

REVIEW SYMPOSIUM

WHAT DO UNIONS DO? by RICHARD B. FREEMAN and JAMES L. MEDOFF

EDITOR'S INTRODUCTION by JOHN F. BURTON, JR.*

FOR several reasons, *What Do Unions Do?*¹ by Richard B. Freeman and James L. Medoff is the most significant book on the topic in recent years. The theses are provocative, including a number favorable to unions. The authors are prominent, as members of the Harvard Economics Department. The arguments have been previewed elsewhere, such as in the 1979 article in *The Public Interest* on "The Two Faces of Unionism." And the current presentation is accessible to a general audience. The result is a book that has been praised by scholars such as John Kenneth Galbraith and John T. Dunlop and reviewed in popular media such as *The New York Times* and *Business Week*. These factors explain our special treatment of the book: a brief summary followed by a series of comments by prominent scholars.

The central message of Freeman and Medoff (F&M) is that there are two faces to unions—one with largely undesirable consequences for society and one with largely beneficial results—and the empirical evidence demonstrates that the bene-

ficial face predominates. The undesirable side of unions is the monopoly face, which enables unions to raise wages above the competitive level. The desirable side is the collective voice-institutional response face, which enables unions to channel worker discontent into improved workplace conditions. The empirical evidence consists of new sets of data on workers, firms, and other actors in the industrial relations system that the authors, their students and colleagues, and other scholars have analyzed during the last decade.

One example of the empirical analysis concerns the union impact on relative wages. F & M conclude that union workers earned about 30 percent more than comparable nonunion workers in 1980, the largest such advantage since the depression and one that could not be sustained in the 1980s, as witness the negotiated give-backs in recent years. The monopoly face of unions stresses the loss of economic efficiency resulting from such union wage advantages, but F & M find the loss in 1980 was only 0.2 percent of GNP. Moreover, F & M also note the salubrious consequences of the union relative wage impact, such as substantially reducing the inequality of wages among the overall workforce by about 3 percent.

The main thrust of the F & M analysis is

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to move beyond the union influence on wages into such topics as the union impact on fringe benefits, turnover, and productivity. Here their analysis draws on the theory underlying the collective voice-institutional response face of unions. They argue that workers have two basic mechanisms to deal with job dissatisfaction: by quitting the firm (exit) or by working through a union to change the job conditions (voice). The two methods lead to different consequences in the labor market. Thus, in determining the mix of wages and fringe benefits in the compensation package, the exit solution will favor the younger, more mobile workers while the voice solution will favor the older workers more interested in fringes. The data support this difference, since unionized firms typically devote a larger fraction of compensation to fringe benefits.

F & M also use the exit-voice dichotomy to help analyze the union impact on productivity. The collective voice approach stresses that unions can induce employers to provide more rational management and that both the higher wages and improved personnel policies resulting from union pressure can reduce turnover, which will translate into higher productivity. F & M marshal evidence to demonstrate that collective bargaining does lead to lower quit rates and thus higher productivity. The other productivity-enhancing features of unions are also analyzed in a series of empirical studies, which indicate that generally (but not invariably) productivity is higher in unionized than in nonunionized firms.

Although the influence of unions on wage inequality and productivity may be beneficial to society, there is a rub for employ-

ers, namely, unionized firms earn lower profits than other comparable firms. This is one reason employers have become increasingly aggressive in dealing with unions during recent decades. F & M assert that as much as one-half of the decline in union election success in organizing campaigns can be traced to unfair labor practices by employers. The result has been a slow strangulation of private-sector unions: membership as a percent of private non-agricultural workers dropped from 34 percent in 1956 to 24 percent in 1980. Moreover, the union share is projected to fall to 10 percent of the labor force unless unions improve their organizing record. For F & M, the declining share of organized workers is unfortunate because of the generally beneficial effects of unions on efficiency, income distribution, and social organization (including the promotion of desirable general social legislation). To reverse the decline in union strength, F & M suggest that unions need to increase their organizing efforts and that employers and unions must improve their bargaining solutions—including limiting the union wage premium, which reached extremely high levels by 1980. The primary solution to reversing the union fortunes, however, is a substantial revision in current labor law to limit the power of management to oppose unionization.

This summary of a 16 chapter book necessarily slights some of its themes, but should provide a sufficient backdrop for the comments of the authors in our symposium. We express our appreciation to these critics, to Professors Freeman and Medoff for their book and their response, and to Professor Larry Mishel of the ILR School for suggesting this symposium.

COMMENTS by REVIEWERS

*Comment by Orley Ashenfelter**

The response of the popular press to *What Do Unions Do?* has been only just short

of breathtaking. True, the reviews in the *Los Angeles Times* and *Time* were superficial, and the reviews in *The New York Times* (there were two!) were enthusiastic but took the authors to task for failure to discuss the unmeasurable (such as "the degree to which union opposition to government wage guidelines makes it difficult to stabilize

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prices"). The big surprises, however, were the careful and generally favorable reviews in those bastions of capitalism, *Fortune* and *Business Week*. To be sure, the latter review contrasted the F & M book with a strong "theoretical" antidote book that argues the proposition that unions should not exist in a free economy. *Business Week* recommended reading that theoretical work, apparently to stiffen the backbones of any managers who might be swayed by the F & M evidence, and then proceeded to observe that the "theory" did not predict anything and that, unlike F & M, its author did not provide any evidence either.

No doubt one reason for the popularity of the F & M book is its accessibility. Although many industrial relations specialists will no doubt think of it as a quantitative and updated version of Bok and Dunlop's *Labor and the American Community*, I think the authors are more deeply indebted to Lewis's *Unionism and Relative Wages in the United States*. The difference is all too apparent, however. Lewis's book was read by at most a handful of other scholars, whereas F & M's book made *Time* magazine.

In my view, we are all indebted to F & M for putting this provocative and intrinsically interesting literature on the quantitative impact of unions into a form that is both comprehensive and accessible to our students. As a consequence, we may see more quantitative research on trade unions in areas still in need of it.

We are also indebted to the authors for opening up the analysis of several new topics. In some cases, as in assessing the union effect on fringe benefits, they accomplish this mainly by applying the energy, for which they are famous, to data unavailable when Lewis first studied the subject. Their studies of the union impact on quits, productivity, and profits are, on the other hand, pioneering.

What, then, are we to make of these studies? First, there are studies of the differences between the wage rates of union and nonunion workers with the same measurable characteristics. A now considerable uniformity of empirical estimates indicates a union wage advantage of from 10 to 30 percent that varies over time and across age, race, gender, and occupation groups.

Generally speaking, union wage rates tend to be smoothed relative to any of these characteristics, so that skill differentials, for example, are smaller in the union than in the nonunion sector.

So far, I think there would be little disagreement among empirical scholars studying this phenomenon. But what interpretation are we to give it? F & M wish to draw two further inferences. First, they argue that these union-nonunion wage differences are monopoly wage gains and amount to an arbitrary, but perhaps normatively desirable, redistribution of income by nonmarket methods. Second, a monopoly wage gain that is unaccompanied by an explicitly efficient and enforceable employment bargain implies economic inefficiency in the allocation of labor. The inefficiency arises because employers adjust to the monopoly wage advantage by hiring too few workers in the union sector and too many in the nonunion sector to maximize the gross national product. The authors estimate this "social cost of monopoly wage gains" at around \$5 to \$10 billion in 1980. *Fortune* magazine remarked approvingly that F & M "gain much credibility by instantly conceding certain major points in the anti-union case."

The key part of this "social-cost" calculation rests on the assumption that the union wage effect causes labor to be reallocated from the union to the nonunion sector, an assumption that is by no means logically necessary in conventional bargaining models. Indeed, if unions and employers struck efficient bargains, as is typically envisioned in a bilateral monopoly, workers would achieve their wage gains without producing social costs.² It is most important to recognize, however, that this "social cost" proposition may be subjected to empirical testing. If there is a social cost to monopoly wage gains, it must be the case that increases or decreases in union wage gains are associated with decreases or increases in the relative employment of union workers. Unfortunately, F & M swallow the social-cost argument hook, line, and sinker, with-

²This point is elaborated in Ashenfelter and Brown (1983). In such models, unions only redistribute income.

out any of the empirical testing necessary for its acceptance.³ We can only hope that they will turn their talents to the necessary empirical work in future efforts.

An important research question that has not been so well documented empirically is the impact of unions on productivity. As with the study of the union impact on wages, there are many obstacles to inference about this controversial topic. The main difference between the two subjects is that far fewer data have been brought to bear on the study of productivity. It seems reasonable, therefore, to wonder whether a consensus of the studies a decade from now will be consistent with F & M's contention that unions raise productivity.

A glance at Table 11-1 in *What Do Unions Do?* (p. 165) makes it clear that only one study drives the F & M generalization regarding productivity. Studies of specific industries show both positive and negative effects of unions on the productivity of firms. The only broadly based study is Brown and Medoff's 1978 paper that analyzes data on manufacturing industries aggregated by state. In reading that paper, I was struck by the fact that when the authors do not assume that capital-labor ratios are the same in union and nonunion plants (within state-industry groups), they estimate the productivity effect of unions at zero. In other words, the point estimate of the union productivity effect in the most general model fitted to the data is negligible.

Brown and Medoff, and now Freeman and Medoff, reject this result because the implied productivity effect of unions is imprecisely estimated in the general model and does not lead to statistically significant results. This is not a tenable methodological position, as F & M indicate in their critique (p. 237) of the Getman, Goldberg, and Herman study of deceptive and illegal management campaign tactics in union elections. To paraphrase them, a finding that the results are not statistically significant means only that the "true effect cannot be estimated with great precision." Until

better evidence is available, it may be more reasonable to conclude that unions have little or no effect on productivity.

Finally, the most important new results in the F & M book are also among the most convincing. These are the apparently uncontroverted studies indicating that quit rates are lower among union workers than among nonunion workers comparable in skills and wage rates. This is the key finding to support the value of F & M's voice-institutional response model of unionism. If the advent of unionism meant solely an increase in wages, then quit rates would be unrelated to union status once wage rates are controlled. Establishing the relationship between unionism and quit rates provides F & M the ingenious key to measuring the value ("social benefit") of the voice role unions apparently play.

Perhaps one of the reasons for the popular success of this book is the public belief that unions do, after all, do some good; that they represent a misunderstood attempt to reach partially efficient agreements in a complex environment; that they do this primarily at the expense of the profits of firms in highly monopolized industries; and that the pendulum of public policy has swung too far in the anti-union direction. If so, the public will have read the message of this book correctly. For academics, bringing so much empirical work to bear on an important topic ought to be reward enough.

*Comment by Barry T. Hirsch**

What Do Unions Do? summarizes nearly a decade of research by Richard Freeman, James Medoff, and their students. The book is well written and easily accessible to students and an interested general audience. It unquestionably will receive wide attention and influence public and scholarly opinion. Although the authors' dispassionate statistical analysis of unionism contrasts markedly with the surprisingly emotional reaction of many to labor unions, open-minded readers cannot help but be

³The most direct evidence on this issue does not provide much support for a strong employment effect of union wage gains, but very little evidence on this issue is available. See Pencavel and Hartsog (1984).

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influenced by the book. The appearance of this volume also provides the profession with a convenient summary of important literature, previously available only through a large number of published articles and working papers. The book is not a perfect substitute for the original research papers, but it does manage to synthesize seemingly disparate topics into a coherent whole.

The publication of *What Do Unions Do?* makes this a particularly appropriate time to evaluate the contribution of the F & M research program. Much of the book provides statistical evidence that is now familiar and widely accepted by labor economists. Rather than commenting on each of the book's many topics, I will summarize what I believe are the major contributions and shortcomings of the broader body of literature. Although this review will accentuate what I see as unresolved problems in F & M's research, I should state at the outset that I greatly admire their work.

At least three major research contributions have emanated from the F & M research program. First is their development of a reasonable a priori case for economically efficient unionism. Where the workplace is characterized by public goods (for example, such shared working conditions as job safety and work pace) and the reluctance of many workers to reveal their true preferences, the labor market's reliance on the preferences of marginal workers and exit behavior can produce an inefficient outcome. By contrast, collective voice and the representation of median preferences under unionism *may* produce a more efficient outcome. Although I have little problem with these basic arguments of the authors, I wish they had provided a more detailed conceptual development of the conditions under which collective voice does improve efficiency within the firm; some discussion of the possibility for effective voice and efficient managerial structures in a nonunion environment (with and without the threat of unionism); and an explicit analysis of the median-voter and principal-agent models as they apply to trade unions. (For an attempt along these lines, see Faith and Reid, 1983.)

A second contribution of the F & M research program has been to apply stan-

dard microeconomic theory and econometric techniques to topics that have been almost exclusively the bailiwick of an industrial relations literature not known for its methodological rigor. Their research has brought about the beginnings of a synthesis of economics and industrial relations and, equally important, has stimulated a rapidly growing body of complementary research in the two fields. The third and possibly most important contribution of their research is the rather massive statistical evidence the authors have brought to bear on the various dimensions of labor union activity. Simply put, our knowledge of unions and the labor market is far greater as a result of their efforts than it was a decade ago. Moreover, their careful presentation and analysis of data has now become the norm for research on this subject.

Central to F & M's analysis of unionism is their evidence on productivity and economic performance. They recognize that "what unions do to productivity is probably the most controversial and least widely accepted result in this book" (p. 180), and they then proceed to provide a nondogmatic and carefully hedged discussion of that finding. Despite their careful discussion, however, I find their evidence, interpretation, and conclusions regarding union effects on economic performance to remain the most questionable part of their research. It is this topic on which I have chosen to focus.⁴

F & M conclude that "productivity is generally higher in unionized establishments than in otherwise comparable establishments that are nonunion, but that the relationship is far from immutable and has notable exceptions" (p. 180). I doubt whether even their economy-wide generalization holds. They summarize industry-by-state evidence for manufacturing, showing 10 to 31 percent union productivity effects in 1972 and 1977. But such large productivity effects are inconsistent with evidence on lower profits under unionism, despite their argument to the contrary

⁴Addison and Hirsch (1984) provide a fuller discussion of several of the points raised below.

(p. 184). For example, Brown and Medoff estimate a productivity effect of over 20 percent along with a union wage effect of 24 percent. The Census of Manufactures measure of labor's share of value added is .53 (Brown and Medoff, 1978: 367); thus, per-unit costs would increase by about 13 percent (.53 times 24) with no productivity effect. Combining the estimated productivity and factor-cost effects, per-unit costs clearly should decrease, while profits seemingly would increase (although, as Clark [1985] shows, one cannot directly infer changes in profit rates from changes in unit costs). Yet the existing studies reviewed by F & M show that unions unambiguously decrease profit rates by significant amounts.

Further doubt arises when one considers Clark's (1985) finding of a small negative union productivity effect for a sample of large product-line businesses in manufacturing and Warren's (1984) estimate of a large negative union coefficient in an economy-wide, annual time-series estimation of the Brown and Medoff equation. Based on this evidence and that on union wage and profit effects, I would conclude that overall union productivity effects are at best only moderately positive.

Likewise, one is not encouraged by the numerous studies finding residual total factor productivity growth to be significantly lower over time the higher the level of unionism and by one study suggesting that productivity growth is negatively related to changes in, and the level of, unionism.⁵ The finding of slower productivity growth in industries with high or growing union coverage casts some doubt on the robustness of the production-function studies, although unionism can logically increase factor productivity while decreasing productivity growth (Hirsch and Link, 1984). Moreover, slower growth in unionized settings may result, in part, from industry life-cycle effects or, over the long

run, from firms' responses to decreased profit expectations.

It is not surprising that estimates of union productivity effects vary enormously by industry. The number of relevant studies is still quite limited, but two patterns are discernible. First, productivity effects are largest in those industries where union wage effects are largest. For example, large union wage and productivity effects are found in construction (Allen, 1983 and 1984; Mandelstamm, 1965); in cement to moderate union effects are found in cement (Clark, 1980a and 1980b); while in industries showing no evidence of union wage effects zero or negative productivity effects are found (Ehrenberg, Sherman, and Schwarz, 1983, for public libraries; and Pencavel, 1977, for British coalfields.)⁶

A second and related pattern is that union productivity effects appear to be largest where competitive pressures are most intense. For example, (Clark 1980b) finds the largest productivity effect in the Southwest, where nonunion competition is most extensive, while Mandelstamm (1965) identifies competition from contractors in nearby Detroit as a chief source of the greater efficiency in highly unionized Ann Arbor than in less unionized Bay City, Michigan.⁷ Further evidence of this second pattern is the finding of no union productivity effects in the relatively noncompetitive hospital industry (Sloan and Adamache, 1984) or among regulators in the public sector (Noam, 1983), even in the face of positive union wage effects in both cases.

I have dealt at some length with the evidence on productivity because I believe it crucially affects one's evaluation of F & M's voice-response paradigm. I agree wholeheartedly with the authors that "unionism per se is neither a plus nor a minus to productivity" (p. 179). Nonetheless, my interpretation of the evidence is that it is not so much union voice that makes possible posi-

⁵Following Terleckyj (1984) numerous studies have found productivity growth to be negatively related to the level of unionism (Bozeman and Link, 1983, provide a complete list of references). Hirsch and Link (1984) relate U.S. productivity growth to the level of and changes in unionism, while Maki (1983) provides similar evidence for Canada.

⁶It would be interesting to know if the change in U.S. coal from large negative union productivity effects has been accompanied by a similar change in the union wage effect and, if so, where cause and effect lie.

⁷Following the reasoning above, the large union-nonunion productivity differential in construction can be viewed as a response to both a large union wage effect and a rapidly growing nonunion sector.

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 non-agricultural 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

⁸For 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 infamarginal, 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

⁹For 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.

*Comment by Daniel J. B. Mitchell**

What Do Unions Do? is a landmark in social science research. F & M have culled conclusions about unions from a vast array of data sets, surveys, and articles and expressed them in a fashion accessible to

most readers. It is hard to imagine any course in labor economics that would not include readings from this book. Because the authors' contribution is so obvious, I will concentrate on the few deficiencies and omissions in the volume.

One deficiency is a lack of analysis of bargaining. The outcomes of that process are analyzed, but the process itself is barely mentioned. Strikes are discussed only to show that their social cost is low. Yet the threat of a strike is precisely what extracts the various concessions from management that F & M document. Since bargaining is neglected, management motives—other than merely wanting to pay less—are also neglected. Related to this omission is the neglect of determinants of wage-change.

If there is a secret villain in the F & M study, it is the old Gregg Lewis approach in *Unionism and Relative Wages in the United States* (1963). Although F & M politely refer to Lewis's book as "influential" (p. 44), they view his wage-centered "monopoly" model as excessively narrow. Yet there is more life in the traditional model than F & M believe, if that model is expanded to include bargaining strategy and some of the authors' own insights. One can agree with the need to avoid a narrow focus, without having to jump to F & M's "voice" model, which ultimately adds little to their analysis.¹⁰

The traditional model, when combined with the median-voter approach favored by F & M, explains much of what they observe. Since unions possess the strike threat, they can extract concessions from management. And, since unions are controlled by senior workers, they tilt the pay package toward those workers. There is no need for a voice model to explain the bias toward tenure-related fringes found by F & M.

The authors stress the importance of the noncompensation aspects of unionized workplaces, such as workrules and partic-

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¹⁰In this review, I substitute the phrase "traditional model" for "monopoly model" to avoid the pejorative connotation of the latter. The prettier term "wage improvement" model could just as well have been used. I follow F & M's use of the term "voice" with its normative implication. (Who would want to be accused of stifling someone's voice?) The more negative term "vested influence" could have been substituted.

ularly industrial jurisprudence via grievance and arbitration mechanisms. But these characteristics can be explained without invoking the concept of voice. As in any system of price controls, the rules needed to prevent chiseling proliferate. Under rent controls, for example, landlords will attempt evasions such as reducing building maintenance or converting to condominiums, and controllers must create elaborate regulations to limit such evasions. Similarly, unions that succeed in raising wages attempt to prevent employer chiseling by pressing for limits on such actions as subcontracting, supervisors' performance of bargaining-unit work, the awarding of promotions and work assignments, and the introduction of new technology. Since F & M ignore this aspect of workplace rules, they consider those rules to be rare inefficiencies. These restrictions, however, may well be "second best" efficiency enhancers. They render the union wage advantage mainly a matter of income redistribution and limit artificially induced substitutions.

Also, rules proliferate, so some interpretive mechanism is needed to handle disputes. Here there is also a strategic interest. Strikes are costly, and managers want long-term, enforceable contracts with limits on the right to strike. In return, the price they pay to the union is a mechanism for resolving contract-interpretation disputes. Arbitration meets the needs of both parties.

Bureaucracy and formality may also contribute to the outcomes that F & M attribute to the union provision of voice. For example, many nonunion government employees are covered by "union-esque" practices, such as seniority in layoffs and advancement and formalized grievance procedures, and the progressive, nonunion firms surveyed by Foulkes (1980) are large and have centralized personnel bureaucracies. And, as noted above, formality is necessary in the union sector under the traditional model if employer chiseling is to be prevented.

Of course, large, nonunion employers do not necessarily require bureaucratic personnel systems in the absence of the union threat. Jacoby's historical studies (N.d.) indicate that such firms created their per-

sonnel departments and gave them centralized control only when the union threat was strongest. The point remains that the concept of voice adds no special insights to the story.

F & M's analysis of efficiency also suffers from their stress on voice. They usefully point out that productivity in the union sector is higher than in the nonunion. But is that fact the result of voice? At times, the authors seem to say that heeding the tastes of senior workers is automatically efficient (as opposed to "fair"); but, since those workers are less mobile than others, the presumption should run the other way.

F & M imply that nonunion employers want to make implicit contracts favoring seniors, but they cannot do so without unions because workers need protection from retaliation to voice their preferences. If that is the case, nonunion employers ought to be imploring Congress to set up mandatory neutral labor courts and works councils—with high penalties for employer retaliation—to help them effect such implicit contracts. Such behavior has yet to be observed.

F & M downgrade the evidence of Foulkes and other researchers who have investigated the practices of progressive, nonunion firms, some of which match union wages. That evidence undermines the argument that it is the profit-reducing effect of union wage pressure that causes employers to ignore the productivity gains resulting from unionism. Claims made by such employers that they gain managerial flexibility by remaining nonunion should not be lightly dismissed.

F & M argue that management exaggerates gains from flexibility because "production lines are machine-run" (p. 173). Yet F & M attribute productivity gains from voice to "more rational, professional management" (p. 163). They cannot have it both ways. One can only note (mischievously) that had the National Science Foundation allocated its funds by seniority rather than discretion, this book, given the age of the authors relative to the median age of the profession, would never have been written! More seriously, if managers say discretion is valuable, there is no more reason to doubt them than there is to doubt the various

worker preferences cited—and taken at face value—by F & M.

The argument for greater productivity from voice stems from a small number of studies. Several of these are based on value-added comparisons, which the authors acknowledge are upward biased. Those based on physical output are not conclusive: two regarding cement plants show mild productivity gains; and one regarding coal mining shows gains in one year followed by a string of losses. A construction study shows large gains in an industry in which an inherently large volume of employee turnover makes the voice model a dubious explanation.

As F & M note, survey data indicate that union workers are more dissatisfied with their jobs than their nonunion counterparts. They explain this puzzle as a byproduct of the adversarial environment created by bargaining, which weakens their case for added productivity from voice. The authors paint a rather dismal picture of disgruntled unionists who nevertheless cling to their jobs because they believe that the alternatives are even worse. The evidence is too contradictory to warrant more than a timid conclusion of “more research is needed”; it simply does not support the voice-productivity connection.

Ultimately, F & M's supposed productivity gains from voice stem largely from the degree of lower turnover in union firms. They find that these gains roughly offset the monopoly loss caused by the union wage effect. Had they remembered to subtract from their model the cost of working time lost to strikes, the rough offset would probably have turned into a slight net social cost, using a strict economic calculus.¹¹ But strict calculus is not behind F & M's policy conclusions.

Imagine that the authors, after carefully analyzing Italian railway timetables, with

elaborate controls for changes in rolling stock and with surveys of passengers and crew members, were able to determine that Mussolini *did* make the trains run on time. It is doubtful they would advocate fascism on that basis. Similarly, their support for labor law reform is not really based on efficiency. In the final analysis, F & M believe that employees *should* have more (union) voice, even if some social costs are entailed—a respectable position that ought not be made contingent on further scrutiny of the cement industry.

F & M also leave some important questions hanging. There is reference in the volume to the wave of union wage concessions that began in 1979. The section devoted to this topic (pp. 55–57) has the earmarks of a last-minute addition, however; and, as already noted, wage-change determination is a decidedly overlooked topic of the study.¹² The authors fail to note that unions could add efficiency to gain-sharing and profit-sharing plans—which have been features of some prominent concessions—by monitoring the internal employer data on which these contingency arrangements are based. (Nonunion workers must rely on employer interpretations of profitability under such plans.)

With hindsight, the authors note that the widening of the union-nonunion wage differential in the 1970s contributed to the concessions and the perilous conditions of several industries. This precursor to concessionary bargaining raises important questions about how well the parties to bargaining—given the adversarial nature of their encounters—deal with long-run economics. Interestingly, it is precisely the sen-

¹¹F & M put the monopoly cost of unions at 0.2 to 0.4 percent of GNP (p. 57) and the gains from voice-related turnover reduction at 0.2 to 0.3 percent of GNP (p. 110). Hence, the two influences cancel. They note later in the book that strikes amount to about 0.2 percent of working time (p. 217), but they do not weigh this third effect, with appropriate downward adjustments for substitution and similar offsets, against the others.

¹²For example, F & M discuss wage escalators very superficially, even though they are an important distinguishing characteristic of the union sector. (Very few nonunion workers are covered by escalators.) On page 54, they cite a manufacturing study indicating that union workers received about the same wage increases whether or not they had escalator coverage. BLS data indicate that for the private sector as a whole, escalated contracts provided significantly larger wage gains than nonescalated contracts, until the pattern reversed during the dramatic reductions of CPI inflation in the early 1980s. Thus, F & M's casual acceptance of escalator neutrality is open to question.

ior workers so prominent in the voice model who have to be concerned with the long term.

F & M project for the future a steady-state unionization rate of 10 percent, if the organizing trends of the early 1980s continue (p. 242). They note heightened management resistance to unionization, much of which they attribute to widening union-nonunion wage differentials. Their proxies for management resistance, taken from NLRB data, show a decided upward turn in 1970, following a period of rising strike activity and contract-ratification rejections. Thereafter, the indexes rise rapidly during a decade in which FMCS data indicate rapid growth in grievance rates.¹³

The evidence suggests that the overall climate in the union sector (not just wage determination) contributes to the responses of nonunion employers faced with organizing drives. Unions therefore face a dilemma. Poor relations with unionized firms create negative externalities for the labor movement as a whole, but the nature of externalities is that they are ignored by the parties who create them.

Voice models are not much help in explaining the pattern of union organizing efforts. But the traditional model—with its familiar “taking wages out of competition” incentive—predicts that organizing will tend to be confined to the sectors that are already partially organized. Analysis of NLRB data supports this prediction. In 1980, for example, the one-third of industries under NLRB jurisdiction that exhibited the least organizing contained almost two-thirds of employment and disproportionately high numbers of female and white-collar workers.¹⁴ Unions are staying on familiar ground

and are thereby avoiding the expanding sectors of the labor market.

Finally, since F & M's data predict continuing union slippage, one is forced to ask whether their 10 percent unionization rate would be a sufficient incentive to maintain progressive personnel policies in large, nonunion firms. If not, will workers ultimately seek political solutions to problems they cannot solve through collective bargaining? What will happen to a society that allows its workplace safety valve to be shut off? *What Do Unions Do?* is a fascinating study because it raises these provocative and profound questions far beyond the usual concerns of labor economists.

*Comment by Melvin W. Reder**

Like its subject, this book has (at least) two faces. One is a popular summary of what the authors believe to be known about the measurable concomitants of unionism. The other visage is that of an “anti-antiunion” tract that earns the dust jacket praise of the Secretary-Treasurer of the AFL-CIO, Thomas R. Donohue. Such duality of purpose results in an irrepressible conflict of intellectual interest.

This conflict will probably not affect sales adversely. Those who concur in the message of the tract will rejoice in the support seemingly afforded by truly unrelated statistics, while those who dissent will have to contend with a very influential book. That the book will be influential can be predicted from its lively popular style and the authors' willingness to provide easy answers to hard questions. Whatever they may think of the tract, those who seek a handbook on the

¹³The annual reports of the Federal Mediation and Conciliation Service indicate that requests for panels of arbitrators almost tripled between fiscal years 1970 and 1980. These requests are mainly for grievance arbitration. F & M cite a report that 28 percent of union members filed a grievance over a two-year period as an example of union democracy (p. 208), but they do not draw inferences from this rate for the state of workplace industrial relations in the late 1970s.

¹⁴Using data from the 1980 annual report of the NLRB, I ranked industries by the proportion of employees who were offered the opportunity to vote

in a union representation election. I omitted the public sector Postal Service, construction (since elections are not much used in that industry because of loose employee-employer attachments), and transportation industries (because of data problems related to the Railway Labor Act). There were 30 remaining industries. The text refers to the bottom ten. It might be noted that the proportion of elections won by unions is higher in this sector than others, suggesting unions require better odds before they will venture off unfamiliar turf. Generally, white-collar win rates for unions have been higher than for other workers by a substantial margin, another confirmation of this tendency.

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economic consequences of unionism will add to royalties, because of the authors' temporary monopoly of the subject. I suspect that many instructors will, like me, assign the book and punctuate the lectures with caveats.

Serious though it is, the tension between advocacy and analysis concerns me less than the numerous leaps from the perception of association to the attribution of causality. Variations in hourly compensation, worker productivity, the ratio of fringe benefits to cash payments, and so on are properly found to have been associated with the presence or intensity of unionization. But often the arrow of causality runs from these other phenomena to the incidence of unionization, as well as in the opposite direction. This is because unionism and its many correlates interact in a complex network of relationships from which inferences of cause and effect can be drawn only with great difficulty.

In contemporary economics, the only widely accepted procedure for drawing inferences, such as those of F & M on the empirical effect—or lack of it—of unionism on other variables, is to estimate a multi-equation model in which unionism and its associated variables are embedded, and then to infer the effects of variations in measured degrees of unionism on other variables of interest (such as wage rates and worker productivity) from the appropriate parameter estimates. Among other requirements, setting up such a model demands careful specification of whether the unionism variable is exogenous or endogenous and, if the latter, how it is to

be explained. It is no reproach to F & M that they have not undertaken this task. Despite substantial recent increments to the relevant data stock, as well as improvements in economic theory and econometric techniques, the state of the art is far from equal to such an undertaking, and probably will continue to be so for a long time to come.

In the meantime, there is room for one or more interpretive essays on what the accumulating body of empirical information implies. Subject to quarrels over important matters of detail, this book could be interpreted as such an essay. But, if that

were the authors' intent, even in part, they have seriously misled their readers by persistently blurring the distinction between interesting speculation and econometric estimation.

So much for methodological complaint. The substance of F & M's argument is that unionism has two faces: (1) a monopoly face associated with resource misallocation and restrictive work rules that are inimical to worker productivity, that cause higher product prices, and that reduce output and union employment; and (2) a voice face associated with collective action by workers to tilt company policies in directions favorable to long-service employees and toward deferred benefits as distinguished from benefits paid currently. F & M contend that the adverse effects of monopoly are outweighed by the benefits associated with voice. Subject to appropriate adjustments in the details of the argument, I consider their discussion to be one of several possible interpretations of the recent history of unionism in the United States.

A major defect of their account is the inadequate attention paid to the adverse effects of unionism on the welfare of non-union workers. If unionism does increase the real income of union members as F & M allege, then either nonunion workers believe that the costs of unionization exceed the value of the advantages it confers or they have been excluded from unions, or both. For the sake of brevity, I will concentrate on the second possibility. Unions exclude workers by insisting upon terms of employment that set unit labor costs above those at which all those willing to work on union terms will be hired, and they concomitantly ration scarce employment opportunities by seniority rules.

Disproportionately, the workers who are thereby excluded from jobs are would-be immigrants prevented from entering the country or actual immigrants compelled to work as "illegals"; teenagers; members of ethnic minorities; and part-time household workers. The employment opportunities of these groups are further inhibited by union-supported legislation on behalf of minimum wages, school-leaving regulations, and other labor market restrictions.

The effect of unionism on the earnings,

employment, and labor-force status of these groups, especially immigrants, is at least as important as the effects that F & M explicitly consider.¹⁵ Granted, these effects are not so readily summarized as the union-nonunion differentials on which F & M focus. Nonetheless, ease of measurement is not the sole or even the primary criterion for choosing the instruments with which to appraise an important institution like unionism. Contrary to F & M, I do not think that what unions do, and have done, can be adequately judged without explicit consideration of the legislated impediments to the functioning of labor markets that have been strongly supported by unions as a prop for their bargaining policies.

A further difficulty: F & M argue as though the only critics of the monopoly face of unionism have been (slightly caricatured but still recognizable) neoclassical economists. They ignore the long history of protest over the restrictionist policies and practices of the AFL by, for example, the Knights of Labor, the IWW, the CIO, and the early civil rights movement. Put differently, what unions do is not only a matter of how they have affected the U.S. economy through collective bargaining, but also how they have contributed to the structuring of both the U.S. and the world economy (through restrictions on both trade and immigration) by exercise of their political influence.¹⁶

¹⁵F & M briefly address these issues in chapter 5 ("Labor's Elite: The Effect of Unionism on Wage Inequality") and chapter 10 ("What Unionism Does to Nonorganized Labor"). For lack of space, I cannot discuss these chapters in detail. Neither chapter, however, speaks to the concerns raised here, and the measurements presented in these chapters are misleading.

¹⁶To hold the labor movement *solely* responsible for U.S. policies on international trade and immigration would, of course, be incorrect. Nevertheless, U.S.

To summarize, F & M have written a defense brief on behalf of labor unions. The brief is in the nature of a rebuttal to charges that the monopolistic practices of unions reduce real output, worker productivity, and employment and increase inequality in the distribution of labor earnings. Although I am not persuaded that these charges are true, I consider the authors' rebuttal to be both unsuccessful and unnecessary. To establish either the rebuttal or the original accusation would require a far more detailed model of the functioning of the U.S. economy than it will be possible to devise, let alone estimate, in the foreseeable future. But in basing their defense of union institutions upon an unavoidably inadequate econometric analysis of their directly measurable effects, F & M have missed an opportunity to develop a case for the "redeeming social value" of unionism.

Such a case might well be based on the importance to a free society of having terms of employment—including working conditions—responsive to the "voice" of incumbent job holders. F & M are to be praised for importing Albert Hirschman's exit-voice antinomy to the analysis of union behavior. Nevertheless, they have not attempted to develop and adapt these concepts to make them useful in analyzing the specific characteristics of unions. The actual, and even more the potential, effect of union voice operates not only through worker morale, turnover, and productivity, but also through the social choice mechanisms operative in a society. The great omission of this book lies in its failure to consider what unions *sometimes* do to the sociopolitical climate of a nation.

unions have played a significant role in determining these policies—and they continue to do so. Moreover, the behavior of U.S. unions toward immigration is very similar to that of unions in other developed countries.

REPLY by RICHARD B. FREEMAN and JAMES L. MEDOFF*

What Do Unions Do? sought to organize recent empirical analyses of the effects of unionism on major economic and political outcomes in the United States and to spark additional research on often neglected aspects of the institution. In any volume of this scope, one makes numerous decisions and provides many interpretations that can and invariably do draw critical comment from those who would weigh and evaluate evidence differently. We appreciate the opportunity given us by the editors of *ILRR* to respond to the comments contained in the *Review*, as well as those given elsewhere. We will focus on the points raised by Ashenfelter, Hirsch, Lipsky, and Mitchell, dealing not with areas of agreement or praise (which we appreciate) but with areas of disagreement and misinterpretation that offer opportunity for advancing knowledge. As Reder's comment deals not with the empirical evaluation we wrote but with a political apologia that we did not write, impugning our scholarly motivation and honesty, it is best dealt with outside the scholarly literature.¹⁷

Broadly put, the serious commentary on the book raises substantive issues regarding methodology, specific findings and choice of topics, and our overall theme about the two faces of unionism. We deal with these in turn.

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¹⁷Test the reader regard our summary dismissal of Reder's review unfair, we note: his castigation of our book for being endorsed by the Secretary-Treasurer of the AFL-CIO, as if that is somehow a sin, while ignoring the endorsement also on the dust jacket by the head of a major business group, The Committee for Economic Development; his claim that he will correct our errors in his class without specifying them; his claim that our major failure is to ignore what unions "sometimes" do to the body politic, again without specifying what he has in mind; and his bold assertion that current methodology calls for complete "true" models, without specifying them. We find it impossible to deal with these comments in a scholarly context.

Methodology

Our primary methodological thrust, and that of the dozens of other researchers whose work we cite or build on, has been to contrast the level of certain critical economic variables between union and non-union settings, controlling for other factors that affect the variables of concern and are correlated with unionization. Fully aware that no one knows the "true" structural model of the economy, we sought to gather diverse pieces of statistical evidence to cast light on the effects of unionism, using longitudinal (before and after) as well as cross-section data to separate causal relationships from spurious associations. In contrast to many econometricians who have used better data and more refined technique to address more esoteric and less answerable questions, we sought to make a collage of data on issues that matter to the general economist and the public at large. Even Griliches (1984) seems to concur with our views of how the new wealth of micro data should be approached:

We should be using the newly available data sets to help us find out what is actually going on in the economy and in the sectors that we are analyzing without trying to force our puny models on them. The real challenge is to try to stay open, to learn from the data, but also, at the same time, not drown in the individual detail. We have to keep looking for the forest among all these trees (p. 74).

The view of social science underlying our methodology is not unique to us: it was put forth elegantly by Feldstein (1982) in his 1980 Fischer-Schultz lecture:

As a practical matter, we often need different studies to learn about different aspects of any problem. The idea of estimating a single complete model that tells about all the parameters of interest and tests all implicit restrictions is generally not feasible with the available data. Instead, judgements must be formed by studying the results of several studies, each of which focuses on part of the problem and makes false assumptions about other parts (p. 830).

By taking such an approach, we are led naturally to broad generalizations regard-

ing union effects, subsuming into residuals much variation that occurs across specific cases. Lipsky raises several problems with this procedure and offers some suggestions for ways to improve upon it. We agree that our understanding of what unions do would improve if we "began to incorporate, into our statistical analysis, measures that capture the various forms in which union collective bargaining appears," particularly the structure of collective bargaining; our experiments with the percentage organized in an industry or area is an admittedly small step in that direction (Freeman and Medoff, 1981 and 1984). We also agree with Lipsky that our work has given short shrift to "organizational and behavioral factors that are an important source of variation in outcome," and look forward to detailed studies of particular unions, sectors, and systems that yield substantive conclusions about variation. Our colleague John Dunlop has made a similar point to us with the forcefulness for which he is famed. While we certainly recognize in *What Do Unions Do?* that different unions do different things in different settings,¹⁸ our attempts to explain this variation are one beginning.

Specific Findings

Clearly, the specific finding on which reviewers express greatest skepticism is the alleged overall higher productivity of union than of nonunion work places. As we wrote in our book:

Our "answer" to the debate over what unions do to productivity is probably the most controversial and least widely accepted result in this book. Some cavil at the finding because of a strong prior belief that the higher productivity of unionized sectors is, in fact, the result of the substitution of capital for labor or the hiring of "better" workers, which have not been correctly taken into account in the union productivity studies. Others criticize because they believe that higher productivity implies lower costs in the union setting (which it does not), making managerial opposition to unionism difficult to understand. Yet others find the result troubling

because of counter examples known to them personally. While the new work deals with these problems, at least in part, the controversy is unlikely to disappear. Age-old debates do not often end with a bang, even with computerized evidence (p. 80).

We believe that studies of particular sectors, with if possible output measured in physical units, offer the best chance for pinning down the union productivity effect. Whether, in a decade or so, the evidence will be more consistent with the view that on net unions have no impact on productivity, as Ashenfelter suggests, or that they are indeed associated with higher productivity (balancing out higher wages to permit union firms to compete even if they lack "rents" to cover the higher wages) is clearly a matter of speculation. We agree with Hirsch that the issue may depend on the structure of markets in which unions are found, with productivity effects more likely to be positive where wages are high and competition strong. We believe that when we began our inquiry, most economists would have predicted negative union productivity effects in the vast majority of settings. At the least, our econometric research on the impact of unionization on productivity has shifted many "priors" to be more consistent with the voluminous case work of Slichter, Healy, and Livernash (1960).

Hirsch also emphasizes the impact of collective bargaining on the growth of productivity, citing results showing a negative relationship between unionism and productivity growth. As indicated in the book, we believe this to be a relatively unexplored subject that is of major importance both theoretically and empirically. After all, even small effects on growth rates are likely to outweigh static welfare triangles in almost any social-welfare calculation. Our assessment of the productivity-growth research as inconclusive remains. The Hirsch and Link findings cited by Hirsch lend additional weight to the belief that unions adversely affect productivity growth. Our most recent analysis of this question reveals no relation between unionization and productivity growth using Census of Manufactures data, and a positive relation using National Income and Product data (primarily because services have had low pro-

¹⁸See, for example, our discussions of wages, quit rates, productivity, and profitability in chapters 3, 6, 11, and 12, respectively.

ductivity growth and have been relatively nonunion), lending weight to our conclusion that the evidence is mixed.¹⁹ We eagerly await more evidence and better models of what unions do to productivity growth.

A related issue pertains to the union-nonunion differential in managerial flexibility. Mitchell accuses us of "downgrading" the evidence of Foulkes and others on this matter. Far from it. Chapter 10 gives quite good play to Foulke's valuable investigations. The reason we believe productivity losses from lost flexibility are exaggerated by managers is not that "production lines are machine-run," but rather that substitution parameters that measure flexibility are *second order parameters* in production functions, so that considerable differences in the parameters between union and nonunion firms are possible without causing sizeable productivity differences.²⁰ Indicative of the attention we give to flexibility per se is our effort (the first, to our knowledge) to estimate elasticities of substitution in union and nonunion settings (Freeman and Medoff, 1982: 220–33). Flexibility is important, but not in productivity comparisons.

In addition to describing their disagreement with certain sections of the book, the commentators note several subjects on which they wish we had done more. Here, we have only the defense of finite life. In writing a book on such a vast topic as trade unionism, one must give short shrift to some topics or end up like Sisyphus, never completing the task. We admit to sins of omission on the dynamics of wage negotiations, internal union behavior, detailed treatment of the most recent developments in collective bargaining, strikes, and, of course, the public sector—a sufficiently difficult terrain to merit quite a different analytical treatment.²¹

¹⁹We have worked with Linda Bell and Wayne Gray on this topic.

²⁰In technical parlance, if we have a general production function, the first-order Taylor series expansion terms do not include the elasticity of substitution. It enters with the second-order terms.

²¹The National Bureau of Economic Research is currently engaged in a research project focusing on public sector unions.

General Theme

In one sense, our overall theme that unions should be viewed in terms of their monopoly and voice-response impacts on the economy appears to have been accepted by most reviewers of our work here and elsewhere. Whether we have properly weighed the costs of the monopoly face against the benefits of the voice face is, however, obviously a point of contention.

At one stage we envisaged a Denison-type "sources of growth" accounting table to summarize the pluses and minuses of unionism, but in the end we shied away from that, believing that our knowledge was insufficient to sustain such a procedure. Ashenfelter properly notes that, by failing to examine in depth the hypothesis that unions may simply be enforcing efficient contracts, we have not dealt sufficiently with the monopoly costs of unionism. Mitchell and Lipsky also feel that we have perhaps been too ready to criticize unions as monopoly institutions. Hirsch calls for "a more detailed conceptual development of the conditions under which collective voice does improve efficiency within the firm." We concur with comments of this nature, which underscore the need for more evidence. When this information appears, we will amend our calculations where necessary.

Conclusion

In an article one tries to sustain at least one point, leaving numerous issues for other researchers or for future research. In a book one is obligated to be more encyclopedic: one tries to sustain a theme along many dimensions, fully cognizant that one's knowledge and the available evidence are greater on some matters than on others. We are grateful to the reviewers of *What Do Unions Do?* for highlighting those parts of our work, and that of our colleagues elsewhere, that need buttressing and possibly amending. As research follows the patterns suggested by Ashenfelter, Hirsch, Lipsky, and Mitchell, we will all enjoy the benefits of increased knowledge and deeper understanding. If this research forces us to make more changes in our assessment of unions than we currently believe are likely, we will not be displeased.

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