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What Do Unions Do? Comment

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Abstract
[Excerpt] In *What Do Unions Do?*, F & M gather together an impressive amount of evidence showing that unions are on net beneficial for society. This book will not end the debate over whether unions are good or bad for society, but it represents a milestone that will surely influence the course of the debate in the future.

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tive productivity effects, apart from what I believe are modest contributions from reduced turnover and grievance procedures. Rather, it is the expectation of lower profits resulting from higher union wages and benefits that makes it necessary for firms in relatively competitive environments to increase monitoring, improve managerial structures, and the like (in other words, the traditional "shock" effect of unions).

Whether one believes unionism in those settings is beneficial to the economy depends crucially on one's assumptions. To the extent that one believes that slack (or "X inefficiency") and long-run rents accruing to capital are widely prevalent in the U.S. economy, the union effect may be largely benign. I suspect, however, that the significant union effect on profitability, even if restricted to firms with some market power, is likely to decrease long-run investment in long-lived capital and research and development, and to decrease long-run productivity growth. (Hirsch and Connolly [1984] find, for example, that unionism lowers the market valuation of R and D investments and decreases firms' R and D intensity.) Welfare losses associated with such effects are likely to be larger than the modest static efficiency losses resulting from union wage increases. Questions concerning the long-run dynamic effects of union rent-seeking clearly warrant continued study.

Even with these reservations, I must conclude that the F & M research program has significantly enhanced our knowledge and understanding of unionism. Although final evaluation of this literature must await further study, I suspect that most of the findings and conclusions in *What Do Unions Do?* will stand the test of time.

*Comment by David B. Lipsky**

In *What Do Unions Do?*, F & M gather together an impressive amount of evidence showing that unions are on net beneficial for society. This book will not end the debate over whether unions are good or bad for society, but it represents a milestone that will surely influence the course of the debate in the future.

As almost all readers of the *Review* must now know, F & M believe that unions have two "faces." At one and the same time, the authors maintain, unions exercise monopoly power and serve as a mechanism that provides workers with "collective voice." On the basis of their findings, the authors judge the deleterious consequences of union monopoly power to be outweighed by the beneficial effects of collective voice, thereby tipping the social balance sheet in favor of unions. For many neoclassical economists, this assessment has been a hard nut to swallow. But for many of us in the industrial relations tradition, F & M's findings have complemented our own research. Modern industrial relations scholars, heirs of the Webbs, Commons, and Perlman tradition, have not been uncritical of unions but have concluded that, at their best, unions make a positive contribution to the general welfare. Orthodox economists, on the other hand, have not often given industrial relations scholars a respectful hearing. By speaking to their fellow economists in their own language, Freeman and Medoff have provided those of us in industrial relations with the effective "voice" we have all too often lacked with the economics profession.

F & M's methodology is a familiar one to social scientists. In most of the chapters of this book, the authors consider the effect of unionism on some "outcome," such as the wages of union members, the wages of nonunion workers, fringe benefits, wage differentials, quits and layoffs, productivity, and profits. Each outcome measure is used as the dependent variable in a regression model that includes some measure of unionism. One's faith in the validity of the authors' conclusions is, in most cases, buttressed by the thoroughness of their tests, the breadth of their choice of samples and model specifications, and their willingness to acknowledge anomalous findings. For many of the outcome measures, particularly wages, fringes, and turnover, the findings are very robust and can hardly be doubted. In other cases, such as productivity and profits, their findings are much more tenuous and require further testing.

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There is no gainsaying the power of F & M's methodology, but most social scientists also recognize its limitations. I need not spell out all of them here, but I would like to make note of a few I find particularly troublesome. Essentially, F & M arrive at their conclusions by comparing union workers with nonunion workers, or unionized establishments with nonunion establishments. Unionism in their scheme of analysis remains an abstraction: one union is like every other union, one collective bargaining relationship like all the rest. Even industrial relations scholars (including this reviewer) have frequently relied on the same approach, and fellow sinners should be reluctant to cast stones. But it would certainly advance our understanding of what unions do if we moved away from undifferentiated measures of unionism and began to incorporate, into our statistical analysis, measures that capture the various forms in which unionism and collective bargaining appear. The authors devote a scant three pages to the structure of collective bargaining, for example, but it is likely that bargaining structures have an independent influence on outcomes. Only a handful of data-based studies have examined that influence.

Indeed, F & M's approach fails to account for the influence on outcomes of a host of factors that industrial relations scholars believe to be important, such as the history of the parties and their relationships; the customs and traditions of the work site; the personalities, attitudes, and leadership skills of the actors; the negotiating strategies and tactics used by the parties; the degree of inter- and intra-organizational conflict; and the availability of various dispute resolution procedures. Because industrial relations scholars have recognized that these factors have an independent effect on bargaining outcomes, they have increasingly moved away from models that assume outcomes are a function simply of exogenous economic and demographic variables and toward more complex models often based on a systems paradigm. Although systems models have their own problems, it is worth pondering whether we can really understand what unions do if we ignore organizational and behavioral factors that are an important source of variation in outcomes.

Another limitation is the highly static nature of F & M's analysis, which relies heavily on cross-sectional testing of data sets assembled in the 1960s and 1970s. The authors' findings may tell us less about what unions currently do than about what unions did during an era that is now history. F & M use the most suitable data sets available to them, but the issue is whether a snapshot of union effects in the 1960s and 1970s remains an accurate picture of the consequences of unionism in the 1980s. Many industrial relations scholars believe that collective bargaining has recently been moving through a period of historic transformation in the United States.[8] By contrast, F & M dismiss the idea that we have entered a new era in industrial relations, arguing that the recent wave of concessionary agreements has merely served to return union wage premiums to more normal levels.

I believe that the signs of major change are too abundant to be ignored. One sign is the precipitous drop of union membership in many sectors of the economy. F & M do examine the "slow strangulation of private-sector unions," noting that the continuation of current trends portends a "disastrous decline" in the level of unionization to a bare 10 percent of the nonagricultural labor force before the end of the century. The authors attribute the drastic contraction of the labor movement primarily to legal and illegal management opposition, which they say has increased by "leaps and bounds," and to the ineffectiveness of government policies designed to regulate such conduct. They minimize the responsibility of the unions for their own misfortunes, perhaps because union organizing efforts are so difficult to quantify.

They largely ignore the effects of heightened international competition, technological change, government deregulation, and a conservative political climate on union organizing and labor

[8]For two recent articles that make this case, see Strauss (1984) and Kochan, McKersie, and Cappelli (1984).

relations. They also seem impervious to the fact that collective bargaining has become substantially more decentralized in the wake of the collapse of industrywide agreements, the abandonment of pattern bargaining, the movement to two-tiered wage contracts, and other structural changes. If, in fact, we have moved into a new era, the kind of static analysis used by F & M provides us with only a limited understanding of the dynamic forces now reshaping American labor relations. The larger question is whether an enfeebled union movement can have the same influence in the future as it had in the past.

The collective voice–institutional response model represents a useful framework for understanding certain aspects of unionism, but it does not constitute a full-blown theory of union behavior and effects. It rests heavily on the assumption that nonunion employers respond to the needs of the marginal worker—younger, more mobile employees—while union employers are forced to consider the needs of the inframarginal, median worker—more senior, less mobile employees. "In a unionized setting . . . the union takes account of *all* workers in determining its demands at the bargaining table, so that the desires of workers who are highly unlikely to leave the enterprise are also represented," according to the authors. But they merely assert rather than demonstrate this view. Its validity depends on whether the median-voter model represents an accurate depiction of the internal political process of unions. F & M ignore ample evidence in the industrial relations literature that some unions—many fear too large a number—fail to measure up to the democratic model and are instead dominated by an oligarchic leadership or an entrenched bureaucracy.

F & M do examine some indicators of union democracy and conclude that there "is a great deal of democracy . . . throughout the labor movement, particularly at the local union level." But their case is not particularly persuasive, resting on imperfect indicia such as union constitutional provision, membership attitudes, the turnover of union leaders, charges of improper conduct brought under the Landrum-Griffin Act, and a limited number of case studies conducted by other scholars. The authors simply do not confront this important issue with the same care and diligence they use in their analysis of unions' economic effects. But if unions are not the democracies F & M believe them to be, their contention that collective voice "fundamentally alters the operation of a labor market and, hence, the nature of the labor contract," producing more socially optimal outcomes, fails to have credibility.

The authors are surprisingly uncritical of the view that unions do have a monopoly face. To them, whether unions are primarily monopolistic or primarily voice institutions is entirely an empirical question. In my judgment, however, the utility of considering unions as monopolies is more than an empirical question: it is a critical conceptual and theoretical issue. At best, the monopoly model of unions is a useful metaphor; at worst, it is an utter distortion of the nature of unionism.

Clearly, unions are not *literally* monopolies: to cite only a few of the well-known flaws of the monopoly model, unions cannot be monopolies because they do not actually sell the services of their members; they are not profit maximizers (nor is it evident that they engage in any form of maximizing behavior); they lack meaningful cost functions; they do not (in the absence of the closed shop) control the supply of labor; and as F & M themselves emphasize, they adhere to the precept of the standard wage rather than engaging in price (wage) discrimination, as true monopolies do.[9] By uncritically accepting the theoretical possibility that unions' monopoly effects may, under some circumstances, outweigh their voice effects, F & M actually grant that the weight of empirical evidence may yet prove them wrong. This stance should give pause to union advocates who have greeted their research with unqualified praise.

On the other hand, the authors are so intent on making the best possible case for the social utility of unionism that even the most ardent union supporters ought to

[9]For a criticism of the monopoly view of unionism, see Mishel (N.d.).

blanch a bit at their efforts. In examining unions' economic effects, they rest their case on a thorough analysis of masses of data; but in dealing with such issues as union democracy, corruption, and political influence, the authors too often rely on incomplete or imperfect evidence. They conclude, for example, that "the amount of union corruption is no more than, and probably less than, business corruption," basing this conclusion largely on Department of Labor reports on criminal convictions under the Landrum-Griffin Act and the Hobbes Act, clearly imperfect indices of the extent of union crime. F & M are probably right about union corruption, but one wishes they had based their argument on sturdier evidence.

I also wish that such an important book contained more graceful prose and that the editors had corrected the numerous typographical errors that assault the reader's eye. (To note only two examples, Daniel Mitchell's important book is consistently called *Union Wages and Inflation*, rather than *Unions, Wages, and Inflation*, and this journal is sometimes cited as the *Industrial Labor Relations Review*.) Nevertheless, *What do Unions Do?* should be required reading for all students of labor economics and industrial relations. By providing readers with the most comprehensive survey to date of empirical evidence on unions' economic effects, it serves as an effective antidote to the view that unions have only harmful consequences. At the same time, its dependence on static economic models and methods reminds us that we need more comprehensive, integrated theories if we are ever to understand what unions actually do.