken for granted in the earlier discussion, as it is in most microeconomics.) In reality, firms lie somewhere in a production chain of suppliers and customers, with some serving as the edge markets that deal with either the ultimate consumers or the raw material producers. Thus, a firm can choose a direction in which to be oriented, but bounded rationality tends to make it difficult to orient both ways at once.

Orientation is in the most problematic direction; the unproblematic direction is left to habitual ties (which become the most stable part of the network) or passive pricing. An upstream orientation may be produced by inflation or war shortages; in such a case, downstream prices are left as fixed or customary and firms focus on coaxing suppliers. Empirically, this orientation is indexed by the size and prestige of sales and advertising departments vis-à-vis procurement. Thus, there are dual-state spaces, for upstream and downstream orientations. The computational model shows where market regions in state space, favoring an upstream or downstream orientation, differ in their sustainability and profitability.

White's model also includes profits and prices as variables. Price is a by-product of market equilibrium: the stable array of niches in state space that constitutes a market. Price variations can be predicted from the four basic quality-volume and demand–producer cost parameters. Profitability is predicted by closeness to the diagonal through the symmetrical quadrants. (In other words, the maximal profit line is the diagonal in which the demand-cost of the production ratio goes up at exactly the same rate for both volume and quality.)

White's theory provides an entirely new method for analyzing the internal dynamics of markets and thus proposes an economic sociology alternative to mainstream economic theory, even proposing a new theory of prices and profits. It remains to be shown, of course, how the model may be worked out in detail, empirically tested, and implemented. The very existence of this kind of theoretical model, in however schematic a form, should make us optimistic, however, that the second phase of economic sociology is finding tools with which to decipher the varieties and dynamics of economic networks.

Like Harrison White, Ronald Burt (chapter 7) seeks to show how social relationships affect competitive dynamics. His chapter outlines the theory of structural holes, which makes an important contribution to economic sociology because it locates competitive advantage in social relationships rather than in attributes of firms such as capabilities, strategies, and market position. The theory is also important because it cuts across levels of analysis and can be applied to industries, firms, and individuals. Moreover, it allows quantification of the relative advantage (or disadvantage) of actors based on their network ties. And finally, Burt's theory is important because the core proposition—that advantage is found in sparse rather than in dense networks—is not obvious. In Burt's language, "structural holes . . . create a competitive advantage for an individual whose relationships span the holes" (this volume, 155). Information-hole spanners enjoy two kinds of advantages over people who inhabit smaller but denser networks: access to more information, and being better positioned to engage in brokerage and hence influence the outcome of transactions.

Burt's argument goes further by asserting that having the right connections—connections to people who are otherwise unconnected—constitutes a form of social capital. Social capital arising from social location is, in Burt's view, a "contextual complement" to more mundane forms of human capital such as education and skills. Social capital of this type is an asset that benefits individuals and groups. But whether social capital that benefits individuals is also a social asset that improves the efficiency of institutions and benefits society is less clear. Burt is mute on this point. His position is in sharp contrast to those of Robert Putnam (1993) and earlier political scientists like Edward Banfield (1958), for whom social capital lay principally in institutions and associations.

Burt presents evidence that advantages accrue to individuals and groups whose networks are rich in structural holes. To illustrate: structural holes promote career advancement and team performance, accelerate task completion, increase the probability that early-stage investments will advance to the initial public offering (IPO) stage, and foster organizational learning. Some of this evidence is drawn directly from network data, and other evidence is inferential because it is based on observed associations of contact diversity with the advantages accruing to individuals and groups. But evidence about the impact of structural holes on entrepreneurial behavior, which in some respects is at the core of the structural hole argument, remains limited: "Although an obvious site for research on the network forms of social capital, quantitative research on networks in entrepreneurship has been limited to the most rudimentary of network data" (this volume, 174). Network research on entrepreneurship will have to overcome the challenges of comparing successful entrepreneurs with failed entrepreneurs (or people who never attempted entrepreneurial careers) and, in particular, the challenge of comparing the holes surrounding people who subsequently succeed as entrepreneurs with the holes surrounding those who are less successful.

Burt's pioneering may lead to further theorizing about structural holes. The structural hole argument in its barest form is that position confers advantage. But position confers advantage only if two events mediate structural holes and advantage: people recognize that they occupy a potentially advantageous position, and they pursue this advantage. The Burt formulation of structural holes thus becomes something like: (1) occupy hole position; (2) recognize potential advantage of hole position; (3) pursue advantage; (4) realize advantage. Looking at structural holes this way raises the question of whether a different formulation is possible: (1) recognize advantage of hole position; (2) pursue hole position; (3) occupy hole position; (4) realize advantage of hole position. The Burt formulation, in other words, gives causal primacy to position (occupying a hole position), while the alternative gives primacy to cognition (recognizing that hole positions can be advantageous) and motivation (pursuing this advantage). This difference opens some questions not yet addressed by Burt. One question is closely tied to issues of entrepreneurship and innovation: How are structural holes discovered in the first place? The answer could lie in individual differences (in cognition, motivation, or capabilities), but a partial network explanation is also possible: people at the periphery of existing networks—for example, marginal individuals or sojourners—are more motivated to search for holes than people at the core. Another question not yet addressed is why holes persist given the standard microeconomic prediction that their advantages will be competed away. The answer could lie in the power of people occupying holes to protect their turf by building barriers to entry—think, for example, of Bill Gates.

Whether the advantages accruing to individuals and groups as a consequence of structural holes should be described as social capital depends largely on how one views social capital. Does one, following Burt, Portes and Mooney (this volume), Bourdieu (1977 [1972]), and Coleman (1990), think of social capital as accruing to individuals and groups, as human capital does? Or does one, following Putnam (1993) and others, think of social capital as above all social and accruing first to society and then to individuals? Research evidence will not settle these questions. We should point out, however, that fields like accounting and finance are paying greater attention to the intangible assets of firms, which are based in firms' knowledge and their customer relationships, as distinguished from the tangible assets reported on their balance sheets. It may be that new terms like "social capital" and "intangible assets" cut too broad a swath, and that, as research moves forward, more specific language will be utilized to describe the relationship-based and knowledge-based assets of individuals, groups, and firms.

Gender Inequality and Economic Sociology

What determines how hard people work? This question is fundamental to our understanding of the economy, and a rich area for integrating the research of sociologists, economists, and psychologists. William Bielby and Denise Bielby examine the literature on effort and commitment on the job and gender differences in this area. They criticize this literature for its sloppy conceptualizations of effort and commitment and its reliance on stereotypes about gender differences even in the absence of firm evidence of such differences.

We can delineate three broad approaches to thinking about determinants of effort on the job. Some approaches follow a narrow neoclassical model in which self-interested actors make rational decisions about how to allocate their effort. Here the assumption is that workers give less effort to their work except when material incentives make effort pay. Married women with children are presumed to be in a gender division of labor in the family that encourages this allocation of less energy to the job. Some evidence is supportive, such as the lower earnings of mothers. But other evidence subverts the view, such as the finding that on average women report slightly more effort and commitment than men.

The economic model also assumes that people prefer leisure to effort at work and will convert paid work time to leisure if such "shirking" is not penalized. Thus, economists have developed the efficiency wage theory, which argues that when surveillance of workers is especially expensive, employers decide that it is cheaper to pay an above-market-clearing wage than to pay for more supervisors or more surveillance equipment. Such "efficiency wages" motivate effort because, even with minimal surveillance, employees run some risk of getting fired for shirking, and the higher wage increases the cost of losing such a job. But economists have speculated that paying women workers efficiency wages would be less necessary (because of their alleged "docility" and the attendant ease of supervising them) or less effective (because more women than men plan to work intermittently and thus have less to gain in lifetime earnings from a given wage increment). This interpretation of the behavior of women workers has been offered as an explanation of the dearth of women in jobs that pay efficiency wages. Of course, sociologists also suggest the simpler hypothesis that garden-variety sex discrimination keeps women out of such jobs.

If economists' models have featured rational, selfish, lazy workers motivated only by money and inclined to be "free riders," a distinct psychological literature has seen organizational commitment and group effort as a collective orientation that involves other-regarding, altruistic behavior. The latter view of work effort and commitment easily incorporates the stereotype of women as socialized to be other-regarding to suggest that women would be better workers and citizens of organizations than men. This literature also sometimes finds women to be more public-spirited. However, Bielby and Bielby criticize these authors for being too quick to accept this conclusion because it corresponds with culturally held stereotypes, and they caution that often gender differences are tiny and explained by unexamined differences in the immediate structural constraints and incentives faced by men and women. Cecilia Ridgeway and Lynn Smith-Lovin (1999) and Elizabeth Aries (1996) have recently reached similar conclusions.

True to their structural bent as sociologists, Bielby and Bielby urge that future research on work effort and organizational commitment focus on the structural and situational features of the workplace that affect effort. They note that sociological research has found little effect of pay on effort or commitment; by contrast, the autonomy that a worker's job allows has been found to increase effort and commitment considerably. To be sure, these inferences, coming as they do from non-experimental, cross-sectional survey data, are subject to many possible selectivity biases, so it is hard to know whether causal effects are being accurately tapped. They suggest that we study how effort is affected by the characteristics of jobs and supervisors, the gender and race composition of work groups and the worker-supervisor match, and the gender meanings associated with tasks, as well as the material incentives examined in more conventional research.

In her chapter, Barbara Reskin rethinks conventional notions about discrimination. Her discussion ranges across the sociological, economic, legal, and psychological literatures. She argues that the conceptualization that lawmakers had in mind when they passed the Civil Rights Act of 1964—and that many judges still have in mind—is of discrimination as deliberate and based on animosity toward a group. Social scientists, she argues, have recognized two other types of discrimination.

Economists introduced the notion of statistical discrimination to refer to the practice of reducing information costs by using statistical generalizations about a race or sex group to infer an individual's probable characteristics. Although economists usually think of statistical discrimination in terms of using correct estimates of the direction and magnitude of group differences, some writers use the term more broadly to include perceived differences that do not exist at all or are not as large as believed, even at the group level. Like discrimination based on animosity, statistical discrimination is deliberate, but it is motivated by the desire to find qualified workers while minimizing information costs rather than by emotional aversion for a group.

Sociologists have also recognized structural discrimination, which Reskin defines as using a criterion other than race or sex that effectively screens out more members of one group than of another. For example, requiring twenty years of experience for a management job will screen out more women than men. With the *Griggs* decision, U.S. law encoded the doctrine of disparate impact, saying that if a screening device can be shown to have a disparate impact, employers can be prosecuted for discrimination if they cannot show that using it is job-relevant (*Griggs v. Duke Power Co.*, 401 U.S. 424, 1971). This type of discrimination, too, is deliberate, although it may not be based on animosity.

Reskin draws our attention to a fourth kind of discrimination, which is unconscious and unintentional. She reviews the social-psychological literature that documents the tendency of human cognition to be systematically distorted in a way that creates favoritism toward ingroup members. This tendency can come into play over and over in the decisions that affect employment outcomes, hiring, performance evaluation, promotion, mentoring, and termination. In Reskin's view, these pervasive, unconscious biases make discrimination the default option. The law, she argues, is not well tailored to redress this type of discrimination. Thus, we will eliminate it only if organizations explicitly tailor policies to minimize it. The impact of such discrimination could be decreased by laws that hold employers accountable for any correlation between race or sex and an organizational outcome that is not justified by qualification-relevant criteria. Among the organizational strategies that could discourage these biases are requiring that decisionmakers have individual information on the people on whom they are making decisions, and holding them accountable for using this information consistently.

In chapter 10, James Baron, Michael Hannan, Greta Hsu, and Ozgecan Kocak advance the institutionalist thesis that the cultural blueprints in the minds of the founders of a firm affect many aspects of how the firm is set up, with lasting effects that exemplify path dependence in organizational evolution. They examine how these cultural models affected the gender composition of the core technical and scientific workforce in 170 high-tech firms in Silicon Valley.

In-depth interviews with the founders of these firms led to a rich qualitative database on cultural blueprints. Their content analysis of these data led to five models. The "star" model, much like academic science, selects individuals based on their potential, assumes motivation from their intrinsic interest in the scientific work, and relies on internalized professional norms for social control. The "engineering" model selects individuals for their specific technical skills, assumes that these "techies" find the work inherently interesting, and relies on peer norms of excellence for social control. "Bureaucratic" and "autocratic" models also select for technical skills but rely on rules and monitoring for control. The "commitment" model (associated with firms like Hewlett-Packard) features a strong and motivating corporate culture in which individuals are selected on the basis of whether they are perceived to fit the culture.

The type of blueprint selected by the founders of these firms had no effect on women's share of the core technical, scientific, and engineering jobs at the start-up. However, net of women's initial representation, firms following the commitment model showed the worst record for increasing women's share of jobs over time. The authors explain this long-term effect in terms of what Rosabeth Kanter (1977) called "homosocial reproduction"—the tendency of leaders in an organization to trust others like themselves in terms of racial, sexual, and other sociodemographic markers, and thus to fill key positions with such people. Baron and his colleagues think it is the premium placed on fit and peer culture in the commitment model that makes homosocial reproduction more extreme in firms run on this model. We could speculate that a controlling culture with homogeneous leaders makes for more tightly linked social networks and social capital in "high-commitment" firms, and that this may be what makes it harder for women or other outsiders to penetrate such a culture. (As Alejandro Portes and Margarita Mooney note in their chapter, one individual's social capital can be another individual's exclusion.)

This analysis has broad implications for the study of organizations in economic sociology. Baron and his colleagues make a plea for recognition of the importance of the blueprints in place at an organization's founding. They call for research that incorporates measures of such founding conditions, noting that they have been absent from most data sets. Their chapter illustrates one area of convergence of the neo-institutional and population ecology perspectives in the study of organizations. Neo-institutional views stress cultural templates; population ecology assumes considerable inertia in organizational routines (in juxtaposition with the neoclassical view in which firms change strategies as market conditions and incentives in the regulatory environment change). The cultural blueprints of founders constitute a kind of organizational birthmark that is consistent with both ecological and neo-institutional views.

In chapter 11, Viviana Zelizer provides an example of what she calls for in chapter 5: extending economic sociology into spheres traditionally considered beyond its boundaries, and using a key idea from cultural sociology—that the type of social tie involved in an "intimate transaction" often determines the meanings that are invoked. By intimate transaction, Zelizer is referring to interactions that often involve both love and money, such as those in relationships between a husband and wife, a man and his mistress, or a parent and child. She criticizes the tendency to see this personal sphere as operating according to principles completely different from those of the public world of the economy and the polity. This private-public dualism (roundly criticized by feminists) sees the personal as the sphere of love and particularism, and the public sphere as the realm of self-interest and commodification. Underlying the notion that different principles explain behavior in the two realms is the often moral exhortation that the personal sphere be protected from contamination by commodification and self-interest. Such objections are often raised when feminists suggest that marriage should be more contractually based, or that economically dependent wives are owed some payment for their reproductive labor. Some feminists have pointed out that women's key problem is often too little commodification of what they do, not too much. Zelizer notes that money transfers coexist with intimacy all the time and in fact represent significant capital flows in the economy. Examples include support of an economically dependent spouse, payment of nannies, children's allowances, bequests to adult children, and wedding gifts of money to friends or relatives. These are large flows in the economy!

Zelizer proposes that we should not expect fundamentally different theories to explain the more and less personal spheres. Thus, she rejects the "Hostile-Worlds" view that different theories apply. But she also rejects what she calls the "Nothing-But" view, the main example of which is the imperialistic version of rational choice that sees all human behavior as explained by optimizing behavior. She rejects the idea that Nothing-But political processes, or "nothing but" some reductionist view of cultural beliefs, can explain both spheres. She proposes that we understand all spheres of action instead in terms of differentiated social ties. The type of social tie dictates which cultural meanings or rules or values are invoked, and which types of money flows and other exchanges or gifts for love are seen as appropriate. In this view, a major form that culture takes is defining what behavior and feeling is appropriate for what type of social tie.

A major lesson of her analysis for economic sociology reiterates the message of her other chapter in this volume—that we should not bound economic sociology too narrowly, and that boundaries should not be set using unexamined and incoherent criteria. She wants economic sociology defined broadly to include much that has usually been left out, such as household production and consumption and even sexual transactions. But such a definition raises the interesting question of what part of sociology *is not* economic sociology. She does not address this question, but her discussion suggests that it makes sense to define economic sociology as involving market and non-market production, distribution, and consumption.

The Economic Sociology of Development

The fourth part of this book focuses on economic development, which has been on the sociological research agenda for a long time. The chapters by Alejandro Portes and Margarita Mooney and by Susan Eckstein use concepts drawn directly from economic sociology. In chapter 12, Portes and Mooney elaborate on one of the key emerging concepts in economic sociology. They argue that social capital—as an attribute of communities and regions—can contribute to economic and social well-being by fostering collaboration and entrepreneurship. They warn, however, that the causal relationship is complex, contingent on a number of factors, and subject to a considerable degree of historical path dependence—and thus difficult to reproduce in a different context. Portes and Mooney begin by critically examining the concept of social capital, that is, the "ability to secure resources by virtue of membership in social networks." In this view, actors embedded in a dense network of relationships can benefit from altruistic norms of moral obligation or bounded solidarity to obtain desired resources. In addition to altruism, resource access can be facilitated by instrumental factors, including simple reciprocity and enforceable trust.

Portes and Mooney believe that entrepreneurship at the community and regional levels benefits from dense networks of relationships between actors. They are critical of the previous research linking social capital to development that confuses the ability to secure resources through networks with the resources themselves and neglects the tendency of excessive levels of social embeddedness to stultify entrepreneurial initiatives and reinforce patterns of inequality.

Portes and Mooney use three case studies to illustrate the usefulness of the concept of social capital to understand development outcomes: flexible manufacturing in the Italian region of Emilia-Romagna; handicraft production and trade among the Otavalan Indians of Ecuador; and community infrastructure projects funded by Salvadoran exiles in the United States. These cases suggest three general points. First, social capital is hard to "engineer" because it tends to originate from fairly unique historical processes, frequently unrelated to economic variables. Thus, fascism in Italy, colonialism in Ecuador, and the Salvadoran civil war of 1980 to 1992 prompted the development of ingroup identities. These identities led to altruistic and instrumental behaviors conducive to economic innovation and dynamism. Second, social capital does not necessarily reduce competitive behavior but rather infuses it with meaning, sets normative limits to its scope and intensity, and helps channel it toward the welfare of the community. And third, one possible path to successful economic transformation is to build on existing social structures and relationships to foster entrepreneurial collaboration between individual and organized actors in the community. Although persuasive, these arguments invite further research to explore exactly what contingencies may render the presence of social capital insufficient for development success.

While Portes and Mooney explore how social capital may contribute to development, Susan Eckstein seeks to understand forms of resistance to economic adversity. In chapter 13, she argues that responses to trade and price liberalization in Latin America and to state downsizing during the 1990s have not been primarily class-based. Rather, responses have ranged from individual exit strategies (emigration and migration) to popular resistance by groups defined according to their functional status, such as farmers, consumers, students, debtors, neighbors, or squatters. To expand her careful analysis of differences across Latin American countries, Eckstein draws on several key concepts in comparative political sociology as she assesses the likelihood of individual versus collective responses. She argues that the chances of collective responses are greater when the state is weak, state-society relations are not institutionalized, the level of politicization of the society is high, and few options other than exit are perceived.

The concepts of repertoires and interpretive frames also figure prominently in Eckstein's analysis. She documents how disgruntled individuals and groups throughout Latin America have drawn from modernist, premodernist, and postmodernist strategies of action to "address and redress the deprivations and injustices they experience" in their daily lives. Thus, protesters have embraced symbolic responses, emphasized custom and tradition over rights, and turned to new channels of communication, such as the Internet, to express their grievances. As Eckstein observes, the rise in nonmodern ways of protest runs counter to the fact that purely modernist and class-based ways of articulating demands and exercising protest have been made ostensibly easier by Latin America's recent turn to democratic rule after decades of more or less explicit authoritarianism.

Eckstein's analysis and conclusions contribute to an economic sociology that strikes a delicate balance between the macropolitical structure and culturally rooted action. This chapter invites more research on economic action and social mobilization aimed at exploring the limits of either approach in isolation when it comes to examining economic change.

The Revival of Economic Sociology

In our view, the independent evolution of organizational sociology, social stratification, the sociology of development, and the sociology of culture precludes the growth of a more integrated economic sociology focused on making sense of economic structures, changes, and trends in the contemporary world. As Bruce Carruthers and Brian Uzzi (2000) point out, actors' identities, relations, and roles are changing rapidly as the boundaries of social groups, occupations, professions, firms, industries, and even countries are becoming blurred. For example, the study of internal organizational processes is becoming difficult without attending to social stratification and culture, and the relationship between the organization and its environment is rooted in political, cultural, and developmental processes. Similarly, social stratification takes place in an organizational, cultural, and political context, and contemporary cultures and subcultures are shaped not only by social stratification but also by organizations such as firms and the state. Finally, cross-national patterns of economic development are intricately related to social, organizational, and cultural changes.

Like the social tumult of the 1960s, which challenged prevailing sociological theory and cleared the way for major theoretical developments, the dynamic complexity and globally connected nature of today's economic phenomena are driving economic sociology toward an even more integrative and sophisticated theoretical approach. The study of economic sociology is well positioned to rise to the challenge of addressing these loose ends and integrating organizational, stratification, cultural, and development processes into explanations of economic phenomena.

It is our conviction that at this moment of reemergence economic sociologists need to embrace multiple theoretical and methodological approaches. We see room for comparative-historical, cultural, evolutionary, and structural approaches to the study of economic phenomena, among others, and a need to collect both quantitative and qualitative data at different levels of analysis. Inductive and deductive reasoning remain powerful and complementary tools. We envision economic sociology as an opportunity for dialogue among all sociologists and as a way to reclaim the rich classic heritage of studying the economy and the society as a whole.

The revival of economic sociology promises to place the study of economic phenomena at the center of the sociological arena, where it will attract the participation of researchers who were previously focused on social stratification (including class, gender, and race), culture, organizations, or economic development. This integrative effort is an opportunity to reaffirm sociology's historical roots as a social science. Economic sociology will succeed to the extent that it can move beyond compartmentalized specialties and identify common concepts and postulates. Each of the chapters in this volume represents an attempt to reorient the sociological study of the economy and break free from the boundaries of the past.

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