Revisiting Union Wage and Job Loss Effects Using the Displaced Worker Surveys

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Abstract: Standard empirical estimates of union wage effects have long been challenged due to concerns over unobserved heterogeneity correlated with union status and endogenous job change. The biennial Displaced Worker Survey (DWS) supplements to the Current Population Survey arguably provide an opportunity to account for these challenges. Examining job changes among workers displaced by exogenous job losses reported in the 1994 through 2016 DWS, we examine wage changes involving moves between union and nonunion jobs. Consistent with much earlier limited evidence from the DWS, we find that longitudinal estimates of union wage effects are roughly the same size as standard cross-section estimates, suggesting minimal ability bias. Displacement rates for union and nonunion workers have been highly similar for more than two decades.

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Introduction

Labor economists have a long history of studying the wage effects of unions. This topic was a principal focus of work by H. Gregg Lewis (1963, 1986) and has remained a focus among labor economists, albeit less so as union density has declined.[1] There have been several key econometric concerns in this literature, some that would upwardly bias and others that would attenuate union wage gap estimates. One concern, going back at least to Lewis, is omitted ability bias due to skill upgrading. The skill upgrading conjecture is that employers in unionized firms are able to hire more productive workers (in ways unobservable to the researcher) given the presence of wage premiums. Lewis (and others) conclude that such skill upgrading will cause union wage gap estimates to be upwardly biased.

Subsequent research, however, has called into question whether skill upgrading bias is substantial. First, such behavior need not follow from theory. Wessels (1994) provides a simple but persuasive challenge to the skill-upgrading hypothesis. If firms upgrade in response to a union wage increase, the union can then bargain in a future contract for an even higher wage in order to restore the premium. Employers, anticipating this, may respond by not upgrading. Firms that upgrade will face higher future wage demands and will have distorted their factor mix, using a higher skill labor mix than is optimal given its technology.

Second, and more fundamental, the selection mechanism varies across the earnings distribution. As characterized by Abowd and Farber (1982) and Card (1996), there exists two-sided selection. Workers queue for union jobs and employers select from among those queues. Given wage compression within unionized firms, employers are able to hire above-average workers in the left tail of the applicant attribute distribution (e.g., those with high ability, motivation, and reliability given their low levels of schooling, age, etc.). In the right tail of the attribute distribution, workers with high abilities may prefer work in nonunion companies where such abilities may be more highly valued than in a unionized workplace where there are standardized contractual wages and compressed earnings. Positive selection in the left tail coupled with negative selection in the right tail may roughly offset each other so that OLS union wage gaps at the mean of the distribution provide roughly reliable union wage gap estimates.

A relatively direct way to account for unmeasured ability/productivity differences is to use longitudinal evidence, identifying union wage effects based on workers moving between union and nonunion jobs. One can either include worker fixed effects or estimate difference equations, with the change in log wages a function of the change in union status (and changes in other non-fixed wage determinants (the two approaches are identical if there are just two periods). Although the longitudinal approach has the advantage of accounting for worker

[1] Jarrell and Stanley (1990) have conducted a meta analysis of union wage effects. Hirsch (2004) provides an overview of estimation issues pertinent to the analysis in this paper. Firpo et al. (2011) examine econometric methods to estimate union wage gaps across the distribution and decompose gaps into portions due to differences in covariates and in coefficients.

heterogeneity, it faces two serious problems. First, given a relatively small number of union status changers over a one-year period (as in the CPS), the ratio of measurement error to signal is high, seriously attenuating union wage gap estimates. Authors in this area have used alternative approaches to account for the attenuation bias (see Freeman 1984; Card 1996; and Hirsch and Schumacher 1998). Second, in order to change union status, one typically needs to change jobs. Job switching, however, is endogenous, determined in part by differences in wage offers.

In this paper, we estimate union wage effects using the biennial CPS Displaced Worker Surveys from 1994 through 2016. Although the DWS supplements to the CPS began in 1984, the union status of the displacement job was first added in the 1994 survey. Our analysis builds on previous work by Raphael (2000), who used the 1994 and 1996 DWS to estimate union wage effects. We are unaware of studies other than Raphael's that use the DWS to estimate union wage effects.[2] The absence of such studies is surprising, given that the nature of the DWS helps overcome several of the difficulties involved in estimating union wage effects (it also introduces unique problems of its own). As emphasized by Raphael, the DWS provides longitudinal information, but without the substantial measurement error in union status changes seen for the two-year CPS panels. Given that the number of job changes and thus union changes over one year is quite small, even low rates of misreported union status causes severe attenuation in CPS longitudinal estimates, an issue addressed (imperfectly) in the literature in alternative ways (e.g., Freeman 1984; Card 1996; Hirsch and Schumacher 1998). As compared to CPS panels of observations one-year apart, misreporting of union status in the DWS produces substantially less measurement error in the union change variable because the true level of union changes in the DWS is substantial because the entire sample has changed jobs, thus sharply reducing the noise-to-signal ratio. Moreover, the DWS has the advantage that job change due to displacement is largely exogenous, particularly so when the cause is a plant closing (Gibbons and Katz 1992).

A related topic is whether union workers are more or less likely to be displaced. This question helps address the question of union effects on firm performance and whether union or nonunion enterprises are more likely to fail. Freeman and Kleiner (1999) addressed this question, based in part on their analysis of the 1994 and 1996 DWS. They conclude that displacement was roughly equivalent for union and nonunion workers based on the finding that the percent of displaced workers who are unionized was similar to the percent of union

[2] Henry Farber has produced a series of papers since 1993 (for example, see Farber 1993, 2015) using the DWS to measure the incidence, pattern, and severity of job displacement and earnings losses over time. He has not examined union-nonunion differences. Kuhn and Sweetman (1998) have examined union wage effects for workers in Canada who have been displaced, finding particularly large losses among workers with substantial tenure.

workers in the overall private workforce. To our knowledge, this question has not been reexamined using Displaced Worker Surveys after 1996.[3]

To preview our results, we first provide descriptive evidence on the frequency of plant closings and other forms of displacement among union versus nonunion workers, an interesting question on its own. We find highly similar rates of union and nonunion displacement from the early 1990s through the end of 2015, both during recessions and in boom years. Union density rates among those displaced from private sector jobs are similar to union density in the overall private workforce. We then turn to examining union wage effects based on displaced workers changing union status between their prior displacement job and their current wage and salary job. We find that union wage gaps are substantial, on the order of 20 percent, similar in cross section and longitudinal analyses, similar among union joiners and leavers, and similar across the measured skill distribution.

Displaced Worker Surveys

The principal data source in this paper are the Displaced Worker Surveys, which have been administered as supplements to the CPS biennially since 1984, either in January or February. We begin with the February 1994 DWS, the first to report union status (membership) on an individual's displaced job.[4] The DWS supplement is administered only to individuals who have been classified as displaced. Our principal sample includes all workers ages 20 to 65 who were displaced from a private sector wage and salary job within the previous three years and currently hold a wage and salary job (it need not be in the private sector). To be classified as displaced from a private sector wage and salary job (and asked union status on that job), one must have lost their job due to one of three reasons – a plant or company closed down or moved, insufficient work, or a position or shift abolished. Workers who ended jobs due to a seasonal job completed, a self-operated business failing, or "some other reason" are not asked about union status on the displaced job and not analyzed in this paper. The supplement provides information on job characteristics of the displacement job such as weekly earnings, hours worked, overtime, receipt of benefits, and union status.[5]

[3] Analysis of union effects on displacement and business failure in the U.S. has been provided in other studies, but such research has been limited by difficulty in measuring unionization in establishment and firm datasets. Freeman and Kleiner (1999) include a limited analysis on firm failures in their paper. Dunne and Macpherson (1994) utilize longitudinal plant-level data and show that there are more employment contractions, fewer expansions, and fewer plant "births" in more highly unionized industries, but they find that unions have no effect upon plant deaths. DiNardo and Lee (2004) examine survival rates for establishments following union certification elections with close outcomes and conclude that successful union organizing drives have a negligible on survival. They also fail to find effects on wages or other outcomes.

[4] The 1994-2000 DWS supplements were administered in February and the 2002-2016 supplements in January. DWS supplements prior to 1994, which do not provide union status on the displaced job, were administered in January.

[5] The union question in the DWS, beginning in 1994, asks whether a worker was a union member at their displaced job. There is no coverage question asked of non-members.

In the month of the displacement survey (January or February, depending on the year), the individual is also administered the regular monthly CPS questions including demographics and detailed information on current employment status, hours worked, location, industry, and occupation. Earnings, hours, and union status on the current job is asked only of the quarter sample in January who are in the outgoing rotation groups. The remaining three-quarters of the sample are asked these questions when they are outgoing either in February, March, or April if interviewed in January, or in March, April, or May if interviewed in February. We link information on earnings, hours, and union status during the outgoing rotation group months with the January or February DWS survey, thus providing labor market information on earnings and hours on both the current primary job and the displaced job. The combined information from the DWS supplement, the monthly CPS, and the CPS earnings supplement administered to the outgoing rotation groups enables us to compare earnings on the previous job from which one was displaced with the primary job currently held.[6]

Descriptive Evidence on Union versus Nonunion Displacement Rates and Reemployment

We first provide estimates of the level of displacement among private sector union and nonunion wage and salary workers. Because our focus is on relative wage changes associated with displacement, our primary sample includes only those displaced workers who are currently working either in private or public sector wage and salary jobs. (We subsequently examine evidence on the share of displaced workers who are not employed at the time of the survey). Displacement is measured for each three-year period prior to the biennial DWS. Displacement levels and rates are shown in Tables 1a and 1b and in Figures 1a and 1b for the three-year periods 1991-93 through 2013-15, based on the biennial DWS surveys conducted in 1994 through 2016. The displacement figures in Table 1a and Figure 1a include all forms of displacement – a plant or company closed down or moved, insufficient work, or position or shift abolished. Throughout the paper, we provide results first for all displacements, followed by results for the subset of displacements due to plant closings. We do not analyze individuals with job loss due to seasonal jobs completed, a self-operated business failed, and "some other reason." These individuals are not asked whether they were a union member on the displaced job.

Turning to Tables 1a and 1b, the numerator of the displacement rate is the estimated number of union or nonunion workers who were displaced during the previous three years, measured within the DWS using supplement weights. As noted in previous work (e.g., Farber 2015), the DWS measure of displacement fails to account for multiple displacements during the three year period. The denominator measures the population of employed union members and nonunion workers, these estimates derived from the CPS outgoing rotation groups. For such estimates, we use the three year average of union members and nonmembers calculated for each year's January-December CPS-ORG files and updated annually by Hirsch and Macpherson at

[6] Individuals are matched using household ID by year, state, person line number within households, sex, and age range.

Unionstats.com.[7] For example, for the January 2016 DWS, the estimated population of employed union members and nonmembers is averaged over the years 2013-2015.[8]

Shown in Table 1a are the estimates of displacement levels and rates for union and nonunion workers for the displacement periods 1991-93 through 2013-15. Levels and rates of displacement clearly vary with the business cycle. The levels and rates of all displacement for both union and nonunion workers were highest in 2007-09 (as reported in 2010), with rates of 13.9 and 13.1 percent for union and nonunion workers. The lowest levels and rates occurred in 2013-2015 (reported in 2016), with rates of 5.0 and 5.6 percent for union and nonunion workers. Table 1b provides identical information for the subset of displacements that are due to plant closures.

Figures 1a and 1b graphically show the relative union and nonunion displacement rates by DWS year, the first figure showing all displacements and the second plant closings only. The clear takeaway from Figure 1a is that displacement rates for union and nonunion workers are highly similar. Union displacement rates are slightly higher than nonunion rates in about half the years, while the opposite is true in the remaining other years. When we restrict the sample to plant closings (see Figure 1b), the level of displacements and displacement rates are less than half the magnitude seen for all displacements. We see a highly similar pattern over time, however, and no systematic difference between union and nonunion rates. (Focusing on plant closures, one might discern steeper slopes (greater cyclicality) for union than nonunion displacement.)

Similar union and nonunion displacement rates support the conclusion that unionization is not associated with higher (or lower) rates of business failure or insolvency. This conclusion previously was reached by Freeman and Kleiner (1999) based on the 1994 and 1996 DWS. Freeman and Kleiner, however, did not explicitly calculate displacement rates. Rather they reached this conclusion by calculating the percent of union workers among those recorded as displaced in 1994 and 1996, and then found that this share was similar to union density among employed workers. We provide equivalent evidence across all DWS survey years through 2016.

As seen in Table 2 (and Figure 2a), we find union density (shares) among workers displaced (for any reason) in each of the 12 three-year displacement periods to be highly similar to union density in the overall private sector (i.e., the average private sector density, as calculated in the CPS-ORGs and reported at Unionstats.com). Over the 12 periods, union density rates were slightly higher in the displacement sample six of the periods and slightly lower in the other six periods. Evident in Figure 2a is that union density rates in the

[7] The *Union Membership and Coverage Database from the CPS* is described in Hirsch and Macpherson (2003) and updated annually at Unionstats.com.

[8] Farber's estimates of displacement rates (e.g., Farber 2015) include in the denominator an estimate of the number of displaced workers not currently employed. We have not included such estimates in this initial draft. Had we done so, displacement rates would be slightly lower.

displacement samples trended downward over the 22 year period at a rate similar to that seen in the overall private sector, albeit with more real and/or sample variability. The same conclusion is reached when one uses the narrower measure of displacement based solely on plant closures, as shown in Figure 2b.

Figures 2a and 2b also show that over time, the share of displaced workers who are unionized declines sharply. This tells us little, however, about changes in relative union-nonunion displacement rates. The decline in the union share of displaced workers simply mirrors the decline in private sector unionization over time.

In short, evidence from the DWS suggests that that there is no substantive difference in private sector job loss rates for union versus nonunion workers/companies. A caveat is that we are not controlling for other covariates. For example, union workers are more likely to work in larger establishments and firms, and large businesses have lower failure rates. Were we able to control for employer size, one might find a positive relationship between unionization and displacement. Industry and occupation structure is correlated with business failures and also differs among union and nonunion workers. Our priors are that industry/occupation differences should lead to a higher displacement rate for union workers, just the opposite of employer size. In a future draft we can examine whether worker attributes and industry/occupation structure are similar in the union and nonunion displaced samples and the overall private sector.

Wage analysis

A standard approach to measuring union (and other) wage differentials is to estimate a semi-log human capital earnings function of the general form:

$$lnW_{it} = \alpha + \beta X_{it} + \theta UN_{it} + \varepsilon_{it}$$

where W is hourly earnings; X is a vector of worker, location, and job attributes (inclusion of job characteristics may or may not be appropriate, depending on the questions being asked); and U is a categorical measure of union status on the displaced job and/or the current job. Concerns regarding worker-specific differences (heterogeneity) correlated with union status make attractive estimation of longitudinal analysis of the form:

$$\Delta lnW_i = \beta' \Delta x_{ikt} + \theta' \Delta U_i + \Delta\varepsilon_i ,$$

Letting U be union and N nonunion, ΔU takes on the value 1 for *NU* transitions, -1 for *UN* transitions, and 0 for *UU* and *NN* transitions. Estimates of the union gaps θ' are based on the average worker-specific wage changes between union (nonunion) displacement jobs and subsequent nonunion (union) reemployment jobs. As shown above, symmetry is assumed regarding the absolute value of wage gains from *NU*, losses from *UN* transitions, and wage growth for *UU* and *NN*. In the empirical work that follows we relax these restrictions and find substantively larger losses from *NU* than gains from *UN*.

Table 3a provides preliminary results on wage change equations and union wage effects. Our results are based on a sample of those displaced for any reason from a private sector job within the past three years and currently employed in a wage and salary job (private or public sector) at the time of the survey. The longitudinal results provide relatively clear-cut evidence on union wage effects among displaced workers. Assuming symmetry between wage gains (losses) for joining (leaving) a union job, we find a raw (no controls) union gap of 0.197 log points (or 20 percent).[9] The estimate is not sensitive to the addition of controls. The union log gap estimate moves from 0.197 to 0.180 (column 1 versus 5) once one controls for demographics, tenure, state, living in an MSA, time, a move to a new city or county, changes in detailed industry and/or occupation, and a dense set of industry and occupation dummies (on the current job).

We next provide separate evidence for the roughly third of the total sample whose displacement was due to plant closure (Table 3b). The plant closure sample has the advantage of better approximating the wage effects of exogenous job changes, but the disadvantage of a much smaller sample (roughly a third as large). That said, we find highly similar union wage effects for the plant closure and full displacement samples, although there is a tendency for the union wage effects in the plant closure sample (Table 3b) to be slightly larger than estimates in the full sample (Table 3a).

We next drop the assumption that wage gains (losses) from joining (leaving) a union job are symmetric. In Table 4a, we find reasonably clear evidence that losses from leaving a union job exceed gains from joining a union job, as found by Raphael using the 1994 and 1996 DWS. Absent controls, the raw wage gaps (column 1) are 0.129 for moving to a union job and -0.253 for those displaced from a union job and taking a new nonunion job. In the dense specification in column 4, the union gap estimates are 0.122 and -0.227 for union joiners and leavers. In short, the DWS evidence clearly shows that union wage effects based on longitudinal evidence are substantive using data in which job change is largely exogenous and there is minimal attenuation due to mismeasurement of job and union changes.

Table 4b provides identical specifications shown in Table 4a, but is restricted to subset of the sample that was displaced by plant closures. These estimates arguably provide the best evidence of union wage effects based on largely exogenous changes in union status. Although we see the same overall pattern in the plant closing and all-displacement samples, the plant closing only sample produces *NU* and *UN* union wage gap estimates that are relatively more symmetric than seen for the entire sample. For example, union joiner and union leaver wage gaps are 0.18 and -0.21 in Table 4b (column 4) based on the plant closing sample, as

[9] We simply refer to log point changes as percentage changes, albeit percentages with a base intermediate between the union and nonunion wage (roughly the geometric mean). The standard conversion from a log differential to an arithmetic percentage is $[\exp(\beta)-1]100$, where β is the log gap.

compared to 0.12 and -0.23 based on the full displacement sample (Table 4a, column 4). In short, the plant closure results suggest there is approximate symmetry in union gaps for joiners and leavers.

In work not shown, we examined differences between public and private union status. A nontrivial portion of job changers take new jobs in the public sector, where unionization is more likely than in private sector jobs. This raises the concern that we may be confounding wage gains and losses associated with changes in union status with wage changes due to moving into or out of public sector employment. We are unwilling to draw inferences based on our work to date. Our initial specifications were possibly inappropriate and unreliable. We included detailed industry/occupation dummies in which many categories were either all public or all private workers.

Additional evidence from our regression analysis in Tables 4a and 4b include the following. Independent of the change in union status, wage growth in moving from a displaced private sector job to a new jobs is about 3 to 5 percent lower among workers who transition between two union jobs compared to those transitioning between two nonunion jobs (see the "Stay Union" coefficients in Tables 4a). This pattern is more erratic in the much smaller plant closing sample (Table 4b). Wage changes between the two jobs are less favorable for older workers, with an additional 10 years of age being associated with an approximate three percent lower wage change (see the Age coefficient in Table 4a). Coefficients on changes in industry and occupation show substantial wage losses from each of about 3 percent or more, consistent with prior evidence of wage declines associated with industry- and occupation-specific human capital losses (e.g. Neal 1995, Helwege 1992). And those who have moved residence across counties following displacement realize an approximate 3 percent wage loss (Table 4a).

In results not shown, we provide estimates of union wage effects across the skill distribution, proxied by education group. Standard wage level analysis produces substantially higher than average union wage effects for those least skilled (high school dropouts) and substantially lower than average union wage effects for those most educated (college graduates). We find this same pattern using wage level analysis in the DWS, with either the initial displacement wage or the subsequent reemployment wage. Such a pattern, however, is likely to be driven by two-sided selection, positive in the left tail and negative in the right tail. When we instead use longitudinal analysis based on changes into and out of union membership, we find highly similar union wage gap estimates across education groups (if anything, highest for the college graduates, among whom union density is low). This result reinforces the evidence provided by Raphael (2000) using the first two DWS files (1994 and 1996) that identified union status on the displaced job, as well as earlier work using longitudinal analysis on matched CPS panels by Card (1986) and Hirsch and Schumacher (1988).

Our analysis of wage effects based on moving between union and nonunion jobs has masked the broader question of overall earnings losses (or gains) associated with displacement. Several papers by Farber, most recently Farber (2015), has provided detailed analysis of this issue. He has typically focused on changes in weekly earnings, finding little aggregate loss (on average) during healthy labor market periods, but average losses in excess of 10 percent or so during recessionary periods. Not surprisingly, changes in weekly earnings are heavily influenced by changes in hours and shifts between full-time and part-time employment. Based on our entire sample, we do not find an average loss in real hourly earnings between workers' displacement job and current job. We have not accounted for the earnings increases that workers would have realized absent displacement (see Farber 2015).

A final point to consider is whether sample selection bias affects our results. The wage change analysis is necessarily conducted only for those displaced workers who are reemployed at the time of the survey. If workers displaced from union jobs have different reemployment rates, we may be misstating their wage losses. In Table 5, we show rates of non-employment (not in the labor force or unemployed) among workers displaced from union jobs and nonunion jobs. We find a substantive difference. Among all displaced workers, 40.9 percent of those who were in union jobs are not reemployed, as compared to 34.3 among those from nonunion jobs, a 6.6 percentage point difference. These differences are larger among those displaced due to plant closure, with 42.4 percent union non-employed versus 30.1 percent non-employed among those from nonunion jobs, a 12.3 percentage point difference. In short, we do not observe the potential wage change for a substantive share of workers, more so among displaced union than nonunion workers. Why the higher non-employment among displaced union workers than among displaced nonunion workers? There are several possibilities. One is that displaced union workers may have been less employable and/or faced lower wage offers, perhaps because of less transferable human capital. Alternatively, displaced union workers typically had a relatively high union wage; thus, they are likely to have higher reservation wages for a post-displacement job. Moreover, some (many?) displaced union workers may receive retiree health benefits or pensions. In a future draft, we can examine differences in the age structure for union and nonunion displaced workers. And matching displaced workers who remained in the CPS sample in March, we can examine whether these workers received pension income and/or were covered by health insurance. The sample sizes will be tiny, but the information is potentially informative.

Conclusion

Two clear-cut punch lines emerge from our analysis of displaced workers over more than two decades. First, displacement rates among union and nonunion workers are remarkably similar, on average. In any given period union displacement may be somewhat higher or lower than nonunion displacement, but there is no systematic and substantive long-run difference. Second, wage analysis based on displaced workers moving between union and nonunion jobs shows that that union wage effects are sizable, on the order of 15-20 percent.

Union wage effects based on the longitudinal analysis are highly similar across the skill distribution (proxied by education), in contrast to standard wage level analysis. This result is consistent with there being two-sided selection into unionization with respect to skill, positive in the left tail due to employer selection and negative in the right tail due to worker selection.

Wage losses from displacement from union jobs are larger than are gains from transitions into union jobs. Moreover, we may be understating these losses to the extent that fewer displaced union workers reenter employment. What we cannot say is to what extent the lower reemployment rate seen for union workers is due to their higher reservation wages, the availability of benefits (retiree health and pensions, etc.), or poorer job opportunities.

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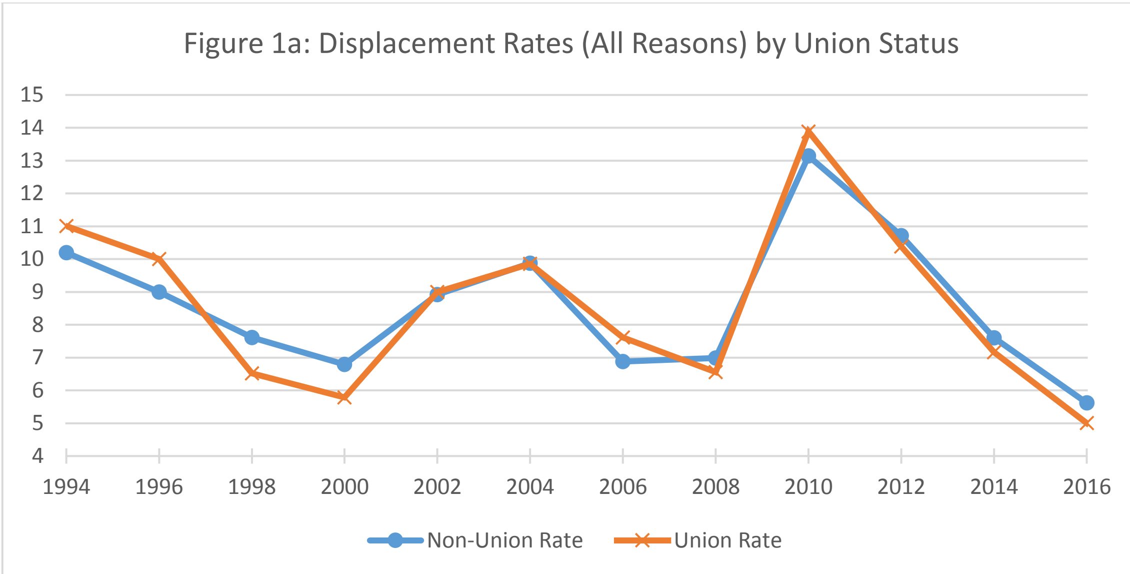

Table 1a: Displacement Rates (All Reasons) for Union and Nonunion Workers

		Nonunion			Union		
Survey Year	Disp Years	Displaced (1000s)	Employed Base (1000s)	Disp Rate	Displaced (1000s)	Employed Base (1000s)	Disp Rate
1994	1991-93	7,734	75,834.0	10.2	1,073	9,751.3	11.0
1996	1993-95	7,171	79,701.7	9.0	956	9,553.9	10.0
1998	1995-97	6,438	84,535.3	7.6	613	9,403.5	6.5
2000	1997-99	6,034	88,884.0	6.8	542	9,362.7	5.8
2002	1999-01	8,197	91,901.7	8.9	831	9,235.9	9.0
2004	2001-03	9,167	92,853.9	9.9	862	8,748.2	9.9
2006	2003-05	6,578	95,609.4	6.9	632	8,303.8	7.6
2008	2005-07	6,929	99,239.3	7.0	532	8,116.6	6.6
2010	2007-09	12,979	98,778.0	13.1	1,102	7,936.5	13.9
2012	2009-11	10,335	96,482.9	10.7	752	7,242.4	10.4
2014	2011-13	7,576	99,728.6	7.6	515	7,182.4	7.2
2016	2013-15	5,824	103,652.0	5.6	371	7,407.0	5.0

Source: Calculations from the biennial CPS Displaced Worker Surveys (DWS), 1994-2016.

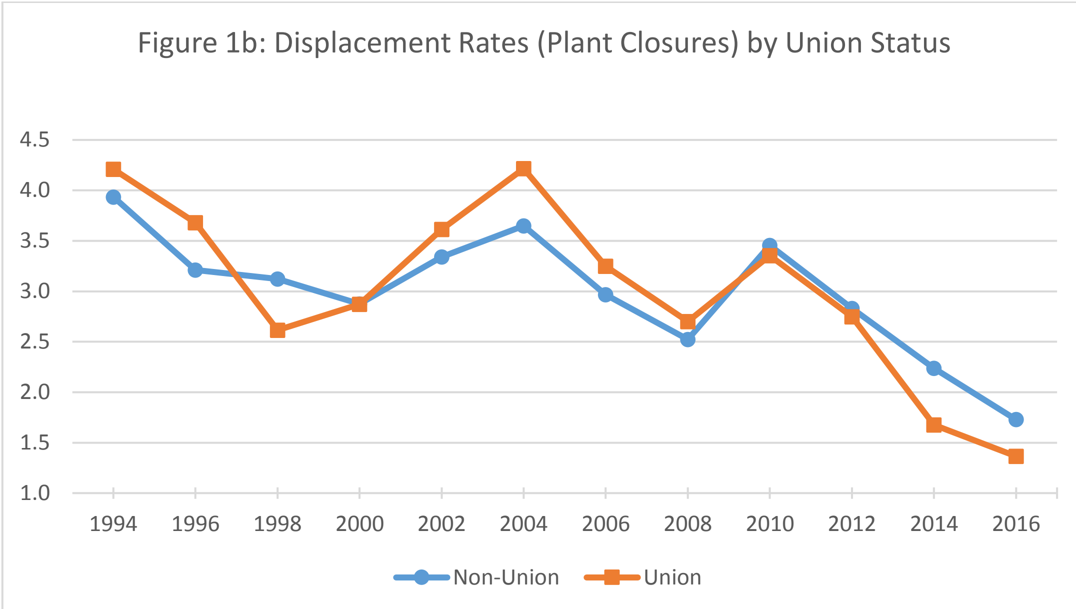

Table 1b: Displacement Rates (Plant Closures) for Union and Nonunion Workers

		Nonunion			Union		
Survey Year	Disp Years	Displaced (1000s)	Employed Base (1000s)	Disp Rate	Displaced (1000s)	Employed Base (1000s)	Disp Rate
1994	1991-93	2,984.4	75,834.0	3.9	410.4	9,751.3	4.2
1996	1993-95	2,559.8	79,701.7	3.2	351.8	9,553.9	3.7
1998	1995-97	2,639.6	84,535.3	3.1	245.8	9,403.5	2.6
2000	1997-99	2,555.7	88,884.0	2.9	268.8	9,362.7	2.9
2002	1999-01	3,069.5	91,901.7	3.3	333.9	9,235.9	3.6
2004	2001-03	3,389.3	92,853.9	3.7	368.8	8,748.2	4.2
2006	2003-05	2,835.4	95,609.4	3.0	269.9	8,303.8	3.2
2008	2005-07	2,504.1	99,239.3	2.5	219.3	8,116.6	2.7
2010	2007-09	3,411.6	98,778.0	3.5	266.2	7,936.5	3.4
2012	2009-11	2,730.8	96,482.9	2.8	199.0	7,242.4	2.7
2014	2011-13	2,231.0	99,728.6	2.2	120.3	7,182.4	1.7
2016	2013-15	1,792.0	103,652.0	1.7	101.1	7,407.0	1.4

Source: Calculations from the biennial CPS Displaced Worker Surveys (DWS), 1994-2016.

Figure 2a: Comparison of Union Density among Workers Displaced for All Reasons versus All Private Sector Workers

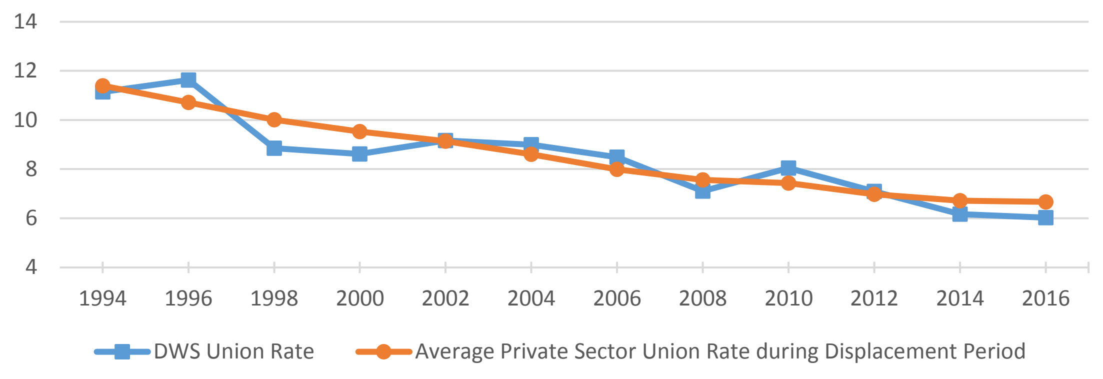

Figure 2b: Comparison of Union Density among Workers Displaced by Plant Closures versus All Private Sector Workers

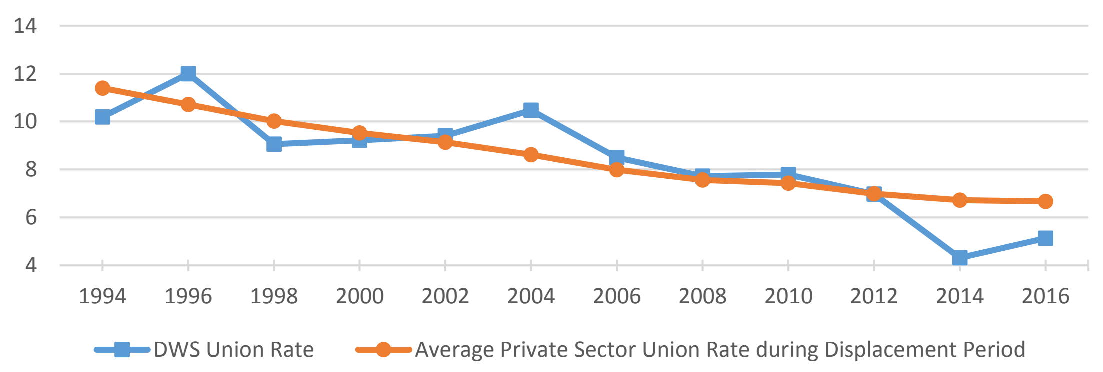

Table 2: Union Density among Displaced and All Private Sector Workers

Survey Year	Disp Years	DWS %Union All Displaced	DWS %Union Plant Closures	%Union Private Sector
1994	1991-93	10.2	11.1	11.4
1996	1993-95	12.0	11.6	10.7
1998	1995-97	9.1	8.8	10.0
2000	1997-99	9.2	8.6	9.5
2002	1999-01	9.4	9.2	9.1
2004	2001-03	10.5	9.0	8.6
2006	2003-05	8.5	8.5	8.0
2008	2005-07	7.7	7.1	7.6
2010	2007-09	7.8	8.0	7.4
2012	2009-11	7.0	7.1	7.0
2014	2011-13	4.3	6.2	6.7
2016	2013-15	5.1	6.0	6.7

Source: Calculations from biennial CPS Displaced Worker Surveys (DWS), 1994-2016.

Table 3a: Estimates of Union Wage Differentials
(Displaced for Any Reason)

	(1)	(2)	(3)	(4)	(5)
Δ Union	0.197***	0.187***	0.177***	0.176***	0.180***
	(0.017)	(0.017)	(0.017)	(0.017)	(0.018)
Mover		-0.011	-0.033**	-0.033**	-0.036**
		(0.016)	(0.016)	(0.016)	(0.017)
Change Industry		-0.035***	-0.044***	-0.031**	-0.035**
		(0.014)	(0.014)	(0.014)	(0.015)
Change Occupation		-0.027**	-0.030**	-0.029**	-0.032**
		(0.013)	(0.013)	(0.013)	(0.015)
Tenure		-0.007***	-0.007***	-0.007***	-0.008***
		(0.001)	(0.001)	(0.001)	(0.001)
Tenure^2		0.008***	0.007***	0.007***	0.010***
		(0.003)	(0.003)	(0.003)	(0.003)
Age		-0.003***	-0.003***	-0.003***	-0.003***
		(0.000)	(0.000)	(0.000)	(0.001)
Black			-0.056***	-0.050**	-0.038*
			(0.020)	(0.020)	(0.021)
Hispanic			0.003	0.004	0.010
			(0.018)	(0.018)	(0.019)
Female			0.024**	0.050***	0.042***
			(0.011)	(0.012)	(0.015)
Married			-0.037***	-0.039***	-0.031***
			(0.011)	(0.011)	(0.012)
Constant	0.084***	0.284***	0.180	0.167	0.106
	(0.005)	(0.022)	(0.171)	(0.176)	(0.614)
Education	No	No	Yes	Yes	Yes
Geography	No	No	Yes	Yes	Yes
Broad Occ/Ind	No	No	No	Yes	No
Detailed Occ/Ind	No	No	No	No	Yes
Observations	9,384	9,281	9,211	9,211	9,211
R-squared	0.014	0.028	0.047	0.056	0.208

Note: Education includes 11 dummies for various levels of education. Geography includes a dummy for MSA and state fixed effects. There are 23 broad industry and 21 broad occupation codes.

Table 3b: Estimates of Union Wage Differentials
(Plant Closure Displacements Only)

	(1)	(2)	(3)	(4)	(5)
Δ Union	0.216***	0.191***	0.182***	0.174***	0.195***
	(0.027)	(0.027)	(0.028)	(0.028)	(0.034)
Mover		-0.006	-0.023	-0.027	-0.038
		(0.028)	(0.029)	(0.029)	(0.035)
Change Industry		-0.047**	-0.056**	-0.044*	-0.091***
		(0.024)	(0.024)	(0.024)	(0.030)
Change Occupation		-0.026	-0.025	-0.016	-0.034
		(0.023)	(0.023)	(0.023)	(0.028)
Tenure		-0.012***	-0.012***	-0.012***	-0.013***
		(0.003)	(0.003)	(0.003)	(0.004)
$Tenure^2$		0.025***	0.024**	0.024**	0.024**
		(0.010)	(0.010)	(0.010)	(0.011)
Age		-0.004***	-0.004***	-0.004***	-0.003***
		(0.001)	(0.001)	(0.001)	(0.001)
Black			-0.049	-0.042	-0.002
			(0.034)	(0.034)	(0.040)
Hispanic			-0.002	-0.007	0.005
			(0.031)	(0.031)	(0.037)
Female			0.044**	0.071***	0.085***
			(0.019)	(0.021)	(0.030)
Married			-0.038*	-0.035*	-0.020
			(0.020)	(0.020)	(0.024)
Constant	0.102***	0.363***	0.019	0.051	0.247
	(0.009)	(0.039)	(0.329)	(0.336)	(0.797)
Education	No	No	Yes	Yes	Yes
Geography	No	No	Yes	Yes	Yes
Broad Occ/Ind	No	No	No	Yes	No
Detailed Occ/Ind	No	No	No	No	Yes
Observations	3,242	3,219	3,197	3,197	3,197
R-squared	0.019	0.040	0.078	0.107	0.379

Note: Education includes 11 dummies for various levels of education. Geography includes a dummy for MSA and state fixed effects. There are 23 broad industry and 21 broad occupation codes.

Table 4a: Estimates of Asymmetric Union Wage Differentials
(Displaced for Any Reason)

	(1)	(2)	(3)	(4)
Enter Union	0.129***	0.140***	0.126***	0.122***
	(0.026)	(0.026)	(0.026)	(0.028)
Leave Union	-0.253***	-0.227***	-0.217***	-0.227***
	(0.023)	(0.023)	(0.024)	(0.025)
Stay Union	-0.034	-0.031	-0.030	-0.055*
	(0.025)	(0.025)	(0.027)	(0.029)
Mover		-0.010	-0.032**	-0.035**
		(0.016)	(0.016)	(0.017)
Change Industry		-0.036***	-0.031**	-0.036**
		(0.014)	(0.014)	(0.016)
Change Occupation		-0.028**	-0.030**	-0.034**
		(0.013)	(0.013)	(0.015)
Tenure		-0.007***	-0.007***	-0.007***
		(0.001)	(0.001)	(0.001)
Tenure^2		0.007***	0.007***	0.010***
		(0.003)	(0.003)	(0.003)
Age		-0.003***	-0.003***	-0.003***
		(0.000)	(0.000)	(0.001)
Constant	0.092***	0.287***	0.179	0.090
	(0.006)	(0.022)	(0.176)	(0.614)
Black	No	No	Yes	Yes
Hispanic	No	No	Yes	Yes
Female	No	No	Yes	Yes
Married	No	No	Yes	Yes
Education	No	No	Yes	Yes
Geography	No	No	Yes	Yes
Broad Occ/Ind	No	No	No	Yes
Observations	9,384	9,281	9,211	9,211
R-square	0.016	0.029	0.057	0.209

Note: Education includes 11 dummies for various levels of education. Geography includes a dummy for MSA and state fixed effects. There are 23 broad industry and 21 broad occupation codes.

Table 4b: Estimates of Asymmetric Union Wage Differentials
(Plant Closure Displacements Only)

	(1)	(2)	(3)	(4)
Enter Union	0.166***	0.179***	0.154***	0.177***
	(0.045)	(0.044)	(0.045)	(0.054)
Leave Union	-0.251***	-0.201***	-0.188***	-0.208***
	(0.036)	(0.037)	(0.038)	(0.045)
Stay Union	-0.081	-0.043	-0.021	-0.005
	(0.053)	(0.054)	(0.055)	(0.068)
Mover		-0.005	-0.027	-0.038
		(0.028)	(0.029)	(0.035)
Change Industry		-0.048**	-0.044*	-0.091***
		(0.024)	(0.024)	(0.030)
Change Occupation		-0.027	-0.017	-0.034
		(0.023)	(0.023)	(0.029)
Tenure		-0.012***	-0.012***	-0.013***
		(0.003)	(0.003)	(0.004)
$Tenure^2$		0.025***	0.024**	0.024**
		(0.010)	(0.010)	(0.011)
Age		-0.004***	-0.004***	-0.003***
		(0.001)	(0.001)	(0.001)
Constant	0.110***	0.364***	0.057	0.250
	(0.010)	(0.039)	(0.336)	(0.798)
Black	No	No	Yes	Yes
Hispanic	No	No	Yes	Yes
Female	No	No	Yes	Yes
Married	No	No	Yes	Yes
Education	No	No	Yes	Yes
Geography	No	No	Yes	Yes
Broad Occ/Ind	No	No	No	Yes
Observations	3,242	3,219	3,197	3,197
R-squared	0.020	0.040	0.107	0.379

Note: Education includes 11 dummies for various levels of education. Geography includes a dummy for MSA and state fixed effects. There are 23 broad industry and 21 broad occupation codes.

Table 5: Non-Employment Rates of Displaced Union and Nonunion Workers

	Union	Nonunion	Difference
Displaced for any reason	40.9	34.3	-6.6
Displaced due to plant closures	42.4	30.1	-12.3

Note: Non-employment rates from private sector displacement within the past three years, compiled from the biennial Displaced Worker Surveys, 1994-2016.