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WHAT DO UNIONS DO?
THE 2004 M-BRANE STRINGTWISTER EDITION

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ABSTRACT

The Journal of Labor Research 20th anniversary symposium review of What Do Unions Do? offers a unique opportunity to examine how the claims made in the book have fared in ensuing research and to ponder what parts of the book I would change if I could. This paper responds to the 18 critical essays in the journal. It recognizes three major errors of omission: failure to take account of unionism outside the US; failure to analyze public sector unionism; and failure to analyze the effects of unionism on economic growth; and the problem of determining the "optimal level of unionism" on the basis of estimates of what unions do. Ensuing research has found that What Do Unions Do? correctly identified union effects on turnover, fringe benefits, earnings inequality, political action, profits, managerial flexibility and human resource management, and that wage effects vary widely. Estimates of the union effect on productivity tend to be positive but modest, ruling out negative effects on average, but not conclusively establishing positive effects. Critical comments from some of the symposium panelists notwithstanding, I believe that the bulk of the evidence supports the What Do Unions Do? claim that management opposition has been a major factor in the decline in union density in the US.

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I. *Introduction*

When Bruce Kaufman first told me that he was organizing a 20th anniversary review of What Do Unions Do? in which researchers would assess the book's findings and arguments in light of ensuing research and events, I was both honored and worried. Honored that he and so many other labor experts felt that the book was sufficiently durable to merit re-assessment and criticism. Worried that some of the criticisms might be harsh. Our knowledge of unions has advanced over 20 years. The economy and unions have changed over the period. I know more than I had known in 1984, and so too would the reviewers. If before the reviews began, I could rewrite a few chapters ... add another chapter ... modify some sentences ... improve, update, correct ... I would be more comfortable. With the benefit of hindsight revision, I could guarantee that the reviews would be encomiums ... huzzahs ... bravissimos.

Then a friendly member of the Union of String Theorists offered me the opportunity of a lifetime – a chance to travel in his 11-dimensional M-brane stringtwister back in time through the nearest Gott loop to improve and revise the book. "Think of how you can outmaneuver and stymie and unnerve your critics!" my union friend said. "It would be like re-recording the Nixon tapes!" "But what if string theorists, even the unionized band, have it wrong, and there are just 4 dimensions to space-time?" I replied, a bit nervous at trusting the latest M-brane string theory. Like other economists, I have more faith in the Invisible Hand than in hands-on economic engineering, and like other labor specialists, I prefer evidence to theory. "Don't worry, " came the reply, "The Union of String Theorists reduces turnover, increases pay, raises productivity, lowers inequality and improves fringe benefits. And, if by chance, there are only 4 dimensions, the M-brane stringtwister will do nothing worse than scramble your brains into chaotic perturbation.

That would be a disaster for a physicist, but who knows what it will do to a labor economist? Scrambled brains and chaotic perturbation might turn your neurons on and give you a new start in life. If you wrote about economies in Calabi-Yau space, you'd be eligible to join our union. Our health plan covers scrambled brains."

So I have considered the corrections, revisions, and changes I would make in the book if the 11-dimensional M-brane stringtwister works ... and new ideas for the next phase of research on trade unionism if the world has are fewer than 11 dimensions. To guide my deliberations, I read the papers in the *What Do Unions Do?-Twenty Year Perspective* and reviews the large literature on unionism in the US and other countries that followed *What Do Unions Do?*

As the Symposium papers make clear, some findings and interpretations in *What Do Unions Do* have held up well over time and across settings. But the book also missed some things and ... (*sotto voce*) may even have got some others wrong. In the period since the book, research by many scholars, including participants in the perspective and myself – older and presumably wiser than when the book appeared – has enriched and altered thinking about unionism. In areas where I believe the book got the story right but where controversy remains, I will argue that the evidence favors WDUD. In areas where the reviewers have correctly identified weaknesses, I will make the appropriate changes or if the 11-dimensional M-braner doesn't work, shout uncle. I conclude by unscrambling ideas for future work.

II Basic themes

There are three main themes in WDUD.

As many commentators have stressed, the first theme is that studying the effects of unions on wages as if unions were a textbook monopoly misses critical aspects of what unions do, and indeed of what any democraticizing institution does at the workplace. Following Albert Hirschman's (1970) exit-voice terminology, I labeled the missing component collective voice. [1] This placed unions as a social institution front and center. Going beyond the standard exit-voice model, WDUD sought to capture the two-sided nature of collective bargaining by stressing that management's response to unionism or worker voice was critical in determining outcomes. For employee voice to be effective at the workplace, management must listen. For employee voice to be effective more broadly, the state and society must listen. The stress on voice as a mode of communication and information in WDUD fit well with the emerging economics of information of the 1980s. The notion that unions could improve compensation packages by obtaining workers' preferences for non-wage benefits was influenced by Michael Spence's work in industrial organization and the development of new goods. The combination of information via voice and union bargaining power guaranteeing that management listens to worker concerns, creates the potential for unions to improve firm-level economic outcomes.

I recognize now but was oblivious then to other ways in which collective voice can and does operate in variegated institutional settings. Throughout the European Union, mandated works councils provide collective voice without extracting monopoly wages at the workplace. As union density has fallen in the US and UK, firms have created mechanisms for worker participation through employee involvement committees, joint consultative councils, and so on. My analysis with Joel Rogers in *What Workers Want* (1999) highlights that many workers want unions or other labor institutions to help them give input into workplace decisions – the voice face of unionism. I take the criticisms of WDUD for ignoring nonunion voice as valid, but note that voice without power is too often ignored by higher-ups in organizations that the early institutional economists stressed, as Kaufman (Winter, 2005) reminds us.

The second theme of the book is that any evaluation of the benefits and costs of unionism has to balance the costs due to monopoly wage or compensation setting against the economic benefits that unions bring through lower turnover, improved productivity, more desirable distributions of compensation between wages and benefits, reduced dispersion in pay for workers, and the political success of unionism in advancing the well-being of workers broadly. WDUD took the view that union wage or compensation gains extracted efficiency losses on the economy that showed up in “Harberger triangles”. This rejected the theoretical possibility that management and labor would reach efficient bargains over wages and other workplace conditions and transfer income from firms to workers without loss of employment or output (Leontief, 1946). Even without Coase-style efficient bargaining, the empirical calculations showed that the allocative losses due to unionism were relatively modest, of a magnitude that the benefits of unionism could largely offset or even dwarf. The fall in the union wage gap in recent years (Hirsch, MacPherson,and Schumacher, 2004; Blanchflower and Bryson, 2004) implies that the monopoly-face losses have become even less debilitating for the aggregate economy. But the book had a serious weakness in analysing the economic cost of unionism: its social calculus was based almost exclusively on a comparative statics allocative analysis. WDUD gave little attention to the effects of unions on growth – be it employment growth or output growth due to investment, and research and development – that should enter any social evaluation of unionism.

The third theme was that unionism does most social good when union workplaces compete with nonunion workplaces. That competition is good for firms is a cardinal principle of economics. That competition between unions and other institutional forms or among unions is good for the labor market is contestable. At one extreme, some opponents of unionism find it hard to believe that any rate of unionization above zero could benefit the economy. At the other extreme some analyses imply that centralized bargaining is optimal. Examining Nordic labor relations, Mancur Olson (1990) argued that all-encompassing unions are optimal since they internalize the negative externalities (usually, loss of jobs) that result from union wage gains. In a similar vein, Calmfors and Driffil (1988) claimed that unions that cover only a moderate share of the work force have the greatest allocative losses. By contrast, my work with Lazear (1995) argues that the curve relating worker power within a firm to output has an inverse U shape, so that the ideal level of worker power is intermediate between what workers/unions want and what management wants. Kaufman and some others in the Symposium are on the right track to critique the simplistic way WDUD dealt with the complex issue of the optimal level of unionization.

As Pencavel notes in his Symposium paper (2005), empirical evidence on the optimal level of unionization and associated mode of collective bargaining is mixed. At times Scandinavian countries and Austria who operate with high levels of unionism and "neocorporatist" arrangements have had superior economic performances, but these are small open economies that may not need domestic competition to keep noses to the grindstone. At other times, countries with middling levels of unionism, such as Ireland or the Netherlands, have done well, using social pacts that cover the entire work force to resolve macro-economic problems. And countries with low levels of unionization, such as the US, have outperformed other capitalist economies in some periods – for example, from the mid-1990s through the mid 2000s.

As a devotee of competition, I wish the evidence spoke clearly that unions bring more positive things to an economy in a competitive setting . But the cross county time series regressions that analysts use to make inferences about the effects of different levels and forms of unionism on aggregate economic outcomes give what Pencavel generously calls “fickle results” from which it is not possible to make valid generalizations.[2] If the M-brane stringtwister works, I will modify the discussion of the optimal level of unions considerably, stress how little robust evidence we have, and make the theoretical point that institutional details may matter a lot. I would also stress that the problem is a Coasian one of whether or not the institutions help or impede labor and management to reach efficent bargaining outcomes, and that current theory provides little help in sorting out this issue.

Kaufman makes two other critical comments about the assessment of the optimal level of unionisation that also deserve attention. First, accepting that unions reduce turnover, which most firms find desirable, he notes that we lack evidence that turnover was excessive absent unionism in the economy as a whole. Given the high rate of mobility in the US, I would be shocked if turnover is sub-optimal, but even so the point is valid: what is good for the firm may not be good for the economy. Moreover, views about the optimal level of turnover for the economy have changed over time. In the 1950s some American labor economists worried that declines in mobility were creating “industrial feudalism” (Ross, 1958; Kerr, 1997). In the 1970s and 1980s, many heralded Japanese lifetime employment as economically beneficial, since guaranteed tenure would induce workers and firms to invest more in specific human capital. In the 2000s many view the high mobility in the US as superior to longer term job tenures. The changes in views are not rooted in research on the economy-wide benefits and costs of higher turnover, persumably because no one has devised a convincing research strategy to identify the optimal level of turnover in the US or other economies.

Second, Kaufman notes that the optimal level of unionism depends on two factors that WDUD did not treat: the presence (and presumably effectiveness) of other institutional mechanisms for worker voice; and the ability of unions to redress imbalances in bargaining power in external labor markets and in the national polity. Alternative institutions of effective worker voice would reduce the optimal level of unionism, as mandated works councils have arguably done in the EU; while union effectiveness in improving worker bargaining power would arguably raise the optimal level of unionism on a national scale, at least to the extent that the Freeman-Lazear model of worker power within a firm applies economy-wide.

In the M-brane stringtwister revision, I have added a long chapter on the optimal level of unionization, but must admit that, for all the mathematical twisting and turning, not even the symmetries in Calabi-Yau manifolds identify a single answer.

III. Errors of omission

There were (at least) three serious errors of omission in WDUD

1. The rest of the world

The first is that the book dealt almost exclusively with the US, as if the rest of the capitalist world had no unions or as if there was nothing to learn from what unions did outside the US. Looking back, it is easy to shake one's head at the American hubris of entitling the book <u>What Do Unions Do</u> with so little attention to the rest of the world. Having spent considerable time in the United Kingdom and elsewhere learning about how unions and labour institutions operate outside the US, I am acutely aware of differences in the rules governing labor practices among advanced countries and in the difference between confrontational US labor-management relations and the more – dare I say? – civilized relations in most EU countries. Retitling the volume *What Do US Unions Do* would surely be the first change I will make in the 2004 stringtwister edition (with a promise of further volumes on other advanced economies).

As a mea culpa for ignoring the rest of the advanced world, I note that prior to the National Bureau of Economic Research's Comparative Labor Markets project, labor economics in the US was itself highly US-centric. We left the experiences of other countries to area specialists, sociologists, comparative industrial relations, and the few residual Marxists. Assessing the extent to which WDUD offers a general analysis of unionism requires evidence on what unions do outside the US as well as within the US.[3] There was also, however, a powerful practical factor that limited WDUD to the US: the difficulty of obtaining for other countries micro data of the type that formed the heart of our research. Hard as it may be to believe, the Workplace Industrial Relations Survey was not yet readily available and the OECD had yet to publish the *Employment Outlook*, which helped spur analysis and micro data-gathering on labor issues in advanced countries. Nowadays, with international students in our classes, labor economist colleagues and micro-data sets in virtually every country, entire censuses on laptops, and the potential of web-based surveys covering many countries, there is no such excuse.

In any case, Pencavel's Symposium paper on unionism from an international perspective is on target. We have as much to learn about unionism from the experiences of other countries, particularly countries with more centralized and state-influenced economic systems, as we do from the decentralized US economic system. The fall in union density in many OECD countries shows some convergence in the position of unions among advanced countries, but Pencavel correctly rejects the notion of convergence in labor institutions broadly. Collective bargaining coverage has diverged across countries, as countries with mandatory extension of contracts continue to rely on union-management negotiations to set pay. France, with a smaller percentage of workers unionized than the US has nearly all of its work force covered by collective bargaining.

Going beyond the OECD, there is also much to be learned from the experiences of unionism in developing countries which WDUD and the Symposium have ignored. At one stage the World Bank, the IMF, and other international financial institutions viewed unions and collective bargaining as adverse to desireable economic policy. The ILO necessarily took the opposite view. But as research has proceeded, the World Bank (though not the IMF) has moved toward a perspective closer to that in WDUD. In 1995 the Bank's *World Development Report* stated that "Free trade unions are a cornerstone of any effective system of industrial relations that seeks to balance the need for enterprises to remain competitive with the aspirations of workers for higher wages ...unions can help raise workplace productivity and reduce workplace discrimination." In 2003, the Bank published Toke and Tzannatos' review of studies of unions, *Unions and Collective Bargaining: Economic Effects in a Global Environment*, that concluded that unions were more likely to improve than harm developing economies. Part of the Bank message has been that represession of unions in developing countries can be associated with excessive intervention and regulation by the state.

Going beyond the micro-economic analyses of WDUD and related research, there is a missing element in WDUD and the Symposium commentary that the experience of developing countries brings to the fore. This is the role that unions have historically played as a democratic voice institution fighting for democracy and free markets. Solidarnosc helped overturn the Soviet Empire. COSATU helped undo the Apartheid regime in South Africa. The trade unions in Zimbabwe have stood against President Mugabe's seizure of private property and destruction of the economy. The Peronist unions helped stabilize Argentina in the aftermath of its 2001 economic crisis and have been a responsible force in Argentina's ensuing recovery. At some unknown future date, free trade unions will help China move toward democracy.

The big lesson from bringing the rest of the world into the WDUD analysis is that unions are mutable social institutions that operate differently in different institutional settings. Unions have a monopoly face and a voice face. The mix between them and the impact of unions on economic and political outcomes depends on the environment in which unions operate. The US is one environment. There are many others.

2. The public sector

The second important error of omission is that the book deals solely with private sector unions. Police workers, firemen, teachers, government employees of diverse sorts make no appearance. This was not because we believed, as George Meany once claimed, that public sector workers were non-organizable, but because the public sector environment differs so much from the private sector environment as to require a different analytic model. In both sectors, unions and management have a mutual interest in increasing the demand for the products or services and a conflict over the division of revenues. In the private sector, unions can do little to raise demand for goods or services made by union labor, so most managements oppose unions as an institution that reduces profits. In the public sector, unions are often an effective advocate of increased public services, producing a different attitude among managers. Police unions and police departments gain from greater budgets for security, teachers unions and school boards gain from greater resources for education, and so on. A realistic analysis of public sector unions has to factor in their role in shifting demand for public services through lobbying legislatures or city government and through campaigns to convince citizens that they need more public services, as well as their role in shifting the allocation of budgets toward workers.

Given that the public sector has maintained its density in the US and in other countries while private sector density has fallen, omission of the public sector is a serious weakness of WDUD.[4] If one were to analyze the impact of unionism by sector proportionate to collective bargaining coverage or membership today, nearly half of one's research effort would be devoted to the public sector[5]. Thus, WDUD is about a declining proportion of the US unionized work force. Blanchflower and Bryson include estimates of union wage effects on public sector workers in their analysis and Gunderson reviews other evidence on public sector unionism, but the symposium in general has followed the WDUD concentration on the private sector. In succeeding work, I have tried to remedy the WDUD exclusion of the public sector by examining the growth of public sector unionism (Freeman, 1986) , the effect of public sector unions on wages and benefits using a before/after design (Freeman, Ichniowski, Lauer, 1989) and, working with NBER colleagues, have tried to illuminate how public sector unions affect outcomes differently than private sector unions and why they have developed so differently(Freeman and Ichniowski).

3. Dynamics, growth, and the macro-economy

The most egregious error of omission in WDUD is the cursory treatment that the book gave to the effect of unions on growth and dynamics. As the book was nearing publication, I became concerned about this, and undertook some last second regressions of productivity growth on union density across industries, which gave insignificant negative coefficients on the rate of unionization. Reviewing the current state of work in this area, of which his own is the most important (Hirsch, 1991) , Hirsch concludes that "there exists no strong evidence that unions have a *direct* effect on productivity growth" (2004, p 431), consistent with the WDUD analysis .

But productivity growth is not the same as economic growth at the firm or industry level. The standard growth accounting equation relates output growth to total factor productivity growth *and* to the growth of capital and labor. Hirsch's analysis of investment in physical capital and R&D spending in the US shows that unionized firms and sectors invest less than otherwise comparable nonunion firms and sectors, which implies lower rates of growth for organized parts of the economy. The logic for the lower investments is the union impact on profits and the quasi-rents. If unionized firms earn lower profits from given investments because unions shift some of those profits to workers, they will invest less than otherwise comparable nonunion firms. These results have been replicated in different data sets, models, and time periods, in the US. In addition, a wide body of research has found that employment growth in unionized sectors falls short of that in nonunion sectors, which further adds to the picture that unions reduce the growth of firm or sectoral output primarily by affecting usage of factors. By contrast, work on unionism in the UK has not found adverse union effects on investment or R&D, possibly because UK unions have a modest impact on wages and presumably profits. Still, even in the UK, unions are associated with lower employment growth. Given a choice between current members and possible new members in the future, unions will favor the former, sacrificing growth and future members for the benefit of current members, as Peter Kuhn (1982) pointed out in his PhD dissertation (written as WDUD was nearing publication). This view is consistent with the findings, including Freeman and Kleiner (1999) and that of DiNardo and Lee (2004), that unionism does not increase the risk of plant or firm closure, even while it reduces employment growth.

But the fact that US unions cut into quasi-rents and reduce R&D spending and investment in physical capital and lower growth of employment at the level of firms or sectors does not mean that unionism reduces economic growth nationally. If union firm A does not make a given investment because unions lower the prospective profit on that investment, nonunion firm B may make the investment, so that the rate of national investment and growth is unchanged. But in the US, unions have arguably contributed to aggregate economic growth through a very different mechanism: by increasing the savings of American workers through union negotiated defined benefit retirement plans. My analysis of 1979 Current Population Survey showed that 83% of union workers had pension coverage compared to 39% of nonunion workers (Freeman, 1985). Budd's Symposium paper finds a similar though modestly smaller difference in the 2002 Current Population Survey: 76% of workers covered by a union contract having a pension plan compared to 46% of nonunion workers. Since pension fund moneys are deferred compensation and thus savings, these figures show that unions increase the savings of American workers and thus the supply of capital for economic growth. Given that saving and investment rates are highly correlated by country (Feldstein-Horioka, 1980), this can be expected to increase investment and growth. Whether this effect of unions on growth is more or less important than the reduction growth at the industry or firm level due to union capture of profits is an unexplored issue that deserves scholarly attention.

Turning to macroeconomic fluctuations, Mitchell and Erickson note that WDUD failed to address the impact of union wage setting and other policies on macro-economic adjustments, and in particular on the prevalence of profit-sharing modes of compensation that could help maintain full employment and low inflation. While some unions have negotiated substantial profit-sharing, for instance the UAW in its bargaining with the Ford Motor Company, workers and unions generally prefer greater certainty in wage payments. Given the less than stellar 1990s-2000s economic performances of France, which has the highest rate of profit sharing in the European Union, and of Japan, which relies substantially on profit sharing through its spring and winter bonus system (Freeman and Weitzman, 1987), I find it difficult to believe that this is a major factor in macro-economic performance. But, then, like most micro- economists, I find macro economics to be highly problematic since it seeks to reach generalizatios and to judge theories on so few data points.

From the international perspective, unions play a bigger role in macro-economic policy-making and political developments outside the US than in the US. In some settings, as Pencavel notes, unions have supported disastrous populist macroeconomic policies. But in Scandinavia, unions have been in the forefront of linking labor market policies and macro policies in ways designed to strengthen markets and in pushing the frontier of economics in thinking about these issues. When one talks with a Latin American economist, there is often a deep suspicion of what unions do to the macro-economy. When one talks with a Scandinavian economist, there is no such suspicion – in part because the unions regularly consult with the best macro-economists and make decisions in a macro-economic context. Even within advanced Europe, moreover, there are strikingly different experiences. McLennan (2005) associates the successful macro-economic performance of the UK with the decline in private sector unionism that followed the Thatcher industrial relations reforms. But over the same period, Ireland adopted an EU social pact for wage-setting, maintained union density and influence, and outperformed the UK. OECD analyses of the impact of collective bargaining and unionisation on macro outcomes published in various Employment Outlooks tell no clear story. Again, the message I draw is that unions are mutable social institutions that operate differently in different institutional settings.

IV. The test of time: Do the empirics stand up?

*"Give me your evidence or shut the #**~! Up" – Punk Labor Economist, circa 1984*

WDUD was written when structural modeling, usually of labor supply behavior, was the vogue among labor economists. To those lucky enough to have missed this phase of research, structural modeling meant developing sophisticated models of optimizing behavior and then estimating those models with as much econometric sophistication as possible on data that was rarely rich enough to yield clear conclusions nor to illuminate real behavior. Little time was given to finding the psuedo experiments or valid instruments that might truly identify responses to economic incentives. Since none of the modelers knew the right structure and most dismissed behavioral economics as outside the space of economic investigation, this research taught us more about modeling than about the world. My view then and now is that this approach does not add greatly to our knowledge of economies. Taking a very different approach, the institutional economists and industrial relations experts whose work Kaufman summarizes in his second symposium paper built their knowledge of the world largely on case studies (Kaufman, Winter, 2005).

As the punk economist quote at the outset of this section indicates, the methodological approach of WDUD differs from these modes of analyses. Research on WDUD was motivated by a (sometimes aggressive punk) belief that progress in understanding unions would come more from analyses of large bodies of statistics covering multiple outcomes than from blackboard theorizing or case studies. It is on large bodies of statistical evidence, generated in the ensuing 20 years, that nearly all of the Symposium reviewers have judged the durability of the book's message.

My reading of the state of research on what unions do, which the Symposium papers summarize admirably, is that the WDUD claims that have undergone the most substantial and rigorous empirical analysis remain valid today. The empirical assertions about what unions do to wages, dispersion and inequality of pay; fringe benefits; quits and turnover; profitability; job satisfaction; human resource management policy; and political activity and outcomes, appear robust over the past two decades. Research on union effects on productivity gives a wide range of estimates that are consistent with the WDUD claim that "Unionism per se is neither a plus nor a minus to productivity. What matters is how unions and management interact at the workplace" (Freeman and Medoff, p 179), though not with some of the more positive gloss put on that generalization in the book, which Hirsch softly critiques as being "overly optimistic' about where the average effect would ultimately lie. I will reduce the gloss if the M-brane stringtwister gives me the chance.

Overall, however, since the Symposium participants agree, sometimes enthusiastically and sometimes grudgingly, that *What Do Unions Do?* got most things right, I don't see great need for M-brane stringtwister improvements, much less for Richard Nixon erasures of the record. To be sure, I would have chosen adjectives from a more benign thesaurus on some contested points than did some of the symposium participants, but I have no problem with their substantive assessments. What unifies empirical labor economists as a group is that our conclusions are data driven rather than prior driven. Our priors can be found in the adjectives or gloss/spin we give to particular findings, but not in the findings themselves. Stripped of verbiage, we let the evidence do the talking. But, adjectives aside, there is one WDUD claim with which some Symposium participants disagreed strongly enough that I feel the need to put on my "punk labor economist" outfit and go to battle. This is the claim that management opposition to unionism has been a major cause of the precipitous drop in private survey sector unionism. After commenting on the research results about which there is general agreement, I will defend this claim against the criticisms.

1 Union wage effects (Blanchflower and Bryson, 2004)

When I think union wage effects, I do not think WDUD. I think H. Gregg Lewis. The WDUD research on union wage effects dovetails closely with Lewis's later work, in part because Gregg visited NBER and Harvard to analyze CPS files, where research assistants working on WDUD did some work for him as well. Our analyses of the micro data showed substantial differences in union wage effects among groups of workers and industry, occupation, and region. Thanks to Hirsch, Macpherson and Shumacher (2004) and Blanchflower and Bryson (2004), we know now that the CPS estimates were biased downward because the Census hot deck procedure of imputing earnings to missing observations did not include unionism as a hot deck variable. Still, Blanchflower and Bryson show that this does not affect the patterns of differentials among workers or sectors. They confirm the finding that there is no single union wage effect, but a set of effects that depend on worker and sectoral characteristics. In addition, WDUD argued that declines in union density should make the demand curve for union labor more elastic and thus lead to a decline in the union differential; and that the differential should vary countercyclically, as nonunion wages respond more rapidly to immediate market pressures than union wages. Blanchflower and Bryson show that both of these expectations are supported in the data. The falling union premium in the private sector in turn means that the estimated monopoly effect of unionism through static reallocation of labor is smaller than stated in the book.

2 Inequality (Card, Lemieux, Riddell (2004)

When I first analyzed the effect of unionism on dispersion of pay, most economists (at least around Chicago) believed that unions raised inequality by increasing the pay of labor's elite and reducing the pay of nonunion workers by displacing labor from unionized sectors. The analysis summarized in WDUD showed that union-induced reductions in the dispersion of pay among unionized workers and union-induced reductions in the white collar/blue collar pay differential dominated these factors, so that on net unions reduced pay inequality. Because this finding ran counter to the then prevailing wisdom, I compared inequality within firms and across the entire economy in cross-section data and examined longitudinal data that followed workers from union to nonunion employment or conversely. Much of this work focused on men in the private sector. Ensuing research on wage inequality, reviewed and extended by Card, Lemieux, and Riddell, has covered more groups – women, public sector workers – with more powerful models that deal with variation in coverage and with differing union wage effects among groups of workers – and found that the union impact on dispersion of earnings is robust across time, models, and data.

As Card, Lemieux, and Riddell note, it is harder to examine the union impact on dispersion of pay outside US and countries with decentralized labor relations systems. If nearly everyone in Sweden is covered by collective bargaining, if the Netherlands extends collective bargaining agreements to non-covered workplaces, and if Italy changes pay nationally through the Scala Mobile, as it did for many years, one cannot use comparisons of union wages and nonunion wages to infer the effects of unionism. There are two ways to deal with this problem. First, one can compare dispersion of pay across countries with differing levels of collective bargaining coverage. Such comparisons invariably show lower dispersion of earnings in economies where collective bargaining is more prevalent. The other and potentially stronger test is to exploit changes in wage-setting institutions over time to test if changes in collective bargaining coverage or modes of determining pay are accompanied by changes in dispersion. The evidence for Italy, where the dispersion of pay narrowed when Italy set pay through the scala mobile, and widened when Italy abandoned that form of pay-setting for more decentralized market-driven wage determinination shows the same pattern. For better or worse, unions and other wage-setting institutions reduce dispersion of pay.

3 Quits, turnover, and dispute resolution (Addison and Belfield, 2004, Lewin, 2005; Verma, 2005; Hammer and Avgar, 2005)

"We can after all credit unions with lower turnover" (Addison and Belfield)

The essence of the exit-voice analysis of unionism is that there are two substitute ways for workers to deal with workplace problems: quitting or voicing their problems to management. High exit should accompany low voice and conversely. The innovation of WDUD was to test this analysis with individual level data and then to use these data to break the inverse relation between unionism and turnover into a part due to the monopoly face of unionism – higher wages and better benefits and a part purportedly due to union voice. The individual level data showed overwhelmingly that union workers have lower quit rates and greater years of job tenure, which reflects the cumulation of turnover – a result that has stood up in ensuing work. Standard econometric analysis shows, moreover, that the wage and benefit effects on turnover fell far short of explaining the full union impact on turnover. This leaves the question: does voice or something else explain the remaining effect?

When I gave seminars on the union quit relation in the early 1980s, some economists proposed a simple alternative – that union workers quit less because they were less able than nonunion workers and would get bad jobs outside of the union setting. This is an interesting hypothesis, but one inconsistent with the argument that some of the same economists raised against estimates of union wage differentials – that those estimates were biased upward because unionized firms hired more able workers drawn to the firm by higher wages. In any case, the "union workers quit because they are less able" story died a quick death. Longitudinal data on quit behavior showed that the same worker had a lower propensity to quit in a union job than in a nonunion job.

The work reviewed by Addison and Belfield , Lewin, Hammer and Avgar confirms that, yes, unions reduce quits and turnover in ways that go beyond the effects of unions on wages and benefits. Addison and Belfield's analyses of the British WERS data file offers a particularly powerful test of the generality of claims about union effects on turnover and satisfaction. The analysis relates to the UK and uses data gathered with a different sampling design than I used to analyze the union effect on quits and tenure in the US. That the magnitudes of the union "voice" effects in their table 5 for the UK "largely mimic those of Freeman and Medoff" (p 579) for the

US made my day when I read it.

But what we call the voice part of the union effect is essentially a residual – the reduction in quits/increase in tenure that the monopoly face of unionism fails to explain. In the first volume of the *Journal of Labor Research,* (Freeman, 1980) I gave some modest evidence that the union effect was correlated with measures of voice, but my analysis was at best suggestive. There remains, as Addison and Belfield stress, more to do to identify the route by which unionism lowers turnover beyond improving wages and benefits. We need measures of the effectiveness of nonunion voice as well as union voice at workplaces. We need to exploit matched employee-employer data sets to test the voice interpretation of the union impact and to suggest alternative interpretations, as well. We could also benefit from some experiments, in laboratories or in the field, to illuminate the exit-voice tradeoff.

Lewin argues convincingly that individual workers' use of grievance systems cannot account for the large quit-reducing and tenure-increasing effects of unions. He points out that workers who file grievances are often less productive and may experience worse employment relations or even higher quit or turnover behavior than other workers. Clearly, a high grievance rate is no measure of collective voice working to improve labor conditions or productivity, but of its opposite. A good worker-management relation resolves problems early, without going to formal dispute channels. If management listens to collective voice, and if the union is responsible, problems are solved before they produce formal grievances. Unlike Lewin, I view the finding "from extant studies of HRM and business performance ... that the presence or availability of a grievance procedure is positively associated with organizational performance, especially when bundled with certain other high involvement-type HRM practices" (Lewin, p

227) as precisely the positive collective voice/institutional response face of unionism. The word collective means group level activity, and the words institutional response means organization level activity. When union voice works successfully and management responds positively, there should be few grievances and lots of collaboration to solve problems.

Looking at workplace level data, Verma highlights the fact that union workplaces are more likely than nonunion workplaces to have formal mechanisms for collective voice. In Japan joint consultation committees and grievance procedures are more commonly found in union than in nonunion workplaces. In Australia, employee surveys and meeetings, grievance and equal employment procedures, consultative and safety commitees, task forces, and health and safety representatives are more likely in union than in nonunion workplaces. He concludes that "unions are strongly associated with giving workers a voice in the workplace" (Verma, 2005), the sine qua non for the exit-voice model to offer insight into union-nonunion differences in turnover and other outcomes.

The problem in reaching closure on the analysis of collective voice is that we lack measures of the amount and quality of collective voice at particular workplaces. If workplace or organizational level measures explained some of the substantial union residual in quit or tenure equations above and beyond union effects on compensation, the voice hypothesis would gain support. If they explained none of the residual union effect, we would have to look for explanations in other ways. If well-operating voice mechanisms, nonunion as well as union, were associated with lower turnover, analyses would be pushed back a stage to try to determine why collective voice works well in some settings and not in others. The phenomenon to explain is not simply the large union impact on quits and tenure, with compensation fixed that WDUD identified, but also the variation in impacts across settings.

4 Unions and benefits (Budd, 2004)

WDUD claimed that unions raised fringe spending and that roughly one half of the increase in fringe spending was the result of the voice face of unions amalgamating worker preferences toward public goods at the workplace and getting workers to reveal those preferences to management. The public goods argument was right out of a standard public economics course. The assertion that union voice would lead workers to reveal their true preferences toward benefits was also rooted in basic theory. The argument was that absent some control over how the employer would use information about workers' preferences, workers would not tell employers their true valuation of benefits for fear that the employer would extract all of the surplus, as a discriminating monopolist would do. A union or some organization was needed to guarantee that some of the benefit from the information went to workers and thus to induce them to reveal their true preferences.

Budd's review of the post WDUD literature on benefits supports both propositions for the US and extends the results to the UK, where he finds that employer pensions, sick pay, and annual leave are positively impacted by unionism. The US evidence from March Current Population Surveys on the prevalence of health insurance and pension plans shows massive difference between union and nonunion workers, "with in many cases the union-nonunion differences hav(ing) widened" in the regression analyses compared to the WDUD results. Similarly, his evidence from the Employer Costs for Employee Compensation is also highly supportive of the impact of unionism on spending on fringe benefits. Budd's estimated voice effect of unionism accounts for about one-quarter of the total impact of union coverage on fringe benefits overall and over 50 percent of the union impact in low wage jobs – part and parcel of the union impact in reducing dispersion by raising the economic position of the lower paid more than of the higher paid.

Budd advances the analysis of the impact of unions on employee benefits by identifying two effects that WDUD did not examine. These are the impact of unionism on employee awareness of benefits and the impact of unions on employee use of those benefits, which together he labels the *union facilitation effect.*. The evidence shows that union members have greater knowledge of benefits than covered nonmembers and, what is perhaps more important, are more like to take-up benefits than nonunion workers. The finding that union members know more about legally mandated benefits and are more likely to receive them than are non-members supports the basic thrust of the voice model: that unions can improve the functioning of the job market through provision of information and assistance to workers in obtaining their rights at workplaces. As the union share of employment falls, not only is the provision of important privately negotiated benefits, such as pensions and health coverage at risk, so too is the delivery of socially mandated benefits.

5. Unions reduce profits (Hirsch, 2004).

That unions reduce profits is central to the theme of WDUD. It is central to the redistribution of earnings from employers to workers. It is central to the argument that management has economic motivation to oppose unions. Surprising as it may seem, when I sought to determine what unions do to profits in NBER working paper 1164, which was the back-up for the chapter in WDUD, there were virtually no econometric investigations of unionism and profitability. Industrial organization economists focused on the relation between product market concentration and profits while labor economists were concerned with wages, not with profits. Talk about low hanging fruit. As Hirsch summarizes, ensuing work in this area "points unambiguously to lower profitability among union companies"(Hirsch, 2004, p 432) irrespective of the particular measure of profits, mode of analysis, level of data, or time period covered. What has not been clarified is the extent to which the union reduction in profits comes from economic rent or monopoly profit (which would make it pure redistribution) and the extent to which the reduction comes from normal returns – quasi-rents – with undesirable efficiency consequences. Hirsch's and other's findings on lower R&D and investment in unionized firms or sectors implies that at least some of the reduction comes from unions reducing normal profits or quasi rents and thus affecting growth.

6 *Job Satisfaction* (Hammer and Avgar, 2005).

That union workers express less satisfaction with their workplace situation than nonunion workers even though unions bring better material rewards and opportunity to exercise collective voice is one of the puzzles that *What Do Unions Do?* uncovered by examining the relation of unionism to outcomes rather than wages. The finding that union workers report less satisfaction has been replicated in many studies in different countries, so the key research question is what explains this pattern. The exit-voice interpretation was that reported disatisfaction differed from the "true" disastisfaction that leads to quits because union workers developed more critical attitudes about what they should get from their workplace. Hammer and Avgar list eight possible explanations, including some interesting variants of the exit-voice analysis such as unions socializing members to be attuned to particular job outcomes, and the natural alternative hypothesis, that the content of union and nonunion jobs differs greatly, with union jobs intrinsically less satisfying.

Ensuing work has found that while much of the union/nonunion difference in job satisfaction can be accounted for by detailed characteristics of the job and workplace, there often remain significant negative effects of unionism on satisfaction. Accounting for the negative satisfaction result with detailed measures of job content does not by itself, however, resolve the satisfaction puzzle. This is because the puzzle is not simply that union workers report less satisfaction but that they also quit less. The correlation between unionism and job satisfaction is seemingly inconsistent with the correlation between unionism and turnover, since one would expect that any variable that lowers turnover would raise satisfaction, as wages does. Thus, a job content/workplace characteristics explanation of the satisfaction result must show not only that content/workplace variables eliminates the negative correlation with satisfaction but that it also eliminates the negative correlation with quits. This has not been done, to my knowledge.

Hammer and Avgar correctly criticize the terminology in WDUD regarding the way union voice affects attitudes. We called the dissatisfaction not "real", which does not capture properly what the analysis says. I much prefer the Hammer and Avgar statement that "union members have different expectations, values, or frames of reference for evaluating their job outcomes" (Hammer and Avgar, 2005). The methodological implication that follows from this is that any model of how unions affect job satisfaction must analyze its effect on both the expectations/values/frames and on objective outcomes. Since I left Chicago many years ago, I can also agree that "pure economic theories are too simple as models of the causes and consequences of job attitudes", (Hammer and Avgar, p 261) without fear of being instantly slapped down by the Invisible Hand.

7. Political Activity (Masters and Delaney, 2005)

"Clear it with Sidney" purported statement by Franklin Delano Roosevelt, referring to Sidney Hillman, President of the Amalgamated Clothing Workers Union.

Unions devote considerable resources to politics. They seek two goals: legislation or administrative policies that allow them to function successfuly as organizations; and laws that benefits workers broadly. The WDUD analysis found that despite the heavy involvement of unions in the Democratic party, unions were unable to get Democratic administrations to enact laws favorable to them as institutions but that by working with other groups, unions helped enact pro-worker legislation more broadly.

Masters and Delaney's review of union political activity in the past 20 years corroborates this picture. As union density has fallen, unions increased their political efforts, offsetting at least part of their reduced share of the work force. In 2002 AFL-CIO political operatives claimed that they had increased the union share of the electorate in the face of falling density, but this was the result of erroneous analysis of exit poll data (Freeman, 2004). The National Election Studies data given in Masters and Delaney's table 7 show a continuous drop in the percentage of the electorate from union households from 26% in 1980 to 15% in 2000. They also show that the union-nonunion difference in voting Democratic in Congressional elections increased over this period, but the increase was insufficient to compensate for the decline in the union share of voters. In addition, while union financial contributions to political campaigns are large, they are dwarfed by business contributions.[6]

The efforts of unions to gain advantage for their organizations and workers broadly through politics were largely unsuccessful in the 1990s. During the Clinton Administration unions failed to get striker replacement or labor law reform and failed to stop the NAFTA treaty that they believed, rightly or wrongly, would harm American workers. The most they could do Masters and Delaney report, was to prevent Congress enacting laws that could weaken unions. But, as density has fallen, so too has union ability to deter anti-union policies. At the state level in the early 2000s, Oklahoma enacted a right-to-work initiative and the Republican governors of Indiana, Missouri, and Kentucky states rescinded state level collective bargaining regulations.

Why has the huge union effort in politics borne so little fruit? One hypothesis is that the union movement has become so linked to the Democratic Party that the Democrats take them for granted and see little value in battling for union goals, while the Republicans see them as their main political opponent,[7] and thus seek to weaken thems. Another possible hypothesis consistent with the Masters and Delaney analysis is that union political campaigning provokes massive responses by business, which can draw on much larger resources, which influence Democratic as well as Republican politicians. Masters and Delaney offer a third hypothesis, that while labor is good at the mechanics of politics, it lacks "the passion of ideas", that while difficult to test, has the flavor of a correct assessment of the state of trade unions. In any case, the message from WDUD that political action is unlikely to revive union fortunes seems as or more valid today than it did twenty years ago .

*8. The illusive productivity effect (*Hirsch, 2004)

Prior to the Brown and Medoff (1978) production function analysis of the effect of unions on productivity, discussion of unions and productivity often revolved around horror stories of featherbedding , for instance by the musicians union or printing unions. The cross-state two digit industry production function analysis, by contrast, suggested that unions had a huge positive productivity effect. As additional and more refined evidence accrued, however, estimates of union productivity effect shrunk – for instance, Kim Clark's 6-8% estimated productivity effect for cement (Clark, 1981) or his -2% effect in business line data (Clark, 1984). The cross state production function analysis was too aggregate to hold up under scrutiny. Yes, value added in a two digit industry was higher in unionized Minnesota than in nonunion Mississippi, controlling for capital and labor inputs, but the difference was more likely due to manufacturers producing goods of different quality for different markets than that unions massively raised productivity.

Ensuing work has supported the modest magnitudes found in the more refined estimates. Belman (1992) reported that approximately 2/3rds of the studies of unionism on productivity found positive effects, though estimates varied across sectors and sometimes in the same sector (Belman, Appendix I). Doucouliagos and Laroche (2003) performed a meta analysis of 73 union productivity studies in the US, Japan, and UK and found a near zero effect among all studies, but a positive effect one for US manufacturing. Hirsch correctly stresses the "substantial diversity in the literature about union productivity" (p 429) and views the existing evidence as showing that the average union effect is very close to zero. Based on the three reviews I regard the evidence as more favorable to a positive union effect than he does but agree that the strongest conclusion from this research is that there is a lot of variation in estimated union effects[8].

Why is the estimated impact of unions on productivity so varied? There are two plausible explanations. The first is that the production function methodology is too crude to uncover anything more than the basic link between capital and labor, and perhaps quality of labor, and output. Measures of output almost never capture the same physical units: there are subtle variations even in tons of cement that would escape any econometrician. The second explanation, which is consistent with the WDUD story, is that what matters is not unionism per se but the interaction of unions with management, which can differ across industries, firms, and even establishments within a firm. If this is the case, the only way to progress in understanding union productivity effects is to replace the 0/1 union variable with new measures of "collective voice/ nstitutional response" in unionized (and nonunionized) settings – the true quality of labor and management relations, which no major US survey even seeks to uncover, but where the UK WERS surveys and their clones provide a valuable model.

9 Management flexibility and HRM workplace practices (Verma, 2005; Lewin, 2005; Hirsch, 2004; Addison and Beldfield, 2004)

The claim that unions reduce management flexibility and alter human resource management is not a WDUD original. The encyclopedic *The Impact of Collective Bargaining on Management* by Slichter, Livernash, and Healey, published in 1960, told in excruciating detail the multiple ways in which management operated differently under collective bargaining than in its absence. The book exhausted me and probably all other students assigned to read its 900+ pages. The innovation of WDUD and ensuing research was to go beyond case studies to measure quantitatively the presence or absence of procedures that limited management flexibility in decision-making and that institutionalized personnel or HRM policies.

Verma's analysis shows that the generalization that unions formalize decision-making and reduce discretion and increase the time spent in making decisions holds today. Some of the formalizations, such as formal job posting, probationary periods for new workers, and formal off the job training, are likely to increase productivity. Others, such as less use of temporary help, use of more formalized promotion procedures but less reliance on appraisals in salary, promotion, or layoff decisions, have no clear impact on productivity. Less use of performance related pay and individual incentives in union settings is likely to reduce individual effort, but may increase workers' willingness to help other workers and thus add to team productivity. Whatever the effects of formalization on productivity, there is no gainsaying that unions limit the range of managerial decision-making.[9]

Lewin's evidence on grievance procedures tells a similar story: greater prevalence of formal procedures at union workplaces than at nonunion workplaces, though it is the growth of nonunion dispute resolution systems that is the most striking development in the past two decades or so. The diminution of the union-nonunion gap raises the question of why nonunion firms have mimicked grievance procedures (and possibly other aspects of union workplaces that limit managerial discretion), and thus of the benefits and costs of managerial flexibility. If Verma's hypothesis that unions can "claim partial credit for improving voice in the nonunion sector" (p 35) through threat effects is valid, we would expect to see some of the formal systems weakening in nonunion firms, as union density continues to fall.

10. The decline of union density (Flanagan, 2005; McLennan, 2005; Lewin, 2005)

The final chapter in *What Do Unions Do* attributed much of the decline in private sector union density to the anti-union activities of management and predicted that density would keep dropping below 10 percent in the forseeable future on the basis of a stock-flow model of membership. While the chapter is cautious to call this a projection rather than a prediction, Lane Kirkland, then head of the AFL-CIO, dismissed the analysis as academic fooltomery (though he probably used somewhat different words), and discouraged me from organizing an NBER conference to explore the decline in depth. After all, F&M were young whippersnappers with no practitioner experience. I remember thinking, maybe he knows things that we don't and maybe we will get caught extrapolating trend declines just before density suddenly turns up. Twenty years later, with private sector density at 8.5% and falling, it is clear the numbers told the story.

Seeking to understand the fall in density, Medoff and I examined and rejected the then common belief that the fall primarily reflected changes in the structural composition of industries. Instead, we argued that economically motivated management opposition to unionism was the major driving force for the fall in density, and estimated that by itself management opposition accounted for abour one-quarter to one-half of the drop.

Flanagan, McLennan, and Lewin disagree with this finding. In their world, the decline in union density results largely from a loss of worker interest in organizing, with management opposition presumably contributing much less than our estimated 25-50%. I have no sympathy with their arguments. The neo-classical economics part of me finds it incomprehensible that firms, seeking to preserve profits against union wage and benefit increases, would spend large sums of money and management time to defeat unions in NLRB representation elections if this was not effective. The industrial relations part of me finds it incomprehensible that field observation of organizing campaigns, descriptions by union organizers, claims by anti-union consultants, and statements by workers, could be so wrong about the role of management. The common sense part of me wonders why management would oppose card checks, fast elections, and neutrality clauses in collective agreements if management had little impact on the outcomes of union organizing campaigns.

To be sure, management opposition is not the sole cause of the decline of unionism in the

US: the WDUD estimated share fell far short of 100%. As Flanagan points out, density has fallen in many EU countries where management opposition is nill. But EU countries often mandate extension of collective bargaining, so that union contracts cover workers regardless of their membership. Free-riding behavior, which is minimal in the US, not management opposition, leads some workers to stay out of unions under those circumstances. As long as collective contract coverage remains high, union density is not a battlefront between labor and management, as it is in the US. Foreign experiences aside, Flanagan agrees that management makes economically rational decisions in opposing unionism, and argues that "the root cause of greater management opposition is relatively decentralized collective bargaining arrangements under which union compensation gains expose unionized employers to significant competitive pressure". This is consistent with the book and with ensuing work I have done with Blanchflower (Blanchflower and Freeman, 1992). Where Flanagan and I disagree (aside from rhetoric) is on whether management opposition, (proxied by charges of illegal management practices per NLRB election) must rise continuously, as it did in the 1960s and 1970s, for opposition to deter unionization. He believes that the measured stability of this proxy from the mid 1980s to the 1990s invalidates the WDUD analysis that opposition is important. I do not see the logic of this claim. Once firms have found the level of opposition that succeeds in deterring unionization, why should they give more resources to opposing unions? The economic principle is to spend until the marginal benefit equals the marginal cost, and then to maintain that level. By the 1980s, management had achieved the level of opposition necessary to deter union organizing drives.

Building on Flanagan's analysis, McLennan rejects "the claim that anti-union activities directed by high-paid consultants hired by business is a major reason for the precipitous fall in unionism" (McLennan, 2005) as not credible. Again, I have great faith in the rationality of management. If consultants are not helping defeat unions, why would management hire them? If spending company resources to defeat union drives does not succeed, why do it? Given that McLennan stresses that "the finding that unionization lowers profitability was of greatest concern" to management, standard economic anlaysis suggests that management will seek to protect those profits by opposing unions. Management does not fight unions in the interests of workers. The WRPS asked managers in organized firms whether they thought unions benefited workers and the majority said yes.[10] This suggests that they share rather than disagree with "F&M's position that the decline in union density is harmful to workers' interests", as Mclennan says.[11]

The principal alternative hypothesis for the fall in union decline is that nonunion workers no longer seek to unionize as much as in the past. Flanagan says "there is much stronger evidence of declining demand for union representation among nonunion workers" (p 59). To the contrary, the "declining worker interest" hypothesis runs counter to the survey evidence that shows that a large and increasing proportion of workers want unions in the US. The WRPS showed that 32% of nonunion workers would vote for a trade union, with approximately 8-12% percent of workers reporting that management's attitude would shift their vote in an NLRB election. Lipset-Meltz found that 48% said they would vote union in an NLRB election[12] – a proportion higher than in Canada. Comparable questions for other English-speaking countries show that the US has the highest proportion of nonunion workers who support unionism (Boxall, Freeman, Haynes). The Peter Hart survey evidence in figure 1 shows the percentage of nonunion workers who have said they would vote union has risen over time.

Apparently accepting that the survey evidence shows a huge number of workers wanting but unable to gain union representation, McLennan questions how much faith we should put in workers' responses. He notes, "there is considerable difference between a telephone answer to a hypothetical question ... and the individual's binding decision to support a union". I agree. It is harder to support a union at a workplace, particularly if your management and supervisor are telling you to oppose the union than it is to say, yes, I favor a union on a telephone survey. But there is no reason to believe that "the telephone response bias" has grown over time (which is needed to account for the trend in the Hart results); differs among countries (which is needed to account for the greater expressed desire for unions in the US than elsewhere); or is so large as to invalidate the survey evidence (if only 10% or nonunion workers were in workplaces where a majority favored unions but where opposition deterred them, density would more than double).

In any case, in summer 2005 the AFL-CIO undertook an experiment that effectively tested whether what people were saying on surveys truly meant they would join a union organization, albeit in a different instutional setting than voting in an NLRB election against the wishes of management. The AFL-CIO sent organizers into ten cities to ask people to join WorkingAmerica (www.workingamerica.org), a non-collective bargaining affiliate. The organizers signed up 800,000 members in six months and signed up thousands more on the Internet. This, despite the fact that WorkingAmerica offered uncertain benefits beyond information about labor issues and solidaristic support. The ease with which organizers signed up workers shows a deep hunger for an employee organization to help them with workplace and labor market problems, consistent with the survey results. Whether American trade unions can tap this hunger and deliver services to the millions who favor unions but cannot gain collective bargaining at the workplace will go a long way to determining whether unions can reinvent themselves in the US (Freeman, May 2005).

In sum, I give both management and workers credit for knowing their economic interests and give management credit for competence in defending their economic interest, and workers credit for telling the truth about what they want.

V. New Braners: What Next for Unions and Research?

"F&M told us what private sector unions do while there remained unions to do it" (Hirsch)

The Symposium papers show that *What Do Unions Do* identified empirical regularities about trade unions that have stood the test of ensuing analyses and changing economic developments, and which appear to apply broadly to unions across the world. The reason that ensuing research has corroborated most WDUD results is not that the book's analyses were flawless – the diverse criticisms invalidate that interpretation – but rather that the book relied on micro data sets with thousands of observations that allow us to differentiate fact from fancy and uncover general patterns, rather than on structural modeling or limited case studies.

Some Symposium participants disagree with how Medoff and I packaged the factual regularities – the interpretation or spin that we put on them. Our analysis reflected our priors, based on the Hirschman exit-voice model, and also a benign view of unions as an institution that tries to improve the well-being of normal employees. By examining a diverse set of outcome variables, rather than concentrating on union wage effect as in previous literature, we gave ourselves a difficult conceptual task. Research on the multiplicity of effects of unions does not easily lend itself to a single theme which a book demands, as our editor, Martin Kessler, continually reminded us. We struggled to weave all the findings together to give a unified picture of this complex social institution.

Symposium critics point out that the exit-voice/institutional response model that we used was incomplete in various ways.[13] We did not examine potentially complicated interactions between the voice and monopoly faces of unionism nor their relation to loyalty. We did not investigate the operation of nonunion voice institutions. Kaufman correctly infers that our analysis sought to balance economists' concern with monopoly issues with institutional concerns – to walk the narrow path between devotees of the Invisible Hand and observers of visible hands. Today, with globalization having augmented the imbalance of power between labor and capital that he stresses, I would use the stringtwister to add a chapter on that issue. Even with stringtwister amendations, however, I accept that the facts established in the book would be open to alternative interpretations. This is because the extant data, generated by the world rather than by controlled experiments, does not provide definitive tests as to whether variables are causally linked as WDUD claimed or are linked for other reasons.

Some participants in the symposium disagree with the *What Do Unions Do* bottom line that the positive voice face of unionism dominates the negative monopoly face, so that society as well as workers lose from the decline of unionism. However, many of those dubious that traditional unionism is the appropriate institutional vehicle for voice accept the notion that workers need some institutional support in navigating the workplace and labor market. They direct attention at firms developing nonunion channels of voice. Given the continual decline in private sector union density,[14] their perspective and that of *What Do Unions Do* converges in one important way: recognition that the nation needs to reform private sector labor insitutions that have changed little since the New Deal. By reform I do not mean labor law reform AFL-CIO style nor management style per McLennan's paper but reforms that give labor and management greater leeway, protection, and incentive to experiment with institutions of worker representation and participation that fit a 21st century economy. From this perspective, post WDUD research should be guided not by the F&M issue of "what private sector unions do while there remain unions to do it" but by the question "what new labor institutions can give workers sufficient voice at workplaces to improve their well-being and the national economy?"

New Braners

To answer this question, we need new knowledge in three broad areas.

The first is in determining what workers and management want from labor institutions and labor laws. Joel Rogers and I (1999) took a first cut at finding out what workers want from our book with that title, but we barely touched on what management wanted. Someone should undertake a detailed analysis of the attitudes of managers toward labor relations and unions, covering the gamut from CEOs to direct supervisors. Moving beyond surveys, we need experimentation with institutions beyond what current law allows. My preferred way to open the door for experiments with new labor codes would be to turn legal regulation of private sector labor over to the states, as Canada does to provinces, and then to assess what works or does not on the basis of state practice.

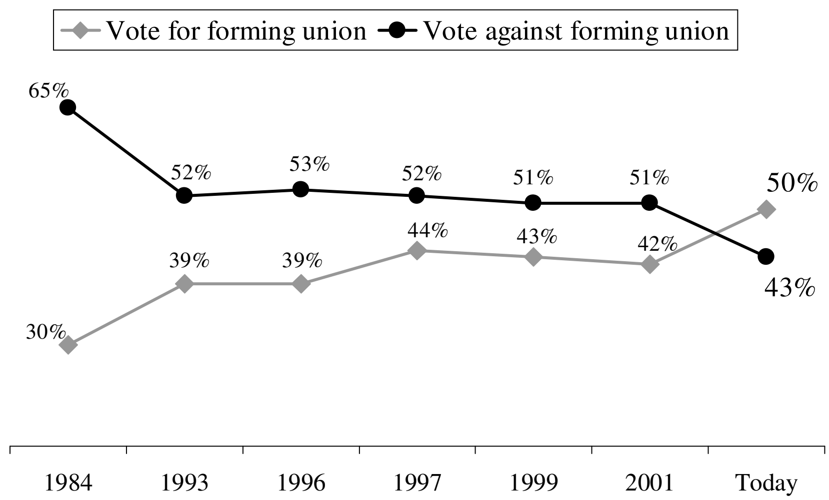
The second area where we need additional knowledge is in the potential for new forms of unionism to deliver collective voice and other services to workers outside of collective bargaining. Rogers and I have suggested a particular form – open source unionism – in which the organization combines inexpensive provision of information and advice over the Internet with geographicallybased meetings and minority unions at particular work places (Freeman and Rogers, 2002a, 2002b). The key to this form is that it enrolls members irrespective of employer agreement to collective bargaining and thus takes management opposition out of the equation. Some unions are experimenting with open source forms, with the AFL-CIO's WorkingAmerica being arguably the most extensive variant (Freeman, May 2005). Historically, the Knights of Labor fit our open source model, but without modern information technology to reach members. Prior to the enactment of state collective bargaining legislation, state and local employees had associations that did not bargain. Some government unions, including federal government unions, operate without collective bargaining contracts today. Whether this or some other restructuring of unionism (see Lerner for a very different proposal) would enable unions to improve the lot of workers despite low union density and global economic competition should be on every labor researcher's agenda.

The third area where we need additional knowledge to illuminate the road toward institutional reform is the link between the economic and social environment and union performance, which arguably underlies the variability in union wage and productivity effects. This would require better measures of labor-management relations than 0/1 collective bargaining dummy variables and analysis of the factors that lead to varying union policies and management "institutional response". Most important, since 92% of private sector workers in the US are nonunion, we need to explore the variation in the effects of nonunion institutions on outcomes and the factors that lead firms to choose one set of institutions over another. Finally, since 94% or so of the world's work force resides outside the US, more research resources should go to studying how unions operate under the far wider range of economic and social structures outside the US, in particular to the potential development of real unions in China , and union activity in India and other large developing countries, who compete with us in world markets through low wages.

At the end of his Symposium paper, Hirsch offers some “rather speculative ideas on the future of workplace voice” with which I have considerable sympathy. Like him, I am favorably inclined to some variant of David Levine’s proposal for “conditional deregulation” – exempting firms from some labor regulations when they have formed independent worker committees that take responsibility for compliance in national regulations. This would require the abolition or major reform of section 8(a)(2) of the NLRA. The Dunlop Commission discussed the idea of conditional deregulation, but did not endorse it as “practical” since neither unions nor employers wanted it. I am also favorable to widening the choice of labor representation that US managers and workers currently face: from a collective bargaining majority union or nothing to a choice that would include intermediate organizations, joint committees or works council type arrangements, which also would require changes in section 8(a)(2). However, as noted, I think that the best path toward a new labor code in the US is to weaken federal preemption of private sector labor. If the US turned large parts of labor law to the states, there would be a ferment of innovation. States would experiment with conditional deregulation or with alternative labor organization defaults or with different ways of ascertaining worker desires for unionism, of moving parties to first contracts. They could experiment with different ways to penalize firms or unions that act unfairly. There is no reason why “right to work” provisions should be the only place where states can choose alternative ways of regulating union and management labor relations activity within their jurisdication. Weakening federal preemption and giving the states more leeway to experiment with alternative forms seems to be the best way to open the door to these and other possible innovations.

In conclusion, we should direct post-WDUD research on labor relations toward providing ideas and evidence to help labor, management, and the country move beyond Depression Era labor institutions. Thankfully, we can contribute to this policy relevant research agenda without risking having our brains scrambled in the 11 dimensional M-brane stringtwister or writing our papers about labor relations in Calabi-Yau space.

Figure 1: Surge In Support For Union Representation among nonunion workers



Source: Peter Hart Polls, “today” is 2003

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End Notes

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1. Freeman, 1976
2.My interpretation is harsher. These data and the measures used to reflect collective bargaining systems across countries are sufficiently weak to allow analysts with different priors to reach contrary conclusions from comparable data (Freeman, June 2005).
3. Even today, while US economists are more aware that there are interesting things to learn from economies beyond our own, colleagues from other advanced countries have trouble publishing papers in leading US journals on labor issues. And articles on labor markets in developing countries are found largely in development journals.
4.While private sector union density has trended downward since the mid-1950s to reach 8.5% in 2004, public sector density rose in the the 1970s and 1980s and then stabilized at about 35% of the public sector work force.
5. In 2004 48% of workers covered by collective bargaining were in the public sector, and 47% of members were in the public sector. http://www.bls.gov/news.release/union2.nr0.htm
6. McLennan suggests that business is reasonably even-handed in its political donations between the parties and that Democrats outspend Republicans in the 2004 election. Delaney and Masters' shows reasonably even division of business spending between parties in 1992 but a huge

difference in 2002. Data on party spending in the Presidential Election shows higher expenditures by Republicans (www.opensecrets.org/presidential/index.asp)

7.Republicans had a more positive agenda toward unions and workers in years past.. McLennan reminds us that the Nixon Administration enacted considerable pro-worker legislation. George Schultz, John Dunlop, and Bill Ussery were among the most professional distinguished Secretary's of Labor in US history – all three appointed by Republicans.

8.Analysis of union impacts on productivity outside the US also give a mixed picture. Addison and Beldfield note that UK studies found negative union productivity effects in the 1980s but not in the 1990s, as the economy became more competitive and that some German studies of the effects of works councils on productivity find high productivity effects while others find essentially no effects.

9.Limiting management decisions aguably should improve productivity in badly managed firms but may lowersit in well-managed firms. This suggests that the second moment of the distribution of productivity among union firms would be smaller than the second moment of the distribution of productivity among nonunion firms. This, however, would seem to run counter to the evidence that unionized firms with the most advanced HRM practices have particularly high productivity,

10.Sixty four percent of managers in firms with unions reported that unions made the lives of workers in their firm better. Freeman and Rogers, p 88

11.I am also puzzled by McLennan's criticism of the claim by Freeman and Rogers that business spends money to keep unions out of workplaces when they can, since he observes that "most senior business executives prefer a union-free environment".

12.The WRPS used a 3-point scale, yes, no, don't know. The Lipset-Meltz survey used a 5 point scale, differentiating the yes into definitely yes (16%) and probably yes (32%).

13.Verma: "Although F-M make a clear conceptual distinction between the union's monopoly and voice roles, it is operationally very difficult to separate the two" (p. 6).

Addison-Belfield: "Union voice ... is in urgent need of restatement ... to tackle various lacunae" (P 590).

14.Given that unionism has grown in sudden unexpected spurts, I have a more open view than most researchers that union fortunes could change rapidly, at least if unions managed to develop new forms and innovate their mode of operation – Freeman (2004, 2005)