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THE IMPACT OF COLLECTIVE BARGAINING: CAN THE NEW FACTS BE EXPLAINED BY MONOPOLY UNIONISM?

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ABSTRACT

In this paper we focus our attention on the question of whether union/nonunion differences in nonwage outcomes can, in fact, be explained in terms of standard price-theoretic responses to real wage effects, as opposed to the real effect of unionism on economic behavior.

We reach three basic conclusions. First, unions and collective bargaining have real economic effects on diverse nonwage variables which cannot be explained either in terms of price-theoretic responses to union wage effects or be attributed to the poor quality of our econometric "experiments". Second, we find that while sensitivity analyses of single-equation results and longitudinal experiments provide valuable checks on cross-sectional findings, multiple-equations approaches produced results which are too sensitive to small changes in models or samples to help resolve the questions of concern. Finally, on the basis of these findings we conclude that the search for an understanding of what unions do requires more than the standard price theoretic "monopoly" model of unionism. New (and/or old) perspectives based on institutional or industrial relations realities, contractarian or property rights theories, or other potential sources of creative views are also needed.

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The recent outpouring of empirical studies on the impact of collective bargaining on the economy has provided us with a large body of new evidence regarding differences between union and nonunion workers and union and nonunion enterprises along many dimensions other than rates of pay.

Can the observed union/nonunion differences in non-wage outcomes be explained primarily in terms of preunion characteristics of firms or individuals? Can the observed differences be explained as a response to the effect of unions on wages? Or does the new evidence suggest that unions have an important impact on economic performance through routes ignored in the standard monopoly model of the institution?

The need for and value of new theories of trade unionism depends on the answers to these questions.

In our initial review of recent literature 1/ we focused largely on the question of whether union/nonunion differences obtained in cross-section studies were illusions, which primarily reflected the poor quality of our econometric "experiments". In this paper we wish to focus greater attention on the question of whether union/nonunion differences in nonwage outcomes can, in fact, be explained in terms of standard price-theoretic responses to real wage effects, as opposed to the real effect of unionism on economic behavior.

The paper is divided into four sections. Section I summarizes the recent empirical findings about union/nonunion differences in non-wage outcomes on which the interpretative debate focuses. The second section examines the results of econometric probes designed to assess whether the effects set out in

Section I are best interpreted as illusory due to innate differences between union/nonunion workers or firms, as real for price theoretic reasons or real for reasons that go beyond standard price theory and thus require new theoretical perspectives. The final section lays out our conclusions regarding the implications of the evidence for the standard price theoretic or monopoly model of unionism as opposed to other perspectives, both old and new.

We reach three basic conclusions:

1) Unions and collective bargaining have real economic effects on diverse non-wage variables which cannot be explained either in terms of price-theoretic responses to union wage effects or as illusions, attributable to the poor quality of our econometric "experiments".

2) Some econometric techniques for probing union/nonunion differences provide useful insights into the real effects of unions while others do not. In particular, we find that sensitivity analyses of single-equation results and longitudinal experiments provide valuable checks on cross-sectional findings while multiple-equations approaches produce results which are much too sensitive to small changes in models or samples to help resolve the questions of concern.

3) On the basis of these findings we conclude that the search for an understanding of what unions do requires more than the standard price theoretic "monopoly" model of unionism. New (and/or old) perspectives based on institutional or industrial relations realities, contractarian or property rights theories, or other potential sources of creative views are also needed.

I. The Evidence in Question

It is important at the outset to lay out the union/nonunion differences in economic variables on which modern work has focused. Accordingly this section briefly summarizes the central findings of recent research, including for purposes of comparison and ensuing analysis results on wages as well as non-wage outcomes. As a guide to the discussion, Table 1 gives the central findings of these studies categorized by the following substantive issues: the level and structure of compensation; internal and external mobility; work rules, (management) flexibility and (employee) satisfaction; and inputs, productivity and profits. The reader will notice that our set of issues is not exhaustive. We have, in particular, neglected such important topics as the internal operation of unions, strikes, and the survival of the organization itself, in part because these topics do not lend themselves to the union/nonunion comparisons which form the bulk of the research on the topics in the table. In addition, we concentrate exclusively on the private sector. While, as noted, we have no pretence that our set of issues is all-encompassing and while our listing of relevant references is undoubtedly incomplete, we believe that the table provides a reasonably accurate picture of the empirical results in question.

The Level and Structure of Compensation

The first and probably still the most widely studied issue is the differential between union and nonunion wages. The early literature on this differential was summarized in Lewis's influential 1963 book, Unionism and Relative Wages in the United States. Since the publication of Lewis's book, a number of new sources of individual-level data (such as the May Current Population Survey) which permit estimation of the wage effect have become available. With micro-

data of this kind, it is possible to compare the wages of union and nonunion workers with similar demographic characteristics who are also in the same detailed industry and/or occupation. As Johnson (1975) has reviewed some of this work, our summary will be brief. The post-Lewis micro-data estimates (derived with Ordinary Least Squares (OLS)) have generally found wage differentials noticeably above the 10 to 15 percent range given in Lewis's book. However, the analyses that have looked within more detailed cells, especially those with industry as a dimension, have tended to yield estimated differentials near the top end of the 10 to 15 percent range. This makes very good sense given that the studies summarized by Lewis normally examined a very narrowly defined group of workers. Another form of data which has been used in recent studies pertains to individual establishments. These data (from surveys such as the Employer Expenditures for Employee Comnpensation Survey (EEC)) permit the estimation of wage effects for production or nonproduction workers among firms of the same size within the same 3-digit Standard Industrial Classification (SIC) industry. Estimates using these data are quantitatively closer to those of Lewis, yielding union/nonunion differences of 10 percent or so. All told, with rare exception, recent studies confirm the existence of a sizeable union/nonunion wage differential.

Another important aspect of the union wage effect which has been examined is the difference between union and nonunion wage adjustments to varying economic conditions. The recent work on the cyclical variation in wage rates has confirmed the earlier finding of Lewis that the union/nonunion wage differential has tended to be greater during economic downturns, which suggests that the reduction in (the growth of) real wage rates in response to a reduction in product demand is smaller under trade unions. Interestingly, the work of

TABLE 1

1: Recent Evidence on Union/Nonunion Differences Based on Cross-Sectional Data

Variable (Price Theoretic Control)	Finding	Partial Listing of Relevant References
Level and Structure of Compensation		
Wage Rates	All else (measureable) the same, union/nonunion hourly wage differential is between 10% and 20%.	Ashenfelter (1976), Freeman & Medoff (1981a) Lewis (1980), Mellow (1981a), Oaxaca (1975), Welch (1980).
Cyclical Responsiveness of Wage Rates	Union wages are less responsive to cyclical variation in market conditions than are nonunion wages.	Ashenfelter (1976), Hamermesh (1972), Johnson (1981), Lewis (1963), Medoff (1979), Medoff and Mitchel (1980a, 1980b), Fay (in process) Parson (1968), Raisian (1979).
Determinants of Compensation Differential	Other things equal, the union compensation advantage is higher the greater the percentage of a market's workers who are organized. The effects of market concentration on wage differentials is unclear. The differentials appear to be very large in some regulated markets. They appear to decline as firm size increases.	Dalton & Ford (1977), Donsimoni (1978), Ehrenberg (1979), Freeman & Medoff (1981a), Hayden (1977), Hendricks (1975), Kahn (1978), Kochan (1980), Lee (1978), Mellow (1981b), Weiss (1966).
Fringes (Wages(or total compensation) held fixed)	All else the same, union/nonunion hourly fringe differential is between 20% and 30%. The fringe share of compensation is higher at a given level of compensation.	Duncan (1976), Freeman (1981), Goldstein & Pauly (1976), Leigh (1979), Solnick (1978), Viscusi (1980).
Wage Dispersion	Wage inequality is much lower among union members than among comparable nonmembers and total wage dispersion appears to be lowered by unionism.	Freeman (1980c,forthcoming,) Hyclak (1979-1980), Plotnick (1981).

TABLE 1 (cont.)

Variable (Price Theoretic Control)	Finding	Partial Listing of Relevant References
Wage Structure	Wage differentials between workers who are different in terms of race, age, service, skill level, and education appear to be lower under collective bargaining.	Ashenfelter (1976), Bloch & Kuskin (1978), Johnson & Youmans (1971), Kiefer & Smith (1977), Leigh (1978), Pfeffer & Ross (1980), Schoeplein (1977), Shapiro (1978).
Internal & External Mobility		
Promotions (Wages and "labor quality" held fixed)	Seniority independent of productivity is rewarded substantially more in promotion decisions among union members than among otherwise comparable non-union employees.	Halasz (1980), Medoff & Abraham (1980b, 1981b), Yanker (1980).
Quits (Wages and "labor quality" held fixed)	The quit rate is much lower for unionized workers than for similar workers who are nonunion.	Blau & Kahn (1981), Block (1978a), Farber (OLS Results 1979), Freeman (1976, 1980a, 1980b), Kahn (1977), Leigh (1979).
Temporary layoffs (Wages and "labor quality" held fixed)	There is much more cyclical labor adjustment through temporary layoffs in unionized manufacturing firms than in otherwise comparable firms that are nonunion.	Blau & Kahn (1981), Medoff (1979).
Terminations (Wages and "labor quality" held fixed)	Terminations are more likely to be on a last-in-first-out basis among union employees, ceteris paribus.	Blau & Kahn (1981), Medoff & Abraham (1981a, 1981b).

Work Rules, Flexibility and Satisfaction		
Rules (Wages held fixed)	There are important differences in the prevalence and nature of various rules in union and nonunion settings, such as those stipulating the role of company service and the way grievances are to be handled. Union work places appear to be run more by rules, with more rigidity in the scheduling of hours and less worker flexibility.	Freeman (1980a), Kochan & Bloch (1977), Kochan & Helfman (1977), Medoff & Abraham (1981b).
Management Practices	Management in unionized cement firms appears to be more professional (less paternalistic or authoritarian), more standards oriented, and more in touch with work performance than management in similar nonunion firms.	Clark (1980a).
Management Flexibility (Factor prices held fixed)	Management in unionized manufacturing firms appears less able to substitute nonproduction worker hours for production worker hours but seems no less able to substitute capital for production labor than similarly situated nonunion management.	Freeman & Medoff (forthcoming b).
Satisfaction with Jobs Overall (Wages held fixed)	The stated level of overall job satisfaction is lower, but the wage gain required to induce a job change is higher for union members than for otherwise comparable employees who are not members.	Borjas (1979), Freeman (1976, 1978a), Kochan & Helfman (1979), Mandelbaum (1980).

TABLE 1 (cont.)

Variable (Price Theoretic Control)	Finding	Partial Listing of Relevant References
Evaluation of Rules and Conditions (Wages held fixed, where appropriate)	Unionized workers state that they are more satisfied with their wages and fringes, less satisfied with their supervision, and less satisfied with their working conditions than nonunion workers. The extent to which stated job security grows with tenure is substantially greater under unionism. While the probability of viewing promotions as fair declines with service among nonunion employees, it increases among union members.	Duncan & Stafford (1980), Freeman & Medoff (1982), Kochan & Helfman (1979), Viscusi (1980).
Inputs, Productivity, and Profits		
Pre-firm Quality of Work Force	Other things equal, workers in unionized firms tend to have more "human capital".	Allen (1979), Brown & Medoff (1978), Farber (1979), Frantz (1976), Kahn (1979), Kalachek & Raines (1980).
Capital Intensity (Wages held fixed)	Unionized firms in manufacturing, construction, and underground bituminous coal appear to have higher capital-labor ratios than similar nonunion enterprises.	Allen (1979), Brown & Medoff (1978), Clark (1980b), Connerton, Freeman & Medoff (1979), Frantz (1976).
Productivity (Capital intensity and "labor quality" held fixed)	In manufacturing and construction and in the underground bituminous coal industry in nonturbulent times, unionized enterprises appear to have greater productivity than those that are nonunion, all else equal. In underground coal, productivity appeared to be lower under unionism in the turbulent years around 1975.	Allen (1979), Brown & Medoff (1978), Connerton, Freeman & Medoff (1979), Frantz (1976).

TABLE 1 (cont.)

Variable (Price Theoretic Control)	Finding	Partial Listing of Relevant References
Input, Productivity, Profits (continued)		
Profitability (Industry, type of business held fixed)	The rate of profit per unit of capital appears to be lower under unionism.	Brown & Medoff (1978), Clark (forthcoming), Frantz, (1976), Freeman & Medoff (forthcoming, b), Hayden.

Johnson (1981) and Mitchell (1981a) and an analysis of Current Wage Developments establishment-level data suggest that the union wage effect grew substantially during the 1970s to a point where it is roughly comparable to its level in the 1930s.

One key question to ask about the union/nonunion wage differentials is, "how do they vary across settings?" Recent empirical work on this subject has been based on the notion that union wage gains will be high where the elasticity of demand for labor, and hence the cost of increased relative wages in terms of lost members, is low. The evidence that, at least in the manufacturing and construction sectors of our economy, union wages but not nonunion wages grow with the fraction organized in the relevant product market is consistent with this claim; this is because a high percentage organized is likely to be associated with a low demand elasticity for union products and thus a low demand elasticity for union members. Other work has concentrated on the effect of market regulation on the union wage effect. Ehrenberg (1979) presents evidence consistent with the claim that union wages are raised by the regulation of public utilities. Hayden (1977) argues that the sizeable impact of unionism on trucker wages (40 percent or so) is attributable both to ICC regulation of the sector and to the National Master Freight Agreement, which created industry-wide bargaining.

A significant piece of the new work on union effects has been concerned with the composition and distribution of compensation. With regard to fringe benefits, recent analyses have demonstrated that the "union fringe effect" is bigger, in percentage terms, than the "union wage effect". Data from the 1968, 1970, and 1972 EEC indicate, for example, that holding constant the characteristics in employees' establishments, blue-collar workers covered by collec-

tive bargaining received fringe benefits that were about 28 to 36 percent higher than those of blue-collar workers who were not covered (compared to a union wage advantage of 8 to 15 percent). For workers receiving the same total compensation per hour, the fringe share of labor cost was markedly higher in the union setting (Freeman, 1981). Looking at separate fringes, the largest union/nonunion percentage differentials on a per hour basis are for pensions, life, accident and health insurance, and vacation pay.

Since their inception, unions in our country have been concerned with wage inequality as well as the level of wage rates. The practice which most exemplifies unions' efforts on this front is the long standing policy of pushing for "standard rates", that is, uniform rates for comparable workers across establishments and for given occupational classes within establishments.

Estimates presented in Freeman (1980c) show that, for blue-collar workers, wage inequality is substantially lower among union members than among similar nonmembers. Consistent with this, estimates of separate wage equations for union and nonunion workers have found that virtually all standard wage-determining variables are associated with smaller earnings differentials under unionism. Moreover, union wage policies appear to contribute to the equalization of wages by decreasing the differential between covered blue-collar workers and noncovered white-collar workers. If we add the apparent decrease in inequality due to wage standardization and the apparent decrease due to reduction in the white-collar/blue-collar differential to the apparent increase due to the greater wages of blue-collar union workers, we find that the apparent net effect of unionism is to reduce total wage inequality. Evidence on inequality of earnings across standard metropolitan statistical areas (SMSAs) and states and over time also shows a negative relationship between unionism and dispersion in pay.

In short, it appears that the structure of wages in the United States has been compressed by the wage policies of organized labor. Finally in related work, several studies show union wage impacts to be larger for blue-collar as opposed to white-collar workers, for younger as opposed to older employees, and for the less as opposed to the more educated; some studies indicate a greater wage effect for blacks than for whites, but others do not.

Internal and external mobility

The new work on unionism has, as noted earlier, turned attention to outcomes other than the wage level. One of the most important set of issues receiving this attention involves the impact of unionism on the internal and external mobility of employees. To evaluate the effects of unionism on firms' employment policies (the awarding of promotions, the ordering of layoffs, etc.) it is necessary to have knowledge of what is actually happening inside both union and nonunion firms. Survey evidence collected by and discussed in Medoff and Abraham (1980b, 1981a, 1981b) and recent case studies have provided relevant information concerning the role of seniority independent of performance in firms' promotion and termination decisions. With respect to promotions, the survey data reveal that whereas 68 percent of private sector unionized employees outside of agriculture and construction work in settings where senior employees are favored substantially when promotion decisions are made only 40 percent of the nonunion work force is employed in such settings. When the analysis is restricted to hourly employees, the estimates of concern are 68 percent for union members and 53 percent for the nonunion labor force. Regressions with the survey data which include controls for firm size, industrial sector, and geographic region yield differences similar to those just given. Moreover, case

studies of a number of U.S. firms tell the same story: company service counts more in promotion decisions in union settings.

One of the central tenets of the collective voice/institutional response model is that among workers receiving the same pay, unions reduce employee turnover and associated costs by offering "voice" as an alternative to "exit". Recent evidence using newly available information on the job changes of thousands of individuals and on industry-level turnover rates shows that with wages and diverse other factors held constant, unionized workers do have substantially lower quit rates than nonunion workers who are comparable in other respects. The reduction in quits and the accompanying increase in tenure appear to be as substantial for blacks as for whites and greater for older than for younger workers.

With less ability to reduce (the growth of) real wage rates and with lower quit rates, unionized firms can be expected to make greater use of other adjustment mechanisms, such as average hour reductions and layoffs. Both establishment-level and individual-level data sets demonstrate that, holding fixed wages, worker characteristics, and industry conditions, temporary layoffs and recalls are a more important form of labor adjustment in unionized manufacturing firms than in otherwise comparable firms that are nonunion. Moreover, the use of temporary layoffs relative to the use of average hours reductions appears to be greater under unionism. Hence, it seems that the layoff/recall syndrome which has received much recent attention is, for the most part, a unionized manufacturing (in particular, durables) phenomenon.

With respect to the order of layoffs, evidence from the seniority survey just cited reveals that among those who had witnessed work force reductions rules protecting senior workers against being permanently laid off before their

junior coworkers are more prevalent and stronger under trade unions. For hourly employees, 95 percent of the responses pertaining to groups covered by collective bargaining indicated that seniority in and of itself receives substantial weight in termination decisions, compared to 70 percent of the responses pertaining to noncovered groups. As for "strength", 68 percent of the survey responses pertaining to unionized hourly employees stated that a senior worker would never be involuntarily terminated before a junior worker, whereas only 28 percent of the responses pertaining to nonunion hourly employees stated that this is so. These survey results could not be explained in terms of company characteristics and are consistent with the findings of Blau and Kahn (1981) who used individual-level data.

Work Rules, Flexibility and Satisfaction

Other personnel practices and procedures also appear to be affected by the presence of unionism. In Clark's (1980a, 1980b) study of six cement firms which were recently unionized, management practices appear to have changed significantly with the coming of a union, in directions which can be labelled "productivity oriented". These observations gain credence from the fact that they are similar to those of Sumner Slichter, James Healy, and E. Robert Livernash, who conducted a myriad of case studies concerning the relationship between unionism and management behavior for their classic 1960 opus, The Impact of Collective Bargaining on Management. It should be noted that, with evidence of the type which has been collected, it is difficult to infer whether managers were moved from non cost-minimizing behavior to cost-minimizing behavior or whether the type of behavior which is cost-minimizing is different in union and in nonunion environments. Nevertheless, a key tenet of our collective

voice/institutional response view of trade unions is: management responds to the shock and sustained pressure of unionism in ways which cannot be fully understood with a standard price theoretic model.

It would seem reasonable, given what is believed about the objective function of the typical union, to find less management flexibility in unionized establishments than in otherwise comparable establishments that are nonunion. Consistent with this view, evidence drawn primarily from the 1972 Census of Manufactures and the EEC show that within U.S. manufacturing the ease of substitution for production labor, particularly substitution of nonproduction for production labor, is lower under trade unionism. However, it should be mentioned that the limited evidence does not indicate that unionism is associated with a lower elasticity of substitution between labor and capital and thus with whatever technological change is embodied in new capital.

Several recent studies examining the impact of unionism on the stated job satisfaction of workers have found union workers expressing less satisfaction, or in some instances no more satisfaction, with their jobs than similar nonunion workers, when compensation is held fixed, and even when compensation is not held constant. At the same time, however, union members are also more likely to state that they are "unwilling to change jobs under any circumstance" or "would never consider moving to a new job" than are their "more satisfied" nonunion counterparts, even when the wage is fixed. One interpretation of these results is that the collective voice of unionism provides workers with a channel for expressing their preferences to management and that this increases their willingness to complain about undesirable conditions.

Evidence has also been accumulated concerning workers' stated satisfaction with particular aspects of their jobs. Some of the findings most relevant

to the discussion at hand are: (1) union members are much more likely to state that they are happy with their wages and fringes than are otherwise comparable nonunion employees; (2) there appears to be a strong tendency for unionized workers to state they are less happy with their supervisors and have worse relations with them; (3) there is a tendency for unionized workers to report their physical work conditions are less desirable than those reported by unionized workers (4) the extent to which stated job security grows with tenure is substantially greater under unionism; and (5) while the probability of viewing promotions as fair is negatively related to seniority in nonunion settings, it is positively related to seniority under unionism.

Inputs, Productivity, and Profits

When unions raise wages or otherwise alter labor costs, enterprises can be expected to change factor inputs and modes of organization in such ways as to raise the marginal revenue product of labor up to the point where it equals the new marginal cost of labor. Two of the most important ways in which firms could potentially do this are to hire "higher quality" workers and to increase their capital/labor ratios. Evidence has been offered showing that blue-collar union workers do in fact have somewhat more "human capital" than similar nonunion workers. With May CPS data for 1973-1975, blue-collar union members are found to be three to four years older than otherwise comparable nonunion blue-collar workers, and to have slightly more education. Separate wage equations for males and females, which differentiate workers by schooling, age, and region, lead to the conclusion that unionized production labor has about 6 percent more "human capital" within 2-digit manufacturing industries (Brown and Medoff, 1978). It should be noted, however, that an index of labor quality based on weights from

wage regressions is at best only a crude approximation to an index based on "true" productivity weights, as is implied by evidence that a substantial fraction of seniority/earnings differentials cannot be explained by seniority/productivity differentials (Medoff (1977) and Medoff and Abraham (1980a, 1981a)). Moreover, it should be recognized that indices of the sort being discussed ignore potentially very important, but not measured, worker characteristics.

With respect to capital/labor ratios, evidence from the 1972 Census of Manufactures suggests that, as expected, capital-labor ratios are higher in unionized settings within two digit manufacturing industries. What is perhaps more surprising is that the substantially higher capital/labor ratio under trade unions cannot be fully explained by the union wage effect (nor by the union effect on total compensation). Thus, even one of the major price-theoretic response variables seems to be affected by unionism in ways that possibly go beyond the standard compensation-level path.

A number of studies conducted during the past few years have sought to isolate "as well as is possible with existing data" the effect of trade unionism on the productivity of otherwise comparable workers utilizing the same amount of capital. The Brown and Medoff (1978) study, based on 1972 state-by-industry data for U.S. manufacturing, found that unionized enterprises had higher productivity than otherwise comparable nonunion establishments within the same 2-digit SIC industries. The magnitude of the effect varied from .24 ln points (when they allowed the regression to determine the elasticity of output with respect to capital) to .07 ln points (when they imposed an estimate of the output/capital elasticity which was viewed as an upper-bound to the true value). Studies of particular manufacturing industries--wooden household fur-

niture and cement--have also found a positive productivity differential. Allen reports a sizeable positive differential in construction, using a value output measure. His result is supported by the findings of Mandelstamm (1965), who avoided the potential problems of measuring output in dollar terms by having union and nonunion contractors cost out an identical project.

That unionism can be associated with lower as well as higher productivity has been documented for the U.S. underground bituminous coal sector, where unionized mines were estimated to be about .25 ln points more productive than comparable nonunion mines in 1965, but about .20 points less productive a decade later. One potential explanation for the observed change in union/nonunion productivity differentials is that the "quality" of industrial relations in that sector appeared to change over time.

Note that in all of these studies the relationship between unionism and productivity is estimated with the price-theoretic responses to higher wages held fixed. Thus, the higher productivity is <u>not</u> due to employers substituting higher quality labor or capital for the more expensive uncovered workers, at least to the extent that can be discerned with the existing measures of those variables. As a result, some effort has been devoted to determining the nonstandard routes which may underly the apparent union impact on productivity. One relevant finding is that roughly 25 percent of the union/nonunion productivity differential in the manufacturing sector can be explained by the union/nonunion differential in quit rates. Other evidence suggests that a significant piece of the union productivity effect can be explained by the union/nonunion differential in the quality of management practices.

The association of unionism and profitablity has been examined only recently, in part because, like labor quality and capital, profits are an extremely difficult variable to measure. While there is undoubtedly considerable variation across data sets, the available evidence suggests that in general unionism is associated with a lower return to capital. In some cases, the gross profit margin (profit as a percent of value added) is not noticeably different in union than nonunion sectors, but the capital-labor ratio is higher under trade unions, producing a lower return to capital. In other cases, the gross profit margin as well as the return to capital appears lower. That unionism does not always reduce profitability, however, is also apparent: Hayden found profits as well as wages in trucking rising after the Teamsters negotiated the first National Master Freight Agreement in 1964. All told, on the basis of existing and ongoing studies, it does appear that productivity under unionism is not sufficiently greater than productivity in nonunion settings to offset the higher compensation plus the higher capital intensity, which would be necessary if profits per unit of capital were to be left unaffected.

II. Econometric Probes into the Reality of the Non-wage Effects of Unions

As noted in Table 1, most studies of the union impact on factors other than the wage level try to control for the wage effect and potential price-theoretic responses to higher union wages, and have found that the bulk of the union/nonunion differences under discussion cannot be explained in terms of measurable price-theoretic variables. For instance, the substantial union/nonunion differential in quit probabilities exists even when individuals' wages and fringes are held constant. Or to choose another example, the union/nonunion productivity differentials discussed above were estimated with

models which controlled for labor quality and capital intensity.

Analysts in the industrial relations tradition interpret the existence of significant union effects above and beyond measured price-theoretic routes as real--reflecting the nature of the economy's basic institutions. These individuals believe that the key task for research on trade unions involves gaining a better understanding of the origins, operations, and interactions of the institutions, since the non price-theoretic actions of firms and unions matter greatly in determining economic performance.

Devotees of the standard price-theoretic model perceive the union/nonunion differences in nonwage variables quite differently: they see these results as illusory, due in part perhaps to inability to control adequately for preunion characteristics of workers or firms and/or to a failure to capture the relevant price theoretic routes due to the poor quality of the empirical experiment conducted. This point of view leads to the search for observed or unobserved differences which existed between individuals or firms before they were unionized or for important price-theoretic stimuli or responses which had not been captured by the models or with the data of concern and which could be causing the estimated union/nonunion differences. Since these alternative views, and a third view that even union wage effects are illusory, have been discussed in our earlier work,[2] we concentrate here only on the econometric efforts to evaluate the hypotheses.

At the outset, it is important to recognize that the econometric problems of concern occur because the observed union/nonunion differences do not come from the "ideal" experiment needed to estimate the effects of unions on economic outcomes. This experiment would involve unionizing a randomly chosen nonunion individual or firm, while holding all else of relevance in the world

fixed, and observing the resultant changes. Unfortunately, all of the statistical "experiments" conducted depart from the ideal, at least to some extent, for two reasons. First, all the relevant factors cannot be held perfectly fixed when we compare unionized individuals or firms to nonunion individuals or firms or even to themselves when they were nonunion. Second, it is unlikely that individuals or firms with similar measured characteristics became unionized on a random basis.

The absence of a series of ideal experiments sets the stage for the econometric efforts at probing the reality of the observed union/nonunion differences in nonwage variables, on which we focus next.

Potential Econometric Explanations and Assessments of Their Validity

> The real reason you have obtained those union/nonunion differences is that you have omitted (mismeasured, not observed) a key variable which is correlated with unionization, and that variable is...
>
> But you have the wrong causality. It is not that unionism causes...; it is that ...causes unionism.
>
> It seems obvious that your results are due to selectivity; there is an unobserved factor out there which affects whether or not workers are unionized and the market outcome of concern.
>
> -Frequently heard assertions at seminars throughout the country.

There are three key econometric problems than can arise in doing empirical work on the impact of unionism (or any factor) on economic outcomes: omitted, mismeasured, or unobserved variable bias; simultaneous equations bias; and sample selection bias. Each of these potential reasons why estimated union/nonunion differences might be spurious arises because of the aforementioned lack of an ideal experiment. These potential problems have been appealed to in attempts to explain the observed union/nonunion differentials depicted in Table 1. Those whose priors come from the price theoretic monopoly view have

used the three potential forms of bias to argue that the observed differentials in Table 1 are illusions.

There are various methods for dealing with each of the potential bias problems which arise in analyses of cross-sectional data. Heuristically, these methods can be divided into three broad categories: (1) Approaches which probe the cross-sectional results through various forms of "sensitivity" analysis designed to see how results might be "driven" by the poor quality of the experiment. In this category we include such techniques as: expanding the list of controls; using the omitted variable bias formula; imposing coefficients on mismeasured variables; and using the variance/covariance matrix of coefficients to examine the sensitivity of results to alleged experimental problems. Given outside information on, for example, the relationship between the omitted variable and included variables or on the degree of measurement error in the variables of concern or on the likely magnitude of selected coefficients, estimates can be made of the likely impact of omitted, mismeasured, or unobserved variables. By making particularly strong assumptions or picking particularly large (or small) values of the relevant correlation coefficients, one can "stack the deck" against the estimated union effect and thus get a good notion of its strength. (2) Techniques which seek to treat the alleged experimental problem through complex systems of equations in which both the relevant variables and/or their exact functional form are used to identify the "true" union impact. Such techniques can be used to deal with unobserved or mismeasured variables but are most commonly used to treat the simultaneous equations and sample selection problems. The methodology is to postulate a "true" model which enables one to deal with the alleged experimental problem and to solve the resultant equations

to obtain the coefficient of concern. (3) Approaches which seek to obtain new and better quality data designed specifically to deal with particular experimental problems, especially measurement error and omitted variables.

A very different approach to the three types of problems described is to apply a different experimental design to the problem of estimating union effects by examining longitudinal (before/after) rather than cross-sectional data. Longitudinal information provides what is perhaps the most direct way of dealing with the essential cross-sectional data problem--that we are comparing <u>different</u> people or firms rather than conducting the ideal experiment described earlier. If one obtains longitudinal data in which omitted, mismeasured, or unobserved variables are constant over time, one can obtain estimates of union effects purged of biases due to these problems. Similarly, by enabling us to compare outcome variables before and after unionization, such data provide the proper recursive structure for dealing with both the "union causes" versus "causes unionism" question and the problem of cross-sectional selectivity bias.

It is important to recognize, however, that longitudinal studies are themselves subject to potential experimental problems not unlike those with cross-sectional data. One potential difficulty is that when persons change jobs, other relevant variables are also likely to change, such as occupation or industry or tasks at work, which may be omitted, mismeasured or unobserved in the analysis. Another potential problem is that classical measurement error bias may become more severe because the systematic parts of variables are differenced away. Third, since only a limited number of persons are likely to change union status in a given period of time, longitudinal studies may be prone to a sample selection problem not unlike that in cross-sectional studies.

Longitudinal calculations reveal the effects of changing union status on the position of workers (firms) who change: if those persons (firms) differ in some fundamental way from other workers, the results may not generalize to the entire population. Whether the selectivity of union and nonunion changers is an important phenomenon and, if so, in what way it affects results are unclear *a priori*.3/

Recognizing the problems of longitudinal analyses does not of course vitiate the fact that before/after data provide a distinct and real set of potential experiments which can go a long way toward dealing with the potential difficulties with cross-sectional work discussed above. By following the same individual (firm) over time as he/she/it changes status from nonunion to union or vice versa, one is able to control in a more natural way for all missing or unobserved variables which do not change over time. The longitudinal data are an invaluable complementary form of information to the more widely used cross-sectional data.

Results of Econometric Probes

Much recent work on unionism has used the econometric techniques alluded to earlier to probe the union/nonunion differentials summarized in Table 1. What have been the results of these efforts to obtain better estimates of the "true" union effect on economic outcomes? To what extent are the Table 1 differences "moved" by sensitivity probes which use new data or information to evaluate the effect of omitted, mismeasured, or unobserved variables in a specified study? How sensitive are the empirical results to probes which rely on extensive cross-sectional modeling in which unionization is taken as endogenous, for reasons of either simultaneity or selectivity? What are the results of panel or

longitudinal studies designed to deal with the potential "experimental" problems with cross-sectional analyses? In short, what does the evidence say about possible ways of answering, and about possible answer to, the frequently heard seminar assertions regarding the potential problems with the cross-sectional investigations of the impact of collective bargaining?

Our review of the relevant econometric studies yields three conclusions. First, the econometric probes do not invalidate the findings summarized in Table 1 by attributing all or the vast bulk of observed differences to the inadequacies of the experimental comparisons. Studies which probe the sensitivity of cross-sectional findings to omitted, mismeasured or unobserved variables show that while these experimental problems appear to bias union coefficients somewhat, they are far from the sole explanation of the ordinary least squares regression results. Studies which use longitudinal data to deal with the problems of unobserved factors, simultaneity, or sample selectivity tend to yield lower estimates of union effects than do OLS studies using cross sectional information, but they also fail to eliminate the bulk of estimated impacts. Studies which seek additional data regarding the potential causality of union effects through surveys of firms also tend to find real union impacts on behavior.

Second, union/nonunion differences in the nonwage outcomes of concern appear to be no more affected, and in some instances, to be less affected by the absence of the perfect experimental data than are the union/nonunion differences in wages. Moreover, the relevant analyses suggest that both sets of differences are real. Furthermore, the studies examined imply that probes of the wage differential can be expected to shed much light on the likely outcome of probes of other differentials.

Third, studies which use systems of equations with cross-sectional data to "correct for" potential simultaneous equations and sample selection bias provide very little insight into whether the Table 1 union/nonunion differences are real or illusory. The models employed rely on "restrictions" or "exclusions" which are far from convincing. More importantly the results show great instability in the face of seemingly small changes in the model or the sample analyzed. In some cases the systems yield union effects much below those obtained with OLS; in others they yield effects much above those from OLS; and in yet others the systems of equations give about the same results as does OLS. In a surprisingly large number of cases, the systems yield results so implausible on a priori grounds as to be dismissed out of hand. While this instability and implausibility does not demonstrate that the OLS union/nonunion differences are unbiased, it does indicate that the system of equations methodology does not offer a reliable and useful way of improving on these estimates.

We consider next the evidence regarding these conclusions. We review first the results of efforts to probe cross-sectional findings with sensitivity analysis, better data designed to deal with omitted variables, and systems techniques. Then we review the growing body of evidence which uses longitudinal experiments to check on the cross-sectional findings.

Probing the Cross-sectional evidence

Table 2 summarizes some recent efforts to assess the validity of cross-sectional findings using one or more of the methods discussed in the preceding section. For each study the table shows: the type of bias being focused on, the econometric technique employed, the variable analyzed, the data used, the key empirical results, and the appropriate references. While our listing is

undoubtedly incomplete, we believe it is broadly representative of the pattern of results in extant work. Because of the initial concentration of quantitative analyses on wages, the table is top heavy with the results of econometric probes into the union wage effect.

The first and undoubtedly the most widely used technique for dealing with data inadequacies is to test the sensitivity of results to the inclusion of detailed industry or occupation controls in the data set under study. Addition of such controls in some sense leads to finer experiments by focusing on union effects within more detailed groupings. Alternatively, to the extent that missing or mismeasured variables differ across the relevant sectors, inclusion of a large number of variables can be justified by pointing out that they help control for those variables. Even when one might argue that exclusion of detailed controls is theoretically "correct", it is useful to know whether these variables "matter". In many studies attempts are made to obtain information on the posited missing variables at an industry level and to add those variables in place of the dummy controls. This provides a means of evaluating what industry dummies in fact stand for, but offers a weaker test of the extent to which results stand up to addition of numerous covariance controls.

In most cases in which additional controls are added to analyses, either by augmentation of data sets with industry-level variables or by inclusion of numerous industry or occupation dummy variables, the greater refinement of the comparison set reduces the estimated impact of unionism. But this occurs only up to the point of, say "1"- or 2-digit industry or occupation controls. Additional controls appear to have only a modest effect on the estimates. Consider, for example, the effect of adding industry controls to the equations estimating the effect of unionism on the usual hourly pay of private, male wage

and salary workers using 1976 May CPS data. With a standard log-linear hourly earnings functional form which includes race, years of education, age minus years of education minus six and its square, three region dummies, and a blue-collar dummy variable, the effect of adding industry controls on the estimated coefficient of the union membership dummy (member = 1) is shown below.

Industry Controls	Estimated Union Member Coefficient In May 1976 CPS (Standard Error)
None	.29 (.01)
1-digit Census (20)	.21 (.01)
2-digit Census (45)	.19 (.01)
3-digit Census (200)	.18 (.01)

As is common in such sensitivity probes, the reductions (in absolute value) in union coefficients approach zero very quickly as the number of industry dummies grows, and the estimated union/nonunion difference of concern does not vanish.

Addition of other variables designed to reflect union/nonunion comparisons by holding fixed work place conditions likely to cause compensating differentials yields similar results: union/nonunion wage differentials diminish but do not disappear. The most sizeable reduction, obtained by Duncan and Stafford (1980), showed that addition of variables relating to the nature and intensity of work to a ln (wage) equation reduced a union coefficient estimate of .29 to .19. Other studies by Brown (1980) and Leigh (1981), however, show no such relation between union/nonunion differentials and characteristics of work places.

There have been a limited number of studies which have sought to evaluate the effect of measurement error or omitted variables on estimated union/nonunion differentials. In their study of productivity, Brown and Medoff

Table 2

EVIDENCE OF ECONOMETRIC PROBES INTO UNION/NONUNION DIFFERENCES USING CROSS-SECTIONAL DATA[a]

Issue and Technique	Variable, Data Set, Sample	Result	Reference
Omitted, Mismeasured, or Unobserved Variable Bias			
Enter additional dummy variable(s), or other variables to obtain finer comparisons	Wages; quits; layoffs; dispersion; productivity; etc. Diverse Diverse	Addition of various dummies for 2- and where possible 3-digit Census or SIC industry or for occupation can reduce but not eliminate estimated union/nonunion differential; similar results from adding average characteristics using industry figures and from adding variables capturing work place characteristics.	Diverse studies.
Set coefficient on mismeasured variable at predetermined level	Productivity Census of Manufactures; CPS All workers in manufacturing industries	Union coefficient is reduced substantially by forcing estimated coefficient of capital/labor variable to equal an upper bound of capital's share of value added in Census of Manufactures data set, but still implies that unionized estalishments are moderately more productive (by a lower bound of 6%).	Brown & Medoff (1978).
Use omitted variable formula to discern likely bias	Quits CPS; PSID; NLS Older Men; NLS Younger Men; All Workers	Correcting for omitted fringe benefits variable and mismeasured alternate earnings variable can most likely reduce large union coefficient by no more than 1/4.	Freeman (1980b).
	Productivity Cement company data Production workers	Capturing true labor quality is unlikely to greatly reduce the union productivity effect in cement.	Clark (1980a, 1980b).
Collect new data	Productivity Cement company data; Underground bituminous coal mine data Production workers	Physical output data for cement plants and coal mines obtained to deal with problems of distinguishing output variation from price variation indicate that the fact that the earlier union productivity studies used a value measure cannot explain the estimated positive union effect; in addition, these data point to the importance of the quality of labor-management relations as a mediating factor in the union-productivity relationship.	Clark (1980a, 1980b), Connerton, Freeman, & Medoff (1979).

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EVIDENCE OF ECONOMETRIC PROBES INTO UNION/NONUNION DIFFERENCES USING CROSS-SECTIONAL DATA[a]

Issue and Technique	Variable, Data Set, Sample	Result	Reference
	Role of Seniority per se Surveys of companies All workers	Union/nonunion differences in the relationships between seniority and both terminations and promotions cannot be explained in terms of an unobserved union/nonunion differential in the relationship between seniority and contribution to firm.	Medoff & Abraham (1980b, 1981a).
Construct unobserved variable model	Fringes EEC Production Workers	Magnitude of union coefficient is sensitive to precise model for unobserved establishment characteristics, but qualitative conclusion that unions are associated with higher fringe benefits is not.	Freeman (1981).
Simultaneous Equations Bias			
Replace a union variable with a predicted union variable	Wages E&E; Census All workers	Union/nonunion wage differential declines for 49 manufacturing industries from 37% with OLS to 27% in a 2-SLS model.	Pencavel (1970).
	Wages E&E; Census All Workers	Union/nonunion wage differential declines for 2-digit SIC manufacturing industries from 46% OLS to 19% or 4% in 2-SLS models and to -9% in a 3-SLS model.	Ashenfelter Johnson (1972).
	Wages; quits E&E; Census Production workers	Both wage and quit differentials grow substantially (in absolute value) with data for 3-digit SIC manufacturing industries, when 2-SLS replaces OLS; the wage differential rises from 50 to 80%.	Kahn (1977).
	Wages SEO All workers	Wage differential reduced from 11% to 6% by fitting a system of equations in which the estimated union coefficient is unbiased by assumption.[b]	Schmidt & Strauss (197(Olsen (1978). Schmidt 1978
	Wages; quits NLS Young Men All workers	Wage differential rises with selectivity correction from 32% to 51%; differential in quit probability switches from significant negative (-.487) to near significant positive (.878).	Farber (1979).
	Wages NLS Young Men All workers	With selectivity adjustment, union differential rises from 22/38% to 28/105% for young and middle-aged black employees and from 25/13% to 37/46% for young and middle-aged white employees.	Leigh (1980b)

EVIDENCE OF ECONOMETRIC PROBES INTO UNION/NONUNION DIFFERENCES USING CROSS-SECTIONAL DATA[a]

Issue and Technique	Variable, Data Set, Sample	Result	Reference
	Wages CPS detailed occupational data Hospital workers	Results vary with data set and model, with estimated differentials moving, in many cases quite substantially, in both directions (presented in Table 4).	Cain et al, (1980); McLaughlin, (1980); Podgursky, (1980).
	Wages PSID: Michigan Time Use Survey All workers	Differential increases from 19% to 24% with union made endogenous on work conditions.	Duncan & Stafford (1980).
	Dispersion Aggregate Industry All workers	Union impact of reducing dispersion is not significantly affected by simultaneity adjustment.	Hirsch.
Sample Selection Bias Add an inverse Mills ratio term to outcome equation or estimate a system which explicitly recognizes correlation between selection and outcome equation.	Wages NLS Older Men All workers	Coefficients in separate union and nonunion equations are only moderately affected by addition of inverse Mills ratio; estimated union/nonunion differential rises moderately.	Duncan & Leigh (1980).
	Wages SEO Operatives	Sizeable wage differential declines slightly (from 18% to 16%) with selectivity correction.	Lee (1978).
	Wages PSID All workers	Estimated union wage differential rises significantly to 40% from its OLS value of 13% in one calculation and modestly in another (from 6% to 9%).	Neumann (1977).

TABLE 2 (cont.)

Issue and Technique	Variable, Data Set, Sample	Result	Reference
	Wages CPS detailed occupation data Hospital workers	Results vary with data set and model with estimated differentials moving, in many cases quite substantially, in both directions to large positive or large (in absolute value) negative (see Table 5 for specific results).	Cain et al, (1980), McLaughlin (1980) Podgursky, (1980).
	Wages CPS detailed occupation data Health care employees	Sizeable increases in wage differential for nurses aides (to 89%); moderate increases for health aides and technical workers; decline to -6% for nurses.	Feldman, Lee, and Hoffbeck (1980).
	Wages Survey of Hospital Directors of Nursing	Union coefficient in wage equation goes from insignificant positive (OLS) to insignificant negative (2-SLS).	Sloan & Elnicki (1979).
	Turnover Hospital survey data set Health care workers	Percentage reduction in turnover associated with unionism is large (50%) even when 2-SLS is used to correct for selectivity.	Becker (1978).

Notes: [a]The following abbreviations are used throughout this table and the remainder of the paper for data sources: CPS represents the Current Population Survey, E&E represents Employment and Earnings, EEC represents the Expenditure for Employee Compensation survey, NLS represents the National Longitudinal Survey, PSID represents the Panel Study of Income Dynamics survey, and SEO represents the Survey of Economic Opportunity, and for statistical techniques: OLS represents ordinary least squares, 2-SLS represents two-stage least squares, and 3-SLS represents three-stage least squares.

[b]This result is reported in the Schmidt response to Olsen's piece, which pointed out a flaw in the original Schmidt & Strauss model.

(1978) probed the extent to which the coefficient on unionism could be explained by classical measurement error in the capital/labor ratio by exploiting the fact that with the Cobb-Douglas production function, under profit maximization, the coefficient of this ratio should equal capital's share of value added. Because unionization and capital/labor ratio are positively correlated, they found that mismeasurement of the capital intensity variable may have substantially biased upward the estimated impact of unionism on productivity. However, even when the coefficient of the capital/labor variable was forced to equal an upper bound estimate of capital's share, there remained a nonnegligible positive union productivity effect. In a study of quits, Freeman (1980a, 1980b) used the omitted variable bias formula to assess the sensitivity of the apparent union effect on quits to the omission of fringe benefits from the analysis and to measurement error in alternative wages. The formula was applied using information from other data sets in conjunction with strong assumptions designed to yield lower bound estimates of the union effect. The lower bound estimates showed a significant and large effect about half as large as the initial OLS impact. In another study dealing with omitted variables Clark (1980a, 1980b) examined the likely effect of omitted labor quality on the union/nonunion productivity differential. Using a formula describing how labor quality enters the production process, and exogenous information on possible quality changes during the period since his sample of cement plants had gone from nonunion to union, he concluded that only a small piece of the differential he had originally estimated could be explained by this uncaptured work force dimension.

There have been some recent efforts to generate new data sets to deal with omitted or mismeasured variable problems. To determine whether union

effects on productivity, measured by value added, might be due to union effects on the price rather than the output component of value added, Connerton, Freeman, and Medoff gathered data on tons of coal, while Clark gathered data on tons of cement. The coal study found sizeable positive union productivity effects when industrial relations in the sector appeared to be good but negative effects in a period of seemingly poor industrial relations. The cement study found positive union effects on physical output per worker in that industry. To determine whether union/nonunion differentials in the extent to which seniority reduces the probability of termination and increases the chance of promotion could be explained by an unobserved union/nonunion differential in the relationships between company service and current contribution, Medoff and Abraham (1981b) asked companies to compare the termination and promotion probabilities of senior and junior employees whose performance was equal. Based on more than 500 responses, it was concluded that the greater importance attached to seniority per se under unionism could not be explained in terms of an uncaptured differential in the way performance and seniority were related.

Finally, the recently developed "unobservables" models (see Chamberlain, 1977) were used by Freeman (1981) to assess the possibility that part of the estimated union impact on fringes was due to an omitted firm characteristic. The analysis showed that the extent to which the OLS differential could be attributed to unobserved firm differences depended greatly on the way the model was constructed. When it was assumed that there was no within-firm spillover from blue-collar unionization to white-collar fringes, the original fringe differential was reduced substantially by the firm-effects correction. Hence, any conclusion concerning the impact of unobserved firm effects on the

union/nonunion fringe differential depends crucially on one's a priori logic concerning the "true" unobservable model to be used.

Simultaneous Equations

Several analysts have sought to explore the causality of observed union effects using simultaneous equations models in which unionism is endogenous, i.e., determined by the equations of the system. In the outcome equation(s) the actual union variable is replaced by a predicted variable. Identification of the system is obtained either by exclusion of one (or more) variables from the outcome equation, but not from the unionism equation, or on the basis of different functional forms for the two equations.

The first analyses using the simultaneous equations technique focused on industry aggregates. Both Ashenfelter and Johnson (1972) and Pencavel (1970) showed that, depending on the particular model employed, a large positive OLS union/nonunion wage differential in U.S. manufacturing was substantially reduced; Ashenfelter and Johnson estimated a differential of 46 percent with a single equation (OLS) model, a differential of 19 percent with one two-stage model, a differential of 4 percent with another two-stage model, and a differential of -8 percent with a three-stage model. The more recent work on manufacturing by Kahn (1977), who used 3-digit SIC data, whereas the previous researchers used 2-digit data, but followed the same general procedure, generated quite different results: substantial increases (in absolute value) in both the union wage and quit effects upon correcting for the endogeneity of unionism. Kahn's estimated wage differential rose from 50 to 80 percent when he changed his technique from OLS to two-stage least squares and his estimated quit effect also rose noticeably. In a later study, Kahn (1979) attempted to control

for union-induced increases in labor quality. Using his 3-digit SIC manufacturing data, he found a wage differential of about 50 percent with a two-stage model versus 25 percent with an OLS model. Hence, seemingly small changes in the models employed and in the degree of data aggregation have yielded very different results with systems designed to correct for potential simultaneous equations bias in analyses of aggregate cross-sectional data.

A widely divergent pattern of results has also been obtained when roughly similar simultaneous equations models have been estimated with similar bodies of individual-level data. Schmidt (1978), relying on functional form for identification, reported a decline in the effect of unionism from 10 percent to 4 percent with SEO data (his two equation model was not, however, needed to obtain unbiased estimates, since it assumed away the correlation that gives rise to the bias problem). On the other hand, Duncan and Stafford (1980) showed an increase in the estimated coefficient of unionism when unionism was made endogenous in their model which focused on work conditions, as did Leigh (1980a). Applying a simultaneous equations model with both a wage and a quit equation to the young men NLS data, Farber (1979) obtained an increase in the union wage effect while at the same time switching the sign on the standard quit effect from negative to positive, the opposite of Kahn's quit result. Farber found his results somewhat puzzling. Overall, in the regressions cited in Table 2 (including those from Cain, et al. presented in detail in Table 3), there is an alarming amount and pattern of instability when actual unionism is replaced by predicted unionism; in somewhat more than half the cases, the estimated union coefficient rises, counter to expectation, often to rather large values, while in many cases in which the coefficient declines it becomes negative.

"best" way to identify the systems of concern and the results obtained seem to be highly sensitive to the one chosen, as well as to the data and sample with which it is used. While the problems addressed by the techniques may be real, the econometric solutions offered can do little to solve them with extant cross-sectional data. Econometric manipulations of these data do not appear to be a good substitute for better data, for experiments more suitable to answering the problems of concern, or for genuine institutional or theoretical knowledge about better data, for experiments more suitable to answering the problems of concern, or for genuine institutional or theoretical knowledge about the interactions bewteen union, employers, and workers.

Longitudinal data

The results of some recent studies of union effects that exploit the before/after nature of longitudinal data sets to obtain estimates of the effect of unionism on the same person or firm are summarized in Table 4. These studies, which ask "How does the characteristic of a worker (firm) change when he/she (it) goes from union to nonunion status or vice versa?" yield estimates of union wage and non-wage effects which, while frequently smaller (in absolute value) than those obtained in comparable cross-sectional analyses, are always quite consistent with the cross-sectional findings. In contrast to the attempts to deal with the problem of causality and selectivity with systems of equations, in no case does a longitudinal analysis result "blow up". Finally, but perhaps most importantly for the present discussion, the non-wage union/nonunion differentials seem to be inexplicable in terms of unobserved (or observed) fixed price-theoretic or monopoly effects.

Table 3

EVIDENCE OF ECONOMETRIC PROBES INTO UNION/NONUNION DIFFERENCES USING CROSS-SECTIONAL DATA[a]

Issue and Technique	Variable, Data Set, Sample	Result	Reference
	Wages CPS detailed occupation data Hospital workers	Results vary with data set and model, with estimated differentials moving, in many cases quite substantially, in both directions (presented in Table 4).	Cain et al, (1980); McLaughlin (1980); Podgursky (1980).
	Wages PSID; Michigan Time Use Survey All workers	Differential increases from 19% to 24% with union made endogenous on work conditions.	Duncan & Stafford (1980).
Sample Selection Bias Add an inverse Mills ratio term to outcome equation or estimate a system which explicitly recognizes correlation between selection and outcome equation	Wages NLS Older Men All workers	Coefficients in separate union and nonunion equations are only moderately affected by addition of inverse Mills ratio; estimated union/nonunion differential rises moderately.	Duncan & Leigh (1980).
	Wages SEO Operatives	Sizeable wage differential declines slightly (from 18% to 16%) with selectivity correction.	Lee (1978).
	Wages PSID All workers	Estimated union wage differential rises significantly to 40% from its OLS value of 13% in one calculation and modestly in another (from 6% to 9%).	Neumann (1977).
	Wages CPS detailed occupation data Hospital workers	Results vary with data set and model with estimated differentials moving, in many cases quite substantially, in both directions to large positive or large (in absolute value) negative (see Table 5 for specific results).	Cain et al, (1980), McLaughlin (1980) Podgursky (1980).
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Issue and Technique	Variable, Data Set, Sample	Result	Reference
	Wages Survey of Hospital Directors of Nursing Nurses	Union coefficient in wage equation goes from insignificant positive (OLS) to insignificant negative (2-SLS).	Sloan & Elnicki (1979).
	Turnover Hospital survey data Health care workers	Percentage reduction in turnover associated with unionism is large (50%) even when 2-SLS is used to correct for selectivity.	Becker (1978).

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[b]This result is reported in the Schmidt response to Olsen's piece, which pointed out a flaw in the original Schmidt & Strauss model.

We conclude that the highly sensitive results obtained with both aggregate industry and individual-level data sets when unionism is "predicted" raise serious questions about the usefulness of the simultaneous equation methodology for analyses of what unions really do. The technique appears to be trying to squeeze out of the data more than the data contain; it does not, in our view, provide a reliable way of addressing the illusion/reality question.

Sample Selection

The recently popular technique for dealing with potential sample selection bias--adding an inverse Mills ratio term to outcome regressions, which corrects for the potential bias under certain assumptions (see Heckman, 1976)--has been used in a number of analyses of the union/nonunion wage differential. In the first such piece, Lee (1978), using exclusion of variables as well as functional form for identification, reduced slightly the OLS wage differential for operatives (from 17 to 16 percent) with data from the SEO. Leigh (1980b), fitting models very similar to those used by Lee, analyzed NLS data for both older and younger men. He found that wage differentials were increased, rather than decreased, by the selectivity adjustment in both samples. In several cases they were increased by extremely large amounts; in three of six sets he presented, the selectivity-adjusted percentages were at least three times as large as the OLS estimates. Another very substantial increase in estimated wage differentials was obtained by Neumann (1977); with PSID data for 1974 his adjusted estimate was 40 percent while his OLS estimate was 13 percent. However, when Neumann used average data for 1968-1974, the difference was much smaller: 9 percent versus 6 percent. Overall, the results from adding sample selectivity "correction" terms to wage regressions appear to be as unstable and divergent as

those obtained with simultaneous equations "corrections". Studies that differ only slightly in specification, data, or group covered show wide differences in the impact of the "corrections" on OLS results.5/

Work focusing on the wage differential in a given sector, hospitals, tends to confirm this judgment. Becker (1978) and Sloan and Elnicki (1979) found that selectivity adjustments reduced estimated union coefficients, whereas the results in Table 3 from Cain, et. al. (1980) and McLaughlin (1980) for various groups in this sector show as many increases as decreases in the union coefficient upon addition of the inverse Mills ratio to regressions using the same survey data and model. In yet another study, Feldman, Lee, and Hoffman (1980) obtained increases in the union wage effect for several occupations in the health sector but obtained decreases in the union wage effect for nurses when they corrected for selectivity.

Podgursky's (1980) work with the CPS files provides yet additional evidence which calls into question the usefulness of the inverse Mills ratio technique for analyses of union/nonunion differentials. In his work on private sector production workers, an initial positive OLS differential of 10 percent (significant at the .01 level) becomes a highly dubious negative 63 percent (again significant at the .01 level) when an inverse Mills ratio term is added to a wage equation.

What is one to make of the aberrant results obtained with the simultaneous equations (predicted unionism) technique and with the inverse Mills ratio technique for examining whether observed union/nonunion differences are real or illusory? We believe that the empirical results just presented strongly suggest that there is little to be learned from using either of the two techniques for analyzing the impact of unionism. Unfortunately, there seems to be no obvious

"best" way to identify the systems of concern and the results obtained seem to be highly sensitive to the one chosen, as well as to the data and sample with which it is used. While the problems addressed by the techniques may be real, the econometric solutions offered can do little to solve them with extant cross-sectional data. Econometric manipulations of these data do not appear to be a good substitute for better data, for experiments more suitable to answering the problems of concern, or for genuine institutional or theoretical knowledge about the interactions bewteen union, employers, and workers.

Longitudinal data

The results of some recent studies of union effects that exploit the before/after nature of longitudinal data sets to obtain estimates of the effect of unionism on the same person or firm are summarized in Table 4. These studies, which ask "How does the characteristic of a worker (firm) change when he/she (it) goes from union to nonunion status or vice versa?" yield estimates of union wage and non-wage effects which, while frequently smaller (in absolute value) than those obtained in comparable corss-sectional analyses, are always quite consistent with the cross-sectional findings. In contrast to the attempts to deal with the problem of causality and selectivity with systems of equations, in no case does a longitudinal analysis result "blow up". Finally, but perhaps most imortantly for the present discussion, the non-wage union/nonunion differentials seem to be inexplicable in terms of unobserved (or observed) fixed price-theoretic or monopoly effects.

As was the case with cross-sectional studies, there are still more longitudinal analyses of wage rates than of other outcomes of concern. The magnitude of the difference between longitudinal and cross-sectional estimates of union wage effects varies somewhat by study. Chamberlain found that the effect of unionism estimated with the longitudinal data in the young men NLS was about six-tenths as large as the effect estimated with cross-sectional data. Mincer found the longitudinal effect roughly two-thirds as large as the cross-sectional effect. Mellow's analysis of the May-May matched CPS tapes, by contrast, obtained a longitudinal effect that was about 40 percent of that estimated in CPS cross-sectional regressions. One possible explanation of the greater difference between the CPS results and other results is that in the CPS, unlike other surveys, workers do not typically respond for themselves, raising the possibility of greater measurement error in the union variable using the CPS than using the other surveys. As noted earlier, classical measurement error can be expected to become a more serious problem in longitudinal than in cross-sectional data. Finally, with respect to wages, Duncan and Stafford and Leigh have presented figures on the change in wages for workers who switch union status and those who remain union or nonunion. These figures, given in Table 4, provide several interesting comparisons which illuminate the nature of the longitudinal experiment. From them one can compare the wage changes of workers who were nonunion in the first period and became union members in the second period to the wage changes of workers who were nonunion in both periods or to the changes of those who began as members but left their unions or to the changes of workers who were unionized in both periods. A similar set of comparisons can also be made for workers who began as union members but left their union. Each comparison provides an answer to a different question concerning

the impact of unionism on wage rates. For present purposes, it suffices to note that in all relevant comparisons, the results in Table 4 show a substantial union wage impact of a magnitude somewhat smaller than, but consistent with, the Table 1 findings.

In each case, the results confirm the cross-sectional findings. Hence, they imply that the set of non-wage cross-sectional differentials discussed above cannot be explained in terms of unobserved differences in price theoretic or monopoly variables or in terms of any other constant person or firm differences.

A longitudinal analysis of quit behavior with PSID data produced estimated coefficients on the union variable roughly equal to those obtained in cross-sectional analyses. This finding is consistent with longitudinal analyses of quits, permanent separations and tenure on NLS data for men.

Longitudinal studies have also roughly replicated the cross-sectional differentials for fringes, dispersion, and productivity. In the case of fringes, Mincer (1981), using NLS data for working men, found estimates of union/nonunion differentials whose magnitudes differ only modestly from comparable cross-sectional estimates. With respect to dispersion, Freeman (in process), analysing young male blue-collar workers, observed that among those who moved from union to nonunion employment there was a substantial increase in wage dispersion while among those who moved in the opposite direction there was a significant decrease, with both changes in dispersion only modestly smaller (in absolute value) than the cross-sectional dispersion differential. Finally, with regard to productivity, Clark (1980a) found only a modest diminution in his estimated effect of unionism on productivity in the cement industry when he went to a before/after data file.[6]

TABLE 4: Evidence of Econometric Probes into Union/Nonunion Differences Using Longitudinal Data

Variable, Data Set, Sample	Result	Reference
Wages NLS Young Men All workers	Changes in wages from going union to nonunion (UN) as opposed to remaining union (UU) and of going nonunion to union (NU) as opposed to remaining nonunion (NN) are about six-tenths as large as the comparable cross-sectional differentials.	Chamberlain (1980).
Wages May CPS All workers	Wage Differential of about 8% in longitudinal analysis compared to 19% in cross-sectional analysis.	Mellow (1979).
Wages; Work Conditions PSID; Michigan Time Use Survey All workers	Change in wages; UN 7%; NU 55%; UU 33%; NN 40%; Estimated UN change in "choice of work" is positive while NU change is negative. Estimated UN changes in "freedom to increase work hours" is near zero while NU change is negative and substantially so in absolute value.	Duncan & Stafford (1980).
Wages; Work Conditions NLS Young Men All workers	Change in wages; UN 45%; NU 118%; UU 71%; NN 81%. Estimated UN change in "progress at work" is positive while NU change is negative. Estimated UN change in "job pace" is positive while NU change is zero.	Leigh (1980a).
Permanent Separations; Fringes; Wages PSID NLS Young and Old Men All workers	Men who join unions have sizeable reduction in quits; reduction related to union wage gains; joiners also obtain less training than nonunion workers; union wage effect of 20% among young reduced in half; results for old men less clear.	Mincer (1981).

TABLE 4 (cont.)

Variable, Data Set, Sample	Result	Reference
Quits PSID	Quit differential in longitudinal study is roughly the same as in comparable cross-section investigations.	Freeman (1978b).
Tenure; Dispersion NLS Young Men Blue-collar Workers	Tenure and dispersion differentials in longitudinal study are quite consistent with those from comparable cross-sectional analyses.	Freeman (1982).
Productivity Cement company data Production workers	NU change is approximately equal to the comparable cross-sectional differential.	Clark (1980a, 1980b).

Overall, the longitudinal analyses suggest that much of the cross-sectional union/nonunion differentials presented in Table 1 are "real" and cannot be explained in terms of price-theoretic or monopoly effects. Since, as noted earlier, it is likely that there are some potential problems with analyses which estimate union impacts by focusing on marginal as opposed to average workers, we endorse neither the longitudinal nor the cross-sectional results as the answer. However, the fact that they regularly point in the same direction--there is much more to unions than their monopoly power--is reassuring.

III. Conclusions

This paper has reviewed a significant body of evidence regarding the impact of trade unionism on economic performance and sought to evaluate antithetical views regarding whether estimated differences in nonwage outcomes between union and nonunion workers and firms are real effects which can be fully understood in terms of monopoly unionism. The review has yielded conclusions on both the substantive questions at hand and the methodologies which have been used to address their validity.

With respect to the reality of the differences in nonwage variables, the preponderance of evidence indicates that union effects estimated with cross-sectional data are real. This statement is based on econometric probes into the cross-sectional findings and analyses of longitudinal data sets. While both types of investigations have shown that cross-sectional union effects tend to be somewhat overstated, no effect has been explained away as due solely to the poor quality of the relevant econometric experiment. Moreover, since the effects of unions on nonwage outcomes come from models which generally hold fixed the level of wages and variables affected by wages, the evidence supports the view that

unions do much more than simply raise wages through their monopoly power.

Thus, our examination of the existing evidence indicates that the new facts about the impact of collective bargaining cannot be fully explained by monopoly unionism. For this reason, we have offered elsewhere our "collective voice/institutional response" view of trade unions.7/ While we feel that this view is of value for understanding the institution, it is our belief that other views which go beyond monopoly unionism can also play major roles in explaining what unions do.

With respect to methods for evaluating the quality of standard cross-sectional experiments, some techniques appear more useful than others. In particular, we have found that sensitivity analyses of single-equation results and longitudinal experiments provide valuable checks on cross-sectional findings while multiple-equations approaches produce results which are much too unstable to help resolve the questions of concern.

Our conclusions seem to have three messages for future research on trade unionism. First, the operating assumption that trade unions have important and real nonwage effects which cannot be explained in price-theoretic terms is strongly supported by the extant evidence. Second, the search for a valid answer to the question of what unions do should involve more than just manipulating existing data with sophisticated techniques; it should have at its heart the collection of new evidence concerning the functions and operations of trade unions and their interactions with firms and employees. Third, the illusion/reality question should be asked not only of empirical results on the impact of collective bargaining, but also of the efforts to probe these findings. In sum, monopoly unionism appears unable to explain much of the impact of collective bargaining, even with the assistance of econometric model builders.

Footnotes

1/ See Freeman and Medoff, (1981b).

2/ See Freeman and Medoff, (1981b).

3/ Several arguments can be advanced regarding the possible problems involved in infering union effects for the population from what happens to a sample of changers. To see the first, consider wages. To the extent that voluntary job changing is viewed as an investment in mobility, there is likely to be a tendency for both union and nonunion job changers to experience the same wage gains, as both would change only if they could earn the appropriate return. This would bias comparisons of the differences in the wage growth of union-to-nonunion and nonunion-to-union changers toward zero. One would most likely get better estimates by looking solely at changers who left their firm involuntarily for reasons unrelated to their individual actions (e.g., those whose firms went out of business). Another point is that observed wage changes of union-status changers depend on where the changers were in the relevant wage distribution. If union or nonunion changers came disproportionately from either end of the distribution of concern, the estimated wage changes would not reflect those that would result from a person selected at random.

4/ Specifically, counting the number of cases in Tables 2 and 3 in which actual unionism was replaced by predicted unionism shows eight instances in which union coefficients declined from OLS levels, four to negative values, and twelve in which the coefficient rose compared to OLS values, five of which reached levels in excess of .40 ln points.

5/ Specifically, counting the number of cases in Tables 2 and 3 in which a selectivity correction term was introduced shows ten instances in which union coefficients declined from OLS levels, six to negative values, and eleven in

which the coefficients rose compared to OLS values, two of which reached levels in excess of .40 ln points.

6/ Brown and Medoff (1978) gathered data by 2-digit industry for 1929 and 1953 to use with data on unionization in these two years found in Lewis (1963, pp. 289-290) in an effort to capture productivity before and after unionization. They regressed the change in ln (value added/labor) on the change in ln (captial/labor) and the change in fraction unionized. With only 20 observations they could not estimate the union productivity effect with any precision. The estimated coefficient on the change in fraction-unionized variable ranged from negative to positive depending on the data used and the assumptions made.

7/ See Freeman and Medoff, (1979) and (forthcoming, c).

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