Estimating the 'True' Cost of Job Loss: Evidence using Matched Data from California 1991-2000*

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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. We find that reporting of displacements in the DWS suffers from recall error, while administrative measures tend to overstate job loss. Measurement error in survey data in both displacement and wages are correlated with worker demographics. When we use similar estimation methods estimates of earnings losses at displacement are similar from both data sets and larger than those based on the DWS alone. Correcting for misclassification errors in displacements suggests standard estimates understate the cost of displacement if errors are random, but overstate it if errors are due to 'salience.'

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1 Introduction

The extent and cost of worker displacement are recurring subjects of interest to economists and policy makers. 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 between ten and fifteen percent. 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 covering the U.S. labor market 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 wages of those displaced.[3] Similarly, the DWS is likely to under count 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 Insurance Base Wage (UI-BW) files in estimating the cost of job loss.[4] Estimates based on UI-BW files typically imply larger effects of job loss on earnings, ranging from 15 to 30 percent. Overall, the incidence and cost of job loss estimated by the UI-BW can exceed those

[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 (2004), and Esposito (2004).

[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 attrition and sample selection effects.

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 direct 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 may be 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 can depend 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. 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 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.[9] An increasing literature has

[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. Only recently von Wachter, Song, and Manchester (2009) have analyzed displacement with administrative data for the entire United States.

[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. Von Wachter and Bender (2006) examine how evidence from job losers can be used to study selection and sorting processes for young workers. Schmieder and von Wachter (2010) examine wage premiums due to past favorable labor market conditions lead to displacement, and whether these premia are lost after job loss.

[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]This has been subject of an earlier literature concerned with the effect of misclassification error on the

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 understanding the role governments can play in helping workers to adjust after a layoff (Hallock 2009), and in formulating appropriate labor market policies and government programs. Such policies may include job re-training, advance notification, and limited income transfers.[10] 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.[11]

This paper examines the sources of differences between estimates based on the DWS and the UI-BW and reassesses the 'true' incidence and 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 the DWS response, and the events as recorded in the UI-BW. 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). We then examine how successfully the disparate data sources record the same type of information concerning job loss and reconcile disparate estimates of the cost of job loss. We go on to estimate the cost of job loss in the matched data accounting for random and non-random misclassification error in the dependent and independent variable. Last, we provide basic insights on the sources of discrepancies between the two data sets.

Our findings suggest that recall error in the DWS may be pervasive. Workers do not report a substantial fraction of displacements recorded in the administrative data. The

measurement of labor force transitions (e.g., Poterba and Summers 1984, 1986), and the effect of reporting errors on measures of the incidence and duration of unemployment spells (e.g., Akerlof and Main 1982). See Bound, Brown, and Mathiowetz (2001) for an overview.

[10] In the US, this has taken the form of the Unemployment Insurance Benefits, Job Training Partnership Act (JTPA), the Worker Adjustment and Retraining Act (WARN), and the Workforce Investment Act (WIA). For a succinct overview of the costs and benefits of layoffs see Butcher and Hallock (2006), and references cited therein. For an overview of policy options for helping dislocated workers see Department of Labor (1995)

[11] 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).

degree of misclassification error varies with worker characteristics. Conversely, our findings suggest that the UI-BW appears to overstate the incidence of job displacement. Turning to 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 closer to estimates in the UI-BW.

However, once we estimate the costs of job loss for different different sub-groups in the matched sample, important differences emerge. In particular, workers not reporting themselves displaced in the DWS but with a job loss recorded in the UI-BW have substantially lower earnings losses than workers reporting themselves displaced. This suggests that reporting errors may be non-random and may vary with the cost of job displacement. Random misclassification would lead estimates based on either data source to be a lower bound on the average cost of job loss. Instead, if workers' displacement reports are based on the 'salience' of their earnings reductions, we show this would lead estimates based on the DWS to overestimate average earnings losses.

The paper makes several contributions to the existing literature. First, besides Oyer's (2004) succinct study based on information on displaced workers from a large firm, this is the only paper to analyze the incidence of recall errors in job displacement using matched survey and administrative data. This allows us to provide significant insights to the existing discussion of measurement error in the DWS which is based on survey data alone. It also contributes to an important earlier literature on recall errors in the incidence and duration of unemployment (e.g., Bound et al. 2001). We confirm key findings from that literature, mainly based on the CPS, suggesting that workers do not report a significant fraction of past employment events, that reporting error is correlated with worker characteristics, and that some of the discrepancies at the individual level partly average out.[12] Yet, perhaps not surprisingly, recall problems appear worse for displacement than for unemployment.[13]

Second, we assess the effect of misclassification error in the displacement measure on estimates of the cost of job loss. While some studies have analyzed the effect of random misclassification error in union status using multiple noisy measures (e.g., Freeman 1984, Card 1996), no such analysis exists for displacement or unemployment. Moreover, we are not aware of a study discussing the effect of non-random misclassification error in an independent

[12]See Levine (1993) for the most direct comparison of contemporaneous and retrospective individual and aggregate information from the CPS. Similar findings are reported by Mathiowetz and Duncan (1988) based on the PSID validation study from a single manufacturing firm.

[13]In principle, displacement could be an event that is more easily recalled than than unemployment under the complex definition followed by the CPS. However, in many cases unemployment is the result of a more severe job loss, suggesting the reporting bias in displacement may be no smaller than that in unemployment. Moreover, the notion of job loss is harder to report because it is more vaguely defined and leaves more room for interpretation to the respondent.

variable, even though the problem arises in any evaluation of the effect of a program based on self-reported participation. Yet, existing evidence from unemployment – workers forget to report short unemployment spells (Levine 1993), underreporting of past unemployment is pro-cyclical (Akerlof and Yellen 1985) – and from earnings dynamics – workers appear to misreport transitory but not permanent earnings fluctuations (Pischke 1995) – confirm our findings that 'salience' may be an important factor in recall. Estimates assuming two random noisy measures yield substantially larger costs and substantially lower incidence of job loss. When we allow for a role of salience, these estimates can be reinterpreted as partial average treatment effects, leading to a more plausible interpretation of our findings.

Third, we find that 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. Although the careful study of displaced workers by Oyer (2004) warns of recall bias in earnings for displaced workers, no estimates based on representative samples exist. Similarly, the current evidence on the correlation of measurement error in earnings with worker characteristics is mixed,[14] in particular when it comes to weekly wages.[15] An advantage of our administrative data with respect to the existing literature is that it is not topcoded, and thus is better at assessing measurement error for older or more educated workers.

Finally, by assessing the degree of measurement error in two commonly used data sources we contribute to the growing literature on job displacement. We find 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 the cost of job loss. Our results also imply that the DWS provides a lower bound on the incidence of job loss. Conversely, the UI-BW probably overstates the incidence of job loss, but may provide a lower bound on the cost of job loss.

[14]While using the PSID validation studies Duncan and Hill (1985) find measurement error to be correlated with characteristics, this does not appear to be the case in Bound and Krueger's (1991) findings based on the CPS matched to Social Security earnings records.

[15]Most of the information on measurement error in weekly wages pre-dates the 1980s (see Bound et al. 2001).

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

2.1 The Incidence and Cost of Job Loss

A job displacement is typically defined as a lasting involuntary job separation due to the operating decisions of the employer that are independent from the worker (such as a plant closing, insufficient work, or abolition of a shift). The incidence of job displacement then is simply the fraction of workers that had such an involuntary job separation during specified time interval. The cost of job displacement is typically defined as the difference in earnings before and after the job loss, relative to the regular evolution of earnings of a control group of comparable workers. The basic model of interest for differences of today's log wage relative to that of a base period prior to job displacement ($\Delta \ln w_i$) can be written as

$$\Delta \ln w_i = \alpha + \lambda D_i^* + x_i \varphi + \eta_i \tag{1}$$

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. The coefficient λ measures the differences in wage growth between displaced workers and their non-displaced counterparts.

Alternative data sources differ in how they approximate the ideal measure of job displacement, in how they measure wages, and in what information they provide about the control group. When using administrative data, often from the UI-BW files, researchers typically define a job displacement to be an instance where workers separate from their stable job during a year when the main employer suffers a mass-layoff (usually defined as a sudden and lasting reduction in employment, see Subsection 2.3 for more details). Since administrative data provide longitudinal information on worker's earnings, usually a dynamic model of equation (1) is estimated, where the evolution of earnings losses before and after the time of job displacement is measured relative to earnings growth of the control group.

When using survey data, researchers have to rely on respondents' judgment on which job separation fits in the above 'ideal' definition, and on the ability to correctly recall the time of occurrence and what prior earnings levels. When longitudinal data on a control group of similar workers is available (such as in panel surveys), again a dynamic version of equation 1 is estimated. In case of the Displaced Worker Survey this is not feasible, since the data contain only information on displaced workers themselves, with limited detail on prior earnings and job histories. Thus, researchers have analyzed the first difference in earnings over time, sometimes using average earnings of workers in similar age and education cells as control group (e.g., Farber 2003).

Clearly, given the differences in the measure of job displacement and differences in the methodology used to estimating the costs of job loss, one would not expect to see similar results from the Displaced Worker Survey and the UI-BW files. However, the differences turn out to be striking enough to warrant further inquiry as to whether it is differences in methodology, measurement error, or the underlying concept of job loss and earnings measured that can explain the discrepancies.

To give an overview of the contrast in the basic estimates from our two main data sources, before examining the cost of job loss using the matched file, we examine its component parts. Thereby, we also explore the role of differences in estimation methods in explaining different results.

2.2 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. 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.[16] With the consistent three year retrospective job history, and near consistent 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

[16] 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 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.

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).[17]

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).[18] The lower panel shows that the short- to medium-term cost of job loss as approximated by changes 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 significantly lower than typical wage losses obtained from administrative data. Californians also had lower average costs of job loss than the US, in the DWS.[19]

2.3 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) and Schoeni and Dardia (2000). The file contains longitudinal information on workers' quarterly earnings and employer size from 1990 to 2000. Presence of a state employer identification number (SEIN) for each job spell allows us to date job separations. Since the cause of job separation is unknown, we follow Jacobson, Lalonde and Sullivan (1993) and von Wachter, Song, and Manchester (2009) and declare a job separation a displacement if a worker separates from a 'stable job' while their firm experienced a 'mass-layoff'.

A 'mass-layoff' is supposed to have occurred if the firm's employment in the year following the separation is 30 percent or more below the maximum level at the beginning of the time

[17] The displacement rate is defined as the (weighted) number of workers reporting themselves as having been displaced within the last three years 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: $\ln w_{it} = \ln w_{it} - \ln w_{i(t-j),j=1,2,3}$.

[18] 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).

[19] 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.

period of study. While some workers will have quit their jobs in such a 'mass layoff' or from distressed firms, the majority 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).

A 'stable job' is a job that lasted at least six quarters or at least 16 quarters before displacement occurs. This leads us to choose either the first quarter of 1993 or the second quarter of 1995 as the first date of job separation. The earlier separation window allows us 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. The later separation window allows us to identify more long-term job holders in an analogous fashion with Jacobson, Lalonde and Sullivan (1993).

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).[20] Third, 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.[21]

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 model for estimation we use is:

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

where $i = 1, ..., n$ and $t = 1, ..., s$. The dependent variable (w_{it}) is the quarterly real earnings

[20] 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) and von Wachter, Song, and Manchester (2009) for a discussion of this point.

[21] 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.

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 $D_{it}^k, k = -m, -(m-1), ..., 0, 1, 2, ...$, 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.[22]

Row 1 of Table 2 and Figure 1 summarize estimates of the displacement rate and the overall short and longer-term wage loss at layoff based on the JLS definition of displacement. As suggested above, displacement rates are about twice as high in the administrative data as in the survey data for all definitions.[23] Similarly, the results based on the UI-BW file imply percentage earnings losses that are considerably larger than those suggested by the DWS in Table 1. Figure 1 shows that 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.

For purposes of direct comparison with the DWS, Table 2 displays also 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 and 7, we estimate the exact same cost of job loss for both data sets). 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.

It is important to note that the comparison between the typical estimates of the earnings loss at displacement based on the two data sources is affected by methodological differences. First, an often noted problem with the DWS is that it has no control group. A comparison

[22] 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.

[23] The displacement rate was defined as the number displaced under the chosen definition, 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.

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. 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. Finally, an 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).[24] 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.[25]

[24] 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).

[25] Other work, for example that by Topel (1990) notes that the DWS may substantially underestimate

A potential drawback of estimates based on the UI-BW file is that they are necessarily based on potentially 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 – summarized in Table 2 and in Figures 2 and 3 and addressed 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.

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. It also 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. Yet, we will see that mismeasurement of displacement and survey wages further distorts estimates based on the DWS. Similarly, the UI-BW appears to measure displacements with error.

3 Comparing the Incidence of Displacement using Matched Data

3.1 Misclassification Errors in Job Displacement

To reconcile the disparate estimates from the DWS and the UI-BW, we create a unique match between the two data sources for California from 1990 to 2000. This match allows us to re-estimate the cost of job loss based on the same individuals, the same earnings information, and the same methodology. It also allows 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. In this section, we discuss what can be learned if we consider the two data sources as providing two noisy measures of the same underlying 'true' event of job displacement.

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).

The discussion in Section 2 suggests that neither data source is likely to provide a perfect measure of job loss. There are several potential sources of errors or discrepancies in the measurement of displacement. sThe two data sets do not share a common 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. It is conceivable, for example, that workers only report job displacements that were associated with a spell of unemployment or larger earnings losses.[26]

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 of the SEIN recorded for that individual. We provide evidence that the number of recorded job transitions in administrative data is substantially higher than what is reported by workers (Section 5.4). Moreover, nothing is known about why an individual changed jobs, and thus typically researchers deem workers to have been displaced if the 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).[27]

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. This intuition is born out by recent evidence from the Job Openings and Labor Turnover Survey (JOLTS) (Davis, Faberman and Haltiwanger 2006), showing that for employment reductions of 30%, the fraction of quits relative to layoffs is small and declines further for larger layoffs. Here, we will also examine plant closing, an event where very few workers are likely to leave voluntarily. Another fact researchers have used to limit the degree of voluntary mobility counted as displacement in administrative data is that the incidence of quits falls rapidly with job tenure. However, by construction, these measures will miss all workers displaced from employers not experiencing a mass-layoff or who have lower tenure.

If we use a noisy measure of displacement when estimating equation (1), the resulting estimate of the cost of job loss is likely to be biased. The direction of the bias will depend on the type of measurement error. We will consider two types of measurement error – standard ('classic') measurement error, and measurement error due to recall based on 'salience'.

[26]Part of the problem with the DWS has been the definition of the worker's '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'.

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

Using notation from equation (1), suppose that the 'true' incident of displacement, D_i^*, is not observed. Instead, we observe a noisy measure. Consider first the standard case of purely random measurement error. We can write the observed measure as

$$D_i = D_i^* + \epsilon_i,$$

where ϵ_i is uncorrelated with any variables in the outcome equation. However, since D_i is binary, it is negatively correlated with ϵ_i. In this case it is well known that measurement error will lead to attenuation bias (Freeman 1984, Card 1996). The probability limit of the OLS estimator can be shown to be proportional to

$$\delta \equiv \frac{\pi}{p} \frac{1 - \pi_{01} - p}{1 - p},$$

where $\pi_{01} \equiv Pr\left\{D_i = 0 | D_i^* = 1\right\}$ is the probability of misclassification, and $p \equiv Pr\left(D_i = 1\right)$, $\pi \equiv Pr(D^* = 1)$.[28] The attenuation bias increases with the degree of misclassification and the underestimation in the average incidence. The OLS estimator thus provides a lower bound on the underlying average cost of job displacement.

With two noisy measures of the 'true' incidence of displacement, we can improve upon this bound, and in special cases recover consistent estimates of the cost of job loss λ. Let D_i^{UI} and D_i^{DWS}, denote the two imperfect measures of D_i^* from the UI-BW and DWS, respectively. Consider again the standard case in which the measurement error of these variables is uncorrelated with observable characteristics and with each other, conditional on the 'truth.' Black, Berger, and Scott (1999) have shown that given two such noisy measures, one can obtain a consistent estimate of 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 π. 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 of displacement and four conditional means of wage changes can be expressed as functions of the underlying misclassification probabilities and remaining parameters. For example, given our assumptions it is straightforward to show that

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

[28] I.e., $plim\hat{\lambda} = \lambda\delta$. This calculation can be extended to include the role of worker characteristics (Card 1996).

and

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

and similarly for the remaining conditional probabilities and expectations. The model is just identified and can be estimated using minimum distance.

We now turn to the dicussion of the second case, recall errors based on 'salience'. There are good reasons to believe that in the case of recall of past events, measurement error is not random. The following four patterns have emerged from the literature on recall of unemployment spells.[29] First, there is considerable underreporting of past unemployment spells. The degree of underreporting reaches up to 60-70% even for prime age men (e.g., Levine 1993, Mathiowetz and Duncan 1988). Second, some of the discrepancy at the individual level disappears at the average level. Third, the degree of underreporting varies with the length of the unemployment spell and the state of the labor market (e.g., Levine 1993), providing indirect evidence that the likelihood of reporting unemployment spells varies inversely with the cost of unemployment. Based on this evidence, the gap in the recalled and concurrent unemployment rate has been taken to be a measure of the salience of unemployment (Akerlof and Yellen 1985). Finally, the degree of recall error depends on workers' demographic characteristics. In addition, Pischke (1995) provides evidence that workers do not recall transitory earnings fluctuations well, but correctly report changes in permanent income.

These findings suggest that salience might also be at play when workers recall job displacement. Job displacement is an event that is less clearly defined as unemployment. Thus, we might expect that its recall by the worker is even more dependent on the surrounding experience. However, with the exception of Oyer (2004), none of the previous studies are concerned with salience and recall of job displacement. Similarly, none provide direct evidence on the correlation of the cost of unemployment in terms of lost wages and reporting errors. Conversely, little is known about how 'salience' in reporting affects estimates of the effects of job loss or unemployment on earnings. More generally, not much is known about the effect of correlated recall bias in a dependent variable. Yet, this problem is likely to arise in many economic applications (such as estimating the effects of self-reported participation in labor market programs on earnings).

Suppose that the impact of a displacement on workers' earnings λ_i varies by individual. The main idea behind salience is that the probability of reporting a job displacement increases with λ_i. To understand the potential impact of such a correlation, consider the following

[29] For a comprehensive summary see Bound et al. (2001). Goodreau, Oberheu, and Vaughan (1984) provide evidence of underreporting of welfare spells. For recent evidence on survey and administrative measures of employment status see Abraham, Haltiwanger, Sandusky, and Spletzer (2009).

special case. Suppose job losers whose wages are not affected by job displacement (i.e., for which $\lambda_i = 0$), do not report themselves displaced in the DWS, but that if the job loss is costly (i.e., $\lambda_i < 0$) reporting is correct. Similarly, assume that no worker reports a displacement if there was none. The resulting measurement process can be represented by

$$D_i^{DWS} = \begin{cases} 1 & if\ D_i^* = 1 \& \lambda_i < 0 \\ 0 & if\ D_i^* = 1 \& \lambda_i = 0 \\ 0 & if\ D_i^* = 0 \end{cases}$$

With these assumptions, the resulting model for earnings can be written as

$$\Delta lnw_i = \alpha + \lambda_i D_i^{DWS} + x_i \phi + \eta_i$$

$$D_i^{DWS} = D_i^* + v_i,$$

where we assume no other source of bias, i.e., $v_i \equiv (1 - D_i^*) \, 1 \{\lambda_i = 0\}$. Given that $cov(v_i, \lambda_i) > 0$, the resulting OLS estimates will be again biased. As shown in Appendix A, the OLS estimator now *overstates* the negative effect of job loss (relative to the average effect of job loss $\bar{\lambda} = E\{\lambda_i\}$). This bias is akin to what is often called self-selection bias, only that here it arises from individuals' reporting decisions, not individuals' choices.

A large literature has addressed ways of obtaining consistent estimates of the average treatment effect $\bar{\lambda}$ in this kind of selection model (e.g., Heckman, Lalonde, and Smith 1999). In general, consistent estimation requires the presence of an instrumental variable for D_i^{DWS}. Given the difficulty in obtaining estimates of the average treatment effect, more recently the literature has shown that instrumental variable estimates would obtain an estimate of the local average treatment effect (e.g., Angrist and Krueger 1999)

As exemplified by the limited overlap of the measures of displacement in the DWS and the UI-BW files, it will be difficult to find a good instrument for D_i^{DWS}. An alternative approach is to follow the lead of Black, Berger, and Scott (1999) and, treating our two measures of displacement as two noisy measures, assess the implications of the assumed measurement process for the empirical moments in the data. Given our assumption on the process of measurement error in the DWS, Appendix B shows that the resulting predictions for our moments are

$$\bar{y}_{00} \equiv E\left\{\Delta lnw_i | D_i^{DWS} = 0 \& D_i^{UI} = 0,\ x_i\right\} = \alpha + x_i\phi$$
$$\bar{y}_{01} \equiv E\left\{\Delta lnw_i | D_i^{DWS} = 0 \& D_i^{UI} = 1,\ x_i\right\} = \alpha + x_i\phi$$
$$\bar{y}_{10} \equiv E\left\{\Delta lnw_i | D_i^{DWS} = 1 \& D_i^{UI} = 0,\ x_i\right\} = \alpha + x_i\phi + \bar{\lambda}_{10}$$
$$\bar{y}_{11} \equiv E\left\{\Delta lnw_i | D_i^{DWS} = 1 \& D_i^{UI} = 1,\ x_i\right\} = \alpha + x_i\phi + \bar{\lambda}_{11}$$

The model offers an alternative interpretation of the empirical moments in the data based on partial average treatment effects:

$$\bar{\lambda}_{11} \equiv E\left\{\lambda_i | D_i^{DWS} = 1 \& D_i^{UI} = 1\right\} = \bar{y}_{11} - \bar{y}_{00}$$

$$\bar{\lambda}_{10} \equiv E\left\{\lambda_i | D_i^{DWS} = 1 \& D_i^{UI} = 0\right\} = \bar{y}_{10} - \bar{y}_{00}$$

Thus, $\bar{\lambda}_{11}$ is the average treatment effect for workers reporting a displacement present at a mass-layoff in the UI-BW; $\bar{\lambda}_{10}$ is the average treatment effect for workers reporting a displacement not present at a mass-layoff. The average wage growth of workers not displaced in both data sources ($\bar{y}_{00}$) is equal to the wage growth of workers only recorded as displaced in the UI-BW ($\bar{y}_{01}$). These workers are assumed not to have an effect of displacement ($\lambda_i = 0$), and thus independent of their true displacement status, their wages are predicted to evolve the same as for non-displaced workers. Since $\bar{\lambda}_{01}$ is equal to zero by assumption, the average treatment effect of all workers with a displacement in either data source can be rewritten as

$$\bar{\lambda} = \bar{\lambda}_{11} p_{11} + \bar{\lambda}_{10} p_{10},$$

where $p_{11} \equiv Pr\left\{D_i^{DWS} = 1 \& D_i^{UI} = 1\right\}$, etc. We will return to these predictions in Section 5. The simple 'salience' model provides a sensible description of our data. However, since some of the assumptions appear to be too restrictive, in Section 5 we will also discuss more general measurement error models based on 'salience'.

After discussing the matched data, in the following sections, we will first present evidence on the joint probabilities of job displacement in the two data sources (Subsection 3.3). Then we discuss information on average changes earnings for the different groups (Section 4). We will then use these moments to try to recover the 'true' incidence and cost of job displacement (Section 5).

3.2 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.[30] Second, we link the matched March-DWS data from respondents in CA 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-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.[31] Comparing the wage measures, we find 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

[30] 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).

[31] 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 chose 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.

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 when discussing measurement errors in wages in Sections 4 and 5.

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

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.[32] 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 substantially larger than those found in the DWS.[33]

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. For example., it is 20% if we do not impose a restriction on firm size (definition 6). Similarly, mobility in the UI-BW is 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 California economy in the mid to late 1990s.

The restriction on whether the employer size declines is also clearly important (rows 4 to 8) when comparing displacement rates. The reduction in employment is appears an important indicator of whether job separations are involuntary in administrative data. Using the Job Openings and Labor Turnover Survey (JOLTS), Davis et al. (2006) show that the quit rate declines rapidly with the size of employment reductions. According to their data, it turns out at 30% employment loss the fraction quitters among job separations is already low.

[32] 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.

[33] 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.

To further exclude voluntary quitters, we can also examine job separations at plants that close, where the JOLTS predicts hardly any voluntary quitters are present. Among the three stated reasons for job loss in the DWS, plant closure should be the most comparable definition of job loss with respect to the UI-BW.[34] In fact, Oyer (2004) reports that for displaced workers from a single company, workers report the incidence of plant closings very accurately. However, Oyer (2004) also finds that for more complex plant-level employment reductions, the defintion of reason for job loss in the DWS may create ambiguities. The results for plant closures are shown in rows 9-12 in Table 4. 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 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).[35] 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).

Table 4 suggests that the UI-BW may overstate the degree of job displacement for workers not displaced in the DWS. On the other hand, the measure proposed by Jacobson, Lalonde,

[34] 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.

[35] 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.

and Sullivan (1993) may not capture some actually displaced workers. To try to maximize the degree of overlap between the two data sets, we replicated our match allowing a displacement in the DWS to match with any job separation in the UI-BW, while trying to remove 'false' displacement from the UI-BW file by imposing the JLS restrictions in the absence of a displacement in the DWS. This is shown in Appendix Table 3. In the resulting sample, there is a higher degree of overall overlap - about 25% of workers with a job loss in the UI-BW (JLS Definition) report a job loss in the DWS. However, it is still modest in absolute terms. 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).

The discussion of Table 4 establishes two recurring "themes" of the paper that add to the results of the general comparison in Tables 1 and 2. First, there is only modest to small overlap in the incidence of job loss between the two surveys even for the same group of individuals. An important reason for the discrepancy is that the DWS tends to undercount job separation during clearly identifiable events in the administrative data such as a mass-layoff or a plant closing. Conditional on having a job loss in the administrative data, only 15-25% of workers report a job loss in the DWS. As suggested above, this finding is not completely surprising given the literature on recall of unemployment spells. Using individual level data from the CPS, Levine (1993) reports that 40-50% spells in the past year go unreported. The fraction rises up to 60% for short spells (lasting up to 4 weeks). Using administrative and survey data from a single company, Mathiowetz and Duncan (1988) report 66% of unemployment spells in the past year go unreported, while the number reaches 75% for very short spells. Given that recall in the DWS is over three years, that job displacements often do not involve unemployment, and that job displacement is likely to be a more difficult concept, our findings are in line with the previous literature. Consistent with the literature on contemporaneous and retrospective unemployment rates, the difference in the *average* displacement rate is smaller than that at the individual level (e.g., Bound et al. 2001)!

A second implication from Table 4 is that the UI-BW appears to overstate the rate of job loss. However, the degree of overstatement depends on how we define job loss in the administrative data. 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.

3.4 Misclassification 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.[36] 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). These findings echo earlier results based on the CPS the recall of unemployment spells, where younger and non-white workers have lower retrospective reporting rates (e.g., Levine 1993). However, we do not confirm a higher recall bias for women. We are not aware of a study of recall error by education group.

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.[37]

[36] 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.

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

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.[38]

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 DWS, the estimated loss is substantially larger. The estimated cost of job loss for all displaced workers in the DWS is -20.6% (row 1), three times larger than -7.3% obtained by the survey wagedata. It is also considerably 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 (UI-BW=DWS=1), the difference due to different sources of earnings information is -41.8% (row 2, column 6) vs. -12.5% (row2, column 5, from DWS wages). The former, based on UI-BW earnings, is much closer to the estimate of -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 our 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. 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

[38] 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.

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). Including zero wages makes an important difference. It makes a bigger difference for wages in the DWS, since for job losers there is a higher incidence of non-employment in weekly than in quarterly data. For workers with a job loss in both surveys (UI-BW=DWS=1), the discrepancy in survey and administrative earnings in levels vs. logs is less drastic (row 2, columns 5 and 6).

Another important source of discrepancies is that in several cases earnings in the administrative data are higher than recorded 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.[39] 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.

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 difference between the change in levels and in logs is partly due to the group (UI-BW=1, DWS=0). Among these, some workers retire or otherwise leave the labor force and thus have zero quarterly earnings, thus lowering the estimate in the UI-BW in levels. The cost of job loss is very large and similar for workers displaced in both samples (UI-BW=DWS=1), 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 may be because, as suggested by the results of Table 4, the UI-BW is likely to overstate the incidence of job loss. For example, as discussed above,

[39] 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).

some of these workers may be leaving their employer voluntarily and benefit from the job change. They may also retire and have zero earnings. On the other hand, workers with low earnings losses may not report themselves as displaced in the DWS. Similarly, some of these workers leave 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. Finally, 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 considerably 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 with low earnings losses 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 1997, 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.[40] 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

[40] 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.

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%, almost double the comparable estimate without a control group (-20.6%).[41]

Including a control group substantially matters for estimating the cost of job loss in the administrative data as well (-28.9% vs. -6.6%). This is also borne out by the results in wage levels (comparing Panel D vs. Panel B with no control group). As in the case for simple pre/post differences in wage levels, the estimated cost of job loss captured in the DWS is again larger than in the UI-BW once we include a control group.

The results in Table 5 emphasize that once we 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. 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 of the same order of magnitude. These results provide direct evidence on the importance of a control group in the DWS, complementary to findings reported in Farber (1997, 2003) based on the CPS. Compared to a control group, the inclusion of pre-job loss characteristics makes very little difference.[42] The evidence also confirms that the UI-BW may understate the cost of job loss because it includes non-displaced movers. For the DWS, the fact that those workers not reporting a job separation during a mass-layoff have considerably lower declines in earnings implies that earnings losses at job loss may be overstated, something we return to in Section 5.3.

[41] 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. It may also be due to differences in the nature of the data.

[42] 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.

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. In particular, 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 high wage workers – may misreport 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.

This is an important question, since past work by Oyer (2004) and Duncan and Hill (1985) suggests workers recall past wages with error. Analyzing displaced workers from a large company who answered a questionnaire similar to that in the DWS, Oyer (2004) finds that a small fraction of workers significantly overstate their past earnings. Based on data for all workers from another large firm, Duncan and Hill (1985) report that recall error is correlated with worker characteristics. On the other hand, using data from the CPS matched to Social Security earnings records, Bound and Krueger (1991) do not find correlation of contemporaneous measurement error and characteristics. However, the earnings data from Social Security they used was topcoded, while the earnings data from the UI-BW is not.[43] We are not aware of any other analysis of recall error in past wages and their correlation with characteristics of a representative sample of workers.

While the UI-BW records are employer reported and should be the 'true' measure of the wage received by an individual in a job, there is evidence that these data also contain some degree of measurement error (Kornfeld and Bloom 1999).[44] For the work here, any

[43]Another difference is that we analyze weekly instead of annual earnings, as is typically done in the literature on measurement error. A drawback of our data is that we have to use weekly wages to infer about quarterly earnings (or vice versa).

[44]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 underreport, earnings of individuals in short-term or low wage jobs. Employers 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

systematic bias in UI-BW earnings will affect the level difference between UI-BW and DWI wages. However, this will only affect 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.[45]

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 wages with 3 components:

$$\ln w^{*} = xb + v \tag{2}$$

$$\ln w_{dws} = lnw^{*} + \varepsilon_1 \tag{3}$$

$$\ln w_{ui-bw} = \rho + lnw^{*} + \varepsilon_2 \tag{4}$$

where lnw^{*} is the log of (unknown) true wages, $\ln w_{dws}$ 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 through:

$$\varepsilon_1 = \gamma x + \xi \tag{5}$$

with ξ iid.[46] 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 regress log earnings in the UI-BW on earnings in the DWS and vice versa for both current and past earnings. It is well known (and shown in Appendix C) that within our model the resulting coefficients provide an estimate of the signal-to-total-variance ratio. Second, given our assumptions, we can estimate the degree

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 underreport 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.

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

[46] 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).

of correlation in the error of current and past survey earnings with worker characteristics.

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 coefficient estimate of 0.535 (0.469) on the current (past) UI-BW in a regression of DWS wages suggests that measurement error in wages is pervasive and larger than in the DWS (as compared to the estimates for the coefficients 0.838 and 0.615 on current and past DWS wages, respectively, in a regression of UI-BW wages). These coefficients would be equal to 1 without measurement error, and as shown in Appendix C their magnitude depends on the degree of iid measurement error in earnings in the two data sources ($\sigma^2_{\epsilon_2}$ and σ^2_{ξ}).[47] 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 Tables 8 and 9), the estimates (and implied error variances) are similar in the two surveys.[48]

Second, most of the coefficients on the other 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.[49] 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).[50] 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 stronger for past than for current wages. Like-

[47]If we regress UI-BW wages on DWS wages, the coefficient estimate is $p\lim\widehat{\beta} = \sigma^2_v/(\sigma^2_v + \sigma^2_{\varepsilon_2})$. If we regress DWS wages on UI-BW wages the coefficient estimate is $p\lim\widehat{\theta} = \sigma^2_v/(\sigma^2_v + \sigma^2_{\xi})$.

[48]Note that this implies the overall measurement error for DWS earnings, $\sigma^2_{\epsilon_1}$, is likely to be higher in the DWS, since it is augmented by the degree of correlation in measurement error with worker characteristics.

[49]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 $p\lim\widehat{\delta} = (b\sigma^2_{\xi} - \gamma\sigma^2_v)/(\sigma^2_v + \sigma^2_{\xi}) = b\sigma^2_{\varepsilon_1}/(\sigma^2_v + \sigma^2_{\varepsilon_1}) = 0$.

[50]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.

wise, 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 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. Since we had seen 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 more educated workers may underreport high earnings in the CPS.

Overall, these 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. Regarding the literature based on the DWS, the attenuation bias coefficients indicate that some results might record 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); similarly for union membership (Kuhn and Sweetman, 1999). However, it also appears that the influence of past tenure on the cost of job loss may have been over-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.[51]

[51] 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

5.2 Estimating the Cost of Job Loss Under Random Misclassification Errors

The evidence presented so far suggests that both the DWS and the UI-BW measure displacement with some degree of error. Thus, even if we are willing to treat earnings as recorded in the UI-BW as free of correlated measurement error, the estimates in Table 5 are unlikely to provide consistent estimates of the cost of job loss. The extent extent to which they provide useful measures and whether one can obtain consistent estimates depends on the source of misclassification error. In this section we consider the benchmark case in which misclassification errors in both data sources are completely random. In this case, the estimates of earnings losses in Table 5 will underestimate the true degree of job loss.

As discussed in Section 3.1., Black, Berger, and Scott (1999) [BBS] show that given two available noisy measures and uncorrelated measurement error, 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.[52]

Second, BBS show that given two available measures, one can obtain a consistent estimate of the true underlying cost of job loss λ, as well as measures of the misclassification probabilities and the true underlying displacement rate $\pi = Pr(D^* = 1)$. The parameters we are interested in are seven: in addition to λ and π, αcaptures the average change in wages for non-displaced workers, and $\pi_{10}^{UI-BW}, \pi_{10}^{DWS}, \pi_{01}^{UI-BW}, \pi_{01}^{DWS}$ are four probabilities of misclassification. The moments available are also seven; three conditional probabilities of displacement and four conditional means of wage changes. These seven empirical moments are a function of the seven parameters. Thus, the model is just identified and can be estimated using minimum distance.

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

(1991). Bound and Krueger did not analyze measurement error in past earnings.

[52] 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 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.[53]

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 estimator 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.[54]

Table 7 also displays the estimated misclassification probabilities for the two measures of displacement. The results confirm the discussion in Section 3. A recurring theme of this paper is that the DWS underreports job loss, and this clearly stands out again here (column 6). Conversely, it appears that the maximization algorithm tends to set the misclassification

[53] 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.

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

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).[55] 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 explains 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). This suggests that estimates based on mass-layoff may be particularly affected by attenuation bias from measurement error. 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 considerably smaller than that reported in the UI-BW, and smaller than even the displacement rate in the DWS. This finding (as with the other results in the table) may be partly 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 modest (Appendix Table 3), this may not be too surprising.[56]

The results 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. If misclassification errors can assumed to be random, 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, 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. However, since plant closings do not exhibit higher overlap in the two data sources (Table 4), this

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

[56] Using the predicted relationship between the empirical moments and estimated parameters, we also examined the in-sample fit of the model. The model does very well at fitting the joint probabilities of displacement. It also does a good job at predicting large earnings losses for observations with $(D_i^{UI} = 1, D_i^{DWS} = 1)$ and considerable gains for the group in $(D_i^{UI} = 0, D_i^{DWS} = 0)$. Also roughly consistent with the data, the model sets $E(\Delta w|D^{UI} = 1, D^{DWS} = 0)$ greater-equal zero, consistent with the fact that π_{01}^{UI} is large. However, the model cannot explain why $E(\Delta w|D^{UI} = 0, D^{DWS} = 1)$ is smaller zero. There are two reasons for this failure. First, the model sets π_{01}^{UI} to zero, implicitly assuming that there are no displacements other than at mass-layoffs. Second, this interacts with the fact that the estimated 'true' displacement rate π is small. Both of these results are likely due to the fact that the overlap between the two data sources in measured displacement is small.

finding should be treated with caution.

5.3 Salience as Source of Recall Error

The interpretation of these findings changes if the misclassification of displacement is not assumed to be random. We have argued that there are good reasons to believe that workers underreport displacement events with small or temporary consequences for earnings. There is ample evidence of significant underreporting of short or inconsequential welfare or unemployment spells (e.g., Goodreau, Oberheu, and Vaughan 1984, Mathiowetz and Duncan 1988, Levine 1993). Similarly, there is evidence that workers report fluctuations in permanent earnings more accurately than temporary earnings variations (Pischke 1995). Consistent with a role for 'salience,' in section 4 we found that average earnings changes for workers recorded as displaced in the UI-BW but not in the DWS were zero (though the change was again negative when compared to the control of workers never displaced).

In Section 3.1, we described a basic model of reporting in the DWS based on salience. In this model, workers do not report a displacement if the consequence to earnings is zero (i.e., $\lambda_i = 0$). As a result, the simple OLS estimate is predicted to *overestimate* the negative effect of job loss due to self-selection bias. Using two noisy measure of displacement, based on our empirical moments we can identify partial average treatment effects for two subgroups of displaced workers. In particular, we obtain

$$\bar{\lambda}_{11} \equiv E\left\{\lambda_i | D_i^{DWS} = 1 \& D_i^{UI} = 1\right\} = \bar{y}_{11} - \bar{y}_{00} = -0.641$$

and

$$\bar{\lambda}_{10} \equiv E\left\{\lambda_i | D_i^{DWS} = 1 \& D_i^{UI} = 0\right\} = \bar{y}_{10} - \bar{y}_{00} = -0.361$$

Based on reporting error within a random coefficients framework we can more easily interpret the apparent heterogeneity in the estimated cost of job loss. It is very plausible that the effect of job loss on earnings differs by the environment of job loss. Workers who report themselves displaced in the DWS and were also recorded as displaced in the course of mass-layoff in the UI-BW have large earnings losses of -0.641 log points (about-47%). Workers displaced in the DWS but not in the UI-BW had a loss of $-.361$ (about -30%).

The model has sensible implications for the average treatment effect among all workers with a job loss in either data source. According to the model, the average treatment effect would be -0.17 log points (about -16%). This is a much more plausible estimate than the large numbers obtained for λ in the previous section. Overall, we believe that not treating the DWS and UI-BW measures of displacement as though they equally affected by random measurement error leads to more sensible interpretations of the data. The model also performs reasonably well if we compare its restrictions for misclassification probabilities with the estimates in Table 7 – in particular, it seems setting both π_{01}^{UI} and π_{10}^{DWS} to zero does not appear to be a bad approximation.

However, not surprisingly, the data also indicate that the simple process for recall error in the DWS we assumed is too restrictive. in particular, it is unlikely that the true losses of workers displaced in the UI-BW but not in the DWS are truly zero. In fact, if we compare the average earnings of workers displaced in the UI-BW but not in the DWS ($\bar{y}_{01} - \bar{y}_{00}$) the effect is $-.219$ and not zero for the JLS measure of displacement (though it is approximately zero for the case of plant closings).[57] This implies that a substantial fraction of workers recorded as displaced in either one of the two data sources but not the other (about 60%) may in fact have a wage loss. This would imply a substantially larger average effect among all job losers.

Under more general assumptions on salience-induced measurement error (e.g., if we let $Pr\left\{D_i^{DWS} = 1\right\}$ vary in a more general fashion with λ_i), obtaining the average treatment effects for different subgroups depends on recovering the misclassification probabilities. However, just counting parameters and moments makes it clear that the model is not identified without additional restrictions. We have seven empirical moments, as before. The number of parameters increases, because instead of a single treatment effect λ, we now have treatment effects for three subgroups. As we discuss in Appendix B, of these only $\bar{\lambda}_{11}$ can be identified without further assumptions (as in the basic model). It is possible that with additional moments or additional restrictions a more general model would be identified. We leave the estimation of more complete models to researchers with larger data sets that can use additional moments in the data for identification.

5.4 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 (von Wachter, Handwerker, Hildreth 2008) and in the appendix. Despite a rich amount of data, given limited sample sizes, this analysis should be

[57] This effect is driven by earnings growth in the control group, since the average earnings loss for these workers is small (see Table 5).

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. These workers are less likely to be recorded as displaced in the UI-BW. It also appears that the DWS may underreport displacements from large employers. These workers may not realize that the reason for their leaving the firm is a layoff. Similarly, the DWS appears to miss an important degree of job mobility occurring 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 occurrence. 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 have shown above that 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 data by restrictions on pre-displacement job tenure or firm employment events.

5.5 Discussion of Misclassification Bias in DWS and UI-BW File

Based on these findings, what should a researcher interested in estimating the cost of job loss do? Consider the typical case in which a researcher does not have access to matched survey and administrative data. If we believe that the two data sources yield noisy measures of the same underlying events, and that misclassification is random, then estimates from either the DWS or the UI in Table 5 provide a lower bound of the true cost of job loss. The error arises because the UI-BW includes workers as job losers who appear not to be truly displaced, while the DWS misses a substantial fraction of displacement events.

Maintaining the assumption of random misclassification error, to minimize the Type I errors (ignored displacement), one would prefer the UI-BW data to the DWS. This is useful since the results suggest that the administrative data delivers a lower bound for the cost of job loss. 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 rate of Type II errors (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 without a control group based on positive survey wages alone lead to even larger underestimation of the true cost of job loss than in the UI-BW.

If instead we believe that recall errors in the DWS are partly driven by salience, then the OLS estimates of equation (1) based on the DWS do not represent a lower bound. Instead, results may overestimate the average cost of job loss. In this case the UI-BW file is preferable to the DWS, for two reasons. First, since measurement error in the UI-BW indicator of displacement is unlikely to be correlated with the cost of job loss, OLS estimates based on these data will still represent a lower bound of the true average effect of job loss.[58] Second, with a measure of firm-level employment changes available, it is possible to implement an instrumental variable (IV) strategy that is not affected by misclassification error in the displacement indicator. In particular, if one uses, say, occurrence of mass-layoff at the employer as instrument for job mobility, the IV estimator is the ratio between the effect of being present at a mass-layoff on earnings and on job separation. If job separations are dated correctly, neither estimand is affected by wrongly classifying displacements.[59] Another advantage of the IV estimator is that in the presence of heterogeneous costs of job loss it can be interpreted as average treatment effect of those induced to move by the employment shock. No such interpretation is available for the OLS estimator.

What if the researcher does not accept the assumption that the two data sources measure the same underlying events? In this case, 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 mass-layoff"). Based on the results of this paper, since job loss as reported in the DWS frequently does not reflect firm-level events observed in the UI-BW (even clearly identifiable events such as plant closing) we cannot say with the same confidence what event is reported as a "displacement" in the DWS data.

Finally, what should a researcher do who does not have access to administrative data such as the UI-BW, but instead would like to work with the DWS? Our results suggest that the incidence of job displacement can be used as lower bound of the true rate of displacement. In the presence of salience, this bound can assumed to be tighter during recessions, when more workers are likely to report job loss. In terms of the cost of job loss, our findings confirm the importance of using a control group based on, say, the March CPS (Farber 2003) and of dealing with zero earnings (for example by reporting quantiles). Yet, even technically correct estimates will be affected by measurement error. The impact of the error depends on its source. If salience is important, as we argue is likely, then OLS estimates based on the DWS cannot be interpreted as lower bounds. One approach is to follow the lead of Akerlof

[58] If the UI-BW indicator is also correlated with the cost of job loss, then the OLS estimator can be shown to be a weighted average of the treatment effects; since the treatment is binary, the weights will be larger with workers exhibiting larger costs of job loss (see Angrist and Krueger 1999). However, there is no reason to believe that the UI-BW displacement measure should be more correlated with the cost of job loss than the DWS measure in the presence of reporting based on salience.

[59] The instrumental variable estimator requires the additional assumption that the effect of the mass-layoff is only on job separators. For further discussion, see von Wachter, Song, and Manchester (2009).

and Yellen (1985) and instead interpret estimates based on the DWS as a measure of “felt” costs of displacement, i.e., the impact of displacement that workers deemed relevant enough to report. However, given the potential role of other sources of measurement error, we defer this question to future research.

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 incidence and cost of job loss. We find that both the DWS and the UI-BW file provide noisy measures of job loss. The DWS misses a substantial amount of job displacement recorded in the administrative data. This recall error is correlated with characteristics, and partly averages out at the aggregate level. Thus, one can treat the displacement rate in the DWS as lower bound of the true incidence of displacement. Recording of displacement in the UI-BW on the other hand tends to overstate the incidence of displacement.

Our estimates confirm that earnings losses after job displacement are large. They are significantly underestimated by conventional estimates based on the DWS because of a lack of a control group of non-displaced workers and because zero earnings are ignored. Once we use the same sample, the same earnings measures, and the same methodology, we find that an important part of the discrepancy between estimates of the average cost of job loss based on DWS and on the UI-BW disappears. Both of these measures are lower bounds if misclassification errors in the measure of job loss are random. However, consistent with evidence from the prior literature, we find that recall error in the DWS may be correlated with the 'salience' of the displacement event. In this case, the DWS would overestimate the cost of job displacement, while the UI-BW would still provide an underestimate. Under salience, we show that from two noisy measures of job loss one can recover partial average treatment effects of the cost of job loss.

We have also provided additional evidence as to the sources of discrepancy in the job loss measure between the two data sources. A majority of workers counted as displaced in the UI-BW but not in the DWS come from large employers and experience little direct 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. We also found that there is significant reporting error in current and past DWS wages, and that this error 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 academic work, and as an indicator of the health of the labor market and the formulation of policy. Given its accessibility, its long time-series, and its coverage of the entire U.S. labor market, the DWS is likely to remain the main soure of information on job displacement. However, our findings suggest that the displacement numbers and the associated cost of job loss from the DWS have to be interpreted appropriately. 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 and wage outcomes.

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 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 indicate that even using a control group and accounting for zero earnings the estimates from both data sources may be biased. The direction of the bias and the parameters that can be recovered depend on assumptions on the nature of the underlying processes of measurement error.

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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)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(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

	Full Three-Way Matched Sample	Displaced Workers in 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**			**All Displaced in Respective Data Source**	
Source of Earnings Information	**Either DWS or UI-BW**	**UI-BW wage for DWS job displacement**	**DWS**	**UI-BW wage for DWS job displacement**	**UI-BW**	**DWS**	**UI-BW wage for DWS job displacement**	**UI-BW**	**UI-BW wage for DWS job displacement**	**UI-BW**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**
Panel A: Changes in Log Wages										
Job Displacement in the DWS	-0.073(+) (0.034)	-0.206(*) (0.053)							-0.124(*)	
Job Displacement in UI-BW File (Job sep. year empl. size drops 30%, 6 qrts ten., empl. size>=50)	-0.066 (0.031)		-0.057 (0.041)	-0.138 (0.061)	-0.004 (0.030)	-0.125 (0.054)	-0.418 (0.106)	-0.517 (0.125)	-0.157	-0.126
Panel B: Changes in Quarterly Earnings, Including Zeros										
Job Displacement in the DWS	-2340.3(+) (237.8)	-780.2(*) (268.3)							-318.6(*)	
Job Displacement in UI-BW File (Job sep. year empl. size drops 30%, 6 qrts ten., empl. size>=50)	-688.7 (758.9)		-2231.2 (271.2)	-1739.6 (287.0)	-238.9 (866.7)	-2697.3 (495.7)	-4378.7 (683.6)	-3628.3 (742.9)	-2117.3	-2087.5
Panel C: Changes in Log Wages Relative to Non-Displaced Workers										
Job Displacement in the DWS	-0.259(+) (0.036)	-0.392(*) (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(+) (281.2)	-3138.2(*) (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**				
Parameter Estimated	ß	ß	θ	θ				
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) in Appendix C 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

	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)
Definition of Displacement in UI-BW File (Number corresponding to Table 4)	**(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.