Variation in the Cost of Job Loss by Worker Skill:

Evidence Using Matched Data from California, 1991-2000[1]

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Abstract

This paper uses a unique match of survey and administrative records to provide estimates of the short- and long-term cost of job loss by education and occupation in the California in the 1990s. We find that education and occupation have a hump-shaped effect on the cost of job loss. Workers with a college degree fare better than workers without a high school degree, who in turn have smaller losses than workers with some college or a high-school degree. This suggests higher education may allow workers to better respond to earnings shocks. It may also convey access to scarce high-wage jobs permanently lost at displacement. We also show that job loss reduces the rates of home ownership and private health insurance coverage, and that these are likely to be only partially explained by long-term earnings losses. Job loss appears to reduce workers' lifetime outcomes along multiple dimensions.

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1 Introduction

An increasing number of papers have shown that displaced workers can suffer large and lasting losses in earnings (e.g., Ruhm 1991, Jacobson, Lalonde, and Sullivan 1993, Schoeni and Dardia 2003, Couch and Placzek 2009, von Wachter, Song, and Manchester 2009). A growing literature also shows that displaced workers fare worse in a number of other outcomes, only part of which are directly related to reductions in earnings, such as increases in mortality (Sullivan and von Wachter 2009), losses in health insurance (Olson 1992, Simon 2005), increased job instability (Stevens 1997), and losses in consumption (Gruber 1997).

Less is known about the mechanism underlying the effects of job displacement. A long-standing hypothesis is that workers lose both specific skills and job-specific match components, as well as non-competitive rents paid by their firm or sector (e.g., Topel 1990, Ehrenberg 1990). The role of industry-specific skills, job-match components, and industry- and employer-specific rents also emerges from recent studies based on firm and industry characteristics (Jacobson, Lalonde, and Sullivan 1993, Couch and Placzek 2009, von Wachter, Song, and Manchester 2009).

Less attention has been paid to whether higher skilled workers fare better after a job displacement. Yet this is an important question both for understanding the incidence and the mechanisms of the cost of job displacement. On the one hand, more educated workers are likely to be more flexible, and may be able to recover lost skills, job match component, or rents more quickly. On the other hand, they may lose access to higher paying jobs that are typically not available to lower skilled workers, and may suffer larger losses. Whether education helps to protect workers from labor market shocks is also a question of more general interest.

However, the existing evidence on the effect of education on the cost of labor market shocks on earnings is ambiguous, and there is even less evidence with respect to other outcomes. The evidence from studies of job loss suggest either similar effects (Farber 2003) or smaller effects for highly educated workers (Stevens 1997). Evidence from labor market entrants suggests that the most highly skilled college graduates suffer short-lived effects from entering in the labor market in a recession, whereas lower skilled college graduates suffer lasting effects (Oreopoulos, von Wachter, and Heisz 2008). High-school graduates do not suffer persistent losses from starting to work in difficult times (Kondo 2008). More generally, mature higher educated workers suffer lower earnings losses and lower unemployment in recessions (Blanchflower and Oswald 1994). We also know that more educated workers are more geographically mobile. On the other hand, they may be more able to accumulate specific skills (Neal 1998) and have access to potentially hard-to-obtain higher paying jobs

(Gibbons, Katz, Lemieux, and Parent 2005).

Part of the difficulty in studying the short- and long-term effect of job loss by education on earnings or other outcomes has arisen due to a lack of appropriate data. The dynamic effects of job displacement are typically measured using longitudinal administrative data. The key advantage of these data sets are long time series, large samples, the availability of a control group of non-displaced workers, and information on displacement events at the employer level (such as a mass-layoff or plant closing). However, these data sources typically lack detailed information on worker demographics, occupation, or education. As a result, studies based on administrative data can neither control for detailed worker characteristics, nor assess the effects of job loss by worker skill. Unfortunately, survey data is only of partial help due to short time series, small samples, and recall error in job displacement and earnings.

To measure the short- and long-term effect of job displacement on earnings and other outcomes for workers of differing educational backgrounds, we work with a matched data source that provides a "bridge" between the benefits of survey and administrative data. The data consists of a merge of quarterly earnings records from California's Unemployment Insurance Base Wage (UI-BW) file for the 1990s with individual responses to the March Current Population Survey (CPS) Demographic Supplement. The matched sample contains information both on worker characteristics such as occupation and education as well as longitudinal information on quarterly earnings and workers' employers. This data set allows us to address some of the gaps in the previous literature, since it has larger samples and better earnings and employer information than typical survey data, and also more detailed information on worker characteristics than typical administrative data. In addition, for those workers who were displaced before the date of their survey interview, the matched data contains information on outcomes such as health insurance coverage and home ownership as of the survey date.

Using the matched data, we make the following contributions to the literature. First, we assess whether the short- and long-term effects of job displacement on earnings differ by educational background and occupation. To the best of our knowledge, this is the first analysis of the effect of job loss by education and occupation using administrative data. Second, we study the effects of displacement on an outcome not previously studied in the literature: the incidence of home ownership. Home ownership is particularly interesting because it should not respond to temporary earnings fluctuations. We also analyze the response of health insurance coverage and unemployment insurance receipt, and assess whether education exhibits a protective effect. Third, we use our findings to assess whether the absence of education and occupation affects estimates of existing studies based on administrative data without this information. We also assess other potential criticisms of the use of adminis-

trative data to study job loss, such as potentially arbitrary definitions of job loss. Finally, our study analyzes the effects of job losses for workers with relatively short job tenure in a booming economy. By studying this "best case" scenario, we complement existing studies focusing on higher tenured workers displaced in more sluggish labor markets (e.g., Jacobson, Lalonde, and Sullivan 1993, Schoeni and Dardia 2003, Kodryczki 2007, Couch and Placzek 2009).

Our findings draw a complex picture of the role of education in affecting the cost of job loss. Workers who have completed 4 or more years of college have substantially lower and shorter lived earnings losses after job loss than average. Workers with some college or a high-school degree suffer larger and more lasting earnings losses, while workers without a high-school degree suffer lower losses. Thus, the cost of job loss appears hump-shaped in education. A similar pattern emerges from our analysis of the cost of job loss by occupation. This is consistent with the notion that the most highly educated are protected against labor market shocks, perhaps because they have more general skills or because they are more flexible. Similarly, it appears that middle-educated workers lose access to better-than-average jobs at a displacement. The lowest skilled may have lower earnings losses because they never had access to these jobs.

We also find that workers who have been displaced tend to show a lower rate of home ownership than never-displaced workers with comparable characteristics. If long-term investments such as buying a house are more affected by the level of long-term earnings rather than short term earnings fluctuations, this suggests that job loss substantially affects workers' life-time choice sets. However, it may also be consistent with short-term credit constraints. We also find that displaced workers have lower rates of health insurance coverage, an effect substantially stronger for less educated workers.

Finally, our findings of substantial earnings losses within education and occupation groups suggests that other studies based on administrative data are unlikely to be substantially confounded by differential group-specific trends, despite the absence of such education and occupation information in these data. We also find that our estimates are very robust to different definitions of mass-layoff. Overall, our analysis of job loss of workers with shorter average job tenure in the booming California labor market confirms that job displacement can be very costly even in favorable economic circumstances.

The rest of the paper is structured as follows. The second section presents the data and discusses our statistical approach. The third section presents the basic estimates of the cost of job displacement in California in the 1990s and some basic sensitivity analysis. The fourth section discusses our analysis by education, occupation, and other groups. The fifth section summarizes the analysis of other outcomes. The final section offers a preliminary conclusion.

2 Data and Estimation Approach

2.1 Data

To measure the cost of job loss and patterns of recovery for workers by their characteristics, we created a matched sample using data from the California UI-BW files and the March CPS. The California UI-BW is a similar file to those used by Jacobson, Lalonde and Sullivan (1993), Schoeni and Dardia (2003), and Couch and Placzek (2009). The file contains longitudinal information on workers' quarterly earnings and employer sizes from the second half of 1991 to 2000. We merge this data with information from individuals residing in California who were interviewed in the March CPS from 1991 to 2000 using respondents' Social Security Numbers. Our sample consists of the subset of respondents to the March CPS in California who reported a valid Social Security Number which appeared in the California UI-BW file.

The presence of a state employer identification number (SEIN) for each job spell in the UI-BW allows us to date job separations. Since the cause of job separations is not recorded in the UI-BW file, we follow Jacobson, Lalonde and Sullivan (1993) and declare a job separation a displacement if a worker separates from a 'stable job' while their firm is experiencing a 'mass-layoff'. We follow Jacobson, LaLonde and Sullivan in identifying 'mass-layoffs' as those separations that occur when the employer's employment in the year following the separation is 30 percent or more below the maximum level at the beginning of the time period of study. While some workers will be voluntary leavers in such a 'mass layoff,' the majority would have involuntary leavers. This intuition is confirmed by tabulations from the JOLTS showing that in situations of a 30 percent or greater layoff, the fraction of quits among leavers is small (Davis, Faberman, and Haltiwanger 2006). Yet, such a definition using a fixed threshold must remain arbitrary. To directly assess the robustness of our estimates with respect to this assumption, we examine the results of defining 'mass layoff' below, including an examination of defining mass layoffs only where the employer closes completely (a 'plant closing').[2]

In this work, we define a 'stable job' as a job that lasts at least six quarters or at least 16 quarters. This leads us to choose either the first quarter of 1993 or the second quarter of 1995 as the first possible date of job separation. The advantage of the earlier separation window is that it allows us to observe some job losses during a recessionary period. While it is likely that the California economy was recovering faster than the rest of the US by 1993, there are likely to still be some jobs lost due to recessionary effects in some industries. The advantage of the later separation window is that it allows us to identify more long-term job

[2]Plant closing in the UI-BW was defined as the SE IN (State Employer Identification Number) ceasing to exist (and not returning).

holders in a more analogous fashion to Jacobson, Lalonde and Sullivan (1993), who required at least 6 years of pre-displacement tenure in their work..

Further sample restrictions we impose are also similar to Jacobson, Lalonde, and Sullivan (1993). First, firms with less than 50 employees in the first quarter were removed, as a change of only a few employees might otherwise be coded as a 'mass layoff' from these firms. 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).[3] Third, for multiple job holders, we only concentrated on the primary (highest paying) job.[4] Table 1 provides basic sample statistics of the matched sample. Further data description can be found in von Wachter, Handwerker, and Hildreth (2008).

2.2 Methodology

Our main statistical model allows us to measure the effect of job displacement before and after the job loss relative to a control group of non-displaced workers. This approach is taken from Jacobson, Lalonde, and Sullivan (1993), and has become standard in the analysis of job displacement (Schoeni and Dardia 2002, Couch and Placzek 2009, von Wachter, Song, and Manchester 2009). We implement the model separately for different demographic, education, or occupation groups. To see what this modification entails to the original approach, we can write our main estimation equation as

$$w_{ijt} = \theta_i + \gamma_{jt} + \sum_{i=-16}^{16} \delta_j^k D_{ijt}^k + u_{ijt} \tag{1}$$

where i indexes individuals, j indexes groups, and t indexes quarters. The dependent variable (w_{ijt}) is the quarterly real earnings for an individual of a given group. The θ_i are worker specific fixed effects that capture the impact of permanent differences in workers in their observed and unobserved characteristics. The γ_{jt} are group-specific quarter time dummies. The error term (u_{ijt}) is assumed to have constant variance and to be uncorrelated across individuals and time. The dummy variables $D_{ijt}^k, k = -16, -15, ..., 0, 1, 2, ...,$ jointly represent the event of displacement and time periods before and after displacement. Thus, estimates for the coefficients δ_j^k will not only indicate the earnings change at the time of displacement

[3]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.

[4]This sample is different from Schoeni and Dardia (2003) 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 group j, but will also show the effect of displacement k time periods before and after displacement.[5]

Given the presence of the displacement-dummies D^k_{ijt}, the group-specific year-effects γ_{jt} capture the regular evolution of earnings for non-displaced workers in similar skill or demographic groups. In the absence of information on worker skills or demographics, the cost of job displacement is typically measured relative to the regular evolution of all non-displaced workers γ_t. This approach may over-state the cost of job displacement if the group-specific earnings trend is declining. Conversely, it may under-state the cost of job displacement if the trend is increasing. By estimating our model separately by education or occupation groups, we effectively control for education- or occupation-specific earnings trends that most other studies would assign to the cost of job displacement.

To see this explicitly, consider the following standard data generating process (DGP) for earnings:

$$w_{ijt} = \alpha_i + \gamma_{tj} + m_{ij} + \psi_j + \varepsilon_{ijt} \tag{2}$$

where α_i reflects fixed worker skills, γ_{jt} captures group-specific earnings trends, m_{ij} are job-specific components of wages (rewards for good matches, or rents), and ψ_j are industry or occupation or education level wage components. This data generating process implies that the earnings change at a random displacement is

$$\Delta w_{it} = \Delta\gamma_{tj} + \Delta m_{ij} + \Delta\psi_j + \Delta\varepsilon_{ijt}. \tag{3}$$

Ignoring changes in idiosyncratic components of wages, the cost of displacement would be the loss in any job-match component (Δm_{ij}), such as industry- or firm-specific skills, plus the loss of a group-specific component ($\Delta\psi_j$), such as industry-specific non-competitive rents. The regular evolution of wages that would occur in the absence of displacement ($\Delta\gamma_{tj}$) should not be counted as part of the cost of job loss. Such differential trends in earnings by groups can arise because of different evolution of earnings within industries, but also within education or occupation groups. Differences in trends can also arise if firms lay off workers who on average have lower earnings growth.

Ideally, to control for differential earnings trends we would use information on the regular evolution of worker productivity, but this is typically difficult to measure. To address

[5]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.

this problem, Jacobson, Lalonde, and Sullivan (1993) introduced worker-specific linear time trends. Von Wachter, Song, and Manchester (2009) have included separate year-effects for industries and earnings-classes. Yet, while making important progress in addressing the problem of differential trends, these approaches may be too coarse. In particular, a large literature on human capital earnings functions shows that earnings evolve separately for different education groups even within industry (e.g., Katz and Autor 1999). Similarly, more recently it has been shown that occupation may be a better measure of worker skill than industry (e.g., Autor, Levy, and Murnane 2003, Autor and Dorn 2009).

Studies based on survey data confirm that differences in the incidence and effect of job loss by education and occupation groups can indeed be significant (Farber 2003, Stevens 1997). Thus, the absence of information on education and occupation is a potentially important drawback of analysis of job loss based administrative data. Here, we can exploit access to our matched sample between CA UI records and the March CPS to directly estimate the cost of job loss by education and occupation groups.

3 The Cost of Job Loss in California in the 1990s

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 results in Jacobson, Lalonde, and Sullivan (1993) [JLS], Schoeni and Dardia (2003), or Couch and Placzek (1999). Workers experience an initial earnings loss at job loss of about 15-20 percent, and this loss is still evident four years after the event. Thus, even in an increasingly tight labor market, the cost of job displacement can be substantial. However, the recovery is clearly more pronounced than in some earlier studies. Thus, the average worker does not appear to experience permanent effects of displacement in our data. As we will see below, the average hides substantial differences across subgroups of workers.

An advantage of using administrative data is that we can examine the effect of alternative specification choices on the estimated incidence and cost of job loss. Thus, to assess the robustness of our estimates, we briefly summarize here an extended sensitivity analysis, described in more detail in a longer working paper (Hildreth, von Wachter, and Handwerker 2008). First, we varied the definition of 'distressed employer' in various ways; we required the 30% drop to occur in the year the worker leaves the firm, we considered 60% gradual and instant declines, and we analyzed plant closings. Overall, we see that there are some differences in the estimated displacement rate and earnings loss depending on the definition

of job loss.[6] However, the differences are rather gradual in nature, such that the alternative definitions of mass-layoff are likely to capture a similar underlying experience for affected workers. The exception is plant closing, which shows significantly larger losses, especially once a control group of non-displaced workers is included. Since smaller firms are more likely to close or to suffer sudden, large percentage declines in employment researchers have to be aware that choosing an alternative definition of a 'distressed employer' is likely to change the background of displaced workers.

We also varied the amount of required job tenure prior to displacement. Requiring a minimal amount of pre-displacement tenure is important to isolate workers laid-off from their stable job from other forms of job mobility when no other information on the reason for job change is available (Von Wachter, Handwerker, and Hildreth 2008). However, using 6 quarters to define a 'stable job' is necessarily arbitrary, and dictated by data availability. Thus, we also considered workers displaced after 16 quarters of job tenure in (beginning in 1995.2). Since these workers faced a very different economy than those displaced beginning in 1992.4, we also considered workers displaced with 6 quarters of job tenure beginning in 1995.2. The results suggest that holding the amount of job tenure constant, being displaced at the beginning of a boom leads to significant but shorter lasting costs of job loss. This is not true for higher tenured workers; they suffer much longer lasting losses, even if displaced in a boom economy. We also find that including worker fixed effects makes a bigger difference for shorter tenured workers displaced in a boom. This confirms that alternative definitions of 'displacement' in the UI-BW will affect different types of workers (see, e.g., von Wachter, Handwerker, and Hildreth 2008).

For the purposes of this paper, these results 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 work with a six quarter tenure restriction.

4 Heterogeneity in the Effect of Job Loss

4.1 Differences by Education Groups

Studies based on administrative data typically cannot analyze differences in the effect of job loss by education or occupation groups or control for education or occupation-specific

[6] The figures of the full dynamics of job losses for other specifications are shown in our longer working paper.

earnings trends. On the other hand, studies based on survey data (e.g., Farber 2008) cannot look separately at the patterns of declining earnings before displacement (the "dip"), falling earnings at the time of displacement (the "drop"), and increases in earnings after these drops (the "recovery"), by skill-groups. Here, the match between administrative and survey data allows us to both control for flexible trends for education and occupation groups and study differences in the dynamic pattern of earnings changes around job loss for differing education and occupational groups.

Figure 2 shows the resulting estimates of the cost of job loss for four education groups. The figure clearly displays a differential pattern by group. While workers with a college degree suffer small and short lasting effects of job loss, the effects for workers with some college or a high school degree are strong and long lasting even in the booming CA labor market. These patterns are all the more striking given that the minimum tenure required before displacement for these workers is only one and a half years (in contrast with the more typical six years in the literature). The long-term earnings losses for workers without a high school degree is persistent but somewhat smaller.

The pattern emerging from Figure 2 is that high-skilled job losers recover quickly and low-skilled workers' losses are lasting but moderate, while the largest and more persistent losses are borne by workers in the middle of the skill distribution. This is consistent with empirical studies of the earnings effect of entering the labor market in a recession. Oyer (2008) and Kahn (2006) find that college graduates suffer persistent earnings reductions; Oreopoulos, von Wachter, and Heisz (2008) show that these reductions are short-lived for top college graduates, while college graduates at the bottom of distribution suffer permanent earnings reductions. Similarly, Kondo (2008) shows that the effect of labor market entry in a recession for high-school graduates is detrimental but temporary.

A plausible interpretation of these findings is that high-skilled workers move more quickly to jobs comparable to what they held before displacement (Oreopoulos, von Wachter, and Heisz 2008). They might also have more general skills (e.g., Katz and Autor 1999), allowing them to better adapt to changing requirements, and may be more able to re-accumulate any lost industry, occupation, or firm specific skills (Neal 1998). Middle skilled workers on the other hand, may lose access to career jobs or jobs at better firms. They also may have had more job specific skills that are hard to transfer to the post-displacement job (Neal 1995). Low-skilled workers may lose less because they never had access to high-paying or career-type jobs, and may have accumulated less specific skills.

Our findings on the short and longer term costs of job displacement by education groups complement mixed results based on survey data. On the one hand, estimates based on the Displaced Worker Survey (DWS) reported in Farber (2003) suggest that short-term earnings

losses of highly educated displaced workers are similar (if not larger) than those of middle skilled workers. Yet, these results are based on approximate control groups, and recall bias may mute the difference between education group if workers only report costly job losses (von Wachter, Handwerker, and Hildreth 2008). On the other hand, estimates based on the Panel Study of Income Dynamics (PSID) in Stevens (1997) suggest that both the immediate and the longer term losses are largest for workers with 13-15 years of education. Results reported in Stevens (1997) further suggest these findings are partly driven by differential labor supply responses. However, the hump-shaped battern in the cost of job loss we find here also appears to hold for hourly wages.[7]

4.2 Differences by Occupation Groups

The effects of job loss by occupation group also reflect the differences between skill groups found in Figure 2. However, nuances in the cost of job displacement among lower skilled workers emerge. Figure 3 shows estimates of the cost of job displacement by four major occupation groups. Consistent with the finding for college graduates, workers in management and professional occupations experience comparatively small and relatively short lived effects of job displacement. For workers in more middle-skilled occupations such as technology and sales, we see effects that are initially large and persistent, but fade after four years. For service occupations, the effect is initially large, but fades relatively quickly within two to two and half years. Only for those in production (operatives and laborers) do there appear to be permanent effects. These permanent effects resemble the effect for workers without a high-school degree – they show steep initial losseswith ensuing quick recoveries to levels that appear permanently somewhat below the pre-displacement levels. Yet, it also appears that for these workers, there is a lasting downward trend in earnings that is not fully captured by the evolution of earnings for the control group. This suggests that there might differences across industries, addressed below.

These results largely confirm the pattern we found for high, middle, and low skilled workers. However, the findings also indicate some heterogeneity among lower-skilled occupations that may not be captured by differences in education. Thus, the pattern in Figure 3 suggest that occupation is an additional relevant dimension by which the cost of job loss differs. The differences among occupation and education groups documented in Figures 2 and 3 also suggest that ignoring differential trends in earnings by occupation may confound existing estimates of the cost of job displacement. However, the fact that there are still substantial costs of job displacement within education and occupation groups suggests that typical es-

[7] See Figure 11 in Farber (2003) and Table 5 in Stevens (1997).

timates successfully capture the main pattern of the cost of job loss and are not confounded by strong differential trends.

4.3 Age and Industry

Other studies of job loss based on administrative data (in states other than California) typically have access to basic worker and employer characteristics, such as worker gender and age, or firm industry. Here, we summarize our replication of the analysis of the cost of job loss by these groups, and discuss how these differences are affected by education controls. Figure 4 shows that older workers experience much larger and more permanent earnings losses after a job displacement than other workers. The figure also shows that middle-aged workers recover relatively quickly from their earnings losses, while younger workers appear to suffer permanent losses. These results are consistent with those in Von Wachter, Song, and Manchester (2009) for the entire United States, who also show that recovery among middle-aged is confined to the late 1990s, and does not hold in other periods.

These findings are consistent with a role for human capital accumulation as well as rents. Older workers are likely to have accumulated more specific capital which they are apt to lose upon changing jobs. They are also more likely to have found better job matches or jobs at firms or industries paying non-competitive rents. Moreover, for these workers the short horizon of their remaining careers may imply that the increased effort to re-accumulate skills and search for better jobs may not be worth the life-time payoff. On the other hand, middle-aged workers may find the effort worthwhile. It is harder to explain why younger workers should bear lasting losses from displacement. They may permanently lose the opportunity to accumulate certain skills or access to good jobs. Yet, this argument rests on the presumption that there is a "window of opportunity" for career formation limited to certain age-groups. A more plausible explanation might be that these workers lost better than average jobs, and may have reverted to the mean of the wage-offer distribution after displacement.

Another often-studied dimension of the cost of job displacement is industry. As shown in Figure 5 and discussed in more detail elsewhere (e.g., JLS, Schoeni and Dardia 2003), losses tend to be larger and very persistent for workers displaced from the manufacturing sector. Yet, losses are also substantial and persistent for workers displaced from wholesale and retail trade. Workers from transportation, communication and utilities tend to recover relatively quickly. Consistent with the results for service occupation, losses appear quite small and short lasting in the service sector. While the sectoral differences may be as expected, it is clear that potentially important differences by education levels or occupation groups will be averaged over at the industry level.

4.4 Differences by Marital Status, Gender, and Race

We also calculate similar estimates by gender, race, marital status, and previous job tenure. To better summarize the findings for various groups, and also estimate a model including all group-level interactions, we follow JLS and re-parametrize our main model to yield separate estimates for the dip in earnings prior to displacement, the drop in earnings at displacement, and the ensuing pattern of recovery. We then estimate our displacement regression with separate group-interactions (this analysis corresponds to that of the figures). We also estimate a pooled specification with multiple interactions. Yet, because of the small sample size of our matched sample, many interactions are statistically insignificant.

The results of this analysis are summarized in Table 2. The table confirms that education and age are the most precisely estimated interactions. They are also most robust to the inclusion of additional interactions. Interestingly, the effect of displacement for young workers declines somewhat when education-interactions are included. This suggests that some of the persistent effect for young job losers observed in the literature derives from the fact that younger job losers tend to have lower education (especially once we impose a minimal amount job tenure at the lost job). The results in Table 2 also confirm the relative magnitudes for industry and occupation groups, although only the smaller losses for managerial and professional occupations and for service sector occupations are statistically significant at conventional confidence levels.

Difference among gender and race are too imprecisely estimated to yield a clear picture. Judging from the dynamic pattern (not shown), the effects are slightly larger for men, but similar in overall magnitude. Earnings losses tend to be lowest for whites, appear increasing for blacks, and are initially large but shorter lived for Hispanics.

The one additional difference that is significant and is robust to the inclusion of additional controls is the differential effect of displacement by marital status. Married individuals tend to have larger earnings losses, holding differences in gender, age, job tenure, occupation, or education constant. While marital status is likely to be correlated with other attributes, typically we would expect an omitted variable bias to be positive; for example, the earnings premium for marital status is positive for men. The fact that the effect is negative and robust to obvious correlates such as age and education suggests that marital status has an independent effect on the cost of job loss. This would be the case if married workers are less mobile geographically, perhaps because of spousal employment or because they are more likely to have children. This finding is consistent with a significant role of age in the cost of job loss. Both older and married workers are less likely to be mobile, and are thus more likely to suffer from the effects of displacement.

Overall, our analysis of sub-groups suggests that there is considerable heterogeneity in

the cost of job displacement. Education, occupation, and age emerge as relevant and robust dimensions. Thus, a promising explanation of the differential cost of job displacement would be differences by skill in the ability of workers to adjust to new work environments and to search for better jobs. The role of marital status indicates that mobility costs may matter, but age may play a role in itself by affecting the relevant time horizon over which skills are accumulated.

5 Medium-term outcomes of displacement

5.1 Baseline Estimates

The findings thus far suggest that some groups of workers suffer substantial earnings losses after a job displacement, even with low minimum pre-displacement job tenure and during a booming labor market. This complements the existing literature showing that larger and more persistent losses can occur for higher tenured workers in more difficult economic times. Yet, despite an increasing literature suggesting that outcomes other than earnings are affected by job loss, very little is known about changing lifetime circumstances for job losers. We are especially interested in medium to longer term outcomes that persist after the dust from the initial shock and adjustment has settled and workers have returned to regular employment.

Typically there is no information on such additional outcomes in administrative data and samples in survey data are often too small to study them. Here, we use our matched sample to study the effects of job displacement on three additional outcomes; whether individuals own a home, whether they have private health insurance, and whether they are currently receiving benefits from unemployment insurance (UI). The decision to own a home in particular should be affected by workers' long-term income expectations, not by short-term fluctuations in income. If home ownership responds to job loss, it is either because workers believe their long-term earnings capacity is diminished, or because their short-term earnings situation affects their ability to obtain credit. A similar argument can be made with respect to private health insurance. The incidence of UI benefits measures the initial effect of displacement on unemployment, as well as any longer term effect of job loss on employment stability (Stevens 1997).[8]

Our data allows us to regress the incidence of these three outcomes on whether a worker experienced a job displacement at some point in the previous few years. The incidence of job displacement and all other sample restrictions are defined as in the foregoing analysis.

[8] We also examined the incidence of divorce and the number of young chilfred in the household, and found no significant impact of displacement on these outcomes.

The resulting estimates average over the short, medium, and longer term impact of job displacement. Unfortunately, we cannot trace out the dynamic effect of job loss in this context, due to small samples. However, as just discussed, the effect of job loss on home ownership would not be expected to vary with time since displacement, at least in the absence of credit constraints. We know from other literature that the effect of displacement on access to private health insurance tends to fade, but is persistent. Similarly, job instability increases for up to ten years after displacement (von Wachter, Song, and Manchester 2009). Finally, only a minority of our matched observations in the March CPS fall close to a displacement date. Thus, our estimates are likely to be at least indicative of a longer-term pattern.

Our findings suggest a substantial effect of job displacement on these lifetime outcomes. We find that displaced workers are less likely to own a home and to have private health insurance. We also find an increased incidence of UI benefit receipt. These results are shown in Table 3. The table shows the difference in the outcomes for displaced workers relative to non-displaced workers, controlling for gender, race, and three education dummies. The table shows the results for our main measure of displacement, as well as for plant closing. Since, as discussed in Section 3, losing a job during a plant closing has a larger effect on earnings in the CA economy of the late 1990s, this is helpful here given our limited samples. To gauge the magnitude of the effect, the table also shows the average incidence of the outcome in CA tabulated directly from the March CPS.

For all displaced workers, the reduction in the rate of home ownership is between 4.1 (mass-layoffs) and 8.3 (plant closing) percentage points. From an average of 60% of home ownership in California in the 1990s, the implied percentage effect ranges from 7% to 14%. The larger effect for plant closing is likely to be due to stronger earnings losses (the overall earnings loss is about 50% bigger for plant closing than for mass-layoffs). Despite the controls for demographics and education, it may also be due in part to a lower rate of home ownership among workers employed by firms more likely to close (e.g., smaller employers are more likely to close).

These results suggest that job loss affects circumstances whose choice is typically associated with long-term outcomes, such as home ownership. While access to credit in the short-run is likely to be limited due to unemployment, most unemployment spells are short. Thus, it is unlikely that credit constraints alone explain this finding. The effect is consistent with the fact that lasting earnings declines affect the long-term choice set of displaced workers. However, the implied elasticity of home ownership with respect to long-term earnings is substantial; for mass-layoffs, based on a coarse measure of long-term earnings reductions it is around 67% (simple before-after comparison without distinction between short- and long-term effects yields an earnings effect of displacement of about 10%, see Table 1, von

Wachter, Weber, and Handwerker 2008). The correct elasticity is likely to be somewhat larger, since the percentage loss in the present discounted value of earnings is likely to be smaller for this sample. This appears too large for the entire effect to be explained by losses in earnings alone.

Our measure of the effect of displacement on health insurance is 4.5 and 9.15 for mass-layoffs and plant closings, respectively. Relative to a baseline of about 63%, this again implies percentage effects between 7% and 15%. Again, part of the difference is likely to be due to differences in the effect of job loss, and part may be due to pre-existing differences not eliminated by our demographic and education controls. These effects are smaller than those reported by Olson (1992) in the Displaced Worker Survey for the 1980s, but they are similar to findings reported in Simon (2005) using the Survey of Income and Program Participation. Part of the difference in health insurance between displaced and non-displaced workers appears to fade after two to three years while workers are on the job (Simon 2005).

The decline in the incidence in health insurance is not surprising if it is simply due to workers re-optimizing their consumption choices in the face of lower earnings. Since health insurance is typically provided through employers, this would suggest we observe that jobs without health insurance should pay higher wages. However, there is no evidence in these studies that workers taking jobs without health insurance receive higher pay. Rather, jobs providing health insurance are also more likely to be higher paid. Thus, it appears that the loss in private health insurance adds to the loss in long-term earnings.

Finally, Table 3 also displays the effect of displacement on the incidence of UI benefits. Interestingly, the rise in the incidence of UI benefits is 4.6 percentage points for mass-layoffs, but an insignificant 2.1 points for plant closing. Relative to the benchmark of 4.5 percent in the California population, this suggests a large increase in take up of unemployment insurance in the population of displaced workers. However, the right benchmark is the incidence among stable workers in larger firms, the group of workers we focus on here, and these workers have a low probability of receiving UI. Thus, the effect we measure can be interpreted as effect of the persistent increase in the incidence of unemployment following a displacement (Stevens 1997, von Wachter et al. 2009).

5.2 Estimates by Education Groups

We extend our analysis of the effect of job loss on home ownership, health insurance, and unemployment insurance take up to the effects for education subgroups. To maximize sample size, we consoludate our education categories to two: workers with twelve years of schooling or less and workers with some college or more. **As shown in Table 4**, the effect of job loss

on these outcomes is generally stronger for less educated workers, in particular for health insurance. The percent reduction in health insurance relative to the baseline average is about 16% for workers with high school or less who lost their job during a mass layoff (35% for those displaced during a plant closing). The effect is only about 4% for workers with some college or more. This is consistent with findings in the literature that workers who experience larger earnings losses are also more likely to lose coverage by health insurance.

Perhaps not surprisingly, the effect of displacement on the incidence of UI benefits is also larger for lower educated workers, although the difference is not as stark. In contrast, the percentage effect of job loss on home ownership is similar across education groups. The results suggest the percentage drop is about 7% (mass-layoffs) or 14% (plant closings), irrespective of education group. This might be explained in part by the inclusion of workers with some college, who we find have lasting earnings losses, in the higher-educated group with workers who had four or more years of college, and who we find have smaller earnings losses. However, the contrasting effect of education on home ownership and health insurance suggests that education does correlate with other job characteristics such as health insurance.

Overall, these findings indicate that education protects workers to some extent from the adverse effects of job loss on both earnings and later job characteristics. However, education does not appear to play such a role for home ownership. In particular, while the most highly educated avoid lasting adverse earnings effects, those with some college or a high-school degree suffer considerable and persistent earnings losses. Part of the larger earnings losses are mitigated by a higher take-up rate of unemployment insurance. However, the higher incidence of unemployment insurance take-up we find is more likely to be a sign of persistent difficulties in finding another stable job, rather than an income stream that allows these workers to close the gap in earnings.

6 Summary and Discussion

This paper has analyzed the effect of job displacement during a mass-layoff in California during the 1990s using a match between administrative wage records and survey information from the March Current Population Survey (CPS). Our results confirm that even job displacements in the booming California labor market were costly for affected workers. Besides earnings losses lasting four or more years, these workers also experienced losses in health insurance coverage and home ownership over the medium-term. Yet, in contrast to studies of the effect of job displacement in slack labor markets such as Jacobson, Lalonde, and Sullivan (1993), the average displaced worker in our sample eventually recovers from the initial earnings loss.

The availability of the match between survey and administrative data allows us to study the differences in the effect of job loss by worker skill and other characteristics. Our results indicate that the average effect masks important differences in the cost of job loss across the population. In particular, the effect of job loss on earnings appears to be hump-shaped in education. While college graduates suffer lower and shorter lasting earnings losses from displacement, middle-educated workers (with a high-school degree or some college) suffer larger and more persistent effects. Workers without a high-school degree lie in between. This same pattern also appears by occupation groups, with managerial and professional occupation experiencing substantially shorter effects of job loss than laborers and sales and technical occupations, with service occupations lying in between.

Thus, while a college degree clearly buffers the effect of job loss, more education does not protect workers in the middle of the skill distribution. These pattern are consistent with the notion that highly skilled workers are better at adjusting because of more general skills, the ability to re-accumulate skills faster, or because they have a comparative advantage in job search. It is also consistent with the view that workers in the middle of the skill distribution may permanently lose access to scarce high-quality jobs at displacement.

Differences in education can also explain some of the persistent effect of job loss for younger workers found in the previous literature. Age also appears to have an independent effect on the cost of job loss. In part, this may be explained by a reduction in geographic mobility with age, consistent with the robust negative effect we find for marital status on the effect of job loss. Yet, age appears to have an affect beyond this coarse control for mobility.

Using information from the matched sample, we have also analyzed the effect of job displacement on other lifetime outcomes. Our paper is the first study to document how job loss reduces the incidence of home ownership. This is consistent with a reduction in life-time resources, but our findings appear to be too large to be explained by earnings alone. We also confirm findings from previous research that job loss reduces the incidence on access to health insurance, a pattern particularly pronounced for lower skilled workers.

Finally, we have discussed the implications of our findings for studies based on administrative data that do not have access to information on worker education or occupation. Given the heterogeneity we find, existing studies measure an average of differential treatment effects. However, they are unlikely to be confounded by differential earnings trends among displaced workers by education or occupation groups.

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Table 1: Sample Characteristics for Matched UI-BW and March Current Population Survey, California 1990-1999

Variable	N	Mean	Std Dev
5% Subsample of the California UI-BW, used in Figure 1			
Observations at the person-quarter level			
firm size in leave year is 30% below earlyavg	6,388,192	0.2651	0.4414
plant closed in leave year	6,388,192	0.0396	0.1949
wage	6,232,619	$10,541	$30,113
Observations at the person level			
wage in 1991 Quarter 3	187888	$8,881	$13,405
wage in 1999 Quarter 4	175190	$13,237	$80,684
firm size in leave year is 30% below earlyavg	187888	0.2651	0.4414
plant closed in leave year	187888	0.0959	0.2945
Full Sample Matched between the March CPS and the California UI-BW			
Observations at the person-quarter level			
firm size in leave year is 30% below earlyavg	220,592	0.2693	0.4436
plant closed in leave year	220,592	0.0382	0.1917
wage	215,715	$10,512	$19,297
Observations at the person level			
wage in 1991 Quarter 3	6488	$8,966	$7,444
wage in 1999 Quarter 4	6111	$12,725	$28,618
firm size in leave year is 30% below earlyavg	6488	0.2693	0.4436
plant closed in leave year	6488	0.0382	0.1918
Subsample without missing variables, used in regressions shown in Table 4			
Observations at the person level			
age	5464	40.7187	9.1989
Black	5464	0.0456	0.2086
Hispanic	5464	0.3367	0.4726
Asian	5464	0.0950	0.2932
female	5464	0.4510	0.4976
4 category education	5464	2.8029	1.0288
wage in 1991 Quarter 3	5464	$8,860	$7,641
wage in 1999 Quarter 4	5216	$12,796	$30,283
firm size in leave year is 30% below earlyavg	5464	0.1323	0.3389
plant closed in leave year	5464	0.0245	0.1547

Notes: See text for description of sample restrictions.

Table 2: Dynamic Effect of Job Displacement on Earnings by Worker Characteristics

	Separate Regressions			Interacted Regression		
	Dip	**Drop**	**Recovery**	**Dip**	**Drop**	**Recovery**
Main Effect				87.626	-1739.87	-30.7059
				(180.93)	(1727.3)	(142.74)
Main Effect Men	-18.7	-839.7	26.3			
	(56.8)	(542.3)	(44.9)			
Difference for Women	22.1	770.6	84.8	34.6728	926.019	105.783
	(74.8)	(714.5)	(59.1)	(77.407)	(740.4)	(61.18)
Main Effect Workers Age 55+	88.5	-1679.8	-44.6			
	(106.7)	(1015.8)	(85.8)			
Difference for Workers Age 35-44	-157	861.3	61.8	-118.309	1116.16	64.5166
	(128.9)	(1231.8)	(103.1)	(132.66)	(1269.5)	(106.02)
Difference for Workers Age 35-44	-86.3	1389	139.8	-68.4313	1512.22	145.343
	(123.2)	(1172.9)	(98.4)	(124.67)	(1187.9)	(99.545)
Difference for Workers Age 35-44	-81.5	2216.7	209.4	-68.9331	2330.75	209.849
	(129.2)	(1230.7)	(103.6)	(129.83)	(1237.2)	(104.15)
Main Effect for Workers With at Least a BA Degree	97.7	660.5	225.9			
	(67.4)	(644.6)	(54.3)			
Difference for Workers with Less Than HS Degree	-172.5	-1316.4	-213.4	-91.3401	-692.583	-184.509
	(115.7)	(1107)	(91.9)	(136.58)	(1305.4)	(108.76)
Difference for Workers with A HS Degree	-139.1	-1556.9	-151.5	-102.552	-1242.36	-108.334
	(103.4)	(985.1)	(81.5)	(107.53)	(1024.7)	(84.934)
Difference for Workers with Some College Education	-115.3	-1327	-196	-102.923	-1306.13	-189.397
	(93.7)	(896.8)	(74.8)	(96.002)	(919.71)	(76.962)
Main Effect for Workers with Less than 1 Year Job Tenure	-82.3	-1002	-5.2			
	(62.7)	(598.4)	(48.7)			
Difference for Workers with More Than 1 Year Tenure	119.3	959.6	137.4	111.317	1130.04	113.816
	(77.6)	(741.2)	(60.9)	(84.603)	(809.03)	(66.379)
Main Effect for Single Workers	27.8	281.6	146.1			
	(65.6)	(628.3)	(51.4)			
Difference for Married Workers	-45.3	-953.2	-94.8	-67.3893	-1300.58	-129.087
	(79.4)	(759.5)	(62.5)	(84.665)	(811.12)	(66.857)

Notes: Regressions include age-controls and year effects. See text for further explanations.

Table 3: Dynamic Effect of Job Displacement on Earnings by Worker Occupation and Industry

	Separate Regressions		
	Dip	**Drop**	**Recovery**
Occupation Prior to Job Displacement			
Main Effect for Operatives and Laborers	-89.3	-953.1	-2.2
	(69.6)	(618.3)	(56.3)
Difference for Managers and Professionals	225.9	1672.1	211.2
	(95.1)	(874.1)	(76.8)
Difference for Technicians and Sales	22.2	-93.9	75.3
	(96.9)	(892.8)	(77.3)
Difference for Service Workers	40.9	450.7	24.8
	(64.2)	(411.7)	(53.8)
Industry Prior to Job Displacement			
Main Effect for All Other Industries	-122.8	-1059.8	38.7
	(115.3)	(1098.3)	(93.2)
Difference for Manufacturing Industries	34.3	-171	-94.7
	(133)	(1266.6)	(107.3)
Difference for Transportation, Communication and Utilities	16.3	385.9	105.1
	(191.2)	(1831.1)	(151.4)
Difference for Trade	166.1	764.4	81.2
	(139)	(1325.5)	(110.6)
Difference for Services	236.6	1981	144.2
	(134.9)	(1287.2)	(108.5)

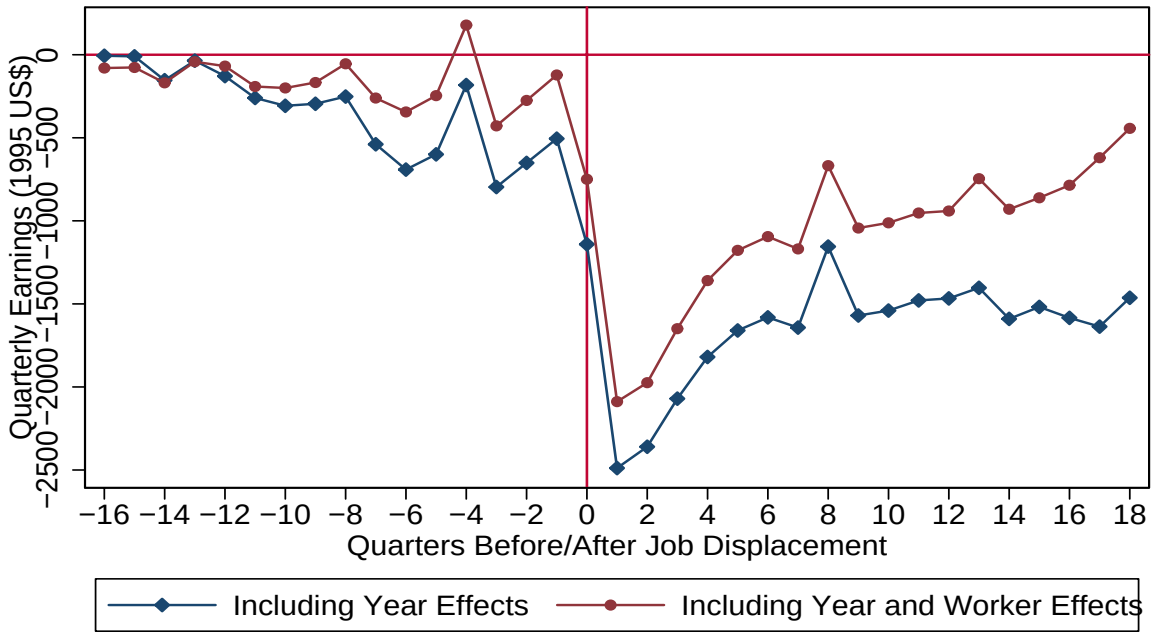
Notes: Regressions include age-controls and year effects. See text for further explanations.

Table 4: Regression Estimates of Displacement on Other Medium-Term Outcomes

		Lives in owner-occupied housing	Has Private Health Insurance	Has income from Unemployment Insurance
Regression Estimates				
	Group			
	Overall	-0.0414	-0.0456	0.0459
Displacement		(0.0185)	(0.0151)	(0.0084)
	High School	-0.0351	-0.0788	0.0599
	Education or Less	(0.0326)	(0.0317)	(0.0165)
	More than High	-0.0516	-0.0314	0.0332
	School Education	(0.0232)	(0.0163)	(0.0094)
Plant Closing	**Overall**	-0.0831	-0.0915	0.0206
		(0.0391)	(0.0330)	(0.0178)
	High School	-0.0824	-0.1671	0.0533
	Education or Less	(0.0659)	(0.0668)	(0.0333)
	More than High	-0.0991	-0.0372	-0.0074
	School Education	(0.0491)	(0.0355)	(0.0200)
Mean values for California in 1991-2000 CPS				
	Overall	0.5938	0.6299	0.0451
		(0.0016)	(0.0016)	(0.0007)
	High School	0.5145	0.4800	0.0530
	Education or Less	(0.0024)	(0.0024)	(0.0011)
	More than High	0.6681	0.7636	0.0391
	School Education	(0.0023)	(0.0021)	(0.0010)

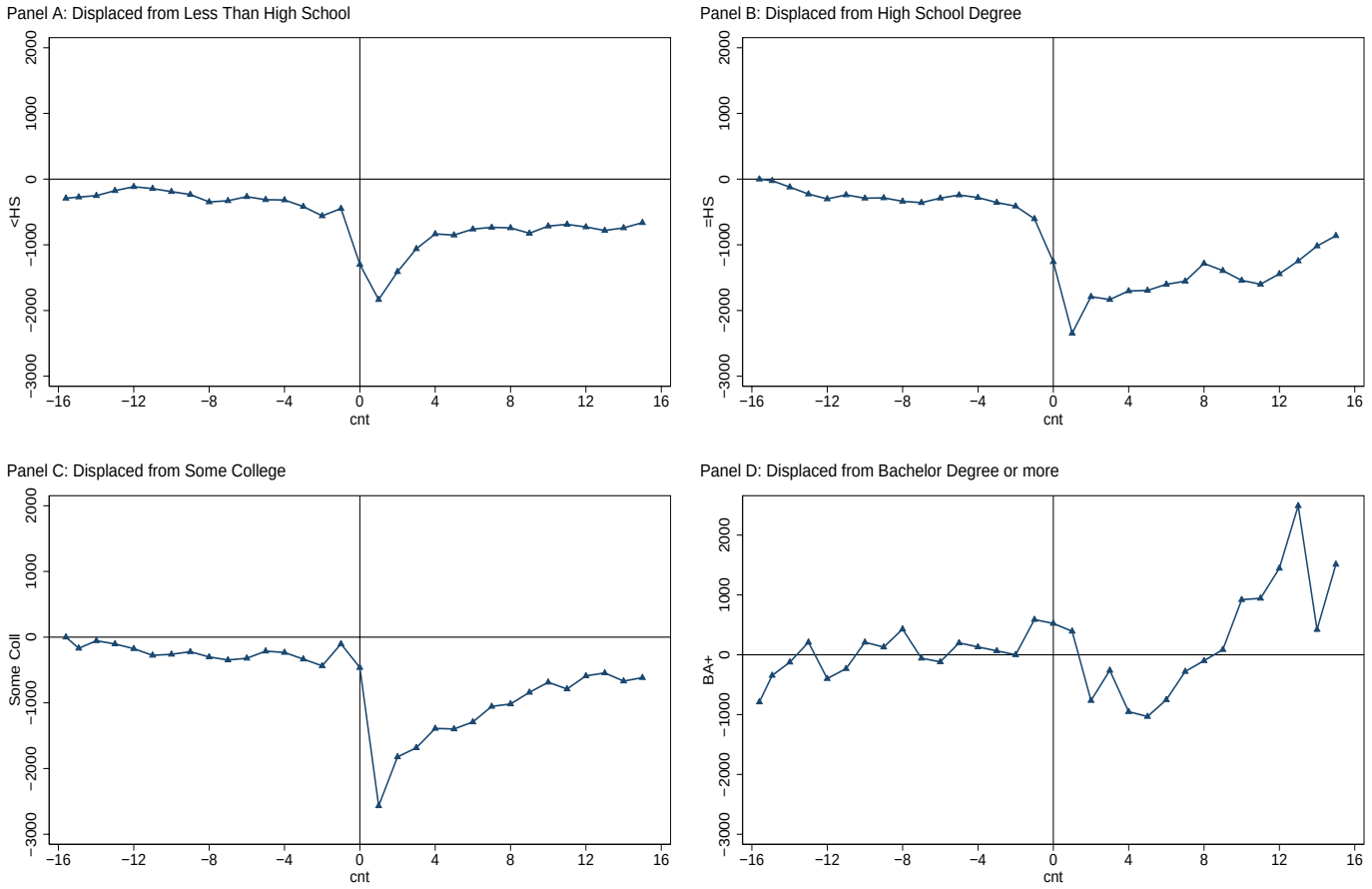
Notes: Coefficients included in each regression but not shown: sex, four race categories, four education categories (when regressions are not split by education).

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



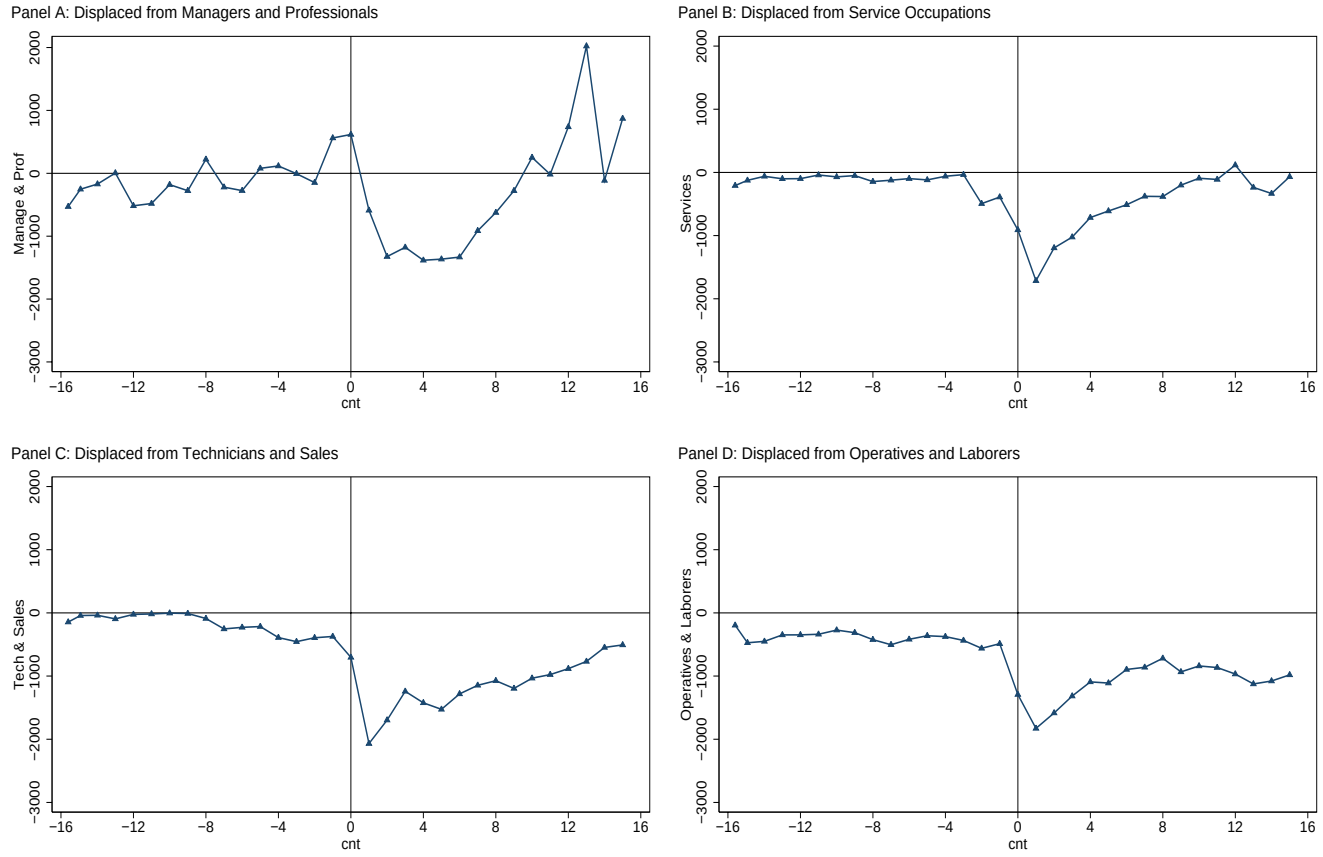
Displacements occuring 1993.1−1999.4. Source: 5% of California UI−BW File, 1990.3−1999.4 (see text).

Figure 2: Earnings Loss at Displacement by Education Group
6 Qrts of Tenure at Job Loss, Displaced 1993.1−1999.4.



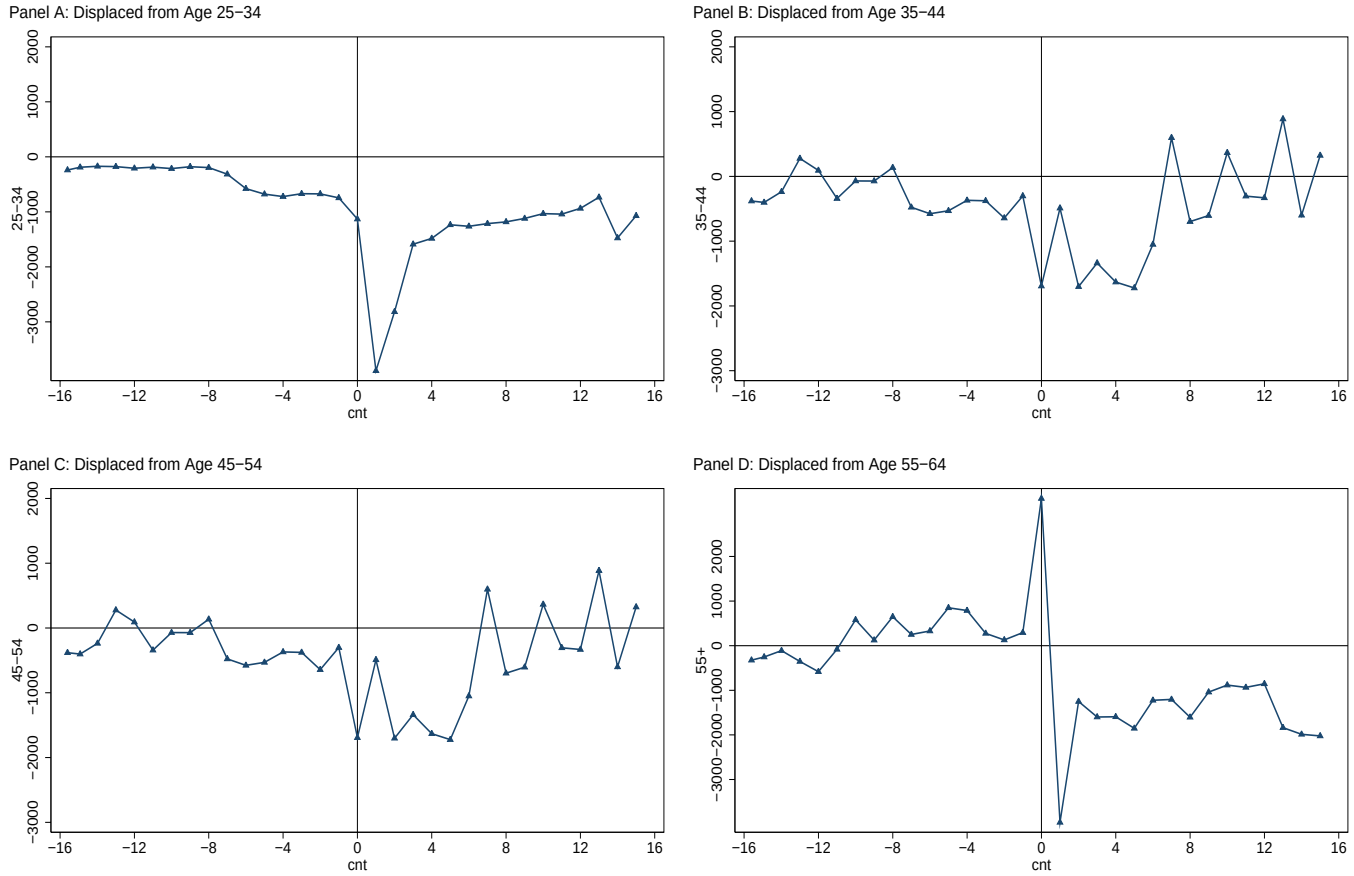
Source: Merged California UI−BW and March CPS Data (see text).

Figure 3: Earnings Loss at Displacement by Major Occupation
6 Qrts of Tenure at Job Loss, Displaced 1993.1−1999.4.



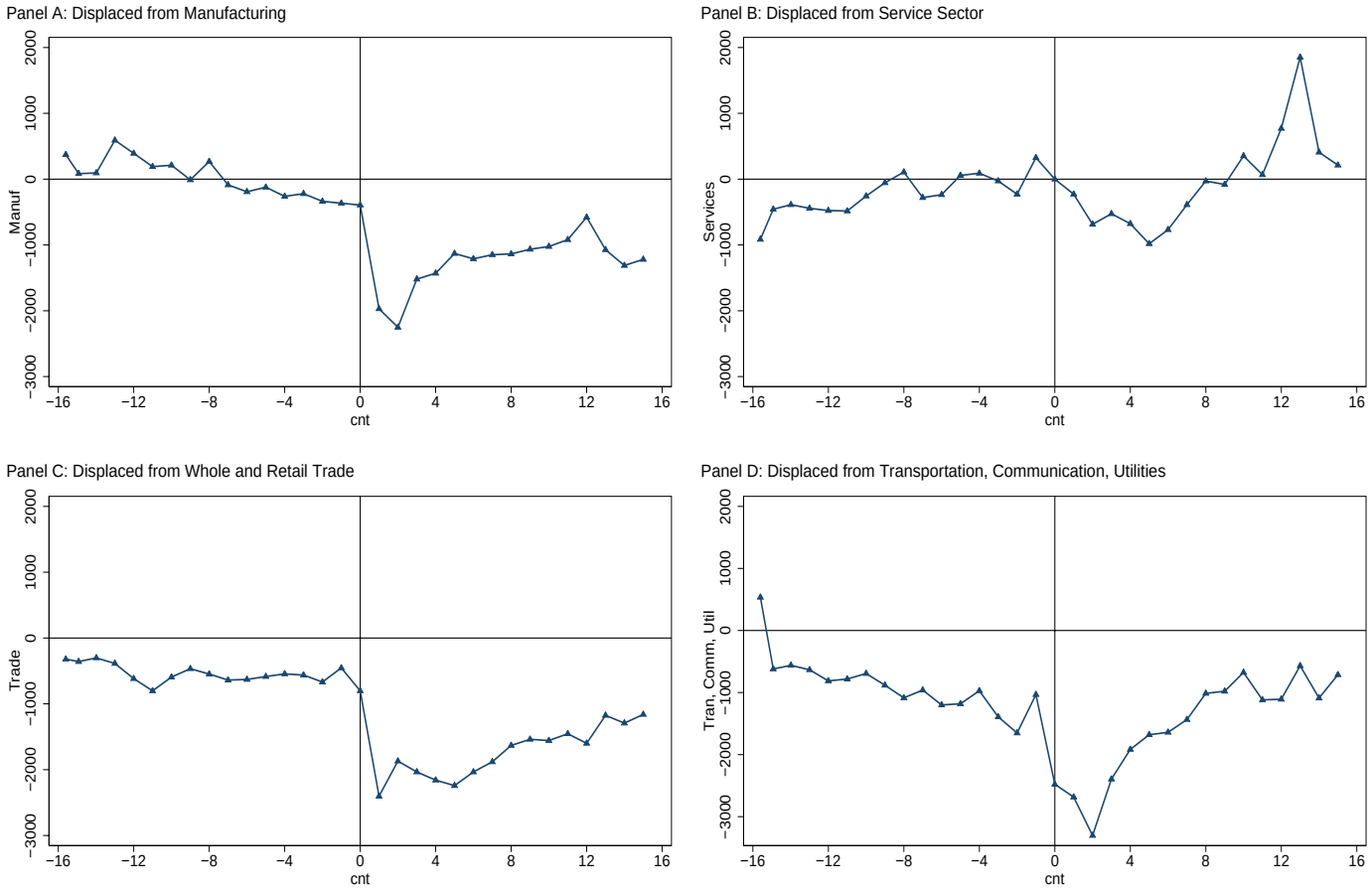
Source: Merged California UI−BW and March CPS Data (see text).

Figure 4: Earnings Loss at Displacement by Age at Displacement
6 Qrts of Tenure at Job Loss, Displaced 1993.1−1999.4.



Source: Merged California UI−BW and March CPS Data (see text).

Figure 5: Earnings Loss at Displacement by Major Industry
6 Qrts of Tenure at Job Loss, Displaced 1993.1−1999.4.



Source: Merged California UI−BW and March CPS Data (see text).