THE IMPACT OF COLLECTIVE BARGAINING:
ILLUSION OR REALITY?

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ABSTRACT

This paper reviews a significant body of evidence regarding the impact
of trade unionism on economic performance and seeks to evaluate antithetical
views regarding whether estimated differences between union and nonunion
workers and firms represent: illusions created by poor experiments, real
effects explicable solely in price-theoretic terms, or real effects which
reflect the non wage-related dimensions of trade unions. The review yields
conclusions on both the substantive questions at hand and the methodologies
which have been used to address their validity.

With respect to the illusion/reality debate, the preponderance of
extant evidence indicates that union effects on a wide variety of economic
variables estimated with cross-sectional data are real. Moreover, since
the effects of unions on nonwage outcomes generally come from models which
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 as an
economic monopolist. While, in this study, we do not examine interpretations
of these nonwage effects, the effects represent an empirical foundation
for the "institutional" view of unionism, which is described in Section I.

With respect to methods for evaluating the quality of standard cross-
sectional experiments, some techniques appear more useful than others.
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 unstable to help resolve the questions of concern.

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In recent years there has been an outpouring of empirical studies on the impact of collective bargaining on the economy. While many of these analyses focus on the traditional question of wage determination under unionism, considerable effort has also been devoted to estimating the effect of the institution on other market outcomes. As a result of this work we have a large body of new evidence regarding differences between union and nonunion workers and union and nonunion enterprises along many dimensions.

Can the observed union/nonunion differences be explained primarily in terms of preunion characteristics of firms or individuals? Is it that all union/nonunion differences arise only because of the "union wage effect" and are observed only when one or more price-theoretic responses to this effect are being ignored? Or can it be that unions have important effects on the performance of our economic system through routes ignored in standard price theory?

There are a number of different positions on whether union effects are real or illusory. One belief is that the apparent union/nonunion differences are illusory because of the way trade unions were superimposed on various groupings of establishments or individuals. A second view is that unions have real effects on economic performance, but that all of these effects operate through price-theoretic routes; any effects which appear to be

inexplicable in terms of standard price theory are taken as illusory.
Finally, there is the perception that unions influence outcomes through
institutional channels and, in so doing, have important real nonwage
effects on our economy.

The preunion characteristics belief that apparent union/nonunion
differences are illusory seems to be held primarily by those who see the
world as close enough to satisfying the conditions of perfect competition
that, in the short run, unions are more of an epiphenomenon than a
substantive force.[1] While it is unlikely that anyone really believes that
every apparent union effect is an apparition, the preunion characteristics
view lies behind many attempts to explain away particular results suggesting
that unions have meaningful economic impacts.

Those whose vision of what unions do comes from standard price theory
tend to focus on what we have elsewhere called the "monopoly face" of
unionism and believe that every real effect of unions works through price-
theoretic channels.[2] Thus, these individuals tend to limit their focus to the size
and ramifications of "the union wage effect," treating any estimated union
effect which cannot be rationalized in terms of a price-theoretic response
to the wage effect as illusory, that is, as reflecting the poor quality of
the experiment at hand.

Those in the industrial relations tradition believe that unionism
influences outcomes primarily through what are often labelled "institutional
channels" (the "collective voice/institutional response face" in our earlier
work). While this group accepts the existence of important real price-
theoretic union effects, it believes in the reality of non price-theoretic

effects as well. In fact, the primary concern of researchers with an industrial relations world view is with the nonwage effects of collective bargaining.

This paper examines the arguments and empirical evidence concerning whether union/nonunion differences represent illusion or reality, defined in accordance with either the price-theoretic or institutional views. In it we seek to determine the extent to which the union/nonunion differences found in myriad market outcomes are: (1) illusory, explicable in terms of the degree of unionism among workers or firms with innately different characteristics; (2) real, working through price-theoretic routes of impact; and (3) real, working through institutional routes of impact.

While we recognize that to some extent we have set up artificial polar cases, and that no sensible researcher would be expected to rely solely on any one of the views for explaining all union/nonunion differences, we believe that the differences noted permeate much of the recent literature on unionism and that the "ideal types" provide a fruitful guide to understanding efforts to determine what unions in fact do.

The paper is divided into four sections. Section I summarizes the recent empirical findings about union/nonunion differences on which the interpretative debate focuses. The second section lays out the theoretical and econometric explanations of the observed union/nonunion differences that have been put forth by the various camps in illusion/reality debates. Section III provides a summary of the results of new studies which can be used to assess whether

the effects set out in Section I are best interpreted as illusory, real for price-theoretic reasons, or real for reasons that can be called "institutional." The final section presents a brief summary of our findings.

By way of anticipation, we reach two main conclusions. First, unions and collective bargaining have substantial real effects on diverse economic outcomes; union/nonunion differences appear to reflect much more than the poor quality of our econometric "experiments." Second, many of the real union effects are the result of institutional factors, which many economists have neglected in recent years; the price-theoretic view of reality seems to be much too narrow.

I. The Evidence In Question

It is important at the outset to lay out the union/nonunion differences about which illusion/reality interpretative questions have arisen. Accordingly, this section briefly summarizes the results of recent research concerning the impact of unionism on certain key aspects of the labor exchange. As a guide to the discussion, Table 1 gives the central findings of these studies categorized by the following substantive issues: compensation; internal and external mobility; work rules and environment; 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

Table 1

RECENT EVIDENCE ON UNION/NONUNION DIFFERENCES BASED ON CROSS-SECTIONAL DATA

Variable	Finding	Partial Listing of Relevant References
Compensation		
Wage Rates	All else (measurable) the same, union/nonunion hourly wage differential is between 10% and 20%.	Ashenfelter (1976), Freeman & Medoff (forthcoming a), Lewis (1980), Mellow (1981a), Oaxaca (1975), Welch (1980).
Fringes[a]	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), Hyclak (1979-1980), Plotnick (1981).
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).
Cyclical Responsiveness of Wage Rates[a]	Union wages are less responsive to labor market conditions than nonunion wages.	Ashenfelter (1976), Hammermesh (1972), Johnson (1981), Lewis (1963), Medoff (1979), Mitchell (1980a, 1980b), Parson (1968), Raisian (1979).
Determinants of Compensation Differential	Other things equal, the union compensation advantage is higher the greater the percent 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 (forthcoming a), Hayden (1977), Hendricks (1975), Kahn (1978), Kochan (1980), Lee (1978), Mellow (1981b), Weiss (1966).
Internal & External Mobility		
Promotions	Seniority independent of productivity is rewarded substantially more in promotion decisions among union members than among otherwise comparable nonunion employees.	Halasz (1980), Medoff & Abraham (1980b, 1981b), Yanker (1980).

Table 1

RECENT EVIDENCE ON UNION/NONUNION DIFFERENCES BASED ON CROSS-SECTIONAL DATA

Variable	Finding	Partial Listing of Relevant References
Internal & External Mobility (Continued)		
Quits[a]	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[a]	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	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 and Environment		
Rules[a]	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 (1979), 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[a]	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).
Worker Assessment of Jobs		
Satisfaction with Job Overall[a]	The stated level of overall job satisfaction is lower, but the wage	Borjas (1979), Freeman (1976, 1978a), Kochan & Helfman (1979), Mandelbaum (1980).

Table 1

RECENT EVIDENCE ON UNION/NONUNION DIFFERENCES BASED ON CROSS-SECTIONAL DATA

Variable	Finding	Partial Listing of Relevant References
Worker Assessment of Jobs (Continued)		
Satisfaction with Job Overall[a] (Continued)	gain required to induce a job change is higher for union members than for otherwise comparable employees who are not members.	
Evaluation of Rules and Conditions[a]	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 & 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[a]	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[b]	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), Clark (1980a), Connerton & Freeman & Medoff (1979), Frantz (1976).
Profitability[b]	While profit per unit of sales appears to be the same in similar union and nonunion manufacturing firms, the rate of profit per unit of capital appears to be lower under unionism.	Brown & Medoff (1978), Clark (1980a), Frantz (1976), Freeman & Medoff (forthcoming b).

Notes: a) Wages or total compensation was held constant in generating this finding.
         b) Variables reflecting price-theoretic responses were held constant as well as possible in generating this finding.

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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.

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. A comparison of the union wage effect by groups suggests larger impacts for black as opposed to white men, 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. In addition, substantial differences have been noted in the size of the differential by industry.

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 Compensation 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.

While a tremendous amount of effort was devoted in the past to studying union/nonunion differentials in wage rates, very little attention was devoted to analyzing union/nonunion differentials in fringe benefits. With the passage of time, this allocation of resources has become less defensible since the share of total compensation associated with voluntary fringes has been growing rapidly. In contrast to Rice's 1966 cross-industry analysis, which found no union effect on fringes, the recent studies cited in Table 1 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 collective 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.

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.

Since their inception, unions in our country have been concerned with the structure 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 metropoliton statistical areas (SMSA's) 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, with respect to wage adjustments under varying economic conditions, recent analyses of cyclical variation in wage rates have 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 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 1970's to a point where it is roughly comparable to its level in the 1930's.

Internal and External Mobility

The new work on unionism has, as noted earlier, expanded the set of

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outcomes under study. One of the most important topics receiving attention has been 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 essential 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 diverse factors (including wages) 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 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, temporary layoffs tend to be used instead of average hours reductions to a greater extent under unionism. Hence, it appears 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 co-workers 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 and Environment

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 myriad 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.

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.

Workers' Assessment of Jobs

Several recent studies examining the impact of unionism on the stated
job satisfaction of workers have found union workers expressing less satis-
faction, or in some instances no more satisfaction, with their jobs than
similar nonunion workers, 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 in-
creases 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.

There have been a number of recent studies which have attempted 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 24 percent higher productivity than otherwise comparable nonunion establishments within the same 2-digit SIC industries. Studies of particular manufacturing industries--wooden household furniture and cement--have also found a positive productivity differential. Allen reports sizeable differences 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 25 percent more productive than comparable nonunion mines in 1965, but 20 percent 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.

Some effort has been devoted to explaining the routes underlying 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 profitability has been examined only recently, in part because, like labor quality and capital, profits are an extremely difficult variable to measure. What the available evidence does suggest is that while the gross profit margin (profit as a percentage of the value of output) is no different in unionized firms than in similar nonunion firms, the rate of return on capital is lower in unionized settings. Thus, it appears 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. Conceptual Explanations of the Estimated Differences

Consider the union/nonunion differences in economic outcomes presented in Table 1. How can each finding be explained? Which conceptualization of markets and unions is most consistent with the observed differences? How can alternative theoretical perspectives be analyzed empirically?

In this section we examine the arguments underlying the three potential explanations of union/nonunion differences set out at the beginning of the paper: (1) the "preunion characteristics" explanation under which union differences are "pure illusion," explicable by the unique characteristics of organized workers or firms; (2) the "price-theoretic" explanation, under which unions increase the relative compensation of their members through their monopoly power, eliciting in turn certain price-theoretic responses which explain all

other differences; (3) the "institutional response" explanation, under which unions have, along with their price-theoretic effects, important nonwage effects.

The Preunion Characteristics Explanation

In many discussions of estimated associations between unionization and other variables, we have heard that observed union/nonunion differences only reflect the preunion characteristics of workers or firms. As the column headed "Potential Explanation" in Table 2 indicates, the preunion character-istics under discussion are work force quality, the preference structures of workers, firm quality, and the production functions of firms. As the table's second column portrays, these preunion characteristics have been offered as explanations of virtually every union/nonunion difference shown in Table 1. While not a necessary condition for the view, those who offer the preunion characteristics explanation of union/nonunion differences generally assume that there are no excess profits or quasi-rents in the economy and, hence, that systematic differences in wages or other cost-affecting factors among competing establishments in the same product market cannot exist. If they did, the enterprise with higher (lower) costs would be driven out of business (dominate the market). Since observed differences cannot reflect cost differences, they must be offset by counterbalancing forces in the form of differences in (observed or unobserved) worker or enterprise characteristics. Under this view, the commonly observed union wage advantage would be attributed to unobserved aspects of labor or firm quality or unobserved differences in work conditions, which require compensating differentials of the classic type; if union workers are paid more, then

Table 2

THEORETICAL EXPLANATIONS OF OBSERVED UNION/NONUNION DIFFERENCES

POTENTIAL EXPLANATION	OFFERED, FOR EXAMPLE, TO EXPLAIN OBSERVED OR HYPOTHESIZED DIFFERENCES IN:
Preunion Characteristics	
Quality of work force	Wage rates; fringes; wage dispersion; quit rates; productivity.
Workers' tastes	Fringes; cyclical responsiveness of wage rates; role of seniority in promotions and terminations and other rules; quit rates; temporary layoffs; physical working conditions; job satisfaction.
Quality of firms	Wage rates; fringes; quit rates; certain rules; management practices; management flexibility; productivity; profitability.
Firms' technologies	Physical working conditions; productivity.
Price-Theoretic Responses	
Substitution between workers of different quality	Cross-market union compensation differentials; productivity; profitability.
Substitution between capital (or other nonlabor inputs) and labor	Cross-market union compensation differentials; productivity; profitability.
Substitution between compensation and non-compensation quality of jobs	Wage rates; fringes; cyclical responsiveness of wage rates; temporary layoffs; rules; physical working conditions.
Postunion sorting	Postunion quality of work force; postunion workers' tastes (and, thus, the differences listed above under preunion quality of work force and preunion workers' tastes).
Survival of firms	Postunion quality of firms; postunion firms' technologies (and, thus, the differences listed above under preunion quality of firms and preunion firms' technologies).
Institutional Responses	
New means for preference revelation and aggregation	Fringes; wage dispersion; cyclical responsiveness of wage rates; quit rates; role of seniority in promotions and terminations and other rules; temporary layoffs; physical working conditions; job satisfaction; management practices; productivity; profitability.
More channels for complaints, suggestions, and other relevant information	Quits; job satisfaction; productivity; profitability.
Increased pressure for cost-minimizing behavior	Productivity; profitability.

their pay "advantage" solely reflects compensation for more human capital or less desirable nonwage dimensions of the job.

Column 2 in Table 2 reveals the following logical problem with using the preunion characteristics argument to explain a large number of union/ nonunion differences: in several instances the analysis leads to contradictory statements about the direction of the differences prior to unionization. For example, one way of explaining lower quit rates at unionized firms is to posit that union workers quit less than nonunion workers at given wages because they are not as potentially productive in the outside market as otherwise comparable nonunion employees. However, this runs counter to the preunion characteristics explanation of higher union wages and productivity, that unionized workers are innately better. Similarly, if, with wages fixed, unionized workers quit less, a logical deduction might be that, for whatever reason, unions happen to be located at better plants. However, this runs counter to the compensating differential explanation of the union/nonunion wage differential, that unionized work places are worse than those that are nonunion. Because preunion characteristics arguments are usually invoked to explain observed differences in outcomes one at a time, these logical problems have not received adequate consideration among adherents of the preunion characteristics view of the set of observed union/nonunion differences.

The preunion characteristics analysis runs into a second logical problem with respect to the mechanism by which unionized workers or firms can be expected to have any particular set of preunion characteristics. Put most strongly, if unions have no real effects on economic variables, there

is no reason to expect them to locate or survive among workers or firms with any given characteristic. This suggests that, by itself, an explanation of all observed union/nonunion differences in terms of differences in preunion characteristics of workers or firms is exceedingly tenuous; it relies on post hoc ergo propter hoc arguments about the locus of unionism. It could be that unions happen to organize high wage workers or firms, low quit employees, high productivity firms, work places with bad conditions, and so on, without having any real economic effects. However, there is, as far as we know, no mechanism that would produce such a locus of organization under the null hypothesis that unions have no real effect on any of the specified variables.

Finally, there are two questions which must be asked of those who hold the preunion characteristics view of all observed union/nonunion differences: "If all union effects are illusory, why do workers join unions?" and "Why do employers oppose them (in many cases with vigor)?"

The Price-Theoretic (Monopoly Face) Explanation

In the monopoly model of unionism, unions are assumed to raise wages above competitive levels in the organized sector. This creates higher costs of production in organized firms. How can these firms survive?

One possible answer to this question is that unions organize the entire relevant product market so that unionized firms face no nonunion competition. If production costs are higher for all establishments in a sector, the sector's output and employment will be lower than in the absence of unionism, but the sector will survive.

Another possible explanation is based on the assertions that product

markets are not perfectly competitive and/or that firms in a given industry

have different cost structures (for reasons unassociated with unionism), which

permit companies to make excess profits and/or quasi-rents. In this world,

even if labor costs per effective labor unit rose as a result of unionism,

firms could remain in business. As long as the increased unit labor costs

came at the expense of excess profits and/or quasi-rents, but not at the

expense of normal profits, the enterprises would not go out of business.

Thus, this view stresses the idea that unions are likely to survive where

we find limited entry possibilities for firms, where entry takes time, and/

or where some enterprises have cost advantages over their competitors.[3]

The price-theoretic arguments just given imply that unions are likely

to be located where their gains induce small price-theoretic responses in

the form of either factor or product market substitution; i.e., where the

demand elasticity for labor is relatively low. It is likely, in turn, that

the comparisons of union/nonunion differences overstate, to some extent at

least, the potential impact of unionism on currently unorganized labor, inasmuch as

the set of price-theoretic responses relevant to today's unorganized are

likely to be larger than the responses relevant to those who became unionized

in the past.

While the monopoly model of unionism sees union/nonunion differences

which are explicable in price-theoretic terms as real, there is one variant

of the model under which the differences border on being labelled "illusory."

This variant relies heavily on the "postunion sorting" of workers with

different innate abilities and tastes. It grants the possibility that

unions can cause wages to rise but then adds that these increases will be met by essentially costless offsetting responses in labor quality or work conditions. For the labor quality response to accomplish the offset, the elasticity of substitution between different groups of workers must be infinite over the period of concern. If so, an initial union wage effect, defined in terms of an average labor unit working at a work place with average noncompensation characteristics, will lead firms to substitute higher quality for lower quality labor until the entire initial wage effect is eliminated, and thus will bring about a sorting of workers in terms of their quality. For the work conditions response to effect the offset, the firm must be able costlessly to let work place quality deteriorate. If so, the firm would let work conditions worsen until the entire wage advantage has been "paid for" by poorer nonwage job characteristics. In this world, when the dust settles, the only real observed differences between union and otherwise comparable nonunion firms will be a difference in the uncorrected (for differences in labor quality and the quality of work conditions) wage differential, a difference in work force quality, a difference in the quality of work conditions, and a difference in any ratio defined in terms of non quality-corrected labor units.[4]

Analogous responses by employers can also turn apparent union nonwage effects into a mere sorting of workers by tastes. Consider, for example, what might happen if unions were to raise fringes relative to wages in an establishment. Workers with strong preferences for fringes, who might be expected to seek those fringes in their current firms, would seek employment in the union sector. Unionized firms would attract fringe lovers, and would devote a larger share of each compensation dollar to fringe benefits than would

other firms, even though the aggregate expenditures on fringes might be unchanged in the economy.

In short, in this framework unions can have real immediate effects; but these effects are posited to erode away through essentially costless adjustments so that any differences observed over a longer run might properly be called "illusory."

The Institutional Response Explanation

Economists in the industrial relations tradition view the observed union/nonunion differences from yet another perspective: in terms of "institutional responses" to union-induced changes.

The institutional response explanation differs from those considered earlier in that it is based on the premise that, for a variety of reasons, the economy differs from the Pareto optimal world in which only standard price-theoretic responses are possible. Several reasons are often cited for such divergence between the real world and the economic model. First, while many firms (individuals) seek to maximize profits (utility), they do not achieve the optimum in the relevant period. Second, there are important public goods and externalities which are found, among other places, where people work. Third, there are important barriers to exit and entry in external and internal labor markets and other markets of less immediate concern. Fourth, there is imperfect information in internal and external labor markets and in other markets as well. Fifth, firms making excess profits and quasi-rents are found in many industries. Sixth, some firms (individuals) may be satisfied with less than an optimum, as emphasized by Simon (1955)

in his model of "satisficing" behavior. In various related guises this view of the world has been the basis of a number of views about the labor exchange, such as: the internal labor market view (Doeringer and Piore (1971)), the X-efficiency view (Leibenstein (1966)), the idiosyncratic exchange view (Williamson, Wachter, and Harris (1975)), and the collective voice/ institutional response view. What is most important about these views is that they suggest that understanding the effects of trade unions requires analyses of, among other things, the internal operations of firms, relevant organizational issues, the provision of various public goods at work places, and the quality and quantity of information flows.

The belief that labor in internal and external labor markets is, at least to some extent, immobile is very important to the institutional response view. In a world where there is not perfect exit and entry in labor markets and workers cannot express fully their preferences by "voting with their feet," there are opportunities for unions to influence economic be-havior through means not generally recognized in standard price-theoretic analyses and possibly to increase efficiency and social welfare as well.

In light of the discussion of labor immobility, it should be noted that a union can increase both the rate of compensation and the number of its members if it organizes firms with monopsony power. While monopsony is usually presented as an extremely rare event in labor economic texts -- "Today we have very few one company towns" -- the tendency for workers to be tied to the same firm for many years (for whatever reasons) is consistent with the belief that elements of monopsony are omnipresent. The point is that as long as firms face upward sloping labor supply curves, which is

quite likely given the costs to employees of switching jobs (embodied in unvested pensions, rights accruing with seniority, etc.,) and to employers of finding and training new employees, the firm can act like a monopsonist (equating marginal revenue product with the marginal, as opposed to the average, cost of labor). This creates a situation in which unionism can increase employment and social welfare by raising wages (see Viscusi (1980)).

The Price-Theoretic/Institutional Debate Over What Is Real

Most researchers try to control for the potential price-theoretic routes of union impact in estimating union/nonunion differences in nonwage variables. Hence, as indicated by the notes to Table 1, many 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 quite differently; they see these differences as illusory--
the result of the poor quality of the empirical experiments which can be
conducted. In their eyes, nonwage union/nonunion differences are only
observed because the relevant price-theoretic routes have not been captured.

In sum, there is a clear and very important split among those who
believe that unions have some important real impacts: one group
says that only the wage matters; the other says that while the wage matters,
it most certainly is not all that matters.

From the Conceptual to the Econometric

Each of the conceptual views just discussed has implications for how
one would approach observed union/nonunion differences econometrically.
The preunion characteristics perspective would push us toward searching
for observed or unobserved differences which existed between individuals or
firms before they were unionized. Alternatively, the price-theoretic view would
lead us toward searching for important price-theoretic stimuli or responses which had
not been captured by our models or with our data and which could be causing the
estimated union/nonunion differences. Finally, the institutional response framework
would encourage us to seek important institutional factors with the potential to
explain estimated union impacts not explicable in terms of measurable price-
theoretic variables. This section provides a general nontechnical summary of
the potential econometric issues which arise in empirical work on unions and
the ways the issues are approached.

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 (at this point in time) 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, most of the statistical "experiments" we conduct depart from the ideal for two reasons. First, we cannot hold all the relevant factors perfectly fixed when we compare unionized individuals or firms to themselves when they were nonunion. Second, it is unlikely that individuals or firms with similar measured characteristics became unionized on a random basis.

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 that 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 preunion characteristics view have used the three potential forms of bias to argue that the observed differentials in Table 1 are illusions. Those whose priors are based on the price-theoretic view have used the biases to offer econometric explanations of union/ nonunion differentials observed after measurable price-theoretic effects have been netted out. In contrast, those in the institutional response camp tend to believe that the observed relationships between unionism and other variables are real and merit further investigation concerning their existence and locus across specified institutional settings.

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) Tech-

niques 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 different people or firms rather than conducting the ideal experiment

described earlier. If one obtains longitudinal data in which omitted, mis-

measured, 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 who change: if those persons 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.[5]

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 or firm over time as 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.

III. Illusion or Reality? 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 studies? In short, what does the evidence say about possible ways of answering, and about possible answers to, the frequently heard seminar assertions regarding the potential problems with the cross-sectional analyses of the impact of collective bargaining?

In this section we review the relevant econometric studies which address these important questions. Our review yields the following two key 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, 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. Moreover, the results show great instability in the face of seemingly small changes in the model or the sample analyzed. In some cases these techniques yield union effects much below those obtained with OLS; in others they yield effects much above those from OLS; in yet others the systems of equations give about the same results as does OLS. In a surprisingly large number of cases, these techniques 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 two conclusions. We review first the results of efforts to probe cross-sectional findings with sensitivity analysis, better data designed to deal woth 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 3 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.

Table 3

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 establishments 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).

Table 3

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 (1976), 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).

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).
	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).

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 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).

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.

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.

-40-

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 (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 the OLS estimate of the coefficient was below capital's share and 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 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 sizable positive union productivity effects when industrial relations in the sector were good but negative effects in a period of 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 561 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. However, when a within-firm spillover was allowed, which seems to be the more reasonable assumption, the original differential was not lowered by the 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 in 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 per-
cent 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. 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 3 (including those from Cain, et al. presented in detail in Table 4), 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.

While most authors have not discussed the sensitivity of their findings to minor changes in specification, the statements of those who have indicate that the instability discussed above is not a purely cross-researcher phenomenom, since a given individual working with a given data set appears likely to find that slight changes in specification lead to large changes in results. For example, Duncan and Stafford(1980, p. 367) wrote that "the estimated union coefficient [is] sensitive to the exogenous variables omitted from the [wage] equation." Similarly, Mitchell (1980, p. 204) stated: "In general simultaneous-equation estimates require assumptions concerning which variables are exogenous and which serve to identify particular equations.

Table 4

RESULTS OF "WISCONSIN" REGRESSIONS WITH CPS DATA IN WHICH UNIONISM IS TREATED AS A PREDICTED
ENDOGENOUS VARIABLE OR IN WHICH THERE IS A SAMPLE SELECTION TERM

Group	OLS Union Effect	Union Effect With Predicted Unionism	Union Effect With Inverse Mills Ratio
Cain, et al. (1980)[b]	Percentage Wage Differential[a] (level of significance)		
Hospital employees			
-privated nonprofessional workers	6 (.10)	64 (.06)	31 (.05)
-private, registered nurses	-15 (>.10)	16 (>.10)	-6 (>.10)
-government, nonprofessional workers	-3 (.07)	95 (.06)	16 (>.10)
-private, technicians	-20 (.10)	31 (>.10)	4 (>.10)
-government, registered nurses	3 (>.10)	-24 (>.10)	1 (>.10)
-private, licensed practical nurses	16 (>.10)	13 (>.10)	28 (>.10)
-government, technicians	-10 (>.10)	-84 (>.10)	-72 (>.10)
-government, licensed practical nurses	20 (>.10)	55 (.07)	21 (>.10)
McLaughlin (1980)[c]	Percentage Wage Differential[a] (t-statistic)		
Hospital employees			
-private nonprofessional workers	6 (1.74)	36 (1.69)	22 (1.30)
-private, registered nurses	3 (.66)	16 (.46)	6 (.22)
-government, nonprofessional workers	4 (1.41)	5 (.07)	-22 (.51)
-government, registered nurses	0 (.00)	-57 (1.37)	-27 (.77)
Podgursky (1980)[d]			
Private sector Production workers	10 (3.0)	-72 (4.3)	-63 (4.0)

Table 4

(Continued)

RESULTS OF "WISCONSIN" REGRESSIONS WITH CPS DATA IN WHICH UNIONISM IS TREATED AS A PREDICTED ENDOGENOUS VARIABLE OR IN WHICH THERE IS A SAMPLE SELECTION TERM

ɔtes:

[a]These differentials give the estimated percentage amount by which the wages of union members exceed those of otherwise comparable nonunion employees. Although the data were transformed where necessary to yield differentials, the original t-statistic or level of significance (depending on what the author presented) is given.

[b]The data set used to derive these estimates is a pooled file of 1973-1976 May CPS microdata. The dependent variable for each occupational group in the particular government/nongovernment sector was the real hourly wage rates of individual hospital workers. Regressors in the OLS Union Effect model included a zero-one union status dummy variable as well as a vector of personal characteristics, region of country, size of SMSA, year, and sub-occupation group. In the Union-Effect-With-Predicted-Unionism model, predicted union status (provided by a probit computation) replaced the zero-one union status variable. In the Union-Effect-With-Inverse-Mills-Ratio model, the hazard ratio was added as a regressor to the OLS Union Effect model. Interactions of the union status dummy variable with variables for race, year, and full-time/part-time status were included in each model. The significance level refers to the combined effect of the set of union and union-interaction variables.

[c]The data set used and the variables included in the models are essentially the same as in note b above, except that part-time workers were excluded. The only important difference in the specification is that interactions terms were not included as additional regressors in the McLaughlin regressions. In fact, the McLaughlin results are virtually identical to the Cain, et al. specifications without the interaction terms.

[d]The data set used was the March, 1971 CPS. The dependent variable in these regressions is the log of annual earnings of full-time, full-year, nonfarm, private sector production workers. In addition to a zero-one union status dummy variable and a percent-of-industry unionized variable, regressors included a vector of personal characteristics, region of country, size of SMSA, industry, and industrial concentration.

Experiments by this author suggest that changing assumptions can produce wide variations in results ranging from negative union wage effects to ridiculously large positive effects."

Perhaps the most far reaching work on the stability of models which replace a union variable by a predicted value, in the context of a model in which unionism is taken as endogenous, has been done at the University of Wisconsin by Cain, et al. (1980), McLaughlin (1980), and Podgursky (1980). Their findings for the wage differential, summarized in Table 4, show that the same simultaneous equations model, estimated with data for comparable employee groups, yields results which swing back and forth over a highly implausible range (from -84 percent to 95 percent).[6] The Podgursky results, which show the union/nonunion wage differential swinging from a positive 10 percent with OLS to a most certainly absurd negative 72 percent using a two-stage least squares procedure, are particularly striking as they relate to one of the groups most frequently studied in the literature.

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.[7]

Work focusing on the wage differential in a given sector, hospitals, also does not yield stable or seemingly plausible results. Becker (1978) and Sloan and Elnicki (1979) found that selectivity adjustments reduced estimated union coefficients, whereas the results in Table 4 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 simulta-
neous 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 between 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 5. As before, there are more results on wage rates than on other outcomes of concern. The wage studies, which ask "How does the wage of a worker change when he/she goes from union to nonunion status or vice versa?" yield estimates of union wage effects which, while lower than those obtained in comparable cross-sectional analyses, are of sizeable and significant magnitude, supporting the claim that unionism does indeed raise the wages of individuals. 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."

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 in the 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 5, 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 5

show a substantial union wage impact of a magnitude somewhat smaller than,

but consistent with, the Table 1 findings.

Longitudinal data have also been used to study the union effect on

quits, productivity, and work conditions. With respect to quits and producti-

vity, the results confirm the cross-sectional findings. Longitudinal

analysis of quit behavior in a pooled PSID sample yields estimated coeffi-

cients on the union variable roughly equal to those obtained in cross-sectional

analyses, indicating that the lower quits of union workers cannot be explained

TABLE 5

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 non-union (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)
Productivity Cement company data Production workers	NU change is roughly the same as the comparable cross-sectional differential.	Clark (1980a, 1980b)
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)
Quits PSID All workers	Quit differential in longitudinal study is roughly the same as in comparable cross-sectional studies.	Freeman (1978b)
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 pressures" is negative while NU change is positive. Estimated UN change in "job pace" is positive while NU change is zero.	Leigh (1980a)

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Table 5

(Continued)

VARIABLE, DATA SET, SAMPLE	RESULT	REFERENCE
Wages May CPS All workers	Wage differential of about 8% in longitudinal analysis compared to 19% in cross-sectional analysis.	Mellow (1979)
Wages PSID; NLS Men	UN and NU changes are about two-thirds as large as the comparable cross-sectional differential.	Mincer (1980)

by an unobservable variable labeled "innate propensity to quit." 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 sample.[8] With respect to work conditions,

the results are somewhat less clear. Duncan and Stafford (1980) found a

decline (increase) in the quality of certain work conditions when workers

joined (left) unions in the PSID whereas Leigh (1980a) found no such

effects in the NLS.

Overall, the longitudinal analyses suggest that much of the cross-

sectional union/nonunion differentials presented in Table 1 are real rather

than illusory. 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 is reassuring.

IV. 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 between union and

nonunion workers and firms represent: illusions created by poor experiments, real

effects explicable solely in price-theoretic terms, or real effects which

reflect the nonwage—related dimensions of trade unions. 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 illusion/reality debate, the preponderance of evidence indicates that union effects on a wide variety of economic variables estimated with cross-sectional data are real. Diverse econometric probes into these findings and examination of longitudinal as well as cross-sectional data have supported the reality of the union impact on economic performance. While magnitudes of coefficients have been altered by the probes, in almost no case has the evidence been explained away as due solely to the poor quality of the relevant econometric experiments. Moreover, since the effects of unions on nonwage outcomes generally come from models which 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 as an economic monopolist. While, in this study, we have not examined interpretations of these nonwage effects, the effects represent an empirical foundation for the institutional view of unionism described in Section I.

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 wage and nonwage effects 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 used to probe these findings.

[1] Those who hold this view generally also explicitly or implicitly assume that there are no quasi-rents due to cost advantages unassociated with unionization, as discussed at greater length on p. 19 below.

[2] See Freeman and Medoff (1979).

[3] A related possibility is that unions organize enough new firms in a competitive industry to offset the demise of existing firms whose costs were raised by unionism. In this scenario, the relevant capital market condition is that firms earn a normal _expected_ rate of return over their lifetimes.

[4] We ignore the case of an infinite elasticity of substitution between labor and other factors, since this would imply that we would not observe unionized work forces to compare with those which are nonunion.

[5] 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 percentage 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 fell in the relevant wage distribution. If union or nonunion changers came disproportionately from either end of the distribution of concern, the estimated wages changes would not reflect the overall mean differential.

[6] Specifically, counting the number of cases in Tables 3 and 4 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 percent.

[7] Specifically, counting the number of cases in Tables 3 and 4 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 percent.

[8] Brown and Medoff 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 (capital/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.

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