Review Article

Sociological imperialism in three theories of the market

FABIO ROJAS*

Department of Sociology, Indiana University

Abstract: This essay reviews three prominent sociological studies of firms and markets: Neil Fligstein’s *The Architecture of Markets*, Glenn Carroll and Michael Hannan’s *The Demography of Corporations*, and Harrison C. White’s *Markets from Networks*. The review essay discusses how economic sociology focuses on processes ignored by economists. However, research findings and theoretical developments are rarely reconciled or integrated with economic research. The conclusion discusses possible links between the varying schools of economic sociology and heterodox economic traditions such as contemporary institutional economics.

Introduction

The boundary between economics and sociology has weakened. Since the rise of rational choice theory, economists have felt that social institutions can be fruitfully understood as the outcome of utility maximizing behavior. Armed with microeconomic models and game theory, economists have analyzed a wide range of social behaviors such as marriage (Becker, 1973), voting (Downs, 1957), and even addiction (Becker and Murphy, 1988).

At the same time, a growing number of sociologists argue that firms and markets emerge from cognitive processes, culture, and social relations. From this perspective, economic analysis can be improved by opening the ‘black box’. That is, our understanding of economic life can be improved by de-emphasizing the neo-classical emphasis on stable preferences and utility maximization. Instead, analysts should focus on the cultural frameworks affecting preferences and the social institutions that facilitate profit seeking.

The intellectual trafficking between economics and sociology raises a number of obvious questions; namely, what is to be gained by using sociological theory?

*Correspondence to: Fabio Rojas, Email: frojas@indiana.edu

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Do these analyses add to the economics literature? While it may be true that economists have not developed a theory of culture and applied it to their own field, it does not necessarily mean that such a theory would increase our understanding of firms or markets. In short, what are the gains to trade?

This review essay examines instances of mutual concern to economists and sociologists to assess some trends in recent economic sociology. By focusing on a few key works in recent economic sociology, this essay explores the advantages of a sociological approach and possible contributions to economic thought. I have chosen three recent works of economic sociology that are considered to be exemplars in their respective traditions. The goal of this essay is not to comprehensively review each work, which is the culmination of years of research, instead I look for instances in which economists and sociologists have spoken on the same issues and ask if the sociological approach produces insights not found in other work. In some cases, this can be seen by comparing the empirical findings of both sociologists and economists. In other cases, I tease out the implications of economic theory to see if the theory is logically consistent with arguments made by sociologists.

This essay’s argument is that the evidence is mixed. In some cases, sociological analyses add a great deal to our understanding of economic phenomena, while, in other cases, the analysis seems to replicate what has been accomplished in industrial organization theory. Although this essay often adopts a skeptical stance, the goal is to identify tensions between economics and sociology that might help each discipline’s distinctive contribution.

**What is at stake?**

Before turning to the substance of this essay, it is important to ask what is at stake in the exchange between sociology and economics. Rather than seeing economics and sociology as participants in an intellectual division of labor, these two disciplines often see each other as competitors. Each side, to some extent, sees the tools of their discipline as a vital and unifying approach to the study of social life. For example, Gary Becker takes the strongest view in his treatise, *The Economic Approach to Human Behavior* (1976), when he states that economic analysis has the potential to organize all knowledge of social life:

> Although I am arguing that the economic approach provides a useful framework for understanding human behavior, I am not trying to downplay the contributions of other social scientists, or even to suggest that the economists are more important . . . At the same time, however, I do not want to soften the impact of what I am saying in the interest of increasing its acceptability in the short run. I am saying that the economic approach provides a valuable unified framework for understanding all human behavior. (13)

Of course, Becker represents one end of a continuum. Other economists cede more room to sociological analyses. An economist who is quite friendly to
sociology is George Akerlof (1984), who suggested that many sociological insights could be reinterpreted as preference descriptors. For example, George Akerlof and Rachel Kranton (2002), draw heavily upon the sociological literature to argue that people’s preferences reflect a desire to conform within a group. In an essay on rational choice theory, sociologists Douglas Heckathorn and Robert Broadhead (1996) echo Akerlof’s suggestion that social research might be based on a rational choice foundation. In Heckathorn’s view, researchers might translate sociological claims and findings into a neo-classical terminology. This approach to rational choice theory could become the basis of a future social science that reconciles economics and sociology. Sociology becomes the branch of neo-classical theory dealing with human interactions and socially oriented behavior.

Sociologists have taken a similarly strong stance in insisting that their field offers a unique and indispensable contribution. In the introductory chapter of the first edition of *The Handbook of Economic Sociology* (1994), Neil Smelser and Richard Swedberg argue that economic sociology is fundamentally different from mainstream economics, by which they mean neo-classical economics. Economists had failed to look at the sources of preferences and the historical contingencies that permit economic institutions to develop. Citing scholars such as Arthur Stinchcombe, they claim that economists see preferences as unproblematic. Economists should be aware of ‘meaning structures’ and how historical contingencies lead to specific preferences.

Smelser and Swedberg point to two major contributions that sociologists make: culture and social structure. First, sociological analyses focus on meaning and culture. Of course, Smelser and Swedberg do not suggest that economists believe that humans are unreflective actors. Rather, they point out that economic models depict humans as having easy-to-describe stable preferences that explain their actions. Most economic analyses assume preferences and deduce testable predictions. For many economists, investigating preferences might lead to empty explanations. As Stigler and Becker argued in ‘De Gustibus Non Est Disputandum’ (1977), appeals to preference variations might lead to *ad hoc* arguments. Therefore, economists should start with preferences and try to explain how people pursue their goals.

Economic sociologists argue that preferences are dependent on other aspects of social life that should not be ignored. A simple example: there seem to be important international differences in voter preferences, leading to different economic policies. Differences in voter preferences are due in large part to shared understandings of citizenship rights. These shared understandings of what individuals owe their government and how government should relate to the private sector constitute a ‘meaning structure’, a constellation of ideas that determine what is acceptable, appropriate, and logical. In the United States, for example, debates over economic regulation hinge on notions of free enterprise that do not fully exist in other nations. On a deeper level, Western economic institutions, such as the corporation, depend on ideas such as fiduciary
responsibility. Without the idea that one person should act as a responsible agent for another, the modern firm would not be possible.

The point of emphasizing culture is that utility maximization happens in a group context. What is considered optimal depends on socially constructed meanings. In a very important sense, individuals do not form preferences \textit{ex nihilo}; they do so through interactions with others. Preference formation, to some extent, is the outcome of a cultural transmission process, in which individuals learn how to interpret the world and how to evaluate the outcomes of their actions. Therefore, ‘meaning structures’, by which sociologists mean socially shared rules for understanding or evaluating the world, are salient to economic analysis.

The second sociological contribution is an emphasis on social structure. Repeatedly, Smelser and Swedberg insist that the economy is ‘in’ society. This may seem like a trivial statement, but sociologists have something more concrete in mind. What they mean is that markets and firms depend on other social institutions, such as religions, families, and the state. Sociologists also argue that consumers, firms, and entire economies depend crucially on things such as status orders, identities, social networks, and group dynamics. Smelser and Swedberg would argue that economic analysis is incomplete because it takes these institutions and social dynamics as ‘given’. Weber’s Protestant ethic thesis illustrates this point well. According to Weber, market institutions – such as the firms arising in early modern Europe – were developed by Protestants with a particular religiously inspired work habit. Greatly simplifying the argument, religious habits were imported to the economic realm, which transformed Western markets. In short, the capitalist economy is an outgrowth of Western religious institutions. For Weberian scholars, any account of the rise of the West that ignores this important link is incomplete. A few Weberians have formulated a ‘civilization’ analysis of markets that situates markets as one of a large array of institutions reflecting fundamental social values (Hamilton, 1994).

In the remainder of this essay, I look at three recent books that make the case for culture and social structure as explanations for economic institutions. Each book is the culmination of an ambitious research program looking into the structure of firms and industries from the sociological perspective. In their own way, each book forcefully argues that culture, meaning, and social structure are all of great relevance to the study of economic institutions. \textit{The Architecture of Markets} summarizes Neil Fligstein’s research on industries and state regulation. It adopts what is known as the neo-institutional approach, which views markets as heavily impacted by culture. The other two books focus more on social structure and the economy. \textit{The Demography of Corporations and Industries}, by Glen Carroll and Michael Hannan, analyzes competition within industries. \textit{Markets from Networks} (2002) is Harrison White’s explanation of the role of networks in creating market niches. In each case, I focus on issues that have captured the attention of both sociologists and economists in order
to see how attention to culture and social structure leads to advances in research.

**Neo-institutional architecture**

The most sociological approach to firms and markets is the new institutional school. The hallmark of neo-institutional sociology is its focus on the organization’s political and legal environment, which, to sociologists, means those persons and groups beyond the firm’s boundaries who shape public opinion, government policies, and the informal conventions coordinating economic behavior. For the neo-institutional school of sociology, firm structure and economic behavior are culturally constrained. Any firm’s structure must be viewed as legitimate, which relies on society’s notions of what is progressive and rational. Otherwise, the firm is subject to political interference. What an organization does and what it looks like must be viewed as ethical, or, at the very least, consistent with the values informing the broader political culture. Talcott Parsons (1956) succinctly expressed this view in an article addressing firm structure and culture:

> The essential point is that the conduct of the affairs of an organization must in general conform with the norms of ‘good conduct’ as recognized and institutionalized in society. The most general principle is that no one may legitimately contract to violate these norms, nor may authority be used to coerce people into their violation. (84)

The research now labeled as new institutional sociology builds on the insights of Parsons and his followers. A common theme in the 1970s and 1980s is the interaction between managers and the public. For example, Meyer and Rowan (1977) made the strong claim that firm structure was a signal to those persons and state agencies that grant legitimacy and resources. By instituting a board of supervisors, for example, a hospital acquires the legitimacy needed to conduct its business because the board signals to the public that the hospital staff accepts public accountability as a value. The board of supervisors is not created to maximize profits, except in the broader sense that it permits the hospital to operate within the confines of the law and public opinion.

Later neo-institutional scholars, and economic sociologists more generally, have scaled back such strong claims. Mark Granovetter (1985) urged sociologists not to reject rational choice explanations but to view profit seeking as occurring within socially constructed domains of action. W. Richard Scott’s (1991) essay, ‘Unpacking Neo-Institutional Arguments’, argues that industries lie on a continuum. Some organizations, such as schools, have inherently vague goals; others, such as manufacturers, have well-defined tasks. Politics and culture will have the greatest impact on firms and markets when organizations have conflicting or ambiguous goals.
Current neo-institutional theorizing recognizes that firms seek both profits and legitimacy. Recent new institutional work views firms and markets as social entities constructed from economic conventions, profit-maximizing routines, and state regulations. In the words of management scholars Paul Ingram and Karen Clay (2000), new institutional theory is about ‘rationality within constraint’; profit-seeking behavior occurs within culturally determined rules. The construction of a large firm is a complex amalgam of elements drawn from the firm’s cultural and political environment, as well as a response to the technical needs of the firm.

In *The Architecture of Markets* (2001), Neil Fligstein offers a comprehensive account of firms and markets that depends heavily on new institutional thinking. A key figure in the sociology of markets and organizations, Fligstein offers a ‘political–cultural’ theory (15–20). In justifying his approach, Fligstein offers a cultural model of economic action. He does not work with neo-classical assumptions, namely, that individuals respond to increases in expected utility. Nor does he use the modified assumption that behavioral economists employ, which is that individuals are responding to expected utility but have systematic biases in perceptions and information processing. Instead, Fligstein asserts that individuals employ interpretive schemes. When individuals interact, they view the social world as a collection of positions – such as ‘buyer’, ‘seller’, and ‘state regulator’. Market interactions are like any other form of social interaction; there are dominant participants, new participants must engage with existing relationships, and most individuals work to enforce and reproduce existing social orders. Social groups with positions of power and social relationship are called ‘fields’, a term borrowed from social theorist Pierre Bourdieu (1977).

For Fligstein, the goal of economic sociology is to understand how economic ‘fields’ are created and reproduced over time. Fligstein’s main argument is that stability, in the sense of preserving existing relationships, not profits, is what firms and states care most about. ‘The theory of fields implies that the search for stable interactions with competitors, suppliers and workers is the main cause of social structures in markets’ (18). Like others in the new institutional school of sociology, Fligstein focuses on what happens outside the firm’s boundaries. The key point is that states and dominant firms are in a position to impose firm structures and social norms that allow managers in dominant firms and political authorities to create stable markets. For Fligstein, firms are risk-averse entities and do what they can to avoid uncertainty and failure.

The state plays a central role in *The Architecture of Markets*. The state is the tool by which various interest groups structure markets. ‘State building can be viewed as the historical process by which groups outside the state are able to get domains organized by the state to make rules for some set of societal fields’, Fligstein writes. ‘At any given moment, there are political projects in the fields that make up states (e.g., “normal politics” and social movements oriented toward altering an incumbent’s ability to set rules)’ (16). Fligstein’s goal is not
to use ‘fields’ and his cultural–political approach to displace profit seeking in theories of firm and market structure. He aims to shed light on the ways that culture and politics lead to the creation of stable arenas where profit seeking occurs (18).

Of course, Fligstein is not unique in observing that individuals work to stabilize their markets through state action. Stigler’s (1968) work on regulatory capture, for example, is all about how industry insiders co-opt the state in order to establish favorable rules and exclude competitors. Much of the public choice literature is dedicated to the ways in which market actors influence the state in order to minimize the uncertainty associated with state regulation.

The public choice theory work deriving from Stigler illustrates an important difference between Fligstein and these other writers. Fligstein’s work on state organization of industry has to do with how interest groups create the state itself and how regulations reflect the interests of major lobbying groups in society. The action often happens ‘outside’ the market; e.g., anti-discrimination statues are demanded by civil rights groups who have nothing directly to do with the market itself. In contrast, the public choice tradition finds that individuals with strong ties to the regulated firms are able to assert control. The public choice tradition views the relationship between interest groups and the state as emerging internally from the market, an outcome of conflicting interests between the regulators and the regulated.

It is interesting to note that the public choice literature makes very little appearance in Fligstein’s work. Aside from a passing reference to Stigler, there is very little sense that Fligstein appreciates how markets can be a source of social power that undermines the democratic control of markets. This highlights what some have identified as a weakness of institutional theory, a tendency to emphasize social influence at the expense of thinking about incentives. It should be noted, however, that some sociologists have shown how firm interdependence leads to political unity, which enhances an industry’s influence on the state and the mitigation of democratic control over markets (e.g., Mizruchi, 1992; Burris, 2005).

These comments about the differences between Fligstein and public choice theorists raise a broader question. How much does Fligstein advance economic research on the topics covered in Architecture? What does a new institutional approach tell us that economics does not? From the sociological perspective, Fligstein’s studies are a major advance. As other reviewers of Architecture have noted, sociologists have had relatively little to say about the cultural determinants of firm structure until the rise of new institutional sociology (Abolafia, 2002). Prior work focused on the technical determinants of firm structure. Key works include J. D. Thompson’s (1967) open structure theory. Although prior scholarship ascribed some role to processes beyond the firm’s boundary, few writers before the new institutional sociology sought to fully explore the implications of assuming that the firm’s political environment was of paramount importance.
The contribution to economics is less clear. Fligstein addresses many topics that have received much attention in the economic literature. He examines corporate governance, determinants of firm structure, national labor agreements, and globalization. Some of his findings, while supporting his larger points, are not surprising or repeat prior research. Other points require further analysis. For example, a central finding is that large firms set trends in industries and entrants copy these firms. At the same time, Fligstein and other sociological neo-institutionalists would admit that new firms often introduce new structures that upset the balance of power in an industry. Interesting recent examples include Amazon.com and Federal Express, which changed their industries in profound ways. What remains unanswered, and mostly unaddressed, is an explanation of when challengers succeed in upsetting an industry and the standards imposed by dominant firms. A neo-classical economist would expect that it is mostly the economic superiority of a new organizational form that transforms an industry, while a neo-institutionalist might suggest that industries might change because of a weakness in the cultural mechanisms that sustain a given industrial configuration. These conflicting accounts are not addressed in Architecture.

In his study of corporate governance, Fligstein acknowledges that much attention has been paid to control within economics, such as seminal work on principle-agent problems and transaction cost economics (Williamson, 1981). How does Fligstein contribute to these literatures? Going beyond principle-agent theory and transaction cost economics, Fligstein offers his political–cultural approach. The key to understanding firm behavior is to realize that ‘groups within firms create and maintain power as a function of their ability to promote organizational survival and create stable markets’ (129). Groups that create stability have the power to determine governance rules within firms and markets. Therefore, the political–cultural theory directs the researcher to examine how groups rise to power within firms and, subsequently, how they set rules within and between firms.

Motivated by his political–cultural theory, Fligstein looks at who leads firms. Economists rarely ask this question (see Helland and Sykuta, 2004, for a recent exception). They often look at contracts, incentives, and monitoring costs, but rarely do they examine the career histories of firm leaders. Presumably, this is irrelevant from a purely economic perspective. From the economic perspective, what matters in corporate governance is whether shareholders can write contracts with the correct incentives that are also easy to enforce. Fligstein’s perspective is that the individuals eligible for leadership positions are those who have already developed some basis of power. What Fligstein argues, and this is well supported in historical studies of US business, is that corporate leadership changed in the twentieth century. Executives with careers in sales ceded firm leadership to finance officers because finance increasingly became an important tool for stabilizing cash flows in large, multidivisional corporations. Therefore,
Fligstein predicts that a leader with a finance background has a positive effect on firm performance, because such persons know how to stabilize income flows and firms as a whole.

To support his hypothesis, Fligstein estimated the effects of leadership on measures of corporate performance such as cash flow and price/earnings ratio and other corporate behaviors such as mergers. He finds that the backgrounds of leaders matter. Firm presidents with finance backgrounds were more likely to head profitable firms that have high stock market evaluations. Finance presidents were also more likely to merge their company with another. These two findings motivate Fligstein’s main theory: that a new, more profitable approach to business emerged in the 1970s that focused on creating stability within firms by managing cash flows, and that a specific group of managers used these techniques to rise to power within US firms. From this position of power, firm leaders sought to stabilize markets by initiating mergers, a strategy that was facilitated by CEOs’ knowledge of finance.

This is a plausible story, and it lends credence to Fligstein’s theory that the power to shape firms derives from the ability to stabilize them. But does this finding really depend on his political–cultural theory? For example, might not the selection of officers with finance careers signal a board’s belief that people able to handle the complexity of modern finance are highly qualified to deal with a firm’s complex market and legal environment? A basic finding in the study of professions is that they are internally stratified. Technically demanding and highly compensated specialties tend to attract the most talented individuals. With the innovation of the capital asset pricing model (CAPM) and Black–Scholes theory in the 1970s and 1980s, finance quickly became one of the most cognitively demanding specialties in modern management. Could a career in finance not be seen as a signal of a manager’s ability? Nowhere does Fligstein deal with such straightforward alternative explanations.

There are other points in *Architecture* where the empirical work can be criticized. For example, Fligstein examines the effects of interlocking directorates (instances when firms share board members) in chapter 6 and finds there is not much effect on firm structuring decisions (131). However, the next chapter presents evidence that they do matter, although he uses a different measure (162). There is no discussion of this inconsistency.

Furthermore, Fligstein does not include more traditional predictors of firm performance and mergers in his regressions. In his analyses, he includes sociological variables – such as the background of firm presidents and the firm’s relationship to other firms. He also includes measures of the other actors who have interests in the firms (banks, for example), but he does not include measures such as prior performance, the competition, and other measures of market conditions, which are standard in economic analyses of firm performance. Therefore, it is hard to judge if Fligstein has extended or substantially modified the basic economics of the firm.
Despite these problems, Fligstein’s work raises an important issue. Profit seeking does not exist in a cultural or political vacuum. The important point is that somebody has to understand profit opportunities. There must be a community of professionals who understand and exploit economic opportunities. This is often the ‘black box’ of economic theories of the firm, how individuals learn about profit opportunities. Rarely do economists ask who in the firm is most likely to understand these opportunities. Nor do they ask how success in identifying new market environments disrupts or changes politics within the firm. Fligstein’s emphasis on control yields a valuable insight with respect to these issues.

In general, Architecture draws attention to processes not addressed by economists but does not fully integrate or confront economic theories. Therefore, it is difficult to fully assess how much Fligstein extends economic thinking on the various empirical topics addressed in his book. I suspect that some effects reported by Fligstein might disappear or be attenuated with the inclusion of variables found in the economic literature. Other scholars have made similar criticisms of neo-institutional analyses of firms and markets. For example, in a recent issue of American Sociological Review, sociologist and management scholar Ezra Zuckerman (2004) argued that Zajac and Westphal’s (2004) neo-institutional article on stock buy-backs failed to fully acknowledge the economic research done on the topic. Until neo-institutional scholars more fully engage with the economic literature, it will be hard to tell exactly how much purchase cultural theories have in explaining economic behavior.

The population dynamics of industries

A second research tradition in economic sociology is the population ecology of organizations. Population ecologists, also known as organizational demographers or organizational ecologists, answer questions about a firm’s behavior in terms of the number of other firms competing for the same resources. The term ‘population ecology’ is drawn from the biological sciences. Biological ecologists try to understand an organism’s structure from its environment, specifically how competition with other organisms determines the dynamics of a population of similar organisms (e.g., how fish in a river compete with other species of animals). Similarly, organizational ecologists believe that the key to understanding an industry (or population of firms) is the number of firms competing over available resources. For population ecologists, the growth and contraction of industries is of utmost importance in understanding individual firms.

The main expositions of population ecology theory are Hannan and Freeman’s 1977 article in the American Journal of Sociology, Hannan and Freeman’s book Organizational Ecology (1989), and Carroll and Hannan’s recent treatise, The Demography of Corporations and Industries (2000). The theory presented in these works builds on the works of dozens of other researchers who have tested the predictions of population ecology in various industries and who have
pushed population ecology theory in new directions through formalization. The
unifying theme in all of this research is that the organizational population is the
unit of analysis, and that population dynamics have important effects on firm
structure and behavior. In the present essay, discussion of population ecology is
drawn from *Organizational Ecology* and *The Demography of Corporations
and Industries* because they contain the most comprehensive and detailed
explanations of the theory to date.

Aside from its explicit modeling on biological theory, organizational ecology
is remarkable for its use of cultural mechanisms to explain population dynamics.
Organizational ecologists start with a specific theory of firm inertia (Carroll and
Hannan, 2002: 3–7). Following Coase (1937), firms and other organizations
are islands of hierarchy within a decentralized market. A key argument is that
organizations have great difficulty adapting to their environment, called ‘inertia’
in this literature. Carroll and Hannan offer economic and sociological arguments
for structural inertia. For example, organizations have internal politics that limit
the ability to change technologies; organizational managers may be unable to
identify the proper response to environmental change; and sunk costs may inhibit
needed changes. In an early chapter of *Demography*, Carroll and Hannan suggest
that a firm’s structure is reinforced by outsiders, who ascribe an identity to the
organization. Thus, a firm’s structure is difficult to change not only because of
internal dynamics but also because of its external cultural environment.

Population is another central concept in ecological theories (Carroll and
Hannan, 2002: 59–82). The ultimate goal of all ecological analyses is to
understand the effects of competition on firms within a given industry. This
requires ecologists to define the population of organizations, by which they mean
all those organizations that share a similar structure and aim to provide similar
sets of goods and services. For organizational ecology, inertia and population
are closely related concepts; if firms can effortlessly change goals and structure,
then it is difficult to speak of distinct organizational populations.

Viewing industries as a population of similar firms competing for the same
customers and funds, organizational ecologists deduce predictions about firm
closure rates (called ‘mortality’ rates) from theories of organizational legitimacy,
structural inertia, and niche width. The argument is as follows: when industries
are young, firms have problems surviving because they are not perceived as
legitimate. Because providers of capital do not perceive the firm’s activity as
legitimate, they are reluctant to invest. Firms in young industries have difficulties
recruiting talented managers and labor. Consequently, they find it difficult to
survive. As an industry ages, its endurance creates legitimacy. Firms find it easier
to acquire the resources that ensure survival in a mature industry. When there are
large numbers of firms in an industry, competition increases the mortality rate
because resources are finite – there are simply not enough customers or donors
to provide all the resources needed by firms.

The ecology approach to firm dynamics mixes both economic and cultural
arguments. On the one hand, ecological theory depends crucially on a basic
model of competition. However, in uncompetitive environments, legitimacy is the key mechanism. Perceptions determine the ability of new firms to survive. This line of reasoning is summarized in Theorem 10.3.2 (Carroll and Hannan, 2002: 228):

Theorem: Organizational mortality rates ($\mu$) initially fall with increasing density, reach a minimum, and then increase with further increases in density

$$\mu(t) \equiv \kappa \mu(t) \frac{\varphi(N_t)}{\min(\varphi(N_t), \ell)}, \text{ and}$$

$$\mu(t)' \equiv \frac{d\mu(t)}{dN_t} < 0, \text{ if } N_t < N^*_\mu \text{ and } \mu(t)' > 0 \text{ if } N_t > N^*_\mu.$$  

Here $\varphi$ denotes the effects of legitimacy, $\ell$ denotes a limit on the positive effects of legitimacy, $N^*_\mu$ is the critical point in density at which mortality starts to increase, $\varphi$ denotes the effects of competition, and $\kappa$ is the effect of other relevant conditions, which are historical and industry specific. The hypothesized functional form of $\mu$ can vary. Empirical tests of the mortality theorem use different functions. For the purposes of the present essay, it is sufficient to note that the empirical prediction is that density on firm mortality rates is ‘U-shaped’ because of the combined effects of inertia, legitimacy, and competition. Carroll and Hannan provide a similar theorem about firm creation rates.

This theorem, and its counterpart for firm creation rates, is population ecology’s main theoretical tool. Although by no means uncontroversial, population ecology has become the default theory for sociologists when they try to understand competitive inter-firm processes. Sociological analyses of intra-organizational competition usually use theorems like the one described above to justify estimating the quadratic effect of the number of firms (‘density’) on the probability of failure. For example, a recent article by James Montgomery (2003) uses concepts of density and niche overlap to explain which Protestant religious organizations survived in the nineteenth century. Other sociologists have used ecological theory to assess the effects of competition among universities (Rojas, 2003: 117–142) and Internet/media organizations (Zuckerman and Rao, 2004), in addition to the dozens of studies of the same industries published in leading sociological journals by self-identified ecologists. Hannan and Freeman (1989: 201–330) extensively review these later studies.

Despite the success of ecological ideas within sociology, a number of critics have asked if ecological theory is logically coherent (Young, 1988). A common theme in these criticisms is that ecologists use imprecise and confusing concepts and theories. Young (1988) took issue with ecologists’ use of the species metaphor, as did Perrow (1986: 208–210). Other critics (Baum and Powell, 1995) argue that ecologists ignored important cultural processes in the drive to present a simple theory of density-mortality effects. A number of scholars have refined ecological theory by arguing that legitimacy means different things at
different points in the history of an industry. The focus on a uniform environment and industries with many firms ignores oligopolies that control the environment and the historically specific factors that permitted such situations (Perrow, 1986: 213–218). Perhaps the most difficult criticism, which has much in common with the present essay, is that ecological theory is a stochastic model that makes predictions about aggregate behavior attributable to multiple sources (White, 2002: 225). That is, ecological theory only describes the outcomes of legitimacy and competition, time-dependent birth and mortality rates, which a critic may explain by appeal to other processes.

An economist approaching organizational ecology might ask how it improves upon standard industrial organization theory, which ascribes a firm’s ability to profitably operate in terms of the number of competing organizations. Contemporary industrial organization theory seems to address many of the same questions as organizational ecology. For example, standard industrial organization theory asks: given an average cost curve for producing goods and consumer demand, how many firms can profitably operate in a market? Economists have provided an answer to this question. Rather than discuss niche width, industrial economists rely on the concept of consumer demand, the total willingness of consumers to pay for the services rendered by a collection of firms. It seems to be the case that population ecologists and industrial economists are referring to similar concepts when addressing the issue of how many firms can survive in a given market, although ecologists might consider ‘niche’ to include both firm inputs and outputs.

It is useful to closely examine industrial organization theory to see how it resembles organizational ecology. Intuitively speaking, industrial organization theory predicts that the number of firms cannot exceed the ratio of the quantity demanded at minimal average cost and the unit price at minimal average cost. Some version of this result is often found in industrial organization textbooks and is frequently expressed as the following result (Panzar, 1989, following Baumol, Panzar, and Willig, 1982):

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\text{Proposition 1: Let } y \text{ denote the total output of a firm. Assume that the average cost function is monotonically decreasing (increasing) before (after) some minimum point } y^M. \text{ Then the number of firms in a feasible (profitable) and cost-minimizing configuration is less than or equal to } \left[ \frac{Q[AC^M]}{y^M} \right] \text{ and there exists at least one feasible and efficient set of firms of size } \left[ \frac{Q[AC^M]}{y^M} \right] - 1; [x] \text{ is the smallest integer greater than or equal to } x. \]

As noted, this theorem addresses the carrying capacity of an environment but not mortality rates, which is what organizational ecologists care about. However, this simple static model can prove something about firm mortality rates. For instance, in this basic industrial organization framework, if \( \left[ \frac{Q[AC^M]}{y^M} \right] > 1 \), then firm mortality will be zero until the number of firms exceeds \( \left[ \frac{Q[AC^M]}{y^M} \right] \). In the basic framework of industrial economics, inertia is not discussed and it
is assumed that firms can adequately respond to market conditions and will be profitable until $[Q[AC^M]/y^M]$ is exceeded. This can be summarized in the following proposition:

Proposition 2: Assuming that $[Q[AC^M]/y^M]$ is sufficiently large, firm mortality rates will be zero until either $[Q[AC^M]/y^M]$ or $[Q[AC^M]/y^M] − 1$ is exceeded.

Once $[Q[AC^M]/y^M]$ is exceeded, some firms will become unprofitable and have to close. After some firms close, profitability will return and firms can continue to operate. The number of firm closures will depend on excess production in the industry. From this line of reasoning and the previous two propositions, we can deduce another proposition:

Proposition 3: Mortality rates will be zero until the number of firms exceeds $[Q[AC^M]/y^M] − 1$, and mortality rates will monotonically increase as a function of density after this point.

This proposition assumes that firms will perfectly divide up the market. Firms will make no errors in producing goods or in converting income into profitability. A more sophisticated analysis would take into account these limitations. A more elaborate theory might also stipulate time-dependent cost curves, which affect firm closure rates.

The purpose in presenting this theoretical exercise is simply to point out that with a little effort, standard economic theory can produce a theory of organizational population dynamics. Using what is found in standard references, I was able to formulate a rudimentary theory of firm closure rates based solely on well-accepted economic theory. Rather than existing beyond the scope of current economic theory, a theory relating market density and firm closure derives readily from neo-classical economic analysis.

This theoretical exercise also shows the differences between organizational ecology and industrial organization. An important difference is that organizational ecologists depend on legitimacy in their explanations. Firms are likely to close in developing markets simply because customers, banks, and others are unwilling to give money to a new business. Economists do not discuss this cognitive aspect of market development. They often see the failure of firms in new markets as simply a matter of firms not finding customers or not learning quickly enough how to be efficient. According to ecological theory, lonely firms are in danger of closure. In contrast, under appropriate conditions in the industrial organization perspective, lonely firms have the market to themselves. Therefore, they have unobstructed profit opportunities.

Organizational ecology has an economically counter-intuitive implication: firms are more likely to fail when the market is least competitive than when there are moderate levels of competition. Rather than going where economists have not gone, as Carroll and Hannan might argue, organizational ecology has produced a theory at odds with basic economic intuition.
What is one to make of this claim? One possibility is that industrial organization theory is wrong and organizational ecology is right. The economist is incorrect when she sees new firms as operating in an environment full of previously unexploited opportunities. Instead, firms in newer industries operate without the institutional safety nets that firms in established industries enjoy. Furthermore, dozens of empirical studies find the predicted density effects. In most of these studies, the main evidence is that the density and density-squared variables have, respectively, positive and negative coefficients. This yields the ‘upside-down U’ shape predicted by the theorem listed above.

One response would be to articulate plausible alternate mechanisms for the same statistical effects. For example, it is entirely possible that firms in newer industries simply have not gone through the learning process, a possibility ecologists allude to in their writings. Presumably, each industry has its own practices that help firms survive and become profitable. By definition, a firm in a new industry would not have access to managers and technicians with deep knowledge of a product or the firm’s clients, or to the technologies that help a firm solve its problems. To take but one example, early personal computer software firms frequently failed simply because they did not know how to generate enough revenue from software because pirates routinely distributed free copies. Until managers developed software licensing tools in the 1980s, software firms were routinely put in danger of bankruptcy by pirates. For example, Microsoft suffered financial misfortune in the 1970s when pirates distributed copies of its BASIC programming language after the firm had spent thousands of dollars developing the software. If one were to estimate the effects of software firm density on firm failure, then the effect of learning to prevent product theft would be conflated with the effects of low legitimacy.

Another response would assert that ecologists have focused on one functional form in their research. That is, almost all ecological analyses search for a curvilinear relationship between density and mortality. This is natural because the quadratic density–mortality relationship is the theory’s main prediction. In the absence of compelling theory, researchers should not randomly search for other relationships in the data. However, given the divergence between industrial organization theory and ecological arguments, it is worth asking if firm mortality data can support other models, and, if so, which models fit best. It might be the case that some industries do not require much support from investors. Such industries might be characterized by low start-up costs, minimal human capital requirements, and simple technologies. Firms in that sort of industry would not require the legitimacy granted by investors. As long as consumer demand was satisfied, firms could easily operate without the aegis of social legitimacy. It might also be the case that standard industrial organization is incorrect. Perhaps estimating a model in which mortality is zero below the industry’s carrying capacity will show that the hypothesis implied by industrial organization theory are incorrect. It is currently an untested hypothesis.
Organizational ecology has succeeded in large part because of its relentless emphasis on density effects. More than any other theory in organizational studies, ecology has chosen a single variable of interest and provided an interesting explanation of its effects that combines structural and cultural arguments. In doing so, ecological theory has produced interesting predictions, which have received ample empirical support. In comparing ecological theory to industrial organization theory, my goal is not to refute or unconditionally accept either. Rather, it is to push the limits of ecological theory by using concepts from industrial organization economics. As ecological theory enters its maturity, it will receive more attention from economists and management researchers, and competing explanations will be offered.

It’s all about connections

In Markets from Networks (2001), Harrison C. White presents an ambitious theory of markets using network theory and sociological identity theory. White’s goal is nothing less than using network analysis to generalize a wide range of economic research. Markets from Networks is more ambitious than either Caroll and Hannan’s Demography or Fligstein’s Architecture of Markets. It seeks to provide a grand unified theory linking sociology and economics.

A difficulty in understanding White’s book is that he employs a highly idiosyncratic vocabulary as well as a great deal of formalism. His texts are filled with capitalized words such as ‘TRUST’ and ‘PARADOX’ and plenty of mathematics.

However, White’s theory is built on neo-classical foundations. It can be explained in terms most economists and sociologists would find familiar. First, it helps to understand that White is synthesizing network analysis with neo-classical analyses of market structure. His goal is to get beyond models of markets in which an undifferentiated mass of firms compete for consumers. Instead, White sees markets as forming complex systems of interactions. He sees a market as a long chain of trading relationships. In a word, a market is a network.

It is important to note that sociologists use the word ‘network’ differently than economists. In economic research, a network denotes a group of individuals who use the same technology or standard. For example, Windows users might be described as a network, a community defined by a shared operating platform. The question for economists is the benefit of establishing or joining a network. In contrast, sociologists have a rather different notion of network that is highly geometrical. A network is a pattern of social ties among individuals or firms. A tie can be broadly defined as trade or socially defined as in friendship. The purpose in examining patterns of social ties is that such an analysis reveals central actors and group structure.

White proposes that markets be thought of as networks, a substantial departure from the standard economic model. Consider the standard theory
of industrial organization, as described in the previous section. In that model, I
assumed a mass of firms competing for a fixed customer base. The firms do not
interact with each other, except by virtue of their competition. Of course, I do not
mean to imply that economists ignore firm interactions. There are voluminous
literatures on mergers and vertical integration. However, these studies often
focus on pairs of firms as the unit of analysis. For example, economists often
ask when it is profitable for a firm to merge with its distributor, or if it is better
to maintain a contract with another enterprise. White seeks to move beyond
dyadic analysis and study markets as complex buyer–seller chains. In much the
same way a biologist looks at food chains, White wishes to view markets as
buyer–seller chains.

What is to be gained by viewing a market as a social network? For White,
the main advantage is that the firm’s activities are determined by where the firm
is located within the network, subject to competition and profitability. White’s
theory merges the economic notion of market niche with the network theoretical
notion of social position. As in standard industrial organization theory, firms are
defined by a market profile – the volume produced of a certain good. The market
profile defines an identity, which further defines who the firm interacts with. It is
an endogenous process. For example, once a firm sells computer hardware, then
other actors recognize it as a hardware seller and insist on interacting with it on
those terms (53). For White, position in buyer–seller chains and social identities
are mutually reinforcing concepts.

White uses modified Cobb–Douglas functions to formalize his theory. The
starting point should be familiar to any economist. The cost of a firm’s output is
\( C(y, n) = q \cdot y^c \cdot n^d \). Similarly, the value of a firm’s output to a buyer is
\( S(y, n) = r \cdot y^a \cdot n^b \). The quantity \( S \) indicates how expensive it is for the buyer
to consume the product. In these equations, \( y \) denotes the volume produced and
\( n \) is an integer-valued index of quality. By describing both inputs and outputs,
White links market profiles to a position within a network of exchanges. An
exchange, i.e. a network link, between two firms can be sustained if goods of a
given quality and quantity can be profitably produced and consumed.

White pays much attention to the ‘market plane’, a two-dimensional vector
space describing the microeconomics of an exchange between buyer and seller.
For White, every producer–consumer relationship is defined by the parameters
in these two equations. Therefore, to understand patterns of trade and industrial
organization, one should ask if a given market configuration is profitable given
the parameters \( a, b, c, \) and \( d \). White simplifies his analysis by focusing on the
two ratios defining relative prices of goods \( a/c \) and the relative product quality
\( b/d \). Regions in this two-dimensional space are defined by the \( a/c = b/d, a/c = 1, \)
and \( b/d = 1 \) lines. White labels different regions of this two-dimensional space
with words like TRUST, ORDINARY, and PARADOX.1

1 White capitalizes the names of regions in the market plane. I retain his grammatical practices.
Exploring life in the ‘market plane’ is the book’s main goal. Using this terminology, White presents a comprehensive market formation theory. The positions available in an exchange network depend on the position’s economic sustainability. Any exchange between buyer and seller imposes costs determined by the ratios $a/c$ and $b/d$. If a firm’s exchanges with buyers and suppliers are profitable as determined by these numbers, then the firm will maintain the tie. Profitability determines the links between buyers and sellers in markets.²

Of the economic sociology reviewed thus far, White’s network theory is closest in spirit to neo-classical economics. Actors have clearly defined preferences and are profit seekers.³ There is little resort to cultural theories, except in pointing out that market positions seem to reinforce perceptions of the firm’s market role. One might be tempted to say that White’s theory is a straightforward extension of neo-classical economics because it situates utility-maximizing behavior in a network context. White says so himself:

The two academic disciplines of sociology and economics are melded in the present modeling approach, which is intended as a framing for a new economic sociology . . . The argument melds optimization derivations from neoclassical microeconomics with social constructions from network sociology. It rejects the system-perfectionism from economics and case exceptionalism from sociology. (323)

The question is whether this new framework gains purchase on problems within economics. White’s book is too ambitious to cover in a single essay. It subsumes a wide range of economics within its analytic framework. Literally, dozens of economic topics are reframed in ‘market plane’ terms.

However, much can be gleaned from selected chapters that illustrate White’s theory. For example, there is a chapter on ‘PARADOX’ markets, in which cost decreases as product quality increases ($d < 0$). The second half of the chapter delves into an important topic, labor market signaling, but it is hard to tell what White contributes to this area. White rehearses Spence’s (1973) seminal analysis of labor market signals and then shows how the expanded Cobb–Douglas model encompasses Spence’s theory. The payoff for showing that Spence’s signaling theory is a special case of White’s hybrid Cobb–Douglas/network approach is noting that some of the technical limitations of the original Spence model are not limitations at all. Educational labor market signals work in a broader range

² In general, network theory’s main goal is not to explain the attributes leading to tie formation, but to start from the assumption that ties indicate a larger social structure. Many network theorists would suggest that the large-scale pattern is what matters (White, Boorman, and Briejer, 1976; Wellman, 1988), although many have also asked what psychological or economic factors lead to social contacts. White’s book is ambitious in that it combines a microeconomic theory of tie formation with an analysis of entire markets.

³ Interestingly, in White’s Identity and Control (1992), some passages suggest that successful market actors do not always have clear goals and preferences.
of market situations than Spence thought possible. Specifically, Spence thought that educational labor market signals failed when the cross-derivative of the volume and quality of education with respect to cost was negative. White points out that this corresponds to the $d < 0$ condition and is thus in the PARADOX region of White’s market plane. Therefore, labor market signals act as expected if certain other parameters take on certain values. White does not redevelop Spence’s signaling theory in its entirety, but he says that Spence never fully considered the possibilities of his model because he did not consider how the job applicant is located in a larger chain of producer–consumer ties. Much of Markets from Networks is a translation of standard results in microeconomics into White’s network theoretic language. Most of his effort is spent setting up the mathematical machinery, with occasional the observation that extends the theory. This is certainly a contribution to economy theory, but not the sort that one might expect from the effort required to build the machinery for White’s hybrid Cobb–Douglas/network approach.

While discussing Spence’s signaling theory, White makes a number of important methodological critiques of economic theory. The main criticism, made throughout the book, is that economic models tend to look at social welfare at the expense of institutional context. Spence, for example, shows that efficient signaling increases social welfare. Nowhere does Spence look at how the labor market might be connected to other markets. In Spence’s model, the job signaling process exists in isolation from other production markets. It is only by looking at this broader context that White can see additional implications of Spence’s labor market signal theory.

While White’s discussion of labor market signals illustrates the book’s tendency to be an exercise in translation and modest theory extension, it does not do justice to White’s larger point that firms and entire markets should be viewed as positions, or nodes, within a vast array of interconnected markets and firms. An economist would be justified in asking if this theory improves our understanding of how markets operate. Aside from the occasional extension of existing theory, does White’s approach justify the effort needed to understand it?

One way to answer this question is to compare White’s theory with how economists have approached networks of buyer–seller relations. A few economists have tackled this problem under the rubric of ‘network industry economics’ (e.g., Economides, 1996; Shy, 2001). Like White, network industry economics notices that buyers and sellers are organized into chains. They ask basic questions about the sustainability of such networks. A few scholars pay attention to the specific structure of the network. They ask questions about the size of the network and if network externalities outweigh the costs of maintaining the network.

Other works pay more attention to a network’s intrinsic structure and are closer in spirit to White’s approach. For example, in their review of the network economics literature, Nicholas and Economides (1996) discuss the ‘hub’ model
of the telecommunications and railroad industries, in which a firm intercedes between a supplier and buyer (Economides and Woroch, 1992). They study the conditions under which it is possible to support a certain structure, to close a node in the network, or to merge. What is interesting is that Economides (24) notes that the answers are known only when demand functions are linear. A strength of White’s approach is that linear demand functions are a special case of the general theory; i.e., one can recreate Economides and Woroch’s example by setting $a = 1$, $b = 0$, $c = 1$, and $d = 0$. White’s general theory is able to encompass hub models found in network economics.

However, nowhere in the current literature on network economics is there a broader attempt to explore the implications of thinking of linked markets or unusual utility functions like White’s ‘PARADOX’ market. Although most economists would admit that markets are interconnected and that firms are not uniform and do not occupy identical positions in markets, standard economic models do not do what White wants to do, which is to show the implications of being connected to other production markets.

In chapter 11, White directly takes issue with economic theory. For example, he concedes that perfect competition models have their uses and real world markets can sometimes approximate perfect competition (222). In his view, the standard textbook treatment of competitive markets is inadequate, even as a foil, because it does not consider how a single market is shaped by its connections to other markets. Every firm is a supplier and buyer, and the ability of a firm to sustain an exchange depends not on its customers but on the constraints imposed by suppliers.

White’s deeper criticism of textbook models is that firms are treated as identical actors. White believes his contribution to be the inclusion of parameters distinguishing firm quality (the $n$ in his modified Cobb–Douglas framework). He correctly notes that it is highly unusual for any market to have actors offering identical products. There is truth to this point. Even firms stocking mass-produced items may end up offering different menus to customers because of differences in warehousing ability. Some firms may offer lower quality, degraded items, while others may specialize in carefully maintained work. Think of car dealers who specialize in new, unused cars from prior production cycles.

Of course, earlier economists dealt with the problem of heterogeneous firms in a single market, so White goes to great pains to distinguish his work. He reviews, for example, Nerlove’s (1965) work, which examines equilibrium price conditions with firms distinguished by managerial ability (230–233). White’s treatment can be at moments off-putting because of ad hominem attacks (he accuses Nerlove of ‘economist’s machismo’) and tortured passages.4 Despite

4 E.g., “The third implication ties in also with the proclivity for pure competition assumptions noted earlier and serves as introduction to the final section of the present chapter, and to generalizations in subsequent chapters, because it illustrates econometric work finding itself drawn into considerations of
this, White does have some valid points. For example, rather than analyze a system of interacting firms, Nerlove and his successors, such as Jacob Marshak, disaggregated the market into individual interactions. Mathematically, it is easier to understand groups of firms offering identical products in isolation from other markets. Equilibrium conditions were computed for each firm, bypassing the need to consider the firm’s connections to other markets. By doing so, economists failed to consider the external forces that internally differentiate markets, causing some firms to adopt the role of providers of low- and high-quality products.

White’s analysis raises the issue of market segmentation. How different is his analysis from traditional economic treatments of market segmentation and specialization? The distinction is that White makes specialization dependent on actors in other markets. The firm’s position within a supply chain determines if specialization is possible. In contrast, textbook analyses of market segmentation adopt essentially a Hotelling approach, or, more generally, a location model approach. Firms compete for customers who view firms as offering different brands. Customers are modeled as points in a geometrical space that is partitioned by firms. There is usually no sense that these markets are connected to any other markets.

In the end, what is one to make of White’s ambitious project, this grand synthesis of network analysis and neo-classical economics? The evidence is mixed. One can clearly see that White is trying to bring context to economic analysis. Rather than seeing every firm or market in isolation, he provides a simple theoretical model describing the emergence of hierarchies within and between markets. Markets with unusual cost curves can be easily accommodated into his framework. On the other hand, White’s claims to novelty might be exaggerated. There are prior treatments of networked suppliers and of market niches.

White’s most important contribution is a plausible formalization of the economy as a complex system of inter-related workers, firms, and markets, a goal that has eluded many thinkers. Most industrial organization theory, and mainstream economics more generally, presents relatively simple actors with complex preferences and actions; e.g., firms in an industry are governed by a single average cost curve. Or consider game theory applications in industrial organization research, in which actors have complex options but simple goals when dealing with others. However, there have always been economists who urged that the economy be considered a complex social order in which actions in one sector ripple to others, an insight crucial to contemporary evolutionary economics. For a review of such concepts from an Austrian perspective, see Gloria-Palermo (2002). Although White’s formulation may be obscure to network structure and how that intercalates with markets of one sort and another’ (253). Sadly, this is one of White’s less cryptic moments.
economically oriented readers, it does present an opportunity to systematically work out the implications of such a view, which may be the book’s lasting legacy.

**Conclusion: an ally for heterodox economics?**

Neo-classical economics is constantly criticized. Neo-classical theory’s emphasis on stable preferences and profit maximization invites other scholars to criticize it as an unrealistic, simplified version of the world. The various schools of heterodox economics each offer their own alternatives. Institutional economists, for example, would have us look at how historically contingent technologies affect economic development (Hodgson, 1988). Feminists look at how gender shapes an individual’s preferences (Barker and Kuiper, 2003). What does this renaissance of economic sociology offer as an alternative to the neo-classical framework?

As mentioned in the introduction to this essay, an important contribution is a theory of culture and social structure and their effects on firms. All three works reviewed in this essay depend crucially on a theory of culture or social structure. Fligstein’s work, and that of neo-institutional theorists more generally, is built on both Parsons and Bourdieu. Like Parsons, Fligstein assumes that actors pursue what they believe to be legitimate behavior. Like Bourdieu, Fligstein assumes that economic roles exist in a complex symbolic system. Carroll and Hannan advocate a simpler theory. The overall age and size of an industry acts as a signal of its maturity. Investors, customers, and others are more willing to assist the firm if there are many other firms. White’s theory is the least cultural, since roles are defined not by Bourdieu’s fields of action, but as positions in a supply chain.

Although each work can be criticized, collectively they show the value of seriously considering culture and social structure as important factors in economic theory. Contrary to the strongest rhetoric depicting neo-classical theory as an ‘autistic’ research tradition (Guerrien, 2002) obsessed with formalism, neo-classical theory’s virtue is that its simple human behavior model can be readily expanded to accommodate unusual preferences. Returning to an earlier example, Akerlof and Kranton (2002, 2005) have written a series of articles based on the assumption that individuals mimic the group’s ideal. Akerlof’s insight is that economics can benefit if it can translate sociological ideas into a neo-classical framework. In a similar vein, White translates network theory into neo-classical theory. Ecological theory is already very close in spirit to economics. In some cases, there are tangible results and novel hypotheses. White, for example, focuses on markets with increasing returns to scale. At other times, it leads to unexpected hypotheses, such as the liability of newness argument.

The important lesson is not that sociological theories offer an entirely new alternative, but that they provide a model for how economics might be expanded to encompass a richer notion of how individuals understand and process their world. Economic sociology’s emphasis on cognitive-social processes presents a
model of human action more sensitive to historical contingency and less dependent on self-interest. As economic sociology gains more visibility, heterodox economists will more likely appeal to economic sociology as a justification for their critique of the mainstream. Economic sociology’s biggest contribution might be its insistence that economics seriously consider sociocognitive processes, which can become a common language for heterodox research.

References


Sociological imperialism in three theories of the market


