

Do Temporary Labor Market Conditions Improve Human Capital or Rents? Evidence from Job Losers*

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February 15, 2009

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420 W. 118th St., New York, NY 10027, USA
JEL Codes: J31, J41, J63
Keywords: Insurance Contracts, Job Loss, Task Specific Human Capital.

Abstract: Previous literature has found that tight labor market conditions during a job raise wages. This has been attributed to human capital improvements or increases in rents associated with re-bargaining of wages. Using the Displaced Worker Survey from 1984 to 2006, we show that most wage gains associated with good labor market conditions are wiped out by job loss. They are thus unlikely to stem from general human capital improvements. We also find that workers with higher wages due to tight past labor market conditions face higher risk of layoff. This further supports an important role of temporary rents, and speaks against explanations based on firm-specific human capital or implicit insurance contracts.

*We would like to thank W. Bentley MacLeod, Janet Currie, Amitabh Chandra, Joshua Goodman and the seminar participants at Columbia for many helpful comments and suggestions. All remaining errors are our own.

Do Temporary Labor Market Conditions Improve Human Capital or Rents? Evidence from Job Losers*

Abstract: Previous literature has found that tight labor market conditions during a job raise wages. This has been attributed to human capital improvements or increases in rents associated with re-bargaining of wages. Using the Displaced Worker Survey from 1984 to 2006, we show that most wage gains associated with good labor market conditions are wiped out by job loss. They are thus unlikely to stem from general human capital improvements. We also find that workers with higher wages due to tight past labor market conditions face higher risk of layoff. This further supports an important role of temporary rents, and speaks against explanations based on firm-specific human capital or implicit insurance contracts.

1 Introduction

An increasing literature shows that labor market conditions have persistent effects on workers' wages during a job spell.¹ Specifically, higher unemployment rates at the beginning of a new job have a negative effect on wages that persists over time. But this negative effect is superseded if current labor market conditions improve. The adjustment is asymmetric in the sense that starting wages on the job do not normally deteriorate as labor market conditions worsen.²

The standard neo-classical model has difficulty explaining these patterns. Hence, three alternative theories have emerged. First, Robert Gibbons and Michael Waldman (2006) argue that jobs created in better times may offer greater opportunities for skill accumulation.³ This suggests that wages may rise in good times because workers accumulate skill. Second, Beaudry and DiNardo (1991) propose an implicit contracts-with-insurance model in which workers hired in bad times accept low wages but can re-bargain them upwards in good times. Third, models of rent sharing and contracting could also lead to persistent effects of labor market conditions if wages are determined at the beginning of a job based on external labor market conditions and subsequently only change when a new outside option arrives (e.g. W. Bentley MacLeod and James Malcomson 1993).⁴ For example, Pierre Cahuc, Fabien Postel-Vinay, and Jean-Marc Robin (2006) have explored such a process of contracting and re-bargaining based on outside job offers in a context of job search. All the explanations discussed in the literature for wages' downward rigidity could explain the observed asymmetry.⁵

¹See Paul Beaudry and John DiNardo (1992), George Baker, Michael Gibbs, and Bengt Holmstrom (1994), James Ted McDonald and Christopher Worswick (1999), Darren Grant (2003), Till von Wachter and Stefan Bender (2007).

²Specifically, the effect of the unemployment at entry into a job is superseded by the minimum unemployment rate during a job spell. Below, we also show that conditional on the minimum, the maximum unemployment rate during a job spell has no significant effect.

³Early descriptions of such a pattern of skill-upgrading and some evidence can be found in Melvin W. Reder (1955) and Arthur M. Okun (1973).

⁴MacLeod and Malcomson (1993) show that rigid wage contracts that are only renegotiated during extreme labor market conditions can be the optimal response to the hold-up problem if there are general investments and turnover costs.

⁵A large literature documents a distaste of downward wage movements among workers. Alternatively, asymmetry in wage adjustments could arise if workers can credibly signal their outside options, but firms cannot signal the need to adjust wages downward (e.g., such as in Sanford J. Grossman and Oliver D. Hart 1983).

These three explanations have very different implications for the origin and effects of persistent labor market conditions. For example, if, as the search model implies, workers accumulate a larger share of match-specific rents in good times, then the employer faces an incentive to replace these expensive workers. In contrast, layoff risk should stay the same if the increase in wages reflects an accumulation of skills. The layoff risk should decline if part of the accumulated skills are specific to the work place. If implicit insurance contracts are an important factor in the labor market one should expect that workers are insured not only against wage cuts but also against layoffs. In particular, the insurance would not make a lot of sense if wage cuts during economic downturns are simply replaced by laying off those workers who benefit the most from the wage insurance at a given moment.⁶

The source of persistent wage differentials matters. First, from the point of view of the aggregate economy, unemployment rates have more persistent effects on real economic activity if they affect skill accumulation rather than the re-distribution of rents.⁷ Second, from workers point of view, if rents can be more easily accumulated or lost than skills, changes in human capital may lead to more persistent effects of economic fluctuations on life-time earnings.

In this paper we use information on the incidence and cost of job displacement in order to distinguish between alternative explanations for persistent wage gains resulting from good labor market conditions. Our first test is based on the structure of wages after job loss. A key prediction of the model by Gibbons and Waldman (2006) is that labor market related wage gains should persist when workers lose their job. If they are caused by insurance contracts or by rent sharing, however, the gains should disappear once the employment relationship is terminated and a worker starts a new contract at a different employer.⁸

⁶This is true in the case in which workers and firms have symmetric information. With asymmetric information, Grossman and Hart (1983) show that layoffs and insurance contracts can coexist. However, we suspect that a contract in which firms can selectively lay off the most expensive workers will be difficult to sustain as equilibrium due to incentive compatibility if the firm has better information about its productivity than the worker (for the same reasons contracts based on stable employment and variable wages are not sustainable). At an empirical level, this is echoed by high prevalence of seniority rules in dismissals and lower mobility and layoff rates for higher tenured workers (which among others are more likely to have experienced more favorable unemployment conditions on the job).

⁷This is the explicit point of Okun (1973), who adjusts his original law pertaining to the contemporary association of unemployment rates and output for persistent effects from cyclical up or down grading of workers' skills.

⁸As further discussed below, this approach is related to recent studies using displaced workers to analyze the

Our second test is based on the incidence of job displacement. If wage gains associated with labor market conditions are driven by an increasing share of match-specific rents, then an employer facing the necessity to lay off part of her workforce has an incentive to first fire workers with better contracts. If on the other hand wage gains reflect human capital differences, and thus differences in marginal productivity, there is no clear reason why these workers should be laid off first.⁹

We implement both tests for the United States using data from the Displaced Worker Survey from 1984 to 2006. We first confirm existing results regarding persistence of the effect of unemployment rates at hiring, and show that the effect of initial unemployment rates is trumped by that of the minimum unemployment rates during the job spell.¹⁰ We then show that wages after a job displacement do not depend on unemployment conditions relating to the lost job. The wage gains associated with favorable labor market conditions are fully wiped out after a job displacement. We also find that the history of unemployment rates pertaining to the lost job has no effect on the incidence of employment, the number of jobs, or occupation or industry mobility after job displacement. This confirms that previous labor market history is not associated with accumulation of skills that make workers more productive, more employable or less likely to switch industry.¹¹

Our second test also rejects an explanation based on skill accumulation: workers that had particularly good labor market conditions during their job, and thus higher wages, have a significantly higher probability of losing their job. The effect is quite strong: conditional on current unemployment, a one percentage point reduction in the minimum unemployment rate during the current job increases the probability of job loss in the following three years by one third of a percentage point. Since an optimal contract should reward the firm for the insurance it provides, this find-

presence of industry and occupation specific skills (e.g., Derek Neal 1995, Daniel Parent 2000, Maxim Poletaev and Chris Robinson 2008).

⁹This also holds if the accumulated skill is specific to the job or the firm. Note that in this case, it will be lost at job displacement, though it is not clear why wages should increase with accumulated firm specific human capital in the first place.

¹⁰While the previous literature has used national unemployment rates, we use unemployment rates at the state level. We also find that the pattern of adjustment to current labor market conditions is asymmetric, i.e. conditional on the initial and minimum rate of unemployment the maximum unemployment rate during the job spell has no significant effect.

¹¹This also suggests that past unemployment rate conditions are unlikely to be correlated with ability or skills unobserved by the econometrician.

ing also tends to support an explanation based on rents over insurance contracts. However, it is also conceivable that firms use layoffs in difficult economic times to renege on their commitments stemming from insurance contracts.

The following section discusses the possibility that the pattern first described by Beaudry and DiNardo (1991) may arise from task specific human capital or as rents in a search framework and describes our main empirical strategy. Section 3 describes the data. In section 4 we present our test based on the effect of labor market conditions on post displacement wages and job search. Our second test, whether wage premiums associated with past unemployment conditions increase the risk of job loss, is at the center of section 5. The last section summarizes the results and concludes.

2 Insurance contracts, task specific human capital and rent sharing

An increasing literature has documented that the unemployment rate at initial hiring has an effect on workers' wages that persists for the duration of the job spell. However, beginning with Beaudry and DiNardo (1991), several papers have shown that the effect of the initial unemployment rate is superseded by the effect of the minimum unemployment rate during the job spell. We replicate this finding for over fifty years of job-entry cohorts from all U.S. states using data spanning over twenty years, and find it to be quite robust. Thus, in the following we will use Beaudry and DiNardo's main statistical model to lay out the predictions from alternative explanations for these findings and to discuss our test based on displaced workers.

Beaudry and DiNardo (henceforth BDN) use the unemployment rate as a measure of labor market tightness and estimate the following model:

$$\ln(w_{i,t}) = U_t\beta_1 + U_{t_0}\beta_2 + \min\{U_h\}_{h=t_0}^t\beta_3 + X_{i,t}\Omega + \varepsilon_{i,t}, \quad (1)$$

where $w_{i,t}$ is the wage of individual i in year t , U_t is the current unemployment rate, U_{t_0} is unemployment at the start of the current job, $\min\{U_h\}_{h=t_0}^t$ is the lowest unemployment rate since the start of the job, $X_{i,t}$ is a vector of controls and $\varepsilon_{i,t}$ the error term. BDN describe how this empirical

specification can distinguish between three different model of the labor market. In a simple spot market model without task specific human capital, only current labor market conditions should affect wages and therefore: $\beta_1 < 0$, $\beta_2 = 0$, $\beta_3 = 0$. In a model with risk averse workers, risk neutral firms and perfectly enforceable contracts (i.e. workers cannot easily get out of a contract; BDN call this insurance contracting with costly mobility), the optimal contract only depends on labor market conditions at the beginning of the job: $\beta_1 = 0$, $\beta_2 < 0$, $\beta_3 = 0$. Finally, if contracts are only partially enforceable, because workers cannot commit themselves not to leave if they get a better outside offer, the optimal contract (BDN call this insurance contracting with costless mobility) is downward rigid and the wage increases only when the outside options are particularly good. The current wage therefore depends only on the tightest labor market since the start of the job and therefore: $\beta_1 = 0$, $\beta_2 = 0$, $\beta_3 < 0$. BDN estimate this specification with CPS and PSID data and find evidence for the last case.

In their 2006 paper, Gibbons and Waldman propose an alternative explanation for the finding that $\beta_1 = 0$, $\beta_2 = 0$, $\beta_3 < 0$.¹² In their model there are two job levels to which workers can get assigned. A worker's productivity on each job level depends on her human capital. The first level has a relatively high baseline productivity (the productivity if workers have no human capital) but productivity increases only slowly with additional human capital. The second level has lower baseline productivity but higher returns to human capital. Human capital increases with experience so that it is generally preferable to have young workers in the first level jobs and promote them once their human capital reaches a certain threshold. However, jobs are thought to involve different tasks and there is a task specific component to human capital. Thus of two otherwise identical workers on a job level, the one who has been on the job level longer will be more productive. A worker who remains longer on the lower job level both becomes more productive on this level and has less to gain from moving to the higher level, since there are fewer periods before his career ends. It thus can even occur that a worker whose promotion gets delayed may subsequently never reach the next

¹²Gibbons and Waldman (2006) focus more on the cohort effects in Baker, Gibbs and Holmstrom (1994), but they emphasize at several points that this can be easily extended to the Beaudry and DiNardo (1991) findings. In footnote 20 on page 78 they outline explicitly what such an extension would look like.

job level. Even if she eventually does, she will have less experience and thus be less productive on that job. The labor market is thought of as a perfectly competitive spot market and workers are paid their marginal product.¹³ The timing of promotions therefore can have long-lasting effects on workers' wages.

In order to explain cohort effects or the pattern found by BDN, it is necessary to assume that during upturns of the business cycle it is more profitable to promote current workers to the higher job level. This may be the case because high level jobs have higher returns during good economic times. Another explanation is that during business cycle booms firms may want to expand while they also find it harder to fill qualified positions. This may cause them to promote their own workers to high skill positions. If the minimum unemployment rate since the beginning of a job is correlated with the returns to promoting workers to higher levels, this would explain the pattern found by BDN.^{14,15}

Models of rent sharing and contracting can deliver a third explanation of the finding that $\beta_1 = 0$, $\beta_2 = 0$, $\beta_3 < 0$. Although this can be shown in a more general context (e.g., such as in the case of hold-up analyzed in detail in Malcomson and MacLeod 1993), this is best discussed in a concrete setting.¹⁶ For example, in typical search models, employers and workers bargain over the sharing

¹³In their paper there is also heterogeneity in workers' ability to learn on the job, which is revealed over time as workers output is observed. Since the signal of their ability is common knowledge, workers get paid their expected marginal product. This distinction does not affect the logic of this argument, however.

¹⁴Gibbons and Waldman (2006) discuss a related mechanism with very similar predictions. It might be the case that human capital accumulation depends on how busy the firm is. In times of economic boom, there are a lot of interesting projects to work on and workers learn a lot on the job, while in times of slack work workers learn less. Regarding the predictions that we are testing here this is essentially equivalent to the main model in their paper.

¹⁵That promotions are correlated with the business cycle is shown in Illong Kwon and Eva Meyersson Milgrom (2007).

¹⁶In Malcomson and MacLeod (1993), two parties agree on a contract to solve the problem of hold-up, where one party renegotiates the price after the other party has made specific investments. The optimal contract that is induced by the hold-up problem can take various forms depending of the exact circumstances (such as the type of investments and which side is incurring them), but in the case of general investments and some switching or turnover costs, assumptions that seem plausible for the labor market, a fixed-price contract is efficient. In such a contract the price (wage) remains constant until the outside option (which depends on the current labor market conditions) for one of the parties binds. The initial price (wage) is determined by initial (labor market) conditions. This explains both wage rigidity and why, over time, the initial conditions become less important. The asymmetry that wages are mostly downward rigid can for example arise because the wage is fixed in nominal terms so that inflation makes it more likely for the worker's outside option to bind. In this model the hold-up problem determines the contract form, while search on the labor market determines the dynamics of the model. To explain that there are still involuntary lay-offs, that is workers leaving even though their outside options do not bind, an additional aspect such as asymmetric information or a norm theory (that workers dislike wage cuts) is required.

of the rent arising from their match (e.g., Dale Mortensen and Christopher Pissarides 1999). Cahuc, Postel-Vinay, Robin (2006) have extended this model to the case in which workers can renegotiate existing wages based on outside offers from other firms. It is natural to assume workers are more likely to obtain an outside bid in a tight labor market.¹⁷ It is also reasonable to assume that wages are downwardly rigid for the many reasons documented in the prior literature, beginning with the notion that workers dislike wage reductions. On the other hand, there is no clear mechanism that would lead to a reduction in wages as the state of the labor market or of the employer worsens. For example, in the context of bargaining, it may be that the employer has difficulty in credibly signaling the true state of the firm (e.g., Grossman and Hart 1983).

Although BDN, Gibbons and Waldman, and the rent-sharing-and-search theory are based on very different views of the employment relationship and the labor market, they have the same predictions for the effect of the minimum unemployment rate during a job on the current wage. However, the models have sharply different implications for what happens in the event of an exogenous shock that ends the job, such as a mass layoff or a plant closing. In the case of insurance contracts and models of rent sharing, an exogenous termination of the contract should wipe out all wage gains associated with previous re-bargaining. The wage on a new job is the result of a new contract which is based on current labor market conditions and which is then potentially raised during the duration of the job. Nothing in the models implies that the previous wage, or the unemployment history during the previous job, should matter. The task specific human capital model on the other hand makes a different prediction. Since wages reflect marginal productivity, and since human capital is transferable across firms (though not completely across tasks), workers with higher human capital on their pre-displacement job should also earn higher wages on a new job after displacement.¹⁸

¹⁷See for example Robert Shimer (2005) for evidence that vacancy movements are pro-cyclical.

¹⁸This is not inconsistent with the large wage losses at job loss that have been found by the literature (e.g. Henry Farber (1997) or Louis S. Jacobson, Robert J. Lalonde and Daniel G. Sullivan (1993), and for the evidence that these wage losses are largest for workers who switch industries, see Neal (1995) and Parent (2000), or occupations, see Poletaev and Robinson (2008)) since it might also be the case that human capital is partly occupation or industry specific. Furthermore all workers might not find positions after displacement where they can work on their pre-displacement job level. The important point is therefore not that there are smaller wage losses after displacement in a model of task specific capital, but that at least some of the wage differences induced by differences in such capital

The differences in these predictions imply that one can test between the human capital and the re-bargaining models by looking at whether the minimum unemployment rate during a job continues to have an impact on wages after a worker loses the job for exogenous reasons. We implement this test by estimating the following wage regression, which is a direct extension of equation (1), on a sample of displaced workers:

$$\ln(w_{i,t}) = U_t\beta_1 + U_{t_d}\beta_2 + U_{t_0}\beta_3 + \min\{U_h\}_{h=t_0}^{t_d}\beta_4 + X_{i,t}\Omega + \varepsilon_{i,t}, \quad (2)$$

where $w_{i,t}$ is now the wage at the post-displacement job at time t . The worker was displaced at time t_d from a job that started at time t_0 . If the insurance contracting model is what explains the BDN finding, then we would expect that labor market conditions during the lost job cease to have an impact on wages after job loss and therefore $\beta_4 = 0$. If on the other hand the BDN finding reflects the fact that workers who experienced very tight labor market conditions have higher levels of human capital we would expect that these tight labor market conditions continue to be correlated with higher wages beyond displacement and therefore $\beta_4 < 0$.

As an additional piece of evidence, we analyze the effect of past labor market conditions on the incidence of employment and job mobility after job loss. If past labor market conditions affect skills, it is plausible that they should raise employment and reduce job mobility after job displacement.¹⁹ A potential shortcoming of the test based on wages and re-employment probability is that it cannot exclude that wage declines at displacement are due to the loss of industry, occupation, or firm specific-skills. We therefore also analyze the effect of past unemployment conditions on the incidence of industry and occupation mobility after a job displacement. If past unemployment rates are correlated with occupation or industry specific skills, we would expect a reduction in the propensity to switch occupation or industry.

Another shortcoming of the basic test is that our evidence on post displacement outcomes cannot distinguish between the insurance contracting and the rent-sharing models. Hence, we

should persist after a job loss.

¹⁹In the literature on job displacement, more educated workers have been found to have higher employment rates and lower job churning after displacement.

devise a second test of the three alternative models based on how the probability of job loss is affected by past labor market conditions. We will come back to this in section 5. In brief, only if past labor market conditions helped to raise workers' share of rents should firms face an incentive to fire these workers first. In neither the human capital nor the standard insurance contract view (with symmetric information) should this be the case. Section 4 discusses this in more detail.

3 Data and Sample

We use data from the Displaced Worker Survey Supplement (DWS) to the Current Population Survey (CPS) for the years 1984 to 2006. In this time period the DWS has been conducted biannually either in January or February. While the CPS is well known to empirical researchers, the structure of the DWS requires a short discussion. The supplement was introduced to “determine the size and nature of the population affected by job displacements.”²⁰ For this purpose the respondents to the CPS are asked whether they lost a job in the previous 5 (before 1994) or 3 (after 1994) years. If they answer yes, they are asked a number of questions regarding the characteristics of the job that they left and their current employment. There are about 50,000 workers over the entire sample period who lost their job. For these workers wages are available for the lost/left job and the current job on a weekly level. Wages are deflated to prices of 1984 and we dropped top coded wage observations. In line with the previous literature we restrict our sample to full-time workers, but our main results hold through when part-time workers are included.

In order to test whether labor market conditions during a job affect the probability of job loss, a population of workers who did not lose their job is required, in addition to the DWS respondents. Furthermore, information on job tenure is required for this population. From 1996 to 2006 this is available in the “job tenure and occupational mobility supplement” of the CPS, which was conducted in January or February. We use this to obtain a sample of workers for which we have tenure information and information on whether they lost a job in the last three years. For this test we therefore restrict our analysis to this later time period.

²⁰See <http://www.bls.census.gov/cps/dispwkr/dwdes.htm>.

As an alternative source of information on job mobility we also use the Survey of Income and Program Participation (SIPP). The SIPP has the advantage that it is a panel of workers that can be observed before and after displacement, avoiding the potential of selection inherent in the analysis of incidence of job loss in the DWS. On the other hand, only a limited number of SIPP panels have information on reason for job mobility, leading to reduced sample sizes and a more limited range of cohorts of job entrants.²¹ The SIPP sample we use is described further in Section 5.

Unemployment rates were taken from two sources: monthly unemployment rates for each state are taken from the website of the Bureau of Labor Statistics (BLS) for 1976 until 2006. These are collapsed to the state year level by taking the mean over the months. The second source is data on insured unemployment rates from the ETA Financial Handbook No 394.²² This provides unemployment rates on a yearly level for each state between 1947 and 1982. The rates are measured in percentage points.²³

Table 1 presents variable means and standard deviations (in parentheses) for different samples. Column (1) reports the means for all workers who reported having lost a job, the baseline sample of the DWS.²⁴ Column (2) reports means for the main estimation sample, i.e. all workers for which the important variables are available (earnings and tenure at lost job, time of lost job, and where

²¹The question about why the job ended is only available starting with the 1990 panel. The question experienced a significant recoding in 1996. We recoded the answers in the two time periods to yield a consistent time series. To make sure our results are not affected by the change, we have also rerun them separately before and after 1996.

²²See the Employment and Training Administration's website at <http://ows.doleta.gov/dmstree/handbooks/394/home.htm>.

²³The two sources are not quite comparable. In particular the insured unemployment rate is about 3 percentage points lower than the BLS unemployment rate for the overlapping time period (1976 to 1982). To generate a long consistent unemployment time series, we impute the BLS unemployment rates using the insured unemployment rates between 1947 and 1976: Using the overlapping time period we regress the BLS unemployment rate on the insured unemployment rate and a constant. The coefficient is about 1.05 and the constant is about 3 (The R-squared statistics is about 60 percent and the graphical fit appears very good for most states). Using these two estimates we then compute simple linear predictions of the BLS unemployment rate for the years before 1976 using the insured unemployment rate. Thus we have unemployment rates for all states from 1947 to 2007. Note that we use the unemployment rate of the state in which the person lives at the time of the interview to calculate the unemployment history during the lost and the current job. In so far as the interviewed person has moved in the meantime this introduces some measurement error. While this might in principle bias our results, the effects are virtually identical when we estimate them on the sample of workers that report not to have moved after job loss.

²⁴We compared the characteristics of this samples to that for all observations in the Jan/Feb CPS for all years. As discussed in detail elsewhere, displaced workers are on average younger, slightly less likely to be female, and less likely to be non-white. Among all CPS respondents in all years, 7.66% reported being displaced in the past 3 or 5 years.

the unemployment rates could be merged). Column (3) reports means for those workers in this group who are employed again and report a wage at the time of the interview, which is the crucial sample for our analysis of post-displacement wages. The size of this sample is a bit more than 30,000 individuals, substantial but smaller than the total number of displaced workers. However, comparing Columns (1), (2) and (3), we find that sample means are very similar, giving us no reason for concern. Columns (4) and (5) will be discussed in section 5.

4 Post-displacement wages and job search

To establish a baseline we begin by replicating BDN's main findings using the DWS. For this we estimate equation (1) using all individuals in the DWS who were displaced from a fulltime job in the previous 3 or 5 years. The wage on the left hand side is the last wage (weekly earnings) prior to job loss. Instead of the national unemployment rate we use state unemployment rates. The vector of controls includes dummies for years since job loss, dummies for the interview year, industry and state fixed effects, years of education, years of experience, tenure at the lost job at the time of job loss, and dummies for the reason for the job loss (plant closed or moved, slack work, and position/shift abolished). Standard errors are computed allowing for arbitrary correlation of the error term within states.

Table 2 Panel A reports these regressions including the three unemployment measures separately and simultaneously. The results are quite similar to BDN: The minimum unemployment rate during a job has a highly statistically significant negative effect on wages. The coefficient is $-.024$ in the specification that controls for all unemployment measures (Column 4) and thus of very similar magnitude as in BDN, who report coefficients of $-.036$ in their CPS sample and -0.059 in their PSID sample. The coefficient on the unemployment rate at the start of the job is negative and significant when put alone in the regression (Column 2). Yet, as in BDN, it becomes very small when put in simultaneously with the minimum unemployment rate. The unemployment rate at job loss has a positive sign in the full specification. This may seem strange in the sense that it is the opposite of what a spot market may predict, but this is likely caused by our sample selection of

workers who get displaced in that period. Thus it appears that in particularly bad times during the business cycle higher paid workers lose their job, which we return to below.

We now turn to our main test of whether the pattern just described is due to accumulated human capital or due to temporary rents. Panel B of Table 2 shows the results from estimating equation (2). The controls are the same as before. Unemployment at the time of job loss now shows a significant negative effect on the post-displacement wage, reflecting the known result that losing a job at a downturn of the business cycle is particularly bad for wages (e.g. see Jacobson, Lalonde, and Sullivan 1993, von Wachter, Jae Song, and Joyce Manchester 2008). Unsurprisingly, the coefficient on the unemployment rate at the beginning of the lost job continues to be very small and is never statistically significant. But most importantly, the coefficient on the minimum unemployment rate shrinks significantly from panel A to panel B. The coefficient shrinks to 0.004 and is not statistically significant. Most of the wage gains due to tight labor markets are therefore destroyed by job loss, as would be expected if they were caused by temporary rents associated with re-bargaining of wages.

Looking at the number of observations in Table 2 suggests a potential problem: While in panel A, there are 44,091 observations, this shrinks to 31,109 in panel B. The difference is due to the fact that a substantial fraction of job losers are not employed at the time of the DWS interview. If the probability of finding a job is higher among workers for whom the Beaudry DiNardo effect is smaller, this may explain the smaller coefficient on the minimum unemployment in panel B. The last column of panel B of Table 2 avoids this problem by looking only at workers with a valid wage at the lost job and at the time of interview. The regressions are the same as in the third column, except that the wage change from pre to post displacement job, measured as $\ln(w_t) - \ln(w_{t_d})$, is now the right hand side variable. The coefficient on the minimum unemployment rate is now positive: a one percentage point decrease increases the wage loss by about 2.14 percent. Thus, in line with insurance contracts or rent sharing, workers who had fewer wage gains due to tight labor markets are less adversely affected if they lose their jobs.²⁵

²⁵As a similar robustness check for whether our results are affected by selection we estimated the models in Panel A Column 3 on the set of workers who are employed again at interview time. The coefficients are very similar to full

There is another channel through which selection may affect our estimates. Suppose workers are reluctant to accept jobs that offer wages below the level of their pre-displacement job. While this may not be completely compatible with perfect information and rationality on the workers side, this seems certainly possible (See Blanchard and Katz 1999). In this case workers with BDN type wage gains might take longer to find a job after displacement, and, conditionally on actually holding a job a certain time after the job loss, their wages may well be higher. It is easy to test for this, since this “insurance contracting with reservation wages” model would imply that the probability of holding a job after displacement is lower for workers who faced tighter labor market conditions during their old job. For the task specific human capital model one would expect either no effect on the probability of holding a job or even the opposite, namely that workers with higher human capital find it easier to find a job after displacement. We therefore also estimate regressions as in equation (2) but with the dependent variable being a dummy for holding a job at the interview time. In this regression the insurance contracting model predicts $\beta_4 < 0$, while the human capital model implies $\beta_4 \geq 0$.

Table 3 shows the results from this regression. There is no indication that better labor market conditions at the old job raise reservation wages. When controlling for all unemployment rates simultaneously in column 1, the coefficient on the minimum unemployment rate is very small and not significant. On the other hand, as expected, both the current unemployment rate and the unemployment rate at job loss reduce the probability of employment.²⁶ Column 2 shows the same specification with the number of jobs held since job loss as the left hand side variable.²⁷ The minimum unemployment rate prior to job loss also has no effect on the number of jobs held since

sample.

²⁶The unemployment rate at job loss has been shown to persistently reduce employment prospects of displaced workers. Note that the negative effect of current unemployment rates on job, industry, or occupation mobility is consistent with the finding that the majority of job mobility is voluntary and pro-cyclical. The majority of job flows are thus reduced by high unemployment rates.

²⁷Note that the fact that past unemployment rates do not affect employment alleviates the concern that the coefficients on the minimum unemployment rates in the regression of displaced workers are biased towards zero due to selective participation. One way to deal with this concern directly is to include all workers who were displaced and set their wages to zero if they are not employed at the time of the interview. This means of course that one cannot use log wages on the left hand side, so we estimated equation (2) putting levels of wages as the dependent variable. The results are reported in the appendix and confirm our main findings.

job loss. Thus, past unemployment histories neither help nor harm the reintegration of workers into employment after displacement.²⁸

A growing literature has argued that skills accumulated on a job may be specific to the occupation or industry. If skill-upgrading due to good external labor market conditions occurs within occupation or industry, part of the loss of the effect of past unemployment rates we find at displacement may arise because workers cannot find a job in the same occupation or industry as their previous employment. If past unemployment conditions were indeed correlated with occupation- or industry-specific skills, we would expect to see that their impact is to reduce mobility between occupations and industries after job displacement. However, as shown in columns 3 and 4 of Table 3, there is no impact of the minimum unemployment rate past job loss on the propensity to switch firms or industries. We also replicated our analysis of wages after job loss for workers who do not switch industries or occupations. If lower minimum unemployment rates increase industry or occupation specific skills, we should see a beneficial effect, at least for workers staying in their respective industry or occupation. However, this is not the case and the results essentially mirror those in Table 2 (not shown).²⁹

Our findings are robust to many different specifications. Most notably, we also included the maximum unemployment rate occurring during the job spell as an additional regression control into our models. The maximum has no significant effects in any specification, nor does it affect the remaining coefficients. It appears that employers cannot bargain workers' wages downwards in periods of high rates of unemployment, consistent with the literature on downward wage rigidity.³⁰

²⁸The fact that past labor market history has no effect on employment and wages after job loss suggests that past unemployment histories are not strongly correlated with unobserved skills. For example, Lori G. Kletzer (1989) found that wages after displacement are correlated with length of job tenure at the lost job, a fact that can be interpreted as suggesting tenure is correlated with unobserved ability.

²⁹We have also estimated a series of regressions where the unemployment rate has been interacted with a dummy for different subgroups (available in the appendix). The effect of the minimum unemployment rate on pre displacement wages is higher for high education, female, non-unionized workers. The effect on post displacement wages is insignificant for all subgroups, except for very low tenure workers (less than 3 years), among whom we do find a lasting effect beyond job loss.

³⁰Without any other unemployment rate, the effect of maximum unemployment during the job spell has a statistically significant negative impact (for the same sample and specification as Table 2, Panel A the coefficient is -0.0088 with standard error of 0.0023). In the pooled model with additional unemployment rate indicators (parallel to column 4), the maximum unemployment rate has an insignificant positive effect without changing the value of the other coefficients (the coefficient is 0.004 with standard error of 0.003).

5 Wage premiums and risk of layoff

Our second test of the sources of persistent effects of unemployment rates on wages is based on the effect of labor market conditions during a job on the probability of job loss. The ideal setting for this would be to follow a cohort of workers who start a job at the same time and then see how job loss within these cohorts is related to the labor market conditions during their jobs. In this case the test would be whether workers who had very favorable labor market conditions, and thus were able to bargain for higher wages, have a higher hazard of job loss conditional on current labor market conditions. As explained above, we would not expect to see such a pattern in the case of human capital accumulation or insurance contracting.

The structure of the DWS does not allow for such an empirical model but it is possible to approximate this. All workers in our data are asked whether they lost a job in the previous 3 years. We therefore select a sample that consists of all individuals that reported losing a job in the previous 3 years and who were employed at their pre displacement job three years ago. We then select another sample of individuals who report having been continuously employed for at least 3 years at their current job. As described in Section 3, this is available starting in 1996. We thus have a sample for which at time $T - 3$ we know tenure at the job they held at that time. This sample is nearly representative of the workers that held a job at time $T - 3$, the only missing group are workers that did not lose a job but switched jobs for other reasons in the last three years, or workers who lost a job but were only very briefly employed at the lost job. Table 1 Column (4) shows the sample of all workers for which we can calculate tenure at time $T - 3$ at the job they were holding then. Column (5) shows the same group plus the workers for which we are not able to calculate tenure at time $T - 3$ but that are employed at the interview time and have at least 3 years of potential experience. These workers should ideally be included in our estimation sample if they were employed at $T - 3$. This group is only about 10 percent larger than the group in Column (4) and has very similar characteristics. The one group that should be included but is still omitted from this are workers who were employed at $T - 3$, left their job (without being displaced), and are not

employed at the time of the interview. It seems likely that this group is not very big.³¹ Overall, we take the fact that Column (4) and (5) look fairly similar as suggestive evidence that the bias from omitting this group is not very big. Since this may not be satisfactory, we will include this group in our analysis based on the SIPP.

For this set of workers at time $T - 3$ we compute the following labor market condition measures: the unemployment at the beginning of that job, the unemployment at $T - 3$, and the minimum unemployment in between. We regress a dummy for whether a worker will lose his job in the next three years (i.e. $T - 3$ until interview time T) on these unemployment measures:

$$P(\text{JobLoss}_{i,T}) = U_{T-3}\beta_1 + U_{T_0}\beta_2 + \min\{U_h\}_{h=T_0}^{T-3}\beta_3 + X_{i,T}\Omega + \varepsilon_{i,T} \quad (3)$$

From BDN we know that $\min\{U_h\}_{h=T_0}^{T-3}$ is negatively correlated with wages at $T - 3$. If this correlation is due to re-bargaining over search rents when outside job offers arise, rather than reflecting productivity differences or insurance contracts, then firms have an incentive to lay off these workers first and it should be the case that: $\beta_3 < 0$. On the other hand, if the correlation is due to human capital differences or insurance contracts there is no such incentive and we expect $\beta_3 = 0$.

Table 5 Column 1 presents results from estimating equation (3) as a linear probability model (the results are very similar for probit estimation). While, not surprisingly, the unemployment rate at time $T - 3$ has a significant positive effect on the probability of losing a job in the next three years, the minimum unemployment rate between start of the job and $T - 3$ has a significant negative effect. Thus workers who had very favorable labor market conditions during their job, and thus higher wages, have a higher probability of losing a job. This is consistent with re-bargaining in the context of job search, since the wage gains associated with better labor market conditions provide an incentive to firms to lay off these workers first. On the other hand, if wage premiums were purely caused by human capital differences there would be no reason they would lead to higher risk of job loss.³²

³¹Just taking all non-employed people would be a bad proxy for this group, since many of the non-employed were also non-employed at $T - 3$.

³²Alternatively, one can look at future tenure durations given the labor market conditions early during a job. Going

To ascertain that our results are not driven by the structure of our DWS sample, we replicated our analysis with the SIPP. The SIPP consists of several separate panels, each of which starts surveying a sample of workers in a year and repeatedly interviews them for several years. While the exact follow up period varies between panels, we use the first 32 months for each SIPP from 1990 to 2001.³³ Our data spans 15 years and we have valid information for about 86,000 workers that we observe in the first and the last month of the 32 month period. Since the nature of the SIPP sample and the years it covers is different from the DWS sample, in a first step we replicated our main analysis of wages. When we use all available information from the SIPP to replicate the BDN specification (see Appendix A-2), the estimated effect of the minimum unemployment rates on wages is very similar to what we found in Table 2. Even when we focus on the sub-sample of displaced workers, we find very similar coefficients. However, since the sample is now much reduced (a tenth of the DWS sample), we lose statistical significance.

We then used the SIPP sample to reproduce the analysis of the effect of past unemployment conditions on job displacement in Table 5 Column 1. In addition, we present two sets of models with different follow-up duration. Since the SIPP has a true panel structure, we know the exact job tenure of all workers in the first month of the panel, which we use as our baseline. For this job we then compute our measures of labor market conditions and estimate the effect of these conditions on the probability of job loss over the following 32 month period. The message from the alternative specifications based on the SIPP is clear: the larger the minimum unemployment rate on the lost job, the higher the probability of job displacement. The magnitude of the estimate from the linear probability model is very similar to that of the DWS. A three point decline in the

to the DWS again, we select a sample of workers who were employed for at least 5 years at the job from which they were displaced. We then estimate a model for how many more years they stay at this job conditional on the labor market conditions during the five years. Thus we regress tenure on the unemployment rate at the beginning of the job and on the minimum unemployment rate during the first 5 years. We also control for the demographic controls we had before (experience is now experience at the beginning of the job) plus state and year dummies. The results suggest that the unemployment rate at the start of the job and the minimum unemployment rate are both positively correlated with future tenure, as would be expected if higher bargained wages increase the probability of layoff. Consistent with costless re-bargaining the minimum unemployment rate has a larger coefficient.

³³This is the longest time period that is available for all panels in this period. In particular these are the 6 Panels: 1990, 1991, 1992, 1993, 1996, and 2001. The 2004 Panel has only information for the first 16 months after the start of the Panel and before 1990, several important variables are missing.

rate of unemployment (roughly the difference in unemployment from a trough to a peak) leads to an increase in the risk of job displacement of about one percentage point.³⁴

6 Concluding Remarks

Increasing evidence suggests that temporary labor market conditions have persistent effects on wages while workers are on the job. Hence, the effect of labor market conditions at hiring is superseded by the minimum rate of unemployment in the course of the job match. Several competing models of wage setting can explain this phenomenon. While these models have similar implications for wages, they represent very different views of the labor market. In particular, while two explanations - based on human capital or insurance contracts - predict that firms have no incentive to lay off higher paid workers, the opposite is true if wage premiums represent rents from bargaining.

We test between the different explanations by analyzing the incidence and structure of earnings losses at job displacement. Using data from the Displaced Worker Survey covering layoffs occurring over twenty years, we find that wage premiums associated with past labor market conditions fade completely at job loss. Thus, they are unlikely to be explained by the accumulation of task specific human capital. Consistent with this interpretation, we find that past labor market conditions have no effect on employment or industry and occupational mobility after job loss.

We also find that lower unemployment rates during the job significantly increase the risk of layoffs. This is not consistent with a story based purely on human capital accumulation or on optimal insurance contracts with symmetric information contracts. It is, however, consistent with wage premiums reflecting an increased share of rents going to workers that cannot be negotiated downwards during bad times. Reasons for this inability to negotiate rents downward can arise because of norms that prevent pay cuts. However, extensions of insurance contracts (or contracts induced by the hold-up problem) may be able to explain these findings. For example, if the outside

³⁴Our findings are consistent with results in Grant (2003) that the minimum unemployment rate during a job reduces quits. Clearly, if workers earn temporary rents, they have less incentive to quit their job, whereas no such incentive is present in the case of human capital accumulation. Unfortunately, our sample sizes are too small to break up the analysis by further reasons of job separation.

options of a firm are hard to observe by the worker, mass-layoffs may be the only way for the firm to cut its wage bills and signal its distress to renege on the contract. Examining these mechanisms is a fruitful avenue for future research. We should stress that our results do not imply that job losers do not lose some form of specific human capital. Yet, it does not appear that human capital accumulation is strongly associated with external labor market conditions during the job spell in the sample we study.

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Table 1: Characteristics of Analysis Samples of Displaced Workers and Comparison to Current Population Survey

	(1) All DWS	(2) Lost Job Sample	(3) Reemployed Sample	(4) Job Loss Risk Sample	(5) Comparison Sample
Number of observations	85344	44091	31109	168134	189825
Panel A: Outcome variables					
Weekly earnings at current job	293.0 [199.6]	312.0 [201.3]	315.5 [205.0]	343.8 [237.5]	339.4 [234.1]
Weekly earnings at lost job	326.0 [233.2]	368.0 [233.9]	369.9 [226.2]	402.8 [248.3]	397.8 [247.3]
Wage loss after displacement $w(t) - w(t_d)$	-43.66 [182.9]	-60.10 [185.7]	-56.71 [185.2]	-67.52 [200.4]	-63.68 [198.3]
Lost or left job in last 3/5 years	1	1	1	0.0733	0.107
Panel B: Demographics					
Years of education	12.93 [2.415]	12.87 [2.427]	13.15 [2.323]	13.74 [2.311]	13.69 [2.342]
Potential experience	20.53 [12.70]	21.18 [12.08]	20.14 [11.11]	25.10 [11.01]	24.34 [11.30]
Married	0.611	0.647	0.648	0.711	0.694
Race non-white	0.145	0.133	0.121	0.134	0.138
Parttime at current job	0.156	0.128	0.177	0.0105	0.0100
Female	0.432	0.389	0.374	0.415	0.416
Panel C: DWS Variables					
Number of jobs since job loss	1.669 [0.996]	1.665 [0.986]	1.631 [0.957]	1.399 [0.797]	1.386 [0.789]
Occupation Switch	0.721	0.709	0.702	0.564	0.560
Industry Switch	0.773	0.766	0.763	0.668	0.663
Tenure at lost job	5.484 [6.816]	5.632 [6.709]	5.275 [6.159]	7.726 [7.700]	7.456 [7.747]
Panel D: Unemployment Rates					
Contemporaneous unemployment	5.936 [1.784]	6.122 [1.823]	5.881 [1.735]	4.849 [1.128]	4.852 [1.128]
Unemployment at beginning of lost job	6.376 [2.061]	6.414 [2.069]	6.302 [2.054]	5.764 [1.826]	5.733 [1.813]
Unemployment at job loss	6.552 [2.192]	6.529 [2.202]	6.272 [2.112]	5.209 [1.265]	5.200 [1.263]
Minimum unemployment during lost job	5.317 [1.657]	5.329 [1.661]	5.220 [1.623]	4.387 [1.173]	4.401 [1.174]

Sample means (and standard deviations in parentheses) for different subsamples used in the paper. Column (2) shows variable means for all workers in the CPS who reported having lost a job in the previous 3 or 5 (prior to 1994) years. Columns (3) and (4) show the samples used in Table 2 Panel A and B. Column (5) shows the sample used in Table 4, Column (1): workers in 1996 to 2004 for whom tenure can be calculated 3 years prior to the interview. Columns (3) to (5) only show workers that were employed fulltime at the lost job. Column (6) comprises the workers in Column (5) plus all workers that are currently employed fulltime with more than 3 years of experience.

Table 2: Effect of Past Labor Market Conditions on Wages

Panel A: Dependent variable: Log of wage at lost job - Beaudry DiNardo Specification				
	(1)	(2)	(3)	(4)
	$\ln(w_{t_d})$	$\ln(w_{t_d})$	$\ln(w_{t_d})$	$\ln(w_{t_d})$
Unemployment at job loss	-0.00410 (0.00281)			0.00422 (0.00315)
Unemployment at beginning of lost job		-0.0125** (0.00159)		-0.00261 (0.00219)
Min unemployment during lost job			-0.0244** (0.00274)	-0.0240** (0.00435)
Observations	44091	44091	44091	44091

Panel B: Dependent variable: Log of wage at current job and change in log wage					
	(1)	(2)	(3)	(4)	(5)
	$\ln(w_t)$	$\ln(w_t)$	$\ln(w_t)$	$\ln(w_t)$	$\ln(w_t) - \ln(w_{t_d})$
Unemployment at job loss	-0.0117** (0.00289)			-0.0129** (0.00338)	-0.00929* (0.00424)
Unemployment at beginning of lost job		-0.00341 (0.00172)		-0.00373 (0.00306)	-0.00108 (0.00330)
Min unemployment during lost job			-0.00736* (0.00361)	0.00447 (0.00649)	0.0214** (0.00631)
Contemporaneous unemployment	0.00417 (0.00389)	-0.00209 (0.00405)	-0.00148 (0.00399)	0.00418 (0.00404)	-0.0252** (0.00364)
Observations	31109	31109	31109	31109	27073

Significance Levels: * $p < .05$ ** $p < .01$, standard errors clustered on state level. Regressions control for education, experience, tenure, union at lost job, nonwhite, female, married, years since job loss, reason for job loss, part-time at lost job, state, year, and industry Data: CPS Displaced Worker Survey 1984 - 2006, Unemployment is measured yearly on state level. t is the survey time of the DWS, t_d the time just prior to the job loss.

Table 3: Effect of Past Unemployment on Job Search Outcomes - Probit

Panel A: CPS - Displaced Worker Survey				
	(1) Currently Employed	(2) # Jobs since Job Loss	(3) Industry Switch	(4) Occupation Switch
Unemployment at job loss	-0.00405* (0.00195)	0.00372 (0.00564)	0.000479 (0.00315)	-0.00111 (0.00270)
Unemployment at beginning of lost job	0.000976 (0.00231)	0.00244 (0.00371)	0.00145 (0.00244)	0.00414 (0.00222)
Min unemployment during lost job	0.000695 (0.00347)	0.00692 (0.00752)	-0.00285 (0.00436)	-0.00740 (0.00436)
Contemporaneous unemployment	-0.0174** (0.00245)	-0.0100 (0.00578)	-0.00370 (0.00244)	-0.000255 (0.00357)
Observations	45810	34659	30898	30875
Mean of Dep. Var.	0.612	1.642	0.763	0.702
Panel B: SIPP				
	(1) Industry Switch All	(2) Industry Switch Displaced	(3) Occupation Switch All	(4) Occupation Switch Displaced
Unemployment at beginning of job	0.00285* (0.00130)	0.0189* (0.00938)	0.00130 (0.00164)	0.00804 (0.00963)
Min unemployment between beginning of job and first month of panel	-0.00754** (0.00289)	-0.0575** (0.0187)	-0.00395 (0.00384)	-0.0150 (0.0188)
Unemployment in first month of panel	0.00166 (0.00301)	-0.0167 (0.0155)	0.00222 (0.00364)	-0.00650 (0.0122)
Observations	75079	3189	75101	3193
Mean of Dep. Var.	0.219	0.527	0.241	0.484

Significance Levels: * p<.05 ** p<.01, standard errors clustered on state level. Coefficients are marginal effects from probit regressions of the dependent variable on measures of temporary labor market conditions during the lost job. Controls are the same as in Table 2. First month refers to first month of the SIPP panel. Data: CPS Displaced Worker Survey 1984 - 2006 and SIPP 1990 to 2001.

Table 4: Effect of Past Unemployment on Probability of Job Loss

Linear Probability	DWS		SIPP	
	(1) Disp	(2) Layoff or Fired	(3) Quit All	(4) Job ends
Unemployment at beginning of job	-0.000517 (0.000682)	-0.000140 (0.000610)	0.000904 (0.000584)	0.00124 (0.00158)
Min unemployment between beginning of job and T-3	-0.00316** (0.00112)			
Min unemployment between beginning of job and first month		-0.00337* (0.00163)	-0.00657** (0.00183)	-0.0162** (0.00422)
Unemployment at T-3	0.00714** (0.00127)			
Unemployment first month		0.00156 (0.00141)	0.000374 (0.00126)	0.0118** (0.00392)
Observations	168134	86703	86703	86703
Mean of Dep. Var.	0.083	0.054	0.064	0.171

Significance Levels: * p<.05 ** p<.01, standard errors clustered on state level. Coefficients from regressions of the dependent variable on measures of temporary labor market conditions during the lost job. Controls are the same as in Table 2. T-3 refers to 3 years before the interview date. First month refers to first month of the SIPP panel. Data: CPS Displaced Worker Survey 1996 - 2006 and SIPP 1990 to 2001.

Table 5: Effect of Past Unemployment on Probability of Job Loss

Probit	DWS		SIPP	
	(1) Disp	(2) Layoff or Fired	(3) Quit All	(4) Job ends
Unemployment at beginning of job	-0.000517 (0.000682)	0.000213 (0.000682)	0.000223 (0.000777)	0.000636 (0.00158)
Min unemployment between beginning of job and T-3	-0.00316** (0.00112)			
Min unemployment between beginning of job and first month		-0.00297 (0.00154)	-0.000619 (0.00143)	-0.00929** (0.00311)
Unemployment at T-3	0.00714** (0.00127)			
Unemployment first month		0.00233 (0.00136)	-0.00257* (0.00125)	0.0109** (0.00415)
Observations	168134	86703	86703	86703
Mean of Dep. Var.	0.083	0.054	0.064	0.171

Significance Levels: * p<.05 ** p<.01, standard errors clustered on state level. Coefficients are marginal effects from probit regressions of the dependent variable on measures of temporary labor market conditions during the lost job. Controls are the same as in Table 2. T-3 refers to 3 years before the interview date. First month refers to first month of the SIPP panel. Data: CPS Displaced Worker Survey 1996 - 2006 and SIPP 1990 to 2001.

Appendix - Not for Publication

Table A-1 : Sample Means - SIPP

	(1) SIPP
Number of observations	86703
Panel A: Outcome variables	
Layoff or fired	0.0595 [0.237]
Quit	0.0670
Job ends	0.182
Industry switch between first and last month	0.220
Occupation switch between first and last month	0.242
Hourly wage first month	14.04 [11.95]
Hourly wage last month	15.79 [19.72]
Panel B: Demographics	
Tenure	8.286 [8.267]
Age	39.12 [11.03]
High School Grad	0.330
Some College	0.281
College Degree	0.238
Post-College	0.0466
Married	0.647
Non-White	0.231
Panel C: Unemployment Rates	
Unemployment loyment at beginning of job held in first month of panel	6.268 [1.879]
Contemporaneous unemployment (first month of panel)	5.664 [1.562]
Min unemployment during job (until first month of panel)	4.832 [1.225]

Data: SIPP Panels 1990 - 2001. Estimation Sample used in paper. All demographics refer to first month in which the worker is observed.

Table A-2 : Effect of Past Labor Market Conditions on Wages

SIPP	Job Losers			Full Sample
	(1) $\ln(w_{t=1})$	(2) $\ln(w_{t=32})$	(3) $\ln(w_{t=32}) - \ln(w_{t=1})$	(4) $\ln(w_{t=1})$
Unemployment at $t = 1$	0.000743 (0.0116)	-0.0223 (0.0163)	-0.00815 (0.0162)	0.0121* (0.00505)
Unemployment at beginning of job held at $t = 1$	-0.000971 (0.00637)	-0.0177** (0.00564)	-0.00464 (0.00894)	0.000681 (0.00113)
Min unemployment during job held at $t = 1$ up to $t = 1$	-0.0219 (0.0116)	0.0213 (0.0162)	0.0160 (0.0173)	-0.0259** (0.00290)
Unemployment at $t = 32$		-0.00232 (0.0162)	-0.0127 (0.0141)	
Observations	6078	3154	3154	111717

Significance Levels: * $p < .05$ ** $p < .01$, standard errors clustered on state level. Data: SIPP Panels 1990-2001. $t = 1$ and $t = 32$ are the 1st and 32nd month a worker is observed in the panel. Job Losers are all workers who hold a job at $t = 1$ and who are displaced within the next 32 months. The full sample consists of all workers employed at $t = 1$. See Table 2 and main text for additional comments on the specification.

Table A-3 : Effect of Past Labor Market Conditions on Wages

Panel A: Dependent variable: Level of wage at lost job					
	(1)	(2)	(3)	(4)	
	w_{t_d}	w_{t_d}	w_{t_d}	w_{t_d}	
Unemployment at job loss	-0.101 (0.924)			2.232* (1.103)	
Unemployment at beginning of lost job		-3.051** (0.554)		-0.338 (1.015)	
Min unemployment during lost job			-5.852** (1.112)	-6.825** (2.137)	
Observations	50316	50316	50316	50316	
Panel B: Dependent variable: Level of wage at current job and change in wage levels					
	(1)	(2)	(3)	(4)	(5)
	w_t	w_t	w_t	w_t	$w_t - w_{t_d}$
Unemployment at job loss	-2.237** (0.813)			-2.408** (0.827)	-2.086 (1.414)
Unemployment at beginning of lost job		0.232 (0.609)		0.480 (0.771)	0.630 (1.197)
Min unemployment during lost job			-0.806 (1.017)	0.0816 (1.297)	5.787* (2.280)
Contemporaneous unemployment	-4.337** (1.241)	-5.573** (1.157)	-5.540** (1.143)	-4.225** (1.248)	-13.61** (1.300)
Observations	50316	50316	50316	50316	50316

Significance Levels: * $p < .05$ ** $p < .01$, standard errors clustered on state level. Specification is the same as Table 2 with the difference that the dependent variable is measured in levels rather than logs and that wages of workers who do not hold a job at time t , the interview time, are set to zero and included in the sample.

Table A-4 : Effect of Past Labor Market Conditions on Wages

Panel A: CPS DWS - Only workers reemployed in same occupation			
	(1)	(2)	(3)
	$\ln(w_{t_d})$	$\ln(w_t)$	$\ln(w_t) - \ln(w_{t_d})$
Unemployment at job loss	0.00534 (0.00583)	-0.0163 (0.00928)	-0.0264* (0.00986)
Unemployment at beginning of lost job	-0.00657 (0.00501)	-0.0107 (0.00597)	-0.00544 (0.00536)
Min unemployment during lost job	-0.0145 (0.0105)	0.00130 (0.0115)	0.0171 (0.0120)
Contemporaneous unemployment		-0.00472 (0.00858)	-0.00321 (0.00957)
Observations	7851	9203	7851
Panel B: CPS DWS - Only workers reemployed in same industry			
	(1)	(2)	(3)
	$\ln(w_{t_d})$	$\ln(w_t)$	$\ln(w_t) - \ln(w_{t_d})$
Unemployment at job loss	0.00702 (0.00794)	-0.00701 (0.0107)	-0.0129 (0.0109)
Unemployment at beginning of lost job	-0.00393 (0.00574)	-0.0131 (0.00826)	-0.0116 (0.00886)
Min unemployment during lost job	-0.0301* (0.0116)	0.00328 (0.0143)	0.0280 (0.0166)
Contemporaneous unemployment		-0.00650 (0.0102)	-0.0100 (0.00972)
Observations	6302	7336	6302

Significance Levels: * $p < .05$ ** $p < .01$, standard errors clustered on state level.

Data: CPS Displaced Worker Survey 1984 - 2006.

Unemployment is measured yearly on state level.

Table A-6 : Effect of Max Unemployment Rate on Wages

Panel A: Log of wage at lost job		
	(1) <i>ln(w_{l_d})</i>	(2) <i>ln(w_{l_d})</i>
Unemployment at job loss		0.00242 (0.00332)
Unemployment at beginning of lost job		-0.00372 (0.00234)
Min unemployment during lost job		-0.0252** (0.00442)
Max unemployment during lost job	-0.00880** (0.00228)	0.00404 (0.00289)
Observations	44091	44091
Panel B: Log of wage at current job		
	(1) <i>ln(w_t)</i>	(2) <i>ln(w_t)</i>
Unemployment at job loss		-0.0112** (0.00403)
Unemployment at beginning of lost job		-0.00264 (0.00367)
Min unemployment during lost job		0.00549 (0.00621)
Max unemployment during lost job	-0.00848* (0.00319)	-0.00366 (0.00472)
Contemporaneous unemployment	-0.0000656 (0.00392)	0.00414 (0.00401)
Observations	31109	31109

Notes: See Table 2.

Table A-7 : Effect of Unemployment on Post-Displacement Wage by Subgroups

Panel A: Log of wage at lost job					
	Female	More than high school	Subgroup Age \geq 40	Tenure > 3 years	Job unionized
	(1)	(2)	(3)	(4)	(5)
	$\ln(w_{t_d})$	$\ln(w_{t_d})$	$\ln(w_{t_d})$	$\ln(w_{t_d})$	$\ln(w_{t_d})$
Unemployment at job loss	0.00420 (0.00313)	0.00419 (0.00310)	0.00409 (0.00312)	0.00439 (0.00314)	0.00453 (0.00315)
Unemployment at beginning of lost job	-0.000862 (0.00276)	-0.00547* (0.00253)	-0.00293 (0.00312)	0.00268 (0.00713)	-0.000663 (0.00237)
Unemployment at beginning of lost job if in subgroup	-0.00458 (0.00434)	0.00693* (0.00297)	0.000355 (0.00405)	-0.00597 (0.00786)	-0.0245** (0.00571)
Min unemployment during lost job	-0.0195** (0.00470)	-0.0185** (0.00429)	-0.0224** (0.00496)	-0.0307** (0.00811)	-0.0264** (0.00452)
Min unemployment during lost job if in subgroup	-0.0127* (0.00491)	-0.0138** (0.00347)	-0.00439 (0.00513)	0.0109 (0.00862)	0.0441* (0.0184)
Observations	44091	44091	44091	44091	44091
Panel B: Log of wage at current job					
	(1)	(2)	(3)	(4)	(5)
	$\ln(w_t)$	$\ln(w_t)$	$\ln(w_t)$	$\ln(w_t)$	$\ln(w_t)$
Unemployment at job loss	-0.0129** (0.00336)	-0.0128** (0.00336)	-0.0129** (0.00342)	-0.0120** (0.00337)	-0.0126** (0.00337)
Unemployment at beginning of lost job	-0.00446 (0.00407)	-0.00540 (0.00413)	-0.00427 (0.00527)	0.0196** (0.00620)	-0.00227 (0.00340)
Unemployment at beginning of lost job if in subgroup	0.00205 (0.00618)	0.00365 (0.00519)	0.000972 (0.00618)	-0.0263** (0.00701)	-0.0186 (0.00983)
Min unemployment during lost job	0.00794 (0.00750)	0.00991 (0.00740)	0.00492 (0.00824)	-0.0212* (0.00905)	0.00252 (0.00689)
Min unemployment during lost job if in subgroup	-0.00999 (0.00927)	-0.0123 (0.00673)	-0.000563 (0.00727)	0.0298** (0.00838)	0.0375 (0.0196)
Contemporaneous Unemployment	0.00407 (0.00408)	0.00383 (0.00397)	0.00418 (0.00403)	0.00483 (0.00404)	0.00417 (0.00408)
Observations	31109	31109	31109	31109	27073

Regressions also control for current unemployment and unemployment at time of job loss. Other controls are education, experience, tenure, union at lost job, nonwhite, female, married, years since jobloss, reason for jobloss, parttime at lost job, state, year, and industry Data: CPS Displaced Worker Survey 1984 - 2006. Unemployment is measured yearly on state level Significance Levels: * $p < .05$ ** $p < .01$, standard errors clustered on state level.