Estimating the 'True' Cost of Job Loss: Evidence using Matched Data from California 1991-2000.[1]

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January 30, 2009

[1]Thanks to John Abowd, David Card, Ken Couch, Hank Farber, Dan Sullivan, and Lars Vilhuber for helpful conversations, and to seminar participants at the Federal Reserve Bank Chicago, University of California Berkeley, CAED 2006, University of Connecticut, and the New York Census Research Data Conference 2006 for helpful comments. We would also like to thank Rosemary Hyson, Arnie Reznek, and Lynn Riggs for helpful inputs, patience, and perseverance in the disclosure process. Anne Polivka provided helpful detailed comments on an earlier draft. Funding was generously provided by the Center for Labor Economics, University of California-Berkeley and the California Census Research Data Center (CCRDC) under the auspices of NSF grant SBR-9812173. Our thanks to the Bureau of Labor Statistics (BLS) for their permission to use the Displaced Worker Supplement (DWS) in this research. Also our thanks must go to the Labor Market Information Division (LMID) and the Employment Development Department (EDD) in California who authorized the use of the California Base Wage file in this work. In particular, Phil Hardiman and Richard Kihlthau were instrumental in obtaining permission and preparing the UI Base Wage file for use at Census. The research carried out in this project (Project number: CES 0429 Hildreth) was authorized under Census data protocols. The research and the views expressed herein are those of the authors and do not reflect the policies of the US Census Bureau, the Bureau of Labor Statistics (BLS), or the California LMID or EDD, or the views of other BLS staff members. While access to the confidential data is restricted, it is not exclusive. Interested parties can apply to use Census data through the RDC network: http://www.ces.census.gov/ces.php/home. All work reported here (both algorithms and results alluded to in the text) is archived as part of the CES Census data program.

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Abstract

Estimates of the cost of job displacement from survey and administrative data differ markedly. This paper uses a unique match of data between the Displaced Worker Survey (DWS) and administrative wage records from California to examine the sources of this discrepancy. When we use similar estimation methods and account for measurement error in survey wages correlated with worker demographics, estimates of earnings losses at displacement are similar from both datasets and significantly larger than those based on the DWS alone. Also correcting for measurement errors in reported displacements suggests both sources of such estimates may yield lower bounds for the true cost of displacement.

1 Introduction

The extent of worker displacement–separation from employment for 'involuntary' reasons–and the costs of their displacement are recurring subjects of interest to economists, policy makers, and those directly affected by displacement. A displacement is typically defined as an involuntary job loss resulting from the operating decisions of the employer.[1] Typically, the cost of job loss is the difference in wages between the job held at present and the job held previously for the displaced worker, vis-a-vis other workers who were not displaced and continue to hold the same job. However, the literature has generated a number of varying estimates of the frequency and cost of job loss based on different methods and data sources. The extent and 'true' cost of job loss remain unanswered questions.

One set of estimates, based on the Displaced Worker Supplement (DWS) to the Current Population Survey (CPS), suggests the cost of job loss is moderate and relatively short lived. The DWS is the mainstay of economic statistics on job loss in the US, and is the basis of the Bureau of Labor Statistics (BLS) official figures on this subject.[2] While these data provide a reasonably consistent series from 1984 to the present, they suffer from some well-known problems, such as recall bias in job displacement and past earnings, a lack of job history for surveyed workers, and no readily available comparison group for the earlier wages of those displaced.[3] Similarly, the DWS is likely to significantly undercount displacement (Farber 2007).

As an alternative to the DWS, beginning with Jacobson, Lalonde, and Sullivan (1993) an increasing number of researchers have attempted to use administrative data from Unemployment

[1] The Displaced Worker Supplement (DWS) interview instructions state that involuntary job loss occurs if the worker lost a job, or left a job, for the following reasons: plant closure, position or shift abolished, insufficient work (slack work), or similar reasons (other). Similar reasons are described as: "... all factors which are based on the operating decisions of the firm, plant, or business in which the worker was employed and which result in the worker losing or leaving a job." (http://www.bls.census.gov/cps/dispwkr/1996/sintrins.htm).

[2] For example, the latest edition using the Displaced Worker Supplement can be found at the following website: http://www.bls.gov/news.release/disp.nr0.htm. In his survey of the literature on displaced workers, Fallick (1996) writes on page six "To date, the only good source of estimates of the number of displaced workers in the United States is the Displaced Worker Surveys (DWS)–Supplement to the Current Population Survey (CPS)."

[3] For a discussion of these issues, see Topel (1990), Jacobson, Lalonde and Sullivan (1993), Stevens (1997), Farber (2003), Oyer (2003), and Esposito (2004).

Insurance Base Wage (UI-BW) files in estimating the cost of job loss.[4] Estimates based on UI-BW files typically imply much larger and more persistent effects of job loss on earnings. Overall, the incidence and cost of job loss estimated by the UI-BW can exceed those obtained from the DWS by a factor of two. The key advantage of administrative data is access to a near universe of workers over a longer time horizon, which allows for a control group of non-displaced workers and an examination of the dynamics of earnings before and after displacement for both displaced and non-displaced workers. However, such estimates are heavily specific to the place and time period of the data collected, and there is often little detail available describing the individuals and events surrounding job loss.[5] Therefore, the resulting analysis depends on assumptions regarding the type of job loss and the workers involved. While both the DWS and the UI-BW data provide valuable estimates of the costs of worker displacement in the US, without further information it cannot be said which of these disparate estimates is more appropriate.

Accurately measuring displacement and the cost of job loss have important micro and macroeconomic implications for the workings of the labor market. Displaced workers are a particularly valuable subject for microeconomic study because they ostensibly change jobs for reasons that are orthogonal to their unobserved ability. An examination of displaced workers' outcomes (especially their pay) can provide insights into the sorting and selection processes in the labor market.[6] It can also give important insights into key aspects of the wage-structure. For example, the cost of job

[4]As this paper concentrates on differences between using administrative files and the DWS, the comments are concentrated on these two data sets. However, other papers, most notably Ruhm (1991), Topel (1990), and Stevens (1997) have used long run household surveys such as the PSID (Panel Study of Income Dynamics) or the NLSY (National Longitudinal Survey of Youth). There are problems specific to using these data, most notably are attrition and sample selection effects.

[5]In a series of papers, Farber (1993, 1997, 2001, 2003) uses the Displaced Worker Supplement (DWS) to estimate the cost of job loss for the whole US from 1981 to 2001 (a total of 10 DWS Supplements). In contrast, Jacobson, Lalonde, and Sullivan (1993) use UI-BW data for Pennsylvania for 1974-1986; Schoeni and Dardia (2003), Couch (2006), and Kodrzycki (2007) use administrative data for the early 1990s for California, Connecticut, and Massachusetts, respectively.

[6]For example, Gibbons and Katz (1991) posit an asymmetric information signaling model where if firms have discretion regarding who they layoff, the market infers that these workers are of low ability, while workers laid off from a plant closing carry no such negative inference. Gibbons and Katz (1991) used the DWS to test the implications of their model and find evidence to support it. Alternatively, Gibbons and Katz (1992) use a matching model to show that if there are industry specific rents, endogenous job change decisions can create important self-selection biases even in first-differenced estimates of industry wage differentials. However, displaced workers from plant-closings are used to approximate exogenous job loss, and can be used to test the predictions of the matching model.

loss rises with firm or industry tenure (e.g., Kletzer 1989, Neal 1995), and this has been interpreted as implying potentially significant social costs of displacement through the loss of specific human capital (Topel 1990, Ruhm 1991, Fallick 1996, and Kletzer 1998).[7] Similarly, the fact that average industry, union, or firm wage differentials are typically lost at job loss suggests an important role of 'rents' in the labor market (e.g., Krueger and Summers 1984, von Wachter and Bender 2006).[8]

Correctly measuring the incidence and cost of job loss also have implications for the pattern of macroeconomic adjustments to exogenous shocks. An increasing literature has debated the role of job separations in cyclical unemployment dynamics (e.g., Shimer 2005, Elsby, Michaels, and Solon 2007). Similarly, the size of the cost of displacement can help explain the persistence of unemployment (termed 'hysteresis') following aggregate contractions of output (e.g., Murphy and Topel 1987). Accurately defining and identifying displacement and its associated costs can also help in formulating offsetting labor market policies and government programs. Such policies may include job re-training, advance notification, and limited income transfers.[9] However, in reviewing the literature on the success of these measures in alleviating the cost of job loss, Kletzer (1998) notes that there is no clear evidence from existing research that shows that these policy measures aid the plight of displaced workers.[10]

This paper examines the sources of differences between estimates based on the DWS and the UI-BW and reassesses the 'true' cost of job loss. To do so, it uses the same two data sources (DWS and UI administrative data files) as well as a unique match between them. Matching at the individual worker-level, we obtain an exact pairing of records in the DWS and the UI-BW for California over the course of the 1990s. This allows, for the first time, a direct comparison between

[7] Hamermesh (1987) was perhaps the first to try to estimate the social cost of displacement tied to the loss of firm specific human capital. If workers lose firm-specific human capital upon displacement, their wages at their new jobs should be the result of years of experience, education, and other non-firm characteristics.

[8] These patterns may also indicate a role for worker selection (e.g., Gibbons, Katz, Lemieux, Parent 2005). The data for this study does not contain sufficient industry or union information to pursue these ideas.

[9] In the US, this has taken the form of the Job Training Partnership Act (JTPA) or more recently the Worker Adjustment and Retraining Act (1989) (WARN).

[10] In the DWS interviewer instructions, the interviewers are urged to obtain accurate responses because "The data on displaced workers are used to determine the size and nature of the population affected by job displacement and, hence the need and scope of the Job Training Partnership Act (JTPA) programs." (http://www.bls.census.gov/cps/dispwkr/1996/sintrins.htm).

the DWS response, and the events as recorded in the UI-BW.

For the first part, this is a descriptive paper on the quality of the information about job loss that is contained within the DWS and UI administrative files. We begin by estimating the cost of job loss for California using the two data sources separately, using the same methods described in the literature (Farber 2003; Jacobson, Lalonde and Sullivan, 1993). After describing how the data sources were matched, we examine how successfully the disparate data sources record the same type of information concerning job loss. We also reconcile disparate estimates of the cost of job loss by reestimating it for both measures of displacement using the same method and earnings information for the same sample of workers. The second half of the paper concentrates on estimating the cost of job loss to workers, accounting for measurement error in both the dependent and independent variables. Last, we exploit the matched data to provide basic insights on the sources of discrepancies between the two data sets.

The results from this paper present a departure from the existing literature in several ways. First, we provide new estimates of the cost of job loss and reconcile existing estimates which differ by orders of magnitude. Second, this paper presents an examination of measurement error in the reported incidence of displacement. To our knowledge, this has not been undertaken before. Third, this is the first time that estimates have been reported on the degree of attenuation bias in current and past DWS wages using individual UI-BW earnings as a benchmark. Fourth, the paper provides first time estimates of the attenuation bias in existing estimates of the cost of job loss from measurement error in wages and the displacement variable.

To summarize our results, we show that estimates of the cost of job loss depend crucially on the use of a control group of non-displaced workers and a measure of earnings incorporating the effect of non-employment. Data sources not equipped to do so, such as the DWS, may severely underestimate the cost of job loss. We find that once we use a comparable methodology and comparable earnings information, the estimated cost of job loss in the two data sources is similar. The resulting estimates tend to be larger than what is typically found in the literature. However, we also find that both data sets mismeasure the incidence of job displacement. We confirm Farber's (2007) hypothesis that the DWS is likely to significantly undercount job losers, while the UI appears to overcount them. In addition, CPS wages appear to be mismeasured, especially when referring to wages before job loss. The measurement error is non-classical in that it is correlated with characteristics such as age, education, and past job tenure.

The measurement error in displacement we find suggests that even estimates based on the DWS or the UI-BW that include a control group and account for zero earnings represent a lower bound of the cost of job loss, with the 'true' cost of job loss likely being significantly higher. Estimates taking into account both sources of error yield substantially larger costs and substantially lower incidence of job loss. However, the lack of overlapping displacements in the two datasets raises the concern that they may not measure the same events. We conclude by discussing potential sources of the discrepancies we find and by summarizing implications for research practice.

2 The Cost of Job Loss in California Based on Survey and Administrative Data

Before examining the implications of estimating the cost of job loss using the matched file, we examine its component parts. We use the DWS to compare job loss and its costs in California with the experience for other workers in the US during the 1990s. We use the UI-BW (using the same method of estimation as Jacobson, Lalonde and Sullivan, 1993) to compare the California economy of the 1990s to Pennsylvania in the late 1970's and early 1980's.

2.1 The Displaced Worker Supplement (DWS) to the Current Population Survey (CPS)

The DWS was created in 1984 and was designed specifically to elicit responses on displacement through layoff (without recall), plant closing, or the employer going out of business. This is separate from a worker's wish to quit or leave a job for his or her own reasons. The starting point for the work here was data from the biannual DWS for 1994, 1996, 1998, and 2000 as supplements to the February CPS.[11] With the consistent three year retrospective job history, and near consistent

[11]The DWS was appended to the January monthly survey from 1984 until 1992, and in 2002 reverted back to the January CPS once again. Because of the monthly rotation design of the CPS, approximately three-quarters of February CPS respondents can be found in the March CPS, but only half of January respondents. The March CPS is important

wording on the job displacement question, the 1990s make a near ideal decade in which to estimate the average cost of job displacement and examine the reasons why displacement occurred. Broadly speaking, the decade runs from a depression (in 1991) to a boom and these data end with the February and March CPS (the first quarter of 2000) right before the end of the boom for the California economy.

In order to provide a means for comparison between the results presented in past work (chiefly Farber 1997, 2003), and the results presented later on in the paper, Table 1 presents a set of comparative results of the cost of job loss for the US and California. The sample selected was workers aged 20 to 64. The estimates include all transitions (whether to or from a part-time or full-time job). Two sets of numbers are presented in Table 1, a displacement rate and the unconditional wage change between pre and post displacement jobs. The figures for Table 1 were calculated in the same manner as Farber (1997). [12]

The results from Table 1 show that the displacement rate for California (CA) in the 1990s was between 7.2% and 12%. As we will see below, this is much lower than the displacement rate obtained from administrative data. The incidence of job loss in CA tended to be above the displacement rate for the US. Part of this was the 'fall-out' from the decline in the aerospace industry and other associated durable goods industries located in California (see Dardia and Schoeni, 2000).[13] The lower panel shows that the short- to medium-term cost of job loss as approximated by changes

for our work, as it was the only month of the CPS that asked for an individual's social security number, necessary for matching survey responses to individual wage records. Other changes that occurred with the 1994 DWS that make it a convenient starting point involved a truncation of the retrospective period over which job loss was examined (from 5 to 3 years), and a major change in the question wording concerning whether or not the individual was a displaced worker. However, in 1994, BLS narrowed the follow up questions to only the three main reasons for displacement (plant closed, slack work, or position abolished). This meant that the 'other' reason for job loss contained no further information. Farber (1997, 2003) discusses this in some detail. For these reasons, the analysis is limited to 1991 to 2000.

[12]The displacement rate is defined as the (weighted) number displaced divided by the number in the relevant February CPS. The unconditional log wage for an individual i for some defined time period t is defined as lnw_{it}, where $i = 1...N$ and $t = 1...T$, and the unconditional wage change is defined as: $\ln w_{it} = \ln w_{it} - \ln w_{i(t-j), j=1,2,3}$.

[13]By the mid to late 1990s, the displacement rate for California had receded close to the US figure, but California workers were still losing their jobs at a faster rate than elsewhere. Part of this was the effect of the 'high-tech' boom for California and the number of start-ups that did not manage to survive (see Campbell, 2004). California's displacement rate was maintained above the US chiefly by 'plant closing' (mainly at the beginning of the decade) and 'slack work'. Both are likely indicators of a dynamic economy where 'start-up' enterprises either failed to survive or over estimated the extent of the market for their product or service (Campbell, 2004).

in log wage ranged from 17.9% in the early 1990s recession to 2.7% in the height of the late 1990s expansion. The ball park of these estimates is an order of magnitude lower than typical wage losses obtained from administrative data. Californians also had lower average costs of job loss than the US, in the DWS.[14]

2.2 The California Unemployment Insurance Base Wage file (UI-BW)

The California UI-BW is essentially the same type of file as used by Jacobson, Lalonde and Sullivan (1993). However, the California Base Wage file does not include any individual worker details (such as age or gender), and it does not have the same history from which to identify long term job holders. To determine high-tenure workers, workers were required to hold a job for either the first six quarters or the first 16 quarters before displacement occurs. While this is not ideal, it is close to the approach from Schoeni and Dardia (2000) who use California administrative records for a similar time period. The reason for choosing the first quarter of 1993 as the separation quarter was chiefly to obtain some job loss from a recessionary period. While it is likely that the California economy was recovering faster than the rest of the US by 1993 (see Table 1), there are likely to be some jobs lost due to recessionary effects in some industries. Below, we also use the second quarter of 1995 as the start point to try and identify long-term job holders in an analogous fashion with Jacobson, Lalonde and Sullivan (1993).

The main problem with estimating the cost of job loss using administrative data is that the cause of job loss is unknown. Separations from the place of work might be voluntary or involuntary. To overcome the problem of identification to some degree, Jacobson, Lalonde and Sullivan (1993) suggest a 30 percent rule. A ‘mass layoff’ sample was constructed where a worker was declared part of a mass layoff if the separators’ firms’ employment in the year following their departure was 30 percent or more below the maximum level at the beginning of the time period of study. While some workers will have quit their jobs in ‘mass layoff’ or from distressed firms, the majority

[14] Most of the reductions in the cost of job loss for California, relative to the whole US, came from the larger fraction of workers losing their previous job by reason of ‘position abolished’. Workers displaced for this reason did not experience the same wage loss when moving to another job as other workers in the US.

would have been required to leave on an involuntary basis. Below, we report results for alternative definitions of ‘mass layoff’ at the firm level, including an examination of ‘plant closing’. Plant closing has a direct analogue with the DWS definitions for displacement. Plant closing in the UI-BW was defined as the SEIN (State Employer Identification Number) ceasing to exist (and not returning).

Further sample restrictions we impose were also similar to Jacobson, Lalonde, and Sullivan (1993) and Schoeni and Dardia (2000). First, firms with less than 50 employees in the first quarter were removed. Given the interest of this paper, it would make little sense for small employers to be included where a change of only a few employees might be miscoded as a ‘mass layoff.’ Second, an individual had to have at least one quarter of work per year after the initial displacement (in 1993 quarter 1 or 1995 quarter 2).[15] Third, an individual had to have continuous employment up to the first quarter of 1993 or the second quarter of 1995. In other words, an individual had to have at least 6 or 16 quarters of continuous employment before the first separation. Fourth, for multiple job holders, we only concentrated on the primary (highest paying) job. Finally, we drew a 5 percent random sample for all individuals left in the UI-BW at the start of the sample period to make the computations tractable.[16] For every worker as part of the 5 percent sample we retained all information on the firms at which the worker was employed. Due to the basic nature of the UI-BW file, this consisted of employer size each quarter and an indicator of whether the employer exited at any point

Drawing on the program evaluation literature, Jacobson, Lalonde and Sullivan (1993) develop an applied framework in which current earnings are a function of dummy variables indicating the displacement period, the past periods of earnings, and the future periods of earnings. The basic

[15]The focus on workers with continuing attachment to the CA labor force is necessary since we do not observe earnings outside the CA economy. Otherwise, we may wrongly assign zero or missing earnings to workers having found employment in another state. See Jacobson, Lalonde, and Sullivan (1993) for a discussion of this point.

[16]This sample is different from Schoeni and Dardia (2000) whose sample restrictions are all workers employed in SIC’s 366, 372, 376, 381, 382 (aerospace sectors) and a 20 percent random sample of all individuals working in the non-aerospace durable goods sector.

model for estimation using the Jacobson, Lalonde and Sullivan (1993) framework is:

$$w_{it} = \alpha_i + \gamma_t + \sum_{k \geq -m} D_{it}^k \delta_k + \varepsilon_{it}$$

where $i = 1, \ldots, n$ and $t = 1, \ldots, s$. The dependent variable (w_{it}) is the quarterly real earnings for an individual. The γ_t are quarter time dummies. The α_i are a worker specific fixed effect that captures the impact of permanent differences in workers in their observed and unobserved characteristics. The error term (ε_{it}) is assumed to have constant variance and to be uncorrelated across individuals and time. The dummy variables $D_{it}^k, k = -m, -(m-1), \ldots, 0, 1, 2, \ldots,$ jointly represent the event of displacement and time periods before and after displacement. Thus, the model will not only indicate the earnings change at the time of displacement, but will also show the effect of displacement k time periods before and after displacement. [17]

Figure 1 shows that using the same estimating equation, and using the workers with only 6 quarters of pre-displacement tenure, the California UI administrative file generates a ‘cost of job loss’ that is smaller in size, but similar in duration to the Jacobson, Lalonde, and Sullivan (1993) [JLS] and Schoeni and Dardia (2000) results. Workers experience an initial earnings loss at job loss of about 15-20 percent, and the loss is still evident four years after the event. As suggested at the outset, these estimates imply percentage changes that are considerably larger than those suggested by the DWS in Table 1.

Table 2 summarizes estimates for the rate of displacement and the cost of job loss obtained from the UI-BW file. For purposes of comparison with the DWS, we estimate the short term wage loss occurring one year before and after layoff, as well as the long term pre/post wage loss excluding six quarters before and after layoff. The measures of wage losses in Table 1 (averaging over 1 to 3 years since job loss) will lie somewhere in between these two measures (in Tables 6

[17] Other characteristics commonly included, such as age or gender, are not available in the CA UI-BW file. To identify the parameters of the model, we have to exclude a set of layoff-period interactions. We exclude all dummies for 16 quarters before layoff or earlier; i.e., we set δ_k to zero for $k < -16$. The analysis is limited to 20 quarters before and after layoffs to keep a balance of workers displaced in different years in our sample. The worker specific time trend (included in the model by Jacobson, Lalonde and Sullivan 1993) was omitted here as the number of quarters of data available was limited to 35.

and 7, we estimate the exact same cost of job loss for both data sets). The displacement rate was defined as simply the number displaced under the ‘chosen rule’, divided by the population at risk (the number in employment) in the same year. The measure is approximately the same as that used for the DWS definition of Table 1. Table 2 shows the displacement rate and the overall wage loss at layoff based on the JLS definition (row 1), as well as four other alternative definitions of job loss from a ‘distressed employer’ possible in the administrative data and further discussed below.

The results in Table 2 have important implications for a comparison with the estimates of the DWS shown in Table 1. When comparing to Table 1, in all cases the implied displacement rates and the estimated costs of job loss are higher in the UI-BW than in the DWS (shown for comparison purposes as a percentage of the mean earnings of displaced workers at baseline in the bottom of Table 2). The displacement rates are about twice as high in the administrative data as in the survey data for all definitions. The implied percent earnings loss at job loss relative to mean initial earnings (shown in column 10) of JLS’s preferred measure (columns 7-9) are about twice as large in the short run, and are only similar in the long-run.

However, a closer examination of Table 2 shows that methodological differences will affect comparisons between the typical estimates based on the two data sources. First, an often noted problem with the DWS is that it has no control group. A comparison between columns 1-3 and columns 4-6 of Table 2 shows that introducing a control group of workers who were not displaced during the period in question is quite important. A substantial portion of the earnings loss would be missed if the regular evolution of earnings absent a job loss was not explicitly considered. Second, the DWS does not distinguish between short and medium term effects of job losses, but instead represents an average of the first three years after job loss. While, Table 2 shows that short run earnings losses are much higher than losses in the longer run (columns 7-9), long-run losses are still substantial. Third, DWS estimates are based on year to year wage changes after job loss rather than comparing to an initial baseline. This means the results are affected by any pre-displacement dips in earnings, receipts of severance pay, or temporary layoffs. Last, another important difference is that the DWS estimates are based on weekly wages rather than quarterly earnings. This ignores the effect of non-employment both in the short run (the survey week) as well as the longer run (the quarter) that instead are captured in the UI earnings definition typically used. Moreover, limiting the analysis to positive wages introduces a bias from selective labor market participation.

All of these concerns will be addressed explicitly in our analysis of a sample of workers for whom we have information from both DWS and UI data. As a preliminary step, consider the short term effect of job loss according to the JLS definition without a control group in the UI data, a loss of -11.76%. If we set zero earnings to missing and only work with positive earnings (as is done in Table 1 for the DWS), we get an effect of -2% (see the lower panel of Appendix Table 1). Thus, changing the methodology and earnings definitions by itself is an important step in making the results more comparable. However, this also confirms that the absence of a control group, the ignorance of dynamics, and the deletion of zero earnings may substantially bias the results in the DWS in favor of smaller costs of job loss.

One measure that should be clearly comparable in the UI-BW and the DWS is 'plant-closing' (although the definition of an employer is not exactly synonymous between the data sets).[18] The final row in Table 2 shows that this is not the case. The plant-closing displacement rate calculated using the UI-BW data is almost twice the displacement rate from the DWS. The cost of job loss at plant closing from the UI-BW data is substantially higher than the DWS if we allow for a control group and worker fixed effects. Only if we simplify our estimates by looking at short term effects based only on laid-off workers and exclude zeros, do we find a higher degree of overlap.[19]

[18] In the UI-BW, we measure an employer as a SEIN; an account number for employers paying their UI. As Abowd and Vilhuber (2004) discuss, a SEIN is not necessarily an establishment at a specific address. For example, all McDonald's in the state of California are filed under one SEIN. In the DWS, individual respondents are supposed to define for themselves what constitutes a plant closure. Interviewer instructions define a 'plant-closing' only as: "Plant closed or moved. The place of business where the employee reported to work is no longer operating. The employer may have moved the place of business away or may have shut down the local operation permanently or temporarily. Includes those persons that are offered relocation with an employer that moves, but turn down the offer." (http://www.bls.census.gov/cps/dispwkr/1996/sintrins.htm).

[19] Other work, for example that by Topel (1990) notes that the DWS may substantially underestimate the amount of worker displacement and incorrectly estimate the timing. Using the DWS from 1984 and 1986, where respondents were asked about a 5 year retrospective job history, Topel (1990) found that the surveys for the years where the years overlap (1981-83) found vastly different estimates of the amount of displacement. In fact, the 1986 DWS only recorded 48 percent of the displacement recorded in the 1984 DWS. Topel (1990) postured that this was due to respondents 'telescoping'; meaning that the respondents assign data that are closer to the time of the survey. Recalling 'layoffs' was far more prone to error than 'plant-closing' (which was recalled more accurately). Interestingly, Akerlof and Yellen (1985) posture the opposite point. If recall bias can affect inferences about the costs and consequences of

These results suggest that simple pre-post differences based on the DWS as presented in Table 1 are likely to miss an important part of the story by ignoring dynamics, ignoring counterfactual earnings developments, and excluding zero earnings. Below, we will also see that mismeasurement of survey wages further distorts these estimates. At prima facie, estimates based on the administrative data appear to be more reliable and preferable estimates. Yet, these estimates have problems as well. On the one hand, as will be discussed in the next sections, they tend to overstate job loss and thereby understate the cost of job displacement. On the other hand, the estimates in Figure 1 and Table 2, row 1 are based on necessarily arbitrary assumptions on who is called displaced. To make sure these estimates are a good benchmark against which to evaluate the quality of information in the DWS, we examine the effect of different specification choices in detail. The results of this analysis – partly shown in Figures 2 and 3 and explained more fully in the Appendix and in our longer working paper – imply that estimated costs of job loss using the UI-BW based on the specifications chosen by JLS and replicated here (and in several other papers) is reasonably robust to variation in the parameters defining displacement. We will thus continue to work with the JLS definition of 'distressed employer'. To maximize sample sizes, we will keep the six quarter tenure restriction.

Overall, it appears that methodological differences in the estimates of the cost of job loss help to bridge an important part of the differences between the two data sets observed in Table 1 and Table 2. In the next two sections of this paper, we will more closely examine the overlap between measures of the incidence and cost of job loss between the two data sets. This will show that the problem is more complex; there may be important differences in what is being measured with each type of data.

3 Comparing the Incidence of Displacement using Matched Data

To reconcile the disparate estimates from the DWS and the UI-BW, we created a unique match between the two data sources for California from 1990 to 2000. This match will allow us to

displacement, and that only costly events are recalled, the estimates of the costs of displacement based on retrospective data will overstate the impact of job loss on the typical worker.

reestimate the cost of job loss based on the same individuals, the same earnings information, and the same methodology. It will also allow us to assess problems in the measurement of displacement and earnings in the two data sources, and to suggest strategies to deal with these problems.

3.1 Creating the Matched File

The DWS/UI-BW matched file was created in two steps. First, we link the February CPS files, which contain the DWS, to the March CPS files, because March was the only month in which individuals were asked to provide their SSNs. Links can be made between the February and March CPS files because of the outgoing rotation group design of the CPS. Approximately three-quarters of the February CPS respondents appear in the March CPS. [20] Second, we link the matched March-DWS data from respondents in CA was matched to the UI-BW using a cross-walk between person identifiers in the CPS and Social Security Number-based identifiers in the UI-BW. The match accuracy varied slightly across years, but was within the bounds expected for this type of analysis.

Table 3 provides descriptive statistics for various stages of the matched sample. Column 1 reports the results of matching the February CPS/DWS to the March CPS and the UI-BW. There is little difference in observable characteristics between the full DWS sample from the February CPS and the three-way matched sample, with the exception of the fraction of more highly educated, perhaps because these are more likely to move (and thus are missed). The fraction displaced, both for all reasons and by specific reasons is similar.

Columns 2 to 4 in Table 3 provide descriptive statistics for the displaced workers within the matched sample. Column 3 provides descriptive statistics for the subset of displaced individuals who provided measures of wages for both current and past employment in the DWS. Column 4 provides the descriptive statistics for the subset of these who reported displacement due to plant closing in their DWS response. There is little difference in observable characteristics between the different subsamples.

For displaced workers, Table 3 also reports corresponding log quarterly earnings from the UI-

[20] Individuals in these files can be matched using person identifiers in 1996, 1998, and 2000 and probabilistic matching in 1994 (see the Data Appendix for more details).

BW if available (where for comparison purposes we have rescaled the DWS weekly earnings to represent quarterly earnings). As discussed in the next section, in many circumstances we did not find a corresponding job loss in the UI-BW, and so the sample sizes in these entries are lower.[21] Comparing the wage measures, we fine that first, current DWS earnings tend to be higher than UI-BW earnings. This is partly because non-employment is likely to affect quarterly UI-BW earnings, especially for displaced workers. However, as discussed in Kornfeld and Bloom (1999), average UI-BW earnings tend to be lower than average earnings from survey data or IRS tax records as well. Second, earnings on the lost job are higher in the UI-BW than in the DWS once we impose restrictions on job tenure or firm size. We will return to these differences in Sections 4 and 5.

3.2 The Incidence of Job Loss in the DWS and the UI-BW File

Section 2 showed important differences in the incidence of job loss in the two data sources. There are many potential sources for this discrepancy, beginning with how displacement is measured in either data source. Part of the problem with examining the incidence and cost of job loss between the two datasets is that they do not share a definition of a 'job'. A job in the DWS is defined as a position at an establishment. If the establishment (plant) closed, or if there is downsizing, then the 'job' no longer exists. However, ultimately what is recorded as a displacement is left to the judgment of the interviewee and interviewer.[22]

By comparison, in the UI-BW file, researchers define a 'job' as the pairing of a worker with a state employer identification number (SEIN). Each time an individual within the UI-BW changes employers, there is a change recorded in the SEIN for that individual. Nothing is known about why an individual changed jobs, and thus researchers deem workers to have been displaced if the

[21] In keeping with the interviewer instructions from the DWS, for the small number of cases where there was more than one job separation in the UI-BW for the corresponding DWS separation, we close the job in the UI-BW that had the longest tenure as the previous 'main' job. In the event of job spells of equal length within the UI-BW (say 2 quarters), the job with the higher wage was designated as the previous 'main' job.

[22] Part of the problem with the DWS has been the definition of the workers' 'last main job'. In the instructions to the DWS interviewers are clearly instructed that this should mean the 'job that was held the longest' (http://www.bls.census.gov/cps/dispwkr/1996/sintrins.htm). Subsequent questions then investigate whether the worker lost their previous job for reasons that were involuntary, and when the displacement occurred. Once again, the interviewer instructions are clear that the year of displacement should refer to the 'last main job'.

SEIN lost 30 percent of its employment in the year following the Workers exit (from the maximum number employed by that firm over the time period of study).[23] As Jacobson et al. (1993) [JLS] argue, this approach will encompass some workers who quit their jobs (before mass layoffs, or would have been discharged for some cause), but the majority should have separated for economic reasons.

Table 4 displays the degree of overlap in the incidence of job loss in the two data sources for alternative measures of job loss. The first columns of the table show the same displacement rates calculated in Tables 1 for the DWS and in Table 2 for the UI-BW file, calculated here for the sample of workers in the matched data. This table shows that our preferred definition of displacement for the UI-BW file (definition 8) produces a displacement rate almost twice the rate computed from the DWS. For individuals in both datasets, UI-BW methods yield a displacement rate of 14.3 percent, while DWS methods yield a displacement rate of 8.4 percent.[24] While some small fraction of the total difference could be attributable to random coding error, we conclude that Jacobson, Lalonde and Sullivan (1993) style methods produce a number displaced that is an order of magnitude larger than those found in the DWS.[25]

To address the issue of comparability between the two definitions of displacement , other rows of Table 4 show the displacement rate for alternative definitions of job loss in the UI-BW. The displacement rate is even higher with the alternative, less restrictive definitions of job loss in the administrative data; e.g., it is 20% if we do not impose a restriction on firm size (definition 6). The restriction on whether the employer size declines is clearly important (rows 4 to 8) when comparing displacement rates. Similarly, mobility in the UI-BW is even larger if we do not impose a restriction on job tenure. It is a well-known fact that most new jobs end early, most likely for voluntary reasons (such as job shopping). This may have been especially true in the vibrant

[23]There are also other restrictions discussed in Section 2.2 above.

[24]The displacement rates shown here differ from the ones recorded in Tables 1 and 2. This is largely the result of using the matched file for these calculations. We checked to see if the use of the matched file produced any bias in terms of calculating the displacement rates. While the displacement rates are lower overall, they maintain an order of magnitude difference between the DWS and UI-BW estimates.

[25]The definition in the table differs slightly from that used in JLS and in Table 2 because to maximize sample sizes we do not exclude recalls and we do not impose that workers have some positive UI-BW earnings after job loss.

California economy in the mid to late 1990s.

At face value it could be argued that the UI-BW definition does not succinctly define involuntary job loss, and that a high number of voluntary exits are also recorded as workers who were displaced. Alternatively, the evidence is also consistent with the notion that individuals as part of the DWS are not responding accurately an involuntary separation from their last main job. Without any further information, it is difficult to investigate why individuals may be misreporting in this manner. One way to address this question is to consider plant closings (rows 9-12 in Table 4). Among the three stated reasons for job loss in the DWS, plant closing should be the most comparable definition of job loss with respect to the UI-BW.[26] However, even if we impose restrictions on firm size and prior job tenure (definition 12 in Table 4), we get still double the rate of job displacement measured for the same individuals.

Table 4 also displays various measures of the degree of overlap in reported displacements between the two samples. The fractions displayed in columns 3 to 6 of Table 4 show that while the number of individuals who report no displacement in the UI-BW file but displacement in the DWS (UI-BW=0,DWS=1), are always a very small fraction of the total; recording displacement in the UI-BW but not in the DWS shows a large fraction. In fact, across nearly all categories the individuals displaced in the UI-BW, but not in the DWS (UI-BW=1,DWS=0), show a higher percentage than where both measures agree (UI-BW=1,DWS=1). While the error rate indicated by the 'off-diagonal' element (UI-BW=0,DWS=1) could easily be the product of miscoding within the UI-BW (especially at the beginning or end of a DWS three year time window), the error rate indicated by the other 'off-diagonal' element is too large to be a random event.

Table 4 also presents the conditional probabilities of reporting displacement from the perspective of the UI-BW file and the DWS (columns 7 to 10). As a result of the higher displacement rate for the UI-BW file, the conditional probabilities between the UI-BW and DWS files show marked

[26] In a strict sense of the definition, 'plant-closing' reported in the DWS should correspond to the closure (and removal of the SEIN) of an employer on the administrative UI-BW file. This will not be true for SEINs with multiple establishments. If the DWS records plant closings of multi-establishment firms these, these will not show up as plant closings in the UI-BW. This would lead the DWS to overstate the incidence of plant closings with respect to the UI-BW, something we do not find in the data.

differences. If the DWS were the true measure, and we impose a tenure restriction and a plant event to exclude voluntary separators in the administrative data, then the latter covers between 22.5% and 33.1% of job losses (definitions 6 and 8).[27] If, on the other hand, job loss as measured in the UI-BW were true, the DWS would fare much worse, covering about 12-14% of events (and even less in the case of plant closings).

The discussion of Table 4 establishes some recurring "themes" of the paper that add to the results of the general comparison in Tables 1 and 2. First, there is not much overlap in the incidence of job loss between the two surveys even for the same group of individuals. Conditional on having a job loss in either data source, only about ten percent have a job loss in both (for definitions 6 and 8). Second, the DWS tends to undercount job separation during clearly identifiable events in the administrative data such as a mass-layoff or a plant closing. Third, it appears the UI-BW overstates the rate of job loss. Fourth, how we define job loss in the administrative data does matter for displacement rates. Imposing restrictions on job tenure and employer events lead to displacement rates that are more sensible and closer to what is reported in the DWS.

3.3 Mis-Classification by Demographic Characteristics

Potential reasons for the discrepancy in the incidence of job loss in the two data sources, such as problems with workers recall, suggest that the differences may vary by worker characteristics. Appendix Table 2 replicates Table 4 by worker characteristics for the measure of job loss suggested by JLS [definition 8]. Conditional probabilities differed for age and education groups, but not for gender and race. Those in the younger age group were far more likely to make errors than those in the other two age groups.[28] Similarly, we see that the higher educated and the non-white have lower coverage rates in the DWS relative to the UI-BW. We obtain similar patterns with plant closings (not shown).

[27] The coverage rate is much higher without tenure restriction, but that does not have as much informative value since almost two thirds of the sample have at least one job separation in the ten year period we consider.

[28] This holds across both types of conditional probabilities: $P(DWS=0|UI-BW=1)$ and $P(UI-BW=0|DWS=1)$. The latter is largely mechanical, due to the tenure restriction in the administrative data. However, the former is not, suggesting age may be a dimension on which reporting of job loss differs.

The results so far suggest that the UI-BW may overstate the degree of job displacement. On the other hand, the measure proposed by Jacobson, Lalonde, and Sullivan (1993) may be too restrictive. If so, the conditional probabilities in Table 4 may understate the degree of overlap between the DWS and the UI-BW. We tried to replicate Appendix Table 2 allowing for a maximal overlap between the two data sets, while trying to remove 'false' displacement from the UI-BW file. This is shown in Appendix Table 3. In the resulting sample, there is a much higher degree of overall overlap, although it is still small in absolute terms. There is a similar difference in overlap across demographic groups, with the young and the middle educated standing out. We will further assess the use of such a sample of "maximal" overlap when correcting for measurement error when estimating the cost of job loss (Section 5).

4 Basic Estimates of Wage Losses at Job Loss in the Matched File

Section 2 showed that there are important differences in the estimates of earnings losses between the two data sets. In this section, we will examine the extent to which these differences remain when we use the same sample, same methodology, and comparable earnings measures. The discrepancies in measuring job displacement examined in Section 3 may also influence estimation of the cost of job loss. Thus, we will also compare differences in earnings losses for the sub-sets of the matched file. We begin with simple wage changes (Section 4.1). Then we add a control group (Section 4.2), something not possible with the unmatched DWS. In Section 5, we explicitly address the effect of measurement error in wages (Section 5.1) and job displacement (Section 5.2) on estimates of the cost of job loss.

4.1 Unconditional Wage Changes

The unconditional wage difference for the subset of individuals who are part of the matched file (corresponding to columns 4, 5, and 6 in Table 4) are shown in Table 5. The definition of the average change in earnings is the same as in Table 1. Note that for workers with a job loss in both the DWS and the UI-BW, there are two possible measures of wage change. Similarly, for workers reporting displacement in the DWS, there are two possible earnings measures (independent of whether there is a job loss in the UI-BW). For space reasons, the table only shows results for our preferred measure of job displacement in the UI-BW file.[29]

If one compares the average change in log wages (Panel A) in the overall DWS (-7.3%) with that of the UI-BW for our preferred definition (-6.6%), the implied cost of job loss does not differ very much. Thus, if we analyze the same group of workers with the same method of estimating the cost of job loss, the costs of job loss in the DWS and UI-BW do not appear dissimilar, consistent with our discussion of results in Table 1, Table 2, and Appendix Table 1.

However, if we consider workers who have a job loss in both data sources (UI-BW=DWS=1) the pattern is markedly different. For these workers, based on the DWS we find an earnings loss of -12.5%, whereas based on the UI-BW, we find an earnings loss of -51.7%. The discrepancy derives from the nature of the wage and earnings information in the two surveys. The DWS records the weekly wage (for the week before the survey week and weekly wages on the lost job); the UI-BW earnings refer to earnings in a given quarter of the year. Thus, there are cases when the weekly wage in the DWS is zero (and thus drops out in the table) and positive but small for the UI-BW (e.g., for workers with multiple spells of non-employment within a quarter). This only affects the post-displacement wage, since pre-displacement earnings are greater than zero in both data sets.[30]

If we use the same source of earnings information from the UI-BW available in the matched sample to measure the cost of job loss in the two surveys, the estimated cost of job loss for all displaced workers in the DWS is -20.6% (row 1), an order of magnitude larger than -7.3% obtained by the survey data. It is also larger than the -6.6% difference in log earnings obtained from the JLS estimate (row 2, column 1). Similarly, for the case of job loss in both data sets, the difference due to different sources of earnings information is -41.8% (row 2, column 6) vs. -12.5% (row2, column 5, from survey wages). The former, based on UI-BW earnings, is much closer to the estimate of

[29] Appendix Tables 4, 5 and 6 replicate the numbers in Table 5 for other displacement measures.

[30] In addition, due to non-employment the implied quarterly earnings from the DWS are larger than that of the UI-BW (since without information on weeks worked in a quarter, we multiply weekly wages by the number of weeks), especially for displaced workers when incidence of non-employment is high. This further increases the difference between the two measures.

-51.7% for job loss in the UI-BW according to the JLS measure (row 2, column 7). Thus, the earnings measure chosen (weekly vs. quarterly, survey vs. administrative) can make an important difference for assessing the cost of job loss.

In the discussion of Table 2, the concern was raised that the exclusion of zero wages or earnings may distort estimates of the cost of job loss. Thus, in Panel B of Table 5 we replicate the results in Panel A for wage levels including zeros. Including zero wages makes a bigger difference for the DWS, since for job losers there is a higher incidence of non-employment in weekly than in quarterly data. Panel B shows that earnings losses in the DWS (scaled to represent quarterly earnings) are significantly more negative than in the UI-BW when measured by survey earnings (column 1, row 1). When using UI-BW earnings for all job losers instead, job losses in either the DWS or the UI-BW produce much more similar numbers (-780.16 vs. -688.77 dollars for DWS (row 1, column 5) and in the UI-BW (row 2, column 1), respectively). Similarly, for workers with a job loss in both surveys, the discrepancy in survey and administrative earnings in levels is less drastic compared to that in logs (row 2, columns 5 and 6).

Another important source of discrepancies is that in some cases earnings in the administrative data are higher than positive earnings in the DWS, especially for highly educated or older workers. There appears to be misreporting of earnings for these groups in the DWS relative to the UI-BW, and the degree of misreporting differs between the pre- and the post-displacement wage. If we look at medians of earnings losses to gauge the role of high pre-displacement earnings levels in the UI-BW (Table 5, columns 8 and 9), we find that the alternative wage measures tend to agree in overall magnitude.[31] Thus, the DWS tends to understate large earnings losses, possibly because it understates large pre-job loss earnings. This is taken up in Section 4.2 and Section 5.1.

[31]Table 5 shows the median earnings loss implied by administrative earnings. For all job losers in the DWS, we find a wage loss of -12.4% (row 1, column 8), which is still smaller but closer to the number based on survey earnings (-7.3%, row 1, column 1). Similarly, for our preferred measure of job loss we find a wage loss of -12.6% based on UI-BW earnings for workers with a job loss in both samples (row 2, column 9, instead of an average of -6.6% in column 1). (Note that the estimate using the earnings from the UI-BW in column 8 for the same definition of job loss is -15.7%; the discrepancy arises because the latter estimate uses information on the timing of job loss from the DWS instead of the UI-BW.) This is much smaller than the mean shown in row 2, columns 6 and 7 (-41.8% and -51.7%, respectively), and much closer to the estimate of -12.5% obtained from the survey wages for the same group of workers (column 5).

To summarize, when we use administrative earnings throughout, the cost of job loss as measured in the DWS (-20.6%) actually tends to be larger than that of workers identified as job losers in the UI-BW (-6.6%), at least for the JLS definition we focus on. If we include zero earnings in Panel B, the cost is similar (-$780.2 in the DWS vs. -$688.7 in the UI-BW). The cost of job loss is very large and similar for workers displaced in both samples (-40% to -50%, with a median of about -13%), a conclusion unaffected by the inclusion of zero earnings.

However, discrepancies between the two data sources remain for workers with a displacement recorded in only one of the data sources. Those labeled as displaced in the UI-BW but not in the DWS (UI-BW=1,DWS=0), have small average earnings losses for all definitions of job loss we consider. This is not surprising given that the results of Table 4 (and Appendix Table 2) suggested that the UI-BW is likely to overstate the degree of job loss. As discussed above, some of these workers may be leaving their employer voluntarily and benefit from the job change. Alternatively, they may be leaving before the actual mass-layoff takes place and have less of an earnings loss as a result. While they could conceivably count as displaced, they may not report themselves that way in the DWS. On the other hand, those labeled displaced in the DWS but not in the UI-BW (UI-BW=0,DWS=1) tend to have significant earnings losses, especially when using UI-BW data. These losses are an order of magnitude smaller than for workers recorded with job loss in both data sets, something we return to in the last section.

Overall, the results in Table 5 suggest that (a) even without a control group the cost of job loss in the DWS can be substantial once we use UI earnings or incorporate zero wages; (b) there is substantial overlap in the estimated cost of job loss between the two data sources once we take into account the nature of the earnings information and use a similar estimation method; (c) job loss in the UI-BW is likely to include some workers who are not truly displaced, and thus on average may *understate* the cost of job loss. On the other hand, the DWS may overstate the cost of job loss if workers who leave a distressed employer early do not consider themselves displaced. Last, (d) it appears that reported wage changes in the DWS are measured with error; this error appears non-classical in that it affects large earnings losses.

4.2 Difference-in-Difference Estimates Controlling for Characteristics

Section 2 suggested that a control group of workers who were not displaced and can be used as a counterfactual is critical for estimating the cost of job loss. The absence of a control group has also been a typical criticism of the DWS (e.g., Farber 2003). In the matched sample, we can introduce such a control group using past earnings information from non-displaced workers obtained from the UI-BW. This is shown in the bottom panels of Table 5. The Table shows the unconditional and conditional difference-in-difference estimates for wages in logs and levels and for different definitions of mass-layoffs.[32] For the DWS, we also show estimates using both survey and administrative information on earnings. Given small sample sizes, the table only shows results for workers who had a job loss in either the DWS or the UI-BW.

As noticed in Table 2 for administrative data, the inclusion of a control group makes a big difference for estimating the cost of job loss in the DWS. If we use the survey wage for job losers and the administrative wage for workers without a job loss in the DWS (for whom no wage from a previous period is available in the CPS), the estimate of the cost of job loss is -25.9%, about three times larger than the corresponding simple mean difference in Panel A (-7.3%). If the same administrative earnings information is used for displaced workers and non-displaced workers we obtain -39.2%, significantly larger than the comparable estimate without a control group (-20.6%).[33]

Including a control group substantially matters for estimating the cost of job loss in the administrative data as well (-28.9% vs. -6.6%). The results in wage levels (Panel D) also confirm the importance of a control group (compare these to Panel B that has no control group). As in the case for simple pre/post differences, the estimated cost of job loss captured in the DWS is again larger than in the UI-BW once we include a control group.

[32] The control group are all workers who report no displacement in the DWS. Similarly, for all other definitions of job loss, we treat as a control group those without a job loss according to the definition in question. The initial earnings in the UI-BW data for workers without job loss is chosen to reflect distribution of the timing of job loss in the DWS (or the respective definition from the UI-BW). So if 40% (30%,30%) of workers report being displaced one (two,three) years before the survey date, we choose initial earnings for stayers to maintain that same distribution of years before the survey date.

[33] The impact of a control group is larger than results in Farber (2003) using a control group based on CPS data from merged outgoing rotation groups (e.g., Figure 10); this is likely because average wage growth was higher in California during the 1990s than the U.S. average. In part it may also be due to differences in the nature of the data.

The results in Table 5 emphasize that once we properly account for zero earnings and a control group, the cost of job loss is substantial and much larger than would be predicted by the DWS alone. These results provide direct evidence on the importance of a control group in the DWS. Compared to a control group, the inclusion of pre-job loss characteristics makes very little difference.[34] The evidence also suggests that the UI-BW may also understate the cost of job loss. The results in the table confirm that once we use a comparable estimation methodology and correct for differences in earnings concepts, the estimated cost of job loss in the two data sources are again similar.

5 Accounting for Measurement Error using the Matched File

5.1 Measurement Error in Wages

The previous section suggested that wage losses estimated using survey wages from the DWS might be understated relative to losses obtained using administrative data from the UI-BW file. This may be due to misreporting of current earnings in the March CPS, or due to recall problems affecting reported wages at the lost job in the DWS. Our finding in Table 5 that large wage changes appear to be understated suggests that some groups of workers who may be more at risk to experience larger wage losses – such as older or highly educated workers – may understate their current or past wages in the CPS. Since we have individual information on both current and past earnings from survey and administrative data, we can directly estimate the contribution of such non-classical, correlated measurement error.

However, while it could be argued that since the UI-BW records are employer reported they should be the 'true' measure of the wage received by an individual in a job, there is evidence to suggest that this is not the case (Kornfeld and Bloom 1999).[35] For the work here, any systematic

[34]This is shown in the last four columns of Appendix Table 5. The controls are two dummies for age and education, a gender, race, and union dummy, and three survey year dummies.

[35]Kornfeld and Bloom (1999) describe how the UI earnings (the UI-BW file) might not be a 'true' measure of earnings for an individual. In particular, they found that when comparing survey earnings data with the UI earnings data, that the survey earnings were higher than the UI. In addition to the missed earnings from the incorrect recording of the social security number, there is reason to believe that employers may actively underreport earnings. For example, employers may fail to report, or under report, earnings of individuals in short-term or low wage jobs. Employers

bias in UI-BW earnings will affect the level difference between UI-BW and DWI wages. But it will only affect the estimates of the difference in earnings between jobs or by worker characteristics if firms' underreporting changed systematically over time or between worker groups; for example, if firms tend to underreport more in a depression than they do in a boom. We are unable to test this with our data. In what follows, we assume that the measurement error in the UI-BW earnings is constant over time, does not depend on worker characteristics such as age and education, and is proportional to the true wage. While this may not hold exactly, there is likely to be less error in the administrative data than in the CPS survey data. In particular, there is no reason to believe that the degree of error in the UI-BW should be correlated with the degree of recall bias in the survey data.[36]

Given the information we have on wages from both data sets (DWS and UI-BW), we can estimate the extent of measurement error in self-reported wages, and the correlation with observable characteristics of the survey respondents (the attenuation bias). To do this, we postulate a basic model for *log* wages with 3 components:

$$\begin{aligned} \ln w^* &= xb + v && (1) \\ \ln w_{dws} &= lnw^* + \varepsilon_1 && (2) \\ \ln w_{ui-bw} &= \rho + lnw^* + \varepsilon_2 && (3) \end{aligned}$$

where lnw^* is the log of a true measure of wages that is unknown, $\ln w_{dws}$ is the log of the wage measure from the DWS; $\ln w_{ui-bw}$ is the log of wage measure from the UI-BW file; x is a vector of regressors (including a constant); v and ε_2 are iid; and ε_1 is correlated with the regressors x

may also fail to accurately report earnings in order to avoid paying unemployment insurance taxes, perhaps avoid later payment of unemployment insurance benefits, or perhaps in collusion with the employee to conceal earnings. Kornfeld and Bloom (1999) use a special set of tabulated means on employer tax returns to the IRS, matched to the same cell means constructed from the UI earnings file. Employer tax returns to the IRS are liable to be reported with a greater degree of accuracy as worker earnings are a business expense that can be deducted, rather than the potential to under report to the state UI agency because the amount is used to assess a payroll tax. Figures produced by Kornfeld and Bloom (1999) show that quarterly earnings reported in the IRS returns are on average about 20 percent higher than the quarterly earnings reported in the UI file.

[36] This is a common assumption in the literature, e.g., see Bound and Krueger (1991).

through:

$$\varepsilon_1 = \gamma x + \xi \tag{4}$$

with ξ iid.[37] The constant ρ in equation (3) will be a function of the average proportional bias in the UI-BW (estimated by Kornfeld and Bloom (1999) to be between 0.7 to 0.9).

We use this model for two purposes. First, we can obtain estimates of the variances of the error terms in equations (1)-(3). These indicate the overall degree of variation present in the data not captured by the observable characteristics, and may contain measurement error or other random wage determinants such as bonuses or specific qualifications. To do so, we estimate the following model:

$$\ln w_{dws} = \beta \ln w_{ui-bw} + \phi x + u \tag{5}$$

where we assume that for the DWS measure of wages, there may exist some correlation between the reported measure of wages and the observable characteristics of the survey respondents as stated in (4). For regression equation (5), the probability limit for $\widehat{\beta}$ is the following:

$$p\lim \widehat{\beta} = \frac{\sigma_v^2}{\sigma_v^2 + \sigma_{\varepsilon_2}^2}$$

Reversing the model (equation 5), so that the wage from the UI-BW ($\ln w_{ui-bw}$) becomes the dependent variable:

$$\ln w_{ui-bw} = \theta \ln w_{dws} + \delta x + u \tag{6}$$

and under the assumption that the error in equation (2) is correlated ($\varepsilon_1 = \gamma x + \xi$), the probability limits become:

$$p\lim \widehat{\theta} = \frac{\sigma_v^2}{\sigma_v^2 + \sigma_\xi^2}$$

Thus, the parameter estimates θ and β contain information on the degree of measurement error in the two data sources.[38] Although the variance elements could be identified separately, we limit

[37] The framework used here is similar in nature to work by Bound and Krueger (1991), Bound, Brown, Duncan, and Rodgers (1994), Lee and Sepanski, (1995), and Bound, Brown, and Mathiowetz, (2001).

[38] For the case where we would assume that UI-BW represents the true wage measure, the probability limit of β is

ourselves to interpreting these summary coefficients for the sake of brevity.[39]

Second, we are interested in the degree of correlation of the measurement error in current and past survey wages with worker characteristics captured by γ (the attenuation bias or loading factor) and the vector of 'true' parameters b. The most direct way to identify these parameters is to substitute equation (1) into equations (2) and (3) and estimate the resulting wage equations:

$$\ln w_{ui-bw} = xb + v + \varepsilon_2 \tag{7}$$

$$\ln w_{dws} = x(b+\gamma) + v + \xi \tag{8}$$

The parameters b and γ can be obtained directly from estimates of these equations.[40]

While simple, the above model nonetheless provides an estimate of the attenuation bias for variables included in wage equations using DWS (or CPS) reported wages and an outside wage source reported by employers (the UI-BW). As such, it is possible to see the extent of bias, and the statistical significance of that bias, for variables that have been used in the past in previous work, for example previous job tenure (Kletzer, 1989), or prior union membership (Kuhn and Sweetman, 1999). The results from implementing the above system are provided in Table 6 for all displaced workers in the DWS. The model numbers at the top of each column correspond to the equation numbers above.

The first aspect of the results in Table 6 (Panel A) is that wages are measured with error in both the CPS/DWS and UI-BW file, and this error is higher for wages at the lost job. In fact, the estimate θ of 0.535 (0.469) on the current (past) UI-BW suggests that measurement error in wages is pervasive and larger than in the DWS (as indicated by the estimates for β, 0.838 and 0.615 for

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equal to one. Similarly, the probability limit for θ is one under the assumption that the DWS wage is not measured with error.

[39] Denote by $\sigma^2_{w_{ui-bw}}$ the variance of the fitted residuals from the regression of equation (7); and likewise, $\sigma^2_{w_{DWS}}$ from equation (8). The variance elements from the above framework can be identified as: $\sigma^2_v = \beta\sigma^2_{w_{ui-bw}}$, $\sigma^2_{\varepsilon_2} = \sigma^2_{w_{ui-bw}} - \sigma^2_v$, and $\sigma^2_\xi = \sigma^2_{w_{DWS}} - \sigma^2_v$.

[40] It might also be noted that the regression equation (7) will provide an estimate of the sum of the variances: $\sigma^2_v + \sigma^2_{\varepsilon_2}$. With knowledge of b from equation (7) regression equation (8) will yield an estimate of γ as well as the sum of the variances: $\sigma^2_v + \sigma^2_\xi$.

current and past wages respectively). These coefficients would be equal to 1 without measurement error, and their magnitude depends on the degree of iid measurement error in earnings in the two data sources ($\sigma^2_{\varepsilon_2}$ and σ^2_{ξ}, see footnote 37). This result can again be explained by outliers in the UI-BW data. If we replicate the table excluding the top and bottom 5% of observations (shown in Appendix Table 4), the estimates (and implied error variances) are similar in the two surveys.[41]

Second, most of the coefficients on the variables are consistent with past results (columns 5-8). For example, wages are increasing in age and education, are lower for women, are increasing in tenure or previous job tenure, and decreasing in the number of jobs held between 'main' past and current jobs.[42] The last columns show a simple regression of changes in the DWS wage on worker characteristics. We find that workers that were older and unionized on their lost job had larger earnings losses, while higher educated workers have lower earnings losses.

Estimates of the correlated attenuation bias (γ) are displayed in Table 6, Panel B (columns 6-8).[43] The general pattern is that older individuals tend to understate their measure of wages, i.e., $\gamma < 0$ (especially the middle age group for the wage on the previous job); similarly, highly educated individuals tend to understate both current and previous wages. In both cases, the understatement is weaker than that occuring for wages from the current job. Likewise, previously unionized workers tend to underreport current wages. Finally, there is some evidence to suggest that workers with longer tenure on their prior job report wages overstate past wages. Factors such as gender, race, or the number of jobs held between main employment spells had little influence on the potential misreporting of wages.

Column 3 shows that these biases lead to significant understatement of wage losses for middle

[41] Note that this implies the overall measurement error for DWS earnings, $\sigma^2_{\varepsilon_1}$, is likely to be higher in the DWS, since it is augmented by the degree of correlation in measurement error with worker characteristics.

[42] The fact that the coefficients on other characteristics in columns 1-4 are non-zero confirms the presence of measurement error. In the case where we would assume that ε_1 is not correlated with any element in x and is iid and when $\sigma^2_{\varepsilon_2} = 0$, the regression equation (5) will yield the following coefficients with probability limit for ϕ: $p\lim\widehat{\phi} = \gamma + b\sigma^2_{\varepsilon_2}/(\sigma^2_{v} + \sigma^2_{\varepsilon_2}) = 0$. Similarly, if we were to assume that the DWS has no measurement error ($\sigma^2_{\varepsilon_1} = 0$) we obtain $p\lim\widehat{\delta} = (b\sigma^2_{\xi} - \gamma\sigma^2_{v})/(\sigma^2_{v} + \sigma^2_{\xi}) = b\sigma^2_{\varepsilon_1}/(\sigma^2_{v} + \sigma^2_{\varepsilon_1}) = 0$.

[43] To obtain standard errors of γ, we have stacked and fully interacted equations (7) and (8) into a system of seemingly unrelated regressions, clustering standard errors at the individual level. The variance of γ then results from the variance matrix of the system.

aged workers and unionized workers (losses are less negative, hence the coefficient is positive). Similarly, losses are overstated for unionized job losers and workers with high tenure on the lost job (for 5 extra years of job tenure, the job loss will be overstated by nine percent). The bias of wage levels cancels out for the wage difference in the case of highly educated workers. We had seem that outliers in the UI-BW may be important. The last three columns of the table shows the same results when we exclude outliers in the UI-BW file. This effectively removes the portion of measurement error correlated with education, reduces the role of age slightly, and leaves the other coefficients (especially for previous job tenure) unaffected. This confirms that the highly educated may underreport high earnings in the CPS.

Overall, the results suggest that measurement error in CPS wages – both current and past wages – appears correlated with worker characteristics; this error loads onto the estimated coefficients on typical regressors in a standard model for wage levels or wage changes. The coefficient estimates for γ in Table 6 allow researchers to subtract-out the effect from this measurement error. In terms of the results in the literature where use has been made of the DWS, the attenuation bias coefficients indicate that some results might be biased into recording either an over or under estimate of the cost of job loss. Estimates by age or education groups such as those shown in Column 4 of Table 6 (Panel B) are likely to be suspect (Farber 2003). However, it also appears that the influence of past tenure on the cost of job loss has been under-estimated in the past, which may have implications for the social cost of job loss (if past tenure represents lost human capital; Kletzer, 1989; Topel, 1990). Overall, even though the administrative data is not itself without problems, in the following we will use earnings from the UI-BW, and concentrate on the effect of measurement error in job displacement on estimates of the cost of job loss.[44]

[44]It is worth noting that these results are not necessarily inconsistent with those of Bound and Krueger (1991) who show that measurement error in annual earnings from the March Current Population Survey does not appear to be correlated with basic demographic characteristics (Table 3). Part of the difference may be due to different data sources. Bound and Krueger use employer provided information on annual earnings obtained from the Social Security Administration which is top coded; the UI-BW data we use is not top coded, though *on average* it appears to understate earnings vis-a-vis IRS earnings records (Kornfeld and Bloom 1999). Top coding may well explain the difference in the results, since lower mean earnings do not preclude the underreporting of large wages by certain demographic groups. However, it should be born in mind that our results for current wages are not very precise; the one variable coming in highly statistically significant in column 6 of Table 6, Part B is union status, which was not analyzed by Bound and Krueger (1991). Bound and Krueger did not analyze measurement error in past earnings.

5.2 Estimating the 'True' Cost of Job Loss

The previous section suggests that neither the DWS nor the UI-BW provide accurate measures of displacement. It is well known that estimates of earnings losses using noisy measures of displacement will underestimate the true degree of job loss (e.g., Freeman 1988). Thus, estimates in Table 5 provide a lower bound on the cost of job loss. Here we can exploit the fact that we have multiple potential noisy measures of job displacement to obtain tighter bounds using methods suggested by Black, Berger and Scott (1999).

The basic model of interest for differences in log wages is

$$\Delta \ln w_i = \alpha + \lambda D_i^* + x_i \varphi + \eta_i \tag{9}$$

where D_i^* is the 'true' measure of displacement for an individual, x_i is a set of covariates for the individual, and η_i is an iid error. $\Delta \ln w_i$ is the difference in log wages between the present and prior job. The measure for wages was taken from the UI-BW file.

We assume that D_i^* is not observed. Instead, we observe D_i^{UI} and D_i^{DWS}, two imperfect measures of the 'true' indicator for displacement from the UI-BW and DWS, respectively. Consider the case in which the measurement error of these variables is uncorrelated with observable characteristics and with each other, conditional on the "truth."[45] Black, Berger, and Scott (1999) [BBS] show that given two available measures, one can improve on this bound in at least two ways. First, in a regression of wage changes on dummies for the three groups of workers who have at least one job loss reported in either data source (i.e., for workers with either $(D_i^{UI} = 1, D_i^{DWS} = 0)$, $(D_i^{UI} = 0, D_i^{DWS} = 1)$, or $(D_i^{UI} = 1, D_i^{DWS} = 1)$), the coefficient on the dummy for $(D_i^{UI} = 1, D_i^{DWS} = 1)$ provides a tighter lower bound.

Second, BBS show that given two available measures, one can obtain a consistent estimate of

[45] We also considered extensions where the displacement indicator is dependent on some element of x (the independent variables). Results from Table 5 showed that the probability of correctly reporting displacement varied with age. Kane, Rouse, and Staiger (1999) provide for the case of additional covariates that are uncorrelated with the measurement error. Their framework could be extended to account for correlated measurement error. While implementing such a framework would be preferable, in this instance, for the matched data, there are insufficient observations, especially within 'cells' of observable characteristics, to obtain estimates with any precision.

the true underlying cost of job loss λ, as well as measures of the misclassification probabilities

$$\pi_{10}^{j} = Pr(D^{j} = 1|D^{*} = 0) \qquad \pi_{01}^{j} = Pr(D^{j} = 0|D^{*} = 1) \qquad j \in \{UI, DWS\}$$

and the true underlying displacement rate $\pi = Pr(D^{*} = 1)$. The parameters we are interested in are seven: $\alpha, \lambda, \pi_{10}^{UI-BW}, \pi_{10}^{DWS}, \pi_{01}^{UI-BW}, \pi_{01}^{DWS}$, and π. The moments available are also seven; three conditional probabilities and four conditional means that are a function of the underlying misclassification probabilities and remaining parameters. Thus, the model is just identified and can be estimated using method of moments. To see how this works, it is straightforward to show that

$$Pr(D^{UI} = 1 \& D^{DWS} = 0) = (1 - \pi_{01}^{UI})(1 - \pi_{01}^{DWS})\pi + \pi_{10}^{UI}\pi_{10}^{DWS}(1 - \pi)$$

and

$$E(\Delta w|D^{UI} = 1 \& D^{DWS} = 0) = \alpha + \lambda \frac{(1 - \pi_{01}^{UI})(1 - \pi_{01}^{DWS})\pi}{Pr(D^{UI} = 1 \& D^{DWS} = 0)}$$

and similarly for the remaining conditional probabilities and expectations.

Empirical counterparts of the moments are available in our data. The joint probabilities are shown in Table 4. To implement the model, we work with a slightly modified definition of wage changes relative to those shown in Table 5. We need to assign each worker a comparable measure of wage loss (valid irrespective of whether he is displaced or not observed in either the UI-BW or the DWS). Thus, we work with a fixed three-year difference in earnings for the treatment and control groups. The model is implemented for several measures of displacement in the UI-BW. We show estimates for a direct match of these displacements with the DWS as in Table 4; to maximize overlap of displacement between the data sources, we also show estimates where we allow any job change in the UI-BW to count as a job loss if there is a corresponding displacement recorded in the DWS (see Appendix Table 3 and in Appendix A). The results are shown in Table 7.[46]

The first column in Table 7 shows the coefficient on a dummy for ($D_i^{UI} = 1, D_i^{DWS} = 1$) in a regression of changes in log wage also including dummies for the cases in which only one of the two surveys indicates a job loss, as described above. Compared to the corresponding OLS estimates for the measures of job loss in the UI-BW from Table 5, the estimates in column 1 of Table 7 indicate a considerable increase in the estimated cost of job loss. For example, for our preferred measure (definition 8), we find that the estimated cost of job loss is now -51.7% in log points (a percent change of -40.4%, compared to an implied percentage change of -27.5% from column 2, row 8 of Table 5). A similar pattern holds for the other measures of job loss displayed as well. This result is a first indication that the OLS estimates of the cost of job loss are attenuated by misclassification bias in the incidence of job loss. Judging from this tighter bound on the estimates the rate of underestimation can be substantial, from 25% to 35%.

Columns 3 and 4 of Table 7 show the results of implementing the method-of-moments estimates of the true cost of job loss. The estimate for the true cost of job loss λ (the percentage change in wage changes resulting from the log difference is also displayed in the table) clearly show that the actual attenuation bias due to misclassification of the incidence of job loss is even larger than what is suggested by column 1 (the lower bound). For example, for measures of job loss 6 and 8, the implied percentage changes shown in column 4 of Table 7 are roughly double relative to what is suggested by the OLS estimates in Table 5.[47]

The results clearly indicate a substantial effect of measurement error in the job loss dummy on estimates of the cost of job loss, whether it is measured by the UI-BW or the DWS. A conservative researcher should consider adjusting estimated wage losses downward by a factor of up to 1.5 to 2 in the case of mass-layoff in the UI-BW or general displacement in the DWS. Interestingly, in the case of plant closing, comparing estimates of the "true" cost of job loss with the OLS estimates,

[46] In the estimation procedure, we constrain all probabilities to lie in the unit interval. Changing the definition of wage changes to that implied by the timing of job loss of the UI-BW measures does not make a difference. Standard errors based on the delta-method will be added in the next draft, but are not currently available due to the modality of our access to the restricted data.

[47] The table also displays a reasonable estimate of the average rate of wage growth in the absence of job loss α.

attenuation bias appears to be significantly smaller, on the order of 25%. Thus, based on these estimates there is less concern with estimates of the cost of job loss during plant closing. These results suggest that the higher effect of job loss at plant closings on earnings found in Table 2 may in fact be due in part to attenuation bias in measures of displacement based on mass layoffs.

Table 7 also displays the estimated misclassification probabilities for the two measures of displacement. The results confirm the discussion of Table 4. It was a recurring theme of the paper that the DWS underreports job loss, and this clearly stands out in the table (column 6). Conversely, it appears that the maximization algorithm tends to set the misclassification probability $Pr(D^{UI} = 0|D^* = 0)$ to zero. While this may be partly an artifact of the estimation procedure, it is certainly consistent with the UI casting a wide enough "net" to capture most true involuntary job changes (column 5).[48] The model also confirms the concern that the administrative data overstates true job displacement more than the DWS (column 7 vs. column 8). Examination of the error probabilities also explain why the error is smaller for plant closing, at least in the administrative data. The UI-BW shows considerably less overstatement of true job loss due to plant closing (column 7). On the other hand, the DWS appears to miss an even higher fraction of plant closing (column 6), confirming the impression in e.g., Table 4 and Appendix Table 10, that plant closure is not an event that workers recall with more accuracy.

Last, column 9 of Table 7 shows the implied true displacement rate. This is an order of magnitude smaller than that reported in the UI-BW, and smaller than even the displacement rate in the DWS. This result (as are the other results in the table) may be in part driven by the low degree of overlap between the two data sets. Yet, none of the results in Table 7 are affected by allowing for a more liberal overlap in displacement between the two data sources (shown in the bottom panel). Given that the absolute degree of overlap is still low (Appendix Table 3), this may not be too surprising.

Overall, if one is willing to believe that the two data sources yield noisy measures of the same underlying events, the following picture emerges: There are a small fraction of workers who are

[48] Changing initial values of the maximization algorithm or trying alternative ways of constraining the parameter space did not affect this result.

truly displaced, and these workers bear very high costs of job displacement. In this scenario, neither of the data sets is able to capture this pattern. The UI-BW includes workers as job losers who appear not to be truly displaced, and thus overestimates the incidence and underestimates the cost of job loss. The DWS misses a substantial fraction of workers who are displaced.

Based on these findings, to minimize the error of the “first type” (ignored displacement), one would prefer the UI-BW data to the DWS. This may be useful since the results suggest that using the administrative data, one will obtain an upper bound for the wage loss estimate (a lower bound in absolute terms). This is important, since administrative data also allows one to obtain a control group of workers who were not displaced, and enables a better handling of zero earnings. On the other hand, although the DWS misses some displacement, due to its lower error of “second type” (wrongly recorded displacement) it leads to less underestimation of the true costs of job loss – when a control group and a more broad earnings concept are available. However, conventional estimates based on survey wages lead to even larger underestimation of the true cost of job loss than in the UI-BW.

If one does not accept the assumption that the two data sources measure the same underlying event (a conclusion that cannot be rejected based on the low degree of overlap between the two data sources found in Table 4) then an important advantage of the administrative data is that a researcher can credibly claim to analyze the effects of a clearly defined event (say, “presence at firm at plant closing”). Based on the results of this paper, since job loss as reported in the DWS does not reflect events at the firm level we can observe in the UI-BW (even clearly identifiable events such as plant closing) one cannot say with the same confidence what event is reported as “displacement” in the DWS data.

5.3 Summary of Comparison of Job Characteristics

Using the matched data, we can provide additional evidence on potential sources of the discrepancy between the two data sets and of mismeasurement of job displacement. We have access to information on job characteristics and career histories, and can assess whether these differ for workers with a displacement in both, or for those displaced only in one but not the other data set. Here, we summarize the salient findings of a more detailed descriptive analysis in our longer working paper (HVH) and in the appendix. Despite a rich amount of data, given limited sample sizes, this analysis should be seen as indicative for future work.

We find that job loss in the DWS may partly capture workers leaving part-time jobs (and remaining in part-time jobs after job loss), which may not be typically viewed as a "job displacement" according to the main definition using the DWS. It also appears that the DWS may underreport displacements from large employers. Similarly, the DWS appears to miss an important degree of job mobility occuring between the displacement and new employment. We also confirm that recall of displacement in the DWS is affected by telescoping – i.e., individuals shift events forward in time relative to their actual date of occurence. These findings confirm that the DWS faces important measurement problems. There are also concerns with the UI-BW, which appears to overstate job mobility. However, we show a researcher aware of these problems can mitigate some of the concerns by a judicious use of restrictions. For example, job mobility can be made more comparable to that in survey dada by restrictions on pre-displacement job tenure.

6 Summary and Conclusions

We have used an unusual match of administrative earnings information from the California unemployment insurance base wage (UI-BW) file to the Displaced Worker Supplement (DWS) of the Current Population Survey (CPS) to reconcile seemingly starkly different estimates of the cost of job loss. Our estimates confirm that earnings losses after job displacement are large and significantly underestimated by conventional estimates based on the DWS. Estimating the cost of job loss based on the same sample, the same earnings measures, and the same methodology, we find that the DWS underestimates the cost of job loss because of a lack of a control group of non-displaced workers; because zero earnings are ignored; and because of non-classical measurement error in survey earnings.

Though closer to the truth, estimates based on the UI-BW file alone are not without problems.

In particular, the UI-BW overcounts job displacement and may understate the cost of job loss. Some of this excess job displacement is not captured by the DWS, which may in turn understate the incidence of job loss, particularly when less severe. As a result, estimates based on displacement in the DWS using a control group, administrative earnings, and incorporating zero earnings tend to show larger estimates of the cost of job loss than estimates based on the UI-BW. Larger than usual estimates are confirmed when we explicitly incorporate the fact that we have two noisy measures of job displacement into our estimates.

We have also documented that there is a lack of overlap in the measures of displacement in the two data sets, and provided some evidence as to the sources of discrepancy. A majority of workers counted as displaced in the UI-BW but not in the DWS come from large employers and experience very little earnings losses. Those workers counted as displaced in the DWS but not in the UI-BW are more likely to be displaced from part-time jobs, to continue to work in part-time jobs, and to have smaller earnings losses. There is also evidence of recall problems in the DWS, even for more clearly defined events such as plant closings. We also found that there is significant reporting error in current and past DWS wages, and that this is correlated with worker characteristics such as age, education, or job tenure.

The results reported here have implications for the future use of the DWS in both academic work, and as an indicator of the health of the labor market and the formulation of policy. Care should be taken in using both the displacement numbers and the associated cost of job loss from the DWS. Our estimates provide indications of the magnitude and direction of potential bias from measurement error in earnings and displacement. Using larger samples of displaced workers, future research should provide more detailed correction factors, taking into account that measurement error in wages and displacement are correlated with worker characteristics.

Given its increased availability, administrative data from unemployment insurance records are likely to be an important source of information for future studies on the costs of job loss. Administrative data are particularly desirable for the study of job displacement, since they allow inclusion of a control group, allow for the study of earnings dynamics before and after job loss, and are less affected by measurement error in earnings. Although our findings suggest that use of administrative data is not without pitfalls, one of the advantages of administrative data is the possibility of assessing the role of different specification choices. The paper has begun evaluating alternative definitions of distressed employer and the role of restriction of pre-job tenure or the timing of job loss. One of the important avenues for future research will be to continue to evaluate how alternative specifications determine which workers are drawn into the pool of job losers, which type of firms are involved, and how this affects estimates of the cost of job loss.

To conclude, we would like to highlight those results that have immediate useful practical implications for future research analyzing the effects of job displacement. First, on the methodological side we confirm that use of a control group and incorporation of zero earnings is crucial to avoid sever underestimation of the cost of job loss. Second, on the measurement side, we find that survey wages, both past and present, are measured with errors that systematically vary by demographic group, and provide estimates of correction factors. Third, we implement approaches correcting the effects of misclassification with two noisy indicators for job loss. These clearly indicate that even using a control group and accounting for zero earnings the estimates from both data sources represent at best lower bounds. Researchers should be aware that the true cost of job loss may be considerably larger than estimates that do not correct for measurement error in displacement and earnings.

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Table 1: A Comparison between the US and California for Displacement Rates and Unconditional Wage Changes, Displaced Worker Supplement of Current Population Survey 1994, 1996. 1998,

Displacement Rates: US

DWS	*Years Covered*	*Plant Closing*	*Slack Work*	*Position Abolished*	*Total*	*Number Displaced*
1994	1991-1993	0.032	0.035	0.020	0.086	6455
1996	1993-1995	0.030	0.037	0.022	0.089	6219
1998	1995-1997	0.028	0.025	0.018	0.071	5446
2000	1997-1999	0.027	0.023	0.016	0.065	5530
Total	1991-1999	0.030	0.031	0.019	0.080	

Displacement Rates: California

DWS	*Years Covered*	*Plant Closing*	*Slack Work*	*Position Abolished*	*Total*	*Number Displaced*
1994	1991-1993	0.041	0.053	0.021	0.115	499
1996	1993-1995	0.038	0.054	0.023	0.116	634
1998	1995-1997	0.032	0.038	0.019	0.088	570
2000	1997-1999	0.027	0.031	0.013	0.071	536
Total	1991-1999	0.034	0.043	0.019	0.100	

Wage Change: US

DWS	*Years Covered*	*Plant Closing*	*Slack Work*	*Position Abolished*	*Total*	*Number Displaced*
1994	1991-1993	-0.174	-0.0136	-0.214	-0.170	2069
		(0.023)	(0.023)	(0.032)		
1996	1993-1995	-0.0139	-0.043	-0.204	-0.120	2215
		(0.020)	(0.022)	(0.027)		
1998	1995-1997	-0.073	-0.023	-0.146	-0.063	2062
		(0.024)	(0.024)	(0.025)		
2000	1997-1999	-0.059	-0.007	-0.142	-0.059	1878
		(0.022)	(0.025)	(0.028)		
Total	1991-1999	-0.109	-0.038	-0.177	-0.102	
		(0.011)	(0.012)	(0.014)		

Wage Change: California

DWS	*Years Covered*	*Plant Closing*	*Slack Work*	*Position Abolished*	*Total*	*Number Displaced*
1994	1991-1993	-0.156	-0.188	-0.201	-0.179	199
		(0.074)	(0.056)	(0.131)		
1996	1993-1995	-0.139	-0.043	-0.062	-0.046	200
		(0.061)	(0.068)	(0.095)		
1998	1995-1997	-0.044	-0.023	-0.091	-0.027	216
		(0.053)	(0.069)	(0.075)		
2000	1997-1999	-0.088	-0.017	-0.092	-0.050	184
		(0.091)	(0.096)	(0.073)		
Total	1991-1999	-0.104	-0.026	-0.105	-0.073	
		(0.036)	(0.036)	(0.047)		

Notes: All numbers are based on publicly available data from the Displaced Worker Supplent (DWS) to the Current Population Survey (CPS). Definition of samples as in Farber (1997) and described in text. Figures for Wage Change are in 1982-1984 dollars. All figures are weighted (CPS weights). The "number displaced" refers to all displaced workers in the first two panels, and to displaced workers with valid observation on wage changes in the last two panels. Standard errors of the mean are in parentheses.

Table 2: Estimates of Displacement Rate and Earnings Losses at Job Displacement Using Alternative Definitions of Displacement, Unemployment Insurance Base Wage (UI-BW) File California, 1991-2000 (Workers with at Least Six Quarters of Job Tenure in 1991.3, Displaced at Firms with at Least 50 Employees in 1991.3 During 1991.4-1999.3)

Definition of Mass-Layoff (Distressed) Employers Using the UI-BW File	Displacement Rate	Only Displaced Workers, Before and After, No Covariates			Displaced and Non-Displaced, Including Year Effects			Displaced and Non-Displaced, Including Year and Worker Effects			Mean Initial Earnings of Displaced	Number of Individuals
		Overall Pre/Post	Immediate Pre/Post	Long Term Pre/Post	Overall Pre/Post	Immediate Pre/Post	Long Term Pre/Post	Overall Pre/Post	Immediate Pre/Post	Long Term Pre/Post		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(1) JLS Definition: Employer size in year of job separation 30% below early average	0.26	-272.76	-1131.67	137.90	-1698.29	-1497.44	-1940.58	-1012.24	-1629.45	-714.25	9623.75	123367
		(45.64)	(85.68)	(58.46)	(58.72)	(90.75)	(109.23)	(47.76)	(151.03)	(71.28)		
(2) Employer size dropped 30% around leave quarter (2 quarters pre/post)	0.23	-135.26	-903.08	213.09	-1550.08	-1285.25	-1784.31	-848.81	-1341.77	-622.87	9601.83	117829
		(55.34)	(102.55)	(71.85)	(71.74)	(108.81)	(136.57)	(53.42)	(183.72)	(78.67)		
(3) JLS Definition: Employer size in year of job separation 60% below early average	0.22	-182.05	-914.77	170.11	-1600.78	-1315.31	-1866.77	-887.51	-1379.91	-668.27	9691.63	117076
		(55.99)	(106.91)	(70.17)	(72.57)	(113.28)	(133.82)	(54.23)	(191.30)	(79.62)		
(4) Employer size dropped 60% around leave quarter (2 quarters pre/post)	0.20	-79.10	-773.94	238.37	-1469.69	-1150.55	-1752.90	-771.93	-1225.60	-584.15	9702.76	113640
		(64.80)	(122.65)	(81.93)	(84.17)	(130.34)	(156.81)	(58.98)	(223.16)	(85.85)		
(5) Employer closed in year of job separation	0.05	-741.31	-1620.03	-286.92	-2894.97	-2364.28	-3063.34	-1248.26	-2435.00	-1258.26	8380.99	95480
		(173.56)	(396.67)	(117.65)	(237.29)	(424.88)	(254.84)	(117.77)	(709.78)	(168.77)		
Percentage Loss In Quarterly Earnings Relative to Mean Initial Earnings												
(1) JLS Definition: 30% drop		-0.0283	-0.1176	0.0143	-0.1765	-0.1556	-0.2016	-0.1052	-0.1693	-0.0742		
(2) Instant 30% drop		-0.0141	-0.0941	0.0222	-0.1614	-0.1339	-0.1858	-0.0884	-0.1397	-0.0649		
(3) JLS Definition: 60% drop		-0.0188	-0.0944	0.0176	-0.1652	-0.1357	-0.1926	-0.0916	-0.1424	-0.069		
(4) Instant 60% drop		-0.0082	-0.0798	0.0246	-0.1515	-0.1186	-0.1807	-0.0796	-0.1263	-0.0602		
(5) Employer closed		-0.0885	-0.1933	-0.0342	-0.3454	-0.2821	-0.3655	-0.1489	-0.2905	-0.1501		

Notes: JLS Definition refers to definition of 'distressed' employer closest to the one chosen by Jacobson, Lalonde, and Sullivan (1993), see text. 'Early average' refers to average firm size from 1990.3 to 1991.2. General sample restrictions also parallel those implemented by Jacobson, Lalonde, and Sullivan (1993). Overall Pre/Post refers to wage change calculated over all available quarters before and after job separation. Immediate Pre/Post refers to wage change calculated over four quarters before and after job separation. Long Term Pre/Post refers to wage change calculated excluding six quarters before and after job separation. To obtain the number of displaced workers, multiply the displacement rate times the number of individuals. Note that the number of quarterly observations used for each regression model is much higher than the number of individuals. The lower panel of the table divides the coefficients of the first half by mean initial earnings. Standard errors in parentheses.

Table 3: Descriptive Statistics for Three-Way Match between California Respondents in February Displaced Worker Supplement (DWS), the March Current Population Survey (CPS), and the California Unemployment Insurance Base Wage (UI-BW) File from 1991.3 to 1999.4, Alternative Samples

		Displaced Workers in Matched Sample		
	Full Three-Way Matched Sample	**All Displaced Workers**	**With Valid DWS Wage on Lost Job**	**With Valid DWS Wage, Displaced at Plant Closing**
	(1)	**(2)**	**(3)**	**(4)**
Number of Individuals in Matched Sample	6699	565	490	184
Fraction with Age 20-35	0.394	0.430	0.445	0.413
Fraction with Age 36-45	0.298	0.296	0.294	0.266
Fraction with Age 46-64	0.308	0.274	0.261	0.321
Fraction without a High School Degree	0.381	0.388	0.382	0.353
Fraction with High School Degree	0.323	0.352	0.353	0.413
Fraction with More Than a High School Degree	0.358	0.324	0.318	0.304
Fraction Female	0.497	0.425	0.433	0.505
Fraction Non-White	0.179	0.138	0.139	0.179
Fraction of Workers Displaced (Lost Job)	0.084	1	1	1
Fraction of Workers Displaced due to Plant Closure	0.032	0.375	0.376	1
Fraction of Workers Displaced due to Slack Work	0.034	0.402	0.402	0
Fraction of Workers Displaced due to Position	0.019	0.223	0.222	0
Fraction Union Member on Current Job	0.006	0.009	<0.015	<0.03
Fraction Union Member on Lost Job	0.009	0.112	0.116	0.060
Average Years of Job Tenure on Lost Job	6.672	6.672	6.718	7.258
	(7.263)	(7.263)	(7.272)	(7.715)
Average Number of Jobs Held Since Job Loss	1.564	1.564	1.561	1.534
	(1.127)	(1.127)	(1.099)	(0.922)
Ln Wage (DWS) Current Job	6.108	6.108	6.110	6.046
	(0.746)	(0.746)	(0.767)	(0.804)
Ln Wage (DWS) Lost Job	6.132	6.132	6.132	6.099
	(0.771)	(0.771)	(0.771)	(0.737)
Ln Wage (UI-BW) Current Job	6.064	5.775	5.762	5.714
	(1.099)	(1.155)	(1.132)	(1.127)
Ln Wage(UI-BW) Lost Job, Definition 5 of Table 4	5.663	5.806	5.793	5.774
	(1.230)	(1.116)	(1.123)	(1.070)
Ln Wage (UI-BW) Lost Job, Definition 6 of Table 4	6.156	6.279	6.266	6.199
	(0.951)	(0.762)	(0.779)	(0.806)
Ln Wage (UI-BW) Lost Job, Definition 8 of Table 4	6.252	6.363	6.348	6.175
	(0.937)	(0.776)	(0.790)	(0.867)
Ln Wage (UI-BW) Lost Job, Definition 9 of Table 4	5.592	5.696	5.704	5.635
	(1.250)	(1.190)	(1.182)	(1.188)
Fraction Employed (DWS)	0.774	0.680	0.688	0.717
Fraction with Positive Quarterly Earnings (UI-BW)	0.778	0.715	0.708	0.734
Fraction Interviewed in CPS/DWS 1994	0.282	0.304	0.316	0.337
Fraction Interviewed in CPS/DWS 1996	0.206	0.255	0.241	0.228
Fraction Interviewed in CPS/DWS 1998	0.269	0.248	0.249	0.217
Fraction Interviewed in CPS/DWS 2000	0.243	0.193	0.194	0.217

Notes: Standard deviations in parentheses. Unless explicitly noted otherwise, all information is from the DWS. For comparison with quarterly earnings in the UI-BW, DWS wages are scaled by the number of weeks to the quarterly level. Wages at current job refer to wages at survey date. All wage, job, and employment information refers to workers reporting themselves as displaced. Wages at lost job refer to self-reported pre-displacement wages in the DWS; in the UI-BW, they refers to pre-displacement wages according to alternative definitions of job displacement as shown in Table 4 and described in the text. For a description of the match see the

Table 4: Measurement Error in Recorded Job Displacement for Individuals in the DWS -- UI-BW Matched File, Alternative Defintions of Displacement in the UI-BW File

	Displacement Rate		Displacement in Either UI-BW or DWS, in Both Data Sources, or in Neither Data Source				Conditional Probability of Job Loss in DWS Given Job Loss in UI-BW		Conditional Probability of Job Loss in UI-BW Given Job Loss in DWS	
Source of Information on Job Displacement	**UI-BW**	**DWS**	**UI-BW=0, DWS=0**	**UI-BW=0, DWS=1**	**UI-BW=1, DWS=0**	**UI-BW=1, DWS=1**	**DWS=1**	**DWS=0**	**UI-BW=1**	**UI-BW=0**
Definition of Displacement in UI-BW File	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**
(1) Any job separation	0.631	0.084	0.360	0.009	0.555	0.075	0.119	0.881	0.890	0.110
(2) Job separation with six quarters of job tenure	0.330	0.084	0.626	0.043	0.289	0.041	0.124	0.876	0.487	0.513
(3) Job separation, employer min 50 employees	0.516	0.084	0.463	0.021	0.453	0.063	0.123	0.877	0.750	0.250
(4) Job separation, 6 qrtrs tenure, employer min 50 employees	0.244	0.084	0.701	0.054	0.214	0.030	0.123	0.877	0.356	0.644
(5) Job separation in year employer size drops 30%	0.481	0.084	0.498	0.020	0.417	0.064	0.1328	0.867	0.758	0.243
(6) Job sep. in year employer size drops 30%, 6 qrts tenure	0.206	0.084	0.738	0.056	0.178	0.028	0.1358	0.864	0.331	0.669
(7) Job sep. in year employer size drops 30%, employer size>=50	0.360	0.084	0.605	0.035	0.311	0.049	0.1362	0.864	0.581	0.420
(8) Job sep. year empl. size drops 30%, 6 qrts ten., empl. size>=50	**0.143**	**0.084**	**0.792**	**0.065**	**0.124**	**0.019**	**0.1327**	**0.867**	**0.225**	**0.775**
(9) Job separation in year employer closes	0.308	0.032	0.682	0.010	0.287	0.021	0.069	0.931	0.675	0.326
(10) Job separation in year employer closes 6 qrts tenure	0.109	0.032	0.868	0.023	0.100	0.009	0.083	0.917	0.288	0.712
(11) Job separation in year employer closes, employer size>=50	0.224	0.032	0.761	0.016	0.208	0.016	0.071	0.929	0.500	0.500
(12) Job sep. in year empl. closes, 6 qrts tenure, empl. size>=50	0.075	0.032	0.900	0.025	0.069	0.006	0.084	0.917	0.198	0.802

Notes: The sample is all workers in the three-way match between Displaced Worker Supplement (DWS), March CPS, and Unemployment Insurance Base Wage (UI-BW) file in California from 1991.3-1999.4 described in column 1 of Table 3. The notation "DWS=1" implies that a job displacement was recorded in the DWS. A 'job separation' refers to change of employer identification number (EIN) between adjacent calendar quarters in the UI-BW. The change in employer size refers to the average size in 1991.3-1992.4. Six quarters of job tenure also refers to the period from 1991.3-1992.4. Employer size of at least 50 refers to the number of workers in 1992.4. For rows (9) to (12), the relevant measure of displacement in the DWS is taking to be plant closing. Thus, it is possible that in the column DWS=0 there are positive values if workers are displaced for other reasons.

Table 5: Wage Changes at Job Displacement for Individuals in the DWS -- UI-BW Matched File for Alternative Samples and Different Definitions of Wage Change

	Average of Difference in Log Wages Before and After Job Loss							Median of Difference in Log Wages	
Source of Information on Job Displacement	**All Displaced in Respective Data Source**	**UI-BW=0, DWS=1**		**UI-BW=1, DWS=0**	**UI-BW=1, DWS=1**			**UI-BW=1, DWS=1**	
Source of Earnings Information	**Either DWS or UI-BW**	**DWS**	**UI-BW for DWS**	**UI-BW**	**DWS**	**UI-BW for DWS**	**UI-BW**	**UI-BW for DWS**	**UI-BW**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
Panel A: Changes in Log Wages									
Job Displacement in the DWS	-0.073(+)		-0.206(*)					-0.124(*)	
	(0.034)		(0.053)						
Job Displacement in UI-BW File (Job sep. year empl. size drops 30%, 6 qrts ten., empl. size>=50)	-0.066	-0.057	-0.138	-0.004	-0.125	-0.418	-0.517	-0.157	-0.126
	(0.031)	(0.041)	(0.061)	(0.030)	(0.054)	(0.106)	(0.125)		
Panel B: Changes in Quarterly Earnings, Including Zeros									
Job Displacement in the DWS	-2340.3(+)				-780.2(*)			-318.6(*)	
	(237.8)				(268.3)				
Job Displacement in UI-BW File (Job sep. year empl. size drops 30%, 6 qrts ten., empl. size>=50)	-688.7	-2231.2	-1739.6	-238.9	-2697.3	-4378.7	-3628.3	-2117.3	-2087.5
	(758.9)	(271.2)	(287.0)	(866.7)	(495.7)	(683.6)	(742.9)		
Panel C: Changes in Log Wages Relative to Non-Displaced Workers									
Job Displacement in the DWS	-0.259(+)		-0.392(*)						
	(0.036)		(0.054)						
Job Displacement in UI-BW File (Job sep. year empl. size drops 30%, 6 qrts ten., empl. size>=50)	-0.289								
	(0.034)								
Panel D: Changes in Quarterly Earnings (Including Zeros), Relative to Non-Displaced Workers									
Job Displacement in the DWS	-3086.1(+)		-3138.2(*)						
	(281.2)		(316.6)						
Job Displacement in UI-BW File (Job sep. year empl. size drops 30%, 6 qrts ten., empl. size>=50)	-1832.5								
	(800.0)								

Notes: Standard errors in parentheses. The wage difference in Panels A and B is computed as the difference between the wage in the survey year and the last wage prior to job loss. This is the same definition of wage change as in Table 1 (either in logs or levels including zeros). For details on definition of job displacement in the UI-BW see Table 4 and text. The notation "DWS=1" implies that a job displacement was recorded in the DWS. To maximize the overlap between the survey and administrative data job loss is allowed to occur up to five years prior to the survey year. Appendix Tables 4, 5, and 6 shows results for other displacement definitions. Entries in Panel C and D are estimated changes in wages of displaced workers relative to workers not losing their job during the sample period. The sample consists of displaced workers (either in the DWS or the UI-BW) and workers in the matched-sample that did not lose their job (in the UI-BW, according to the respective definition). In the first row, the displacement measures is from the DWS. Similarly, ONLY in the first row, when the column is labeled DWS the wage is taken from the DWS. Otherwise, all wages are taken from the UI-BW file. To be comparable with the UI-BW, DWS weekly wages in Panels C and D are rescaled to represent quarterly earnings. Appendix Table 7 and 8 show results for other displacement definitions. Standard errors are in parentheses.

(+) Wage information from DWS.

(*) These entries refer to all job displacements in the DWS, irrespective of their job loss status in the UI-BW file. Wage information from UI-BW file.

Table 6: Estimates of Augmented Earnings Equations for Individuals in DWS -- UI-BW Matched File to Assess Measurement Error in Self-Reported Wages in DWS and Administrative Earnings in UI-BW

	Source of Wage Information					Estimates of Relation of Measurement Error in DWS Wages with Worker Attributes Treating UI-BW as "Truth"		
	UI-BW	**UI-BW**	**DWS**	**DWS**	**DWS**			
Wage Before Job Loss (Past) or After Job Loss (Current)	**Current Log Wage**	**Past Log Wage**	**Current Log Wage**	**Past Log Wage**	***Change in Log Wages***	**Current Log Wage**	**Past Log Wage**	***Change in Log Wages***
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Panel A: Equations 5 and 6	**6**	**6**	**5**	**5**				
Current Log Wage (DWS)	0.838 (0.094)							
Past Log Wage (DWS)		0.615 (0.090)						
Current Log Wage (UI-BW)			0.535 (0.061)					
Past Log Wage (UI-BW)				0.469 (0.071)				
Panel B: Equations 7 and 8	**7**	**7**	**8**	**8**				
Parameter Estimated	b	b	$b+\gamma$	$b+\gamma$	$b+\gamma$	γ	γ	γ
Dummy for Age 36-45	0.251 (0.123)	0.352 (0.106)	0.258 (0.086)	0.227 (0.099)	*0.031* *(0.083)*	0.005 (0.096)	-0.173 (0.109)	*0.178* *(0.117)*
Dummy for Age 46-64	0.176 (0.134)	0.378 (0.124)	0.064 (0.111)	0.329 (0.104)	*-0.265* *(0.097)*	-0.113 (0.079)	-0.138 (0.098)	*0.024* *(0.104)*
Dummy for High School Degree	0.206 (0.115)	0.179 (0.103)	0.123 (0.088)	0.121 (0.090)	*0.002* *(0.082)*	-0.084 (0.083)	-0.094 (0.092)	*0.010* *(0.108)*
Dummy for More Than a High School Degree	0.709 (0.123)	0.589 (0.112)	0.578 (0.086)	0.419 (0.109)	*0.159* *(0.085)*	-0.130 (0.080)	-0.212 (0.098)	*0.082* *(0.099)*
Dummy for Female	-0.333 (0.095)	-0.425 (0.093)	-0.347 (0.078)	-0.379 (0.077)	*0.032* *(0.075)*	-0.014 (0.069)	0.062 (0.085)	*-0.077* *(0.094)*
Dummy for Non-White	-0.063 (0.132)	-0.020 (0.126)	-0.020 (0.093)	-0.034 (0.098)	*0.014* *(0.091)*	0.043 (0.109)	0.025 (0.117)	*0.018* *(0.131)*
Dummy for Union Membership on Lost Job	0.126 (0.164)	0.055 (0.162)	-0.078 (0.155)	0.161 (0.101)	*-0.239* *(0.132)*	-0.204 (0.101)	0.123 (0.142)	*-0.327* *(0.149)*
Years of Job Tenure of Lost Job	0.008 (0.006)	0.007 (0.007)	0.003 (0.006)	0.017 (0.005)	*-0.013* *(0.006)*	-0.005 (0.004)	0.013 (0.006)	*-0.018* *(0.008)*
Number of Jobs Held After Job Displacement	-0.094 (0.056)	-0.104 (0.062)	-0.071 (0.041)	-0.098 (0.047)	*0.026* *(0.034)*	0.023 (0.028)	0.021 (0.053)	*0.002* *(0.045)*
Constant	8.454 (0.144)	8.571 (0.150)	8.519 (0.138)	8.615 (0.122)	*-0.096* *(0.142)*			
Root MSE	0.773	0.722	0.618	0.630	0.582			
R2	0.237	0.278	0.274	0.273	0.118			
Observations	254	254	254	254	254			

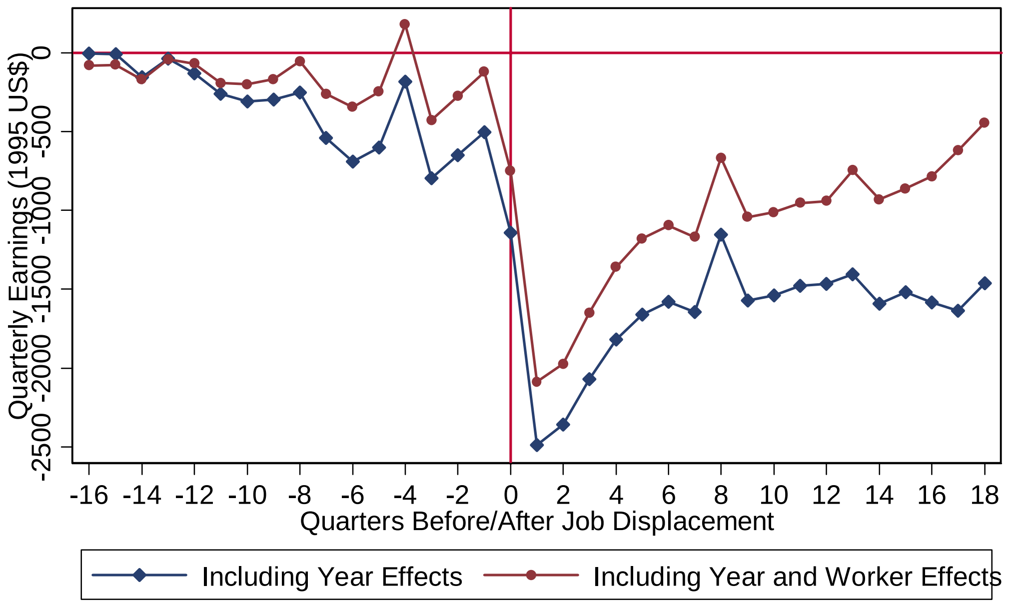
Notes: All regression equations included DWS survey year dummy variables. The top and bottom 1% of pre and post UI-BW earnings are dropped. For comparability with quarterly earnings in the UI-BW, DWS weekly wages are scaled to represent quarterly earnings. Coefficients on control variables in Panel A are shown in Appendix Table 7. Columns 6 to 8 show estimates of correlation of measurement error in DWS weekly wages with worker characteristics treating UI-BW earnings as the 'true' earnings measure (parameter gamma in the text). See equations (1)-(3) and (7)-(8) and discussion in text for additional explanations. Standard errors in parentheses.

Table 7: Difference-in-Difference Estimates of Cost of Job Loss Correcting for Two-Sided Mis-Classification Bias in Job Displacement, Selected Definitions of Displacement

Definition of Displacement in UI-BW File (Number corresponding to Table 4)	Difference-in-Difference Estimate for Workers with Job Loss Recorded in both DWS and UI-BW	Estimated Average Change in Log-Wages of Non-Displaced Workers	Implied 'True' Wage Change at Job Displacement	Percentage Change Implied by Log Differernce in Column (3)	Implied Misclassification Rates				Implied 'True' Displacement Rate
		Alpha	Lambda	=exp(Lambda)-1	Pr(UI=0|True=1)	Pr(DWS=0|True=1)	Pr(UI=1|True=0)	Pr(DWS=1|True=0)	P(True=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Unadjusted Measure of Job Loss in UI-BW (As in Table 4)									
(6) Job sep. in year employer size drops 30%, 6 qrts tenure	-0.421	0.235	-0.786	-0.544	0.000	0.634	0.198	0.064	0.051
(7) Job sep. in year employer size drops 30%, employer size>=50	-0.173	0.199	-0.281	-0.245	0.000	0.000	0.322	0.047	0.034
(8) Job sep. year empl. size drops 30%, 6 qrts ten., empl. size>=50	-0.517	0.210	-1.038	-0.646	0.000	0.564	0.152	0.069	0.027
(10) Job separation in year employer closes 6 qrts tenure	-0.255	0.295	-0.450	-0.362	0.000	0.849	0.071	0.023	0.065
(12) Job sep. in year empl. closes, 6 qrts tenure, empl. size>=50	-0.299	0.262	-0.499	-0.393	0.000	0.829	0.061	0.026	0.037
Panel B: Adjusted Measure of Job Loss in UI-BW (Maximize Overlap Between Two Data Sources)									
(6) Job sep. in year employer size drops 30%, 6 qrts tenure	-0.425	0.231	-0.727	-0.517	0.000	0.535	0.138	0.055	0.045
(7) Job sep. in year employer size drops 30%, employer size>=50	-0.107	0.180	-0.160	-0.148	0.273	0.605	0.103	0.021	0.141
(8) Job sep. year empl. size drops 30%, 6 qrts ten., empl. size>=50	-0.517	0.211	-1.217	-0.704	0.000	0.526	0.150	0.064	0.023
(10) Job separation in year employer closes 6 qrts tenure	-0.258	0.286	-0.407	-0.335	0.000	0.798	0.049	0.019	0.051
(12) Job sep. in year empl. closes, 6 qrts tenure, empl. size>=50	-0.299	0.258	-0.475	-0.378	0.000	0.814	0.060	0.022	0.036

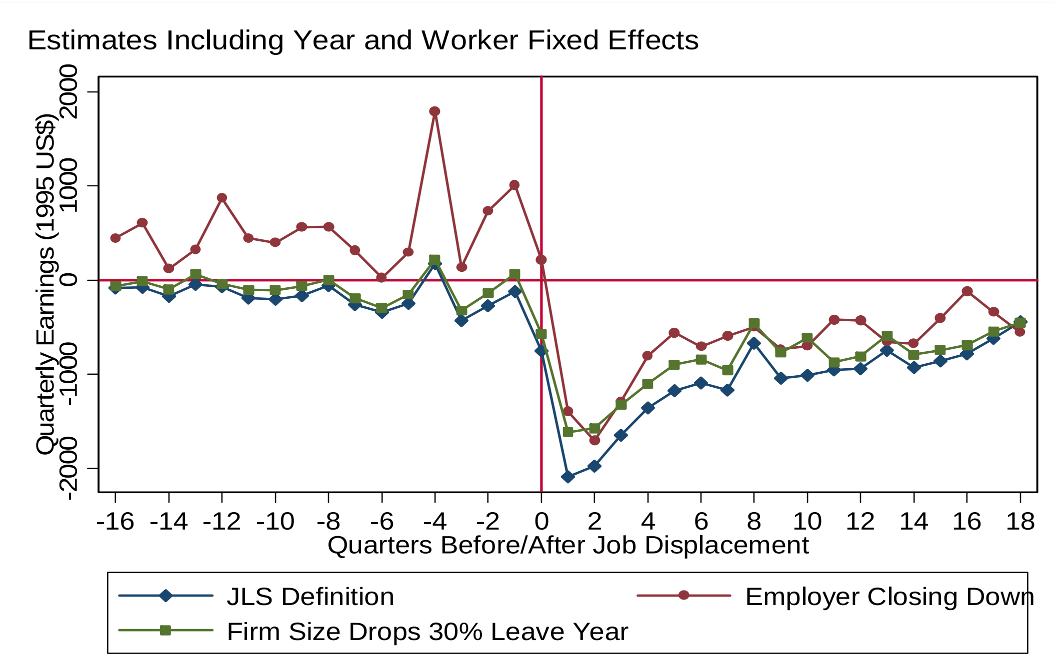
Notes: The first column refers to estimates of equation (9) in the text where the displacement dummy has been replaced by dummies for the events (DWS=1, UI-BW=0), (DWS=0, UI-BW=1) and (DWS=1, UI-BW=1); the estimates shown in the table refer to the coefficient on the latter dummy. The estimates in columns 2-9 are obtained from the method-of-moment estimator of equation (9) described Section 5.2.

Figure 1: Earnings Losses for Displaced Workers from Distressed Employers
(6 Qrts Tenure at Job Loss; Jacobson, Lalonde, Sullivan (JLS, 1993) Definition)



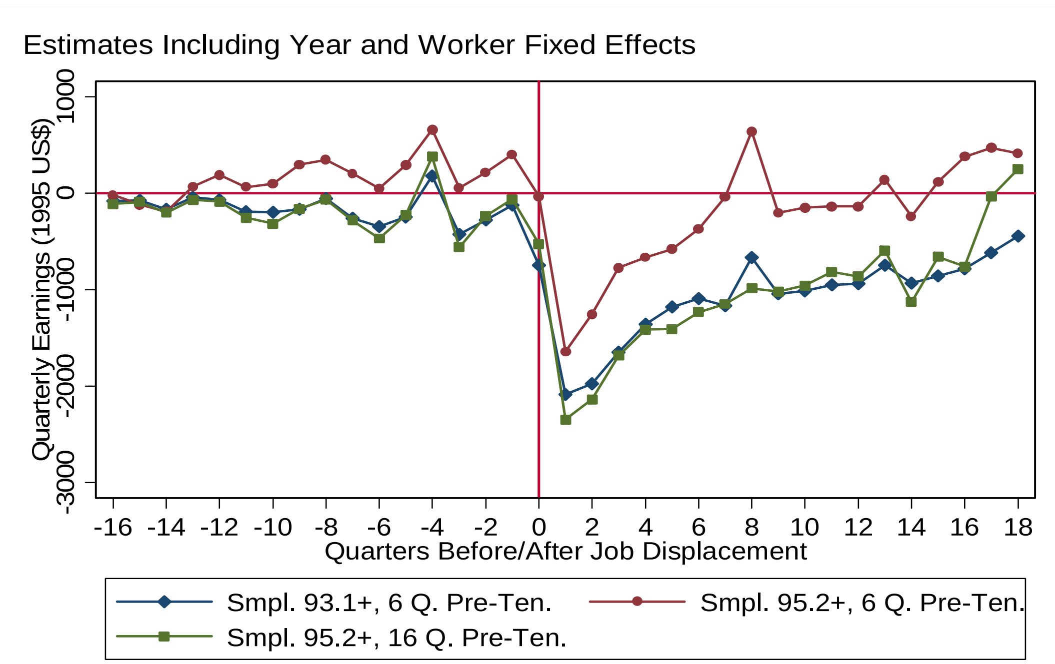
Displacements occuring 1993.1-1999.4. Source: 5% of California UI-BW File, 1990.3-1999.4 (see text).

Figure 2: Earnings Losses for Displaced Workers from Employers Closing Down, Sudden Drops, and JLS Definition (6 Quarters Tenure at Job Loss)



Displacements occuring 1993.1-1999.4. Source: 5% of California UI-BW File, 1990.3-1999.4 (see text).

Figure 3: JLS-Definition of Job Loss, Losses Starting in 93.1 vs. 95.2, Different Tenure at Job Loss (with and without FE)



Source: 5% of California UI-BW File, 1990.3-1999.4 (see text).