these job characteristics are relatively scarce, much more likely to be found among those at the highest levels of organizations than those at the bottom. If Friedman and Greenhaus are correct, however, creating more pioneers of the sort Meiksins and Whalley portrayed requires fundamental changes in work organization, not simply a greater number of family-friendly policies.

Another important theme in *Work and Family—Allies or Enemies?* involves the authors' emphasis on workers' choices and priorities. This focus seems driven more by the authors' prescriptive orientation than by an overarching theoretical perspective. Nevertheless, Friedman and Greenhaus's insistence on attending to people's values, desires, and feelings about how they live and work is a useful corrective to research focusing only on work and family time. If alternatives are to be envisioned and explored, we need to know more about people's priorities and the forces that shape them. As Friedman and Greenhaus note, however, gender is an important subtext to this discussion. Not only do women have fewer choices than men when it comes to career and family, but their lives look different from men's in almost all respects. The divide between the genders, in the authors' view, "is a divide as great as the divide between career and family themselves" (p. 9). In the end, this is what most unifies the three books reviewed here. They all explore the possibilities for change and encourage alternative ways of working. To varying degrees, however, each also shows how these alternatives, their desirability, and people's opportunities to create them are gendered in fundamental ways. Thus, while these books are useful for calling attention to change and what it might look like, they also reveal the intractability of the gender order and its role in organizing work and family.

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Other Reviews

Economics Meets Sociology in Strategic Management. Joel A. C. Baum and Frank Dobbin, eds. Stamford, CT: JAI Press, 2000. 410 pp. $82.50.

Sometimes it seems as if the field of strategic management is like a country that has no foreign policy or standing army and yet is periodically overrun by the armies of bordering states, viz. the disciplines of economics and sociology. What is it that attracts their attention? How much is strategy really worth dominating? Is the field perceived to be truly provocative or just an annoyance to be subjugated? Whatever the answers to these questions, it seems clear that over the past two decades the incursions of economists and sociologists into strategy have improved it substantially as a research discipline. So further argumentative behavior on all sides should be welcomed.

In an attempt to generate as much of this behavior as possible, Baum and Dobbin have put together a compendium of articles and commentaries that show how economics and sociology try to invade, overrun, and generally attempt to sack the strategy field. The book shows just how vulnerable strategy is to attack and at the same time how fundamental its problems are for students of organizations and markets. Sometimes there is fire in the debate among the scholars Baum and Dobbin have found to participate; sometimes there is just smoke. Fortunately, everyone has an interesting and usually pointed thing to say.

The book is organized around four problems in strategy: What makes firms different from each other, if they are? What makes some firms more successful economically, if anything? Why do firms grow through new business diversification? And Why do industries differ in their structures? These have been important questions for economists and sociologists interested in organizations. So the participants in the book are knowledgeable and have more than a casual interest in how these questions should be answered. It should be said, however, that these questions certainly do not constitute the full set of issues in strategy that bear on economics and sociology. One wonders why there were no sections in the book on entry and exit, for example, or on growth models, on inertia and change, on vertical integration, and so on. Economists and sociologists have different perspectives on these topics, and these differences would have been useful to explore. Yet questioning the editors' choices is futile when so much of the work in the book must have been just getting the contributors to join in, repeatedly. In fact, the really interesting thing about the book is the spirit of ongoing, amiable contentiousness. This spirit produces remarkable statements by very well-known researchers who want to say what's on their minds about the wrong-headed views of the other side or simply the bothersome presence of the strategy field in general. The editors of the volume deserve a great deal of praise for generating these debates. Even though the issues highlighted are not brand new, their importance is emphasized by having them stated in the context of others' comments, contrary or supporting.

The book is not a debate between teams with equal numbers. The lineup on the sociology side is much larger. And although the editors have chosen economists who do research in and teach strategy, such as David Teece, Rebecca Henderson, and Sharon Oster, they have chosen sociologists, such as Arthur Stinchcombe and Woody Powell, who do not have this background. One wonders what kind of conversation would emerge if the numbers on each side had been more equal and the debaters' distances from the field more comparable. One scenario is that the debate would have centered more on the empirics relevant to the questions that guide the book. The best empirical research in strategy has been guided by a willingness to examine important questions in ways that are not conventional in economics or sociology. For example, variance decomposition studies are relatively rare in these disciplines but common fare in strategy. These studies focus on identifying which categories—organization, industry, time period, corporate parent, early or late entrant—have the greatest impact on, say, accounting performance or innovation. Most of this research shows a strong effect for organizations, but not for industries, time period, and so on. An awareness of these results, and other results with similar theoretical implications, would have restructured much of the discussion in several sections of the book.

Baum and Dobbin offer two interesting sections on the problem of firm heterogeneity. The first is titled, with a nod to Tolstoy, "All Happy Firms Are the Same." The question is whether firms follow their own drummer (Bettis and Prahalad's dominant logic) or one drummer beating for the industry (DiMaggio and Powell's institutional isomorphism). Bettis, Powell, Scott, and Hill eventually all agree that both drums exist and can be heard more or less by all firms. Charles Hill is the spokesperson for economics in this group, but he is more an advocate for firm heterogeneity than for the single industry production function of microeconomics. At the end of this section, we are left with two conclusions. First, institutional forces influence the practices of firms; otherwise there would be no need for accountants, lawyers, compliance officers, and perhaps that fortunate consultant or board member who carries the innovation of the moment from firm to firm. Second, economic forces also affect firm practices, or so investors, lenders, managers, and workers say. But are all firms the same because of these forces? No, they are not. Does the concept of a dominant logic explain these differences? Apparently not, since it applies to diversified organizations. So this section is somewhat anticlimactic.

The second section is titled, with a nod to Lake Wobegon, "Where All the Firms are Above Average." This section deals directly with the sources of heterogeneity. On the one hand, there are the provenance-free, firm-specific resources that Jay Barney's well-known article has put into the strategy lexicon; on the other hand, there is Stinchcombe's classic argument that the attributes of a firm are a function of its founding era. The debate among these authors is particularly informative because their views of the strategy field and its academic intent vary widely. Barney basically sees it as the end point of all who recognize that firms are different. Stinchcombe sees it as overreaching empirically and pandering to managerial and investor interests, "a trade association of racetrack touts" (p. 281). The problem is in establishing a means of predicting today which firms will be successful in the future. Weighing in on this topic, Rebecca Henderson, the lone economist here and in the role of commentator, tells us that since successive flips of a coin will produce a winner with a string of heads, how can success today indicate anything about a skillful strategy in the past? Barney replies that luck, or, more precisely, draws from some random distribution, is the null hypothesis, and Stinchcombe continues to see strategy as managerial propaganda laid over more fundamental skills in the labor force, and so on. None of this is categorically wrong, as Christine Oliver points out in her commentary. But neither is it categorically right. Although it is analytically convenient to specify a single distribution from which firms draw their decisions, my guess is that empirically there are a number of distributions that underlie managerial choice in an industry, some of which may be firm-specific. Identifying them requires a kind of Bayesian estimation not yet found in strategy research but prevalent elsewhere in management science. So this section of the book is useful in that it shows one more time that strategy has the potential to bring together theory, method, and data to answer fundamental questions that neither economists nor sociologists, with a few exceptions, would be happy spending a lot of time on.

A third section, with which in fact the book begins, addresses the question of why firms diversify. Teece's classic article on economies of scope holds up the economist's flag, while Fligstein's well-known paper on the diffusion of multidivisional firms raises the banner of sociology. As the commentators, John Freeman and Bruce Kogut, observe, these papers are not really on the same topic. And they are subjected to considerable criticism. Freeman attacks Teece for his assumption that managers make diversification decisions using a strict economic calculus, and Kogut attacks him for being oblivious to institutional forces at the country level that affect diversification patterns. Freeman assaults Fligstein for his methodological weakness, while Kogut criticizes his failure to appreciate economic motivations, even as following up on these motivations depends on country-specific structures of opportunity. This section of the book enlivens its topic to an unusual extent, which is not an easy thing to do given the very large amount of research on diversification. Yet it is unlikely that the numerous financial economists who are currently studying diversification and multi-business firms will take note, since the debate here is too skewed away from their interests. Thus, there remains an opportunity to bring together the various strands of research on this important subject.

The last section of the book deals with industry structure. This section pits Harrison White's role-theory-based model of how firms shape an industry space against Sharon Oster's empirical study of strategic groups in 19 consumer-goods industries. White objects strenuously to Oster's lack of viable assumptions about firm behavior, and Oster is accommodating. Wayne Baker shows in his commentary that White has been cited more and that Oster's research has little recent influence in economics. Will Mitchell reviews White and Oster's comments about each other and then extends the discussion to why firms change. Perhaps the most interesting part of this section is White's critique of economics in general, called its "triple failure." The triplet consists of (1) the construal of managerial cognition as individual, not social, (2) the belief in friction-free entry into and exit from markets, and (3) the modeling of firms in isolation and not as networks of firms in interaction. White also points out that micro-economics has developed to serve the interests of lawyers and regulators, not managers, who ignore it and the rest of social science as well. Here is a very well-known sociologist placing his blows well. One only wishes that the opposing economist had been more willing to counterpunch—where are the game theoretic parallels to White's model?

So where do these not-too-distressing debates leave the strategy field? This book may improve strategy research a little, if only because the issues have been deepened. There is no damage to strategy's integrity, since it is already loosely identified, or to the focus of the field, since its problems are recognized as important. Moreover, strategy benefits from the attention. One hopes the editors had fun with their participating scholars, since several more compendiums could be put together just like this one. But economics will have to field a stronger presence. Whatever the topics might be, the more carnage the better.

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Multilevel Theory, Research, and Methods in Organizations: Foundations, Extensions, and New Directions.
Katherine Klein and Steve W. Kozlowski, eds. San Francisco: Jossey-Bass, 2000. 605 pp. $50.00.

The dust jacket of this book states the issue, a long-standing one, precisely:

> Although quick to acknowledge organizations as multilevel systems, organizational science has traditionally developed and tested theoretical models from three distinct points of view-organizational, group, and individual. Each level has become the province of different disciplines, theories, and approaches that have evolved over time. The current challenge is to integrate processes occurring across and within all levels of an organization that affect the behavior of individuals, groups, and organizations as a whole.

There are at least two reasons why this hasn't happened. The first is that there are different epistemological assumptions that influence the various disciplines that constitute organizational studies (Cappelli and Scherer, 1991). I take as a given the general model B = f(PxE), that behavior is a function of the person and the environment, is a useful way to understand behavior in organizations. If this is so, then it is fair to say that different disciplines emphasize P or E, which guided the assumptions of their field. These assumptions reflect how each thinks about and studies organizational matters. For example, the main theoretical models in organizational sociology, population ecology, and institutional theory focus more on the environment part of the model (a big E, while the person element is a small p). These theories tend to be explanations of processes that help explain why organizations take on the characteristics they do—or what happens when they don't develop forms that can adapt effectively to the environment. They don't tell us much about what people do to make this happen (individual performance), why they do it (motivation), or what they are like (personality) as they are involved in these processes. Similarly, organizational behavior/industrial (OB/IO) psychologists tend to focus on the person, big P, and less so on the environment (little e). For