

On *Networks and Markets*¹ by Rauch and Casella, eds.

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1. Introduction

AN ECONOMY CAN BE DEPICTED as a network or graph that links economic actors with one another in a flow of exchange. But so what? Why might it matter that an economy (or market or firm) is a network? If the pattern of trade merely derives from actions taken in the manner expected by neoclassical theory, it would seemingly be irrelevant. After all, economic agents may be expected constantly to be initiating and ceasing interaction with one another as they search for the best deal. At any point in time, one may map out the network of consummated exchanges. Such a characterization would add no information to what is already understood about economic processes.

Yet in recent years, both economists and sociologists have increasingly come to recognize that analysis of economic networks may indeed shed light on observed economic behavior and outcomes. How and why networks matter, however, is a subject of significant debate. And research on these questions displays wide variation in subject

matter and analytical style, as well as such basic conceptual issues as the right definition of an economic network.³

In the face of such cacophony, the publication of James E. Rauch and Alessandra Casella's edited volume *Networks and Markets* (2001) provides a very valuable service. The contributions by sociologists and economists collected in this book, which was the outcome of a 1997 conference, hardly suggest significant agreement between (or even within) the two disciplines in their approaches to analyzing economic networks. Indeed, as Rauch and Gary G. Hamilton point out in the book's introductory essay, convergence should probably not be encouraged lest the distinctive strengths of each field be diluted. The book rewards the reader with much food for thought simply because it illustrates some of the most promising approaches to the study of economic networks, and it allows the raw dissonances between such approaches to emerge. As a result, *Networks and Markets* should serve as a useful resource for a wide variety of scholars grappling with how analysis of

¹ *Networks and Markets*. By James E. Rauch and Alessandra Casella, editors. 2001. New York: Russell Sage Foundation, pp. viii, 346. ISBN: 0-87154-700-7.

² MIT Sloan School of Management. Thanks to Neil Fligstein, Bob Gibbons, and Eric Van Den Steen for their helpful feedback. All errors are mine.

³ The term "economic network" is itself not commonly used, at least not by sociologists. I use the term here to make it clear that we are dealing with networks of relations among actors whose behavior may be said to impact on the economic sphere in some way.

economic networks may improve our understanding of economic behavior and outcomes.

In addition to the introductory essay and Casella's concluding remarks, the book is divided into five chapters, each written by a sociologist or an economist (and in one case, by a research team that includes both sociologists and economists), and followed by a response written in all but one case by a scholar from the other discipline. The chapter topics are—to put it mildly—varied: Which network patterns (between members of the same organization) lead to greater trust and distrust? Why do two (East Asian) economies differ in their patterns of vertical organization? How do trading patterns (in the Marseille fish market) aggregate to produce market-level outcomes (such as downward sloping demand curves)? What is responsible for changes in organizational form (viewed as partnerships between persons with a particular social relationship) observed over time (in fourteenth- and fifteenth-century Florentine banking)? How do ethnic business networks (among English-speaking blacks in Brooklyn) provide benefits to members and how might commercial trade intermediaries do so instead? The analytical tools brought to bear are equally varied. Evidence is drawn from multivariate analysis of survey data, formal modeling, historical archival research, agent-based simulation models, and qualitative fieldwork. Finally, the authors vary greatly in the style of argument they adopt. Indeed, authors and discussants are frequently talking past one another. To put it bluntly, it is not easy to find a common thread other than the contributors' common participation in a conference and their willingness to have their work placed under the common heading of "Networks and Markets."

Nevertheless, this common thread is important. It reflects the shared recognition that economic networks deserve theoretical

and empirical attention and that something is gained from (exceedingly rare) dialogue between sociologists and economists on the subject. Rauch and Hamilton are wise to be conservative in the main objective they offer for the book: that it "may cause practitioners from both disciplines to clarify the concepts that they normally take for granted" (p. 3). In this spirit, I organize the following discussion as follows. First, I provide an overview of the conceptual and operational challenges faced by analysts of economic networks and discuss the three conceptions of economic networks found in the book (and in work on economic networks generally): networks as concentrated exchange, as primordial affiliations, and as structures of mutual orientation. Next, I review each of the contributions in the book and assess how they may help to redirect research by both sociologists and economists on how economic networks function. I highlight the efforts that I believe have the most to offer to scholars from both disciplines. I conclude with some general observations about future research on networks and markets.

2. *What Is an Economic Network?*

There appear to be as many (usually implicit) definitions of economic networks as there are scholarly papers devoted to the topic. Rather than adding yet one more definition to the pile, it is more worthwhile to review the relevant conceptual and operational issues and thereby to establish a working guide for tackling such challenges in research. We may begin by considering the necessary elements in any analysis of a social network, of which an economic network must be considered a subtype: *a set of nodes and the pattern of ties among such nodes*. But what is meant by a node? How do we define the boundaries of the set? What is a tie? What constitutes pattern? And finally, how do we decide when a network is an economic

network as opposed to one that is simply “social?”⁴

The answer to the first question seems relatively straightforward. Analyses of economic networks typically consider two types of nodes: human beings and human collectivities that are assumed to have significant capacity for coordinated action. In short, the nodes may be people or organizations. Yet other types of nodes are often considered in social network analysis generally and in the literature on economic networks in particular. Notable examples include: the country (e.g., Ronan Van Rossem 1996); the industry (e.g., Ronald S. Burt 1982, 1983, 1992); the innovation (e.g., Toby E. Stuart and Joel M. Podolny 1995); and the product (e.g., Beth A. Benjamin and Podolny 1999). To the extent that such analyses are designed to account for the sources of network patterns or their behavioral consequences, they must specify how such nodes relate to agents/actors. A link between agent and node is often achieved through an exercise in aggregation whereby all nodes that relate to a common agent are subject to common effects or have common causes. Alternatively, one may

assume that the nodes in question proxy for unobserved agents. In either case, the link must be made explicit and justified for analyses of network effects to be clear and compelling.

Delimiting the set of relevant nodes is notoriously difficult. As Edward O. Laumann, Peter V. Marsden, and David Prensky (1983) argue in their classic essay on the “boundary specification problem,” the value of a network analysis often rests on whether the rules for including and excluding nodes are sensible and whether they generate data that are not artifacts of those rules. Yet while a crucial decision, defining the set of relevant nodes is inescapably a judgment call on the part of the analyst. Laumann et al. identify two typical approaches—the “nominalist” strategy, whereby the analyst imposes a definition of the relevant set based on *a priori* criteria, and the “realist” strategy, whereby actors are included if they are judged relevant by the actors themselves. Consider if we were to conduct an analysis of the network of relationships among competitors in an industry. A nominalist approach might entail gathering data on all firms that are assigned to a particular standard industrial classification (SIC) code or that use the same set of inputs. A realist approach might involve a form of “snowball sampling” (see, e.g., Bonnie H. Erickson 1979) that begins with known members of the population of interest and follows a citation path until (hopefully) the citation patterns achieve closure. The strengths of the nominalist approach are the weaknesses of the realist, and vice versa. Realist strategies are more likely to include relevant nodes and exclude irrelevant ones. However, realist strategies are idiosyncratic to a particular time and place,⁵ and they may generate certain network properties as artifacts of

⁴ There is an important line of work that departs even from the basic elements of a set of nodes and a set of ties among the nodes. In particular, research on “egocentric” or “personal” networks that was initiated for the study of community structures and intimate relationships (e.g., J.C. Barnes 1972; Elizabeth Bott 1971 [1957]; Burt 1984, 1990; Claude S. Fischer 1982; Mark S. Granovetter 1976; Edward O. Laumann 1966, 1973; Barry Wellman 1979) has been applied to networks within firms (Burt 1992; Podolny and James Baron 1997) and between firms (Vincent Buskens, Werner Raub, and Jeroen Weesie 2000). In an egocentric survey, respondents are asked to describe a short set of contacts according to particular criteria of interest and to characterize the relationships among such contacts. The basic advantage of egocentric surveys is that they elicit higher cooperation rates from respondents because they typically do not ask for identifying information about contacts. The main drawback of this approach is that little or nothing can be said about the larger network from which the egocentric networks have been drawn. The present discussion touches almost exclusively on sociometric or sociocentric networks rather than egocentric networks, though the chapter by Ronald Burt discussed below contains analyses of both types of network data.

⁵ Imagine using a realist strategy to consider variation over time or place in the density of an intra-industry network. Since the data for each time and place will have been generated in a different way, the network densities will not be comparable.

the data collection process.⁶ For an excellent example of a paper that combines both approaches in the analysis of a network among competitors in the Sydney, Australia hotel market, see Paul Ingram and Peter W. Roberts (2000, especially pp. 398–99).

Analyses of economic networks often face two additional boundary-specification challenges that are less commonly encountered in analyses of other types of networks. First, while most social network analyses are conducted on one-mode networks, in which every actor is considered to be at risk of both “sending” and “receiving” the tie of interest to every other actor, most economic networks are more accurately modeled as two-mode networks, whereby there is clear differentiation of roles between one type of actor who is restricted to sending and another that only receives. That is, most markets are “interfaces” (e.g., Harrison C. White 2002; Wayne E. Baker 1990; Ezra W. Zuckerman 1999; Damon J. Phillips and Zuckerman 2001) between occupants of two distinct economic roles—most typically, buyer and seller, though other role relationships are possible (see e.g., Robert Faulkner 1983; Faulkner and Andy B. Anderson 1987; Zuckerman and Tai-Young Kim 2002). An implication of this two-mode structure is that the boundary-specification task requires that one delimit two sets of actors rather than one. Moreover, a more vexing issue arises from the tendency for markets to be only partially role-differentiated. For instance, while trading markets may be considered as one-mode structures (e.g., Baker 1984), Ozgecan Kocak’s (2002) analysis of eBay auctions indicates that markets vary in the extent to which there is differentiation between the buyer and seller roles and that such variation is itself meaningful. The challenge that this issue raises is the following: how does one interpret an absence of a tie

(of a specified type), when such absence could either indicate that the two actors play roles that effectively make such a tie impossible (i.e., a “structural zero”) or that one or the other actor has chosen not to enact such a tie? It is difficult to escape the conclusion that such a determination cannot be made purely on nominalist, *a priori* grounds, but must be made in light of a good working understanding of the context.

A related and critical issue that acquires special salience in the analysis of economic networks is what might be called the node specification problem. Unlike individuals, firms merge with other firms to become (relatively) unified entities—and they disintegrate as well. Any analysis must then consider whether the appropriate node for analysis is the business unit or the firm. And such a decision is greatly complicated by variation (across firms or over time within the same firm) in the extent of integration. This decision is intertwined with the question of how we understand intra-firm relations, which may be considered as simply another form of network tie between independent agents or as something qualitatively different from market interaction. As an example of the former approach, consider how Baker (1984) and Paul DiMaggio and Hugh Louch (1998) appeal to the same behavioral principles (bounded rationality and opportunism) to predict a reliance on networks (patterned exchange in the first and the reliance on “primordial” ties in the second; on the distinction, see below) as does Oliver E. Williamson (1985) to predict the location of firm boundaries. If firm boundaries merely indicate the location of “networks,” however defined, they may be amenable to analysis with a very different array of principles than are typically brought to bear on the question of firm boundaries (e.g., Burt 1992, pp. 238–49; Podolny 1994). Conversely, if firm boundaries signify a qualitative increase in commitment between the transacting parties, the inescapable conclusion is that any analysis of economic

⁶ For instance, network data collected through a snowball sample must be at least somewhat dense.

networks must be informed by a particular theory of the firm.⁷ In either case, the analyst's stance towards this issue should be made explicit and defended.

As we have seen, the two seemingly basic questions of node and boundary specification entail a host of issues for analysts of economic networks to consider. Our discussion also indicates that these two issues are intertwined with the three additional challenges outlined above: defining "pattern," "tie," and "economic." It is perhaps of some comfort then to realize that these three questions may be reduced to a larger, overarching question: *what manner of orientation among the nodes is meaningful such that it constitutes a structure that has causal implications for outcomes of interest?*⁸ Put simply (and circularly), a network is economic if it has effects on future events that are considered economic by the analyst (and her readers). And the network should command interest only if it cannot be fully reduced to the constraints placed upon and choices made by actors, such that a complete account of the relevant causal pathways may be rendered without attention to the network.

The "manner of orientation" among nodes encompasses the two remaining issues of tie and pattern. Regarding the former, it is crucial to recognize that there are infinite ways of defining network ties, which means that network analyses must pay careful attention to the matter of tie definition (see Burt 1984 for an exemplar). That there are as many network structures for a given set of nodes as there are possible tie definitions does not make network analysis any more hopeless than did the recognition that network survey data bear only broad resemblance to underlying behavioral patterns (H. Russell Bernard et al. 1984; Linton C. Freeman, A. Kimball Romney, and Sue C. Freeman 1987). Rather, just as the subjective filtering

of networks necessitates the analysis of "cognitive social structures," (David Krackhardt 1987, 1992; Ece Kumbasar, A. Kimball Romney, and William H. Batchelder 1994),⁸ the implications of any network analysis are similarly governed by the type of tie(s) specified.⁹

The question of pattern is central to the analysis of economic networks. Note two common uses of the term "network:" (1) as above, the pattern of ties among a set of nodes; (2) *a high degree of pattern* in the ties among a set of nodes, where such degree has particular theoretical or empirical meaning. The latter (implicit) definition is commonly found when analysts contrast "network" with "market" and with "organization." Moreover, analysis of economic networks is often justified by the observation that exchange among agents displays greater pattern than the arm's-length relations depicted by orthodox theory. According to this line of thinking, much interaction between legally independent economic agents can be construed as falling within the scope of the "network form of organization," as defined by Podolny and Karen L. Page (1998, p. 59): "any collection of actors ($N \geq 2$) that pursue repeated, enduring exchange relations with one another and, at the same time, lack a legitimate organizational authority to arbitrate and resolve

⁸ This is a particular necessity for testing theories that rely on an agent's recognition of her position in a network. Theories that expect network effects despite agents' ignorance of their positions do not require cognitive network data. At the same time, such analyses are problematic if they rely solely on survey data to capture the network, though less so if multiple respondents report on the same relationship. Difference in such reporting can be productively analyzed, as in Ronald L. Breiger (1976).

⁹ Note that early social network analysts tended to "stack" networks composed of different types of ties in an effort to discern the underlying structure (see e.g. White, Scott A. Boorman, and Ronald L. Breiger 1976; Boorman and White 1976; Burt 1977a,b). More recently, however, the trend has been to recognize that networks composed of different types of ties have different implications (see, e.g., Podolny and Baron 1997).

⁷ Of course, the analytic challenge presented by the shifting unit of analysis is a more general one that plagues any analysis of social aggregations.

disputes that may arise during the exchange.” Analysts of such networks often claim that they cannot be said to be either (competitive) markets or firms (Walter W. Powell 1990), and that they therefore operate under distinct logics (which are sometimes overly praised; see Podolny 1993a; Podolny and Page 1982).

Thus, one approach to economic networks conceives of the ties as market exchanges and ascribes importance to the network because it is more concentrated or patterned than is expected by orthodox market models. A key question for such analyses is whether network structures have causal impact, as is often supposed (e.g., Burt 1982, 1992; Baker 1984) or in fact the network analysis adds nothing to what could be attained from knowledge of the constraints and choices faced by relevant agents. Note as well that sociologists (e.g., Powell, Kenneth Koput, and Laurel Smith-Doerr 1996; Uzzi 1996, 1999) frequently attribute causal impact not to the patterned exchanges themselves but to the commitment between the two parties that such pattern suggests. Such commitment is typically unmeasured though, which raises doubts regarding the interpretation of observed associations.

A second way to understand economic networks is to regard them as economic interactions that are shaped in consequential ways by ascribed or “primordial” relationships. Among sociologists, a focus on social networks that pre-exist the market is associated most closely with the work of Mark S. Granovetter. In the classic article that is frequently cited as having ushered in modern economic sociology’s attention to market processes, Granovetter (1985) argued that Karl Polanyi (1944) had been premature in declaring that contemporary markets were no longer “embedded” in their social contexts. Rather, Granovetter insisted that market interaction could not be understood without attention to the social relationships through which it often occurs. It appears that, in their growing attention to economic

networks, economists have mostly considered primordial networks (e.g., Casella 1996; Casella and Rauch 1998; Rauch and Casella 1998; but see, e.g., Rachel E. Kranton and Deborah F. Minehart 2001). The analysis of such networks has the attractive feature that it seems easier to say which aspect of the relationship is “social” and which is “economic” and thereby to verify the causal impact of the former (see Kaivan Munshi 2001). Yet matters are often not so simple, as I discuss below in my review of the chapter by John Padgett.

A third and final conception of economic networks is the most general, since it encompasses the first two as subtypes: what we might describe as structures of mutual orientation. Such “sociometric” (Jacob L. Moreno 1934) networks, in which one agent’s ties to the others varies in the type, valence, and strength of the connection felt toward the second party (who may describe the relationship differently), are those most frequently analyzed by practitioners of social network analysis (for review, see Burt 1980; Stanley Wasserman and Katherine Faust 1994). This conception of networks might seem to be the least relevant insofar as we are interested in market dynamics, as opposed to intra-organizational processes (where there is a long tradition of conducting sociometric studies of work groups; see e.g. George C. Homans 1950; Peter M. Blau 1963). Yet a large and growing literature on interorganizational relationships has made productive use of sociometric concepts to analyze networks composed neither of market exchange nor of primordial ties. Examples include research on corporate interlocks (e.g. Mark S. Mizruchi 1996); on strategic alliances (e.g. Ranjay Gulati and Martin Gargiulo 1999), and on collaborations (e.g. Stuart, Ha Hoang, and Ralph Hybels 1999). In addition, a growing research stream analyzes the ranking (e.g. Podolny 1993b) and classificatory (e.g. Zuckerman 1999, 2000) systems that emerge implicitly through market interaction and that have implications for

market processes that are unexpected or underemphasized in orthodox approaches. Finally, researchers have begun to focus on structures of inter-firm orientation that are designed to be *orthogonal* to market exchange (e.g., Zuckerman and Stoyan Sgourev 2003). Such networks emerge endogenously to fulfill needs unmet by the market and may have powerful influence on firm behavior and outcomes.

As with work on patterned market exchange,¹⁰ analyses of any structure of orientation must grapple with the challenge of establishing that the structure indeed has causal implications for the agents of interest. This challenge may be boiled down to two issues: unobserved heterogeneity and reverse causality (see Ray E. Reagans, Bill McEvily, and Zuckerman 2003). Insofar as the researcher wishes to argue, for example, that a particular network position confers advantage, it is incumbent upon him to show that any observed association between position and success does not reflect underlying differences in agent “type” or that expectations of success determine the observed network configuration. Addressing such concerns requires a thorough understanding of potential feedback processes and a research design that affords confidence that network effects may be identified.

3. *A Guided Tour of Networks and Markets*

Having outlined the basic challenges that confront the study of economic networks, I now proceed to review the chapters of the volume edited by Rauch and Casella. In particular, I organize the discussion in terms of

the three conceptions of economic networks and discuss the implications held by the conceptual and operational issues discussed above for evaluating the contributions to the book.

3.1 *Networks as Concentrated Exchange*

Kirman on the Marseille Fish Market. Two chapters may be classified as analyses of networks as concentrated exchange. The first, by economist Alan Kirman, is particularly valuable because it both speaks to and transcends ongoing debates as to the implications of such networks. While much recent research by both sociologists and economists recognizes the prevalence of concentrated market exchange, there is little agreement as to its implications. Sociologists generally assume that since relationships among economic agents rarely match the descriptions in economic textbooks, economic theory cannot account fully for key features of market dynamics. Some sociologists go so far as to argue that among agents who are “embedded” in such networks, self-interest is somewhat muted due to strong identification with one’s partners or even larger collectivities (e.g. Granovetter 1995; Uzzi 1997; Powell, Koput, and Smith-Doerr 1996). Such a view is obviously not popular among economists, who prefer to regard “goodwill” (Ronald Dore 1983) among exchange partners as consistent with each actor’s self-interest, perhaps as befitting an intermediate point in the array of relationships that fall between markets and hierarchies (Williamson 1991) or as endogenous to a repeated game between exchange partners (see e.g. George Baker, Robert Gibbons, and Kevin J. Murphy 2002; Julio J. Rotemberg 2002).

In light of such discord, Kirman’s chapter describing research conducted by himself and colleagues (see Wolfgang Härdle and Kirman 1995; Kirman and Annick P. Vignes 1991; Kirman and Nick Vriend 2000; Gerard Weisbuch, Kirman, and Dorothea Herreiner

¹⁰ One might suppose that analyses of primordial networks do not face this problem since they are given rather than chosen. However, as discussed below, there is a great amount of choice involved in the selection of primordial ties in a particular context. Thus, establishing causality with primordial networks is not a trivial task (see Munshi 2001).

2000) on the Marseille fish market should be read widely by both economists and sociologists.¹¹ Upon encountering this chapter, sociologists will think immediately of Baker's (1984) analysis of networks among option traders. Just as Baker showed that (especially during periods of high volume) traders tend to concentrate their trades among a few counterparties, Kirman finds that a surprisingly high proportion of buyers in Marseille refrain from searching beyond their usual source for fish—and even pay a higher price as a result (but typically obtain better service, according to Kirman). Thus, this commodity market may be described as a patterned network of exchange. Kirman also describes two additional patterns that appear “irregular” from the standpoint of neoclassical theory: there is significant price dispersion (just as Baker found) and individual demand curves are not downward sloping.¹²

At this point, most sociologists would be content to conclude that neoclassical theory has been disproved. Kirman's interpretation, however, is more measured and therefore more helpful in stimulating deeper understanding. First, Kirman points out that, while prices are highly variable on a daily basis, the distribution of prices is quite stable when viewed over a month-long interval.¹³

¹¹ Kirman provides a useful background on research on fish markets. Since fish stocks cannot be carried over from one day to the next, changes in supply may be considered as the outcome of a stochastic process, which facilitates the identification of demand. (This assumes that suppliers do not have facilities for storing fresh fish. It would have been helpful to know that this is not an issue.)

¹² Kirman might have provided more systematic evidence to buttress the latter assertion. He displays scatter plots of prices and quantities for two buyers that cannot be described as having a monotone decreasing association. He also reports that he and his colleagues “examined hundreds of such individual relations, and for almost none of them was there significant evidence of” such a pattern. But given the importance of this claim, it would have been helpful to back it up with a more formal test.

¹³ To support this claim, it would be helpful to present some sort of criterion for what constitutes excessive and reasonable amounts of price instability. After

Furthermore, aggregate price-quantity relations do in fact appear to be monotone decreasing. The interesting implication is that “the regularity at the aggregate level . . . is not due to individuals behaving in isolation . . . (according to) the standard competitive model . . . (thereby breaking) any simple link between individual and aggregate behavior” (p. 191). To make this point more precise, Kirman describes agent-based simulation models where buyers and sellers operate according to very simple rules and modify them in response to their level of profit. He shows that such models converge toward a situation in which much of the observed patterns in the Marseille fish market are reproduced. In particular, buyers and sellers generally learn to be loyal to one another (loyal buyers receive better service in the form of priority in line but sellers charge them a higher price for such service) while aggregate price distributions are quite stable.¹⁴

In her discussion, the economist Alessandra Casella argues that Kirman's success at producing “regular” patterns in the aggregate despite little regularity at the individual level (cf., Gary S. Becker 1962; Dhananjay K. Gode and Shyam Sunder 1993) calls into question whether “we should care at all about the underlying structure of individual interactions” (p. 200). That is, perhaps it does not matter how economic agents select transaction partners, nor does the network structure that results have important ramifications. More generally, perhaps Milton Friedman (1953) was right that accurately accounting for economic agents' behavior is unimportant so long as our model makes accurate predictions about the economic outcomes about which we care.

all, it is hardly surprising that prices stabilize at higher levels of aggregation.

¹⁴ He does not describe the shape of the market demand curves in the simulations.

Casella thinks that there are two reasons to care about such networks.¹⁵ First, while the market may generally aggregate micro activity to produce the patterns suggested by orthodox theory, this may not be true for all aggregate features, and particularly, the rate and direction of the path towards equilibrium. Second, she argues forcefully that we should care about how equitably profits are distributed. Casella is right to ask for more detail on the correlation between high prices and privileged service that Kirman asserts is observed in the Marseille fish market. Future research would do well to focus on both the causes and consequences of such enduring exchanges, since they seem to be so common and they may lead to competitive advantage for some and disadvantage for others. Citing her own research (Casella 1996) and that conducted in collaboration with the economist James E. Rauch (Casella and Rauch 1998; Rauch and Casella 1998), Casella describes various (primordial) networks that help solve fundamental problems in international trade but which crowd out institutions that represent more inclusive and universalistic alternatives.¹⁶ Casella points out that a better understanding of the micro foundations of repeated exchange should help us understand how such networks affect both the distribution of surplus and other outcomes of interest (for sociological work on such foundations, see Baker 1984, 1990; Gulati 1995; Gulati and

Gargiulo 1999; Stuart Macauley 1963; Podolny 1994; Uzzi 1996, 1997, 1999).

More generally, Kirman and colleagues' research serves as a wake-up call to both sociologists and economists because it shows how poorly we understand the process by which agent-level activity is aggregated into global patterns. Showing departures from naïve expectations regarding the pattern of economic interaction is insufficient. We must work to connect the dots from boundedly rational individual behavior through interaction among agents to systemic patterns. "Santa Fe style" agent-based modeling recommends itself as a very useful tool in this effort, one that has the distinct promise of being a language and tool that is being adopted across disciplines. As with any modeling technique, however, we should worry that investments made in refining the models may come at the expense of external validity.

Feenstra, Hamilton, and Huang on East Asian Economic Organization. A second look at networks as concentrated exchange is provided by the economist Robert C. Feenstra, the sociologist Gary G. Hamilton, and the economist Deng-Shing Huang (hereafter, FHH). As noted in the discussion by the sociologist Neil Fligstein, FHH tackle "one of the deepest questions for economics and economic sociology: if market processes select efficient systems of social organization, how do we account for the persistent differences we observe in the . . . organization of national capitalisms" (p. 142). In particular, FHH are motivated by longstanding differences in the organization of the South Korean and the Taiwanese economies. While the dominant model of the firm in South Korea is the very large, vertically integrated *chaebol*, business groups in the Taiwanese economy tend to be much smaller and to sell intermediate goods and services to a large number of small and medium-size firms. In attempting to explain this difference, FHH follow an analytical tack that runs opposite to that taken by Kirman. Beginning with "regular" behavior

¹⁵ In light of widespread commitment to "regularity" at the individual level, it is hard to believe that most economists would be content with Casella's retreat. After all, the erosion of this commitment, which indeed seems to be well underway as such research programs as agent-based modeling and behavioral economics gain footing, has profound implications for the culture of economics, if not economic theory and research. As Kenneth J. Arrow stated, "An economist thinks of himself as the guardian of rationality, the ascriber of rationality to others, and the prescriber of rationality to the social world" (Arrow 1974, p. 16).

¹⁶ See Carol A. Heimer (1992) for a rare attempt to grapple with the ethical tension between universalistic norms and the use of networks.

at the agent (firm) level, they build a model that produces “irregularity” at the macro level. Specifically, their model generates multiple sets of stable equilibria, two types of which broadly resemble the South Korean and Taiwanese economies.

There are a few attractive features of the approach taken by FHH. First, there is significant virtue in being conservative in the factors that one brings to bear in explaining such a large-scale issue as this. While they acknowledge the influence of political and cultural forces, it is tempting to see whether variation in national patterns could result from the endogenous workings of the market.

The glaring weakness of this paper, however, is the failure by FHH to adopt a theory of the firm that might explicate the pattern of intra- and inter-firm relations in their model. The model is built around a simple economy composed of an upstream and downstream sector (the latter of which is at least one step removed from the end user), with both sectors characterized by significant product differentiation. They assume that, in this arena of monopolistic competition, there are strong incentives for vertically integrated business groups to form.¹⁷ Such firms are postulated to be “inherently more efficient . . . than a combination of upstream and downstream unaffiliated firms” because they “sell the intermediate inputs to their own firms at marginal cost and to unaffiliated firms at marginal cost plus a markup” (pp. 90–91). Vertically integrated business groups are also assumed to “have an incentive to withhold their intermediate inputs from other groups because they do not want the competing groups to enjoy the same production efficiency that comes from having access to the specialized intermediate inputs” (p. 91). Finally, FHH

include a governance cost borne only by the vertically integrated business groups from taking over the entire economy. Yet this cost is postulated to be so small that, in equilibrium, unaffiliated firms are viable in either the upstream and downstream sectors but not in both. FHH then go on to model the number and type¹⁸ of business groups that are expected in an economy based on the number of existing groups (the circularity is responsible for the multiple equilibria) and the elasticity of the intermediate goods (assumed to be uniform throughout the economy). They describe two sets of equilibria (each defined by a range of elasticities), one which resembles their portrait of the Taiwanese economy, and the other, the South Korean economy.

This analytic set-up is bound to be very confusing to both sociologists and economists. First, it is doubtful whether vertically integrated firms refrain completely from selling to one another simply to avoid giving their competitors good deals. After all, they can always charge each other a portion of the difference. In addition, governance costs are no trivial matter (see e.g. Robert G. Eccles and White 1988; Paul Milgrom and John Roberts 1988; Robert F. Freeland 1996).

Most serious, however, is the decision by FHH to reduce the firm boundary question to the issue of “double marginalization”: the principle that it makes sense for monopolists to merge when they occupy adjacent steps in a value chain. While double marginalization is a longstanding and useful economic principle, it is a thin reed upon which to build a model of economic organization. For one thing, it is

¹⁷ Fligstein rightly points out that it is awkward to define business groups in terms of high levels of vertical integration when, in fact, it is high levels of diversification that is most striking about (East Asian) business groups.

¹⁸ Confusingly (Fligstein gets this wrong), FHH label a business group differently depending on which other firms compete in the economy. If all firms are vertically integrated business groups then they are called V-groups. If there are unaffiliated groups upstream (downstream), then the vertically integrated business groups are called D-groups (U-groups). It would have been much more straightforward to vary the label for the larger pattern of vertical organization rather than the groups themselves.

doubtful that the cases to which it applies are common enough for it to be posited as *the* principle that accounts for the vertical organization of an economy. It also puts FHH in the awkward position of assuming an entire economy to be governed by monopolistic competition. Most important, industrial organization economists have long recognized that there is no reason that double marginalization cannot be solved through contract rather than through merger (Benjamin Klein, Robert Crawford, and Armen Alchian 1978, p. 300). An indication that FHH do not recognize the question of firm boundaries to be an issue is that they cite no one—not even Coase!—who has contributed to the voluminous literature on the theory of the firm. Thus, while the project FHH pursue is an interesting one, the means by which they go about it left this reader scratching his head.¹⁹ Rather, the paper reinforces the point made above: an analysis of networks among firms that vary in their degree of integration is necessarily incomplete unless it specifies a theory of the firm. The adoption of such a theory is especially imperative if the analysis seeks to model both intra- and inter-firm relationships at the same time.

A final point concerning the FHH chapter: While it is an interesting exercise to see how much is gained by using a simple model that focuses solely on market dynamics and brackets political and cultural forces, Fligstein is right to wonder whether this is too smart by half. If it is likely that a particular factor is important, what is the great virtue in leaving it out of the story? The only rationale for doing so would be if there is a strong likelihood that the factor is in fact irrelevant. But it begs the imagination that the differences between Taiwanese and South Korean industrial organization do not result

in significant part from their historical as well as *ongoing* cultural, political, and institutional differences. More worrisome is the prospect that, by deflecting attention from such factors through a model that is built with knowledge of the features that it is designed to explain, these factors may be downplayed in future research. Thus, accuracy may be sacrificed on the altar of parsimony.²⁰

3.2 *Networks as Primordial Relations*

Rauch on Alternatives to Ethnic Networks. It is striking that, while the credo that the social precedes the economic has become popular among sociologists, few studies have demonstrated systematically how primordial social relations might affect market exchange (but see Kenneth A. Frank and Jeffrey Yasumoto 1998; Podolny and Fiona Scott Morton 1999; Roberto M. Fernandez, Emilio J. Castilla, and Paul Moore 2000). Rather, most research conducted under the banner of “embeddedness” actually focuses on the first conception of economic networks, that of patterned exchange (see e.g. Uzzi 1996, 1997, 1999) or concentrated ties of another type (Gulati 1995; Gulati and Garguilo 1999). Accordingly, the two contributions (and one discussion) in *Networks and Markets* that analyze how primordial relations are relevant for the analysis of economic behavior are particularly noteworthy.

The first such contribution, by Rauch, is striking for how casually it dismisses the theoretical objections with which one might challenge the importance of primordial networks. Rauch flatly asserts that “an ethnic business network can be a tool that allows entrepreneurs to avoid the effects of

¹⁹ Also questionable is the assumption made by FHH that firms in more concentrated markets are most sensitive to losses and gains in their market share (and are therefore less willing to sell to their competitors at prices that reflect their low costs).

²⁰ One might argue that it is plausible that there are no ongoing cultural, political, or institutional forces at work but only the path-dependent residue of past forces. This is possible, but one should be concerned that models such as these make it less likely that the irrelevance of such contemporary influences will be assumed rather than tested.

discrimination (p. 270).” By discrimination, Rauch does not mean the market’s failure to evaluate accurately human resources, but rather, the exclusion of businesses owned by minorities or immigrants from the flow of information about such matters as where to find appropriate and reliable vendors for key inputs. Rauch cites examples that illustrate how networks within ethnic communities may fill this void, thereby expanding access to a larger pool of vendors and generating economies of scope via the sharing of referrals and leads. He also discusses similar functions served by transnational networks. Conversely, Rauch argues that the well-known weakness of African American (retail) entrepreneurship may result in part from the absence of such networks. He backs up this conjecture with field research, conducted in Brooklyn during 1995–96, which indicates that the (English-speaking) Caribbean American business community is supported by a large, vibrant grassroots networking organization (though with only a 15-percent membership among retailers) but that no parallel institution for African Americans exists in Brooklyn.

Rauch then outlines a policy proposal that might redress this gap. In particular, he proposes the modification of “at least one independent buying office to better serve African American retailers” (p. 295). Independent buying offices are commercial intermediaries that reduce the costs of sourcing inputs and provide an array of related services to assist retailers. In addition, such buying groups are “loci for information exchange” among a membership base that includes peers who are not direct competitors (p. 288; see also Jay Diamond and Gerald Pintel 1996, p. 210). Rauch also discusses variants of this model such as wholesaler-sponsored voluntary chains and franchise systems. He argues that, while such buying groups may represent functional alternatives to ethnic networks, the market may fail to support such groups in cases such as the African American community. He points out

that commercial intermediaries are subject to a catch-22: they must have “deep knowledge” of specialized markets to provide value to members. However, the incentives to invest in the accumulation of such knowledge are likely to be weak because it is inherently non-contractible.

In their response to Rauch, sociologists Marta Tienda and Rebecca Rajjman provide some reasons to be skeptical of Rauch’s proposal. They describe results from a unique survey of small businesses in Little Village, a largely Mexican neighborhood in Chicago, conducted in 1994. Results from their survey reinforce a caveat issued by Rauch himself. Rauch concedes that his use of grassroots business networking organizations as indicators for the presence of ethnic networks might be misleading if such networks remain entirely informal (p. 280). Tienda and Rajjman show that this concern may be apt in the case of Little Village. In particular, while Mexicans and other ethnic entrepreneurs appear to rely heavily on informal, co-ethnic networks to obtain information regarding both the start-up and operational phases of their businesses, formal organizations seem to be relatively unimportant. Tienda and Rajjman recognize that they cannot say that commercial intermediaries cannot supplement the role played by ethnic networks. However, they suggest that the variance among the cases examined—“strong informal and strong grassroots organizations” among Caribbean Americans; “strong informal ties but apparently weak grassroots organizations” among Mexicans; and weak informal and formal ties among African Americans—“provides a weak basis upon which to draw inferences about the viability of Rauch’s proposition” (pp. 313–14).

This reader agrees with Tienda and Rajjman’s words of caution, which reflect the economic sociologist’s tendency to privilege the informal and social over the formal and economic. At the same time, the questions raised by Rauch deserve greater attention in future research. How indeed do

“grassroots” organizations—defined by Rauch as those that do not enjoy significant external support—supplement or augment more informal (ethnic) ties? How might commercial firms serve as alternatives to either? To the extent that all such intermediaries may be thought of as nodes in an economic network, it presumably should not matter what label is attached to them. Yet it clearly matters. In particular, how an institution is able to define membership seems to matter greatly for the level and type of investments that members will make in it and the level and types of services it will therefore be able to provide. More generally, the dialogue between Rauch and Tienda and Rajman reinforces the challenge represented by the “node specification problem.” To the extent that we suspect that the same ties organized by more or less formal (and commercial) alternatives may function differently, we need a theory of the firm that can explicate such difference.

Padgett on Florentine Banks. A quite different and much more ambitious account of the role played by primordial ties is presented by the political sociologist John F. Padgett. It is also the most challenging chapter for an economist to read, as indicated by the exasperated tone of the discussion by Gregory Besharov and Avner Greif. Besharov and Greif cannot quite figure out what a paper written in Padgett’s style could possibly accomplish. They fault Padgett for failing to justify the biological model he uses to motivate the paper, for never developing the model beyond the metaphorical stage to a point where testable causal claims are made, and for hindering any attempt at substantiating the model by mixing fact with conjecture. Finally, Besharov and Greif criticize Padgett for not motivating the paper through an appeal to “‘real’ consequences . . . (that might) concern economists . . . such as those associated with efficiency or distribution . . .” (p. 267).

Why might anyone be interested in a paper with these faults? After all, none of

Besharov and Greif’s complaints is completely off the mark. Yet if we allow ourselves to be interested in a set of outcomes that is somewhat broader than efficiency and distribution, and if we give some space to papers that are written with a more speculative bent (and Padgett makes it clear that this is how he intends the paper to be read), there is much to learn from Padgett’s chapter. Padgett’s goal is for the chapter to be no less than the successor to Arthur L. Stinchcombe’s classic treatise on “Social Structure and Organizations” (1965). The chapter should thus also be read in the context of the influential research tradition that is generally regarded as having taken Stinchcombe’s ideas the furthest—organizational ecology (see Michael T. Hannan and John Freeman 1989; Glenn R. Carroll and Hannan 2001). Indeed, while Besharov and Greif express bewilderment at Padgett’s decision to model organizational genesis as a biological process, such a strategy should come as no surprise to sociologists and organization theorists, who are accustomed to such models from at least the mid-1970s (Hannan and Freeman 1977; Howard Aldrich 1979). Curiously, Padgett does not refer directly to the work of organizational ecologists. Yet he criticizes them indirectly when he argues that the tendency for “current organizational theories” to explain the organizational forms based on performance-based selection regimes—or “consequentialism” (p. 212)—has hindered the development of research on the origins of new organizational forms. Thus, while existing organization theories “can deal with reproduction, or choice within given alternatives, none can deal with the genesis of the alternatives themselves” (p. 212).²¹

Padgett does not mean to imply that organizational genesis is a form of creation *ex nihilo*.

²¹ Though see Martin Ruef (2000) for a creative extension of the organizational ecology framework to model speciation.

Indeed, he emphasizes that organizational “birth is rooted in a logic of recombination” (p. 213). This recombination does not consist merely of ideas, people, and resources within the restricted domain of an industry or even the economy. Rather, Padgett argues that specialization is a process born of the interaction between the economic domain and other social domains, particularly the political and kinship systems. Since resources, personnel, and individual identity (in the form of multiple roles, each enacted in different domains) interrelate and are interdependent across sectors, this creates the potential for major change in one domain to transform another. That is, while each of the domains function relatively autonomously for stretches of time, their dynamic interdependencies allow for one to effect change in the others. Padgett regards this process by which “the autocatalytically stabilized logic of recombination in any one sector is regulated by the personnel and resource flows produced in other sectors . . . (as) the operational meaning of (*ibid*)” the idea that the economy is socially embedded.

Padgett’s historical research on the partnership structure among fourteenth- and fifteenth-century Florentine banks gives phenomenological flesh to his bare-bones model of organizational genesis. In particular, he describes four types of partnership structures: family firms (father-son or brother-brother partnerships), which prevailed through 1348; guild firms (master-apprentice), which prevailed from 1349 to 1378; social class-based firms (father-in-law-son-in-law or friend-friend), which prevailed from 1380 to 1433, and the Medici-era patronage firms (patron-client), which prevailed thereafter. According to Padgett, the change from one era to the next was precipitated by political crisis, which brought about a realignment (see e.g. James L. Sundquist 1983) of existing elite coalitions. Padgett stresses that realignment in the political sector need not engender change in the economic. Yet it can—and did, in the Florentine case—when banks faced emergency situations. When bankruptcy threat-

ened, the partners “often needed to reach into their . . . political and familial networks to stave off disaster” (p. 236). And in each era, this required the partner to highlight that aspect of their personal identity and corresponding networks that would allow them to be most effective at securing resources from the political sector. Finally, “once created under conditions of stress, moreover, organizational form reproduced through career recruitment . . .” (p. 236).

Padgett concedes that he cannot definitively document this story nor can he do more than sketch the process of transition from one era to the next. However, to the extent that one appreciates the general contours of Padgett’s model, it is worth reflecting on two key implications. First, it is notable how different his view of primordial networks is from that discussed by Rauch, Tienda and Rajman, or even Granovetter (1985). In Padgett’s account, no identities are truly primordial or exogenous. Rather, a personal identity encompasses a wide array of possible roles and associated networks. The question is which identity will be enacted or highlighted in a given situation (Sheldon Stryker 1980). Thus, even a seemingly primordial ethnic or religious identity should be regarded as salient only under particular conditions. Furthermore, changes or variation in those conditions—be they economic (e.g. Orlando Patterson 1975), political (e.g. David D. Laitin 1986; Roger V. Gould 1995), or social (e.g. Mary C. Waters 1990)—may be expected to reduce the relevance of some identities and increase the salience of others. And Padgett argues that actors may often be strategic about which identities and associated networks they stress.

Yet that agents may be strategic in how they define their social networks does not imply that social context (“embeddedness”) is irrelevant. After all, Padgett views the Florentine banking system as the tail that was wagged by the political dog. The point is that the Florentine bankers were strategic within the larger frame set by political

context. Padgett argues that networks between economic agents (within the firm) are reconstructed in response to the demands from other sectors—as indeed, he expects the other sectors to be similarly affected by the economic. Between crises, however, each sector is left to its own “auto-catalytic” devices. Thus ongoing economic activity is framed by the system of roles and identities set by historical conditions, which are themselves the outcome of interaction among the multiple sectors that comprise a society. Clearly, much work is yet to be done to flesh out this largely speculative model. Moreover, while Padgett argues that illustrating this model in the case of Florentine banking is particularly apt because this period and industry are thought to have witnessed the birthplace of financial capitalism, one wonders whether the kind of “embeddedness” he describes no longer characterizes modern capitalism, as Polanyi (1944) argued. Nevertheless, if Padgett or his followers are able to nail down a more precise version of his model of organizational genesis, this would be a major leap forward.

3.2 *Networks as Structures of Mutual Orientation*

The chapter by sociologist Ronald Burt commands particular interest because it addresses a claim made frequently both by sociologists (e.g. James S. Coleman 1988) and by economists (e.g. Greif 1993), albeit with some characteristic differences: that a dense network of “strong” ties (see Granovetter 1973) facilitates the emergence of mutual trust among members of the network. Burt labels this argument the “bandwidth hypothesis,” since it assumes that dense networks facilitate the information flow about reputation necessary for collective sanctions to be applied to shirkers. He is motivated to reexamine this hypothesis, which is often touted as the basis for a group’s “social capital,” because it mixes un-

easily with the argument for which he is best known (Burt 1992, 1997; cf., Burt 1982): that actors who occupy brokerage positions, by virtue of having large networks replete with “structural holes” between contacts, enjoy better access to information and enhanced negotiating leverage. Setting aside the skepticism with which an economist would greet such claims in the absence of a model for network emergence (Why might differential access to structural holes not be reducible to differential endowments? Why might structural holes persist despite rivals’ attempts to obtain similar advantages for themselves?²²) Burt focuses on the objection implied by the bandwidth hypothesis: that brokers have less effective relationships because they do not enjoy the same level of trust that is available to members of dense networks. Indeed, brokers can hardly be said to have an informational advantage if their sources cannot be trusted. Nor can they exert leverage over their exchange partners if the latter are loath to trade with them.

To counter this objection, Burt casts doubt on the assumption that dense networks transmit information about reputation more efficiently than do sparse networks. How could this assumption be false? Burt appeals to social psychological research (H. Paul Grice 1975; E. Tory Higgins 1992) and the reader’s intuition in proposing the “echo hypothesis,” according to which “third parties do not enhance ego’s information on alter so much as they . . . reinforce ego’s predisposition towards alter” (p. 41). The intuition is straightforward and compelling: when discussing a common acquaintance (“alter”) with someone (“ego”), we (acting as the “third party”) are less (more) likely to offer our true opinion—and may even state an opinion at odds with our own—when ego reveals a predisposition towards alter that is

²² On the exogeneity of structural holes, see Burt, Joseph E. Jannotta Jr., and James T. Mahoney 1998; on their rate of decay, see Burt 2002.

counter to (consistent with) ours.²³ Burt provides a useful discussion of the possible motives for such behavior, the most general of which goes under the heading of “etiquette”: one avoids contradicting the preferences of an interlocutor so as not to damage one’s relationship with that person. As a result, existing predispositions get reinforced since *ego* tends to hear reports that “echo” her initial bias.²⁴

In making this argument, Burt calls into question long-standing results from structural balance theory (Dorwin Cartwright and Frank Harary 1956; James A. Davis and Samuel Leinhardt 1972; Maureen T. Hallinan 1974; Paul W. Holland and Leinhardt 1971), which expects transitivity in triads of positive sentiment: *ego* befriends *alter* if *alter* is the friend of a friend. Burt argues that, in fact, relationships may be balanced in intensity rather than in valence. That is, *ego* develops a stronger opinion of *alter*, either positive or negative, if they share ties with numerous third parties who know each of them well. These third parties are not expected to share their true opinions but to shade their stated views based on *ego*’s predisposition, thereby reinforcing it.

Burt provides evidence from social network survey data of three organizations,

²³ Burt convincingly argues that, absent such a predisposition, his results would more likely confirm the bandwidth than the echo hypothesis. His evidence thus provides predictive validity for the assumption that *ego* typically has a predisposition. At the same time, Burt’s argument would be more convincing if direct evidence could be found that the echo effect works only where the third party observes *ego*’s predisposition. This would presumably lead towards some useful scope conditions around the echo hypothesis, which would tell us when we are likely to see it operating and when we are not.

²⁴ Burt’s explanation for why *ego* does not discount *alter*’s tendency to follow the rules of etiquette would not satisfy an economist who expects such behavior to be common knowledge in equilibrium. For Burt to be right, one must be willing to believe that actors might commonly behave in a particular way towards others (i.e., shading gossip out of etiquette) without being able to recognize when others are doing so. This can be thought of as a form of overconfidence (or even solipsism) whereby one attributes to oneself greater social skills than are possessed by one’s interactants. Upon reflection, it is not too difficult to believe that such errors are common.

which appears more in line with the echo hypothesis than with the bandwidth hypothesis. He shows that, consistent with both hypotheses, trust is more likely when a strong tie is embedded in a dense set of third-party connections.²⁵ Contrary to the bandwidth hypothesis, however, *distrust* is also heightened in such a structure. In addition, Burt shows that, in the one case where he has data on a complete network, ties do not balance in valence but they do balance in intensity. That is, *ego* is more likely to have a clear opinion of *alter*, positive or negative, when the dyad is surrounded by people who also have opinions, positive or negative, of one another. Burt considers the results “illustrative” rather than definitive, and this reader concurs.²⁶ Yet if confirmed and extended by future research, Burt’s analysis has important implications (on which, see below) for models of the transmission of information (about reputation) through networks.

The economist Joel Sobel reacts to Burt’s analysis by showing “how an economic theorist might model” the echo hypothesis. He succeeds in producing a game-theoretic model that roughly approximates Burt’s hypotheses but with which an economist would feel more comfortable. In place of etiquette as the third party’s rationale for sharing her true opinion, Sobel postulates

²⁵ Density is measured as the sum of third-party connections weighted by their strength (absolute valence). Thus, at intermediate levels of density, a dyad that is surrounded by numerous weak ties might have the same level of density as a dyad that is surrounded by a few strong ties. While reasonable, Burt might have benefited from clarifying this measurement strategy and subjecting it to some robustness tests. For instance, it would be useful to know whether the echo effect is more pronounced in the second case than in the first. For his part, Sobel seems to interpret Burt’s argument as pertaining only to this second case.

²⁶ Burt’s analysis would be more convincing if he had included both the positive and the negative third-party ties in the same equations in table 2.2 (which is poorly formatted). More description of the data and a justification of the analytic assumptions would have been helpful too. I am guessing that collinearity is an issue here, something that also goes unmentioned.

that the third party has a preference for “decisive action” on the part of ego.²⁷ Sobel also assumes that close third parties are more likely to know ego’s predisposition than are more distant third parties. These two assumptions carry the implication that only when a close third party has information that is not conclusive enough to outweigh ego’s predisposition will he state his contradictory opinion. Otherwise, he echoes that predisposition even when his information weakly contradicts it. Thus, as with Burt, Sobel describes a scenario in which third parties often reinforce ego’s initial bias.

Yet his model feels forced: the effect is akin to the Pharaoh’s magicians mimicking Moses’ miracles but ultimately falling short and certainly not achieving the same level of originality. Sobel grounds his model not in research or a compelling intuition regarding how people actually behave but in what is required to produce the patterns observed by Burt.²⁸ It is hard to see why it is more reasonable to believe that third parties prefer that their associates be decisive than it is to believe that they prefer not to damage their relationships with them. Moreover, he misses the larger point. Burt’s goal is not to challenge rationality (is it irrational to be polite?) but rather the widespread assumption that dense networks improve information flow. By constructing a model that depicts close third parties as choosing when to disclose their opinion depending on ego’s predisposition, Sobel merely reinforces Burt’s point that information transmitted through close ties may be misleading. Thus, the larger

implication of Burt’s analysis—that dense webs of strong ties may actually impede the transmission of accurate information about reputation—is actually strengthened.

We are thus left with a cautionary tale for those who would treat network links as passive “pipes” (see Podolny 2001) through which information or resources flow unimpeded. Such an attitude appears to be increasingly common as the analysis of large scale networks has gained popularity (e.g. Duncan J. Watts 1999; Albert-Laszlo Barabasi 2002). These analyses tend to use the same tools to analyze networks among individuals as they do to analyze physical networks of various kinds. And they typically assume that proximity between nodes in a given network implies a greater tendency to share the same information (or anything else flowing through the network, such as a disease). Yet Burt’s analysis suggests that proximity need not mean shared beliefs and may sometimes imply the opposite. And such effects will only be present when the nodes are human beings (or human collectivities) which filter and alter information in a way that physical entities cannot. More generally, Burt’s analysis introduces healthy space in existing research for the existence of diversity of opinion and information even in relatively closed and stable structures—a possibility that is rarely entertained by either economists or sociologists.

4. *Future Directions*

As should be abundantly clear from this review, *Networks and Markets* does not make for a cohesive volume in the sense that the contributions provide different approaches to analyzing the same question or at least different angles on the same phenomenon. Rather, the topic of economic networks serves as a “big tent” within which a wide variety of issues are explored. One might wish to limit this variance by promoting a more circumscribed definition for economic networks and the types of analyses

²⁷ Sobel is vague as to whether the third party cares whether ego makes the *right* decision. This is important because his discussion seems to suggest that the third party never lies but rather decides what to believe based on what ego will decide—an awkward assumption at best.

²⁸ Moreover, he does not fully succeed. One implication of his model is that ego is more responsive to information from weak than to strong third parties. This is contradicted by Burt’s results and seems unlikely to be true in general.

that should go under this heading. But calls for redirecting research communities, especially when they straddle multiple disciplines, are never heeded. Moreover, the lack of coherence in research on economic networks is to be expected. Since any system of mutual orientation among a set of actors may be regarded as a network, and since such orientation may take various forms and have numerous causes, it would be folly to demand that a single conception take hold.

Indeed, it bears recalling that, even among sociologists, there is really no such thing as social network *theory* as much as there is social network *analysis*—a set of frameworks and tools for analyzing social structure in its various forms. The great promise of social network analysis has been and continues to be its ability to give greater concreteness to sociological concepts that are relational in character. The relevance of network analysis to the study of economic behavior and institutions rests on exactly this promise. The market is a social structure in at least the three senses we have reviewed here: it often consists of patterned exchange, it is influenced by extra-economic affiliations, and it serves as a basis for mutual orientation among economic agents.

In closing, it is worth reflecting on what is perhaps the most famous statement on economic networks, that penned by Adam Smith in the *Wealth of Nations*: “People of the same trade seldom meet together, even for merriment and diversion, but the conversation ends in a conspiracy against the public, or in some contrivance to raise prices.” Smith’s view reflected and contributed to our prejudice against economic networks as collusive devices that gum up the workings of “the market.” And, as Casella’s concern about the exclusiveness of economic networks indicates, there is reason to worry about the efficiency of a capitalism that is governed strictly by “cronies.” But at the same time, imagine an economy that was somehow devoid of networks—i.e., where there was limited interaction among eco-

nomics agents and all interaction that did take place occurred anonymously, at arm’s length. Does anyone believe that such an economy would function as well as our own?

The upshot is not that “networks” are somehow superior to “markets.” Rather, the point is that a network perspective sensitizes us to phenomena that are missed when we regard the economy strictly through the lens of orthodox theory. At the same time, little is lost when we shift our perspective to viewing the economy and the larger set of domains in which it interacts as a system of networks. After all, we have seen that the market itself can be modeled as a network of exchange or mutual orientation. Emerging research on economic networks thus promises a better integrated set of conceptual tools for understanding the social and economic world. A review of the questions and issues raised by the chapters in *Networks and Markets*, however, suggests that there is much work left to be done to make good on that promise.

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