IZA Institute of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 12786

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

Abhir Kulkarni
Barry T. Hirsch

NOVEMBER 2019

I Z A Institute of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 12786

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

Abhir Kulkarni
Georgia State University

Barry T. Hirsch
Georgia State University and IZA

NOVEMBER 2019

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

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

Estimates of union wage effects have been challenged due to concerns over unobserved worker heterogeneity and endogenous job changes. Many believe that union wage premiums lead to business failures and other forms of worker displacement. In this paper, displacement rates and union wage gaps are examined using the 1994-2018 biennial Displaced Worker Survey (DWS) supplements to the monthly Current Population Surveys. For more than two decades, displacement rates among union and nonunion workers have been remarkably similar. We observe changes in earnings resulting from transitions between union and nonunion jobs following exogenous job changes. Consistent with prior evidence from the 1994 and 1996 DWS, we obtain longitudinal estimates of average union wage effects close to 15 percent, similar to standard cross-section estimates and suggestive of minimal ability bias. Wage losses moving from union to nonunion jobs exceed gains from nonunion to union transitions.

JEL Classification: J31, J51, J65

Keywords: union wage gaps, job loss, displaced workers

Corresponding author:
Barry T. Hirsch
Department of Economics
Andrew Young School of Policy Studies
Georgia State University
Atlanta, GA 30302-3992
USA

E-mail: bhirsch@gsu.edu

* We appreciate helpful comments received from Henry Farber on an early draft, attendees at the 2018 Society of Labor Economists meetings, and referees and the editor at ILR Review.

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. There are several econometric concerns in this literature, some that imply upward bias and others that imply attenuated union wage gap estimates. Given the presence of union premiums and lower profits, coupled with substantive managerial resistance to union organizing, one might expect that job displacement (e.g., plant closures) would be higher in union than in nonunion workplaces.[1]

Following the addition of a union membership question in the 1994 biennial Displaced Worker Surveys (DWS), two *ILR Review* papers addressed these topics using the 1994 and 1996 DWS. Freeman and Kleiner (1999) provided evidence that union and nonunion rates of worker displacement were similar. Raphael (2000) used the same two DWS surveys to estimate union wage effects based on union transitions following job displacement. He concluded that longitudinal estimates based on arguably exogenous changes in union status produce union wage gaps similar to standard cross-section estimates of union wage gaps in the larger literature. Given the importance of each of these topics and the addition of eleven subsequent DWS surveys (1998-2018), it is surprising that researchers have not followed up on either of these articles. The purpose of our paper is to update and extend the analyses by Freeman-Kleiner and Raphael. Our analysis of the DWS covers twenty-two additional years, coupled with the advantage of far larger sample sizes than in the two previous studies. Evidence is provided on both union and nonunion displacement rates, as well as estimates of earnings changes associated with changes in union status between individuals' displacement and subsequent jobs.

Union Wage Gaps and Displacement: Background

A key concern regarding union wage gap estimates, going back at least to Lewis (1986), is omitted ability bias due to skill upgrading. The skill upgrading conjecture is that union employers can hire more productive workers given the presence of wage premiums, but that such skills are not fully observable to researchers. Lewis (and others) argued that skill upgrading would cause union wage gap estimates to be upwardly biased. Subsequent research called into question whether skill upgrading is substantial. First, such behavior need not

[1] Meta analyses of union wage and union profit effects are provided by Jarrell and Stanley (1990) and Doucouliagos and Laroche (2009), respectively.

follow from theory. Wessels (1994) provides a simple but persuasive challenge to the skill-upgrading hypothesis. If firms upgrade in response to union wage increases, unions can bargain in future contracts for wages sufficient to restore the premium. Anticipating this, employers may choose not to upgrade. Firms that do upgrade face higher future wage demands and will have distorted their factor mix toward a higher skill labor mix than is optimal given its technology.

An additional concern is that selection may differ across the skill 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 distribution (i.e., those with high ability, motivation, and reliability given their low levels of schooling, experience, etc.). In the right tail of the attribute distribution, workers with particularly high abilities may prefer work in nonunion companies where such abilities are more highly rewarded than in union workplaces with 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 such that OLS union wage gaps at the mean of the distribution provide roughly reliable union gap estimates.

A relatively direct way to account for unmeasured ability/productivity differences is to use longitudinal evidence, identifying union wage (or earnings) 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 (earnings) a function of the change in union status and changes in other non-fixed wage determinants. The two approaches are identical if there are two periods. The longitudinal approach has the advantage of accounting for worker heterogeneity, but it faces two potentially serious problems. First, given misreporting in union status coupled with a small number of union status changers over a one-year period, as observed in the Current Population Survey (CPS) and other data sets, the ratio of measurement error to signal is high (Freeman, 1984). As a result, union wage gap estimates are severely attenuated. Second, changes in union status typically occur due to job changes that are endogenous, determined in part by wage offers. As discussed below, use of the CPS Displaced Worker Surveys (DWS) largely avoids these two potential problems.

In the paper, we estimate union wage effects using the biennial CPS Displaced Worker Surveys from 1994 through 2018. 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 wage analysis builds on earlier 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 DWS helps overcome several of the difficulties involved in estimating union wage effects. 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 rather limited measurement error because the entire sample has changed jobs, sharply reducing the noise-to-signal ratio. Moreover, in the DWS a single respondent reports both prior and current union status in the same survey. By contrast, in the CPS union status changes are measured based on two separate reports on union status, one year apart, and possibly being reported by different household members.[3] In addition to relatively low measurement error, the DWS has the added advantage that job changes due to displacement are largely exogenous, particularly so when the sample is restricted to plant closings (Gibbons and Katz 1992).

A complementary topic is whether union workers are more likely or less likely to be displaced, particularly so for job loss due to plant closings. Evidence on displacement helps address the important question of union effects on firm performance and whether union businesses are more likely to fail than nonunion businesses. Freeman and Kleiner (1999) address 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 workers in the overall private workforce. As is the case for Raphael's study, we are unaware of

[2] Henry Farber has produced a series of papers since 1993 (for example, see Farber 1993, 2017) 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.

[3] In the CPS, a single person in the household is typically designated as the respondent for all household members. Roughly half of all reports in the CPS are provided by a "proxy" rather than a self-respondent.

studies that have followed up on Freeman and Kleiner's use of the DWS to compare union and nonunion displacement since the early 1990s.[4]

In what follows, we first provide descriptive evidence on the frequency of plant closings and other forms of displacement among union versus nonunion workers.[5] Rates of displacement are calculated for both union and nonunion workers from the early 1990s through the end of 2017, both during recessions and in boom years. In addition, union density rates among those displaced from private sector jobs are compared to union density in the overall private workforce, as previously done by Freeman and Kleiner (1999) for the 1994 and 1996 DWS. Following Raphael (2000), we then examine union wage effects based on displaced workers changing union status between their prior displacement job and their current wage and salary job.

Displaced Worker Surveys

The primary data sources in our analysis are the Displaced Worker Surveys (DWS), which have been administered biennially since 1984 in either January or February as supplements to the CPS, plus monthly CPS earnings files matched to the DWS. We begin with the February 1994 DWS, the first to report union membership status at the individual's displaced job.[6] The DWS supplements are administered only to individuals ages 20+ who have been classified as displaced. To be classified as displaced from a 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. 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). Workers who ended jobs due to a seasonal job

---

[4] 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 the 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 effect on survival.

[5] Other reasons for worker displacement are loss of job due to the position or shift being abolished and loss from insufficient work. [There is also an "other reason" category for job loss, but individuals reporting such a loss are not asked the DWS union question (as well as several other variables in the survey). Hence, these individuals are not included in our measures of displacement.]

[6] The 1994-2000 DWS supplements were administered in February and the 2002-2018 supplements in January. DWS supplements prior to 1994, which do not provide union status on the displaced job, were administered in January.

completed, a self-operated business failing, or "some other reason" are present in the DWS supplements, but not asked about union status on the displaced job. These workers are not included in our analysis. The supplements provide information on job characteristics of the displacement job such as weekly earnings, industry and occupation, tenure, and union status.[7]

In the month of the displacement survey (either January or February), individuals are also administered the regular monthly CPS questions including demographics and detailed information on current employment status, hours worked, location, industry, and occupation. Questions on earnings, hours, and union status on the current job are asked only of the quarter sample who are in the outgoing rotation groups. The remaining three-quarters of the sample are asked these questions when they are outgoing in one of the three subsequent months. We link information on earnings, hours, and union status during the outgoing rotation group months with the January or February DWS surveys, thus providing information on earnings and hours on both the current primary job and the displaced job.[8] 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 at the previous displacement job with earnings at the currently held primary job.

Unions and Job Loss: Evidence on Union and Nonunion Displacement Rates

We first provide estimates of the numbers of displaced union and nonunion private sector wage and salary workers from each DWS between 1994 and 2018. The displacement sample includes all workers displaced from a private sector wage and salary job, independent of whether or not they are currently employed.[9] Displacement is measured for each three-year period prior to the biennial DWS. Displacement *rates* for union and nonunion workers are calculated as follows. The numerator of the displacement rate is the estimated number of private sector union or nonunion workers displaced during the previous three years, measured within the DWS using supplement weights.[10] As noted in prior work, the DWS measure of

[7] 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.
[8] Individuals are matched using household ID by year, state, person line number within households, sex, and age range. Match rates were consistently in the 90-95% range, similar to the match rates seen in Raphael (2000). Individuals not matched are primarily those who changed residence between the time of the DWS and the administration of the outgoing rotation group survey.
[9] We subsequently provide evidence on the share of displaced workers employed at the time of the survey.
[10] In the 1994 DWS, "final weights" but not "supplement weights" are provided. For all subsequent years, the DWS includes supplement and final weights. For the 1994 DWS only, we rescale the final weights slightly upward based on the relationship between the final and supplement weights in the subsequent DWS surveys. In all other years we use the preferred supplement weights.

displacement fails to account for multiple displacements during the three year period. The denominator measures the population of employed private sector union members and nonunion workers, respectively, these estimates being 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 Unionstats.com.[11] For example, for the January 2018 DWS, the estimated population of employed private sector union members and nonmembers is averaged over the years 2015-2017.[12]

Displacement levels and rates are shown in Figure 1 for the three-year periods 1991-93 through 2015-17, based on the biennial DWS surveys conducted in 1994 through 2018. The displacement figures first provide measures that include all forms of displacement. We then show figures showing rates for the subset of displacements due to plant closings. Our analysis does not include individuals with job loss due to seasonal jobs completed, a self-operated business failed, or "some other reason." These individuals are not asked whether they were a union member on the displaced job. Appendix Tables 1a and 1b provide the estimates of union and nonunion displaced workers, employment, and displacement rates by the DWS survey years, as seen in Figure 1).

As seen in Figure 1 and Appendix Tables 1a and 1b, levels and rates of displacement clearly vary with the business cycle. The levels and rates of all displacements (i.e., job loss due to a plant or company closed down or moved, insufficient work, or position or shift abolished) were highest in 2007-2009 (as reported in the 2010 survey) for both union and nonunion workers, with rates of 15.3 and 14.5 percent respectively. The lowest levels and rates occurred in 2015-2017 (reported in 2018), with rates of 5.5 and 5.8 percent for union and nonunion workers. Appendix Table 1b provides identical information for the subset of displacements that are due to plant closures, which can be considered as largely exogenous (Gibbons and Katz 1992). Union (nonunion) rates of displacement from plant closures were 3.7 (3.8) percent during 2007-2009; in 2015-2017 the plant closure rates were 1.3 (1.8)

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

[12] In Farber's studies of displacement rates, he typically includes in the denominator an estimate of the number of displaced workers not currently employed. We have not included such estimates in this paper. Had we done so, displacement rates would be slightly lower, more so for union than nonunion workers. As shown subsequently, displaced union workers are somewhat less likely to be employed at the time the DWS is administered.

percent).

Figure 1 graphically shows the relative union and nonunion displacement rates by DWS survey year, showing both the rates for all union and nonunion displacements, as well as the subset of displacements that are from plant closings. The clear takeaway from Figure 1 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; the opposite is true in the other years. When we restrict the sample to the share of displacements from plant closings (in the lower portion of Figure 1), these displacements account for below half of all displacements. The displacement rates fall similarly, with the numerators measuring just those displacements due to plant closings (denominators are the same in both series). We see similar patterns over time for plant closures and the full sample of displacements, with less volatility (in absolute terms) in the plant closure sample.

Similar union and nonunion displacement rates support the conclusion that unionization is not associated with substantively higher (or lower) rates of business failure or insolvency. This conclusion was reached previously by Freeman and Kleiner (1999) based on the 1994 and 1996 DWS. That said, Freeman and Kleiner did not explicitly calculate displacement rates in their primary analysis. Rather, they reached their conclusion based on calculations of the percent of union workers among those recorded as displaced in 1994 and 1996, and then showed that this share was similar to union density among employed workers during those years. We provide an equivalent analysis across all DWS survey years through 2018, as shown in Figures 2a and 2b. Appendix Table 2 provides these density rates.

As seen in Figure 2a, union density measures (i.e., % union members) among workers displaced for any reason in each of the 13 displacement periods are highly similar to union density in the overall private sector. Private sector union density has been calculated using the CPS-ORGs and is reported at Unionstats.com (Hirsch and Macpherson 2003, updated annually).[13] Over the 13 DWS periods, union density rates were slightly higher in the displacement sample six of the periods and slightly lower in the other seven periods. Across all years, mean union density was 8.3 percent, versus 8.4 percent for the overall private sector (see the bottom line of Appendix Table 2). Evident in Figure 2a is that union density rates in the displacement samples trended downward over the 27 year period of recorded

[13] Union density measures posted at Unionstats.com use the same data and are equivalent to the density measures reported annually by BLS.

displacements (1991-2017) 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 show that the share of displaced workers who are unionized declined sharply over time. This decline simply mirrors the overall private sector decline in unionism. Our previous finding that displacement rates for union and nonunion workers are roughly the same stems from the similarity in union densities among private sector workers displaced and among private sector workers overall.

In the bottom rows of Appendix Tables 1a and 1b, we show the average rate of displacement for union and nonunion workers across all years, with equal weighting for each period. Remarkably, the overall displacement rates tallied over 20+ years are nearly identical for union and nonunion workers. Based on all recorded displacements, the aggregate rates round to 9.2 percent of union workers and 9.3 percent for nonunion workers. Restricting displacements to those due to plant closings also produce similar union and nonunion rates, 3.2 percent for both groups of workers.

Although union and nonunion displacement rates are highly similar over time, that does not rule out the possibility that rates might differ by union status if one conditioned on measurable worker attributes, location, or job type. To address this possibility, in Table 3 we provide results from probit displacement equations showing the marginal effects (evaluated at the means) of union status on displacement, using the 1994 through 2018 DWS, matched with the appropriate CPS outgoing rotation groups. In column (1) we regress displacement on union status with no covariates.[14] As seen previously from our estimated displacement rates in Table1a, union membership status is associated with a slightly lower probability of annual average displacement across all years for union versus nonunion workers (9.22 - 9.27 = -0.05 or one twentieth of a percent). In column (1) of Table 1, the marginal effect of union status (absent controls) is -0.0017, effectively indicating no meaningful union-nonunion difference in displacement probabilities. We did not expect these rates to be identical.[15] The

[14] This comparison is imperfect. Union status on the displacement job is provided in the DWS. For those not displaced during the past three-year period, we can measure their union status in the outgoing rotation group month associated with the displacement surveys (February-May during 1994-2000 or January-April during 2002-2018). This measure is noisy since union status may have changed over the three year displacement period.
[15] The Appendix Table 1 and regression samples (Table 1) do not produce identical displacement rates because the non-displacement samples (and their weights) differ from the two sources. Appendix Table 1 (our preferred measures) uses estimates of union and nonunion employment over the three displacement years from large CPS samples for each of the DWS reference years (as provided at Unionstats.com). By contrast, the displacement

denominators (i.e., the populations of employed union and nonunion workers) used to calculate displacement rates in Table 1 are based on the larger and more appropriate full-year ORG employment samples, as provided at unionstats.com. In contrast, the probit (or LPM) model has an implicit "denominator" (comparison group) that is a small subset of the ORG sample; that is, it includes only ORG workers who participated in the CPS during the January or February DWS surveys and who did not report a displacement.

The takeaway from Table 1 is clear-cut. The addition of controls in regressions (2) through (5) produce small absolute differences in displacement rates for union and nonunion workers. All regressions find a tiny negative effect of union membership on worker displacement. The estimated marginal effect of union membership in our most dense regression (column 4) is -0.0049, roughly half of one percent. Although this difference is more than double that seen absent controls (column 1), the magnitude of the union coefficient is modest. Note in column (5), however, that the union coefficient increases in absolute value from -0.0049 to -0.0076 when we omit the log wage measure, which is negatively correlated with displacement. Given that displacement is less likely for higher paid workers, inclusion of the wage partially absorbs any union wage effects on displacement. Arguably, inclusion of the wage measure (as in columns 2-4) understates the effect of unionization in deterring job displacement.[16] Overall, the displacement regressions confirm our previous conclusion that there exists little average difference in the probability of displacement for union and nonunion workers. That said, union coverage is associated with slightly lower job displacement, both with and without accounting for covariates.

Given that union members receive a substantive premium in wages and benefits, while at the same time having relatively small average effects on productivity and somewhat lower profitability, it is reasonable to ask why we do not see higher rates of displacement among union jobs.[17] It may be the case that union workplaces face somewhat stronger constraints in shutting down establishments than do nonunion workplaces. Some union contracts require that management inform and discuss possible closures. Moreover, unions often agree to decrease pay and benefits (e.g., two-tiered wage agreements) in order to prevent closures or

regressions include a full set of the displaced workers, but a much smaller non-displaced sample restricted to wage and salary workers in four outgoing rotation groups eligible for inclusion in each DWS (February-May during 1994-2000 and January-April during 2002-2018).

[16] We thank one of our referees for this suggestion.

[17] Doucouliagos and Laroche (2003, 2009) provide meta-analyses of union effects on productivity and profits, respectively.

substantive layoffs.[18] That said, private sector union density has fallen substantially over time, from 24.2 in 1973, to 10.3 in 1995, to 6.4 percent in 2018 (Hirsch and Macpherson, 2003, updated annually at unionstats.com). Although job displacement has not differed subsequently for union and nonunion workers, job creation has been disproportionately nonunion. Most new jobs are born nonunion and stay nonunion.

Union Wage Gap Estimates from the DWS

As discussed in the introduction, the DWS has advantages for estimation of union-nonunion wage differentials, providing measures of earnings change associated with changes in union membership among workers subjected to an exogenous job change. Our analysis builds on similar work by Raphael (2000) that used the 1994 and 1996 DWS. We extend the analysis to the 1994 through 2018 period (i.e. 13 rather than two DWS biennial surveys). The analysis is restricted to workers whose displacement job was in the private sector, but we retain workers moving from a private displacement job to a subsequent public sector job (6 percent of our estimation sample). Although the public sector is highly unionized it has low rates of displacement. The Bureau of Labor Statistics (BLS) provides summary statistics for each DWS survey, providing displacement levels (for all reasons) by sector both for long-tenured workers (3+ years) and all workers (US BLS, 2016; US BLS, 2018). For the 2016 DWS, the share of long-tenured displaced workers who were displaced from public sector jobs was minimal, roughly 1-in-20 (5.2%). For all displaced workers, the public share was even lower (4.5%). The equivalent public sector shares from the 2018 DWS were lower than in the 2016 survey, 4.0% and 3.2%. Given the tiny samples of displaced public sector workers, we conclude that the DWS is not an attractive data set for estimating public sector union earnings gaps.

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

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

[18] As stated by Freeman and Kleiner (1999, p. 526): "Unions reduce profits but they do not 'destroy the goose that lays the golden egg.' They would be foolish to do so, and while they may make mistakes in collective bargaining (just as management may), they are not so foolish as to force organized firms out of business."

where W is either real weekly or hourly earnings;[19] X is a vector of worker, location, and job attributes (results are shown using alternative sets of controls); 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:

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

We designate U as 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 earnings or wage changes between union (nonunion) displacement jobs and subsequent nonunion (union) reemployment jobs. As seen 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 provide estimates allowing differences in magnitude for NU versus UN and for UU versus NN.

In accordance with analysis by Bollinger and Hirsch (2006), we remove all CPS-ORG observations with an imputed wage for their current job from the dataset. As stated previously, DWS earnings measures are not imputed. The ORG imputation method assigns the wage of a donor to nonrespondents with "similar" attributes. Union status is not a match attribute; hence, the assigned wage does not reflect union status (or other attributes not matched), thus attenuating estimates of the union wage gap (so-called match bias). Bollinger et al. (2019) show that regression results for samples of CPS respondents produce OLS (mean) coefficients highly similar to those from full-sample regressions using matched administrative earnings data for both CPS respondents and nonrespondents. An additional

[19] Weekly earnings on the displaced job is asked of all persons displaced. Weekly hours worked is not reported, but full-time/part-time status is reported and controlled for in our wage regressions. Hourly earnings is asked of hourly workers, roughly half of the total sample. We subsequently compare estimates for the hourly sample using both the hourly and weekly earnings measures; little difference in union wage gaps is found. Current earnings are indexed to the January CPI in the survey year. Displacement earnings are indexed to the annual CPI in the mid-displacement year (e.g., workers in the 2018 survey have their displacement earnings indexed to 2016). Note that BLS/Census does not impute earnings measures for the displacement job. Workers not reporting their displacement (or current) earnings are excluded from our wage analysis, but are included in the analysis comparing union and nonunion displacement rates. Recent work by Bollinger et al. (2019) using CPS data matched to administrative tax records shows that earnings nonresponse is particularly high in the left and far right tails of the earnings distribution, but relatively flat throughout most of the earnings distribution. Standard regression estimates at the means based on respondent-only samples have minimal response bias.

refinement we provide is to drop a small number of extreme outliers with very high or low percentage changes in wages between their displaced and current jobs.[20]

Our presentation of results adopts the following approach. First, in Table 2, we provide detailed regression results for five alternative specifications using the sample of all displaced workers. We subsequently summarize the union wage gap estimates with alternative samples and specifications, but do not show coefficients on the control variables. We separately show union wage gap estimates based on the subsamples of plant closures, which have the advantage of restricting the samples to displacements and job changes most likely to be exogenous (e.g., Gibbons and Katz 1992). In addition, we provide estimates that allow union wage gap estimates to differ between union joiners (NU) and leavers (UN), as well as allowing differences for union and nonunion stayers (UU vs. NN). Finally, we present results from samples restricted to hourly workers only, which allows us to compare differences in union wage estimates using alternative dependent variables, both the change in the log of weekly earnings (our principal measure) and the log of hourly wages, the latter available for hourly workers only. We are not aware of previous studies that have utilized the DWS hourly wage measure. The obvious advantage of using the hourly earnings measure is that it measures pay for an explicit time period of work, as compared to weekly earnings paid to workers with substantive variation in hours worked. The downside of using the hourly wage measure is that it restricts the sample to hourly workers, thus excluding the roughly 40 percent of economy-wide wage and salary workers whose primary jobs are salaried. The weekly earnings measure varies substantially across workers due to work-hour differences. The DWS does designate full-time versus part-time jobs, however, which is an important control in our wage regressions.

Table 2 provides earnings change regression results for our full displacement sample, individuals 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) at the time of the survey.[21]

[20] We thank Hank Farber for the suggestion to remove individuals reporting extreme wage changes (see, also, Farber 2017). Specifically, we restrict the change in log wage variable to values from -2 to 2, and the log weekly earnings measure from -2.75 and 1.94; these bounds are approximately four standard deviations below and above the mean. Union wage gap estimates are a few percentage (log) points higher absent restrictions on wage change outliers. Had we included ORG earnings imputations in our sample, we would have observed considerably more (but mostly false) extreme wage change values. As stated previously, we exclude observations with ORG imputations in order to avoid match bias.

[21] As stated previously, we do not include individuals with job loss due to seasonal jobs completed, a self-operated business failed, or "some other reason." Union member status is not reported for these individuals.

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 union gap (i.e., no controls) of 0.167 log points (18.2 percent).[22] As described below, the union wage gap estimates decline following inclusion of controls. In column (2), controls include whether a worker changed full-time or part-time status, changed location of residence (new city or county) since displacement, changed detailed industry, changed detailed occupation, and categorical dummies for age and job tenure on the displacement job. In column (3) additional controls are added for time period (i.e., survey year), gender, race, ethnicity, marital status, age and tenure, education, and geography (state fixed effects and MSA size dummies). Column (4) adds broad industry and occupation dummies of the current job, and column (5) includes a dense set of industry and occupation dummies. The union wage gap estimates vary from 0.167 absent controls to between 0.141 and 0.146 in columns (2) through (5). Addition of detailed industry and occupation controls increases R^2 values substantively, from roughly 0.20 in columns (2) through (4) to about 0.30 in column (5). Inclusion of dense controls has minimal effects on union wage gap estimates. The wage gap estimate is 0.143 (15.4 percent) in column (5), our most dense earnings change regression.

Table 3 provides a summary of estimates of the union wage gap using alternative samples (all displacements versus plant closures only; all wage and salary or only hourly workers) and alternative dependent variables (i.e., weekly or hourly earnings measures). For each sample, we provide union wage gap estimates based on the same specifications (1) through (5) seen in Table 2. Coefficients on control variables are not shown in Table 3. For all samples and specifications, the estimated control variable coefficients are highly similar to those seen in Table 2.

Line 1 of Table 3 provides summary union wage gap estimates for the five specifications using the full sample and the weekly earnings measure, as seen previously in Table 2. In line 2, we show union wage gaps for the subset of displacements that resulted from plant closures. The plant closure samples consistently produce slightly larger union gap

[22] We will subsequently 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 the approximate arithmetic percentage is [exp(β)-1]100, where β is the log gap. A more exact conversion accounts for the standard errors (Kennedy, 1981).

estimates than do the full samples, typically a 1 or 2 percentage point difference (compare lines 2 to 1, 4 to 3, and 6 to 5).

The third and fourth line samples shown in Table 3 are restricted to hourly workers, which cuts the samples by over half. The hourly samples tend to produce slightly larger union wage gap estimates than do the comparable full samples including hourly and salaried workers (compare estimates from lines 3 to 1 and 4 to 2). Lines 5 and 6 also restrict the sample to hourly workers, but instead uses the weekly rather than hourly earnings measures. A comparison of coefficients from lines 5 to 3 and 6 to 4 allows us to compare differences in results using the alternative earnings measures with identical samples. Union gap coefficients are systematically larger using the weekly rather than hourly earnings measure; sample sizes differ slightly in these comparisons since nonresponse for the hourly and weekly earnings measures are not identical. Consistent with the differences described above, the plant closing sample using the weekly earnings measure (line 6) produces the largest union wage gap estimates, on the order of 0.18-0.19 log points for specifications with controls, as compared to the approximate 0.14 log points seen for comparable specifications using the full sample (line 1).

We next drop the assumption that wage gains (losses) from joining (leaving) a union job are symmetric. In Table 4, we find reasonably clear evidence that losses from leaving a union job exceed gains from joining a union job, as found by Raphael (2000) using the 1994 and 1996 DWS. Table 4 has the exact same sample structure as does Table 3, the only change being that we separately estimate earnings changes for workers transitioning from a nonunion displacement job to a current union jobs (NU) and for those changing from a union displacement job to a nonunion current job (UN). We also include a “remain union” variable (UU), with “remain nonunion” (NN) being the reference group whose earnings change is reflected in the intercept.

The notable outcome seen in Table 4 is that for most samples, we observe particularly large wage losses moving from a displaced union job to a current (i.e., at the time of the survey) nonunion job. For nearly all specifications and samples, we obtain wage loss estimates of roughly 0.20 log points moving from a union to a nonunion job (UN). We observe smaller gains accompanying moves from nonunion displaced jobs to current nonunion jobs (NU), on the order of 0.10 log points in samples 1, 2, 3, and 5. These results are consistent with Canadian evidence from Kuhn and Sweetman (1998), who found that displaced union workers with high tenure levels had particularly large wage losses. Large wage losses in UN transitions would occur if there are large losses in firm-specific skills, but we have no direct evidence that this is the principal explanation for such losses.

That said, differences between wage losses from UN transitions and gains from NU transitions are more limited for the small sample of hourly workers displaced due to plant closures (lines 4 and 6). We note that the symmetry in union wage gains seen for the sample of hourly workers displaced by plant closures in line 4 does not show up for the same group in line 6, the difference between the two being that the wage measure in line 4 is an hourly wage whereas the wage measure in line 6 is weekly earnings. In contrast to the approximate NU, UN symmetry seen in line 4, use of the weekly earnings dependent variable in line 6 produces larger wage losses from UN than wage gains for NU transitions. These results suggest that weekly work hours declined among displaced workers transitioning from union to nonunion jobs, as compared to NU transitions. While some of the asymmetry in the UN and NU wage effects reflects changes in hours worked not accounted for in the weekly wage measure, it cannot explain all the differences. Based on the entire hourly sample (lines 3 and 5), we observe asymmetry in UN and NU wage effects using both the hourly (line 3) and weekly (line 5) earnings measures.[23]

Overall, our DWS evidence clearly shows that estimates of union wage effects based on longitudinal evidence and exogenous job changes are substantial. There is minimal attenuation of the union change coefficients from mismeasurement given that the sample includes only true job changers (i.e., displaced workers), thus resulting in relatively high levels of true changes in union status. This is in stark contrast to the high error rates on the reported change in union status using matched CPS panels one year apart, as found in studies by Freeman (1984), Card (1996); and Hirsch and Schumacher (1998). These studies provided alternative adjustments, albeit imperfect ones, for the substantial attenuation due to misreporting of union status.

[23] As seen in Table 4, UU coefficients tend to be positive and, in some specifications, substantive, indicating that wage change is higher for union stayers than for the nonunion stayer base group (NN). These coefficients are most substantial using the full sample weekly earnings measure. UU coefficients are tiny and insignificant when using the sample of hourly workers with a measure of the hourly wage. The implication is that the large NN group has had a decrease in weekly hours worked (relative to the UU group) between their displacement and current jobs.

Our analysis strongly supports the conclusion that *average* union wage effects over the 1994-2018 time period have been on the order of 15 percent. Union wage gap estimates of about 15 percent have a long history. Earlier work by H. Gregg Lewis suggested union wage gaps of roughly 15 percent based both on industry-level (Lewis 1963) and micro-level (Lewis 1986) data. Recent studies using early micro-level data from the 1950s and beyond also find strong support for union wage gaps in the neighborhood of 15 percent (Farber et al. 2018; Callaway and Collins 2018).

Micro-level union wage gaps compiled annually from the CPS, beginning in 1973 and through 2018, find recent union wage gaps of roughly 15 percent, with private sector union gaps exceeding 15 percent and public sector union gaps below 15 percent. In Table 5, we provide estimates of total, private sector, and public sector union wage gaps, compiled annually from the monthly CPS-ORG files (Hirsch and Macpherson 2019, Table 2a, pp. 21-22). In the three columns on the right-side of Table 5, we report the annual estimates for the years 1994-2018, which correspond to the period over which our 13 DWS surveys were conducted. Shown are the annual CPS log difference union wage gap estimates, which include time-consistent controls for workers, geography, and job attributes (broad industry and occupation). The unweighted mean union wage gaps across the 25 years is shown on the bottom line of the table. The average union gap for all wage and salary workers is 0.163, the private sector mean gap is 0.195, and the public sector mean gap is 0.104. The overall decline in economy-wide union wage gaps, from roughly 0.21 to 0.14, reflects declines in both the private and public sectors.

Although our primary focus is not on changes in union earnings differentials over time, we do examine whether there is any clear-cut pattern over time in union gaps from the DWS. Using our full sample, as shown previously in Table 2 and in Line 1 of Table 5, we interact the union variable ΔU with each DWS survey year, hence providing separate union earnings gaps for the 13 DWS periods. In left-side columns of Table 5, we first show the DWS union gap estimates for each DWS survey displacement period, 1991-1993 through 2015-2017. As discussed previously, these union gap estimates reflect union-nonunion differences in earnings among workers' current job during the DWS survey periods relative to earnings in their previous displaced jobs in the past three years. As expected, given the small samples of union members for each DWS survey, we see substantial variation in the DWS union gap estimates over time (shown on the left-side of Table 5). That said, there is a relatively clear-cut downward trend in the DWS union earnings effects, as seen in the larger literature. Using moving averages of three DWS periods, we see union earnings differentials declining from 0.18 to only 0.10 between the earliest and most recent DWS periods. The average DWS union earnings gap is 0.14 across all displacement periods.

Why the gradual decline in union wage gaps? The most likely explanation is that union bargaining power has weakened over time as overall union density has declined. An older literature (e.g., Freeman and Medoff 1981) finds that the higher (lower) the percentage of workers unionized in given markets leads to higher (lower) wages for both union and nonunion workers. The wage effects for union workers exceed those for nonunion workers. Thus, we would expect average union wage premiums to gradually fall as overall union density declines. Moreover, interpretation and application of labor law by the five-person National Labor Relations Board (NLRB) varies over time. Board members are nominated by the President, with the majority of Board members (typically 3 to 2) leaning toward the views of Democrats or Republicans, depending on the parties of the President and the Senate majority.

It is worth noting that the DWS union earnings gaps tend to be slightly lower than the standard CPS union gaps, with an average 0.140 over all survey years, versus the CPS mean of 0.163 over the years 1994-2018. Apart from the many differences between the DWS sample of displaced workers versus the CPS sample of all wage and salary workers, the methodologies of the two sets of estimates are fundamentally different. The CPS provides standard wage level analysis comparing the wages of union members with the wages of "similar" nonunion workers, approximated through regression controls for measurable worker, location, and job attributes. By contrast, The DWS union gap estimates rely on longitudinal analysis, comparing differences in earnings for displaced workers whose union status changed between their current jobs at the time of the survey and their previous displaced job.

In principle, the DWS analysis has the advantage of accounting for unmeasured person fixed effects (e.g., skills), arguably providing estimates of union earnings effects that account for selection. The finding that union earnings gaps based on DWS longitudinal analysis are on average about 2 percentage points lower than CPS cross-section analysis (0.144 versus 0.163) is of some interest. A lower DWS than CPS union gap is what one would predict if there was positive ability bias in the CPS due to unmeasured positive productivity traits among union relative to nonunion workers. The DWS analysis controls for person effects, although some portion of displaced workers skills may not be fully nontransferable between the displacement and current jobs. Although our results are consistent with an ability bias interpretation, the many other differences between the DWS and CPS methodologies prevent us from placing substantial weight on such an interpretation.

An interesting question is why we see a relatively narrow range of average union wage effects between 10 and 20 percent over time, centered roughly around 15 percent? We speculate that unionization would be neither stable nor viable were average wage effects well below, say, 10 percent or well above 20 percent. If average union wage effects were quite small, it's unlikely we would have seen historically strong support and substantive shares of workers supporting NLRB union organizing campaigns. If union wage and benefit effects were extremely large, say in the 20-plus percent range, managerial pressure to limit high compensation costs would be substantial, particular so in competitive U.S. markets. If union-nonunion compensation differences were to increase well beyond historical levels, we suspect that managerial opposition to union organizing would become even fiercer than what is seen currently.[24]

Additional Evidence on Wage Effects from Displacement

Our analysis has focused on the estimation of union-nonunion wage differentials based on transitions in union status following job loss and subsequent employment. Independent of changes in union status following displacement, earnings changes in moving from a displaced private sector job to a new job differ with respect to worker and job attributes. Coefficients on age and job tenure dummies in our earnings change regressions consistently indicate larger wage losses (or slower wage growth) for older workers and for those with longer tenure in the displacement job. Wage losses are particularly large for workers ages 55 and over and among workers with 20 plus years of tenure. Coefficients on the indicator variables measuring whether displaced workers changed detailed industry and detailed occupation each show substantial wage losses of roughly 6-7 percent from each, with minor differences depending on the specification. These qualitative results are consistent with prior evidence of wage

[24] Although collective bargaining systems differ substantially across countries, we expect that in most countries union wage gaps are similarly constrained, being neither negligible nor enormous. As an example, Rios-Avila and Hirsch (2014) examine union wage gaps and wage dispersion among two Latin American countries, Bolivia and Chile. They find mean conditional union wage gaps just under 15 percent, 14 percent for Chile and 12 percent for Bolivia. Raw wage gaps are substantially higher. Throughout the earnings distribution conditional union wage effects are substantial, the exception being a tiny union effect in the 90th quantile.

declines associated with industry- and occupation-specific human capital losses (e.g. Neal 1995, Helwege 1992). Displaced workers in relatively large metro areas have lower wage losses than those who live in small metro or rural areas. Those who moved their residence across cities or counties following displacement tend to have lower wage losses than do non-movers, on the order of 4-5 percent. For all samples and specifications, women realize wage losses 3-4 percent less than do men.

Our analysis of wage effects due to moving between union and nonunion jobs masks the broader question of overall earnings losses (or gains) associated with displacement. That is not the focus of our analysis. Papers by Farber, most recently Farber (2015, 2017), provide detailed analysis of this issue. He typically focuses on changes in weekly earnings, finding little aggregate loss 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 the sample of hourly workers for whom we observe their hourly wage, we find little average loss in real hourly earnings between workers' displacement and current jobs. As found previously by Farber, we find modest average losses using the weekly earnings measure for the full sample of displaced workers. Such a calculation does not account for earnings increases that workers would have realized absent the displacement (see Farber 2015, 2017). Nor does it account for the possibility that a future displacement might occur (Krolikowski 2018).[25]

The earnings analysis shown previously necessarily was conducted only for displaced workers reemployed at the time of the survey. In Table 4, we showed that earnings losses are particularly large for displaced union workers reemployed in nonunion jobs. If workers displaced from union and nonunion jobs have different reemployment rates, however, we may misstate the relative union-nonunion financial losses of displacement based solely on reemployed workers. Table 6 provides reemployment rates for two samples. The larger one (n=36,641) is an expanded sample that includes displaced workers who did not report displacement job earnings (or information on other key variables) required for the analyses in our paper. For this expanded sample, reemployment rates among union members are 6.54 percentage points lower than for nonunion displaced workers, 58.0 versus 64.6 percent,

[25] As analyzed by Krolikowski (2018), losses resulting from an initial displacement are overstated if compared to workers never displaced. Such a comparison ignores the possibility that displaced workers might have future displacement as well.

respectively. In short, displaced union workers are somewhat less likely to be reemployed and thus not observed in our previous analysis of earnings differences in union and nonunion displacement and current jobs.

We also provide reemployment rates for our more restricted sample that includes only those displaced workers who reported displacement job earnings and other key variables (n=26,958). Reemployment rates among union members were 59.4 percent versus 67.2 percent among displaced nonunion workers, a 7.8 percentage point difference.

Restricting the samples further to plant closings (roughly a third of all displacements), we see substantially larger differences in union and nonunion reemployment rates, 11.0 and 12.9 percentage point lower union reemployment rates for the two plant closure samples. In short, job displacement among union workers due to plant closures are associated with particularly low rates of reemployment.

In Table 7, we show the marginal effects from probit equations estimating the determinants of reemployment. Column 1 (with no controls) provides an expanded sample that includes displaced workers who did not report either earnings or information on other variables required for our previous analysis. Moving from column (1) to columns (2-6), we shift from the full sample to a much smaller sample including all covariates. The decrease in sample size primarily reflects the loss of those absent reported earnings on the displacement job (there were no earnings imputations in the DWS), with modest additional losses due to other missing variables. Reemployment is more likely among whites (the omitted reference group), men, and workers more highly skilled and with higher wages in their displacement jobs. Union workers are substantially less likely to be reemployed, even with rich sets of controls. The inclusion of occupation and industry controls (from the displacement job) sharply reduces the estimated union effect on reemployment, from -0.069 to -0.046 (column 5 versus 6).

We do not directly observe the potential wage losses for the share of displaced workers, union and nonunion, who are not reemployed. An implication of lower union reemployment rates is that financial losses for displaced union workers include not only lower rates of pay among those reemployed, but also lower income resulting from lower rates of employment. If displaced union workers who exit the labor force face particularly large wage losses (as compared to displaced nonunion workers), it is possible that estimates of relative union-nonunion wage losses may be understated. That said, there are multiple reasons why union and nonunion reemployment rates differ. One possible reason is that displaced union workers may be less employable and/or face lower wage offers, perhaps because of less transferable human capital. Alternatively, displaced union workers had likely received a union wage premium; thus, they may have higher reservation wages for a post-displacement job. Moreover, displaced union workers are more likely to have received retiree health benefits or pensions than have displaced nonunion workers, moderating the financial impact of displacement and lessening the need for reemployment.

Conclusion

Our analysis of displaced workers over more than two decades has two clear-cut takeaways, reinforcing earlier research by Freeman and Kleiner (1999) and Raphael (2000). First, displacement rates among union and nonunion workers are remarkably similar. In any given period union displacement may be somewhat higher or lower than nonunion displacement, but there is no substantive long-run difference. Union status appears to have a minimal effect on displacement and, by extension, business failure. Second, wage analysis based on displaced workers moving between union and nonunion jobs shows that union wage effects are sizable, on the order of 15 percent. Wage losses to workers switching from union jobs to nonunion jobs are larger than are gains from transitions into union jobs following displacement. Losses to displaced union workers may be understated given that fewer displaced union workers reenter employment.

References

Abowd, John M. and Henry S. Farber. 1982. Job Queues and the Union Status of Workers. *Industrial and Labor Relations Review* 35(3): 354-67.

Bollinger, Christopher R. and Barry T. Hirsch. 2006. Match Bias from Earnings Imputation in the Current Population Survey: The Case of Imperfect Matching. *Journal of Labor Economics* 24(3): 483-519.

Bollinger, Christopher R., Barry T. Hirsch, Charles M. Hokayem, and James P. Ziliak. 2019. Trouble in the Tails? What We Know about Earnings Nonresponse Thirty Years after Lillard, Smith, and Welch. *Journal of Political Economy* 127(5): 2143-2185.

Callaway, Brantley and William J. Collins. 2018. Unions, Workers, and Wages at the Peak of the American Labor Movement. *Explorations in Economic History* 68: 95-118.

Card, David. 1996. The Effect of Unions on the Structure of Wages: A Longitudinal Analysis. *Econometrica* 64(4): 957-79.

DiNardo, John and David S. Lee. 2004. Economic Impacts of New Unionization on Private Sector Employers: 1984-2001. *Quarterly Journal of Economics* 119(4): 1382-441.

Doucouliagos, Christos and Patrice Laroche. 2003. What Do Unions Do to Productivity? A Meta-Analysis. *Industrial Relations* 42(4): 650-91.

Doucouliagos, Christos and Patrice Laroche. 2009. Unions and Profits: A Meta-Regression Analysis. *Industrial Relations* 48(1): 146-84.

Dunne, Timothy and David A. Macpherson. 1994. Unionism and Gross Employment Flows. *Southern Economic Journal* 60(3): 727-38.

Farber, Henry S. 1993. The Incidence and Costs of Job Loss: 1982-1991. *Brookings Papers on Economic Activity: Microeconomics*: 73-132.

Farber, Henry S. 2015. Job Loss in the Great Recession and its Aftermath: U.S. Evidence from the Displaced Workers Survey. NBER Working Paper No. 21216, May.

Farber, Henry S. 2017. Employment, Hours, and Earnings Consequences of Job Loss: US Evidence from the Displaced Workers Survey. *Journal of Labor Economics* 35(S1): S235-72.

Farber, Henry S., Daniel Herbst, Ilyana Kuziemko, and Suresh Naidu. 2018. Unions and Inequality Over the Twentieth Century: New Evidence from Survey Data. NBER Working Paper No. 24587, May.

Freeman, Richard B. 1984. Longitudinal Analyses of the Effects of Trade Unions. *Journal of Labor Economics* 2 (1): 1-26.

Freeman, Richard B. and Morris M. Kleiner.1999. Do Unions Make Enterprises Insolvent? *Industrial and Labor Relations Review* 52(4): 510-27.

Freeman, Richard B. and James L. Medoff. 1981. The Impact of the Percent Organized on Union and Nonunion Wages. *Review of Economics and Statistics* 63(4): 561-72.

Gibbons, Robert and Lawrence Katz. 1992. Does Unmeasured Ability Explain Inter-Industry Wage Differentials? *Review of Economic Studies* 59(3): 515-35.

Helwege, Jean. 1992. Sectoral Shifts and Interindustry Wage Differentials. *Journal of Labor Economics* 10(1): 55-84.

Hirsch, Barry T. and David A. Macpherson. 2003. Union Membership and Coverage Database from the Current Population Survey: Note. *Industrial and Labor Relations Review* 56(2): 349-54 (updated annually at Unionstats.com).

Hirsch, Barry T. and David A. Macpherson. 2019. *Union Membership and Earnings Data Book: 2019 Edition*. Arlington VA: Bloomberg Law (published annually).

Hirsch, Barry T. and Edward J. Schumacher. 1998. Unions, Wages, and Skills. *Journal of Human Resources* 33(1): 201-19.

Jarrell, Stephen B. and T.D. Stanley. 1990. A Meta-Analysis of the Union-Nonunion Wage Gap. *Industrial and Labor Relations Review* 44(1): 54-67.

Kuhn, Peter and Arthur Sweetman. 1998. Wage Loss Following Displacement: The Role of Union Coverage. *Industrial and Labor Relations Review* 51(3): 384–400.

Kennedy, Peter E. 1981. Estimation with Correctly Interpreted Dummy Variables in Semilogarithmic Equations. *American Economic Review* 71(4): 801.

Krolikowski, Pawel. 2018. Choosing a Control Group for Displaced Workers. *Industrial and Labor Relations Review* 71(5): 1232-54.

Lewis, H. Gregg. 1963. *Unionism and Relative Wages in the United States: An Empirical Inquiry*. Chicago: University of Chicago Press, 1963.

_________. 1986. *Union Relative Wage Effects: A Survey*. Chicago: University of Chicago Press, 1986.

Neal, Derek. 1995. Industry-Specific Human Capital: Evidence from Displaced Workers. *Journal of Labor Economics* 13(4): 653-77.

Rios-Avila, Fernando and Barry T. Hirsch. 2014. Unions, Wage Gaps, and Wage Dispersion: New Evidence from the Americas. *Industrial Relations* 53(1): 1-27.

Raphael, Steven. 2000. Estimating the Union Earnings Effect Using a Sample of Displaced Workers. *Industrial and Labor Relations Review* 55(3): 503-21.

U.S. Bureau of Labor Statistics. 2016. *Worker Displacement: 2013-15*. News Release, USDL-16-1731, August 25.

U.S. Bureau of Labor Statistics. 2018. *Worker Displacement: 2015-17*. News Release, USDL-18-1370, August 28.

Wessels, Walter. 1994. Do Unionized Firms Hire Better Workers? *Economic Inquiry* 32(4): 616-29.

Figure 1: DWS Displacement Rates (%) in Prior Three Years, by Union Status, All Displacements and the Plant Closure Subset, 1994-2018

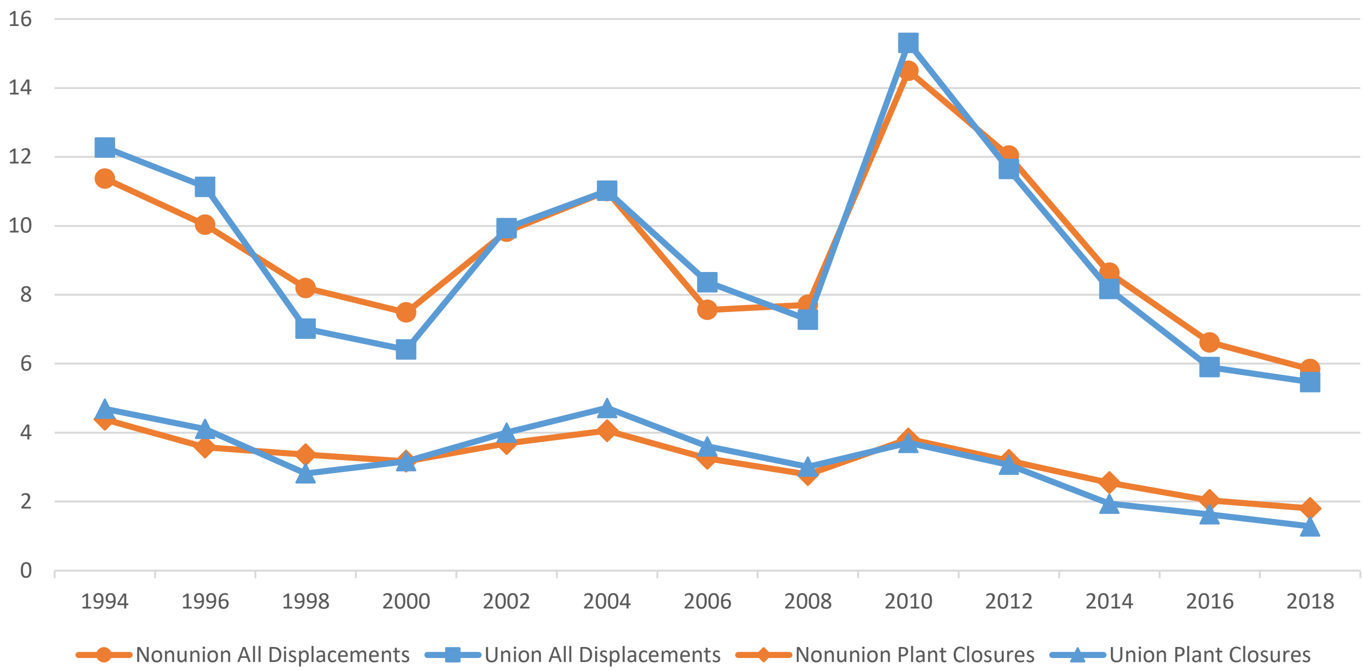

Figure 2a: Comparison of Union Densities (prior three years) among Workers Displaced for All Reasons versus All Private Sector Workers

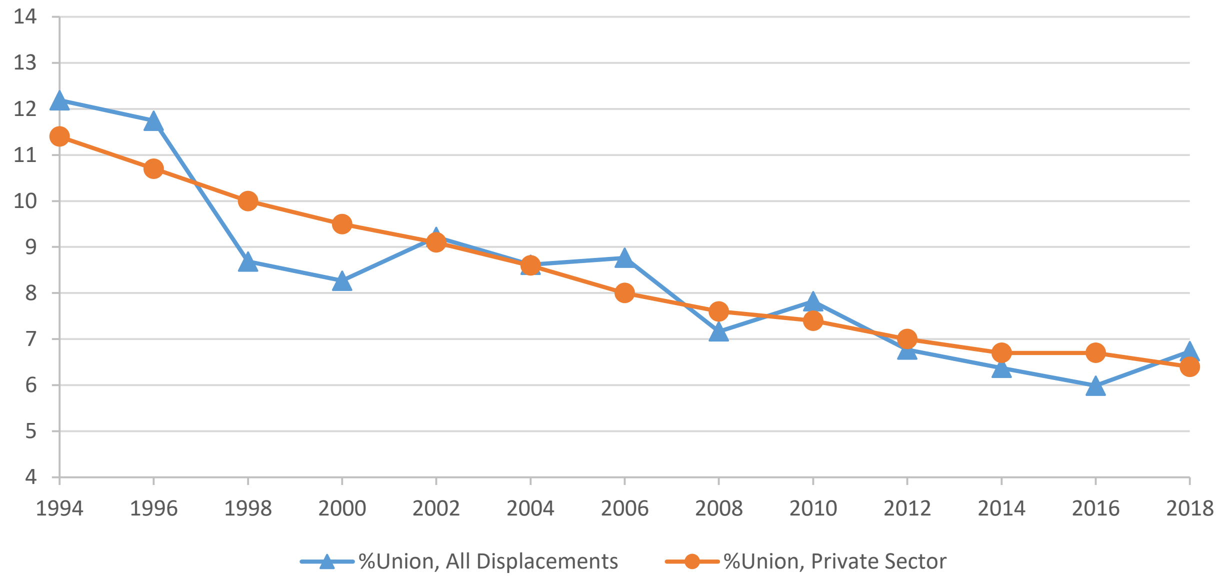

Figure 2b: Comparison of Union Densities (prior three years) among Workers Displaced by Plant Closures versus All Private Sector Workers

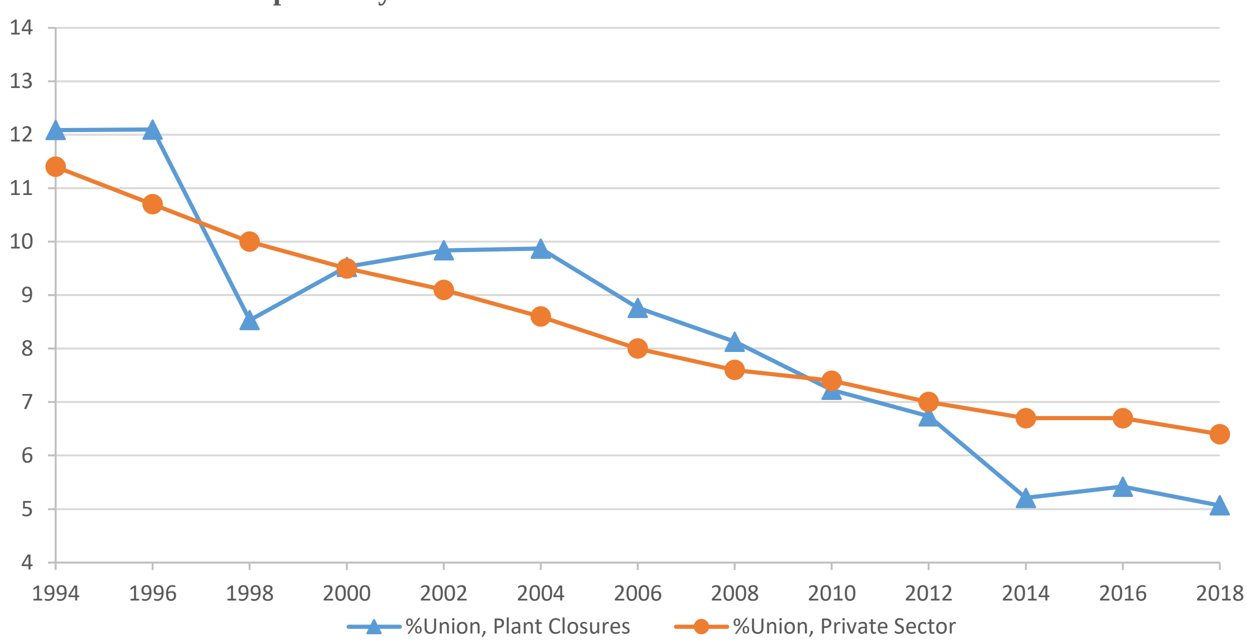

Table 1: Probit Displacement Determinants, Marginal Effects

Variables	(1) Displaced	(2) Displaced	(3) Displaced	(4) Displaced	(5) Displaced
Union (displacement job)	-0.0017	-0.0004	-0.0033**	-0.0049***	-0.0076***
	(0.0014)	(0.0014)	(0.0014)	(0.0015)	(0.0015)
Log wage (disp. job)		-0.0101***	-0.0135***	-0.0136***	
		(0.0005)	(0.0005)	(0.0006)	
HS Grad		-0.0013	0.0009	0.0033**	0.0005
		(0.0015)	(0.0016)	(0.0016)	(0.0016)
Some Coll		0.0023	0.0048***	0.0094***	0.0064***
		(0.0016)	(0.0017)	(0.0017)	(0.0017)
Associate degree, academic		0.0006	0.0013	0.0018	0.0018
		(0.0024)	(0.0024)	(0.0024)	(0.0024)
Associate degree, professional		0.0000	0.0018	0.0035*	0.0020
		(0.0022)	(0.0022)	(0.0022)	(0.0022)
College graduate (BA/BS)		-0.0023	0.0012	0.0087***	0.0013
		(0.0017)	(0.0018)	(0.0018)	(0.0018)
MA		-0.0021	-0.0017	0.0020	0.0002
		(0.0019)	(0.0019)	(0.0020)	(0.0020)
PhD & Professional degree		-0.0218***	-0.0228***	-0.0196***	-0.0222***
		(0.0036)	(0.0036)	(0.0036)	(0.0036)
Female			-0.0225***	-0.0143***	-0.0101***
			(0.0009)	(0.0009)	(0.0009)
Black			-0.0036**	-0.0015	-0.0001
			(0.0014)	(0.0014)	(0.0014)
Hispanic			-0.0021	-0.0021	-0.0008
			(0.0013)	(0.0013)	(0.0013)
Married w/ spouse			-0.0097***	-0.0100***	-0.0110***
			(0.0009)	(0.0009)	(0.0009)
Sets of dummies:					
Age	No	Yes	Yes	Yes	Yes
Time Period	No	No	Yes	Yes	Yes
Geography	No	No	Yes	Yes	Yes
Broad Occupation	No	No	No	Yes	Yes
Broad Industry	No	No	No	Yes	Yes
Observations	432,656	432,656	432,656	432,656	432,656
Pseudo R-square	0.0000	0.0056	0.0113	0.0222	0.0190
Log Likelihood	-2.172e+08	-2.160e+08	-2.147e+08	-2.124e+08	-2.131e+08

Notes: Shown are the probit marginal effects evaluated at the means, using CPS final weights. See text for further discussion. Omitted reference groups as high school dropouts, non-Hispanic white, age less than 25, and not married. Geography includes dummies for MSA and state fixed effects.

Data: 1994-2016 Biennial Displaced Worker Surveys matched to CPS Outgoing Rotation Group files, February-May 1994-2000, and January-April 2002-2018.

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 2: Estimates of Union Wage Differentials for Displaced Workers
Full Sample, Displaced for Any Reason, Weekly Earnings Dependent Variable

Variables	(1)	(2)	(3)	(4)	(5)
Δ Union	0.167***	0.146***	0.143***	0.141***	0.143***
	(0.015)	(0.013)	(0.013)	(0.013)	(0.014)
PT-FT		0.574***	0.569***	0.533***	0.508***
		(0.041)	(0.041)	(0.040)	(0.041)
FT-PT		-0.439***	-0.439***	-0.464***	-0.488***
		(0.027)	(0.027)	(0.027)	(0.028)
FT-FT		-0.005	-0.013	-0.061**	-0.119***
		(0.026)	(0.026)	(0.026)	(0.027)
Mover		-0.027**	-0.039***	-0.043***	-0.049***
		(0.011)	(0.011)	(0.011)	(0.011)
Change Industry		-0.079***	-0.082***	-0.075***	-0.064***
		(0.009)	(0.009)	(0.009)	(0.010)
Change Occupation		-0.077***	-0.074***	-0.070***	-0.054***
		(0.009)	(0.009)	(0.009)	(0.010)
White			-0.032	-0.028	-0.014
			(0.025)	(0.025)	(0.025)
Black			-0.063**	-0.051*	-0.022
			(0.028)	(0.028)	(0.029)
Asian			-0.010	-0.001	0.006
			(0.033)	(0.033)	(0.033)
Hispanic			0.023*	0.025*	0.042***
			(0.014)	(0.014)	(0.010)
Female			0.008	0.026***	0.042***
			(0.008)	(0.009)	(0.010)
Married w/ spouse			0.003	-0.003	-0.010
			(0.008)	(0.008)	(0.008)
Sets of Dummies:					
Age and Tenure	No	Yes	Yes	Yes	Yes
Time Period	No	No	Yes	Yes	Yes
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
R-squared	0.008	0.180	0.199	0.214	0.306
Observations	16,250	16,250	16,250	16,250	16,250

Notes: Dependent variable is change in log real weekly earnings between displaced job and job at the time of the DWS survey. Sample restrictions are as follows: Individuals included are ages 16- 65, with the displaced job in the private sector. Individuals whose current earnings are imputed are omitted from the estimation sample. Observations with log weekly earnings changes outside -2.75 and +1.94 are omitted (approximately 4 s.d. of the mean log earnings change). Omitted race/ethnicity group is non-Hispanic mixed race or other. Education dummies account for 8 levels of education, Age for 5 groups, and Tenure for 5. Geography dummies account for 7 metro designations and for all states and D.C. Broad Occ/Ind dummies account for 11 occupation and 13 industry groups. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Summary Estimates of Union Wage Differentials for Displaced Workers, Alternative Samples and Dependent Variables

Specifications	(1)	(2)	(3)	(4)	(5)			
	ΔU	ΔU	ΔU	ΔU	ΔU	Obs.	Sample	Dep. Var.
Line 1	0.167	0.146	0.143	0.141	0.143	16,250	full sample	Δwk. earnings
Line 2	0.190	0.158	0.153	0.149	0.145	5,797	plant closings	Δwk. earnings
Line 3	0.165	0.150	0.149	0.146	0.150	7,401	hourly workers	Δhourly wage
Line 4	0.195	0.168	0.168	0.162	0.154	2,610	hourly workers, plant closings	Δhourly wage
Line 5	0.176	0.162	0.160	0.156	0.159	7,212	hourly workers	Δwk. earnings
Line 6	0.219	0.195	0.195	0.190	0.182	2,542	hourly workers, plant closings	Δwk. earnings

Included controls for specifications (1) through (5) are identical to those shown for the full sample in Table 2. Observations with log weekly earnings changes outside -2.75 and +1.94 are omitted. Observations with log hourly wage changes outside -2.0 and +2.0 are omitted. For both measures, trimmed values exceed the mean log earnings or log wage by approximately 4 s.d. or more. All ΔU coefficients shown above are statistically significant at the 0.01 level.

Table 4: Summary Estimates of Union Joiner (NU) and Union Leaver (UN) Wage Differentials for Displaced Workers, Alternative Samples and Dependent Variables

Specifications		(1)	(2)	(3)	(4)	Obs.	Sample	Dep. Var.
Line 1	NU	0.075***	0.085***	0.084***	0.079***	16,250	full sample	Δwk. earnings
	UN	-0.249***	-0.201***	-0.195***	-0.195***			
	UU	0.078***	0.056***	0.075***	0.054***			
Line 2	NU	0.058	0.076**	0.077**	0.073**	5,797	plant closings	Δwk. earnings
	UN	-0.277***	-0.211***	-0.202***	-0.199***			
	UU	0.059	0.074*	0.095**	0.083**			
Line 3	NU	0.098***	0.117***	0.115***	0.102***	7,401	hourly workers	Δhourly wage
	UN	-0.216***	-0.176***	-0.175***	-0.180***			
	UU	0.029	0.022	0.036*	0.004			
Line 4	NU	0.158***	0.172***	0.169***	0.155***	2,610	hourly workers, plant closings	Δhourly wage
	UN	-0.218***	-0.165***	-0.166***	-0.166***			
	UU	-0.027	0.012	0.027	0.004			
Line 5	NU	0.110***	0.116***	0.114***	0.101***	7,212	hourly workers	Δwk. earnings
	UN	-0.227***	-0.199***	-0.196***	-0.198***			
	UU	0.062**	0.021	0.033	0.007			
Line 6	NU	0.161***	0.174***	0.159***	0.144**	2,542	hourly workers, plant closings	Δwk. earnings
	UN	-0.254***	-0.208***	-0.217***	-0.219***			
	UU	0.029	0.013	0.020	-0.004			

The samples are identical to those shown in Tables 2 and 3. See the table note to Table 3. NU, UN, UU coefficients are designated as statistically significant as follows: *** at the 0.01 level, ** at the 0.05 level, and * at the 0.10 level.

.Table 5: DWS and CPS Union Earnings and Wage Gaps, 1994-2018

Survey Year	Displacement Years	Annual Union Gap	3-DWS Gap Averages	CPS Wage, Gap, All	CPS Wage, Gap, Private	CPS Wage, Gap, Public
1994	1991-1993	0.208	–	0.206	0.232	0.148
1995	–	–	–	0.198	0.227	0.134
1996	1993-1995	0.156	0.180	0.193	0.222	0.126
1997	–	–	–	0.191	0.214	0.134
1998	1995-1997	0.176	0.161	0.190	0.210	0.135
1999	–	–	–	0.184	0.206	0.137
2000	1997-1999	0.150	0.152	0.172	0.196	0.118
2001	–	–	–	0.170	0.193	0.125
2002	1999-2001	0.131	0.125	0.163	0.196	0.102
2003	–	–	–	0.169	0.198	0.111
2004	2001-2003	0.095	0.140	0.160	0.198	0.093
2005	–	–	–	0.160	0.192	0.111
2006	2003-2005	0.193	0.152	0.157	0.186	0.102
2007	–	–	–	0.158	0.193	0.095
2008	2005-2007	0.169	0.171	0.153	0.185	0.094
2009	–	–	–	0.144	0.181	0.082
2010	2007-2009	0.152	0.139	0.149	0.187	0.083
2011	–	–	–	0.150	0.183	0.095
2012	2009-2011	0.096	0.137	0.151	0.181	0.098
2013	–	–	–	0.150	0.186	0.084
2014	2011-2013	0.162	0.123	0.150	0.188	0.082
2015	–	–	–	0.138	0.172	0.070
2016	2013-2015	0.112	0.100	0.145	0.179	0.085
2017	–	–	–	0.143	0.185	0.085
2018	2015-2017	0.026	–	0.142	0.176	0.075
Means	1991-2017	0.140	0.144	0.163	0.195	0.104

Note: The DWS annual union earnings gaps are derived from a log weekly earnings regression highly similar to specification (5) in Table 2, the only difference being that the union membership measure ΔU is interacted with survey year dummies. The CPS union wage gaps are from Hirsch and Macpherson (2019, Table 2a, pp. 21-22), providing annual union wage gap estimates, beginning in 1973, from log wage regressions based on hourly earnings, with time-consistent controls for workers, geography, and job attributes (broad industry and occupation).

Table 6: Reemployment Rates of Displaced Workers, by Union Status

	Sample Size	Union Rate	Nonunion Rate	Difference
Full Sample:				
Any Reason	36,641	58.03	64.57	-6.54
				(.009)
Plant Closures Only	12,409	56.90	67.92	-11.02
				(.015)
Restricted Sample:				
Any Reason	26,958	59.41	67.23	-7.82
				(.010)
Plant Closures Only	8,806	56.49	69.36	-12.88
				(.017)

Note: Shown are reemployment rates (in percent) from private sector displacement within the past three years, compiled from the biennial Displaced Worker Surveys, 1994-2018. The "Restricted Sample" excludes those who did not report displacement job earnings or had other key information missing; this is the sample used in Table 7, columns 2-6. The full sample is used in Table 9, column 1. Differences shown in the far right column are statistically significant at the 0.01 level.

Table 7: Union Effects on Reemployment Following Displacement, Probit Marginal Effects

Variables	(1) Employed	(2) Employed	(3) Employed	(4) Employed	(5) Employed	(6) Employed
Union (displaced job)	-0.064***	-0.076***	-0.089***	-0.078***	-0.069***	-0.046***
	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)
Log wage (disp. job)			0.019***	0.038***	0.042***	0.036***
			(0.004)	(0.005)	(0.005)	(0.006)
Full time (disp. job)			0.021	0.007	0.000	0.006
			(0.015)	(0.015)	(0.015)	(0.015)
Black			-0.113***	-0.099***	-0.089***	-0.085***
			(0.010)	(0.009)	(0.010)	(0.010)
Asian			-0.039**	-0.052***	-0.035**	-0.024
			(0.016)	(0.016)	(0.017)	(0.017)
Other			-0.072***	-0.058***	-0.059***	-0.047***
			(0.017)	(0.016)	(0.016)	(0.016)
Hispanic			-0.045***	-0.010	0.005	0.015
			(0.009)	(0.009)	(0.010)	(0.010)
Female			-0.045***	-0.051***	-0.050***	-0.074***
			(0.006)	(0.006)	(0.006)	(0.007)
Sets of dummies						
Education	No	No	No	Yes	Yes	Yes
Age	No	No	No	Yes	Yes	Yes
Tenure	No	No	No	Yes	Yes	Yes
State	No	No	No	No	Yes	Yes
Time Period	No	No	Yes	Yes	Yes	Yes
Broad Occ/Ind	No	No	No	No	No	Yes
Observations	36,641	26,958	26,958	26,958	26,958	26,958
Log-Likelihood	-23911.944	-17155.793	-17004.616	-16188.436	-16056.29	-15375.321
Psuedo R-Squared	0.0011	0.0017	0.0105	0.0580	0.0657	0.1053

Notes: Shown are the probit marginal effects evaluated at the means. Omitted reference groups are non-Hispanic whites. The dummies account for 8 schooling, 5 age, and 5 tenure groups.

Column 1 includes the full sample of displaced workers reporting union status on the displaced job. Columns 2-6 have a "restricted" sample that excludes those who did not report displacement job earnings or other missing information for variables included in the regressions above. 5 age dummies, 5 tenure dummies, reference groups are white,

Data: 1994-2018 Biennial Displaced Worker Surveys matched to CPS Outgoing Rotation Group files, February-May 1994-2000, and January-April 2002-2018.

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 1a: Union and Nonunion Displacement, All

		Union			Nonunion		
Survey Year	Displacement Years	Displaced (1000s)	Employment (1000s)	Displacement Rate	Displaced (1000s)	Employment (1000s)	Displacement Rate
1994	1991-1993	1,197	9,751	12.3	8,623	75,834	11.4
1996	1993-1995	1,064	9,554	11.1	7,994	79,702	10.0
1998	1995-1997	659	9,403	7.0	6,931	84,535	8.2
2000	1997-1999	600	9,363	6.4	6,657	88,884	7.5
2002	1999-2001	918	9,236	9.9	9,035	91,902	9.8
2004	2001-2003	964	8,748	11.0	10,220	92,854	11.0
2006	2003-2005	694	8,304	8.4	7,229	95,609	7.6
2008	2005-2007	591	8,117	7.3	7,652	99,239	7.7
2010	2007-2009	1,215	7,937	15.3	14,320	98,778	14.5
2012	2009-2011	843	7,242	11.6	11,614	96,483	12.0
2014	2011-2013	587	7,182	8.2	8,617	99,729	8.6
2016	2013-2015	437	7,407	5.9	6,859	103,652	6.6
2018	2015-2017	412	7,530	5.5	6,292	107,676	5.8
Means	1991-2017	783	8,444	9.2	8,619	93,452	9.3

Appendix Table 1b: Union and Nonunion Displacement, Plant Closures Only

		Union			Nonunion		
Survey Year	Displacement Years	Displaced (1000s)	Employment (1000s)	Displacement Rate	Displaced (1000s)	Employment (1000s)	Displacement Rate
1994	1991-1993	458	9,751	4.7	3,328	75,834	4.4
1996	1993-1995	392	9,554	4.1	2,851	79,702	3.6
1998	1995-1997	265	9,403	2.8	2,840	84,535	3.4
2000	1997-1999	297	9,363	3.2	2,820	88,884	3.2
2002	1999-2001	370	9,236	4.0	3,388	91,902	3.7
2004	2001-2003	413	8,748	4.7	3,767	92,854	4.1
2006	2003-2005	299	8,304	3.6	3,113	95,609	3.3
2008	2005-2007	245	8,117	3.0	2,762	99,239	2.8
2010	2007-2009	294	7,937	3.7	3,771	98,778	3.8
2012	2009-2011	222	7,242	3.1	3,078	96,483	3.2
2014	2011-2013	140	7,182	1.9	2,540	99,729	2.5
2016	2013-2015	121	7,407	1.6	2,107	103,652	2.0
2018	2015-2017	97	7,530	1.3	1,938	107,676	1.8
Means	1991-2017	278	8,444	3.2	2,946	93,452	3.2

Source: Displacement levels are calculated from biennial CPS Displaced Worker Surveys (DWS), 1994-2018, using DWS survey weights. For each survey year, private sector union and nonunion employment are averaged over the three dislacement years, as calculated from the CPS ORGs and reported at Unionstats.com (Hirsch and Macpherson 2003, updated annually). Means for 1991-2017 are based on equal weighting of the DWS survey periods.

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

Survey Year	Displacement Years	DWS %Union All Displaced	DWS %Union Plant Closures	%Union Private Sector
1994	1991-93	12.2	12.1	11.4
1996	1993-95	11.7	12.1	10.7
1998	1995-97	8.7	8.5	10.0
2000	1997-99	8.3	9.5	9.5
2002	1999-01	9.2	9.8	9.1
2004	2001-03	8.6	9.9	8.6
2006	2003-05	8.8	8.8	8.0
2008	2005-07	7.2	8.1	7.6
2010	2007-09	7.8	7.2	7.4
2012	2009-11	6.8	6.7	7.0
2014	2011-13	6.4	5.2	6.7
2016	2013-15	6.0	5.4	6.7
2018	2015-17	6.7	5.1	6.4
Means	1991-2017	8.33	8.35	8.39

Source: Displacement measures for union and nonunion workers are calculated from the biennial CPS Displaced Worker Surveys (DWS), 1994-2018. Private sector union density is compiled from the CPS ORGs and reported at Unionstats.com (Hirsch and Macpherson 2003, updated annually) based on the three displacement years for each DWS survey. Figures 2a and 2b utilize these density rates.