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Does Wage Persistence Matter for Employment Fluctuations? Evidence from Displaced Workers†

By Johannes F. Schmieder and Till von Wachter*

Previous literature has found that tight labor market conditions during a job raise wages. Using the Displaced Worker Survey from 1984 to 2006, we show that wage gains associated with good labor market conditions disappear at job loss. We also find that workers with higher wages due to tight past labor market conditions face higher risk of layoff. These findings suggest an important role of persistent rigidities in the wage setting process that are related to layoff decisions. This supports the notion that downward rigid employment contracts help explain the Shimer (2005) "puzzle" of low wage relative to employment fluctuations. (*JEL* J31, J41, J63)

Recent work by Robert Shimer (2005) has stressed the difficulties of the standard Dale T. Mortensen and Christopher A. Pissarides (1994) model in explaining the cyclical movement of unemployment. As potential mechanism behind observed employment dynamics, several papers (e.g., Shimer 2004, Robert Hall 2005, and Hall and Paul Milgrom 2008) have suggested rigidities in the wage setting process that prevent wages from declining during an economic downturn. However, the ability of wage rigidities to explain aggregate employment fluctuations crucially depends on their sources. For example, on the one hand Hall (2005) finds that contractually rigid wages can generate movements in employment of the right order of magnitude. On the other hand, Leena Rudanko (2009) suggests that employment changes implied by implicit insurance contracts cannot reproduce observed cyclical fluctuations.[1]

* Schmieder: Department of Economics, Columbia University, 1022 International Affairs Building, 420 W. 118th St., New York, NY 10027, and IZA (e-mail: jfs2106@columbia.edu); von Wachter: Department of Economics, Columbia University, 1022 International Affairs Building, 420 W. 118th St., New York, NY 10027, NBER, CEPR, and IZA (e-mail: vw2112@columbia.edu). We would like to thank Paul Beaudry, Janet Currie, Darren Grant, Joshua Goodman, W. Bentley MacLeod, Derek Neal, Daniel Parent, Michael Waldman, two anonymous referees, and seminar participants at Columbia for many insightful comments and helpful suggestions. von Wachter acknowledges finanical support from National Science Foundation grant no. 0453017. All remaining errors are our own.

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[1] The debate in macro and labor economics on how to reconcile large cyclical movements in employment with smaller cyclical fluctuations in real wages well precedes discussion of the so-called Shimer puzzle and dates back to the early days of the real-business cycle literature or even to Keynes. A substantial literature has evolved around the question of whether such patterns can be explained as shifts in labor demand around an elastic labor supply curve (e.g., see Olivier Blanchard and Stanley Fisher 1989 for a summary). The extent of elasticity of labor supply and the source of labor demand shocks has been debated ever since. In parallel, an empirical literature has argued that real wages are more cyclical than initially believed (e.g., Gary Solon, Robert Barsky, and Jonathan A. Parker 1994). However, despite a venerable literature the puzzle of strong employment fluctuations and modest wage changes has remained.

Microeconomic studies of wage dynamics provide a natural source of information as to the extent and source of wage rigidities. For example, an increasing empirical literature has shown that labor market conditions have persistent effects on workers' wages during a job spell.[2] 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, thus pointing to important rigidities in the wage process.[3]

These patterns of persistence have often been explained with the existence of implicit contracts that insure workers against wage declines (See, e.g., Milton Harris and Bengt Holmstrom 1982, Jonathan Thomas and Timothy Worrall 1988, Beaudry and DiNardo 1991, Thomas and Worrall 2007) thus decoupling them from current labor market conditions. However, another long-standing hypothesis in labor economics suggests that they can also be explained by differential human capital accumulation (e.g., Melvin W. Reder 1955, Arthur M. Okun 1973, Robert Gibbons and Michael Waldman 2006). Alternatively, it is straightforward to show that they could also be explained by one-sided rebargaining of downward rigid wage contracts (e.g., W. Bentley MacLeod and James M. Malcomson 1993) or due to job matching (e.g., Marcus Hagedorn and Iourii Manovskii 2009). Among these alternative explanations of wage persistence only some—such as presence of rents—imply truly rigid wages and amplified employment fluctuations.[4] However, it is difficult to distinguish between them based on the dynamics of wages alone.

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 and examine their implications for layoff. Our first test is based on the structure of wages after job loss. A key prediction of the human capital view formalized 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.[5]

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.[6]

[2] See Paul Beaudry and John DiNardo (1991); 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); Mario Macis (2007).

[3] A related earlier literature has estimated models of the covariance structure of earnings and hours to learn about the determinants of wages (e.g., John M. Abowd and David Card 1989).

[4] In this paper we refer to wage rigidity as this dependence of wages on past labor market conditions and relative insensitivity to current labor market conditions (measured by the unemployment rate).

[5] As further discussed below, this approach is related to recent studies using displaced workers to analyze the presence of industry and occupation specific skills (e.g., Derek Neal 1995, Daniel Parent 2000, Maxim Poletaev and Chris Robinson 2008).

[6] This 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

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.[7] 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 implies that previous labor market history is not associated with accumulation of skills or job matches that make workers more productive, more employable or less likely to switch industry.[8]

Our second test also rejects an explanation based on skill accumulation or job matching: 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 finding 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.[9]

These findings have important implications for the microeconomics and macroeconomic modeling of the labor market. First, the evidence suggests that Beaudry and DiNardo's (1991) seminal finding reflect true downward wage rigidities and not efficient changes in the wage structure due to human capital, matching, or perfect insurance. Second, our findings support the notion that even if entry wages are flexible, this overstates the degree of flexibility in the actual cost of new hires, which is determined by the present discounted value of the wage profile during an employment relationship.[10] Third, we are the first paper to provide direct evidence that downward wage rigidities—in the form of persistent effects of past favorable unemployment conditions on wages—lead to higher incidence of job displacement.

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in the first place.

[7] While 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.

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

[9] In other words, our results reject the symmetric insurance contracts with *complete* commitment, but not necessarily a model where firms credibility is derived from reputation. In this case, the firm may try to exploit turbulent economic times such as recessions to layoff expensive workers at possibly little cost in terms of reputation. We are thankful to Paul Beaudry for raising this point.

[10] A recent literature (succinctly summarized in Pedro Martins, Solon, and Jonathan Thomas 2009) suggests that if wages in ongoing employment relationships are rigid, cyclicality of entry wages is particularly relevant for hiring decisions. If wages themselves are a nonlinear function of the evolution of unemployment rates, as BDN's and our findings suggest, the role for entry wages for job creation is more complex as it depends on the expected path of the unemployment rate at hiring.

The following section discusses the methodology of Beaudry and DiNardo (1991) and describes additional implications of their implicit contracting model for labor market outcomes related to job displacement, which will be the basis for our empirical strategy. Section II describes the data. In Section III 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 IV. In Section V we discuss how these pieces of evidence fit into models of insurance contracts and other views of the labor market. The last section summarizes the results and concludes.

I. Methodology

In their influential paper Beaudry and DiNardo (1991) laid out a statistical model that describes how under different assumptions about the labor market, past labor market conditions should influence current wages in different ways. Empirically, they showed 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 US states using data spanning over twenty years, and find it to be quite robust. Beaudry and DiNardo interpret this as evidence for the importance of implicit insurance contracts between employers and employees. However Beaudry and DiNardo did not look at other outcomes apart from wages at the current job.[11] Other papers that support the implicit insurance contract view of the labor market generally use the same methodology. Thus while it is comforting that the main finding of Beaudry and DiNardo (1991) has been shown to be robust in various different settings, these replications are only as convincing as far as one accepts the original test.[12] Furthermore it is unclear whether such contracts are just efficiency enhancing by moving risk away from the risk averse actors, or whether they may make reallocation of labor during a recession more difficult and thus leading to inefficiencies at the macro level. Therefore, in the following we will use Beaudry and DiNardo's main statistical model and show how it can be extended to test for additional predictions of the implicit contract theory 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:

$$\text{(1)} \qquad \ln(w_{i,t}) = U_t\beta_1 + U_{t0}\beta_2 + \min\{U_h\}_{h=t_0}^{t}\beta_3 + X_{i,t}\Omega + \varepsilon_{i,t},$$

where $w_{i,t}$ is the wage of individual i in year t, U_t is the current unemployment rate, U_{t0} 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

[11] In a later paper Beaudry and DiNardo (1995) also look at how hours vary over the business cycle and argue that their finding is also consistent with insurance contracts.

[12] Devereux and Hart (2007) is one exception as they do not find a pattern similar to Beaudry and DiNardo for the UK.

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 enforcible 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 specification (1) with Current Population Survey (CPS) and PSID data and find evidence for the last case.

The implicit contract theory of BDN has predictions that go beyond this basic specification. If the wage premiums associated with past tight labor market conditions purely represent a larger share of the job specific rent then these rents should disappear if the employment relationship ends exogenously and workers start new jobs with contracts based on current labor market conditions.

We implement this test by estimating the following wage regression, which is a direct extension of equation (1), on a sample of displaced workers:

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

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 a related 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.[13] 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.

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

The basic model of implicit insurance contracts does not explain how these contracts relate to layoffs, which are clearly empirically relevant. In fact the low variability of wages in the light of large variations in employment over the business cycle has emerged as a major puzzle to macroeconomists. Hence, we also test how the probability of job loss is affected by past labor market conditions. If past labor market conditions helped to raise workers' share of rents, then these workers are less valuable to an employer than workers who claim a smaller share of the rents. In the standard insurance model of Harris and Holmstrom (1982) or BDN the employers' side of contracts are enforcible and thus employers can neither lower the wage nor lay off workers (otherwise they could use this threat to renege on the contract). Thomas and Worrall (1988) extended this model to allow for limited commitment on the employer side, but even in this case there is no room for layoffs, as contracts are simply renegotiated as the employers outside option constraint becomes binding. Workers with higher premia due to favorable past labor market histories should lose these premia relative to other workers during bad times. As a result, the models do not predict differences in layoffs between these different groups.[14]

To test for such differences in layoff rates we estimate regressions of the following form:

$$(3) \qquad P(JobLoss_{i,t}) \;=\; U_t\beta_1 \;+\; U_{t_0}\beta_2 \;+\; \min\{U_h\}_{h=t_0}^{t}\beta_3 \;+\; X_{i,t}\Omega \;+\; \varepsilon_{i,t}\,.$$

The probability of job loss (during a short interval from time t onwards) is allowed to vary with the unemployment rate at time t, the unemployment rate at the start of the current job t_0, and the minimum unemployment rate during the job. From BDN we know that the minimum unemployment rate is positively correlated with wages at time t, so that this regression is an indirect test of whether these wage premiums are related to larger probability of getting layed off. We will explain the implementation of this test in more detail in Section IV.

II. Data and Sample

We use data from the Displaced Worker Survey Supplement (DWS) to the 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."[15] 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 85,000 workers over the entire sample period who

[14] Sanford J. Grossman and Oliver D. Hart (1983) extended the basic insurance model to a setting with asymmetric information where layoffs are necessary to signal the state of the employer. We will come back to this in Section V.

[15] See http://www.bls.census.gov/cps/dispwkr/dwdes.htm. A series of papers has addressed measurement issues in the DWS, e.g., Robert Topel (1990); Henry Farber (1997); Andrew Hildreth, Elizabeth Weber, and von Wachter (2008).

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.

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.[16] The SIPP sample we use is described further in Section IV.

Unemployment rates were taken from two sources: monthly unemployment rates for each state are taken from the Web site 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.[17] This provides unemployment rates on a yearly level for each state between 1947 and 1982. The rates are measured in percentage points.[18]

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.[19] 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 the unemployment rates could be

[16] The 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.

[17] See the Employment and Training Administration's Web site at http://ows.doleta.gov/dmstree/handbooks/394/home.htm.

[18] The 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^2 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.

[19] We 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 percent reported being displaced in the past 3 or 5 years.

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

	All DWS (1)	Lost job sample (2)	Reemployed sample (3)	Job loss risk sample (4)	Comparison sample (5)
Sample for regressions in		Table 2 Panel A	Table 2 Panel B	Table 4 (first column)	
Number of observations	85,344	44,091	31,109	168,134	189,825
Panel A. Outcome variables					
Weekly earnings at current job	293.0	312.0	315.5	343.8	339.4
	[199.6]	[201.3]	[205.0]	[237.5]	[234.1]
Weekly earnings at lost job	326.0	368.0	369.9	402.8	397.8
	[233.2]	[233.9]	[226.2]	[248.3]	[247.3]
Wage loss after displacement	−43.7	−60.1	−56.7	−67.5	−63.7
$w(t) - w(t_d)$	[182.9]	[185.7]	[185.2]	[200.4]	[198.3]
Lost or left job in last 3/5 years	1	1	1	0.073	0.11
Panel B. Demographics					
Years of education	12.9	12.9	13.2	13.7	13.7
	[2.41]	[2.43]	[2.32]	[2.31]	[2.34]
Potential experience	20.5	21.2	20.1	25.1	24.3
	[12.7]	[12.1]	[11.1]	[11.0]	[11.3]
Married	0.61	0.65	0.65	0.71	0.69
Race non-white	0.15	0.13	0.12	0.13	0.14
Part-time at current job	0.16	0.13	0.18	0.010	0.010
Female	0.43	0.39	0.37	0.42	0.42
Panel C. DWS variables					
Number of jobs since job loss	1.67	1.67	1.63	1.40	1.39
	[1.00]	[0.99]	[0.96]	[0.80]	[0.79]
Occupation switch	0.72	0.71	0.70	0.56	0.56
Industry switch	0.77	0.77	0.76	0.67	0.66
Tenure at lost job	5.48	5.63	5.27	7.73	7.46
	[6.82]	[6.71]	[6.16]	[7.70]	[7.75]
Panel D. Unemployment rates					
Contemporaneous unemployment	5.94	6.12	5.88	4.85	4.85
	[1.78]	[1.82]	[1.73]	[1.13]	[1.13]
Unemployment at beginning of lost job	6.38	6.41	6.30	5.76	5.73
	[2.06]	[2.07]	[2.05]	[1.83]	[1.81]
Unemployment at job loss	6.55	6.53	6.27	5.21	5.20
	[2.19]	[2.20]	[2.11]	[1.27]	[1.26]
Minimum unemployment during lost job	5.32	5.33	5.22	4.39	4.40
	[1.66]	[1.66]	[1.62]	[1.17]	[1.17]

Notes: Sample means (and standard deviations in parentheses) for different subsamples used in the paper. Column 1 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. Column 2 shows the sample of those workers who were employed fulltime at the lost job and with valid information on tenure and earnings at lost job, time of job loss and unemployment rates during lost job. Column 3 restricts this sample further to workers who are employed again at the time of the survey who have valid earnings. Column 4 shows workers in the period 1996 to 2004 for whom tenure can be calculated 3 years prior to the interview. Column 5 comprises the workers in column 4 plus all workers that are currently employed fulltime with more than 3 years of experience.

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 IV.

III. 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. 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 -0.024 in the specification that controls for all unemployment measures (column 4) and thus of similar magnitude as in BDN, who report coefficients of -0.036 in their CPS sample and -0.059 in their PSID sample. It is also similar to results in Grant (2003), who finds coefficients of about -0.02 to -0.03 using various samples from the National Longitudinal Surveys. 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 bad times during the business cycle higher paid workers lose their job, which we return to below.[20]

We now turn to our analysis of whether the wage increases associated with tight labor market conditions are lost at job loss. 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 Louis S. Jacobson, Robert J. Lalonde, and Daniel G. Sullivan 1993; von Wachter, Jae Song, and Joyce Manchester 2009). 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.

[20] A potential concern with the pooled specification in column 4 is that the unemployment rates we use may be highly correlated, leading both to a potentially stronger role of classical measurement error and lower precision. In fact, our standard errors rise somewhat. However, it does not appear that attenuation bias increases significantly. Moreover, we should stress that none of our conclusions substantially rest on the pooled model and derive from the specifications in which the unemployment rates are included separately.

TABLE 2—EFFECT OF PAST LABOR MARKET CONDITIONS ON WAGES

	$\ln(w_{t_d})$ (1)	$\ln(w_{t_d})$ (2)	$\ln(w_{t_d})$ (3)	$\ln(w_{t_d})$ (4)	
Panel A. Dependent variable: Log of wage at lost job - Beaudry DiNardo specification					
Unemployment at job loss	−0.0041 (0.0028)			0.0042 (0.0031)	
Unemployment at beginning of lost job		−0.0125*** (0.0016)		−0.0026 (0.0022)	
Min unemployment during lost job			−0.0244*** (0.0027)	−0.0240*** (0.0043)	
Observations	44,091	44,091	44,091	44,091	
	$\ln(w_t)$ (1)	$\ln(w_t)$ (2)	$\ln(w_t)$ (3)	$\ln(w_t)$ (4)	$\ln(w_t) - \ln(w_{t_d})$ (5)
Panel B. Dependent variable: Log of wage at current job and change in log wage					
Unemployment at job loss	−0.0117*** (0.0029)			−0.0129*** (0.0034)	−0.0093** (0.0042)
Unemployment at beginning of lost job		−0.0034 (0.0017)		−0.0037 (0.0031)	−0.0011 (0.0033)
Min unemployment during lost job			−0.0074** (0.0036)	0.0045 (0.0065)	0.0214*** (0.0063)
Contemporaneous unemployment	0.0042 (0.0039)	−0.0021 (0.0040)	−0.0015 (0.0040)	0.0042 (0.0040)	−0.0252*** (0.0036)
Observations	31,119	31,109	31,119	31,109	27,073

Notes: Standard errors clustered on state level. Regressions control for education (quadratic polynomial), experience (quadratic polynomial), tenure (cubic polynomial), 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 at state level. t is the survey time of the DWS, t_d the time just prior to the job loss.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

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.[21]

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, thus the coefficient

[21] Lori G. Kletzer (1989) and others have found that earnings losses rise with pre-displacement tenure. Longer tenure durations also mechanically imply lower minimum unemployment rates. To avoid attributing losses in wage premiums associated to job tenure to lower minimum unemployment rates during the job spell and thereby understating the role of human capital accumulation, all our regression specifications control for pre-displacement tenure.

on the minimum unemployment rate should be -1 times the coefficient in panel A if the wage premium is completely lost. In fact 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 workers who had wage gains due to tight labor markets experience larger wage losses at job loss by a similar magnitude as the BDN wage premium prior to job loss.[22]

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 Olivier Blanchard and Lawrence F. 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. 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.[23]

Table 3 panel A 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.[24] Column 2 shows the same specification with the number of jobs held since job loss as the left-hand-side variable.[25] The minimum unemployment rate prior to job loss also has no effect on the number of jobs held since job loss. Thus, past unemployment histories neither help nor harm the reintegration of workers into employment after displacement.[26] We discuss the results on industry and occupation mobility in columns 3 and 4, as well as panel B in Section V.

[22] As 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 the full sample.

[23] Note that if industry- or occupation-specific capital matters, workers may search longer for a job in their old industry or occupation. This test cannot distinguish this explanation from that of reservation wages. We are thankful to Daniel Parent for bringing this to our attention

[24] The 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.

[25] Note 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 online Appendix and confirm our main findings.

[26] The 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, 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.

Table 3—Effect of Past Unemployment on Job Search Outcomes—Probit

	Currently employed (1)	Number of jobs since job loss (2)	Industry switch (3)	Occupation switch (4)
Panel A. CPS - displaced worker survey				
Unemployment at job loss	−0.0040**	0.0037	0.0005	−0.0011
	(0.0019)	(0.0056)	(0.0031)	(0.0027)
Unemployment at beginning of lost job	0.0010	0.0024	0.0014	0.0041
	(0.0023)	(0.0037)	(0.0024)	(0.0022)
Min unemployment during lost job	0.0007	0.0069	−0.0028	−0.0074
	(0.0035)	(0.0075)	(0.0044)	(0.0044)
Contemporaneous unemployment	−0.0174***	−0.0100	−0.0037	−0.0003
	(0.0025)	(0.0058)	(0.0024)	(0.0036)
Observations	45,810	34,659	30,898	30,875
Mean of dep. var.	0.612	1.642	0.763	0.702

	Industry switch all (1)	Industry switch displaced (2)	Occupation switch all (3)	Occupation switch displaced (4)
Panel B. SIPP				
Unemployment at beginning of job	0.0029**	0.0189**	0.0013	0.0080
	(0.0013)	(0.0094)	(0.0016)	(0.0096)
Min unemployment between beginning of job and first month of panel	−0.0075***	−0.0575***	−0.0040	−0.0150
	(0.0029)	(0.0187)	(0.0038)	(0.0188)
Unemployment in first month of panel	0.0017	−0.0167	0.0022	−0.0065
	(0.0030)	(0.0155)	(0.0036)	(0.0122)
Observations	75,079	3,189	75,101	3,193
Mean of dep. var.	0.219	0.527	0.241	0.484

Notes: 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 1990–2001.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.

Our findings are robust to many different specifications.[27] 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.[28] As a further

[27] We have also estimated a series of regressions where the unemployment rate has been interacted with a dummy for different subgroups (available in the online 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.

[28] Without 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).

robustness check we redid this analysis using a measure of labor market tightness at the industry level.[29] Since industry unemployment rates are not available far enough into the past we used deviations in industry employment from trend as a proxy for labor market tightness. The results (available in Appendix Table A-8) are quite consistent with the results from state unemployment rates: if the maximum industry employment (this corresponds to the minimum unemployment rate, since positive deviations are tight labor market conditions now) is one percentage point above trend, the pre-displacement wage is about 3 percent higher; as in our main results, there is no effect on the wage at the post-displacement job and wage losses increase by 2.5 percent.[30]

IV. Wage Premiums and Risk of Layoff

We now turn to our test of whether the persistent effects of unemployment rates on wages translate to an effect of labor market conditions during a job on the probability of job loss. The ideal setting for this would be to follow cohorts of workers starting their job at different times and then see how job loss within these cohorts is related to the labor market conditions during their job spells. 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.

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 II, 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

[29] The CPS industry code crosswalk from David H. Autor, Frank Levy, and Richard J. Murnane (2003) has been very helpful for this.

[30] We did some checks whether observable characteristics are correlated with different labor market conditions to get a sense of the potential magnitude and direction of composition bias in our main results (see Appendix Table A-9). The only variables that are significant correlated with labor market conditions are education and experience and those are only correlated with the contemporaneous unemployment rate: In times of high unemployment more lower skill workers are in the displaced sample (less education and experience). However, the coefficients on the past labor market history measures and particularly the minimum unemployment rate are never large or significant. Thus at least for observable characteristics there seems to be no evidence for systematic correlation with the minimum (or starting) unemployment rate.

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 columns 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 entirely 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:

$$(4) \quad P(JobLoss_{i,T}) = U_{T-3}\beta_1 + U_{T_0}\beta_2 + \min\{U_h\}_{h=T_0}^{T-3}\beta_3 + X_{i,T}\Omega + \varepsilon_{i,T}$$

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 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, matching capital, or insurance contracts there is no such incentive and we expect $\beta_3 = 0$.[32]

Table 4 column 1 presents results from estimating equation (4) 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.[33]

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

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

[32] Note that since job tenure is an important determinant of job separation, our specifications also include a flexible polynomial in job tenure.

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

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

Linear probability	DWS	SIPP		
	Disp (1)	Layoff or fired (2)	Quit all (3)	Job ends (4)
Unemployment at beginning of job	−0.0005	−0.0001	0.0009	0.0012
	(0.0007)	(0.0006)	(0.0006)	(0.0016)
Min unemployment between beginning of job and $T-3$	−0.0032***			
	(0.0011)			
Unemployment at $T-3$	0.0071***			
	(0.0013)			
Min unemployment between beginning of job and first month of SIPP wave		−0.0034**	−0.0066***	−0.0162***
		(0.0016)	(0.0018)	(0.0042)
Unemployment first month of SIPP wave		0.0016	0.0004	0.0118***
		(0.0014)	(0.0013)	(0.0039)
Observations	168,134	86,703	86,703	86,703
Mean of dep. var.	0.083	0.054	0.064	0.171

Notes: Standard errors clustered on state level. Coefficients from regressions of the dependent variable on measures of temporary labor market conditions during the job. Controls are the sames 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 1990–2001.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

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.[34] 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 Table 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. 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 specification based on the SIPP (Table 4 column 2) 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

[34] This 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.

point decline in the rate of unemployment (roughly the difference in unemployment from a through to a peak) leads to an increase in the risk of job displacement of about one percentage point.[35] Combining the two main results from Table 2 and Table 4, one can calculate the increase in the probability of job loss that is associated with a wage premium: A one percent wage increase raises the probability of job loss by 0.13 percentage points. This represents an increase in the probability of jobloss of about 1.8 percent (dividing 0.13 by the overall fraction of job loss 0.0733), or—in other words—the elasticity of job loss with respect to wage increases is 1.8.

V. Discussion and Implications

The microeconomics and macroeconomic implications of BDNs main finding of lasting effects of past unemployment conditions on wages depend crucially on the underlying determinants of persistence. For example, cyclical improvements in human capital, a cyclical job matching process, or a pure insurance model of wage-setting are all consistent with a persistent effect of the minimum unemployment rate during a job spell on wages, but imply that the employment relationship is efficient. On the other hand, models of imperfect insurance, hold up, or norms can imply that rigid wages will lead to layoffs. Our findings help to discriminate between these alternative explanations of BDN's stylized fact, and support an important role for true wage rigidities. This conclusion has important macroeconomic implications, which we summarize at the end of this section.

A long-standing hypothesis in labor and macroeconomics—recently formalized by Gibbons and Waldman (2006)—suggests that jobs created in better times may offer greater opportunities for skill accumulation. This suggests that wages may rise persistently in good times because workers accumulate skill.[36] If these skills are general, cyclical wage improvements should not be lost in the case of layoff and should not affect layoff risk. Neither of these predictions is borne out by our findings. If, on the other hand, part of the accumulated skills is specific to the work place, they might

[35] Our 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.

[36] Gibbons 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. In brief, Gibbons and Waldman propose an alternative explanation for the finding that $\beta_1 = 0, \beta_2 = 0$, $\beta_3 < 0$ in equation 1. 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 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. 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.

be lost at job displacement. This seems unlikely, since in this case layoff risk should decline, not increase, with the minimum unemployment rate. Finally if skills are occupation or industry specific this should reduce the propensity to switch occupation or industries, and lower earnings losses for those staying in the same industry after layoff; neither of these implications is supported by our findings. In fact, in our DWS sample (Table 3, columns 3 and 4 of panel A) there is no statistically significant relationship between past labor market conditions and industry or occupation mobility, while in our SIPP sample (panel B of Table 3) we find the opposite of this prediction: tight past labor market conditions are associated with an increase in the probability of switching industries after layoff (though still no effect on occupations).[37] Furthermore workers who are displaced from jobs during which they experienced tighter labor market conditions have higher wage losses even if they are reemployed in the same industry (see Appendix Table A-4).[38] 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.

Another common view of career progression suggests workers earnings increase with time in the labor market through search for better job matches (e.g., Kenneth Burdett 1978, Alan Manning 2006). In the case of on-the-job search, over time only the best job matches survive. The existing literature has suggested that this form of selection can explain increasing tenure profiles (e.g., Katharine G. Abraham and Farber 1987, Topel 1991, Joseph G. Altonji and Nicolas Williams 2005). Similarly, if job offer arrival rates are pro-cyclical, then when times are good only the best job matches survive leading to a positive correlation of minimum unemployment rates and wages (Hagedorn and Manovskii 2009). At a job displacement such a search rent is lost because workers again have to start searching for a good job match. Thus, if selection of this kind were to explain BDNs main finding, this could explain our first main finding that persistent wage premiums related to past business cycle conditions are lost

[37] The fact that we find that tight labor market conditions during a job increase the probability of industry switches after layoff, while clearly inconsistent with a human capital explanation, is also not easily explained by a theory based on contracts and rents. It may indicate that workers with higher pre-displacement wages adjust their search behavior, for example by searching more intensely across industries in order to reach the same level of wages again. However given that we do not find the same result in the DWS data, this interpretation should be viewed with caution.

[38] Another implication of the Gibbons and Waldman model is that whether a worker can make use of his task specific human capital may depend on whether he can find jobs that require these specific skills which in turn may depend on current labor market conditions. To see whether this may be the case we estimated our main models of post-displacement wages and wage losses but added an interaction term of the minimum and the contemporaneous unemployment rate as independent variable (The results are available in Appendix Table A-7). While one has to be cautious in interpreting regressions with 5 unemployment rate measures that are bound to be highly correlated, the results seem to weakly in line with this story: in the post-displacement wage model the interaction term is positive (0.00381) and significant. However the effect is very small: the model implies that when the current unemployment rate is below 6.2 percent, the minimum unemployment rate at the previous job continues to have an effect at current wages, however the marginal effect is small (e.g., around -0.0012 at a current unemployment rate of 3 percent). Furthermore, in the model of wage losses the interaction term is not significant. Since it is clear from our main results that on average wage gains are completely lost, we interpret this as weak evidence that there may be some task upgrading associated with tight labor market conditions but that this can at best explain a very small fraction of the wage increases observed by BDN. We are grateful to Michael Waldman for suggesting this additional test to us. We also included the interaction of current and minimum unemployment rates in our job loss regressions. In results not reported here, the coefficient on the interaction term is positive (albeit small), suggesting that the more difficult economic times get, the less firms are able to selectively lay off only more expensive workers.

at displacement. However, in a matching model wages are again based on a workers marginal product, and there should be no incentives for firms to lay off workers with high wage premiums. This is again not compatible with our second main finding.

A third explanation of persistent effects of unemployment rates on wages is the class of insurance models based on Harris and Holmstrom (1982) suggested by BDN themselves. In BDNs preferred version, firms can credibly commit to a wage schedule, while workers can not, and will renegotiate wages upward when better labor market conditions arise. Although this is typically not discussed explicitly, it seems workers should be protected from layoffs in this model. Otherwise, firms would face an incentive to renege on the insurance contracts, even though they are implicitly remunerated for the provision of insurance by lower average wages. Thus, although this explanation is consistent with our first result, it is not supported by our second finding of increased layoff rates for workers exposed to lower past unemployment conditions.

Clearly, it may be that employers also cannot enter fully binding agreements, such that the wage contract is reneged when outside options of the employer is binding (Thomas and Worrall 1988). In this case, layoffs or separations occur when the outside options of employers and workers cross and there is no more match surplus. However, since contracts are automatically reneged when one option binds, all workers would hit this point at the same time independent of their past employment history. Thus in its pure form the model is not consistent with higher-rent workers losing their jobs, although it may be that this implication could be obtained in an extended version of the model. However, a piece of evidence against this version of the insurance model (and any model in which firms can renege on wage contracts) is that the maximum unemployment rate over a job spell is not correlated with current wages, suggesting firms do not bargain wages downward when their outside options bind.

None of these three models is consistent with both of our main findings. One explanation that does predict the structure of earnings losses at displacement and the determinants of layoffs we find are wage rigidities based on employment contracts. Although the two benchmark models of wage contracts—hold up (MacLeod and Malcomson 1993) or insurance (Harris and Holmstrom 1982)—do not incorporate a role for layoffs, natural extensions could explain our findings. For example, in the insurance model of Grossman and Hart (1983) layoffs arise from the presence of asymmetric information about the firms underlying productivity. Alternatively, it could be the case that whenever the outside option of the firm worsens, this happens so dramatically, that wages do not fall and instead layoffs occur directly. Essentially, the outside option of the employer is immediately below the outside option of the workers, in which case those workers with the highest rents are laid off first. This could explain why we do not observe much responsiveness with respect to the maximum wage.

Another explanation for both of our main findings is the presence of social norms leading wages to be downwardly rigid. For example, workers may be afraid of being cheated or simply dislike wage cuts. The role of such preferences has a long standing tradition in labor economics (e.g., George A. Akerlof, George L. Perry, and William T. Dickens 2000) and has been found to be relevant in survey evidence (e.g., Truman F. Bewley 1998) and laboratory studies (e.g., Ernst Fehr and Armin Falk 1999). An explanation based on norms is of course perfectly complementary to an explanation based on contracts or bargaining. For example, norms may be endogenous and may arise to solve the asymmetric information problem. Similarly, in typical search models, employers and workers are assumed to bargain over the sharing of the rent arising from their match (e.g., Mortensen and Pissarides 1999). Pierre Cahuc, Fabien Postel-Vinay, Jean-Marc Robin (2006) have extended this model to the case in which workers can renegotiate existing wages based on outside offers from other firms. Since it is reasonable to assume workers are more likely to obtain an outside bid in a tight labor market, if wages are downwardly rigid, either because of norms or contracts, such a model would yield a natural explanation for our main findings.

This discussion suggests that the only explanations of both BDNs original result and our two additional findings that wage premiums from low past unemployment conditions are lost at displacement and increase the likelihood of layoff are based on models implying true downward wage rigidities, such as norms or certain types of contracts. This has potentially important implications for the ongoing debate in macroeconomics over the sources of employment fluctuations.

First, our findings suggest that wages of employed workers are downwardly rigid in the course of an employment spell. Second, these results imply that even if entry wages are flexible, this overstates the degree of flexibility in the actual cost of new hires, which is determined by the present discounted value of the wage profile during an employment relationship. Instead of being solely determined by the current marginal product, the cost of new hires—and with it the hiring rate—is determined by the expected time-path of the minimum unemployment rate during a job spell. Third, we provide evidence suggesting that downward wage rigidity actually leads to job displacement.

These findings are relevant in that they speak to potential channels that may help to resolve the so-called Shimer (2005) "puzzle", referring to a lack of employment volatility in the canonical Mortensen-Pissarides matching model. A key channel dampening employment fluctuations in the model is that wages are free to adjust to demand or supply shocks. Hall (2005) and others have suggested that wage rigidities may amplify the effect of such shocks on employment volatility. The findings of this paper, based on earnings changes at job displacement, suggest that Beaudry and DiNardo's (1991) seminal finding indeed reflect true downward wage rigidities and not efficient changes in the wage structure due to human capital, matching, or perfect insurance. We are also the first paper to provide direct evidence that downward wage rigidities—in the form of persistent effects of past favorable unemployment conditions on wages—lead to higher incidence of job displacement.

VI. 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 during 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 and have different macroeconomic implications.

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, consistent with an explanation that wage persistence described in the previous literature represents lasting deviations of wages from workers' marginal product. Our second main result, that lower unemployment rates during the job significantly increase the risk of layoff, suggests that these rigidities indeed represent important frictions in the wage setting process leading to layoffs.

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