DESPERATE MEASURES:
STRIKES AND WAGES IN POST-ACCORD AMERICA[*]

Jake Rosenfeld
Office of Population Research
Department of Sociology
Princeton University

ABSTRACT

Using previously unreleased data on nearly every authorized work stoppage that occurred between 1984 and 2002, this paper tests whether the positive wage-strike relationship held following the breakdown of the post-war labor-capital accord. Unlike in decades past, these findings indicate a complete decoupling of the wage-strike relationship. Even in those industries and regions where unions remain relatively institutionalized, strikes no longer increase aggregate worker pay. Strike activity also fails to narrow worker wage dispersion at the industry-region level. The findings highlight the need for rethinking existing theoretical models on strike activity and wages in this era of capital dominance.

INTRODUCTION

On October 11, 2003, 21,000 grocery workers walked off the job following a

prolonged contract dispute with their southern California employers. Soon 49,000

additional area grocery employees joined them when rival grocery chains locked them

out in a telling display of capital solidarity. Five months later, management approached

the union with a contract proposal that would freeze wages at previous levels, ratchet up

employee contributions to health coverage, and institute a two-tier compensation and

benefit package where new hires would receive much lower wages and leaner benefits

than existing employees. Union strike funds had run dry, whereas management – with a

combined net revenue topping $100 billion – showed no signs of further compromise,

and workers were eager to return to their jobs. The union accepted management's offer

and nearly 90% of union members voted to ratify the deal. The long-idled grocery

workers returned under the new contract in early March, 2004.

Is this the new face of strikes? The strike was once thought of as labor's most

potent weapon in resolving disputes over compensation matters. Research indicates that

during periods when the labor movement is highly institutionalized, strikes (or the threat

of strikes) often work to boost average employee wages, especially in the highly

unionized industrial core (Kalleberg, Wallace, and Raffalovich 1984; Rubin 1986; Rubin

1988; Wallace, Leicht, and Grant 1993). In the United States, the institutionalization of

unions within an overall framework of collective bargaining began with the state's

passage of the Wagner Act in 1935 (see McCammon 1993), and solidified during the

post-World War II years, the era of the labor-capital accord. While restricted from

challenging fundamental power relations within the workplace, labor retained the right to

strike over economic issues throughout the early postwar years (Wallace, Rubin, and Smith 1988). The growth of the union movement following the war provided labor with the organizational base from which workers could successfully strike against their employers for higher wages.

By the mid-1980s, the labor-capital accord had completely broken down.[1] Reagan's hard-line anti-union agenda, including the replacement of the striking PATCO workers in 1981, signaled a new political stance toward organized labor (Wallace, Leicht, and Raffalovich 1999; but see Farber and Western 2002). Capital's increasing mobility further eroded a labor movement predicated on firm-specific bargaining. Unionization levels fell to post-war lows (Clawson and Clawson 1999). Faced with a crumbling organizational base, the strike became an increasingly rare weapon used by unions to settle contract disputes over wages. Figure one plots union densities and strike frequencies between 1984 and 2002. While the overall unionization rate among private sector, full-time employees fell by 40% in a little under two decades, the drop in strike frequency was even more precipitous, declining by over two-thirds in nineteen years.

[INSERT FIGURE 1 NEAR HERE]

In this paper I investigate whether the positive relationship between strikes and worker wages that existed during the immediate postwar decades persisted throughout the final years of the twentieth century. Utilizing new data on nearly every authorized strike that occurred between 1984 and 2002, the analysis tests whether strike frequency works to boost median pay for non-professional workers, net of a variety of other factors including union membership levels.[2] The detailed strike data allow for an investigation into the relationship between strikes and worker wages at the disaggregated industry-

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region level.[3]   Moreover, the data allow for specific examinations into the impact of strikes in those industries where existing research and theory suggest they should have the largest effect. Finally, I test whether strike activity in highly unionized industries and locales operates to narrow workplace wage dispersion net of its affect on average worker pay. The model specifications utilized here provide the most stringent test of the relationship between strikes and wages to date.

If the recent California grocery strike is any indication, strikes no longer have a beneficial influence on worker wages. However, given that the grocery strike occurred in an industry lacking an established union presence, such a dispute may not be representative of walkouts within the (relatively) highly unionized industrial core. Rubin's pioneering work on the topic revealed that strikes serve to boost worker pay only within sectors with a well-established, organized worker presence (Rubin 1986). The data used in her analysis end in 1976, prior to the complete dismantling of the labor-capital accord. Data limitations have previously prevented researchers from adequately investigating whether the strike remains a potent weapon in highly unionized sectors (or any sector, for that matter) during the post-accord period. It may be that strikes remain powerful weapons when confronting capital over compensation issues, perhaps due to their growing infrequency. Or it could be that the strike remains a beneficial tool for workers fighting wage disputes only within a narrow confine of industries. Or, the California grocery strike could well prove representative of the new face of strikes: desperate, defensive measures deployed mainly to retain the status quo. This paper seeks to answer those questions.

UNIONS, STRIKES, AND WORKER PAY

Beginning in the mid-1930s, the institutionalization of the collective bargaining arrangement granted unions a hard-fought legitimacy in lobbying for workers' rights and more equitable pay practices. Since that period, labor market researchers have consistently demonstrated that unionization increases average wages among otherwise comparable workers (see Kuhn 1998 for an overview of the recent literature). Cross-sectional analyses reveal that among observationally equivalent workers, unionization boosts wages around 15% (Robinson 1989). Various analyses using panel data have largely corroborated this 15% finding (Freeman and Medoff 1984; Card 1996; Kuhn and Sweetman 1998). More recent research has shown that at the industry-region level, unions continue to boost median worker pay, despite the rapid loss of union members during the past few decades (Rosenfeld 2004).

If anything, the 15% wage boost attributable to union membership understates the role unions play in raising worker pay. Numerous analyses have demonstrated the existence of union threat effects operating in certain locations and industries (Corneo and Lucifora 1997; Leicht 1989). That is, industries worried about the threat of unionization (often in highly organized locales and industries) may attempt to preempt the threat by raising wages to union levels.

During the period of the labor-capital accord, strikes – representing labor's most potentially disruptive weapon in its fight over compensation issues – operated in a similar manner.[4] While research has consistently shown that union members earn, on average, higher wages than their non-union counterparts, labor analysts have also demonstrated that union members who have participated in a strike earn, on average, more pay than

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their non-striking, unionized counterparts. For example, using a 1979 employee survey, Wallace, Leicht, and Grant (1993) find that workers who had directly participated in a strike in their past had higher wages than otherwise comparable non-strikers. In a unique study focusing on the printing and publishing industry, Kalleberg, Wallace, and Raffalovich (1984) argue that strike frequency sometimes served to increase labor's share, especially during the first few decades following World War II.

Strikes may boost non-striking workers' pay as well, much like union threat effects. A firm anxious about an increasingly agitated shop floor may cave to wage demands by union members hinting at a walkout. Or an employer may think twice about resisting its union's call for increased pay after witnessing a prolonged strike drain a neighboring firm's revenue. Indeed, the positive relationship between strikes and worker compensation that Rubin (1986) finds must in part be due to these threat effects. Rubin's analysis operates at a broad sector level: Following Lewis (1963), she divides the economy into a union and non-union sector and looks at the effect of annual strike rates on these sector-specific wages. Rubin indicates that strike activity boosts worker wages within the union sector, where the institutionalized presence of labor can weather the direct impact of a dispute on workers' well-being. However, even within the high union sector, strikes remain a sufficiently rare event that in any given year the number of non-striking workers dwarfs the number of strikers. Since we know strikes do not perform miracles on the wages of workers who walk the picket line (in fact, they shrink the wages of the striking worker while he walks the picket line), the positive, significant influence of strikes on compensation must in part be due to threat effects.

If indeed strikes no longer work to boost worker wages, then we must expand our understanding of the role of union decline on worker wage stagnation during the recent past. Most of the relevant research has focused on declining membership levels and their impact on wages; results from these analyses may suggest another pathway through which the diminished fortune of the American labor movement has exacerbated wage inequality.

MEASURING STRIKES IN THE POST-ACCORD PERIOD

Reagan's hostile policies toward labor extended all the way down to government spending on labor research. Starting in 1982, budget cuts prevented the Bureau of Labor Statistics (BLS) from collecting data on strikes involving less than 1,000 workers.[5] Past work on the topic indicates that such large labor disputes – strikes involving 1,000 or more workers comprise less than 10% of all strikes – are non-representative of more typical work stoppages along a number of key dimensions (Skeels, McGrath, and Arshanapalli 1988). According to this research, no size-selected sample of strikes in the postwar period results in a representative sample of all work stoppages. The authors conclude with the admonition: "Be aware that sampling by strike size may be hazardous to your intellectual health" (Skeels, McGrath, and Arshanapalli 1988: 589).

To a great degree labor researchers have heeded their warning and quantitative work on strikes in the post-accord period has all but disappeared (for exceptions, see Morris 2003; Dixon and Roscigno 2003). In the only cross-industry, time-series analysis of the impact of strikes on wages during the 1980s, Wallace, Leicht, and Raffalovich (1999: see especially 284) conclude that strikes have become virtually irrelevant for the

economic standing of workers. Circumventing the biased BLS strike data, the authors

utilize monthly Current Population Survey (CPS) data on the number of employees

involved in work stoppages and calculate a measure capturing the percentage of working

time lost due to strikes on a quarterly basis. While their analysis is suggestive of a

decoupling of the wage-strike relationship in the post-accord period, the reliance on CPS

work stoppage data presents a number of shortcomings.

First, data on the number of workers involved in a labor dispute in a given period

conflates two somewhat separate issues: strike frequency and strike size. While the two

are obviously correlated, the influence of an atypically large strike could easily bias strike

activity estimates. For example, a period in which 1,000 strikes erupted at various

industries in firms that employed, on average, 60 employees each would look exactly the

same as a period in which one huge labor dispute occurred, like the California grocery

strike of late 2003. Yet certainly the effects of 1,000 strikes of typical size spread across

various industries and regions would differ from the concentrated impact of one large

labor dispute. Data only on number of workers involved make no such distinction.

Second, their final measure of strike activity – working time lost due to strikes – confuses

average strike duration with average number of strikes. Again, the two are correlated, but

in a period where a work stoppage of any size or duration is an increasingly rare event,

one atypically prolonged dispute could provide a misleading picture of strike activity.

More importantly, Wallace et al (1999) are unable to analyze their data by region or

industry, and their analysis operates at the level of the entire private sector. Since strikes

occur so infrequently nowadays, it would be quite surprising if they were to find a

significant effect of strikes (either positive or negative) on all private sector wages.

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To avoid the data problems inherent in either the BLS or CPS estimates of strike activity, this paper utilizes data provided by the Federal Mediation Conciliation Service (FMCS) on nearly every authorized labor dispute occurring in the United States between 1984 and 2002. The data contain information on strike location, strike size, strike duration and strike date. In addition, the FMCS provided employer and product information that enable me to link each dispute with its appropriate 3-digit Standard Industrial Classification (SIC) code.[6] Such a rich dataset allows for an analysis of the effect of strikes on worker wages at the detailed industry-region level.

Moreover, the data allow for varying definitions of the strike. Given firm and intra-firm bargaining practices in the United States, sometimes two or more unions will represent different sectors of the same firm.[7] If the unions decide to strike, both sets of workers may walk off simultaneously, but technically the action still represents two distinct work stoppages. Or, two sets of unionized workers at two different companies in the same town may coordinate and stage a walkout. Again, given firm- and union-specific bargaining practices, such a walkout would technically constitute two different strikes. The detailed work stoppage data utilized in the following analyses allow for more flexible definitions of a strike. In the following analyses, I create a strike measure that collapses intra-firm work stoppages, and another that collapses intra-industry work stoppages that occur in the same vicinity.

Regardless of how one defines a strike, their frequency declined dramatically during the last few decades. During this time of labor retreat, wages for non-professional, non-managerial workers remained rather stagnant, recovering to mid-1980s levels during the late 1990s economic boom.   Figure 2 plots annual strike frequencies

(introduced earlier in Figure 1) and median full-time worker compensation between 1984 and 2002.[8] The late 1990s uptick in worker pay still left the typical worker little better off than he was in the mid-1980s.

[INSERT FIGURE 2 NEAR HERE]

Research has shown that despite the steep drop in overall unionization levels, unions continue to boost worker pay in sectors where their presence remains substantial (Rosenfeld 2004; see Card 1998 for union effects on wages in the public sector). What has yet to be sufficiently demonstrated is whether strikes fit the same pattern. The results of the analysis could confirm Wallace et al's contention that strikes have no impact on worker wages during the post-accord period. The typical strike during the 1980s and 1990s could simply be labor's last-ditch attempt to restore the status quo in the face of wage and benefit erosion. Or, strikes could still positively affect the wages of those unionized workers directly involved in a dispute, yet represent such a rare occurrence that non-striking firms no longer fear the threat of a stoppage. Either scenario could result in a non-significant relationship between strikes and wages at the industry-region level. On the other hand, strikes could still exert a significant and positive effect on worker pay, especially – as Rubin's work suggests (1986) – within those industries where labor retains a strong (if crumbling) institutional base. Given the paucity of these industries in the post-accord period, such a finding is likely to be detected only in data disaggregated by industry and region.

The paper also investigates the role of strikes in reducing intra-industry pay differentials among workers. While work stoppages may no longer increase average worker pay, they may serve to narrow pay gaps within particular regions and industries.

Swenson (1989), in his discussion of the Swedish and German trade labor movements, emphasizes how distributional issues, and not simply demands to increase worker's share, often trigger strike activity. It is plausible that in this era of labor retreat, strikes operate to narrow worker pay dispersion without affecting labor's overall share.

Finally, it may be that the labor-capital imbalance has grown to such an extent that strikes now operate to push worker wages downward. Cramton and Tracy (1998) show how, following on the heels of the PATCO dispute, employers' use of replacement hiring during work stoppages grew substantially. In another article, Cramton and Tracy (1992) provide data on labor's increasing use of the holdout in contract disputes, perhaps fearing the growing consequences of engaging in an actual strike. The permanent firing of striking workers, coupled with the hiring of replacements at lower wage rates, could dampen overall industry pay. Similarly, the threat of permanent replacement may force striking employees back to work for lower wages, providing another mechanism through which strikes could operate to lower worker pay.

This paper provides the first comprehensive test of the effect of strikes on wages in post-accord America. Past research has established that the relationships between strikes and wages are historically and spatially contingent. What no research has adequately done is establish how these relationships changed following the breakdown of the labor-capital accord.

DATA

The strike data used in this analysis come from the Federal Mediation Conciliation Service (FMCS). With the exception of the airline industry (which falls

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under the jurisdiction of the National Mediation Board), parties unable to resolve contract

differences must file with the FMCS prior to any work stoppage.[9] The strike data

provided by the FMCS contain detailed measures on the number of workers involved in a

dispute, duration, location, date, employer name, union name and company product. One

possible research strategy would be to link these firm-level strike data to firm-level data

on pay rates and unionization levels. Unfortunately, no such comprehensive dataset

exists for the striking FMCS firms. Moreover, a firm-level analysis would only capture

the costs or benefits of striking for the striking workers. This analysis uses detailed

industry-level workforce information, available in the Current Population Survey (CPS).

The aggregated nature of the data captures both the direct effects of a strike on wages, as

well as threat effects operating at the industry-regional level.

What these FMSC data exclude are so-called wildcat strikes, or strikes

unauthorized by the union. Evidence suggests that the frequency of these types of strikes

has been on the decline for decades; in 1980, they made up only 13% of all strikes

(McCammon 1990). Whether or not the proportion of wildcat strikes has increased

during the recent period of labor's retreat is beyond the scope of this paper (see Byrne

and King 1986 for a discussion of wildcat strikes in the manufacturing industry).

Since the FMCS does not organize their strike data by industry, I had to

categorize the data myself. Using a four step process that made use of company

information, product information, and the union involved, I assigned each strike a 3-digit

Standard Industrial Classification (SIC) code.[10] The first step in the categorization

process involved assigning SIC codes to those strikes whose company names appear in

Dun & Bradstreet's *Million Dollar Directory*. The directory categorizes major

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companies in the United States by primary 4-digit SIC code, along with any secondary codes that may apply. However, many large companies span several industries, and many of the firms in the strike database had either changed names, dissolved, or otherwise do not appear in the D&B listing. Step two in the process involved linking specific product searches to SIC codes. If the product listing was missing or ambiguous and the firm did not appear in the D&B database, I then would search for any company information on the internet, making use of the detailed addresses of the companies contained in the FMCS data. Finally, for the handful of strikes that eluded classification after steps 1-3, union information was used to assign a 3-digit SIC code.[11] Given that the models presented below only make use of 1- and 2- digit SIC codes, any minor coding errors at this stage were probably eliminated after aggregation.[12]

Due to changes in employment levels within and across industries and regions over time, a raw measure of total strikes by industry would present a misleading measure of changes in strike activity. To control for employment levels and shifts, this analysis uses CPS Merged Outgoing Rotation Group (MORG) files to construct industry and region-specific measures of employment.[13] I restricted the employment numbers to non-professional, non-managerial workers – i.e., those workers at risk of a work stoppage.[14] I then constructed a specific strike rate for each industry-region cell by merging the CPS employment figures with the strike data. The strike rate represents the total number of strikes occurring in a particular industry and region in a particular year divided by the total number of non-professional, non-managerial employees in that industry-region cell for that year. I also constructed various measures of average strike severity, such as average strike size and duration, in order to test whether the effect of strike frequency on

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wages differed after controlling for average size and length in each industry-region grouping. Such measures also allowed for specific examinations of whether the various measures of strike activity commonly found in the literature (such as total number of persondays lost or a raw strike frequency measure) affected wages differently during the post-accord period.[15]

Table 1 presents strike rates and unionization levels averaged across years and regions for the major industries used in the analysis. Unionization data also come from the CPS's MORG files. Since my primary analysis is at the industry-region level, I aggregate each year of MORG individual-level data into a final dataset with cell entries for each industry-region grouping.

[INSERT TABLE 1 NEAR HERE]

As shown in Table 1, the main analysis divides industries into their 1-digit SIC code designation. For the highly unionized sample, the data are disaggregated down to a combination of 2-digit SIC codes and CPS codes (see bottom half of Table 1 for industry listing).

Besides strike rates and unionization levels, other covariates that could affect changes in non-professional worker pay over time include a range of demographic characteristics such as the racial makeup of the workforce, gender composition, potential experience (age), and educational levels. Human capital theory posits that increasing education levels should translate to wage gains for both workers and managers. Controlling for levels of education, the relegation to lower-paying tasks within occupations should depress median pay in high-minority or high-female cells. To control for these effects and others, the final dataset includes items capturing the percentage of

minority workers within each industry-region grouping, the percentage female, educational attainment scores (separate items for high school dropouts, high school graduates, and those with any college experience), and age measures. This information also comes from the MORG files. Table 2 presents descriptive measures of the control variables used in the analysis at three points in time in order to provide a general picture of recent workforce trends. This table is averaged across all SIC-1 digit industry divisions.

[INSERT TABLE 2 NEAR HERE]

As displayed above, female representation among the non-professional, full-time workers held steady during the period analyzed in this paper. The percentage of minority workers grew substantially, largely mirroring immigration trends. Average educational levels of non-professional workers also grew: By 2002, the vast majority of non-professional, non-managerial workers had completed high school, and a large segment had some college experience (calculated by adding the high school dropout and graduate categories and subtracting from 100). Table 3 below presents similar measures for the restricted, high union sample. Wages in these industries remain higher than in the economy as a whole, but have yet to recover to their mid-1980s levels. The high union sample is somewhat older, and heavily male in comparison to the full sample dataset.

[INSERT TABLE 3 NEAR HERE]

In addition to the control variables presented above, some of the models also include a region-specific unemployment measure, included to capture the general effect of business cycle trends on median worker pay. Regional unemployment numbers come from the Bureau of Labor Statistics (BLS). Finally, given that historically unions

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concentrate in monopoly sectors where employers can more easily pass on the union

wage premium to consumers, I include a median managerial pay measure for each cell.

The measure should partially control for the effect of higher average firm rents on wages,

which, if systematically related to strike frequency, could bias the strike rate effect.

The dependent variable used in all models (also created by aggregating

individual-level MORG data) is the median weekly wage for non-professional, non-

managerial, full-time workers in each industry-region cell. Wage measures are expressed

in constant 2002 dollars. As Table 2 reveals, the educational gains of workers did not

translate into much wage growth during the 1980s and 1990s. Even the late-1990s

economic boom barely raised non-professional worker pay above its mid-1980s level.

Each cell in the data represents one industry-region grouping for a particular year

(for example, wholesale trade workers in the South in 1992). For the main models using

the data organized by SIC-1 digit codes (results presented in Table 4 and Table 5), the

final sample size is 576 (8 industries by 4 regions by 18 years). For the restricted, high-

union dataset, the final sample size is 1,008 (14 industries by 4 regions by 18 years).

Sample sizes do vary across models due to differing model restrictions, such as whether

or not the data are limited to cells with union representation rates above 25%.

METHOD

To best estimate the impact of strikes on weekly worker pay, I fit a fixed-effects

model that includes a separate effect for each industry-region and a time effect for each

year (in this case, year ranging between 1984 and 2002). For industry i at time t,

$$y_{it} = \alpha_i + \nu_t + \mathbf{x}'_{it}\beta + \varepsilon_{it}$$

where α_i is the fixed industry-region effect, ν_t is the fixed year effect, x'_{it} represents the exogenous variables in industry-region i at year t, and ε_{it} is the error term. The fixed effects control for unmeasured differences between industry-regions that remain constant over time, and control for forces that affect the various industry-regions equally but vary over time. For instance, the fixed industry-region effects control for unmeasured industrial characteristics like average industrial concentration or the lower wage scales attributable to regional differences, whereas the year fixed-effects capture the influence of a macro-level economic event like the recession of the early 1990s.

Thus, in its most stringent specification, the model used in this analysis only allows for variation across time within specific industry-regions. Such a specification effectively controls for many of the dominant factors that influence wages. Unlike past analyses, the data and model utilized in this paper directly target the effect – if any – of strike rates on worker wages within particular industries and regions. Also, unlike most research on the topic, the strike rate measure used in the models presented below is lagged one year. Lagging strike activity accomplishes both statistical and theoretical goals. For starters, using a lagged measure partially controls for potential endogeniety issues.[16] Aside from this, specifying a lagged strike rate makes practical sense. A strike always lowers the wages of workers involved during the strike itself, and a strike typically occurs following contract expiration (see McCammon 1994). The net gain of strikes on wages – if any – will not be felt until the strike has concluded *and* the employees begin working under a new contract. An unlagged strike activity measure captures both the wage loss due to the strike itself, along with any long-term work stoppage effects (whether gains, boosts, or no effect at all). Consequently, the use of an

unlagged strike measure confuses short and long-term effects of labor disputes. Since the

FMCS strike data begin in 1984, in order to allow for the lagged strike rate item, all other

covariates in the model begin in 1985.

RESULTS

Models 1 through 3 in Table 4 below present results based on SIC l-digit divisions

(see Table 1 for industry breakdowns). The sample analyzed here includes both highly

unionized industries, such as mining, as well as those industries historically lacking an

institutionalized labor presence, such as retail trade. I estimate these models in order to

provide a baseline comparison with Rubin's (1986) and others' work on this subject.

Like nearly all the macro-level work on this topic to date, Models 1 and 2 in Table 4 lack

industry effects. Instead, the data cover the entire private sector.

[INSERT TABLE 4 NEAR HERE]

Model 1 in Table 4 above includes a linear year term, a percent unionized

variable, demographic controls, managerial pay, and a regional unemployment measure.

The large negative coefficient (significant at the .001 level) on the female representation

variable indicates that a percent increase in the number of females is associated with a

$3.82 drop in average weekly compensation. Age and higher education levels work, as

expected, to boost median weekly compensation. Also, in Model 1 a percent increase in

union representation increases median worker pay by about $1.63. The union coefficient

is significant at the .001 level, revealing that, according to this model, during the 1980s

and 1990s unions continued to increase average worker compensation despite its eroding

institutional base.

Somewhat surprisingly, in Model 1 the strike rate coefficient – representing the effect of the main variable of interest in the analysis – is significant and negatively associated with median worker pay.[17] There is reason to distrust this finding, however. The linear year term does not adequately account for non-linear time trends in worker pay between 1985 and 2002, nor does the model control for industry effects. In Model 2 I introduce a separate period effect, which controls for all unmeasured time-varying non-linear effects on wages. Following this further specification, the regional unemployment measure is insignificant, as the introduction of a year fixed-effect adequately controls for annual changes in unemployment rates.[18] After the inclusion of the year fixed-effects, the strike rate coefficient remains significant and negative. Without industry effects, however, it is hard to discern whether work stoppages actually operate to bring wages down, or whether strike activity remains concentrated in those industries which experienced the greatest wage losses over the past few decades.

Model 3 adds an industry-region effect, which renders the strike rate coefficient non-significant. The inclusion of industry-region fixed effects boosts the R^2 and straightens out some of the puzzling coefficients found in Models 1 and 2. Now, the percent minority variable negatively affects worker wages, lowering them by $3.83 for every percent increase in African-American or Hispanic representation. High school dropouts earn significantly less than individuals with some college experience, while high school graduates are only marginally worse off than the college educated.[19] Other covariates trend in the expected directions, and the positive union effect holds up strongly despite this further specification.

Following Rubin (1986), Models 1 and 2 of Table 5 divide the economy into union and non-union sectors. As Rubin (1986) discusses, a large union presence in an industry can better insulate its workers from the effects of a strike, and strikes in historically high-union industries tend to be more costly to employers.[20] The FMCS data indicate that from the mid-1980s to 2002, over three-quarters of all strikes occurred within the highly unionized sector (see Rubin 1986; Lewis 1963 for a discussion of the union and non-union sectors and the industries that comprise them).[21] Restricting the data to high strike industry-region groupings provides an initial test of whether, following Rubin's work (1986), the positive relationship between strikes and worker wages exists only in the highly unionized sector of the economy.

[TABLE 5 NEAR HERE]

Both models in Table 5 above include industry-region and year fixed-effects. Again, the data are divided into 1-digit SIC code industry divisions. Corroborating Rubin's claims for the low union sector, no significant relationship exists between strikes and wages. Model 2 of Table 5 provides the first detailed, updated test of whether strike activity continues to positively influence wages in the unionized sector in the post-accord period. As shown, after controlling for industry and region effects, the model reveals a complete de-coupling of the strike / wage relationship. The strike rate coefficient is negative and not significant. However, it could be that the broad 1-digit industry groupings obscure strike effects only detectable at a more disaggregated level.

For the models presented in Table 6 below, the data are restricted to fourteen industries within the highly unionized sector (see Table 1 for industry breakdowns). Model 2 in Table 6 further restricts the sample to those industries with union

representation rates above 25%, and Model 3 includes only those industry-region cells with unionization rates above 25% that registered a strike.[22] That is, Model 3 restricts the data to those industry-regions with a large union presence that actively engages in work stoppages.[23] The results of these models should reveal if any positive relationship still exists between strike activity and worker wages, given that the data are restricted to those industries where the labor movement remains relatively institutionalized *and* active.

[INSERT TABLE 6 NEAR HERE]

Model 1 of Table 6 provides no evidence for the existence of a wage-strike relationship during the post-accord period. The strike rate coefficient is negative and not significant. Model 2 restricts the sample to those industry-region cells with union representation rates above 25%. Again, the strike rate coefficient fails to reach statistical significance, and remains negatively signed. Other covariates trend in the expected directions, although the education and age items are no longer significantly related to worker pay. I tested various measures of strike activity (persondays lost, total number of workers involved, raw strike counts, etc.) for their effects on weekly worker wages.[24] None of these variables – either inserted into the model alone or in combination with other strike measures – had a significant influence on worker pay.

If any positive wage-strike relationship exists in post-accord America, Model 3 of Table 6 should capture it. The sample for Model 3 only includes those industry-region cells with a well-institutionalized union presence that regularly strikes. Still, despite this final specification, the strike rate coefficient remains negative and fails to achieve significance. All other covariates in the model operate as expected.

Finally, it may be that while strike activity fails to increase average pay, it reduces intra-industry and region wage differentials. As other research has argued, in certain institutional settings workers will strike not to increase labor's share, but to reduce wage dispersion within the working class (Swenson 1989). Given sample size considerations for the workforce measures, the data used in this analysis are not ideal for testing wage inequality effects. For this reason, the models in Table 7 below provide rough estimates of strike activity's impact on the 80 / 20 weekly pay ratio for workers, as opposed to other measures of inequality.[25]

[INSERT TABLE 7 NEAR HERE]

All models in Table 7 are restricted to those industries within the highly unionized core with union representation rates above 25%. Somewhat surprisingly, the first model of Table 7 reveals a positive association between strike activity and weekly worker pay, although the effect is only marginally significant. Greater unionization, as well as higher median worker pay, works to reduce within industry-region wage inequality. The next two models tease out exactly how strike levels might increase the 80 / 20 weekly pay ratio. The second model indicates that strike activity has no effect on the upper distribution of worker wages, indicating that work stoppages must lower the wage rate of those in the bottom portion of the pay distribution. Indeed, the final model in Table 7 shows a significant, negative effect of strikes on the 20th percentile of worker wages.

Caution should be taken from reading too much into the results from Table 7, however, given the instability of the 80th and 20th percentile measures, as well as the rather modest strike effect.[26] The results from Table 7 do indicate no *positive* role of strikes in reducing wage inequality in post-accord America. While unionization still

seems to boost lower-paid worker wages (a percentage point increase in the unionization rate boosts low-paid workers' wages by about a $1.00), strikes no longer seem to be an effective weapon in labor's arsenal in reducing inequality. Combined with the results from Tables 4 – 6, these findings suggest that at the industry-region level, strikes have had no significant effect on worker pay in the post-accord period.

To ensure all findings are robust, I utilized various other model specifications for Tables 4 - 7. It is well established that, net of other contributing factors, larger firms offer their employees higher wages, although the effect seems to be diminishing over time (Hollister 2004). If average firm size within particular industry-region groupings is systematically related to strike activity, the exclusion of a firm size measure could bias the effect of strike activity on wages. The CPS MORG data do not contain firm size information; however, beginning in 1989, the March CPS survey includes an item asking about employer size. As a check of robustness, all models presented in the analysis were rerun on the truncated (1989-2002) time series with the firm size item: results do not differ and the main models are presented in Appendix 2.

Autocorrelation is often a problem in time-series analyses. As a precautionary measure, I reran all the models with a lagged dependent variable on the right-hand side of the equation, effectively changing the dependent variable from a wage level to a wage change rate. Results remained substantively similar to those presented in the paper, and are available upon request. Standard augmented Dickey-Fuller tests were run on all panels in both the full sample and high union sample; in only a few panels was there evidence of a unit root. I then ran the main models without these panels; results remain substantively similar and are presented in Appendix 3.

DISCUSSION

The analyses presented above use information on nearly every strike in America since 1984 to test whether the previously established positive link between strike activity and worker compensation held following the breakdown of the postwar labor-capital accord. All models presented in the paper reveal that the positive influence of strike activity on wages – whether restricted to the entire workforce, the highly unionized sector, or to those industries within the highly unionized sector that maintain the strongest union presence – has disappeared. Indeed, in sharp contrast with previous research (see, for example, Rubin 1986), strikes no longer affect workers' wages even in those industries with a well established union presence. Strike activity also fails to narrow worker pay dispersion.

This paper expands upon the existing literature in numerous ways. Reagan-era BLS budget cuts severely restricted strike data availability, and consequently, research on the topic has nearly disappeared. Since the cuts, the little research done on strikes has been limited either to individual case studies (see Dixon and Roscigno 2003 for an example) or restricted to those conflicts involving 1,000 or more workers (see Skeels et al 1988 for a discussion of why these disproportionately large stoppages are non-representative of typical labor disputes). Wallace, Leicht, and Raffalovich's (1999) research represents the only investigation of the strike-wage relationship in the post-accord era. However, the authors' reliance on a persondays lost strike measure as well as their data's high level of aggregation render their strike related findings preliminary, at best.

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Given the drastic decline in strike activity over the past twenty years, any strike effect will probably be felt at a highly disaggregated level. The analyses in this paper take advantage of previously unreleased data on nearly every work stoppage in the United States between 1984 and 2002. By linking this detailed strike data to workforce information at the industry-region level, these analyses represent the most comprehensive and rigorous tests of the strike-wage relationship during the 1980s and 1990s. Indeed, given the level of disaggregation, these analyses represent the most comprehensive test of strike-wage effects found in the literature for any period.

Past research has shown that the effectiveness of strikes depends upon labor's ability to protect its employees from the adverse consequences of a work stoppage (Rubin 1986). In order to emerge successfully from a strike, workers require adequate strike funds and the institutional muscle to prevent capital from hiring replacement employees, coupled with a supportive public willing to hold the strike line (if necessary). The analyses presented in this paper hint at a breakdown of these factors – even in those models restricted to the most highly unionized industries. The findings also reveal another pathway through which declining union power has exacerbated wage inequality in the recent past, with strikes no longer working to raise average worker pay at the industry-region level.

However, given the level of data aggregation, these tests cannot detect the typical outcome of an individual strike. It may be that in this era unions call for strikes only when faced with the most desperate circumstances, such as a plant closing, rapid downsizing, or severe pay cuts. Even a "successful" strike under such adverse circumstances is only likely to restore the status quo, and unlikely to translate into real

wage growth for the workers involved. Or, strikes may now be a sufficiently rare event

that even a strike settlement that does lead to wage increases for the workers involved

fails to reverberate across similar firms, leading to a minimal overall effect on wages.

That is, given their relative infrequency, employers may no longer feel threatened by

strikes. Finally, the evidence presented in the paper may indicate an increased use of

lockouts by employers. No work stoppage data distinguishes between strikes and

lockouts, so the lack of a positive strike effect may reflect the increased willingness of

employers to lock out their workers in order to secure wage cuts and other concessions.[27]

In reality, a combination of factors probably contributed to the erosion of the

strike-wage relationship in the 1980s and 1990s. While this paper establishes that at the

industry-region level – even industries and regions once considered firmly within the

union sector – the positive strike-wage link has been severed, future research should

focus on a representative sample of individual firms in order to reveal exactly what

occurs following a work stoppage. Only time-series firm-level data can bring to light the

precise effects an individual work stoppage has on wages for the employees of the

striking firm.

One implication from this paper is the need to rethink our theoretical

understanding of strike predictors in the United States. That is, given the seeming lack of

a wage benefit attributable to strikes, why do they still happen? It may be that the strike

predictors of old (including business cycle conditions such as the unemployment rate,

inflation rate, as well as organizational factors, such as levels of unionization) may no

longer explain strike activity all that well.

The results of these analyses also point to the need for future theoretical work on macro-level trends in strike activity within industrial nations. Previous research posits an inverted U-shaped curve where strike activity increases during the early stages of working class organization, levels off as labor becomes firmly institutionalized within the polity, and then declines as the working class shifts its focus to the political arena (Korpi and Shalev 1979; Korpi and Shalev 1980). Other work suggests that strike activity will decrease with the expansion of the welfare state and welfare benefits. If the market regains its place as the site of distributional struggles following welfare benefit cutbacks, strike activity should increase (Hibbs 1978: see especially page 175).

While perhaps illustrative of tendencies in certain European nations, neither scenario effectively illuminates trends in strike activity in post-accord America. Strikes are an increasingly rare event, but not because of a robust labor movement content to battle capital in the political realm.[28] In contrast to Hibb's contention (1978), if anything, welfare entitlements were scaled back in the 1980s and 1990s and wages were increasingly left prey to purely market forces, yet strikes continued to decline. To adequately capture trends in America, future theoretical work must contend with a withering labor movement, a retreating welfare state, and the decline of the strike as a common weapon used in industrial disputes.

Future macro-level quantitative work should pay close attention to the ongoing rumblings within the American labor movement itself. Just as the political and economic transformations of the early 1980s triggered dramatic changes in the relationships between the union movement, strikes, and wages, current re-alignments may lead to new directions in unions' focus and union effects. For example, new research suggests that

certain unions actively avoid NLRB election campaigns whenever possible, viewing

them as cumbersome, time-consuming affairs that grant management the upper hand (see,

for example, Sharpe 2005). Innovative unions may increasingly seek ways to avoid the

confines of federal regulations when considering sending their members out on strike, or

the strike itself may be replaced with new and less easily defeated bargaining strategies.

CONCLUSION

Representing labor's most potent weapon in its arsenal in decades past, the typical

strike was called during periods of contract re-negotiation to disrupt the productive

process at the site of production itself. Substantial strike funds coupled with the damage

prolonged work stoppages caused employers – especially in capital-intensive industries –

enabled a well-established labor movement to use the strike to achieve wage gains for its

members. Research detected these gains even at extremely high levels of data

aggregation.

Labor's steep slide during the past few decades has resulted in post-war lows in

union representation. Nonetheless, union presence within an industry still translates into

higher wages compared to industries and regions lacking labor representation. Strike

activity, on the other hand, no longer positively influences worker pay at the industry-

region level. Strike activity also fails to translate into a narrower wage distribution for

workers within particular industries and regions. Unions may call for a strike on such

rare occasions that whatever effect the dispute has on the workers directly involved fails

to influence industry and regional pay scales. Employers – emboldened by Reagan's

firing of the air traffic controllers in the PATCO strike – may increasingly use lockouts or

other strategies to squeeze wage concessions from their workers. Union leaders, sensing

the tides shifting toward capital in their long-running campaign to secure strong benefit

packages and wage rates for their members, may simply refrain from striking except in

the most desperate situations. Whatever the specific circumstance, the general effect of

the once-powerful strike has withered away, rendering an already uneven battle that

much more lopsided.

ENDNOTES

1. What date constitutes the turning point in labor-capital relations is an empirical matter that depends on the particular issue under investigation (union membership, strike frequency, labor's representation within the political arena, etc.). However, by the mid-1980s – the starting point for this investigation – labor is in full retreat regardless of what measure of worker strength one is interested in investigating.

2. Airline work stoppages represent the only significant category of strikes missing from the data file, since airlines fall under the jurisdiction of the National Mediation Board and the data used here come from the Federal Mediation Conciliation Service. See the data section of the paper for more details.

3. Like other data on strikes, these data do not distinguish between union-directed walkouts and employer lockouts. I discuss implications of this data limitation later in the paper.

4. Prior to the state's legitimation of the union movement in the 1930s, about 10% of striking workers were not actually union members. By the postwar period, nearly all striking workers belonged to unions (Kaufman 1982), although anecdotal evidence suggests this pattern may be changing.

5. Prior to the cuts, the BLS kept detailed records of every work stoppage involving at least 6 employees.

6. As labor researchers know, government agencies began phasing out the SIC system in favor of the North American Industry Classification System (NAICS) in the late 1990s. Given that most of the data used in this analysis date prior to the conversion, the SIC codes were preferred.

7. As when, say, one union organizes the checkout clerks at a grocery, and another organizes the meat cutters.

8. Figures, tables, and models presented in this paper define a strike as firm specific – that is, I count as one strike when two or more sets of unionized workers stage a simultaneous walkout from the same firm. I do not collapse intra-industry, intra-regional walkouts for the purposes of this paper; however, all models were run according to varying strike definitions with similar substantive results.

9. As specified in section 8d of the National Labor Relations Act (NLRA).

10. About 10,000 strikes had to be categorized in all.

11. The author is aware of the danger of this practice, what with, for example, the UAW now organizing graduate students. The practice was only used for a few strike classifications, with knowledge of the common cross-industry unionization practices.

12. For example, if a striking firm is misclassified as occurring within the metalworking machine industry (3-digit SIC code 354) when it properly belonged to the general industrial machine industry (3-digit SIC code 356), it will not matter in an analysis where all firms producing machinery are grouped together.

13. Regional groupings disaggregated by state are presented in Appendix 1. MORG files were preferred over March files due to the greater sample size of the MORG survey.

14. Except for the few professions that regularly strike, such as nurses and journalists.

15. Rubin's work (1986) suggests that when it comes to affecting labor's share, strike frequency is of greater importance than total persondays lost due to strikes. The detailed FMCS data analyzed here allow for an updated test of this assumption.

16. Unless the strike and wage data are collinear over time. Given the extremely low bivariate correlations between wages and the various strike measures, this was not a major concern.

17. While the coefficient seems large, the actual magnitude of the effect on wages – given the strike rate variable's distribution – is actually quite small. A -1000 coefficient, for instance, translates into about a $1.00 decrease in weekly wage compensation.

18. The inclusion of year effects in the subsequent models always renders the regional unemployment rate non-significant. It is therefore dropped from the models presented in Tables 5, 6, and 7.

19. The lack of a strong college versus high school graduate wage premium is probably due to the occupational composition of the sample, which is limited to non-professional, non-managerial workers.

20. Strikes are not as costly to employers in labor-intensive industries, such as retail trade, where labor representation remains minimal, as they are to employers in capital-intensive industries, such a machinery, where union representation is relatively high (Rubin 1986).

21. This sector includes industries such as mining, construction, transportation, manufacturing and sanitary services / public utilities.

22. The six industries with average union membership rates above 25% averaged across all years include metals, transportation equipment, food / tobacco, transportation, communications, and utilities / sanitary services. While the 25% cutoff is admittedly somewhat arbitrary, I also ran models including a variety of other cutoff points (20%, 30%, 33%, etc.). In none of these models was the strike rate coefficient significant.

23. Roughly 17% of the high union industry-region cells did not register a strike in a particular year.

24. Results from these tests not shown in the tables but available upon request.

25. Ideally, one would use gini coefficients or a 90 / 10 pay ratio. Topcoding and small sample sizes in the MORG files prevent me from constructing a reliable measure of these inequality variables.

26. Both in terms of statistical power and substantive importance: Predictions (not shown, available upon request) indicate that if no strikes had occurred in the 1984 – 2002 period, workers in the 20th percentile of the wage distribution would have enjoyed about a 1% boost in their compensation.

27. Historically, since employer-based lockouts occurred so rarely, this was not so much of a problem. However, it could be that since the erosion of the labor-capital accord, the use of lockouts as a bargaining tactic has increased substantially. See Cramton and Tracy (1998).

28. Korpi and Shalev (1979: see pages 179-180) argue that such a scenario depends on a highly organized working class with a well-established, secure place in the polity. The current American labor movement is - historically speaking - neither well-organized nor a particularly effective political actor.

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Table 1. Strike rate and union representation for selected industries, all years:

Full Sample:

Industry	Strike rate	% Unionized
Mining	.015	19.1
Construction	.010	23.5
Manufacturing	.020	24.7
Transportation	.006	35.7
Wholesale trade	.009	8.1
Retail trade	.001	6.4
Finance, insurance, real estate	.001	4.1
Services	.003	8.0

Highly Unionized Sample:

Industry	Strike rate	% Unionized
Mining	.015	19.1
Construction	.010	23.5
Lumber/wood/furniture	.029	18.7
Metals	.050	34.3
Machinery	.020	18.4
Transportation equipment	.012	47.3
Toys/misc. manufacturing	.012	12.8
Food/tobacco	.019	30.3
Textiles/apparel	.005	14.6
Paper/publishing	.011	22.9
Chemicals	.020	21.2
Transportation	.006	32.3
Communications	.004	40.6
Utilities/sanitary services	.009	40.8

Note: Strike rate represents total number of strikes per thousand non-professional, non-managerial workers.

Means are weighted by cell counts.

Table 2. Weighted means of workforce demographic and wage measures: full sample

	1984	**1993**	**2002**
Non-professional, non-managerial workers:			
Median weekly wage*	$500.16	$480.46	$510.00
Percent unionized	20.4	15.0	11.9
Percent female	42.4	43.8	43.1
Percent minority**	17.3	20.7	26.5
Percent < HS	19.5	14.6	14.2
Percent HS	69.6	43.7	39.9
Age	35.7	37.1	38.6
Managers and executives:			
Median weekly wage	$894.53	$871.66	$952.04

*All wages are expressed in constant 2002 dollars.
** Minority category restricted to African-Americans and Hispanics.

Table 3. Weighted means of workforce demographic and wage measures: high union sample

	1984	1993	2002
Non-professional, non-managerial workers:			
Median weekly wage*	$597.60	$561.47	$586.45
Percent unionized	32.9	26.0	21.4
Percent female	28.5	27.7	23.4
Percent minority**	17.4	20.7	26.3
Percent < HS	23.6	17.9	17.1
Percent HS	68.6	48.6	45.3
Age	36.9	38.2	39.7
Managers and executives:			
Median weekly wage	$1081.45	$1030.66	$1097.57

*All wages are expressed in constant 2002 dollars.
** Minority category restricted to African-Americans and Hispanics.

Table 4. Baseline models predicting median weekly pay.

	Model 1	Model 2	Model 3
Lagged Strike Rate	-525. 31*	-1002. 77***	162. 17
	(242. 31)	(194. 24)	(145. 33)
Year	-6. 82***	——	——
	(. 67)	——	——
Regional Unemployment	-5. 29**	1. 65	. 69
	(1. 59)	(2. 09)	(1. 08)
Managerial Pay	. 08***	. 07***	. 05***
	(. 02)	(. 01)	(. 01)
Worker Variables:			
Proportion Unionized	162. 76***	241. 93***	609. 17***
	(25. 24)	(20. 27)	(39. 01)
% Female	-381. 94***	-405. 97***	-232. 10***
	(14. 94)	(11. 84)	(50. 96)
% Minority	182. 06***	30. 19	-383. 20***
	(30. 46)	(26. 11)	(48. 75)
Less than HS	-744. 11***	-743. 62***	-440. 31***
	(44. 77)	(35. 31)	(52. 66)
HS Degree	-27. 77	-392. 94***	-38. 47[a]
	(19. 90)	(28. 00)	(21. 95)
Age	110. 53***	102. 88***	41. 38**
	(22. 57)	(18. 23)	(13. 83)
Age^2	-1. 29***	-1. 19***	-. 47**
	(. 31)	(. 25)	(. 18)
Year fixed-effects	No	Yes	Yes
Industry-region fixed-effects	No	No	Yes
Constant	11971. 29***	-1348. 21***	-321. 15
	(1507. 05)	(337. 65)	(261. 40)
N	576	576	576
Number of parameters	12	29	60
R^2	. 88	. 93	. 98

Note: Standard errors are in parentheses. Dependent variable in all models is the weekly median wage for non-professional, non-managerial workers. Wages in constant 2002 dollars. Models weighted by cell counts for each industry-region.

[a]p < .10   *p < .05   **p < .01   ***p < .001

Table 5. Fixed effects models predicting weekly worker pay: full sample.

	Model 1	Model 2
Lagged Strike Rate	96. 69	-22. 78
	(275. 06)	(177. 61)
Median Managerial Pay	. 04*	. 02
	(. 02)	(. 02)
Worker Variables:		
Proportion Unionized	282. 76*	431. 99***
	(113. 99)	(66. 37)
% Female	-92. 89	-381. 08***
	(64. 58)	(84. 08)
% Minority	-406. 71***	-199. 71**
	(60. 69)	(69. 80)
Less than HS	-424. 42***	-475. 11***
	(68. 20)	(75. 93)
HS Degree	-112. 69**	71. 16*
	(36. 74)	(33. 96)
Age	58. 65***	-7. 97
	(16. 06)	(27. 33)
Age^2	-. 67**	. 15
	(. 21)	(. 36)
Year fixed-effects	Yes	Yes
Industry/region fixed-effects	Yes	Yes
Restricted to low union industries	Yes	No
Restricted to high union industries	No	Yes
Constant	-623. 78[a]	671. 96
	(324. 29)	(495. 17)
N	288	288
Number of parameters	42	42
R^2	. 98	. 97

Note : Standard errors are in parentheses. Dependent variable in all models is
weekly median wage for non-professional, non-managerial workers. Wages
in constant 2002 dollars. Models weighted by cell counts for each
industry/region grouping.

[a]$p < .10$ *$p < .05$ **$p < .01$ ***$p < .001$

Table 6. Fixed effects models predicting weekly worker pay: high union sample.

	Model 1	Model 2	Model 3
Lagged Strike Rate	-47. 55	-144. 08	-149. 82
	(78. 32)	(106. 78)	(111. 74)
Median Managerial Pay	. 02**	. 03*	. 02
	(. 01)	(. 01)	(. 01)
Worker Variables:			
Proportion Unionized	258. 02***	280. 99***	340. 05***
	(30. 27)	(42. 93)	(46. 87)
% Female	-293. 13***	-287. 45***	-264. 58***
	(32. 38)	(55. 16)	(63. 16)
% Minority	-126. 32***	-161. 30**	-139. 97*
	(31. 15)	(56. 77)	(62. 21)
Less than HS	-236. 69***	-81. 77	-75. 59
	(40. 30)	(70. 74)	(74. 78)
HS Degree	25. 86	-1. 86	28. 17
	(17. 57)	(26. 26)	(30. 73)
Age	-5. 29	33. 53	27. 13
	(13. 95)	(21. 89)	(22. 33)
Age^2	. 12	-. 39	-. 28
	(. 18)	(. 28)	(. 28)
Year fixed-effects	Yes	Yes	Yes
Industry/region fixed-effects	Yes	Yes	Yes
Restricted to highly unionized industries	No	Yes	Yes
Restricted to cells where strike rate > 0	No	No	Yes
Constant	641. 36*	-116. 22	-54. 25
	(264. 52)	(420. 86)	(429. 00)
N	1008	494	426
Number of parameters	82	65	65
R^2	. 96	. 94	. 95

Note: Standard errors are in parentheses. Dependent variable in all models is weekly median wage for non-professional, non-managerial workers. Wages in constant 2002 dollars. Models weighted by cell counts for each industry/region.

*p < .05   **p < .01   ***p < .001

Table 7. Fixed effects models predicting worker pay inequality: high union sample.

	80 / 20	80th Percentile	20th Percentile
Lagged Strike Rate	. 88[a]	66. 28	-153. 43*
	(. 49)	(122. 05)	(73. 44)
Median Managerial Pay x 100	-. 00	. 87	. 83
	(. 00)	(1. 29)	(. 78)
Worker Variables:			
Median Worker Pay x 100	-. 07**	64. 20***	42. 24***
	(. 02)	(5. 51)	(. 03)
Proportion Unionized	-. 83***	-126. 73*	92. 98**
	(. 21)	(51. 36)	(30. 90)
% Female	-. 18	-78. 35	-30. 28
	(. 26)	(64. 87)	(39. 04)
% Minority	-. 06	-106. 17	-27. 48
	(. 26)	(65. 36)	(39. 33)
Less than HS	-. 78*	-329. 13***	17. 4
	(. 33)	(80. 81)	(48. 62)
HS Degree	-. 47***	-144. 18***	42. 64*
	(. 12)	(29. 95)	(18. 02)
Age	-. 33**	-118. 23***	25. 06[a]
	(. 10)	(25. 04)	(15. 07)
Age^2 x 10	. 04**	15. 16***	-3. 17
	(. 01)	(3. 22)	(1. 94)
Year fixed-effects	Yes	Yes	Yes
Industry/region fixed-effects	Yes	Yes	Yes
Restricted to highly unionized industries	Yes	Yes	Yes
Constant	9. 54***	2974. 69***	-361. 46
	(1. 93)	(480. 07)	(288. 87)
N	494	494	494
Number of parameters	66	66	66
R^2	. 73	. 96	. 93

Note: Standard errors are in parentheses. Dependent variable in first model is the 80 / 20 weekly worker pay ratio. Dependent variable in second model is the worker 80th pay percentile, and dependent variable in third model is the worker 20th pay percentile. Wages expressed in constant 2002 dollars. Models weighted by cell counts for each industry/region grouping.

[a]$p < .10$ *$p < .05$ **$p < .01$ ***$p < .001$

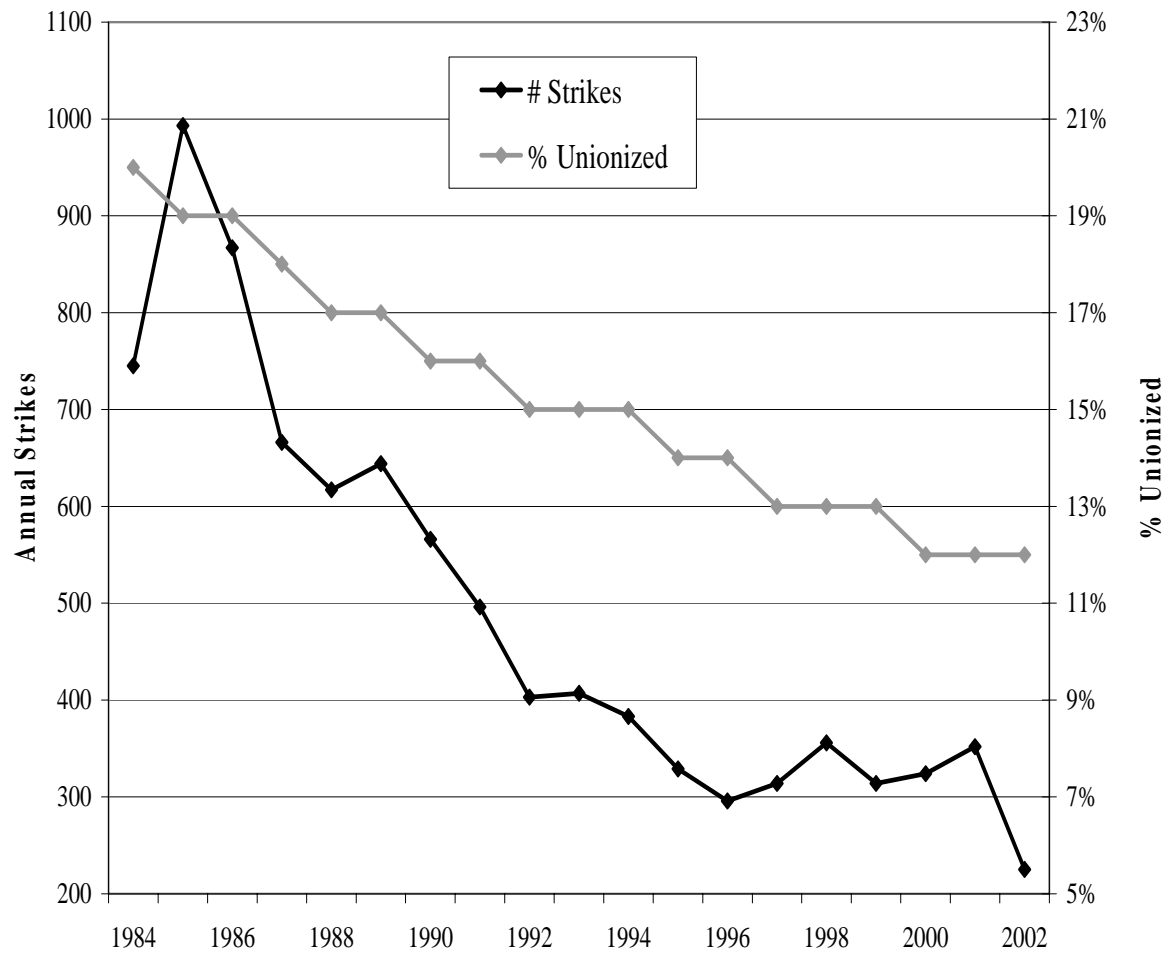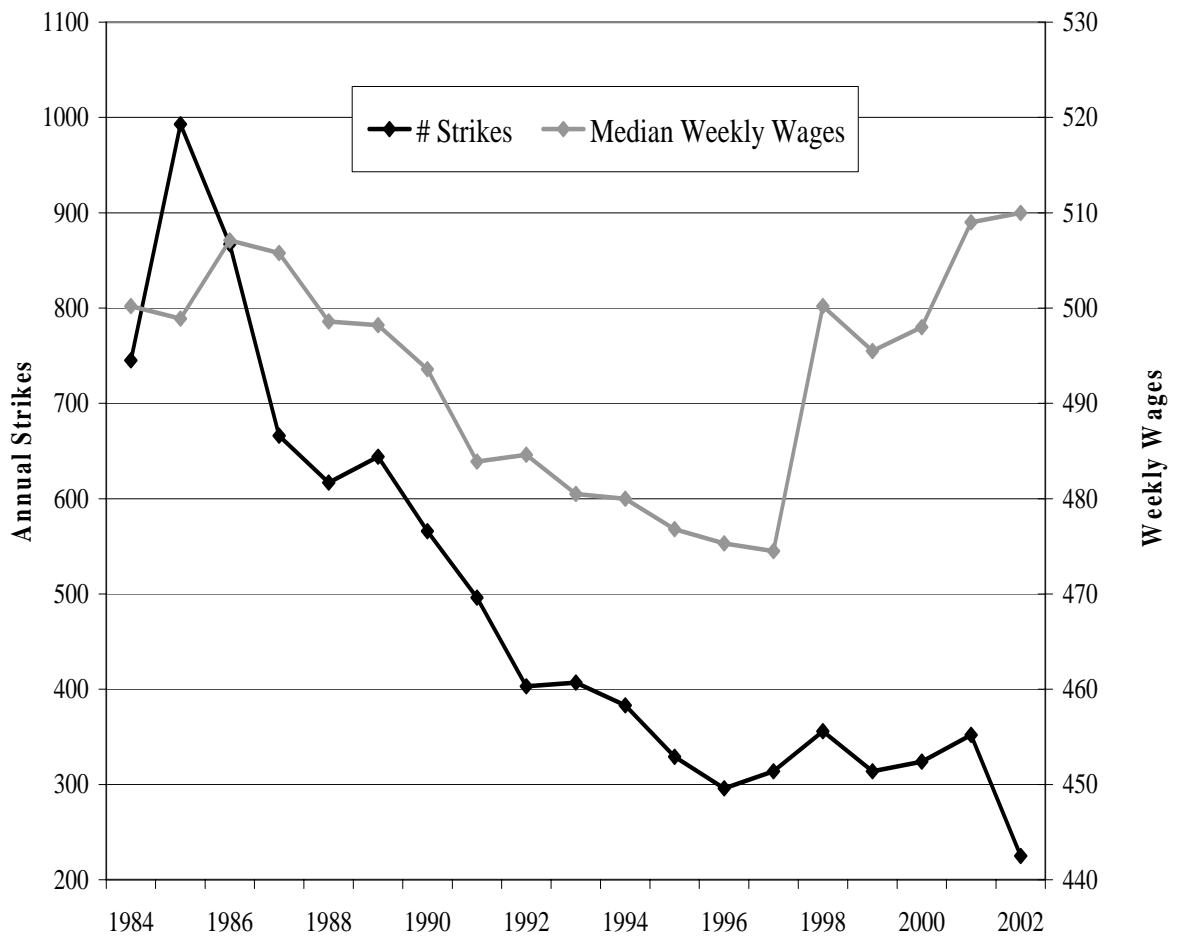**Figure 1. Trends in strike frequency and unionization levels, 1984 - 2002.**

Figure 2. Trends in strike frequency and median weekly wages, 1984 - 2002.

Appendix 1: Regional Groupings by State.

1. Northeast/ mid-Atlantic region

Maine
New Hampshire
Vermont
Massachusetts
Rhode Island
Connecticut
New York
New Jersey
Pennsylvania
Delaware
Maryland
Washington D.C.

2. South

Virginia
West Virginia
North Carolina
South Carolina
Georgia
Florida
Kentucky
Tennessee
Alabama
Mississippi
Arkansas
Louisiana

3. Midwest

Ohio
Indiana
Illinois
Iowa
Michigan
Missouri
Wisconsin
Minnesota
North Dakota
South Dakota
Nebraska
Kansas

4. West

      Oklahoma
      Texas
      New Mexico
      Arizona
      Montana
      Idaho
      Nevada
      Wyoming
      Colorado
      Washington
      Oregon
      California
      Alaska
      Hawaii
      Utah

Appendix 2: Firm size effects on weekly worker pay.

	Full Sample	High Union Sample
Lagged Strike Rate	-137. 19	-47. 14
	(182. 03)	(100. 39)
Average Firm Size	29. 29**	5. 90
	(8. 38)	(6. 40)
Regional Unemployment	-2. 62	-3. 11
	(1. 60)	(2. 05)
Median Managerial Pay	. 03*	. 02*
	(. 01)	(. 01)
Worker Variables:		
Proportion Unionized	398. 03***	176. 07***
	(47. 09)	(34. 54)
% Female	-193. 45***	-250. 42***
	(54. 81)	(35. 51)
% Minority	-278. 20***	-84. 02*
	(52. 24)	(33. 55)
Less than HS	-440. 14***	-282. 48***
	(57. 21)	(44. 10)
HS Degree	-50. 49*	-43. 19*
	(23. 42)	(20. 16)
Age	48. 31**	-3. 56
	(16. 47)	(18. 14)
Age^2	-. 56**	. 12
	(. 21)	(. 23)
Year fixed-effects	Yes	Yes
Industry/region fixed-effects	Yes	Yes
Constant	-439. 95	650. 30[a]
	(311. 79)	(348. 00)
N	448	784
Number of parameters	57	81
R^2	. 99	. 96

Note : Standard errors are in parentheses. Dependent variable in all models is weekly median wage for non-professional, non-managerial workers. Wages in constant 2002 dollars. Models weighted by cell counts for each industry/region grouping.

[a]p < .10   *p < .05   **p < .01   ***p < .001

Appendix 3: Main models minus panels exhibiting unit roots.

	Full Sample	High Union Sample
Lagged Strike Rate	130. 76	19. 63
	(157. 58)	(85. 98)
Regional Unemployment	. 73	-1. 60
	(1. 14)	(1. 43)
Median Managerial Pay	. 05***	. 03**
	(. 01)	(. 01)
Worker Variables:		
Proportion Unionized	636. 61***	261. 69***
	(41. 99)	(33. 07)
% Female	-236. 29***	-277. 03***
	(52. 72)	(35. 26)
% Minority	-399. 12***	-141. 38***
	(50. 87)	(33. 66)
Less than HS	-441. 33***	-207. 14***
	(54. 19)	(43. 34)
HS Degree	-36. 61	42. 71*
	(22. 55)	(18. 78)
Age	39. 75**	2. 58
	(14. 15)	(15. 09)
Age^2	-. 46*	. 01
	(. 18)	(. 20)
Year fixed-effects	Yes	Yes
Industry/region fixed-effects	Yes	Yes
Constant	-333. 45	496. 25[a]
	(265. 99)	(284. 33)
N	504	846
Number of parameters	55	74
R^2	. 98	. 96

Note: Standard errors are in parentheses. Dependent variable in all models is weekly median wage for non-professional, non-managerial workers. Wages in constant 2002 dollars. Models weighted by cell counts for each industry/region grouping.

[a]$p < .10$ *$p < .05$ **$p < .01$ ***$p < .001$