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ISSN: 2365-9793

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ABSTRACT

Sticky Wages on the Layoff Margin*

We design and field an innovative survey of unemployment insurance (UI) recipients that yields new insights about wage stickiness on the layoff margin. Most UI recipients express a willingness to accept wage cuts of 5-10 percent to save their jobs, and one third would accept a 25 percent cut. Yet worker-employer discussions about cuts in pay, benefits or hours in lieu of layoffs are exceedingly rare. When asked why employers don't propose job-saving pay cuts, four-in-ten UI recipients don't know. Sixteen percent say cuts would undermine morale or lead the best workers to quit, and 39 percent don't think wage cuts would save their jobs. For lost union jobs, 45 percent say contractual restrictions prevent wage cuts. Among those on permanent layoff who reject our hypothetical pay cuts, half say they have better outside options, and 38 percent regard the proposed pay cut as insulting. An estimated one-quarter of the layoffs violate the condition for bilaterally efficient separations that holds in leading theories of job separations, frictional unemployment, and job ladders. We draw on our findings and other evidence to assess theories of wage stickiness and its role in layoffs.

JEL Classification: E24, J63, J65

Keywords: wage rigidity, sticky wages, layoffs, unemployment insurance, survey of job losers, worker perceptions

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* We thank Justin Brissette and Thomas Revane for discussions about the unemployment insurance system in the State of Illinois; Olivier Armantier, Charlie Brown, Jason Faberman, Rosella Gardecki, Andreas Mueller, Randy Olsen, Frank Stafford, Evan Starr, Giorgia Topa, and Wilbert van der Klaauw for suggestions about survey design and implementation; Mark Bils, Chris Bollinger, Mike Elsby, Bruce Fallick, Kyle Herkenhoff, Narayana Kocherlakota, Fabian Lange, Edward Lazear, Federico Mandelman (discussant), Peter Orazem (discussant), Roberto Pinheiro, Richard Rogerson (discussant), Ayşegül Şahin, Benjamin Schoefer (discussant), Gary Solon, Thijs van Rens, and Ken West for comments on the paper and advice on our research program; and many seminar and conference audiences for comments. A special thanks to Mischa Fisher, former Chief Economist, State of Illinois, for invaluable assistance in securing the cooperation of the Illinois Department of Employment Security. Angela Guo, Meifeng Yang, and Anna Weixel provided excellent research assistance. We are grateful to the Booth School of Business, the Becker Friedman Institute at the University of Chicago, and the Federal Reserve Bank of Cleveland for financial support. The views expressed in this paper are solely the responsibility of the authors and should not be interpreted as reflecting the views of the Board of Governors, the Federal Reserve Bank of Cleveland, or the Federal Reserve System.

1 Introduction

We design and field an innovative survey of unemployment insurance (UI) recipients that yields new insights about wage rigidity on the layoff margin. Specifically, we ask new UI recipients about their willingness to accept wage cuts to save their lost jobs, whether they had discussions with their former employers about compensation cuts in lieu of job loss, and, if not, why not. Our survey went to field in the State of Illinois from September 2018 to July 2019, a period characterized by unusually low inflation and tight labor markets.

Most UI recipients in our sample express a willingness to accept wage cuts of 5-10 percent to save their lost jobs, and one third are willing to take a 25 percent cut. Yet worker-employer discussions about cuts in pay, benefits or hours to prevent layoffs almost never happen in our sample. When asked why, nearly four-in-ten UI recipients do not know. Thirty-nine percent think wage cuts would not save their jobs, and 16 percent say pay cuts would undermine morale or lead the best workers to quit. For the fifteen percent of our sample who lost union jobs, 45 percent say contractual restrictions prevent wage cuts.

When UI recipients refuse our proposed wage cuts to save their jobs, we ask why. Among those who experienced permanent layoffs (four-fifths of our sample), half point to better outside options, 38 percent regard the proposed pay cut as insulting, and one fifth prefer unemployment to working at the lower wage. Among those on temporary layoff, the most common reason for refusing a hypothetical wage cut is fear it might become permanent.

Our finding that employers do not offer pay cuts in lieu of layoffs is broadly consistent with evidence from employer surveys.[1] While we draw inspiration from these studies, our worker-side survey offers distinct insights and advantages. First, it reveals that most new UI recipients would accept wage cuts in lieu of layoffs, and many are open to large wage cuts. Employer reluctance to offer wage cuts becomes more puzzling in the face of widespread worker willingness to accept them. Second, our survey approach lets us explore worker perceptions about why employers do not offer pay cuts to save jobs. Many simply do not know. Third, employer-side surveys of wage-setting behavior typically involve small samples compiled by cold calling firms and snowball sampling. (Bertheau et al., 2022, is a notable exception.) In contrast, our sample frame is precisely defined, and our sample design lends itself to a systematic, institutionalized approach with ongoing surveys.

To our knowledge, we are the first to document the disjunction between worker-side openness to wage cuts and a widespread unwillingness of employers to even broach the subject. We can discard a few possible explanations for this disjunction. First, less than three percent of respondents point to minimum wage and benefit laws to explain why discussions about

[1] See, for example, Kaufman (1984), Blinder and Choi (1990), Agell and Lundborg (1995), Campbell and Kamlani (1997), Bewley (1999), and Babecký et al. (2010).

compensation cuts don't happen. Second, we ask specifically about lower pay for twelve months to save the lost job. Thus, worker-side openness to wage cuts does not reflect an intention to seek and quickly take a better-paying job. Third, job losers in our sample rarely experience pay cuts in the months leading up to layoffs. That is, we see almost no indication that employers try job-saving wage cuts before resorting to layoffs.

By targeting UI recipients, our sample frame and survey data let us develop new insights about sticky wages on the layoff margin. If sticky wages cause some workers to lose jobs and obtain UI benefits, our frame captures them. On the flip side, our frame selects against employment relationships that survive negative shocks by virtue of downward wage adjustments. Thus, we cannot use our sample to quantify the incidence of job-preserving downward wage adjustments. That said, very few job losers report discussions about compensation cuts to prevent layoffs—irrespective of industry, union status, tenure on the lost job, firm size, and other observables. If employer-worker discussions about wage cuts to save jobs were common, we would expect those discussions to succeed in some instances and fail in others. The failures would show up in our sample. That so few do indicates that discussions about job-preserving wage cuts rarely happen.

Many leading theories of job separations, frictional unemployment, wages, and job ladders adopt two assumptions: First, employment relationships are strictly bilateral in the sense that the relationship's continuation value is uninfluenced by the inputs, preferences and compensation of anyone else who works for the same employer. Second, the employer and worker are identically informed about the continuation value and the value of each party's outside options. Influential theories that adopt these two assumptions include Mortensen and Pissarides (1994), Burdett and Mortensen (1998), Ljungqvist and Sargent (1998), Hall (2005), and Cahuc, Postel-Vinay and Robin (2006).

Taken together, these assumptions imply that no layoff (or quit) occurs if there exists a current wage such that each party wants to continue the employment relationship. *Moreover*, the existence and value of such a wage is independent of whether the employer has other workers and what it pays them. We use our survey data to assess this implication directly, one layoff at a time. Specifically, we look for job-saving pay cuts that are small enough to be acceptable to the job loser *and* large enough to save the job. About one-quarter of the layoffs in our sample meet both criteria. That is, one-quarter of the layoffs violate the bilateral condition for efficient separations that holds in many leading theories.

To our knowledge, we are the first to propose and implement this type of direct evaluation of the efficient layoff condition in a major class of theories. The main weakness in our implementation is its reliance on worker perceptions of whether a given pay cut is large enough for the employer to continue the relationship. We explain how to remedy this weakness using a richer sample design.

Other theories retain a focus on bilateral employment relationships but allow for private information about the value of the worker's product to the employer and the value of the worker's outside options. Examples include Hall and Lazear (1984) and Perry and Solon (1985). These theories give rise to sticky wages and can rationalize layoffs that violate the bilateral condition for privately efficient separations in models with symmetric information. Our evidence on the rarity of employer-worker discussions about pay cuts in lieu of layoffs sits uneasily with these theories. In particular, they lead us to anticipate efforts to re-negotiate wages in reaction to observable shocks. That we see so few such efforts in our layoff data raises a challenge for these theories, which we put in the form of a question: Why don't employers (and workers) probe the possibility of job-saving wage cuts in lieu of layoffs, especially in reaction to observable shocks?

The next section expands on the motivation for our study. Section 3 describes our survey, reports summary statistics, and offers evidence on the quality of the survey data. Section 4 documents the willingness of UI recipients to accept job-preserving wage cuts and the dearth of discussions about pay cuts to save jobs. Section 5 explores worker perceptions about the reasons for sticky wages on the layoff margin and why many job losers refuse wage cuts. Section 5 also quantifies the share of layoffs that violate the bilateral condition for privately efficient layoffs. Section 6 discusses our findings in relationship to the literature and draws some additional lessons from case studies. Section 7 offers concluding remarks.

2 Motivation and Related Research

An important motivation for our study is the prominent role of wage stickiness in Keynesian theories of economic fluctuations, unemployment, and stabilization policy.[2] Taylor (1999) surveys older empirical research on nominal wage stickiness, and we discuss more recent research below. These studies document the extent and nature of wage stickiness and offer insight into its underlying sources. A recurring issue is whether, and how much, the stickiness in observed wages matters for employment, hours, and effort. Barro (1977), for example, explains how long-term employment relationships can give rise to sticky wages and an "apparent nonwage rationing of jobs," even as the contractual employment rule equates the value of labor's product to the value of the worker's time. Our survey yields direct evidence on the value of worker time and how it relates to the wage on lost jobs.

A second motivation is the longstanding question of whether downward wage stickiness

[2]Leading examples include Keynes (1936), Tobin (1972), Gray (1976), Fischer (1977), Taylor (1980), Erceg, Henderson and Levin (2000), Smets and Wouters (2003), Woodford (2003), Christiano, Eichenbaum and Evans (2005), Gali (2011), Michaillat (2012) and Basu and House (2016). A related literature stresses inflation's role in "greasing the wheels of the labor market." See, for example, Akerlof, Dickens and Perry (1996); Card and Hyslop (1997); Schmitt-Grohé and Uribe (2013, 2016). Kimball and Rognlie (2013) provide an insightful exchange on the role of sticky prices and sticky wages in explaining aggregate fluctuations.

is an important source of privately inefficient separations. Stickiness in flow wages need not involve inefficient separations, as highlighted in the analyses of Becker (1962), Barro (1977), and MacLeod and Malcomson (1993). Yet, the literature also recognizes that private information, morale effects, adverse selection in quits, pay equity concerns, collective bargaining, and insider-outsider conflicts can give rise to wage rigidities that generate inefficient separations.[3] Thus, theoretical considerations alone cannot tell us whether sticky wages are a source of inefficient layoffs. That leads us to explore whether wage concessions could prevent some layoffs. It also motivates complementary work by Jäger, Schoefer and Zweimüller (2022), who devise an ingenious test of the efficient-separations hypothesis. Other recent efforts to estimate the employment consequences of wage rigidity include Elsby (2009), Kurmann and McEntarfer (2019), Ehrlich and Montes (2019), and Murray (2021).[4]

Our work also relates to a large literature that uses micro data to document the frequency and nature of wage adjustments over time. Most survey-based studies in this literature find substantial downward nominal wage rigidity.[5] Studies that rely on payroll records find a higher incidence of outright nominal wage cuts, especially when looking beyond base pay.[6] Taken as a whole, more recent work undercuts an older view that nominal pay cuts are extremely rare in ongoing employment relationships. Very few previous studies, however, tell us whether wages are flexible enough to prevent inefficient layoffs.

Experimental studies find evidence of downward wage rigidity in the sense that firms choose to pay wages that exceed outside options, and that wage premia are resistant to underbidding by the unemployed. See Fehr and Falk (1999), for example, who stress the role of hard-to-observe effort and the resulting contract incompleteness as a source of downward wage rigidity that survives even in highly competitive settings. Both experimental and survey-based lines of inquiry suggest that concerns about fairness, reciprocity, and motivation can lead to wage rigidity on the layoff margin and inefficient separations.

[3]We discuss these theories below. That some job losers suffer large and persistent earnings losses adds to the interest in theories of wage rigidity. For evidence, see Jacobson, LaLonde and Sullivan (1993), Couch and Placzek (2010), Davis and von Wachter (2011), Krolikowski (2018) and Lachowska, Mas and Woodbury (2020).

[4]Another line of research builds on the observation that search frictions create space for sticky wages to affect the hiring margin without, at the same time, violating private optimality conditions. See, for example, Shimer (2004), Hall (2005), Blanchard and Gali (2010) and Leduc and Liu (2016).

[5]See Card and Hyslop (1997), Kahn (1997), Barwell and Schweitzer (2007), Bauer et al. (2007), Devicienti, Maida and Sestito (2007), and Dickens et al. (2007). However, Blinder and Choi (1990), McLaughlin (1994), Smith (2000), and Fallick, Villar and Wascher (2022) find less downward rigidity using a variety of survey sources.

[6]See, for example, Nickell and Quintini (2003), Elsby, Shin and Solon (2016), Kurmann and McEntarfer (2019), Ehrlich and Montes (2019), Elsby and Solon (2019), Jardim, Solon and Vigdor (2019), and Grigsby, Hurst and Yildirmaz (2021).

3 Our Survey of Unemployment Insurance Recipients

We now describe the major elements of our survey, report response rates and summary statistics, review labor market conditions at the time of the survey, and offer some evidence on the internal validity of responses to key survey questions. We provide more information about the survey and describe our data cleaning methods in appendix A.

3.1 Sample Frame and Sampling Design

Our sample frame covers persons who began collecting UI benefits in the State of Illinois from 10 September to 24 November in 2018, excluding about one-in-ten benefit recipients with no email address on file at the Illinois Department of Employment Security (IDES). All persons in the sample frame received an email invitation to participate in our online Entry Survey, typically one business day after their first UI benefit payment. If the respondent completed the Entry Survey (and permitted further contact), we sent invitations to take part in one or two Follow-Up Surveys. The last follow-up wave went to field in July 2019.

IDES encourages job losers to file an initial claim for UI benefits in the calendar week after job loss (IDES, 2017). The first full week of unemployment is not eligible for benefits. The second full week is eligible, provided the individual's claim is certified. Certified claimants receive benefit payments the week after each benefits-eligible week. Thus, invitations to our Entry Survey typically arrive 18 to 28 days after job loss, although it can be longer due to delays in claims processing. Respondents received a $10 Amazon gift card for taking the survey, except during the first week of Entry-Survey invitations (September 10 to 14), during which they received $5.[7]

Our Entry Survey asks about demographic characteristics, the lost job, willingness to accept pay cuts in lieu of layoff, whether there were discussions about compensation cuts in lieu of layoff, the reasons for employer reluctance to offer such deals, desired attributes in a new job, reservation wages, and more (see appendix A.4). We tailor the questionnaire for permanent and temporary layoffs based on responses to the question: ''Do you expect to be recalled to your previous job within the next 6 months?'' We ask new UI recipients to respond within two days of receiving the survey invitation.

Invitations for the first Follow-Up Survey went to field 2, 4, 8, and 12 weeks (randomized) after completing the Entry Survey, and invitations for the second went to field 4, 8, 12 and 16 weeks after completing the first follow up.[8] Respondents received a $5 Amazon gift

[7] After this first week, IDES paused sending out invitations to give us time for evaluation. We decided to raise the Entry-Survey incentive from $5 to $10 to increase completion rates, as discussed in appendix A.3.

[8] We randomize intervals between surveys to disentangle unemployment and calendar time effects from the possible effects of repeat applications of the survey instrument. On this point, see Krueger and Mueller (2011) and Davis (2011). Appendix A.5 provides details.

card for participating in the first follow up and $10 for the second. We tailor the follow-up questionnaires based on whether the respondent returned to their prior job, took a new job, was self employed, or still without work. Depending on employment status, the follow-ups probe job search activity, reservation wages, attributes of the current job, compensation on the current job, and more. Figure 1 summarizes our survey timing and illustrates its relationship to the layoff date and unemployment spell.

3.2 Response Rates and Survey Completion Times

We distributed about 30,500 invitations for our Entry Survey and received 2,777 completed responses, a 9.1 percent response rate, as reported in Table A1.[9] The median lag between the invitation and completion of the Entry Survey was 5 days, and the 90th percentile lag was 17 days. Completion rates for the first Follow-Up Survey ($5 incentive) ranged from 64 percent for waves fielded 2 weeks after completion of the Entry Survey to 51 percent for waves fielded 12 weeks afterward. Completion rates for the second Follow-Up Survey ($10 incentive) ranged from 85 percent for waves fielded 2 weeks after completion of the first follow up to 77 percent for waves fielded 12 weeks afterward.

All told, we received 5,484 completed responses, and 1,203 persons completed all three surveys. Our analysis samples consider persons with non-missing information about schooling, gender, age, tenure on the lost job, and at least one earnings observation. Appendix A.6 covers response coding, treatment of extreme values, and measurement of ongoing spell durations. The mean completion time for the Entry Survey is 9 minutes, and the median is 8 minutes. The mean (median) completion time for the Follow-Up Surveys is 4 (3) minutes. These short completion times reflect our efforts to design short, highly focused survey instruments to encourage higher response rates and accurate responses.

3.3 Labor Market Conditions at the Time of the Survey

The seasonally-adjusted unemployment rate in Illinois was about 4.4 percent from September 2018 to February 2019 and then fell, as shown in Figure 2. The national unemployment rate was even lower, consistent with a labor market operating at or near full employment. Inflation was low, at about two percent per year, and had been so for many years.

3.4 Selected Summary Statistics

Columns (1) and (2) in Table 1 present unweighted and re-weighted sample statistics. We select weights to match the Current Population Survey (CPS) distribution of job losers unemployed less than five weeks in cells defined by the cross product of two age groups

[9] The completion rate for our Entry Survey is similar to that of Krueger and Mueller (2011).

(less than 45 years, or not), two education groups (four-year college degree, or not), and sex. Column (3) reports corresponding CPS statistics for the period from June 2018 to February 2019, which spans our Entry Survey period plus three months on either side. Relative to newly unemployed job losers in the CPS, our sample is older, more educated, and tilted to manufacturing. At least in part, these patterns reflect UI eligibility requirements and higher unionization in manufacturing, plus union efforts to raise UI take-up rates (Blank and Card, 1991). Relative to the CPS, our sample also tilts toward women, a common survey response pattern (Curtin, Presser and Singer, 2000). We focus on unweighted results below, but our main findings also hold when we re-weight to match the CPS, as discussed in appendix B.

The median tenure of lost jobs in the Entry Survey sample is 1.8 years, as compared to a median job tenure of 4.5 years for ongoing employment spells in the CPS. More broadly, the tenure distribution of the lost jobs covered by our Entry Survey is highly left shifted relative to the tenure distribution in ongoing employment spells (Table A2). This pattern is to be expected because separation rates fall with job tenure, but it should be kept in mind when thinking about the character of our sample. Figure A1 presents the full job tenure distribution for respondents to the Entry Survey.

3.5 Internal Validity Checks and Predictive Content

A key methodological issue is whether our survey approach yields meaningful data on perceived labor market opportunities and the willingness to accept pay cuts. To address this issue, we first consider how reservation wages relate to realized re-employment wages and, second, how the willingness to accept pay cuts relates to both reservation wages and re-employment wages.

For respondents who find new jobs during the time frame covered by our survey, we measure the re-employment wage ratio as the wage on the new job divided by the wage on the lost job. Figure 3 presents the resulting distribution of (natural) log re-employment wage ratios. It shows wide dispersion, with many workers experiencing large wage gains or losses. The mean re-employment wage ratio is -7 log points and the median is -2 log points. The new job pays exactly the same as the old job in fewer than four percent of cases.

We elicit reservation wages in the Entry Survey by asking, ‘‘Suppose someone offered you a job today that is suitable in terms of hours, skills, responsibilities and non-wage benefits. What is the lowest wage or salary, before taxes and deductions, you would accept?’’ Dividing the response by the wage on the lost job yields the reservation wage ratio. A linear regression of the log re-employment wage ratio on the log reservation wage ratio yields an estimated elasticity of 0.78 (0.07).[10] Thus, reservation wages shortly after job loss co-vary strongly

[10] One might worry that this elasticity estimate suffers from division bias, since we use the wage on the lost job as a scaling variable on both sides of the regression. When we instead regress the log re-employment

and positively with realized wages on new jobs. We see this finding as clear evidence that reported reservation wages capture useful information about labor market opportunities.

To gauge openness to pay cuts, we ask permanently laid-off job losers, "Would you have been willing to stay at your last job for another 12 months at a pay cut of X percent?" We randomize X over 5, 10, 15, 20 and 25. As reported in Table 2, the mean log reservation wage ratio is -11 log points for those who accept pay cuts and +4 log points for those who reject them (Panels A and B, Column (1.2)). This difference is statistically significant at the 99.9 percent confidence level, both overall and when grouping the data by size of proposed pay cuts (Panel C, Column (1.3)). Moreover, the mean reservation wage ratio falls with the size of the proposed pay cut among those who accept pay cuts, and these differences are significant at the 95 percent level (Panel C, Column (1.3)). Likewise, re-employment wage ratios are smaller, on average, for those who accept pay cuts than for those who reject them. This difference is statistically significant at the 95 percent level (Panel C, Column (2.3)), despite the modest sample size. And job losers who say they would accept proposed pay cuts do, in fact, experience mean wage drops on their new jobs (Panel A, Column (2.2)).[11]

In summary, respondents who accept hypothetical pay cuts have lower reservation wage ratios than those who reject pay cuts, and they experience lower re-employment wage ratios. Moreover, those who accept larger pay cuts have lower reservation wage ratios and lower re-employment wage ratios than those who accept smaller pay cuts. Reservation wages are also highly predictive of re-employment wages. Finally, we will show in Section 4.2 that residuals from a standard wage regression are powerful predictors of the willingness to accept job-saving pay cuts. These results indicate that our survey yields meaningful data on the labor market opportunities of unemployment benefit recipients and on their willingness to accept pay cuts to save their lost jobs.

4 The Willingness to Accept Pay Cuts to Save Jobs

We now quantify the willingness to accept pay cuts and characterize how it varies with observable attributes of workers, jobs, and employers. We also document a striking disjunction between openness to wage cuts among job losers and the absence of discussions about wage cuts to save jobs.

wage on the log reservation wage, we obtain an elasticity estimate of 0.84 (0.04). See Figure A2 for the corresponding scatter plots.

[11] Those who say yes to larger proposed pay cuts experience larger pay cuts upon re-employment (Panel A, Column (2.2)), but these differences are not statistically significant.

4.1 Evidence for Hypothetical Pay Cuts

Table 3 presents evidence on the willingness to accept pay cuts to save jobs. Sixty percent of UI recipients on permanent layoff say they would accept a pay cut of 5 percent to keep their previous jobs, and more than half would accept a 10 percent cut. Remarkably, one third would accept a pay cut of 25 percent. Among UI recipients on temporary layoff, 55 percent would accept a 5 percent cut to keep working, and more than one third would accept a cut of 20 or 25 percent. In short, most workers would accept small pay cuts to save lost jobs, and one third would accept cuts of 25 percent. These results hold in an economy that operated at or near full employment. Presumably, the willingness to accept pay cuts is even greater in a weaker economy.

Given our question design, the willingness of most UI recipients to accept pay cuts cannot be rationalized by the option value of search while employed. For persons on permanent layoff, we ask specifically about willingness to accept lower pay "for another 12 months." Thus, a respondent who accepts our proposed pay cut is **not** expressing an intention to quickly seek and find another job while working (at lower pay) on the lost job. Accordingly, we see the results in Table 3 as direct evidence that the wage on the lost job exceeds the value of worker time for most UI recipients. For one third of UI recipients, the wedge between the wage on the lost job and the value of time is at least 25 percent of the wage.

Table 3 also provides some information about worker rents. That forty percent of permanent job losers reject a five percent pay cut to save their lost jobs implies they enjoyed few or no rents. However, Table 3 also implies that the pre-layoff distribution of rents has a thick right tail. Thus, our data point to wide dispersion in worker rents among job losers just prior to layoff. It is reasonable to infer that a similarly wide dispersion in rents prevails for workers who remain employed but are near the layoff margin.

The first two columns in Table 4 present evidence on how the willingness to accept wage cuts varies with observables. Not surprisingly, there is less willingness to accept larger pay cuts. Respondents on permanent layoff are 28 percentage points less likely to accept a 25 percent cut than a 5 percent cut, conditional on controls. We find no statistically significant evidence that openness to pay cuts differs between men and women or varies by educational attainment or experience. However, hourly workers on permanent layoff are 8 percentage points less likely to accept wage cuts, conditional on the other controls. Persons with short tenures on their lost jobs are more open to pay cuts.[12]

One demographic characteristic stands out in Table 4: Black job losers are 11-12 percentage points more likely to accept pay cuts than white job losers. This is a large effect. It is

[12]For permanent layoffs, the estimated tenure effects shrink and become statistically insignificant when we rerun the Column (1) regression after dropping persons who refuse wage cuts because "the pay cut would feel like an insult." We more systematically investigate why some job losers refuse pay cuts in Section 5.2.

statistically significant at the 1 percent level for permanent layoffs and at the 10 percent level for temporary layoffs. This finding aligns well with other evidence that Blacks have less financial wealth and, as a result, exhibit greater sensitivity of consumption expenditures to income shocks. See Ganong et al. (2020) and their references to the literature. Insofar as job loss brings bigger consumption drops for Blacks, it makes sense that they are more willing to accept pay cuts to save jobs. That is exactly what we find.

4.2 Worker Rents and Willingness to Accept Pay Cuts

It is natural to hypothesize greater openness to pay cuts among UI recipients who enjoyed greater rents on their lost jobs. To operationalize this hypothesis, we consider three distinct rent variables. The first is an indicator for whether the lost job was covered by a union contract, based on survey responses to "Was the job that ended on [date] covered by a union contract?" This variable is motivated by a large body of evidence that union jobs often pay wages that exceed what union members can earn in other jobs. See, for example, Freeman and Medoff (1984) and Lewis (1986).

Our second rent variable follows a large literature that interprets industry wage differentials among observationally similar workers as rents that arise because of fairness concerns, employer desires to deter shirking, and the sharing of profits with employees. Prominent contributions include Akerlof (1982), Bulow and Summers (1986) and Krueger and Summers (1988). To quantify industry-level worker rents, we use the wage premiums for eighteen industries that Stansbury and Summers (2020, Figure A8) estimate from CPS micro data. Their estimated log wage premiums range from zero in Accommodations and Food Service to 0.26 in Mining. If these premiums reflect worker rents, we anticipate greater openness to job-saving pay cuts among workers who lost jobs in industries with higher premiums.

Our third rent variable is constructed as the worker-level residual in a standard Mincerian wage regression. Specifically, we obtain the residual from a regression of the lost-job log wage on a quadratic polynomial in potential experience (age minus years of schooling) and dummy variables for four levels of education, sex, six race/ethnicity categories, hourly pay on the lost job, and layoff status (permanent or temporary). This regression yields an R-squared value of 0.32. The standard deviation of the regression residual is 0.51.

Returning to Table 4, we add the rent variables to our statistical model for the willingness to accept pay cuts. Previous results continue to hold, and two of the rent variables have material effects on openness to pay cuts. Workers on temporary layoff are 17 percentage points less likely to accept pay cuts to keep working if they hold a union job, conditional on other variables in the model. (An explanation for this result emerges in Section 5.2 below when we probe why job losers refuse pay cuts.) In contrast, the estimated effect of union status on willingness to accept pay cuts is small and statistically insignificant for

those on permanent layoff. We do not read this result as clear evidence that union status is inconsequential for openness to job cuts among those on permanent layoff, given the sizable standard error on the estimated effect. The lack of precision here reflects our modest sample size (n=71) for union members on permanent layoff.

Surprisingly, we find no evidence that industry wage premiums help explain the willingness to accept job-preserving pay cuts. For permanent layoffs, the coefficient on the industry wage premium is wrong-signed relative to the hypothesized effect. This result is at odds with research that regards inter-industry wage differentials as indicative of worker rents. Perhaps industry-level wage premiums are subsumed in, and better measured by, our worker-level wage residuals. To assess this possibility, we re-estimated the Column (3) specification in Table 4 after dropping the worker-level wage residuals. In this modified specification, the coefficient on the industry wage premium changes to 0.38 (standard error of 0.38). Thus, the modified specification also yields little evidence that industry-level wage premiums help explain the willingness to accept job-preserving pay cuts.

Lastly, we find strong evidence that permanently laid-off workers with higher wage residuals are more open to job-preserving pay cuts. The point estimate says that an increase in the log wage residual of two standard deviations raises the willingness to accept a job-preserving pay cut by 12 percentage points ($2 \times 0.51 \times 0.12 \times 100$). This estimate may understate the true impact of worker-level rents on the willingness to accept job-saving pay cuts, because the simplicity of our wage model may yield a rather noisy measure of rents.

While striking, the effect of residual wages on openness to pay cuts can arise for multiple reasons that correspond to different sources of rents. For example, in settings with job-search frictions, a high wage residual could reflect fortuitous circumstances that let the worker reap an unusually large share of the surplus generated by the employment relationship.[13] As another example, even when a worker's wage exceeds the value of his productivity, firing costs can lead an employer to forego layoffs in the hope that productivity will improve or that the real wage will fall through inflation. In both situations, we expect individuals with higher wage residuals to be more open to job-preserving pay cuts. Fairness concerns and employer desires to deter shirking are other possible sources of worker rents. As yet another example, a high residual wage value could reflect a history of unusually high investments in match-specific capital before the shock(s) that triggered the layoff. Such investments can lead to a situation where the worker's pre-shock productivity value and wage on the lost job exceed his productivity value and wage on other jobs. In this example as well, we expect job losers with higher wage residuals to exhibit greater openness to job-saving wage cuts.

[13]That happens in the sequential bargaining setup of Cahuc, Postel-Vinay and Robin (2006) when the worker has the good fortune to receive an outside job offer that is nearly as attractive in productivity terms as his existing job. The re-bargaining triggered by the outside offer leaves the worker with most of the surplus.

4.3 The Rarity of (Discussions about) Pay Cuts to Save Jobs

The willingness of workers to accept job-saving wage cuts is even more striking when coupled with our next finding: Explicit discussions about cuts in pay, benefits, or hours in lieu of layoff are exceedingly rare. Overall, less than 3 percent of the job losers in our sample report discussions with their former employer about cuts in pay, benefits or hours as an alternative to layoff. The rarity of such discussions holds across industries, job tenure categories, education categories, firm size categories, for union and non-union workers, and by reason for layoffs. (See Table 5.) These discussions are rare even for job losers who express a willingness to accept large pay cuts. Moreover, job losers rarely experienced cuts in their pay, hours, or benefits in the months leading up to job loss, as discussed below.

Workers who lost jobs at small firms are somewhat more likely to have discussed possible cuts in pay, benefits or hours in lieu of layoff, consistent with Bewley (1999, pg. 172-173). Job losers with longer tenures and those who attribute layoffs to slow business conditions are also more likely to have had such discussions. These patterns make sense, because high-tenure workers are more likely to have accumulated match-specific human capital, and because slow business conditions allow more room for discussion and negotiation than business closures or terminations for cause. That said, all of the between-group differences in Table 5 are modest in absolute terms. The key result remains the rarity of discussions about cuts in pay, benefits or hours as an alternative to layoffs.

Actual pay cuts before job loss are also quite rare among UI benefit recipients. Figure 4 makes this point by displaying a histogram for the distribution of log hourly wage changes in the twelve months leading up to job loss. Fewer than 1.5 percent of job losers had a wage cut before their layoffs. More than half experienced no wage change in the twelve months before layoff, and 46 percent received a (nominal) wage increase.[14] The large spike at zero in Figure 4 is to be expected when nominal wages are sticky downward, given low inflation during our sample period and weak labor demand conditions for those soon to be laid off.

Among job losers with no wage change in the year leading up to layoff, three percent had benefit adjustments and three percent had a change in earnings due to a change in hours worked. Another 12 percent experienced some other form of compensation adjustment such as a change in bonuses or tips. Our data do not say whether the various non-wage adjustments were up or down, but they arose for only a small share of job losers.

To summarize, employer-worker discussions about cuts in pay, benefits or hours as an alternative to layoffs are exceedingly rare in our sample of UI benefit recipients. Nominal pay cuts in the twelve months leading up to job loss are also rare. Thus, there is little evidence that employers try job-saving wage cuts first, before resorting to layoffs. These findings are

[14] These results hold for job losers with one or more years of tenure at the time of layoff. Very similar results hold when restricting attention to persons with at least three years of tenure at the time of job loss.

especially striking in light of our evidence that most job losers express a willingness to accept modest pay cuts to save their jobs and one third would accept pay cuts of 25 percent.

As remarked in the introduction, we cannot use our sample to quantify the incidence of job-saving pay cuts in the broader population of workers or employment relationships. Successful efforts to implement job-saving pay cuts are outside the scope of our frame. Nevertheless, our results support the claim that discussions about job-saving wage cuts are rare. In this regard, two observations: First, *unsuccessful* discussions about pay cuts to save jobs *are* captured by our sample frame. Such discussions are quite rare, as we have shown. It follows that successful discussions of this sort are many times more common than unsuccessful ones, or that successful ones are also rare. Second, *some* successful efforts to implement job-saving pay cuts are indirectly captured by our frame. Specifically, if a job-saving wage cut is initially successful at preserving the employment relationship, but later shocks or other developments lead to a layoff, that layoff is in-scope for job losers that claim UI benefits. We will then see UI benefit recipients who experienced a pay cut in the months leading up to job loss. This type of outcome is also rare, as shown by Figure 4. In short, the evidence says that job-saving pay cuts are rare. We turn next to the question of why.

5 Why Are Wages Sticky on the Layoff Margin? Worker Views

In this section, we first present evidence on what job losers perceive about the reasons for wage stickiness at the point of separation. We then consider why many of them refuse pay cuts to save their lost jobs. Lastly, we use the data to estimate the share of lost jobs that potentially could be saved by pay cuts.

5.1 What Job Losers Perceive about the Reasons for Wage Stickiness

If the respondent had no discussion with his former employer about job-saving compensation cuts, we ask: ‘‘If you had to guess, why do you think your employer did not discuss any kind of cuts in pay, benefits or hours?’’ There are several response options, including an ‘‘Other’’ option that allowed for free-form text entries. Respondents could select more than one option, and about 8 percent did so. Table 6 summarizes the response distribution.

Several results stand out. First, 36 percent of the respondents believe the proposed pay cut would not have saved their jobs. This result is consistent with evidence in Bewley (1999, table 11.3) and with the view that many layoffs are bilaterally efficient. In other words, saving these jobs was not in the joint interest of the worker and employer.

Second, nearly four-in-ten job losers don’t know why their employers never raised the possibility of pay cuts in lieu of layoffs. This result suggests that many job losers don’t understand the business considerations that led to their layoffs. It also raises a question that

we did not foresee when designing the survey: Why, when faced with job loss, do so few workers initiate discussions about job-preserving pay cuts? Indeed, only 7 of 2,567 job losers in our sample say they initiated a discussion with their former employer about job-preserving pay cuts. Another 6 job losers say their coworkers or union did so. Apparently, the types of workers who claim UI benefits see themselves as passive actors in the wage-setting process. Why that is so, especially when many express an openness to pay cuts, is unclear to us. It would be useful to explore this matter in future work.

Third, eight percent of job losers say their employers don't cut pay, because it would lead the best workers to quit. Another eight percent say pay cuts would undermine worker morale. Employer-side surveys often uncover greater concerns about adverse selection and morale effects, as in Campbell and Kamlani (1997), Bewley (1999), and Bertheau et al. (2022). We offer a possible explanation for greater employer concerns about morale effects in Section 6.4. However, it's also the case that our survey differs from these employer surveys with respect to sample frames, institutional settings and labor market conditions.

Fourth, 45 percent of those who lost union jobs point to union wage contracts to explain why their former employers did not offer job-saving wage cuts. This finding says that collective bargaining is a major source of wage stickiness on the layoff margin in the union sector. It aligns with other evidence of downward nominal wage rigidities in the union sector. See Card (1990), Dickens et al. (2007), and Babecký et al. (2010), among others.

In contrast, few job losers point directly to company pay scales as a source of wage rigidity on the layoff margin. Even among those who lost jobs at large firms (500 or more employees), only 3 percent point to company pay scales as an impediment to discussions about job-saving wage cuts. Pay scales are important in other respects. Hazell et al. (2022), for example, find that wage offers for entry-level jobs are insensitive to local labor market conditions in many multi-establishment firms. In addition, the perceived concerns in our data about adverse selection on the quits margin point to a form of internal pay scales. Otherwise, companies would use individualized pay cuts to prevent adverse selection.

As previewed in the introduction, we find only a tiny role for minimum wage and benefit laws as a source of downward wage rigidity on the layoff margin. In particular, less than one percent of job losers report that compensation cuts would violate minimum wage laws, and less than two percent report that benefit cuts would violate the law. Moreover, only four percent report an hourly pay rate on their lost job less than 1.05 times the legal minimum.

5.2 Why Job Losers Refuse Hypothetical Pay Cuts

When job losers refuse our proposed wage cuts to save their jobs, we ask why. Here as well, we present several response options, including an "Other" option with free-form entries. 23 percent of the respondents select more than one option. Table 7 reports the results.

Consider UI benefit recipients on permanent layoff who refuse the proposed pay cut. Half of these respondents refuse because they can find another job that pays better. Another five percent select "Other" and enter an explanation that we classify under "I am/would be underpaid." Recall from Table 3 that four-in-ten permanent job losers refuse the proposed pay cuts. Thus, about 22 percent of all permanent job losers see their outside job options as superior to the proposed pay. For these cases, our results indicate that saving the job was not in the joint interest of worker and employer, although we cannot rule out the possibility that a smaller job-saving pay cut might be acceptable to both worker and employer. Similar remarks apply for refusers on temporary layoff, 26 percent of whom say they can find another job that pays more than the proposed wage.

One fifth of refusers on permanent layoff report that not working is preferable to working at the proposed pay, and another seven percent select "Other" and supply a written explanation that we classify under "Can't afford the pay cut." 24 percent of refusers on temporary layoff also report that not working is preferable to working at the proposed pay, and about one percent say they "Can't afford the pay cut." In short, more than a quarter of the refusers see the proposed wage as too low to elicit their labor supply. For these cases as well, our results suggest that saving the job was not in the joint interest of worker and employer.

Thirty-eight percent of refusers on permanent layoff say accepting the proposed pay cut would feel like an insult, and 24 percent of refusers on temporary layoff say the same. Recall that among those who would accept our proposed wage cuts, eight percent say employers don't offer them because it would undermine morale. In the same spirit, Bewley (1999) finds that many managers avoid job-saving pay cuts because they could be perceived as insulting. These various results resonate with theories that stress fairness norms and morale effects, as in Akerlof (1982) and Akerlof and Yellen (1990). According to these theories, productivity suffers when workers feel insulted or wrongly treated by their pay. As a result, it can be unprofitable for an employer to implement wage cuts, even if the new wage lies below the pre-wage-cut value of the worker's product and above the value of the worker's outside options. In these circumstances, the negative morale effects of wage cuts stand in the way of achieving bilaterally efficient outcomes on the layoff margin.

Table 7 contains two other noteworthy results. First, 16 percent of the refusers on temporary layoff from union jobs point to a "union agreement" as the reason for their refusal. This finding reinforces our earlier conclusion that collective bargaining is an important source of sticky wages on the layoff margin in the union sector.

Second, one third of job losers on temporary layoff from non-union jobs—and nearly half of those on temporary layoff from union jobs—refuse temporary wage reductions because they fear the temporary cut could become permanent. These fears make sense if accepting a temporary cut leads to a downward revision in the employer's assessment of the worker's

reservation wage. That would weaken the worker's bargaining position going forward. Private information about the opportunity value of the worker's time is an essential element of this interpretation. Alternatively, acquiescing to a temporary pay cut might undermine norms about what constitutes a fair wage level, or it could lower the reference wage in future negotiations. These interpretations differ greatly in their details, but they share a common implication: Workers may refuse a temporary wage cut even when the proposed wage exceeds the (current) opportunity value of the worker's time. That outcome also constitutes a violation of bilateral efficiency in the employment relationship—one that manifests as an excess of temporary layoffs rather than an excess of permanent separations.

Recall from Table 4 that union workers are much less willing than non-union workers to accept temporary wage cuts to avoid temporary layoffs. This differential is almost entirely explained by greater fears among union workers that a temporary wage cut might become permanent. To see this point, we consider job losers on temporary layoff and calculate the share that refuses our proposed wage cut *and* points to fears that a temporary cut might become permanent as the reason for refusal. This share is 33 percent for those on temporary layoff from union jobs and 17 percent for those on temporary layoff from non-union jobs. The differential of 16 percentage points is nearly as large as the conditional mean difference between union and non-union workers in the willingness to accept temporary pay cuts that we estimated in Table 4.

5.3 How Many Layoffs Could Be Avoided by Pay Cuts?

The foregoing analysis suggests that many layoffs (and many successful claims for UI benefits) could be avoided by wage cuts. To assess how many, we start by considering job losers who meet two conditions: First, they would accept the proposed wage cut. Second, they believe the proposed cut would save their lost job. To flag the second condition, we identify job losers who do *not* say "It would not have prevented my layoff" in response to the question, "If you had to guess, why do you think your employer did not discuss any kind of cuts in pay, benefits or hours?"

Before proceeding, consider how this exercise relates to a minimalist model of job separations. Let r^W denote the worker's reservation wage on the lost job—i.e., the lowest wage such that he wants to keep the lost job, given his outside options and the job's non-wage characteristics. Likewise, let r^E denote the highest wage such that the employer wants to retain the worker. If $r^E > r^W$, the employment relationship yields a positive flow surplus and its continuation is bilaterally efficient. Moreover, it is individually optimal for each party to continue the relationship for any wage $\tilde{w} \in [r^W, r^E]$. If $[r^W, r^E]$ is empty, ending the employment relationship is bilaterally efficient.

Now consider a survey respondent with wage w on the lost job. If the respondent accepts

our proposed wage cut of X percent to save the job, it means that $[1-(X/100)]w \geq r^W$. If the respondent also believes the proposed pay cut is large enough to save the job, it means that $[1-(X/100)]w \leq r^E$. Thus, if both conditions hold for a given lost job, we infer that $[r^W, r^E]$ is a non-empty set and that $[1-(X/100)]w \in [r^W, r^E]$. In other words, we have identified a layoff that could be avoided by a suitable pay cut.

In practice, we find that 28 percent of UI benefit recipients meet both conditions. The corresponding figure is somewhat higher for women, non-union jobs, layoffs by larger employers, and persons with low tenure on the lost job (Table A7). It is only 14 percent when the respondent reports ''Going out of business'' as the reason for the layoff. Looking across industries, layoffs that meet both conditions range from 22 percent in Transportation, Warehousing & Utilities and 23 percent in Leisure & Hospitality to 31 percent in Education & Health Services and 32 percent in Finance, Insurance & Real Estate.

In short, the data suggest that job-saving wage cuts are in the joint interest of worker and employer for more than a quarter of UI recipients. This is a striking result, but it comes with three cautions. First, some respondents who do not meet both conditions might do so if presented with a higher or lower wage cut (smaller or larger X). In this respect, the 28 percent figure is biased down. We can get a sense for the size of this bias by inspecting how the share of job losers that meet both conditions varies with the size of the proposed wage cut (Table A8). For permanent layoffs, the share ranges from 35 percent at a 5 percent cut to 22 percent at a 25 percent cut. For temporary layoffs, it ranges from 41 percent to 24 percent. These results suggest that more than 35 percent of layoffs could be avoided by suitable pay cuts. Even the 35 percent figure is biased down, because it does not reflect wage cuts that are tailored to the specific circumstances of each layoff.

Second, recall from Table 6 that nearly four-in-ten job losers ''Don't know'' why their employer did not discuss wage cuts in lieu of layoff. If they knew, they might say the proposed wage cut would not save their lost job. In this respect, the 28 percent figure is biased up. To assess the potential size of this bias, suppose the share of proposed wage cuts that ''would not have prevented my layoff'' is the same for those who ''Don't know'' and those who do, and that ''Don't know'' status is uncorrelated with whether the proposed wage cut would lead the employer to forego the layoff. Then the implied share of layoffs that would be avoided by the proposed wage cut is 17 percent.[15] A more conservative approach treats all of the ''Don't knows'' as ''would not prevent the layoff.'' That assumption yields a figure of 10 percent for the avoidable share of layoffs.

We also implement a hybrid approach that integrates the adjustments for upward and downward bias. Specifically, we assign some or all ''Don't knows'' to ''would not prevent

[15] We obtain this value from a simulation that randomly assigns the ''Don't knows'' to the other categories listed in Table 6. We then re-compute the share of job losers who meet both conditions.

layoff'' and look across the wage cut categories. As before, a wage cut of 5 percent yields the highest share for layoffs that could be avoided by suitable pay cuts. That share is 24 percent when the ''Don't knows'' are uncorrelated with the acceptability of pay cuts to employers and 13 percent when we treat all ''Don't knows'' as ''would not prevent the layoff.'' We regard the 24 percent figure as our preferred estimate for the share of layoffs that could be avoided by suitable pay cuts.

The third, and most important, caution is that our estimates for avoidable layoffs rely on worker assessments of whether the proposed wage cuts would lead the employer to forego layoffs. Worker perceptions in this regard may diverge from employer views. A sizable divergence would alter our estimates and warrant careful study in its own right. Tackling this matter requires a dataset that elicits *from job losers* their willingness to accept job-preserving wage cuts and *from employers* their willingness to forego layoffs in exchange for pay cuts. While that might seem like a demanding data requirement, we note that every state-level unemployment insurance system in the United States operates an administrative record system that is ideally suited to function as a frame for this type of two-prong sample design. We hope to implement that design in future work.

Our evidence on worker views about the scope for job-preserving pay cuts complements the evidence on employer views presented in Bertheau et al. (2022). They survey nearly 2,500 Danish firms, document how they adjusted labor inputs and compensation during the 2020 pandemic crisis, and probe how firms see the choice between wage cuts and layoffs. According to their Figure 1, 19 percent of firms implemented permanent layoffs in 2020, and 5 percent implemented temporary layoffs. When asked ''Why didn't you lower pay instead of laying off employees?'' (Figure 8), 59 percent of the firms with layoffs say ''Pay cuts would not have saved jobs.'' When asked ''What reduction in the total salary cost (base pay and bonuses) could have prevented layoffs?'' (Figure 9), 18 percent of firms with layoffs say 0-20 percent and another 18 percent say 21-60 percent.

Bertheau et al. (2022) appear to conclude that there is limited scope for job-saving pay cuts. However, it's unclear whether their evidence is at odds with our findings.[16] They do not offer a specific estimate for the share of layoffs that could be avoided by pay cuts. Perhaps more important, economic conditions differ greatly between their study and ours—a major, pandemic-induced downturn in their setting, in contrast to a tight labor market amidst relatively stable economic conditions in our setting. Whether, and how much, the scope for job-preserving pay cuts varies with cyclical conditions is an open and important question.

[16] 61 percent of the firms in their sample ''Do not know'' what size pay cut would prevent layoffs. So, among those that profess to know, 46 percent of firms with layoffs say a pay reduction of 0-20 percent would prevent the layoff.

6 Discussion

6.1 Assessing the Efficient Separations View

The ''efficient separations'' view holds that all layoffs, quits and retentions are privately efficient in the sense of maximizing joint employer-worker surplus. Barro (1977, page 311) explains the logic: ''In the absence of any 'transaction costs' that would, for example, inhibit the contractual arrangements for making side payments between firms and workers, it is apparent that the employment rule would be selected in order to maximize the total pie possessed by the two parties.'' Under this view, ''quits'' and ''layoffs'' tell us which party initiated the separations, but the quit-layoff distinction lacks deeper significance. McLaughlin (1990, 1991) develops this theme in detail and interprets data on quits and layoffs through the lens of the efficient separations view.

Barro (1977) stresses that sticky (flow) wages can give rise to an ''apparent nonwage rationing of jobs'' when, in fact, the employment rule equates the marginal product of labor to the marginal value of the worker's time in all states of nature. Thus, we cannot confidently leap from the abundant evidence of nominal wage stickiness to the conclusion that wage rigidities cause inefficient separations. Moreover, researchers rarely have direct observations on the value of a worker's product and the value of his time. That makes it hard to assess whether sticky wages drive privately inefficient separations. Our empirical design cuts through these challenges in two ways: First, layoffs are clearly consequential for employment, and permanent layoffs also foreclose the possibility of future ''side payments.'' For layoffs then, it is informative to consider flow wages at the point of separation. Second, our survey directly probes whether sticky wages produce layoffs in situations where the perceived value of the worker's product on the lost job exceeds the value of his time.

Our survey data let us assess the efficient separations view on the layoff margin under the maintained assumption of strictly bilateral employment relationships. We can then evaluate (private) efficiency one layoff at a time—i.e., without reference to whether the firm has other workers, what it pays them, or their outside options. We need not concern ourselves with cross-worker dependencies in the production function or in compensation arrangements. That is our approach in Section 5.3. We find for about one-quarter of the layoffs in our sample that there exists a new, lower wage such that the worker and employer would want to continue the employment relationship. Thus, many layoffs violate the efficient separations condition that holds in models founded on bilateral employment relationships.

We think this result is important, because it says that we cannot fully understand why layoffs happen unless we drop the efficient separations view, or step away from the influential class of theories that posit strictly bilateral employment relationships. We consider both possibilities below.

6.2 Theories that Stress Private Information about Outside Options

Hall and Lazear (1984) consider bilateral employment relationships in settings where the employer has private information about the value of the worker's output in the match and the worker has private information about the value of his outside options.[17] When only one party has private information, efficiency on the separations margin is achieved by placing the wage-setting decision in the hands of that party. When both parties have private information, the second-best contract involves real wage rigidity and an excess sensitivity of layoffs (and quits) to labor demand shocks relative to the full-information benchmark. That is, privately inefficient separations arise in some states of the world.

Malcomson (1997) revisits their analysis, allowing for investments that are specific to the employment relationship. He reaches the same conclusion about inefficient separations for the case of two-sided private information. He also notes that employers will make no costly relationship-specific investments if the employee has the sole power to set wages. As Malcomson (1997, pg. 1948) remarks, "A fixed wage contract can typically improve on that." Thus, the combination of private information about the worker's outside option and the need to incentivize specific investments by the employer also give rise to second-best contracts whereby sticky wages generate privately inefficient layoffs in some states of the world.

These explanations for (consequential) wage stickiness on the layoff margin are appealing, because they build on assumptions that are apt for many, perhaps most, employment relationships. In particular, private information about the value of match output and private information about the value of outside options appear to characterize many employment relationships. It is also evident that employers often make specific investments in their employment relationships.

Nevertheless, our results present a challenge for theories that stress the role of private information in the context of bilateral employment relationships. To see why, consider an ongoing employment relationship with an established wage. Suppose there is a shock (or other development) that reduces the value of the worker's product to the employer. Both parties see the shock, although the worker may be uncertain about how much it reduces the value of his product.[18] This shock lowers the employer's optimal wage offer and the worker's optimal wage demand. Thus, we anticipate efforts to re-negotiate to a lower wage in reaction to this type of shock. (If both parties think the shock also reduces the value of the worker's outside options, it only strengthens their incentives to negotiate a wage cut.) While the

[17] In the context of our minimalist model in Section 5.3, the worker has private information about r^W and the employer has private information about r^E.

[18] In the model of Hall and Lazear (1984), this shock can be specified as an additive reduction in M, the value of the worker's marginal product in the employment relationship. In the model of Perry and Solon (1985), the shock can be specified as a leftward additive shift in $f(m)$, which is the worker's subjective distribution over the value of his product to the employer.

re-negotiation may or may not prevent a layoff, the point is that these theories lead us to anticipate discussions about wage cuts in an effort to prevent (some) layoffs. Yet, as we saw in Section 4.3, employer-worker discussions about compensation cuts as an alternative to layoffs are exceedingly rare.

To be sure, re-negotiating pay is costly. So, if neither the employer nor the worker sees some prospect for discussions about wage cuts to lead to a continuation of the employment relationship, neither party would initiate a discussion. More generally, if *both* parties see meager prospects in this regard, modest costs of discussion and re-negotiation could deter the employer and the worker from broaching the subject of job-preserving wage cuts. This line of argument explains the absence of employer-worker discussions about wage cuts when both parties see little surplus over which to negotiate. It may well explain why some layoffs happen when job losers are willing to accept (only) small pay cuts to save their jobs.

That said, we do not think this argument explains our results. Recall that more than 35 percent of job losers say they would accept wage cuts of 20 or 25 percent to save their lost jobs (Table 3). This is not cheap talk, because the same job losers experience mean wage reductions of 20 log points upon re-employment (Table 2). In addition, among those same job losers, the mean reservation wage at the time of the Entry Survey is 15 log points below the wage on the lost job (Table 2). So, they have information that, if revealed to their employer, might save their job. Finally, more than a fifth of job losers would accept a 25 percent wage cut, *and* they believe a pay cut of that size would save their lost job (Table A8). When at least one party sees that much room for job-preserving wage cuts, it is not plausible that the direct time (and aggravation) costs of re-negotiation are large enough to inhibit employer-worker discussions. There must be other important forces at work—perhaps in combination with private information—that deter discussions about the possibility of job-saving pay cuts.

Although they do not draw attention to it, Bertheau et al. (2022, Figure 9) also uncover a head-scratching result from the perspective of theories that stress private information. When asked how large a pay cut could have prevented layoffs, 61 percent of firms with layoffs say they "Do not know." Why, then, don't these employers propose wage cuts as an alternative to impending layoffs? We see no answer to this question in the class of theories the stress private information in bilateral employment relationships as the source of wage stickiness. In contrast, it's easy to understand why employers don't explore the possibility of pay cuts if they are precluded by law or collective bargaining agreements, if employers think pay cuts would violate fairness norms or their internal pay structures, or if employers believe that pay cuts would lead to other problems and costs relative to layoffs.

6.3 Other Explanations for Sticky Wages on the Layoff Margin

Other explanations for sticky wages on the layoff margin include the idea that wage cuts violate fairness norms, thereby triggering drops in effort and productivity (Akerlof and Yellen, 1990); the view that wage cuts undermine employee morale, with negative effects on productivity (Bewley, 1999); the idea that wage cuts hurt workforce quality by inducing adverse selection on the quits margin (Weiss, 1990); the related idea that layoffs, unlike across-the-board pay cuts, let the employer choose which employment relationships to terminate; the observation that company-level compensation policies and collective bargaining agreements often feature pay structures with limited flexibility; and theories of how insider-outsider conflicts can lead to inefficient wage structures and rigid separation policies (Lindbeck and Snower, 2002). Many of these other explanations depart from a strictly bilateral perspective on compensation, separations, and other aspects of the employment relationship.

Our results provide at least limited support for several of these ideas. When asked, ''why do you think your employer did not discuss any kind of cuts in pay, benefits or hours'' as an alternative to layoffs, 8 percent of job losers say it would lead the best workers to quit (Table 6). Another 8 percent say it would undermine morale. Among those who lost union jobs (16 percent of our sample), 45 percent say ''it's not allowed under the union contract'' and another 3 percent point to the ''employer's pay scale'' as the reason. Among job losers on permanent layoff who refuse our proposed wage cuts, 38 percent say ''The pay cut would feel like an insult'' (Table 7).

Employer-side surveys typically find support for these ideas as well, but often with a greater frequency of expressed concerns about morale effects and fairness norms. For example, when Bertheau et al. (2022) ask ''What are the main reasons for not lowering the contractual base pay?", 73 percent of Danish firms agree that it would ''damage morale,'' 61 percent see ''base pay as a commitment,'' and 51 percent worry about ''adverse productivity consequences.'' We point to one reason for the high frequency of employer concerns about fairness norms and morale effects in our case-study discussion below.

In a field experiment, Kube, Maréchal and Puppe (2013) find that surprise wage cuts (relative to an initial offer wage) cause large productivity drops in the cataloging of library books. The productivity drop took the form of a slower work rate rather than a greater incidence of cataloging errors. In contrast, surprise wage hikes (again relative to an initial offer wage) do not yield productivity gains relative to baseline. This pattern of results suggests that wage cuts relative to a reference wage lead workers to exert less effort and produce less per unit time. Mas (2006) finds a decline in the performance of police departments after they lose in final offer arbitration. As Kube, Maréchal and Puppe (2013) discuss, many laboratory experiments also find a positive relationship between wages and effort.

Kaur (2019) offers perhaps the most compelling evidence that fairness norms underpin

downward nominal wage rigidity, with negative consequences for employment in some states of the world. However, her setting—markets for casual daily agricultural workers in rural India—is far removed from that of most employment relationships in a modern advanced economy. The same concern pertains to laboratory experiments and most field experiments as well. Still, it is noteworthy that worker surveys, employer surveys, laboratory experiments, field experiments, and research that exploits plausibly exogenous variation in observational data all point to fairness norms and morale effects as forces that can inhibit job-saving wage cuts in circumstances where they would be bilaterally efficient.

6.4 Lessons from Case Studies

Krueger and Mas (2004) provide an in-depth study of high defect rates in automobile tires manufactured by Firestone in the mid-1990s. Ultimately, defect problems led Firestone to recall 14 million tires. Krueger and Mas amass strong evidence that labor strife at the company's Decatur, Illinois plant was a major reason for the high defect rates. For our purposes, their most notable finding is that defect rates rose at the Decatur plant around the time the company announced it would depart from industry-wide "pattern bargaining" and demand concessions in wages, benefits, and work schedules to take effect a few months later—after the then-current union contract expired. This example illustrates how the act of proposing compensation cuts can undermine productivity.

As Krueger and Mas (2004, page 254) note, the National Highway Traffic and Safety Administration "reported that the Firestone tires under investigation were related to 271 fatalities and more than 800 injuries." In their statistical analysis, Krueger and Mas also find that unusually high defect rates in Firestone tires led to excess fatalities and injuries. When product defect costs are high, the potential for retaliation and performance shortfalls by even a small share of workers can become major deterrents to wage cuts. That is especially so when defects are hard to detect before they cause major harm, and when it is hard to determine who is responsible for defects. Wage cuts can be unprofitable in these circumstances, even when they would be acceptable to most members of an employer's workforce.

The Firestone case suggests why employers might be more concerned about fairness norms and morale effects than the average worker. Specifically, if product defects (or deliberate sabotage) are sufficiently costly, a plan for job-saving wage cuts that is both profitable and acceptable to most employees can be derailed by fears of how a few aggrieved employees might retaliate. If those few can be identified in advance and terminated, the best available action may be to fire them and cut wages for others. If they cannot be identified in advance, or if it is infeasible to selectively fire them, broad layoffs can be the best feasible action. That can be true even when layoffs are bilaterally inefficient for most employer-worker pairs.

Lee and Rupp (2007) examine on-time flight performance at US airlines in reaction to

large, permanent wage reductions for pilots. They interpret on-time flight performance as a proxy for unobservable pilot effort and focus their investigation on the first few years after the 9-11 terrorist attacks. As they remark (pages 726-27), ''every large U.S. 'legacy' carrier experienced substantial wage reductions as a result of bankruptcy, negotiations under the threat of bankruptcy, or a contractually mandated arbitration decision between a carrier and its pilots' union.'' Carrier-wide wage cuts ranged from 8 to 33 percent and took effect at various times from July 2002 to May 2005. On a cumulative basis, the smallest pilot wage cut at any legacy carrier was 15 percent.

Lee and Rupp find only modest, short-lived drops in on-time flight performance after these carrier-level pilot wage cuts took effect.[19] Even the short-lived drops in on-time performance were limited to non-bankrupt airlines. Bankrupt carriers did not see on-time performance drops. Apparently, it is sometimes possible to implement large, permanent wage cuts without generating a persistent, material productivity drop.

The circumstances surrounding these pilot wage cuts are distinctive, or at least unusual, in several respects. First, airline financial distress during this period was obvious, widely recognized, and arguably not the ''fault'' of the airlines or their management. Second, legacy airline pilots were highly paid even after the pay cuts. For both reasons, perceptions of unfairness would seem less salient in this setting than in many others. Bewley (1999) also states that ''...pay cuts were more easily accepted when employees understood that company problems justified them'' (pg. 202), and ''a company can reduce raises or even cut pay if it has serious problems'' (pg. 378). The apparent role of airline financial distress is also broadly consistent with Kahneman, Knetsch and Thaler (1986), who find that respondents consider pay cuts acceptable if they avert firm losses but not if they simply raise profits.

Third, there was little scope for adverse selection on the quits margin to derail pilot wage cuts during this period. Pilots, especially pilots at legacy carriers, were paid much more than they could earn in other jobs because of their highly specialized skills (Hirsch and Macpherson, 2000). Seniority-based pay schedules also made it financially unrewarding for pilots to quit one carrier to start flying for another carrier. In contrast, Sandvik et al. (2020) find that pay cuts for sales staff in a call center raised quit rates by the most productive employees. Finally, disgruntled pilots may have little opportunity to impose large costs on their employers without, at the same time, jeopardizing their own safety. In that respect, the situation for airline pilots differs from that of Firestone's production workers.

[19] Lee and Rupp (2007, Table 8) also report large positive abnormal stock returns for the pay-cutting carrier in reaction to 10 of 12 announcements of pilot wage-reduction agreements. The average carrier-level abnormal return over a three-day window around the announcement date is 22 percent. Thus, the wage-reduction agreements did not simply confirm prior expectations.

7 Concluding Remarks

Our data and analysis reveal insights that are hidden from employer surveys and previous studies. For example, we find that more than half of unemployment benefit recipients would accept wage cuts of 5-10 percent to save their lost jobs, and one third would accept a 25 percent cut. Employer reluctance to cut wages to save jobs becomes more puzzling when so many job losers are open to the idea. We also find that worker-employer discussions about cuts in pay, benefits or hours in lieu of layoffs are exceedingly rare. To our knowledge, we are the first to document the disjunction between widespread worker-side openness to job-saving wage cuts and a pervasive unwillingness of employers to even broach the subject.

When asked why employers don't discuss ''any kind of cuts in pay, benefits or hours'' as an alternative to layoffs, some job losers say it would undermine morale or lead the best workers to quit. Among those who reject our proposed wage cuts, 38 percent do so because ''the pay cut would feel like an insult.'' These results echo findings in employer surveys, but concerns about fairness norms and morale effects in our survey data are less frequent than what typically emerges from employer surveys. Our discussion of the Firestone case study by Krueger and Mas (2004) offers a possible reconciliation. Specifically, the high costs and deadly consequences of defective tires illustrate how retaliation and performance shortfalls by a few workers can derail job-saving pay cuts for the larger workforce. That remains true even when layoffs are bilaterally inefficient for most employer-worker pairs.

Jobs in the private sector covered by union wage agreements account for about 15 percent of all layoffs in our sample and about three quarters of the temporary layoffs. Forty-five percent of these union job losers say contractual restrictions prevent employers from offering wage cuts in lieu of layoffs, and another three percent point to employer pay scales. In addition, 15 percent of union job losers who refuse our proposed wage cuts explain that it would violate the union agreement. These results suggest that collective-bargaining agreements are a major source of sticky wages on the layoff margin in union jobs.

We also use our survey data to look for job-saving pay cuts that are small enough to be acceptable to the job loser *and* large enough (in the worker's estimation) to save the job. We find pay cuts that meet both conditions for about one-quarter of the layoffs. This result is a material departure from the efficient separations benchmark within the class of theories that focus on strictly bilateral employment relationships. That class informs much economic thinking about job separations, frictional unemployment, wages and job ladders. In concurrent research, Jäger, Schoefer and Zweimüller (2022) find large deviations from the efficient separations benchmark via an entirely different analysis. Our evidence is also hard to square with theories that stress private information in models that focus on bilateral employment relationships. More broadly, our findings point to the need for theories

of compensation policies and practices at the level of organizations (rather than bilateral matches) to fully understand why layoffs happen.

It's worth re-iterating that our findings pertain to a single state (Illinois) with a diversified economy during a period of low inflation and tight labor markets. The role of sticky wages on the layoff margin surely varies over time and space. In this regard, several hypotheses warrant more attention in future work: (1) Workers are even more open to job-saving wage cuts during recessions. (2) High inflation, as in 2021 and 2022, relaxes the bite of sticky wages on the layoff margin. (3) Collective bargaining raises the incidence of layoffs that violate the bilateral condition for privately efficient separations. (4) Performance-based pay and other flexible forms of compensation (bonuses, tips, equity options, etc.) reduce the incidence of such layoffs. (5) Concerns about fairness norms and the negative morale effects of wage cuts are more common—and a more important force in deviations from bilateral efficiency—when sub-par worker performance is costlier to the employer, harder to detect before negative consequences manifest, and harder to source to specific individuals.

The survey approach developed in this paper is suitable for tackling hypotheses (1) to (4). Tackling (5) requires a two-prong sample design that asks job losers *and* their former employers about the same layoff events and, indeed, about the same wage-reduction events. This two-prong design will also yield more powerful evidence regarding the other hypotheses. For example, asking both job losers and their former employers about their openness to job-saving pay cuts would yield sharper inferences about why layoffs happen and whether employer and worker views diverge in this regard. Although unusual, this type of two-prong sample design is entirely feasible. Every state-level unemployment insurance system in the United States operates an administrative record system that tracks unemployment benefit recipients and links them to their former employers. The same is true for unemployment insurance systems in many other countries.

Ultimately, we see this line of research as providing stronger empirical foundations for theorizing about wage stickiness and layoffs. It may also yield insights into how managerial practices, third-party mediation efforts, and policy changes can reduce the communication, coordination, contractual and other frictions that inhibit wage adjustments on the layoff margin. Insights in this regard could reduce the frequency of layoffs and the volume of claims on the unemployment insurance system.

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Figure 1: Survey timing and sample design

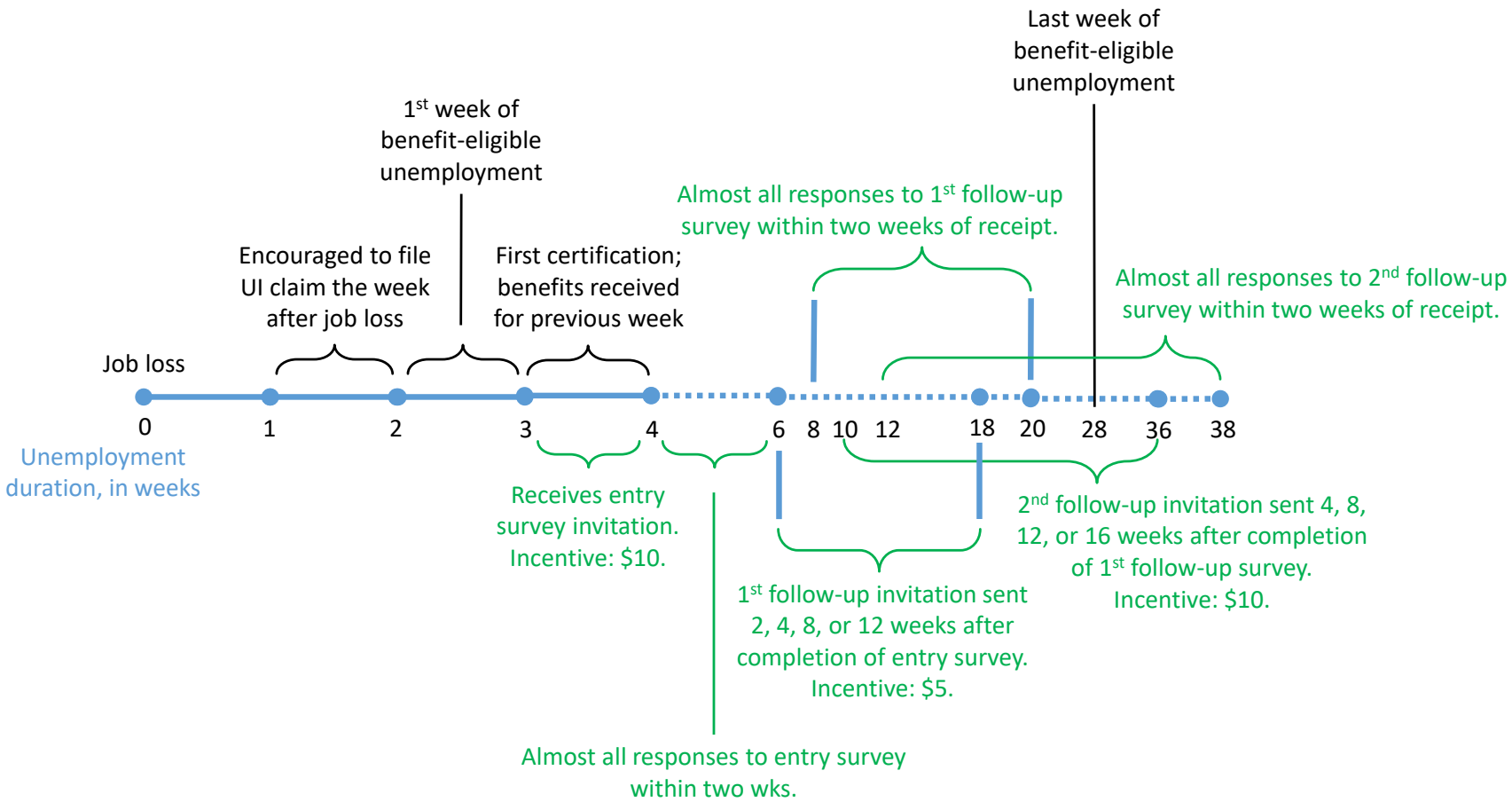
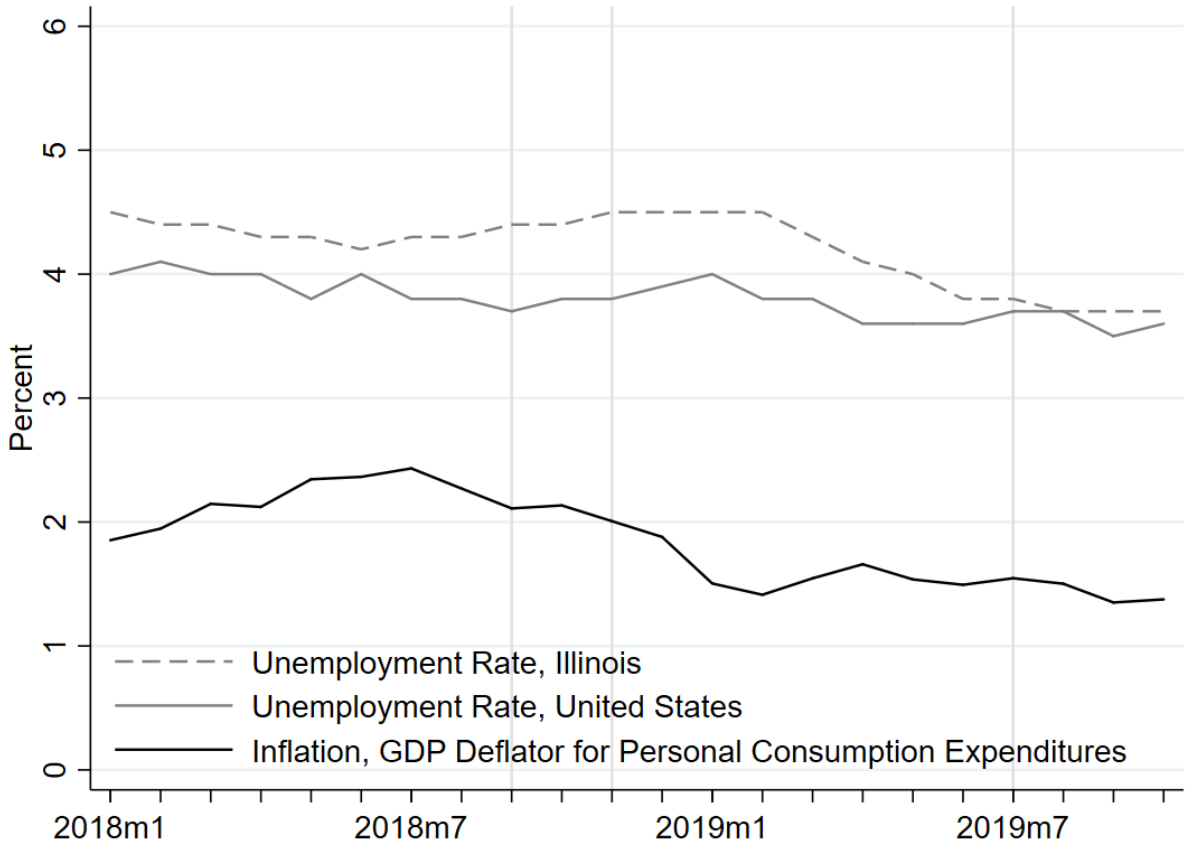
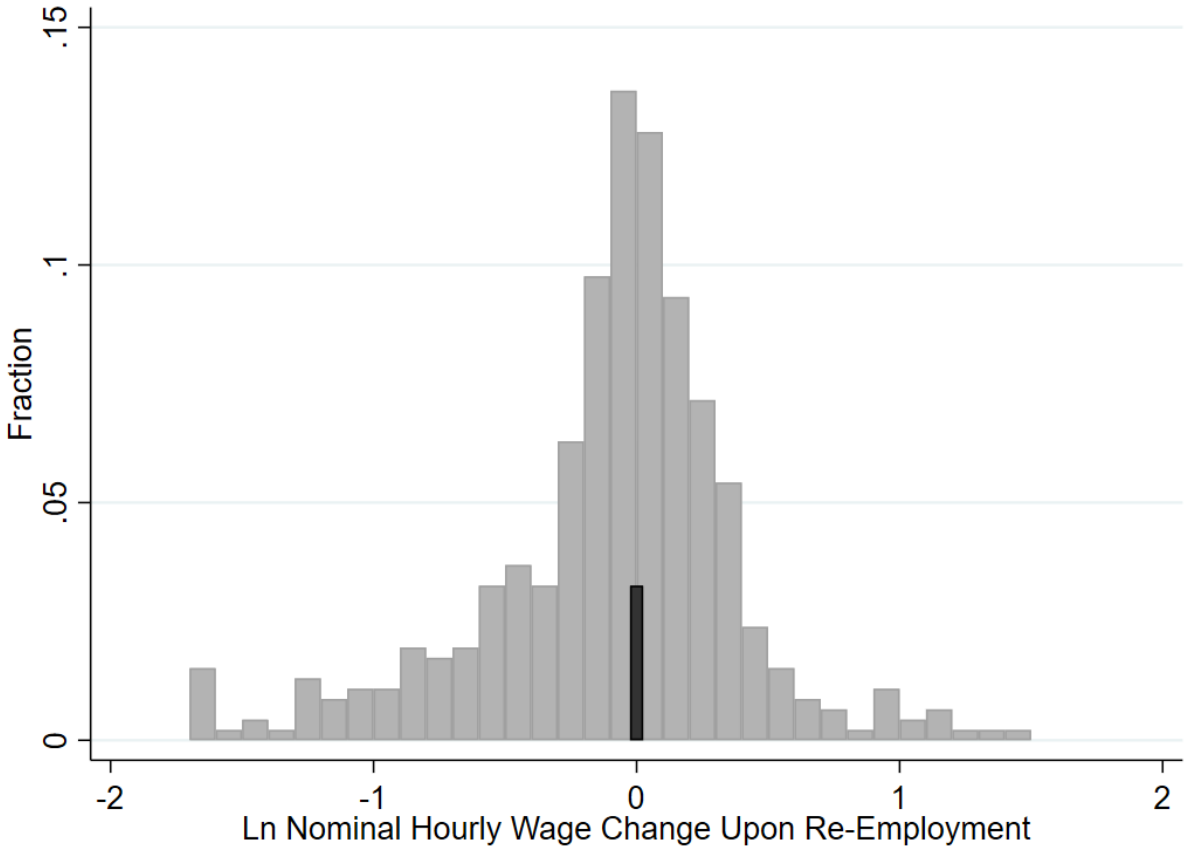


Figure 2: Survey sample period and seasonally adjusted unemployment and inflation rates



Note: Seasonally-adjusted unemployment rates in the U.S. and Illinois from January 2018 to October 2019 and the 12-month percent change in the headline personal consumption expenditure (PCE) price index. The first vertical line depicts when our Entry Survey invitations were initially sent out (September 2018), the second vertical line depicts when our last Entry Survey invitations were sent out (November 2018), and the third vertical line depicts the last month we sent out invitations to our Follow-Up Surveys (July 2019). Data for the U.S. are reproduced from statistics published by the U.S. Bureau of Labor Statistics reports using the Current Population Survey (BLS, 2019). Data for Illinois are reproduced from the Bureau of Labor Statistics' Local Area Unemployment Statistics program (LAUS, 2019). See section 3.3 for details.

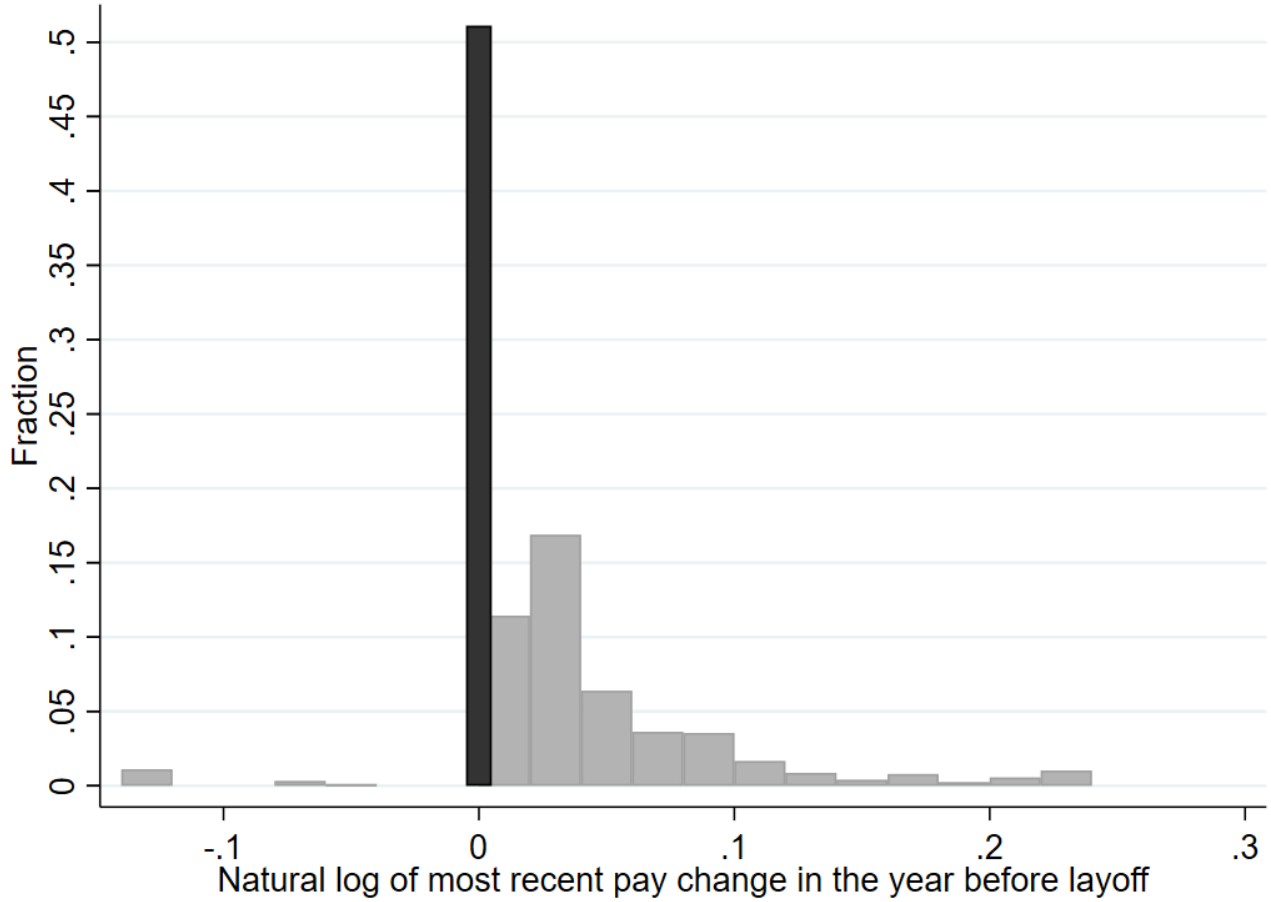
Figure 3: The distribution of wage changes for re-employed job losers



Note: This figure presents the histogram of wage changes for respondents who experienced a permanent layoff **and** found a new job in the time frame covered by our survey. The wage change measure is the natural log of the ratio (nominal hourly wage on the new job)/(nominal hourly wage on the lost job). We drop observations with hourly or reservation wages below $2 or above $200 and then winsorize at the 1st and 99th percentiles of the log wage changes. The width of each bin is 0.1 except for the solid black bar, which denotes no wage change at re-employment. The mean (median) of the natural log of the wage ratio is -0.068 (-0.016).

Figure 4: Histogram of log wage changes on the lost job in the prior year

Questions: ''When was the most recent change to your pay or benefits on your previous job?'' and ''How much did your salary or hourly wage change?''



Note: We restrict to individuals with at least one year of tenure on their lost job. We drop observations for which the hourly or reservation wages are below $2 or above $200. We then winsorize log wage change values at the 1st and 99th percentiles. Width of each bin is 0.2. The black bar denotes the same pay and benefits throughout an individual's previous job. Gray bars are for individuals reporting a change in their compensation due to a change in their salary or hourly wage. See section 4.3 for details.

Table 1: The Entry Survey analysis sample: Percentage distributions and comparison to the Current Population Survey

Statistics	(1) Unweighted	(2) Weighted	(3) CPS (US)
Previous employment data			
Previous industry (percent)			
Leisure and hospitality	6.3	6.6	12.5
Finance, insurance, real estate	9.4	7.9	4.4
Construction	5.3	7.4	14.1
Education and health care services	16.7	12.6	17.5
Information and other services	9.3	8.4	5.9
Manufacturing	20.1	25.7	8.6
Mining	0.3	0.4	0.6
Prof., technical, business services	12.1	8.9	13.5
Retail and wholesale trade	9.2	9.9	11.2
Transp., warehousing, utilities	6.1	7.2	5.8
Government or military	1.0	1.0	2.4
Agriculture, forestry, fishing	1.5	1.7	3.7
Data missing	2.6	2.4	0.0
Demographic data (percent of total)			
Female	52.4	42.5	43.1
Age in years			
18-24	6.2	7.9	18.1
25-34	26.3	29.0	24.1
35-44	22.8	24.5	19.1
45-54	22.6	20.1	17.4
55-64	19.2	15.8	14.3
65 or older	3.0	2.6	6.9
Race/Ethnicity			
White, non-Hispanic	63.3	61.1	51.3
White, Hispanic	5.2	5.7	21.0
Black	16.6	18.0	20.1
Asian	3.4	2.4	2.9
Other	4.7	5.4	4.6
Data missing	6.9	7.3	0.0
Education			
High school grad.	13.7	21.0	35.4
Technical training/some college	28.2	40.9	21.4
Associate/bachelor's degree	41.0	29.1	19.3
Grad. degree or higher	16.2	7.7	7.1
Avg. unemployment duration (weeks)	5.3	5.1	2.5
No. of observations	2,567	2,567	3,820

Note: Column (1) reports raw percentages, and Column (2) reports percentages after reweighting the sample to match the distribution of job losers with ongoing unemployment spell durations of less than five weeks for the cross product of two age groups, two education groups, and sex. Appendix A.7 explains how we construct the weights. Column (3) reports the corresponding U.S. percentages in the CPS. Education categories sum to less than 100 because not all categories are listed.

Table 2: Reservation and re-employment wage ratios, UI recipients on permanent layoff

	(1) Log reservation wage ratio			(2) Log re-employment wage ratio		
	(1.1) Obs.	(1.2) Mean	(1.3) p-values	(2.1) Obs.	(2.2) Mean	(2.3) p-values
Panel A. Workers who accept pay cuts						
Accept 5% wage cut	235	-0.09	<0.001	57	-0.077	0.30
Accept 10-15% wage cut	390	-0.10	<0.001	84	-0.106	0.09
Accept 20-25% wage cut	291	-0.15	<0.001	59	-0.20	0.007
Accept wage cut of any size	916	-0.11	<0.001	200	-0.12	0.002
Panel B. Workers who reject pay cuts						
Reject 5% wage cut	150	0.048	0.046	41	-0.11	0.14
Reject 10-15% wage cut	416	0.051	<0.001	93	0.046	0.35
Reject 20-25% wage cut	530	0.025	0.046	127	-0.054	0.21
Reject wage cut of any size	1,096	0.038	<0.001	261	-0.027	0.37
Panel C. Hypothesis tests						
Same wage ratio for those who accept and reject pay cuts	2,012		<0.001	461		0.046
Same wage ratio for those ... by size of pay cut	2,012		<0.001	461		0.073
Same wage ratio across pay cut categories: Accepts	916		0.05	200		0.47
Same wage ratio across pay cut categories: Rejects	1,096		0.37	261		0.15
Panel D. Full-sample summary statistics						
Mean	2,012	-0.031		461	-0.069	
Standard deviation	2,012	0.297		461	0.520	

Note: Column (1) reports statistics about the log reservation wage ratio, in which the reservation wage is measured by responses to the following question: "Suppose someone offered you a job today that is suitable in terms of hours, skills, responsibilities and non-wage benefits. What is the lowest wage or salary, before taxes and deductions, you would accept?" Column (2) reports statistics about the log re-employment wage ratio for persons who find new jobs during the time frame covered by our survey. All p-values are for two-sided tests of the nulls. The wage cut question was "Would you have been willing to stay at your last job for another 12 months at a pay cut of X percent?" We drop observations for which the hourly wage on the lost job or the reservation wage is below $2 or above $200. We then winsorize log wage ratios at the 1st and 99th percentiles. See section 3.5 for details.

Table 3: Percent of UI recipients who would accept a pay cut to save the lost job

For permanent layoffs: *"Would you have been willing to stay at your last job for another 12 months at a pay cut of X percent?"*
For temporary layoffs: *"Suppose your employer offered a temporary pay cut of X percent as an alternative to the temporary layoff. Would you have been willing to accept the temporary pay cut to avoid the layoff?"*

Size of proposed pay cut	5%	10%	15%	20%	25%
Permanent layoffs	60.6	52.3	43.7	38.4	32.4
	(2.4)	(2.5)	(2.5)	(2.4)	(2.3)
	404	413	410	419	423
Temporary layoffs	54.5	42.9	35.8	34.3	37.4
	(5.0)	(5.0)	(4.9)	(4.7)	(4.9)
	101	98	95	102	99

Note: For each type of layoff, we order the rows as follows: Percent of UI recipients who say they would accept the proposed wage cut, the standard error of the estimate, and the number of observations.

Table 4: How the willingness to accept pay cuts varies with observables

Dependent variable = 1 if respondent accepts pay cut, 0 otherwise.

	(1)	(2)	(3)	(4)
Type of Layoff →	Permanent	Temporary	Permanent	Temporary
Proposed pay cut				
10%	-0.09**	-0.14*	-0.09**	-0.16**
	(0.04)	(0.07)	(0.04)	(0.07)
15%	-0.18***	-0.18**	-0.17***	-0.21***
	(0.04)	(0.07)	(0.04)	(0.07)
20%	-0.24***	-0.19***	-0.23***	-0.21***
	(0.04)	(0.07)	(0.03)	(0.07)
25%	-0.28***	-0.19**	-0.28***	-0.19***
	(0.04)	(0.07)	(0.03)	(0.07)
Individual characteristics				
Female	0.01	0.04	0.01	0.02
	(0.02)	(0.05)	(0.02)	(0.05)
Black	0.12***	0.11*	0.12***	0.12*
	(0.03)	(0.06)	(0.03)	(0.07)
Experience	0.01*	-0.01	0.01*	-0.01
	(0.00)	(0.01)	(0.00)	(0.01)
Experience^2	-0.00	0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Tenure on the lost job				
6mos to 2yrs	-0.08**	-0.06	-0.08**	-0.08
	(0.03)	(0.06)	(0.03)	(0.06)
2yrs to 5yrs	-0.08**	-0.06	-0.08**	-0.05
	(0.04)	(0.08)	(0.04)	(0.09)
More than 5yrs	-0.05	-0.21***	-0.05	-0.17**
	(0.04)	(0.07)	(0.04)	(0.07)
Other variables				
Paid hourly (Yes=1)	-0.08***	-0.03	-0.08***	-0.03
	(0.03)	(0.05)	(0.03)	(0.05)
Weeks unemployed	-0.00	0.00	-0.00	0.00
	(0.00)	(0.01)	(0.00)	(0.01)
Rent variables				
Industry wage premium			-0.10	0.77
			(0.39)	(1.15)
Union job (Yes=1)			-0.02	-0.17***
			(0.05)	(0.06)
Wage residual			0.12***	-0.00
			(0.02)	(0.05)
Mean of dependent variable	0.46	0.38	0.46	0.38
Standard deviation of dep. var.	0.50	0.48	0.50	0.48
R2	0.07	0.12	0.08	0.15
Observations	1,909	418	1,909	418

Note: We fit linear probability regression models by OLS. The models include the race/ethnicity and education indicators in Table 1, but we do not show the statistically insignificant ones. The omitted category is a non-Hispanic white man who has at most a high school diploma and who had less than six months of job tenure at layoff. Industry wage premia are from Stansbury and Summers (2020, Figure A8). Wage residuals are from a Mincerian wage equation, as described in the text. Standard errors in Columns (3) and (4) are computed by bootstrapping the Mincerian wage estimation and the second-stage estimation with 1,000 replications. Marginal effects at the mean in probit models are similar. (*** p<0.01, ** p<0.05, * p<0.1)

Table 5: Percent of respondents who discussed a cut in pay, benefits or hours in lieu of layoff

For permanent layoffs: *"Before your employer let you go, was there any discussion about possible cuts to pay, benefits or hours to save your job?"*
For temporary layoffs: *"Did you and your employer discuss a cut in pay, benefits or hours as an alternative to a temporary layoff?"*

	Mean	S.E.	Count
Overall	2.8	0.3	2,567
Type of layoff (p-value: 0.03)			
Permanent	2.4	0.3	2,070
Temporary	4.2	0.9	497
Gender (p-value: 0.16)			
Male	2.3	0.4	1,223
Female	3.2	0.5	1,344
Education (p-value: 0.31)			
High school grad.	3.7	1.0	352
Technical training/some college	2.2	0.5	724
Associate/bachelor's degree	3.1	0.5	1,052
Grad. degree or higher	1.9	0.7	416
Industry (p-value: 0.79)			
Leisure and hospitality	3.7	1.5	162
FIRE	2.5	1.0	241
Construction	2.2	1.3	136
Educ. & Hlth. care services	2.3	0.7	428
Info. & other services	2.5	1.0	240
Manufacturing	3.7	0.8	517
Prof., tech., bus. services	1.6	0.7	311
Retail & wholesale trade	4.2	1.3	237
Transp., warehousing, utilities	2.6	1.3	156
Union job (p-value: 0.84)			
No	2.8	0.4	2,070
Yes	2.6	0.8	382
Tenure (p-value: 0.97)			
0-6mons	2.5	0.7	472
6mons to 2yrs	2.7	0.5	861
2yrs to 5yrs	2.8	0.7	536
More than 5yrs	3.0	0.6	698
Reason for layoff (p-value: 0.02)			
Slow business conditions	4.9	0.9	636
Going out of business	3.0	1.3	167
Reorganization/pos. abolished	2.2	0.6	641
Fired	1.5	0.5	653
Firm size (p-value: 0.02)			
1-49	4.1	0.7	927
50-499	2.0	0.5	845
500+	2.5	0.6	651

Note: We consider the null hypothesis of equal coefficients across the indicated categories (e.g., education groups) and report the p-value in parentheses.

Table 6: Percent of respondents by reason for why no discussion occurred about cuts in pay, benefits or hours

Question: ''If you had to guess, why do you think your employer did not discuss any kind of cuts in pay, benefits or hours?''

	(1) It would lead the best workers to quit	(2) It would undermine morale	(3) It would not have prevented my layoff	(4) It's not allowed under union contract	(5) It would violate minimum wage laws	(6) Benefits cut would violate the law	(7) It would upset the employer's pay scale	(8) Don't know	(9) Other
Panel A. All responses									
Overall									
Count= 2,496	8.4	8.0	36.3		0.9	1.5	2.4	38.9	9.4
	(0.6)	(0.5)	(1.0)		(0.2)	(0.2)	(0.3)	(1.0)	(0.6)
Permanent layoff									
Count= 2,020	9.0	9.1	37.7		1.0	1.6	2.3	39.3	10.7
	(0.6)	(0.6)	(1.1)		(0.2)	(0.3)	(0.3)	(1.1)	(0.7)
Temporary layoff									
Count= 476	5.9	3.2	30.3		0.6	1.3	2.7	37.2	3.8
	(1.1)	(0.8)	(2.1)		(0.4)	(0.5)	(0.7)	(2.2)	(0.9)
500+ employees									
Count=635	8.5	9.8	34.3		0.6	1.9	3.0	34.6	10.2
	(1.1)	(1.2)	(1.9)		(0.3)	(0.5)	(0.7)	(1.9)	(1.2)
Union job									
Count=372	4.6	3.2	26.1	44.6	1.1	2.4	3.2	29.3	4.6
	(1.1)	(0.9)	(2.3)	(2.6)	(0.5)	(0.8)	(0.9)	(2.4)	(1.1)
Panel B. Permanent layoffs: Other detail									
	Employer cost cutting	Bankruptcy	Job outsourced, automated or abolished	Fired for cause or poor performance	Age, gender or race	Miscellaneous	No or uninformative response		
	(Count=22)	(Count=6)	(Count=38)	(Count=20)	(Count=11)	(Count=46)	(Count=74)		
	1.1	0.3	1.9	1.0	0.5	2.3	3.7		

Note: The first entry in each cell is the percent of responses with standard errors in parenthesis. Respondents could select multiple options, so row values need not sum to 100 percent. Column (4) considers only persons who lost jobs covered by union contracts. See section 5.1 for details.

Table 7: Percent of respondents by reason for refusing a pay cut

For permanent layoffs: *"What are the reasons why you would not accept a pay cut of X percent to avoid being laid off?"*
For temporary layoffs: *"What are the reasons why you would not accept a temporary pay cut of X percent to avoid being temporarily laid off?"*

	Count	(1) Can find another job that pays more	(2) The pay cut would feel like an insult	(3) I prefer not working over working at a lower pay level	(4) Other	(5) Uninformative response
Panel A. Permanent layoffs						
A. Permanent layoffs from non-union and union jobs	1,102	50.1 (1.5)	38.0 (1.5)	20.5 (1.2)	19.4 (1.2)	4.4 (0.6)
A.1. Permanent layoffs from non-union jobs	1,031	50.6 (1.6)	38.4 (1.5)	19.8 (1.2)	19.4 (1.2)	4.5 (0.6)
A.2. Permanent layoffs from union jobs	71	42.3 (5.9)	32.4 (5.6)	31.0 (5.5)	19.7 (4.8)	4.2 (2.4)
		Can't afford the pay cut	I am/would be underpaid	Bad fit, unsatisfactory conditions, long commute	Contract violation	Miscellaneous
A.3. Other detail for permanent layoffs from non-union jobs	200	7.6 (0.8)	4.6 (0.6)	3.5 (0.6)	0.5 (0.2)	3.3 (0.6)
		Can't afford the pay cut	Union agreement	Miscellaneous		
A.4. Other detail for permanent layoffs from union jobs	14	7.0 (3.1)	11.3 (3.8)	1.4 (1.4)		
		(1) Can find another job that pays more	(2) The pay cut would feel like an insult	(3) I prefer not working over working at a lower pay level	(4) Other	(5) Uninformative response
Panel B. Temporary layoffs						
B. Temporary layoffs from non-union and union jobs	271	26.2 (2.7)	24.0 (2.6)	24.4 (2.6)	56.8 (3.0)	4.4 (1.3)
B.1. Temporary layoffs from non-union jobs	89	44.9 (5.3)	23.6 (4.5)	28.1 (4.8)	39.3 (5.2)	4.5 (2.2)
B.2. Temporary layoffs from union jobs	182	17.0 (2.8)	24.2 (3.2)	22.5 (3.1)	65.4 (3.5)	4.4 (1.5)
		The temporary cut might become a permanent one	Can't afford the pay cut	Miscellaneous		
B.3. Other detail for temporary layoffs from non-union jobs	35	32.6 (5.0)	3.4 (1.9)	3.4 (1.9)		
		The temporary cut might become a permanent one	Can't afford the pay cut	Union agreement	Contract violation	Miscellaneous
B.4. Other detail for temporary layoffs from union jobs	119	47.3 (3.7)	0.5 (0.5)	15.9 (2.7)	2.2 (1.1)	0.5 (0.5)

Note: The first entry in each cell is the percent of responses among individuals not willing to accept a pay cut with standard errors in parenthesis. Respondents could select all that apply so the rows do not have to sum to 100 percent. See section 5.2 for details.

Appendix Materials

A Survey Details and Data Cleaning

In this appendix we present details about the sample frame, the survey methodology, the UI system in Illinois when our survey was in the field, and data coding and trimming.

A.1 Sample Frame

Our sample population includes all beneficiaries of initial UI claims in the state of Illinois between September 10 and November 24, 2018, with valid e-mail addresses. As described in section 3.2, this includes about 30,500 individuals. As described in appendix A.3, during the week of September 17, IDES did not send out invitation e-mails, which gave us time to make adjustments to the survey and incentives based on the first week of data. During the period September 24 to November 24, IDES sent out approximately 28,000 e-mails, and, because about 88 percent of all initial claimants have a valid e-mail address, this e-mail count implies that about 32,000 individuals received first payments during this period. See Stantcheva (2022) for a recent review about survey methods.

Initial UI claims data from the Employment and Training Administration suggest that during the weeks ending September 1, 2018 and November 3, 2018, there were roughly 80,000 initial UI claims filed in the state of Illinois (DOLETA, 2019).[20] This means that roughly 40 percent of initial UI claims result in a first benefit payment, and about 35 percent of initial claimants in the state of Illinois during our survey received our Entry-Survey invitation. Correspondence with state UI officials in Illinois suggests that the gap between initial claims and first payments is a result of ineligibility, as well as pending adjudication, failure to certify on time or at all, and self-denial, which includes ineligibility based on reported wages or a claimant who is not able and available for work and seeking work.

A.2 Invitation and Reminder E-mails

IDES sent out e-mails to individuals receiving initial benefit payments after filing an initial UI claim between September 10, 2018 and November 24, 2018. These invitation e-mails briefly described the research study and invited initial claimants to participate. The e-mail made clear that the survey was not run by IDES, that participants were not required to take the survey, and that participation would not have any effect on an individual's UI claim. Individuals were informed that their identifying information would not be used in any analysis or published results and that it would not be provided to anyone else. The invitation mentioned the survey was administered by the University of Chicago and the Federal Reserve Bank of Cleveland in partnership with a third-party survey provider. Participants were asked to complete the survey within two days of receiving the invitation e-mail. We could not send reminders about completing the Entry Survey because we did not have access to claimants' e-mail addresses until an individual completed the Entry Survey. We closed the Entry Survey on December 2, 2018.

[20] We choose these dates in the initial claims data because claims take at least two weeks to process and issue a first benefit payment.

Invitations to Follow-Up Surveys were sent on Fridays between September 24, 2018 and July 7, 2019, and Entry-Survey respondents were assigned to follow-up waves by a randomization procedure described in appendix A.5. Follow-up invitation e-mails addressed participants by their first names, assured participants that their data would remain confidential, and encouraged them to complete the survey within two days of receiving the invitation e-mail. The invitation briefly described the research study and the benefits and risks associated with survey participation.

Reminders for the follow-up invitations were sent on Tuesday and Friday, 4 and 7 days after the original invitations. These reminders encouraged participants to complete the survey as soon as possible. We also sent out two (one) final reminders to all participants who had received invitations to the first (second) Follow-Up Survey but had not completed the survey for at least two weeks. These final reminder e-mails were sent out on January 25 and March 8, 2019 for the first Follow-Up Survey and June 28, 2019 for the second Follow-Up Survey. We closed the first Follow-Up Survey on March 17 and we closed the second Follow-Up Survey on July 15.

A.3 Incentives

After the first week of Entry-Survey invitations (September 10 to 14), IDES paused for a week to allow us to evaluate the survey design, completion rates, and the quality of incoming data. IDES resumed sending Entry-Survey invitations on September 24 through November 24.

During the evaluation period the incentive for survey participation was a $5 Amazon gift card and the completion rate was under three percent. We decided to increase the incentive payment to a $10 Amazon gift card. The overall completion rate for weeks after our evaluation period was almost 10 percent, so the increased incentive payment more than tripled our completion rate.

Incentive payments for the Follow-Up Surveys began at $5, but we raised the incentive payment for the second Follow-Up Survey to $10 after two weeks of completed surveys from the first follow-up wave (4 weeks). This change did not seem to materially change our completion rates. The incentive payment for the first Follow-Up Survey remained at $5 throughout the duration of the survey.

A.4 Online Survey Instruments

The Entry and Follow-Up Survey instruments were developed chiefly by the authors with assistance from numerous individuals with survey design expertise.

The Entry Survey included questions about workers' last jobs, such as when they were laid off, their tenure, their industry and occupation, the number of people who worked at their previous location, usual hours, and union status. The Entry Survey also included questions about pay, adjustments to compensation in the months leading up to the layoff, and the reason for separation. Questions about compensation discussions prior to layoff and workers' willingness to accept pay cuts in lieu of layoff were the focus of the Entry Survey. We also elicited workers' reservation wages, their expectations about how their reservation wages may evolve with unemployment duration, and their willingness to relocate to start a new job. We also asked individuals to rate the importance of various job characteristics when evaluating a

new job, including child care arrangements, commuting time, and job security. We collected demographic information once, at the end of the Entry Survey. To receive their incentive payment, in the form of an Amazon gift card, workers had to provide their e-mail address. We used these e-mail addresses to contact respondents with invitations to Follow-Up Surveys if they gave us permission to do so. Some respondents opted out of Follow-Up Surveys. One individual who completed the first Follow-Up Survey was accidentally not invited to the second follow up.

Entry-Survey respondents received invitations to Follow-Up Surveys based on their reported e-mail addresses. At the start of the Follow-Up Surveys we used information from the Entry Survey to verify that the same person was filling out the surveys.

Follow-Up Surveys determined the labor force status of individuals through a series of questions, and the survey was tailored separately for the recalled, those working for an employer, the self-employed, and those looking for work. For employed individuals we gathered much of the same information as for the previous job in the Entry Survey, including industry and occupation information, usual hours, and pay. For those looking for work, we asked about job search activity, job offers, and reservation wages. We did not gather information about job search activity in the Entry Survey, chiefly because we did not want to upset a respondent by indirectly broaching the legitimacy of their UI claim and thereby casting doubt on the intentions of our survey. After verifying a respondent's e-mail address, they were sent their incentive payment.

A.5 Randomization of Follow-Up Wave Assignment

Upon completion of the Entry Survey, individuals were randomly assigned to follow-up waves of 2, 4, 8, and 12 weeks. For example, if a respondent completed the survey during week 1 (any time during that week Monday through Sunday) and they were assigned to the 2-week follow-up wave, then they would get invited on the Friday of week 3 to their first Follow-Up Survey. A similar procedure was implemented for invitations to the second Follow-Up Survey, but the waves were 4, 8, 12, and 16 weeks after completion of the first Follow-Up Survey.

Given this randomization, the maximum unemployment duration in our sample is 38 weeks if we allow for a maximum of two weeks to respond to each survey. This 38 weeks includes at most 4 weeks between job loss and an invitation to our Entry Survey (see section 3.1), at most 12 and 16 weeks for the invitations to our first and second Follow-Up Surveys, and 6 weeks to fill out these three surveys. Indeed, the 98th percentile of the unemployment duration distribution in our sample is 38 weeks, suggesting that very few individuals either received our Entry Survey invitation later than 4 weeks into their unemployment spell or took slightly longer than two weeks to respond to our surveys.

A.6 Data Coding and Trimming

We recode reported earnings, reservation wages, and expected reservation wages in two ways. First, if an individual reports making more than $15,000 per hour, we recode their response to be at the annual frequency. This recode affected 37 gross pay observations. Second, if an individual reports hourly earnings of $300 or more, but less than $15,000, we consider their response to be in cents and divided it by 100. This recode affects 78 gross pay observations.

We trim observations of hourly gross pay, reservation wages, and expected reservation wages below $2 or above $200. Sometimes we winsorize changes in gross pay and reservation

wage ratios below the the 1st and above the 99th percentile. When we apply this winzorization we make note of it in the main text. To be consistent with KM, we set to missing reservation wages for those who are employed but still looking for other work.

We construct unemployment duration by taking the difference between the survey completion date and the worker's reported last day at their previous job. We top code unemployment durations that are greater than 30 weeks during the Entry Survey because job losers must file a claim within 6 months (26 weeks) of job loss and we allow up to 4 weeks for an individual to receive our Entry Survey. Unemployment duration is set to missing for employed workers. We did not ask about labor force status during the Entry Survey because we were worried that workers, who recently received a UI benefit payment, may not respond truthfully and that such a question would jeopardize truthful responses to the rest of our survey instrument. As such, we assume that all workers are unemployed during the Entry Survey.

We calculate potential experience using a person's age less their years of schooling, derived from their highest level of completed education. We collected individual's ages in brackets (18 to 24, 25 to 34, . . . , 65 or older) so we impute a respondent's age to the middle of each age bracket.

Several of our questions offered the option to write in a response, such as the individual's industry and occupation of work and reason for layoff. We hand coded some of these observations to our list of displayed choices and sometimes we created new categories of responses if sufficiently many individuals responded in a similar way. For example, many individuals reported maintenance work and repair at their previous employer as the reason for their temporary layoff. Because this was not one of our original options, we created a new category.

Our conclusions are not affected by any of these recodes or trimming of data.

A.7 CPS Weights

Since we lack access to administrative UI records, we cannot reweight to match Illinois UI benefit claimants. Instead, we use CPS data from June 2018 to February 2019, which were the months when our Entry Survey was fielded with three additional months on each end. We use these data to compute national CPS shares in eight bins defined by: young (less than 45 years old) and old (no less than 45 years old), less (no bacherlor's degree) and more educated (bacherlor's degree or graduate degree), and male and female We reweight each observation in our sample by the share of CPS individuals in each of these bins over the share of our Entry-Survey respondents in each of these bins.

For individuals in our survey that did not reveal their education or age, we impute their response. In particular, we use a multilogit regression with independent variables including gender, temporary layoff status, race, and dummies for previous occupation and industry, to separately predict respondents' age category and educational attainment. We impute a respondent's age and education based on which category is most likely given their observable characteristics. None of our respondents have missing gender.

These weights are little changed if we use CPS individuals who are less than 5 weeks unemployed and not new entrants, who are unlikely to be eligible for UI.

B Results Using CPS Weights

In this section we present our main results with the CPS weights described in section 3.4. Our four substantive findings are not affected by the use of CPS weights.

First, many workers are willing to accept pay cuts, and often large ones, to stay on their previous job, as shown in table A3. About 60 percent of workers on permanent layoff were willing to accept a 5 percent pay cut in lieu of layoff and about one third were willing to accept a 25 percent pay cut. These proportions are similar to those when we do not use weights (see table 3).

Second, discussions about possible cuts in compensation to avoid layoff are exceptionally rare and this lack of discussions is pervasive across the economy, as shown in table A4. About three percent of respondents discussed a cut in pay, benefits or hours in lieu of layoff when we use the weighted sample. The fraction of workers who report having compensation discussions prior to layoff does not vary much by industry, union coverage, tenure, reason for layoff, or firm size, similar to the unweighted results (see table 5).

Third, the fraction of respondents reporting various reasons for why no compensation discussions occurred is similar when using weighted and unweighted data (compare table A5 with table 6). In particular, about one third of workers believe that adjustment to their compensation would not have prevented their layoff, and workers often report adverse selection and morale.

Fourth, the reasons workers report for not accepting a pay cut are similar when using weighted and unweighted data (compare table A6 with table 7). The most common reason, reported by about half of non-union workers is that they can find another job that pays more. And about one third of workers permanently laid off from union and non-union jobs report that the pay cut would feel like an insult. Temporary layoffs often express a fear that the proposed temporary pay cut would become permanent.

Appendix references

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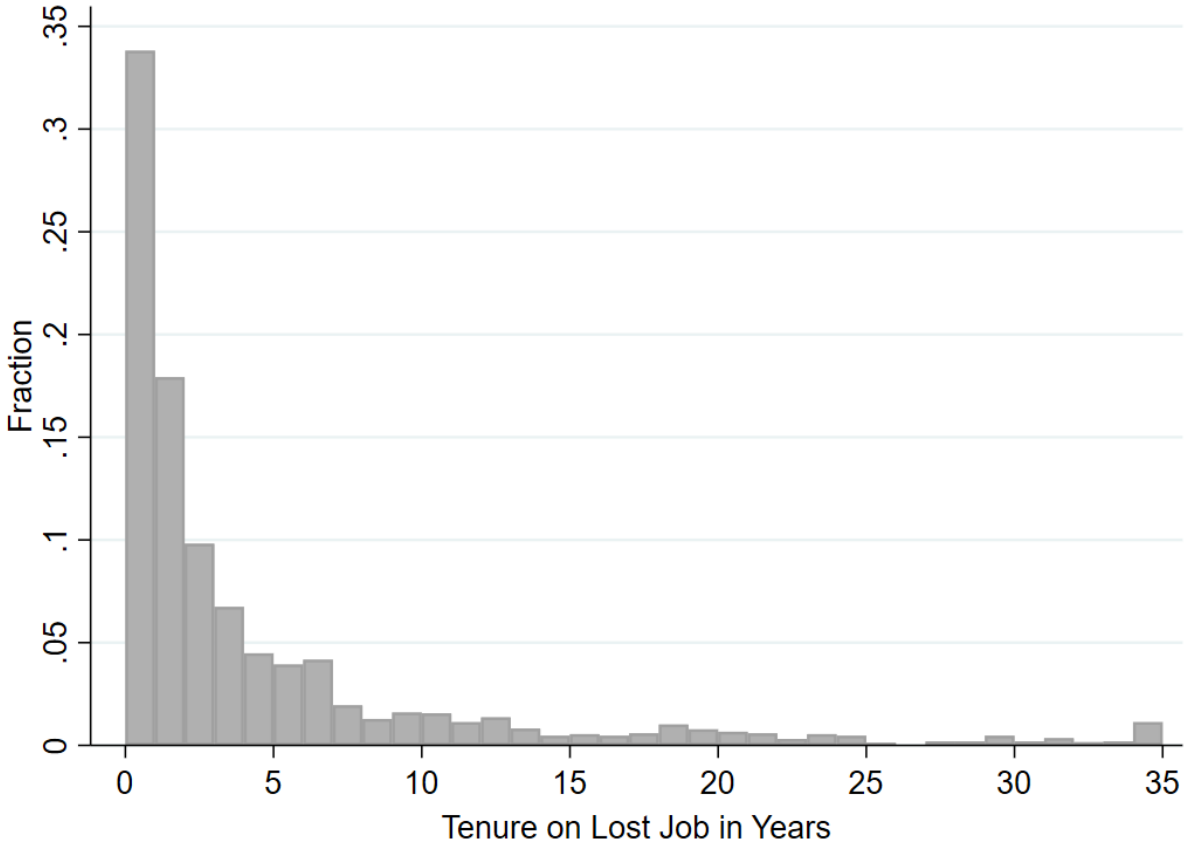
Figure A1: Tenure distribution for Entry-Survey sample

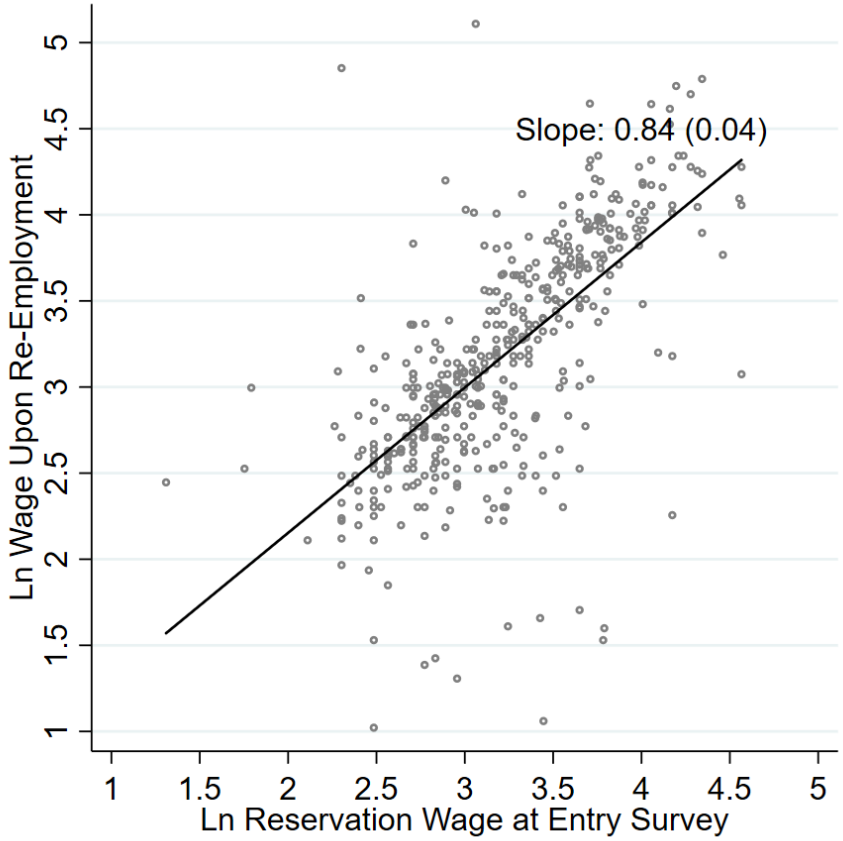


Note: We winsorize observations above the 99th percentile. See section 3.4 for details.

Figure A2: Reservation and re-employment wages

(a) Hourly wage ratios

(b) Hourly wages



Note: Figure A2a presents a scatter plot of the natural log of the re-employment wage ratio (as defined in Figure 3 and Section 3.5) against the natural log of the reservation wage ratio (as defined in Section 3.5). Figure A2b presents a scatter plot of the natural log of the re-employment wage against the natural log of the reservation wage. The sample covers respondents who experienced a permanent layoff **and** found a new job in the time frame covered by our survey waves. In both figures we drop observations for which the reported hourly or reservation wages are below \$2 or above \$200, and we then winsorize log wage ratios in Figure A2a at the 1st and 99th percentiles. The black lines are linear regressions fit with a constant and the log reservation wage ratio (log reservation wage) on the right side in Figure A2a (Figure A2b).

Table A1: Response rates for entry and follow-up surveys

		E-mailed invitations	Incomplete responses	Complete responses	Click-thru rate	Completion rate
Panel A. Entry survey						
		30,571	2,421	2,777	17.0%	9.1%
Number opting out of Follow-Up Surveys		197				
Panel B. 1st follow-up survey						
	Wave					
	Week 2	641		412		64.3%
	Week 4	654		407		62.2%
	Week 8	644		356		55.3%
	Week 12	641		329		51.3%
	Total	2,580	84	1,504	61.6%	58.3%
Panel C. 2nd follow-up survey						
	Wave					
	Week 4	376		321		85.4%
	Week 8	376		318		84.6%
	Week 12	375		287		76.5%
	Week 16	376		277		73.7%
	Total	1,503	15	1,203	81.0%	80.0%

Note: Invitations to the Entry Survey were sent daily between September 11, 2018 and November 24, 2018. Invitations to Follow-Up Surveys were sent on Fridays between September 24, 2018 and July 7, 2019. The click-thru rate is the percent of individuals who received the survey and clicked on the survey link but did not necessarily complete the survey. The number of e-mailed invitations for the first Follow-Up Survey is less than the number of completed responses in the Entry Survey because some respondents opted out of Follow-Up Surveys. One individual who completed the first Follow-Up Survey was accidentally not invited to the second follow up. Incentives to complete the Entry Survey and the second Follow-Up Survey were $10 and incentives for the first Follow-Up Survey were $5. We experimented with different incentive schemes during the first week of Entry Survey invitations and during the first two weeks of the second Follow-Up Survey invitations. See section 3.2 for details about completion rates and appendix A.3 for details about incentives and incentive experimentation.

Table A2: Job tenures in the entry survey sample and CPS tenure supplements

	Entry-survey sample (Davis and Krolikowski)	CPS tenure supplements (Hyatt and Spletzer, 2016)
Median tenure in years	1.8	4.5
Tenure distribution (percent)		
1 yr. or less	34	21
More than 1, less than 5 yrs.	39	28
5 yrs. or more	27	51

Note: The middle column reports statistics for tenures on the lost job in the Entry Survey sample. The right-most column reports statistics for employed persons in the CPS from Hyatt and Spletzer (2016).

Table A3: Percent of UI recipients who would accept a pay cut to save the lost job (weighted)

For permanent layoffs: *"Would you have been willing to stay at your last job for another 12 months at a pay cut of X percent?"*
For temporary layoffs: *"Suppose your employer offered a temporary pay cut of X percent as an alternative to the temporary layoff. Would you have been willing to accept the temporary pay cut to avoid the layoff?"*

Size of proposed paycut	5%	10%	15%	20%	25%
Permanent layoffs	60.2	52.8	43.4	36.0	31.8
	(2.4)	(2.5)	(2.5)	(2.3)	(2.3)
	404	413	410	419	423
Temporary layoffs	53.0	41.1	38.7	35.2	36.1
	(5.0)	(5.0)	(5.0)	(4.8)	(4.9)
	101	98	95	102	99

Note: Standard errors in percent and the number of observations appear beneath the percent of workers for each response. Similar to table 3 in the main text, but uses CPS weights. See appendix B for details.

Table A4: Percent of respondents who discussed a cut in pay, benefits or hours in lieu of layoff (weighted)

	Mean	S.E.	Count
Overall	2.9	0.3	2,567
Type of layoff (p-value: 0.05)			
Permanent	2.5	0.3	2,070
Temporary	3.9	0.9	497
Gender (p-value: 0.32)			
Male	2.6	0.5	1,223
Female	3.3	0.5	1,344
Education (p-value: 0.10)			
High school grad.	3.9	1.0	352
Technical training/some college	2.0	0.5	724
Associate/bachelor's degree	3.5	0.6	1,052
Grad. degree or higher	1.9	0.7	416
Industry (p-value: 0.57)			
Leisure and hospitality	5.1	1.7	162
FIRE	2.6	1.0	241
Construction	2.5	1.3	136
Educ. & Hlth. care services	2.2	0.7	428
Info. & other services	2.6	1.0	240
Manufacturing	3.8	0.8	517
Prof., tech., bus. services	1.3	0.6	311
Retail & wholesale trade	3.5	1.2	237
Transp., warehousing, utilities	2.6	1.3	156
Union job (p-value: 0.45)			
No	3.0	0.4	2,070
Yes	2.4	0.8	382
Tenure (p-value: 0.76)			
0-6mons	2.9	0.8	472
6mons to 2yrs	2.5	0.5	861
2yrs to 5yrs	3.5	0.8	536
More than 5yrs	3.0	0.6	698
Reason for layoff (p-value: 0.01)			
Slow business conditions	5.2	0.9	636
Going out of business	1.7	1.0	167
Reorganization/pos. abolished	2.5	0.6	641
Fired	1.6	0.5	653
Firm size (p-value: 0.01)			
1-49	4.4	0.7	927
50-499	2.2	0.5	845
500+	2.4	0.6	651

Note: We consider the null hypothesis of equal coefficients across the indicated categories (e.g., education groups) and report the p-value in parentheses. Similar to table 5 in the main text, but uses CPS weights. See appendix B for details.

Table A5: Percent of workers by reason for why no discussion occurred about cuts in pay, benefits or hours (weighted)

Question: "If you had to guess, why do you think your employer did not discuss any kind of cuts in pay, benefits or hours?"

	(1) It would lead the best workers to quit	(2) It would undermine morale	(3) It would not have prevented my layoff	(4) It's not allowed under union contract	(5) It would violate minimum wage laws	(6) Benefits cut would violate the law	(7) It would upset the employer's pay scale	(8) Don't know	(9) Other
Panel A. All responses									
Overall									
Count= 2,496	7.9	7.2	34.5		1.0	1.4	2.5	38.9	8.6
	(0.5)	(0.5)	(1.0)		(0.2)	(0.2)	(0.3)	(1.0)	(0.6)
Permanent layoff									
Count= 2,020	8.8	8.6	36.3		1.1	1.5	2.3	39.9	10.4
	(0.6)	(0.6)	(1.1)		(0.2)	(0.3)	(0.3)	(1.1)	(0.7)
Temporary layoff									
Count= 476	5.2	3.3	29.5		0.5	1.1	3.1	36.4	3.8
	(1.0)	(0.8)	(2.1)		(0.3)	(0.5)	(0.8)	(2.2)	(0.9)
500+ employees									
Count=635	7.9	8.0	32.4		0.6	2.1	3.9	33.7	8.9
	(1.1)	(1.1)	(1.9)		(0.3)	(0.6)	(0.8)	(1.9)	(1.1)
Union job									
Count=372	4.7	3.5	25.1	46.7	0.7	2.1	3.5	28.4	4.6
	(1.1)	(0.9)	(2.3)	(46.7)	(0.4)	(0.7)	(1.0)	(2.3)	(1.1)
Panel B. Permanent layoffs: Other detail									
	Employer cost cutting	Bankruptcy	Job outsourced, automated or abolished	Fired for cause or poor performance	Age, gender or race	Miscellaneous	No or uninformative response		
	(Count=22)	(Count=6)	(Count=38)	(Count=20)	(Count=11)	(Count=46)	(Count=74)		
	0.8	0.3	1.8	1.0	0.4	2.2	3.8		

Note: The first entry in each cell is the percent of responses with standard errors in parenthesis. Respondents could select multiple options, so row values need not sum to 100 percent. Column (4) considers only persons who lost jobs covered by union contracts. Similar to table 6 in the main text, but uses CPS weights. See appendix B for details.

Table A6: Percent of workers by reason for not accepting a pay cut (weighted)

	Count	(1) Can find another job that pays more	(2) The pay cut would feel like an insult	(3) I prefer not working over working at a lower pay level	(4) Other	(5) Uninformative response
Panel A. Permanent layoffs						
A. Permanent layoffs from non-union and union jobs	1,102	48.3 (1.5)	38.2 (1.5)	20.8 (1.2)	18.1 (1.2)	4.1 (0.6)
A.1. Permanent layoffs from non-union jobs	1,031	49.2 (1.6)	38.8 (1.5)	19.5 (1.2)	17.9 (1.2)	4.2 (0.6)
A.2. Permanent layoffs from union jobs	71	40.5 (5.9)	33.7 (5.6)	31.8 (5.6)	19.5 (4.7)	3.6 (2.2)
		Can't afford the pay cut	I am/would be underpaid	Bad fit, unsatisfactory conditions, long commute	Contract violation	Miscellaneous
A.3. Other detail for permanent layoffs from non-union jobs	200	7.6 (0.8)	4.3 (0.6)	2.4 (0.5)	0.6 (0.2)	3.3 (0.6)
		Can't afford the pay cut	Union agreement	Miscellaneous		
A.4. Other detail for permanent layoffs from union jobs	14	8.1 (3.3)	10.5 (3.7)	1.0 (1.2)		
		(1) Can find another job that pays more	(2) The pay cut would feel like an insult	(3) I prefer not working over working at a lower pay level	(4) Other	(5) Uninformative response
Panel B. Temporary layoffs						
B. Temporary layoffs from non-union and union jobs	271	26.2 (2.7)	24.0 (2.6)	24.4 (2.6)	56.8 (3.0)	4.4 (1.3)
B.1. Temporary layoffs from non-union jobs	89	44.9 (5.3)	23.6 (4.5)	28.1 (4.8)	39.3 (5.2)	4.5 (2.2)
B.2. Temporary layoffs from union jobs	182	17.0 (2.8)	24.2 (3.2)	22.5 (3.1)	65.4 (3.5)	4.4 (1.5)
		The temporary cut might become a permanent one	Can't afford the pay cut	Miscellaneous		
B.3. Other detail for temporary layoffs from non-union jobs	35	31.7 (5.0)	2.8 (1.8)	2.4 (1.6)		
		The temporary cut might become a permanent one	Can't afford the pay cut	Union agreement	Contract violation	Miscellaneous
B.4. Other detail for temporary layoffs from union jobs	119	46.0 (3.7)	0.8 (0.6)	18.5 (2.9)	2.1 (1.1)	0.8 (0.6)

Note: The first entry in each cell is the percent of responses among individuals not willing to accept a pay cut with standard errors in parenthesis. Respondents could select all that apply so the rows do not have to sum to 100 percent. Similar to table 7 in the main text, but uses CPS weights. See appendix B for details.

Table A7: Percent of layoffs that could be avoided by our proposed wage cuts

	Mean	S.E.	Count
Overall	27.6	0.9	2,493
Type of layoff (p-value: 0.98)			
Permanent	27.6	1.0	2,019
Temporary	27.6	2.1	474
Gender (p-value: 0.06)			
Male	25.8	1.3	1,192
Female	29.2	1.3	1,301
Education (p-value: 0.02)			
High school grad.	32.2	2.5	339
Technical training/some college	28.2	1.7	708
Associate/bachelor's degree	24.6	1.4	1,017
Grad. degree or higher	30.0	2.3	407
Industry (p-value: 0.46)			
Leisure and hospitality	23.2	3.4	155
FIRE	31.9	3.0	235
Construction	23.5	3.7	132
Educ. & Hlth. care services	30.9	2.3	418
Info. & other services	29.9	3.0	234
Manufacturing	26.6	2.0	497
Prof., tech., bus. services	25.5	2.5	306
Retail & wholesale trade	28.6	3.0	227
Transp., warehousing, utilities	22.4	3.4	152
Union job (p-value: 0.17)			
No	27.2	1.0	2,011
Yes	23.8	2.2	370
Tenure (p-value: 0.06)			
0-6mons	32.4	2.2	460
6mons to 2yrs	27.6	1.5	837
2yrs to 5yrs	26.0	1.9	520
More than 5yrs	25.6	1.7	676
Reason for layoff (p-value: 0.01)			
Slow business conditions	26.7	1.8	602
Going out of business	13.6	2.7	162
Reorganization/pos. abolished	28.9	1.8	627
Fired	30.0	1.8	643
Firm size (p-value: 0.34)			
1-49	24.9	1.5	888
50-499	26.5	1.5	827
500+	28.2	1.8	634

Note: This table reports the percent of respondents that would accept the proposed wage cut *and* believe that the cut would save the lost job. We consider the null hypothesis of equal coefficients across the indicated categories (e.g., education groups) and report the p-value in parentheses.

Table A8: Percent of layoffs that could be avoided by our proposed wage cuts, breakdown by layoff type and size of wage cut

	5%	10%	15%	20%	25%	Any
Permanent layoffs	35.0	28.9	28.2	23.8	22.4	27.6
	(2.4)	(2.3)	(2.2)	(2.1)	(2.0)	(1.0)
	391	401	404	408	415	2,019
Temporary layoffs	40.6	26.6	27.8	19.2	24.2	27.6
	(5.0)	(4.6)	(4.7)	(4.0)	(4.4)	(2.1)
	96	94	90	99	95	474

Note: This table reports the percent of respondents who would accept the proposed wage cut *and* that believe the cut would save the lost job. Standard errors in parentheses. The third row in each panel reports the sample size.